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EXTERNALITIES OF POLICY-INDUCED SCRAPPAGE: THE CASE OF AUTOMOTIVE REGULATIONS

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ABSTRACT

Many transportation policies indirectly affect vehicle travel and resulting externalities by inducing changes in vehicle scrappage rates. We leverage the staggered removal of state-level safety inspection programs across the United States within an instrumental variables (IV) framework to produce the first estimates of the fleet-size elasticities of fleet travel distance and gasoline consumption. Our first-stage estimates indicate that the removal of safety inspections caused a 3-4% increase in fleet size on average. Our IV estimates of the fleet-size elasticities of fleet travel distance and gasoline consumption have 95% confidence sets that imply rejection of an assumption commonly used in prior analyses that these elasticities are equal to one. Calculations based on fleet-size elasticities of one result in substantial overestimates of the externality costs from increases in travel and fuel use from delays in scrappage due to the removal of safety inspections.

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A data appendix is available at http://www.nber.org/data-appendix/w30546

1 Introduction

Automobile use comes with a host of negative externalities, including pollution emissions, traffic fatalities, and congestion (Parry, Walls and Harrington, 2007). Economists have long hypothesized that automotive policies can indirectly affect automobile use and associated externalities through changes in the rate at which used vehicles are scrapped and potentially replaced: a phenomenon called the Gruenspecht effect (Gruenspecht, 1982*b*). Policies for which the Gruenspecht effect is relevant are ubiquitous, including fuel efficiency standards (Bento, Roth and Zuo, 2018; Jacobsen and van Benthem, 2015), fuel taxes (Jacobsen and van Benthem, 2015), vehicle safety and emission inspection programs (Alberini, Harrington and McConnell, 1998; Hahn, 1995), vintage-specific vehicle restrictions (Barahona, Gallego and Montero, 2020), license-plate lotteries (Yang et al., 2020), and "Cash for Clunkers" programs (Alberini, Harrington and McConnell, 1998; Hahn, 1995; Hoekstra, Puller and West, 2017). Prior work has found empirical support for the Gruenspecht effect by estimating the effects of policy-induced changes in used car prices on scrappage rates (Bento, Roth and Zuo, 2018; Jacobsen and van Benthem, 2015).

We provide the first empirical estimates of the effect of policy-induced changes in scrappage rates on fleet-wide vehicle use and fuel consumption, which are the sources of most automobile externalities. In the absence of empirical estimates of the vehicle-fleet-size elasticities of fleet travel distance and gasoline consumption,[1] policymakers and researchers have been forced to make assumptions about these elasticities when assessing the costs and benefits of different transportation policies. For example, prior research has assumed that vehicles whose scrappage is delayed are driven the same as the average vehicle (Alberini, Harrington and McConnell, 1998), the average vehicle of the same model (Parks, 1977), or the average vehicle of the same type and age (Jacobsen and van Benthem, 2015), and that when vehicles are scrapped, the travel associated with those vehicles is lost, rather than shifted to other vehicles or modes.[2] On the other end of the spectrum, some studies have assumed that fleet travel distance does not respond to changes in scrappage rates at all (e.g., NHTSA and USEPA, 2020).

[1] Fleet travel distance is typically measured in vehicle-miles travelled (VMT) in the United States.

[2] In 2018, the U.S. National Highway Traffic Safety Administration justified a proposed rollback to the Corporate Average Fuel Economy (CAFE) and light-duty vehicle greenhouse gas emissions standards based on a cost-benefit analysis that made similar assumptions (NHTSA and USEPA, 2018a).

We estimate the fleet-size elasticity of fleet travel distance and the fleet-size elasticity of gasoline consumption by exploiting the staggered removal of state-level vehicle safety inspection programs across the United States from 1970-2017. Namely, we use variation in fleet size induced by the removal of safety inspections to estimate the impact of fleet size on fleet travel distance and gasoline consumption using an instrumental variables approach. The intuition underlying this approach is that the removal of safety inspection requirements reduces expected repair costs, delaying the scrappage of used vehicles on the margin. A key assumption is that the removal of safety inspections affects travel and gasoline consumption only through policy-induced changes in fleet size, an assumption we justify for our setting in Section 4.3.

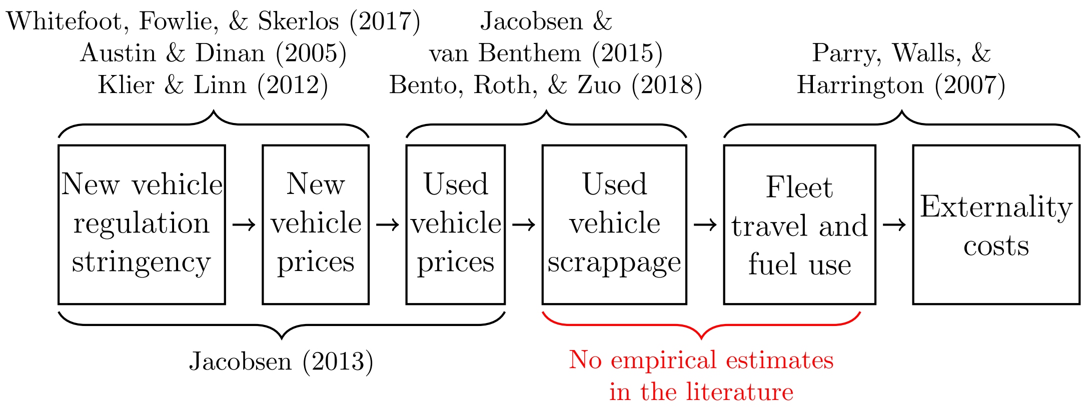
Estimates from our first-stage difference-in-differences model indicate that the removal of safety inspections increases vehicle registrations by 3% to 4%. Estimates from event study specifications demonstrate that the first-stage estimates are not the result of pre-existing differences in trends across treatment and control states. Further, evidence from the decomposition specified in Goodman-Bacon (2021) suggests that the estimates are not driven by idiosyncratic comparisons across states that removed safety inspections earlier versus later. Finally, we provide evidence that the removal of safety inspections in one state did not affect vehicle registrations in neighboring states.

Using an instrumental variables approach, we provide the first empirical estimates of the effects of policy-induced changes in fleet size on fleet-wide travel and gasoline consumption. To address recent concerns regarding weak instruments (Andrews, Stock and Sun, 2019; Lee et al., 2020), we perform hypothesis testing using the Anderson-Rubin test statistic that is robust to the bias induced by weak instruments (Anderson and Rubin, 1949). In our preferred specification, we are able to reject values for the fleet-size elasticity of fleet travel distance greater than 0.64 and values for the fleet-size elasticity of gasoline consumption greater than 0.33. This indicates that there are diminishing marginal increases in vehicle travel and gasoline consumption from policy-induced increases in fleet size. Moreover, we are able to reject the fleet-size elasticities implied by the assumptions made in prior analyses such as Alberini, Harrington and McConnnel, 1998, NHTSA and USEPA, 2018a, and NHTSA and USEPA, 2020. This highlights how our 95% confidence sets can be used to inform the formulation and calibration of transportation models moving forward.

We calculate the externality costs of removing safety inspections implied by our estimates versus the fleet-size elasticities assumed in prior work. The annual externality cost associated with the removal of safety inspection programs is overestimated by at least 90 million dollars per state when assuming fleet-size elasticities of one rather than our preferred estimates, amounting to a total estimation error of approximately 40 billion dollars over 1970-2017.

This study makes three contributions to existing literature. First, as shown in Figure 1, the impact of fleet size on fleet travel distance is an essential link in the causal chain from changes in automotive regulations to the resulting changes in the externality costs associated with vehicle travel. Previous work has examined how changes in policy impact prices in the new and used vehicle markets (Austin and Dinan, 2005; Jacobsen, 2013; Klier and Linn, 2012; Whitefoot, Fowlie and Skerlos, 2017) as well as how changes in used vehicle prices impact the scrappage of used vehicles (Bento, Roth and Zuo, 2018; Jacobsen and van Benthem, 2015). We provide the first empirical estimates and 95% confidence sets on the fleet-size elasticities of vehicle usage and gasoline consumption.

Figure 1: Prior empirical work on the effect of automotive policy on the externality costs from fleet travel distance and fuel use



By completing the causal chain linking changes in policy to changes in aggregate externality costs, our estimates can help improve cost-benefit analyses of transportation regulations. These regulations include vehicle retirement programs (Alberini, Harrington and McConnell, 1998; Hahn, 1995; Sandler, 2012), fuel taxes (Jacobsen and van Benthem, 2015; Li, Timmins and von Haefen, 2009), and vintage-specific vehicle restrictions (Barahona, Gallego and Montero, 2020). Many prior analyses of such policies have been forced due to a lack of empirical evidence to make assumptions about the forgone mileage from scrappage (Alberini, Harrington and McConnell, 1998), the mileage travelled by a replacement vehicle (Sandler, 2012), or both (Jacobsen and van Benthem, 2015).[3] Our findings suggest that prior studies have often assumed fleet-size elasticities of fleet travel distance and gasoline consumption that are too large, resulting in overestimates of the increases in travel and gasoline consumption due to policy-induced changes in scrappage and thus overestimates of the resulting changes in negative externalities, such as local air pollution, traffic fatalities, and congestion (Parry, Walls and Harrington, 2007).

Finally, our work contributes to the broader literature on the Gruenspecht effect. Policies that differentially affect new and old technologies change the scrappage rates of old technologies, which has implications for the policy's impact on the aggregate externality costs associated with the technology (Gruenspecht, 1982*b*). Existing work has investigated how various policies affect the scrappage of older technologies across a variety of sectors, including manufacturing (Levinson, 1999), the power sector (List, Millimet and McHone, 2004; Maloney and Brady, 1988; Nelson, Tietenberg and Donihue, 1993), and air travel (Kahn and Nickelsburg, 2016).[4] Our results indicate that the elasticity of vehicle use with respect to fleet size is less than one, suggesting the need to empirically estimate how policy-induced scrappage impacts the externalities generated from technology use in other sectors as well.

2 Data

We collected data from the Highway Statistics Series administered by the U.S. Federal Highway Administration (FHWA) for all 50 states and DC from 1970-2017. For each year, these data include state-wide vehicle registrations, vehicle-miles traveled, gasoline usage, and road mileage.[5] The gasoline consumption data focuses on road-

[3] In related work, Barahona, Gallego and Montero (2020) formulate and estimate a structural model of the car market to analyze how equilibrium scrappage rates and vehicle usage respond to imposing vintage-specific driving restrictions.

[4] For example, previous work has estimated that provisions in the 1970 Clean Air Act that imposed less stringent environmental regulations on older power plants led to older plants operating for longer than they would have in the absence of the policy (List, Millimet and McHone, 2004; Maloney and Brady, 1988; Nelson, Tietenberg and Donihue, 1993).

[5] Outcome data points for a given state that are repeated across years were removed (e.g. the 2010 data point is the same as the 2009 data point). With respect to vehicle registrations, we know through a source at the U.S. Department of Transportation that there were reporting errors for Colorado from 2002-2010; we remove these data points. See Appendix Section A.2 for further information on how the data are constructed.

way vehicles and thus excludes non-roadway gasoline consumption such as boating and lawn equipment. Annual state-level income, population, and GDP were collected from the Regional Economic Accounts from the Bureau of Economic Analysis. The average motor gasoline price for each state and DC in each year come from the State Energy Data System provided by the Energy Information Administration. Table 1 summarizes the sources of all of the data used in the analysis.

3 Empirical Approach

3.1 Identification Strategy

Traditional models of scrappage posit that an owner will scrap (rather than re-sell) vehicle i at time t if the operational value u_{it} after repair costs c_{it} are paid is less than the scrappage value s_{it} (Gruenspecht, 1982*b*; Parks, 1977).

$$u_{it} - c_{it} < s_{it} \tag{1}$$

Interventions that either increase the value of operation or decrease repair costs thus delay scrappage at the margin. We argue that the removal of safety inspection programs reduces repair costs, resulting in delays in scrappage on the margin.

Safety inspection programs, which were implemented in different states at different times throughout the past century, mandate that personal vehicles must pass an inspection of certain vehicle components that often include the vehicle's brakes, windshield wipers, and suspension (U.S. Government Accountability Office, 2015, p.7). Safety inspections are generally required annually or biannually (U.S. GAO, 2015; NHTSA, 1989). Vehicles that do not pass the safety inspection must be repaired at the owner's expense.[6]

While some states have kept their safety inspection programs in place, many others have since removed these programs due to budgetary concerns and questions about the effectiveness of the programs for improving traffic safety (U.S. GAO, 2015; NHTSA, 1989). Between 1970-2017, 15 states plus the District of Columbia removed existing

[6] We can confirm that this is true for the 15 states contacted during a US Government Accountability Office (GAO) review of the policies (U.S. GAO, 2015, p.8) but note that earlier documentation of inspection programs is unclear about the repercussions for vehicles on the road that fail a safety inspection (NHTSA, 1989, Section III).

Table 1: Data Sources Used in the Analysis

Data	Years Collected	Source	Table(s)	Issue Year
Implicit Price Deflators for GDP	1929-2019	BEA	Table 1.1.9	
All Items CPI-U	1913-2021	BLS	CUUR0000SA0, CUUS0000SA0	
Average Motor Gasoline Price	1970-2017	EIA	Motor Gasoline Prices and Expenditures, 1970-2017 (Prices table)	
Employment	1969-1997	BEA	SAEMP25S	
Employment	1998-2017	BEA	SAEMP25N	
Gas CPI-U	1935-2019	BLS	CUUR0000SETB01, CUUS0000SETB01	
Highway Gas Usage	1967-2018	FHWA	MF-226	2018
Licensed Drivers	1967-2017	FHWA	DL-201	2017
Metro Income	1969-2018	BEA	MAINC1	
Metro Population	1969-2018	BEA	MAINC1	
Non-Metro Income	1969-2018	BEA	MAINC1	
Non-Metro Population	1969-2018	BEA	MAINC1	
Road Mileage	1980-2017	FHWA	HM-220	2018
Road Mileage	1967-1979	FHWA	M-1	1967-1979
State GDP	1997-2018	BEA	SAGDP2N	
State GDP	1963-1996	BEA	SAGDP2S	
Vehicle Registrations	1967-1993	FHWA	MV-201	Summary to 1995
Vehicle Registrations	1994-2017	FHWA	MV-1	1994-2017
Motor Vehicle Maintenance & Repair CPI	1935-2019	BLS	CUUR0000SETD, CUUS0000SETD	
VMT	1980-2017	FHWA	VM-202	2018
VMT	1967-1979	FHWA	VM-2	1967-1979

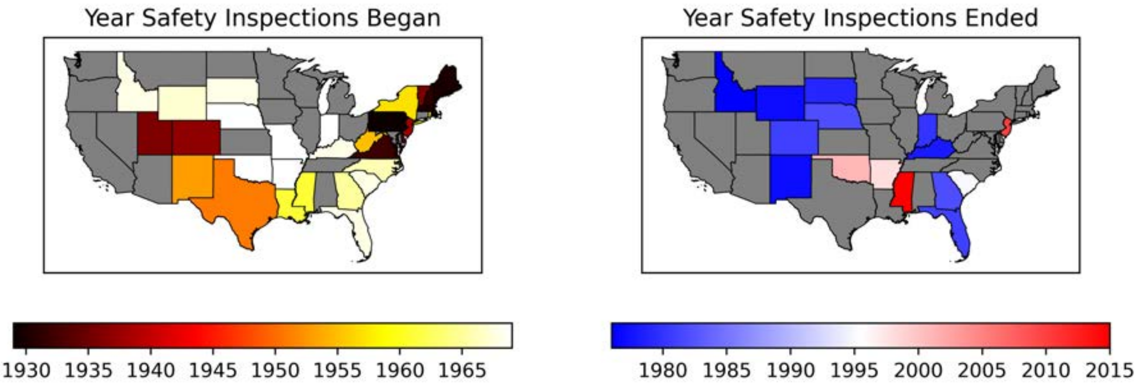
Notes: This table lists the data sources used in the analysis. When relevant, all dollar magnitudes except Average Motor Gasoline Price have been transformed to 2018 USD using the "All Items CPI-U" from the Bureau of Labor Statistics (BLS). Average Motor Gasoline Price has been transformed to 2018 USD using the Gas CPI-U from BLS. If available, we use the most up-to-date version of the tables listed in the FHWA for each issue year.

safety inspection programs implemented before this period, 16 did not remove existing programs, and 19 states never adopted such programs (see Figure 2).[7]

As descriptive evidence that safety inspections impose potentially sizable costs on

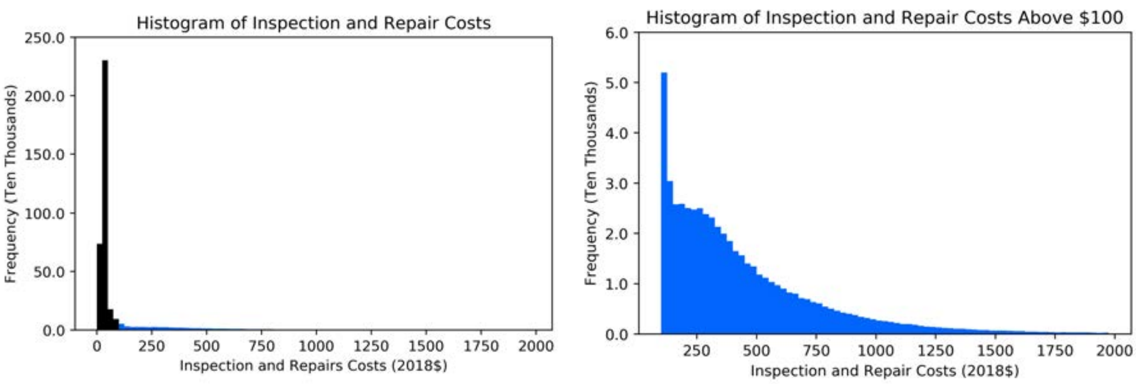
[7] Utah changed safety inspection requirements in 2018, after the end of our sample period (Utah Division of Motor Vehicles, 2020).

Figure 2: Safety Inspection Start and End Years



Notes: This figure plots maps of the year that each state implemented safety inspections (left panel) and the year that each state removed safety inspections (right panel). Program years come from the U.S. Government Accountability Office (U.S. GAO, 2015). State map files are from the U.S. Census Bureau (U.S. Census Bureau, 2018*a*). States colored grey in both the left and right panels never implemented safety inspection programs; states colored grey only in the right panel never ended their safety inspection program during the sample period considered. These maps exclude Alaska and Hawaii; Alaska has never had safety inspections, and Hawaii began safety inspections in 1961 and they are still in place. The District of Columbia had safety inspections from 1939 to 2009.

Figure 3: Histograms of inspection and vehicle repair costs in Pennsylvania



Notes: Data come from the 2007-2016 e-Safety inspection program in Pennsylvania. Values shown include all recorded costs for safety inspection visits that were not denoted as a non-inspection cost (repair costs and sales tax are included). Data points with encoding errors were excluded (20 entries) as well as all data where inspection costs were larger than $50,000, which were likely data entry errors (34 entries). We also remove entries with vehicle identification numbers (VINs) that do not satisfy the VIN check digit condition, contain disallowed characters, or do not have 17 digits (109,244 entries). These requirements are outlined in U.S. National Highway Traffic Safety Administration (2008). For more information on the data, see Peck et al. (2015).

vehicle owners, we leverage data from Pennsylvania's Department of Transportation to examine the magnitude of inspection-related costs. Figure 3 presents a histogram of inspection costs and associated repairs. While average costs are relatively low, there is a long right tail, with costs for some vehicles reaching thousands of dollars. Table 2 shows summary statistics for inspection and repair costs stratified by vehicle age. For vehicles with repair costs on the right tail, particularly older vehicles near the end of

Table 2: Summary statistics of inspection and repair costs for Pennsylvania from 2007-2016: By vehicle vintage

Age Category	Count (Millions)	Mean	Std. Dev.	Median	95^{th} Percentile	99^{th} Percentile
$\leq$4 Years Old	1.37	\$61	\$269	\$31	\$209	\$773
5-9 Years Old	1.2	\$114	\$369	\$33	\$559	\$1,151
10-14 Years Old	0.84	\$121	\$367	\$34	\$585	\$1,214
15-19 Years Old	0.32	\$111	\$323	\$34	\$526	\$1,140
$\geq$20 Years Old	0.13	\$92	\$279	\$33	\$408	\$1,027
All Ages	3.86	\$96	\$331	\$32	\$468	\$1,056

Notes: This table presents summary statistics pertaining to the inspection and repair costs from 2007-2016 from e-Safety inspections in Pennsylvania. Summary statistics are stratified by age. All monetary values are inflated to 2018 USD using the Motor Vehicle Maintenance & Repair CPI.

their life that have low resale value, failing a safety inspection constitutes a substantial shock to repair costs, influencing the scrappage decision illustrated in Equation (1).

Our primary analysis uses the staggered removal of state-level safety inspection programs as an instrumental variable when estimating the impact of changes in fleet size on annual state-wide fleet travel and gasoline consumption. The following subsections specify the first- and second-stage equations associated with this empirical approach. We discuss the identifying assumptions necessary for this approach in Section 4.3.6.

3.2 First-Stage Equation

In our first-stage, we estimate a difference-in-differences (DiD) model where we regress the log of vehicle registrations r_{it}, indexed by state i and year t, on treatment and control variables.

Our first-stage DiD model is:

$$\log(r_{it}) = \lambda_i^{\mathrm{R}} + \tau_t^{\mathrm{R}} + \xi_i^{\mathrm{R}} t + \alpha D_{it} + \mathbf{x}_{it}^{\mathrm{R}} \boldsymbol{\delta}^{\mathrm{R}} + u_{it}^{R} \tag{2}$$

Since the treatment in our setting is the removal of safety inspections, D_{it} is an indicator variable equal to 1 if state i did *not* have safety inspection requirements in year t. The term $\mathbf{x}_{it}^{\mathrm{R}}$ denotes a $1 \times K$ vector of control variables that includes annual state-level average gasoline price, employment, licensed drivers, metro and non-metro income, metro and non-metro population, road mileage, and GDP. We also control for changes in the state's data collection methodology over time, described in detail in Section 3.4. Finally, we include state fixed effects, year fixed effects, and state-specific linear time trends. Standard errors are clustered by state.

3.3 Second-Stage Equations

In the presence of weak instruments, the point estimates from instrumental variables estimation are consistent, but (1) finite-sample bias can be large, and (2) the assumption that the point estimates are approximately normally distributed may be poor (Andrews, Stock and Sun, 2019). Consequently, we conduct inference using an approach that is robust to the presence of weak instruments (Anderson and Rubin, 1949). Though we use terminology typically associated with two-stage least squares, we emphasize that standard errors are estimated using the Anderson-Rubin approach rather than the approach typically utilized when employing two-stage least squares.

The second-stage equations for our two primary dependent variables, annual statewide total vehicle-miles travelled (v_{it}) and annual statewide total gasoline consumption (g_{it}), are:

$$\log(v_{it}) = \lambda_i^{\mathrm{V}} + \tau_t^{\mathrm{V}} + \xi_i^{\mathrm{V}} t + \beta \widehat{\log(r_{it})} + \mathbf{x}_{it}^{\mathrm{V}} \boldsymbol{\delta}^{\mathrm{V}} + \varepsilon_{it}^{\mathrm{V}} \tag{3}$$

$$\log(g_{it}) = \lambda_i^{\mathrm{G}} + \tau_t^{\mathrm{G}} + \xi_i^{\mathrm{G}} t + \gamma \widehat{\log(r_{it})} + \mathbf{x}_{it}^{\mathrm{G}} \boldsymbol{\delta}^{\mathrm{G}} + \varepsilon_{it}^{\mathrm{G}} \tag{4}$$

The coefficients of interest, β and γ, measure the elasticities of fleet travel distance and gasoline consumption with respect to fleet size. For ease of exposition, we refer to these as the fleet-size elasticities of travel and gasoline consumption respectively. As with the first-stage specifications, we include state fixed effects, year fixed effects, and state-specific linear time trends in the second-stage specifications. As before, the sets of control variables $\mathbf{x}_{it}^{\mathrm{V}}$ and $\mathbf{x}_{it}^{\mathrm{G}}$ contain annual state-level average gasoline price, employment, licensed drivers, metro and non-metro income, metro and non-metro population, road mileage, and GDP. We also include data source controls that account for changes in data reporting, which are described in Section 3.4.

Our model estimates elasticities using policy-induced variation in fleet size. Safety inspections affect fleet size directly by reducing the fleet size by one unit for each vehicle scrapped. They may also affect fleet size indirectly by inducing new vehicle sales to replace the scrapped vehicles, potentially shifting fleet composition towards newer, more efficient vehicles and increasing travel per vehicle to the extent that newer vehicles are driven more than older vehicles (i.e, the "rebound effect").

However, in Section 4.3.7, we provide evidence that the aggregate fuel efficiency of the fleet does not change much with the removal of safety inspections. This suggests that the miles that would have been driven by vehicles scrapped due to safety inspections are replaced by miles traveled by other vehicles with similar fuel efficiency. If this is the case, the rebound effect is small in our setting. This evidence also buttresses the exogenoeity assumption required for instrumental variables analysis: the removal of safety inspections does not seem to affect fleet travel distance or gasoline consumption through changes in fleet composition.

3.3.1 Null Hypotheses

Studies that model the impacts of transportation policy typically do not report elasticities of vehicle fleet travel and of gasoline consumption with respect to fleet size. Instead, each model makes assumptions about vehicle mileage and scrappage patterns for specific vehicle groups that result in implied elasticities with respect to fleet size. In this subsection, we characterize the implicit elasticity assumptions made in the prior analyses listed in Table 3.

We consider several null hypotheses pertaining to the fleet-size elasticities of fleet travel distance (β) and gasoline consumption (γ). We term the first set of null hypotheses the "Unit-Elasticity" null hypotheses: $H_0\colon \beta = 1$ and $H_0\colon \gamma = 1$. Setting $\beta = 1$ corresponds to the assumption that the additional distance driven from each vehicle whose scrappage is delayed is equal to some constant distance travelled among all vehicles (Alberini, Harrington and McConnell, 1998, p.6).[8]

Our second set of null hypotheses is based on the results of a recent policy analysis of the Safer Affordable Fuel-Efficient (SAFE) Vehicles Rule as originally proposed in 2018 (NHTSA and USEPA, 2018a). The assumptions in this analysis imply fleet-size elasticities of $\beta = 1.6$ and $\gamma = -4.8$. However, these fleet-size elasticities include

[8]Jacobsen and van Benthem (2015) utilizes this assumption in Supplementary Materials for calculations based on a less complex and less preferred version of the model considered in the main text (Jacobsen and van Benthem, 2015, SI p.9-10).

the "rebound effect": vehicles scrapped due to the policy change may be replaced by newer, more efficient vehicles that are driven more than the average vehicle. Since our empirical evidence suggests that the rebound effect is small when considering the removal of safety inspections, we also test null hypotheses implied by the assumptions in NHTSA and USEPA, 2018a with the rebound effect excluded: $H_0\colon \beta = 0.3$ and $H_0 : \gamma = -6.1$. See Appendix Section B.10 for details on the derivation of these null hypotheses.

Our third set of null hypotheses is based on an analysis of the final SAFE Vehicles Rule implemented in 2020 (NHTSA and USEPA, 2020). This revised analysis assumes that fleet travel distance is independent of fleet size, with variation in fleet travel distance solely stemming from changes in the composition of the fleet. We derive fleet-size elasticities of travel distance and gasoline consumption using results from the U.S. EPA's analysis of the final SAFE Vehicles Rule, giving us the following null hypotheses: $H_0 : \beta = -2.5$ and $H_0 : \gamma = 9.7$ when including the rebound effect, and $H_0 : \beta = 0$ and $H_0 : \gamma = 11$ when excluding the rebound effect. Details are provided in Appendix Section B.10.

3.4 Control Variable Specifications

We consider four specifications based on different levels of controls. In all specifications, we include a set of common controls: annual state-level average gasoline price, number of licensed drivers, miles of roads, employment, GDP, as well as metro and non-metro income and population.[9] Metro and non-metro population and income are included to account for changes in urbanization within a state over time, which could affect travel demand and access to other transportation modes such as public transit.

Vehicle registrations, travel, and fuel use, which are the outcome variables investigated, are reported by each state in each year between 1970-2017. However, the way these data are measured and reported can vary over this time period. The four

[9]This choice of control variables was informed by a review of the relevant literature (Bento, Roth and Zuo, 2018; Duranton and Turner, 2011; Gillingham, Jenn and Azevedo, 2015; Greene, 2010; Gruenspecht, 1982*a*,*b*; Haughton and Sarkar, 1996; Hymel and Small, 2015; Hymel, Small and Dender, 2010; Jacobsen and van Benthem, 2015; Parks, 1977; Schimek, 1997; Small and Van Dender, 2007; Walker, 1968). All of the variables in our set of common controls are log transformed except for metro and non-metro population and income. These latter two variables are transformed using the inverse hyperbolic sine function, which allows for zero value entries (Bellemare and Wichman, 2020).

Table 3: Fleet-modeling assumptions made in related literature used to generate null hypotheses

Literature	**Modeling Assumption**			**Effective Fleet Size Elasticity**	
	Fleet Size	**Fleet Travel**	**Rebound Effect**	**Travel**	**Fuel Use**
Gruenspecht (1982*b*)	Constant	Constant	No	N/A[a]	N/A[a]
Hahn (1995)	Constant	Dynamic	No	N/A[a]	N/A[a]
Alberini, Harrington and McConnell (1998)	Dynamic	Constant	No	1	N/A[b]
Jacobsen and van Benthem (2015)	Dynamic	Dynamic	No	—[c]	—[c]
Jacobsen and van Benthem (2015, SI p.9-10)	Dynamic	Constant	No	1	—[c]
NHTSA and USEPA, 2018a	Dynamic	Dynamic	Yes	1.6	-4.8
NHTSA and USEPA, 2018a	Dynamic	Dynamic	No	0.3	-6.1
NHTSA and USEPA, 2020	Dynamic	Constant[d]	Yes	-2.5	9.7
NHTSA and USEPA, 2020	Dynamic	Constant	No	0	11

Notes: "Constant" versus "dynamic" fleet size (fleet travel) refers to whether or not the model assumes a constant fleet size (fleet travel) throughout their analysis. "Rebound effect" denotes whether or not the model incorporates rebound when simulating travel (i.e., policy-induced scrappage may lead to the purchase of newer vehicles that are driven more than older vehicles). All of the models considered allow the composition of the fleet to change with changes in policy. It should be noted that the most recent CAFE ruling adheres to similar assumptions made in the SAFE FRIA (NHTSA, 2022, p.71).

[a]Since fleet size is assumed to be constant, there is no implied fleet size elasticity of travel or fuel use.

[b]The model in Alberini, Harrington and McConnell (1998) does not incorporate fuel efficiency.

[c]The equivalent fleet-wide elasticity assumptions used in the main model in Jacobsen and van Benthem (2015) could not be calculated due to the complexity of the underlying model.

[d]The analysis allows fleet VMT to change only via the rebound effect. The rebound effect is generally in reference to the fuel efficiency elasticity of travel (see Dimitropoulos, Oueslati and Sintek (2018) for further discussion and review of empirical literature).

specifications control differently for documented and potential changes in the data sources and methods used to compute total vehicle registrations, travel, and gasoline consumption in different states in different years. We refer to these as Data Source Controls (DSCs).

In specification (1), no controls for data source changes are included, but we remove data from Colorado between 2002-2009 because of documented reporting errors. We refer to the DSCs in this case as "None". In specification (2), which is our preferred specification, controls are added for time periods when states document that calculation of registrations changed from counting them directly to relying on transaction data to approximate the number of registrations. We refer to the DSCs in this case as "Documented".

In sensitivity analyses, we also examine two additional specifications that add controls for state-years where sizable jumps in registration data are observed but there is no documentation of potential changes in data sources or methods that might have influenced the data. In specification (3), additional controls are added for all state-years in which the change in vehicle registrations from the prior year—normalized by the magnitude of the state's annual changes across the time period analyzed—are larger in magnitude than that observed in Colorado for the years where documented reporting errors occurred. We refer to the DSCs in this case as "Large Undocumented". In specification (4), a smaller threshold of step changes in registration data is used. Specifically, the threshold is set low enough to control for all step changes whose normalized magnitudes are greater than or equal to an observed step change in Kentucky's data that appears visually suspicious. We refer to the DSCs in this case as "Small Undocumented". Further details on the identification of step changes are provided in Appendix Section A.4.

4 Results

4.1 Effect of the Removal of Safety Inspections on Registrations

The estimates from our first-stage difference-in-differences model can be found in Table 4. Our preferred specification is the one presented in Column 2, as it controls for documented changes in data reporting that may systematically affect measurement of vehicle registrations. Under the preferred specification, removal of safety inspections led to an estimated 3.7% increase in vehicle registrations on average across states where safety inspections were removed.

Additional specifications that add controls for observed patterns in the data that we speculate might stem from changes in data reporting protocols that were not documented by states, as described in Section 3.4, are presented in Appendix B.1. The estimates of the effect of the removal of safety inspections on registrations remain similar in magnitude when considering alternative specifications which eliminate or add controls for documented and undocumented potential changes in data collection. The estimated effects are statistically significant at at least the 10% level across all specifications.

Table 4: Effect of the Removal of Safety Inspections on Log Number of Vehicle Registrations

Dep. Var.: Log Number of Vehicle Registrations	(1)	(2)
Removal of Safety Inspections	0.042**	0.037**
	(0.017)	(0.015)
Data Source Controls	None	Documented
State Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
State-Specific Linear Time Trends	Y	Y
Common Controls	Y	Y
Robust F-Stat	5.766	5.874
Number of Obs.	2,424	2,424
R^2	0.997	0.997

Notes: This table presents difference-in-differences estimates of the impact of the removal of safety inspections on the log of vehicle registrations. The unit of observation is state-year. The construction of data source controls (DSCs) are documented in Section 3.4. Standard errors, reported in parentheses, are clustered by state. * p<0.1, ** p<0.05, *** p<0.01

4.2 Effect of Registrations on Fleet Travel and Gasoline Use

The instrument in our first-stage regression appears to be weak, as measured by the robust F-statistics reported in Table 4.[10] Hence, when estimating our IV coefficient, we perform hypothesis testing that is robust to weak instrument bias using the Anderson-Rubin chi-squared statistic (Anderson and Rubin, 1949).[11] This is especially important given recent work documenting that conventional rules of thumb regarding whether instruments are weak are anti-conservative (i.e., the rejection rate of the presence of weak instruments is artificially high). Failing to account for the presence of weak

[10] Robust F-statistics are calculated as described in Andrews, Stock and Sun (2019, p.737). This statistic is equal to the cluster-robust effective F-Statistic given our single-variable, just-identified IV model (Olea and Pflueger, 2013).

[11] Andrews, Stock and Sun (2019) reviews the desirable properties of the Anderson-Rubin (AR) test in the just-identified case.

instruments when calculating standard errors typically leads to artificially small standard errors (Andrews, Stock and Sun, 2019; Lee et al., 2020).

Table 5: IV Estimates of the Effect of Log Number of Registrations on Log VMT

Panel A: IV Estimates and Robust 95% Confidence Sets		
Dep. Var.: Log Vehicle-Miles Travelled	(1)	(2)
Log Registrations	-0.326	-0.36
	[-3.49, 0.56]	[-3.69, 0.64]
Data Source Controls	None	Documented
State Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
State-Specific Linear Time Trends	Y	Y
Common Controls	Y	Y
Number of Obs.	2,424	2,424
Panel B: Anderson-Rubin Test Statistics and P-Values		
$H_0 : \beta = 1.6$		
AR Stat	7.351***	7.086***
p-value	0.007	0.008
$H_0 : \beta = 1$		
AR Stat	6.168**	5.631**
p-value	0.013	0.018
$H_0 : \beta = 0.3$		
AR Stat	2.098	1.867
p-value	0.148	0.172
$H_0 : \beta = 0$		
AR Stat	0.540	0.535
p-value	0.462	0.464
$H_0 : \beta = -2.5$		
AR Stat	3.166*	2.995*
p-value	0.075	0.084

Notes: The top panel of this table presents IV estimates of the effect of log vehicle registrations on log vehicle-miles travelled (VMT). We include 95% confidence sets robust to weak instruments below these estimates (Anderson and Rubin, 1949). The bottom panel presents Anderson-Rubin chi-squared statistics along with p-values for different null hypotheses pertaining to the parameter β estimated in the top panel. The unit of observation is state-year. All regressions include state fixed effects, year fixed effects, and state-specific linear time trends. The specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. 95% confidence sets are based on standard errors clustered by state. * p<0.1, ** p<0.05, *** p<0.01

Tables 5 and 6 presents IV estimates of the fleet-size elasticities of fleet travel and gasoline consumption, as well as the test statistics and p-values associated with tests of relevant null hypotheses. Based on our preferred specification, we are 95% confident that the fleet-size elasticity of travel is between -3.69 and 0.64. This confidence set is large, including even negative elasticities. A negative elasticity would imply that fleet travel distance decreases with policy-induced increases in vehicle registrations. This could plausibly occur through the "rebound effect" if, for example, delayed scrappage reduces a shift to newer vehicles and newer, more fuel-efficient vehicles are driven more than older vehicles. However, existing estimates of the rebound effect are smaller than those implied by the lower bound of our 95% confidence set (Gillingham, 2014;

Table 6: IV Estimates of the Effect of Log Number of Registrations on Log Gasoline Consumption

Panel A: IV Estimates and Robust 95% Confidence Sets		
Dep. Var.: Log Gasoline Consumption		
	(1)	(2)
Log Registrations	-0.420	-0.47
	[-1.75, 0.27]	[-1.71, 0.33]
Data Source Controls	None	Documented
State Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
State-Specific Linear Time Trends	Y	Y
Common Controls	Y	Y
Number of Obs.	2,429	2,429
Panel B: Anderson-Rubin Test Statistics and P-Values		
$H_0 : \gamma = 11$		
AR Stat	5.877**	6.042**
p-value	0.015	0.014
$H_0 : \gamma = 9.7$		
AR Stat	5.892**	6.052**
p-value	0.015	0.014
$H_0 : \gamma = 1$		
AR Stat	5.707**	5.462**
p-value	0.017	0.019
$H_0 : \gamma = -4.8$		
AR Stat	5.237**	5.479**
p-value	0.022	0.019
$H_0 : \gamma = -6.1$		
AR Stat	5.365**	5.606**
p-value	0.021	0.018

Notes: The top panel of this table presents IV estimates of the effect of log vehicle registrations on log gasoline consumption. We include 95% confidence sets robust to weak instruments below these estimates (Anderson and Rubin, 1949). The bottom panel presents Anderson-Rubin chi-squared statistics along with p-values for different null hypotheses pertaining to the parameter γ estimated in the top panel. The unit of observation is state-year. All regressions include state fixed effects, year fixed effects, and state-specific linear time trends. The specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. 95% confidence sets are based on standard errors clustered by state. * p<0.1, ** p<0.05, *** p<0.01

Gillingham, Jenn and Azevedo, 2015; Greene, 2010; Hymel and Small, 2015; Hymel, Small and Dender, 2010; Small and Van Dender, 2007). Moreover, we find that the removal of safety inspections did not significantly impact the aggregate fuel efficiency of the fleet, suggesting that the rebound effect is small in our setting.

Instead, the wide range of values covered by the 95% confidence set likely reflects statistical uncertainty, especially given that we are using the Anderson-Rubin methodology known to be quite conservative. Despite the statistical uncertainty surrounding the magnitude of the fleet-size elasticity of travel, we can nevertheless reject with our preferred specification the assumptions made in prior literature that this elasticity is either 1 or 1.6 (see Column 2 of Table 5). Moreover, we can reject at a 10% level that $\beta = -2.5$. The top panel of Figure 4 summarizes these results.

The IV estimation of the effect of fleet size on gasoline consumption tells a similar story (see Table 6). In our preferred specification, we estimate that the fleet-size elasticity of gasoline consumption has a 95% confidence set spanning from -1.7 to 0.33 (see Column 2 of Table 5). Despite the statistical uncertainty surrounding the elasticity estimate, we are nevertheless able to reject all of the null hypotheses considered for γ. The bottom panel of Figure 4 summarizes our findings.

Our 95% confidence sets can inform future modeling of transportation policy. If a set of assumptions and calibrated values regarding how policy-induced scrappage impacts vehicle usage and gasoline consumption discords with the set of fleet-size elasticities presented in Tables 5 and 6, this may suggest the need to alter the assumptions or calibrated values used to formulate and simulate the model. Embedded within this recommendation is the need to check not just how policy-induced scrappage impacts vehicle usage but also gasoline consumption, an important determinant of the air pollution externalities associated with driving.

The estimates and 95% confidence sets remain similar if we exclude the controls for documented changes in data reporting (compare Columns 1 and 2 of Tables 5 and 6). However, the 95% confidence sets become considerably wider once we add the large number of controls corresponding to undocumented potential changes in data reporting constructed using jumps in observed number of registrations or the outcome (see Appendix B.1 for these results). Indeed, in some cases, the 95% confidence set ranges from negative infinity to positive infinity (i.e., no value can be rejected with 95% confidence). This large increase in statistical uncertainty is likely due to including too many control variables: specifications (3) and (4) attempt to control for 15 and 54 identified discontinuities in the first stage equation as well as additional discontinuities in the second stage, respectively. Consequently, we do not view specifications based on adding these controls for suspected changes in data reporting as meaningful checks on the sensitivity of our findings, but include them only based on the principle that the results from all specifications explored should be reported.

4.3 Threats to Identification and Robustness Checks

This subsection examines a number of assumptions necessary to interpret the estimates from the first-stage differences-in-differences and IV analyses as causal. Pertaining to the difference-in-differences analyses, we discuss the parallel trends assumption, stable unit treatment value assumption, and the possibility of bias arising from the staggered adoption of treatment. Regarding the IV analyses, we provide evidence that the removal of safety inspections was unlikely to substantially impact household budgets and that the removals did not lead to statistically significant changes in the composition of vehicles in terms of their fuel efficiency.

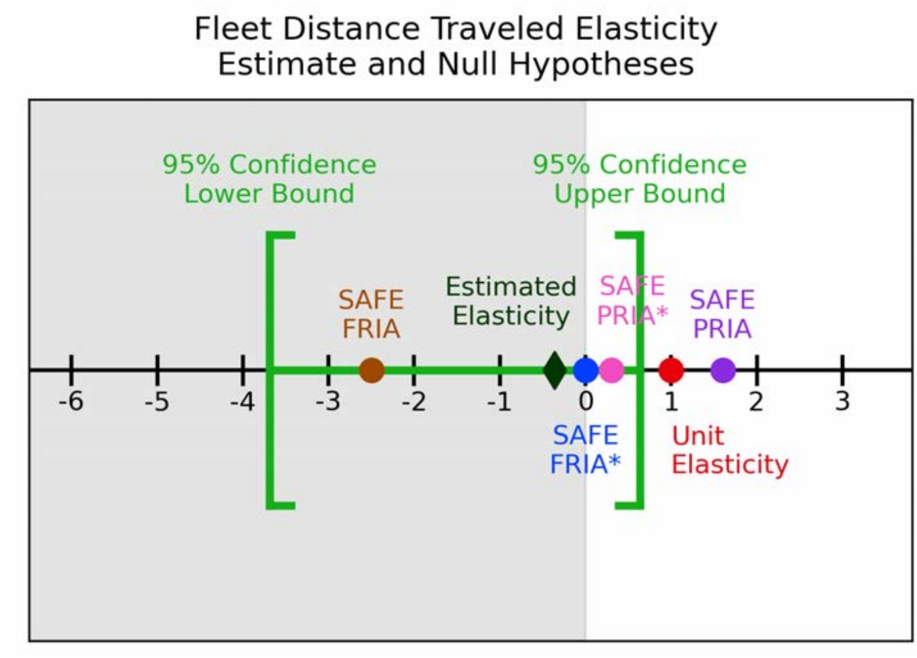
4.3.1 Event Study

The parallel trends assumption is key to interpreting difference-in-differences estimates as causal. Appendix Section B.2 documents that the residuals after controlling for the variables in the specifications listed in Section 3.4 do not exhibit any clear trends prior to the removal of safety inspections in treatment states. As an additional check, we estimate event study specifications to determine whether we can identify pre-existing differences in trends in registrations in the years prior to the removal of safety inspections.

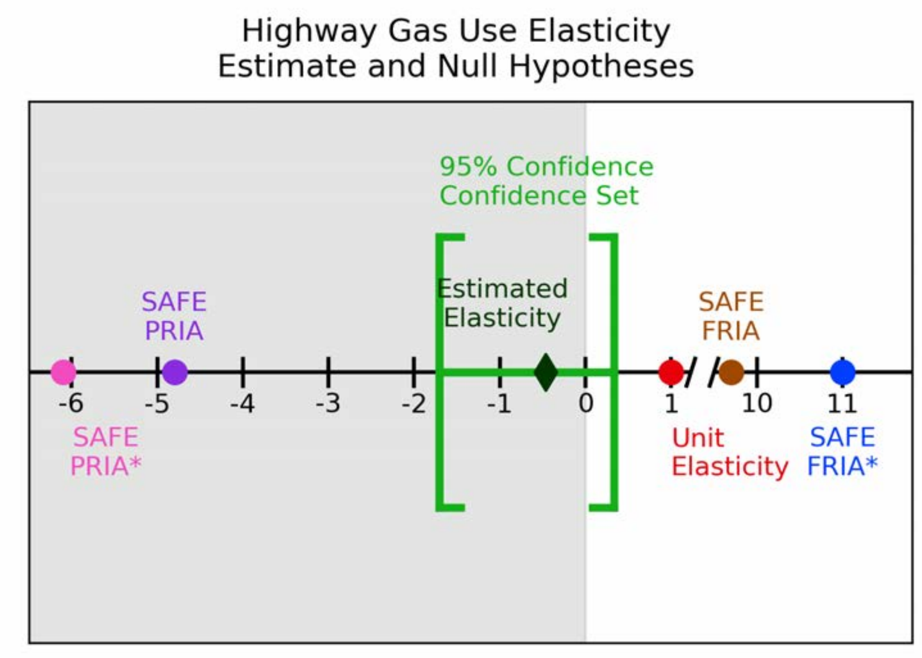
The event study framework has a similar structure to Equation (2), except that the policy indicator is replaced by variables s_{it}^{τ} that indicate whether state i in year t is τ years away from the removal of safety inspections. Formally, our event study framework is:

$$\log(r_{it}) = \psi_i^{\mathrm{R}} + \gamma_t^{\mathrm{R}} + \xi_i^{\mathrm{R}} \cdot t + \sum_{\tau} \eta_\tau s_{it}^{\tau} + \mathbf{x}_{it}^{\mathrm{R}} \boldsymbol{\alpha} + \epsilon_{it} \tag{5}$$

Figure 4: Estimated elasticities with robust 95% confidence sets and relevant null hypotheses



(a) Comparison of estimated fleet-size elasticity of fleet travel distance alongside null hypotheses informed by the literature.



(b) Comparison of estimated fleet-size elasticity of gasoline consumption alongside null hypotheses informed by the literature.

Notes: The top panel of this figure plots the estimated fleet-size elasticity of vehicle-miles travelled and 95% confidence set presented in Column (2) of Table 5. The bottom panel of this figure plots the estimated fleet-size elasticity of gasoline consumption and 95% confidence set presented in Column (2) of Table 6. For both panels, we also plot relevant null hypotheses based on the assumptions made in prior academic work and policy analyses. The points labeled "PRIA" and "FRIA" refer to the null hypotheses that represent NHTSA and USEPA, 2018a; NHTSA and USEPA, 2020 including the rebound effect. Points labeled "PRIA*" and "FRIA*" refer to the null hypotheses that represent NHTSA and USEPA, 2018a; NHTSA and USEPA, 2020 excluding the rebound effect.

We estimate effects for each of the five years before and after the removal of safety inspections. Results from our preferred specification are presented in Figure 5.[12] The event study figures document that all of the estimated effects for years prior to the event are not statistically distinguishable from zero. This indicates that our difference-in-differences estimates are likely not artifacts of differential trends in vehicle registrations across treatment and control states prior to the removal of safety inspections in treatment states.

The event study also shows a statistically significant increase in registrations beginning a few years after the removal of safety inspections. This delay in effect is consistent with inspections largely being required on either an annual or biennial basis depending on the state (U.S. GAO, 2015; NHTSA, 1989). The magnitudes of the post-treatment effects in this event study are also comparable to our difference-in-differences estimates in Table 4.

4.3.2 Stable Unit Treatment Value Assumption

Another assumption necessary for identification in difference-in-differences models is the stable unit treatment value assumption (Lechner, 2010, p.176). This assumption requires that the removal of safety inspections in one state does not impact registrations in other states. In order to investigate whether this assumption holds in our setting, we estimate an event study model that measures the impact on registrations in one state of the removal of safety inspections in neighboring states. The model estimated is shown in Equation (6):

$$\log(r_{iy}) = \phi_i^{\mathrm{R}} + \gamma_y^{\mathrm{R}} + \xi_i^{\mathrm{R}} \cdot y + \sum_{\tau} (\eta_\tau s_{iy}^\tau + \psi_\tau a_{iy}^\tau) + \boldsymbol{\alpha}' \mathbf{x}_{iy}^{\mathrm{R}} + \epsilon_{iy} \tag{6}$$

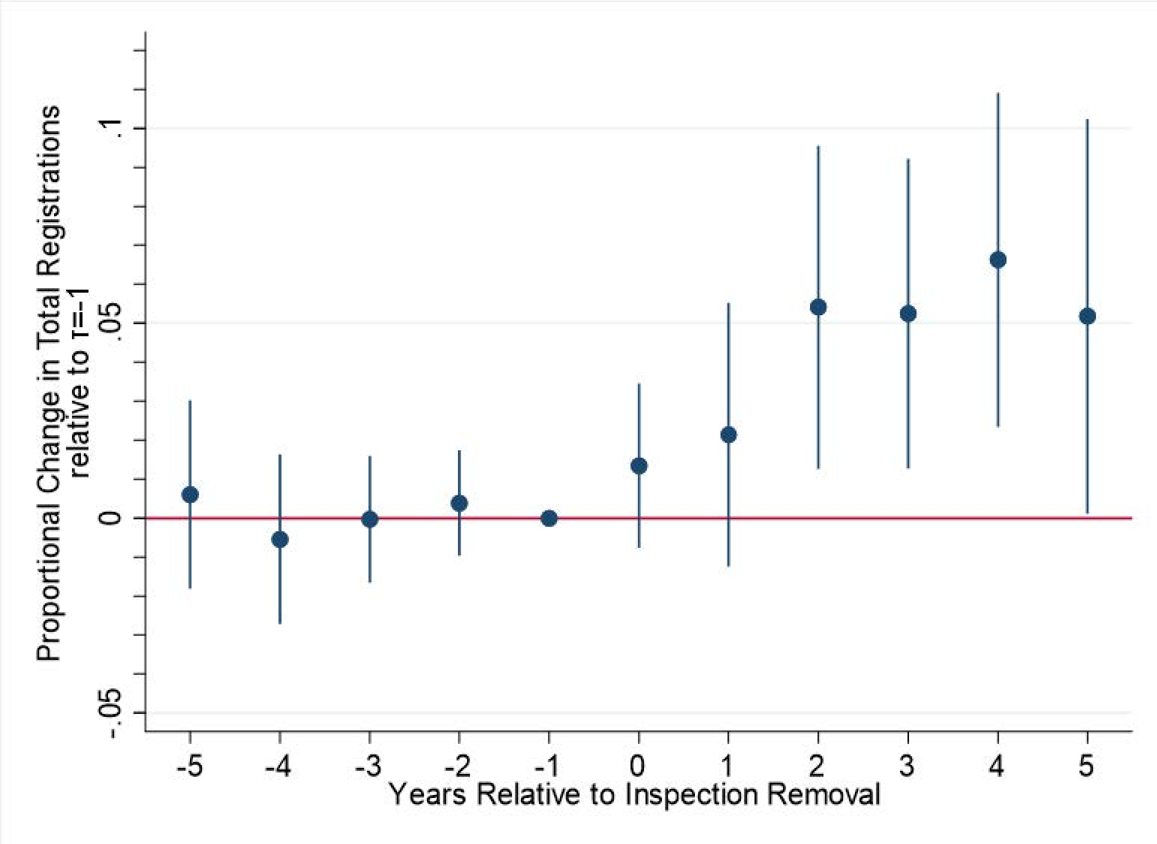
We include the same set of fixed effects and control variables as in the event study specification in Equation (5). The only addition is the indicators a_{iy}^{ν}, which denote the number of states neighboring state i that are τ years from treatment in year y.[13]

The results are shown in Figure 6. The estimated effects of removals of safety inspections in neighboring states are small and not statistically significant. This allows us to conclude that the removal of safety inspections in neighboring states does not

[12]Event study results across all specifications can be found in Appendix B.3.

[13]This model is based on the multiple event study model proposed by Sandler and Sandler (2014). We define "neighboring" states using data provided by U.S. Census Bureau (2018*b*).

Figure 5: Event Study Estimates of the Impact of the Removal of Safety Inspections on Log Number of Vehicle Registrations



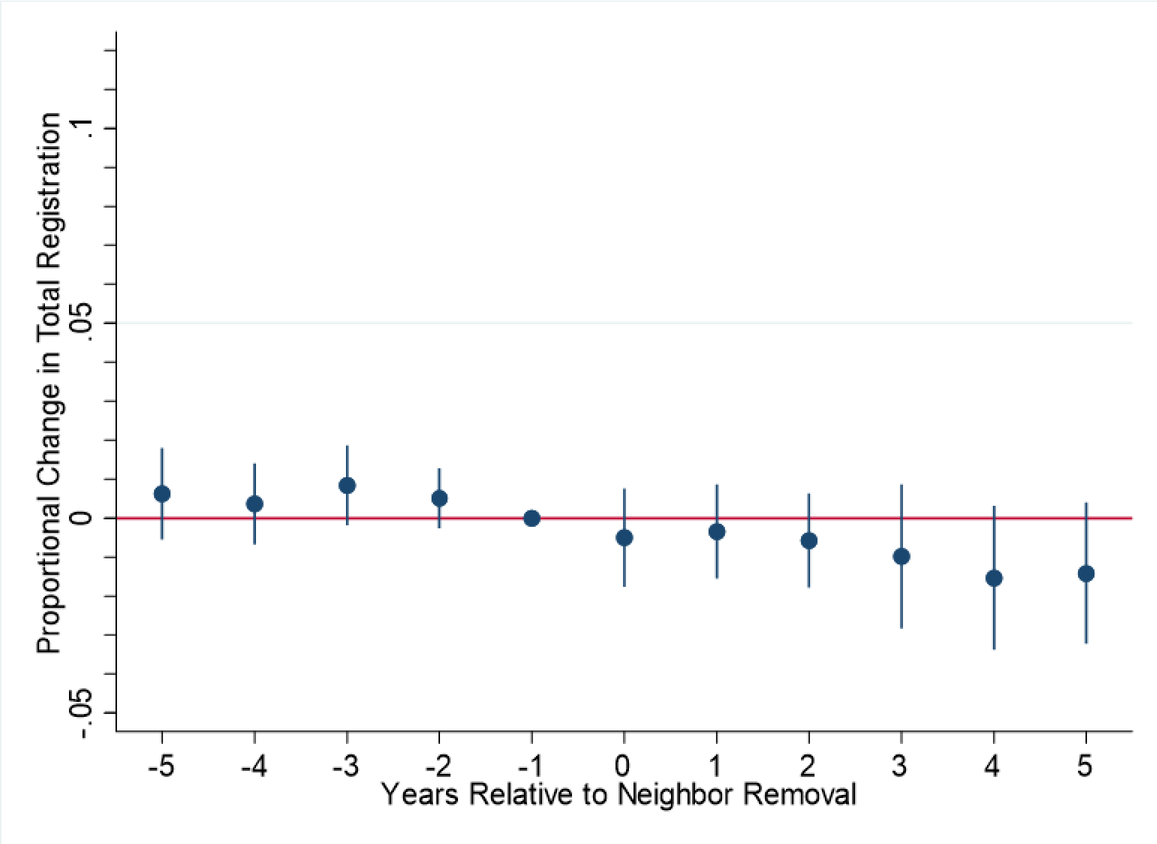
Notes: This figure plots event study estimates of the impact of the removal of safety inspections on the log of vehicle registrations. We estimate separate effects by the years relative to the removal of safety inspections. We also include 95% confidence intervals based on standard errors clustered by state. The unit of observation for this event study regression is state-year. The effect for the year before the event is normalized to zero.

have a statistically significant effect on registrations.

4.3.3 Goodman-Bacon Decomposition

Recent literature has raised concerns that estimates of the average treatment effect on the treated from difference-in-differences models may be biased when the timing of treatment is staggered (e.g., Borusyak, Jaravel and Spiess (2021); De Chaisemartin and d'Haultfoeuille (2020); Goodman-Bacon (2021)). In order to assess whether any particular comparisons across states treated earlier versus later are driving our first-stage difference-in-differences estimate, we decompose the estimate as specified in Goodman-Bacon (2021). Comfortingly, estimated effects based on comparisons across states treated earlier versus later receive relatively small weights in the overall estimate, and none of these timing-based estimates are substantial enough in magnitude to

Figure 6: Effects on log number of registrations of removals of safety inspections in neighboring states



Notes: This figure plots event study estimates of the impact of the removal of safety inspections in *neighboring* states on the log of vehicle registrations. We estimate separate effects by the years relative to the removal of safety inspections. We also include 95% confidence intervals based on standard errors clustered by state. The unit of observation for this event study regression is state-year. The effect for the year before the event is normalized to zero.

unduly influence the overall difference-in-differences estimate. Further description of this analysis is presented in Appendix Section B.12.

4.3.4 Models Without State-Specific Linear Time Trends

In this section, we report results from re-estimating our first-stage difference-in-differences model without including state-specific linear time trends. Without these time trends, the difference-in-differences estimates generally become smaller and less precise. However, the event study coefficients are largely similar with the exception of the years well before the event, which are influenced heavily by the small subset of states treated late in the sample period.

Since the pre-treatment event study estimates remain statistically indistinguishable from zero, and the post-treatment estimates are not sensitive to the inclusion of state-specific linear time trends, the inclusion of these trends is unlikely to be driving the empirical identification of our estimates. Instead, state-specific time trends seem to be useful primarily in increasing precision, and for controlling for longer-term differences across states treated very early versus very late in the sample period. Further details are provided in Appendix Section B.4.

4.3.5 Leave-one-out Analysis

As recommended by Young (2020, p.39), we perform leave-one-out analysis to assess the sensitivity of our estimates to idiosyncratic trends in specific states. We re-estimate our robust IV models under our preferred specification after removing a single state. We do this for all 50 states and DC. Across all subsamples, we can reject null hypotheses of one or greater for the fleet-size elasticity of travel distance and all relevant null hypotheses for the fleet-size elasticity of gasoline consumption at at least the 10% level.

We perform the same analysis for our first-stage difference-in-differences model. The results indicate that the estimates are statistically significant at at least the 10% level regardless of which state is left out. Full results are available in Appendix Section B.5.

4.3.6 Effect of Safety Inspections on Household Budgets

Necessary to interpret our IV estimates as causal is the assumption that our instrument (the removal of safety inspections) does not affect fleet travel distance except through vehicle registrations. One threat to identification would be if the instrument affected household fuel budgets. Household budgets may be larger due to reduced expenditures on inspection-related repairs or may be smaller due to increases in traffic crashes.

For inspection-related repairs, data from Pennsylvania state safety inspections records show that inspection costs are largely less than $30 (see Figure 3 and Table 2). If households do not need to pay inspection costs, roughly 3-5 percent of the resulting savings might be reallocated to vehicle fuel based on average expenditure data by type from 2013-2018 from the BLS Consumer Expenditure Survey (U.S. Bureau of Labor Statistics, 2019). This would result in approximately $1-$1.50 more spent on fuel per household, which would further be spread out across an average of 1.9

vehicles in the household (U.S. Bureau of Labor Statistics, 2019). Due to inspection-caused repair costs tending to be low on average (see Table 2), the low cost of getting an inspection (U.S. Government Accountability Office, 2015; U.S. National Highway Traffic Safety Administration, 1989), and the long time horizon between inspections (U.S. Government Accountability Office, 2015; U.S. National Highway Traffic Safety Administration, 1989), inspection and repair costs are unlikely to significantly impact household budgets.

In addition, previous work has largely found that safety inspections have only limited impacts on traffic safety outcomes such as crashes and fatalities (U.S. GAO, 2015; NHTSA, 1989; Garbacz and Kelly, 1987; Hoagland and Woolley, 2018; Keeler, 1994; Merrell, Poitras and Sutter, 1999). Therefore, changes in household budgets due to policy-induced changes in traffic safety are also unlikely to be cause for concern.

4.3.7 Impacts on the Composition of Vehicles

The removal of safety inspections may have changed both the number of vehicles on the road and the composition of these vehicles. Our analysis focuses on the impact of the policy change on vehicle registrations, fleet travel distance, and aggregate gasoline usage. These outcomes could be affected by changes in the composition of the vehicle fleet due to differential scrappage of vehicles of different fuel efficiencies.

In Appendix B.11, we perform a statistical test of the null hypothesis that the removal of safety inspections did not affect the aggregate fuel efficiency of the fleet. Namely, we test the hypothesis that the effects of the removal of safety inspections on fleet travel distance and gasoline consumption are equal. We find that the estimated effects for fleet travel distance and gasoline consumption are quite similar in magnitude and we fail to reject the null hypothesis that these effects are the same.

Our evidence thus indicates that the removal of safety inspections does not induce compositional changes in the fleet sufficient to cause large changes in aggregate fuel efficiency. However, it is possible that significant compositional changes are induced by other policies examined in prior studies, including those used to construct our null hypotheses. Consequently, as discussed in Section 3.3.1, we also consider null hypotheses adjusted to represent the fleet-size elasticities of travel distance and gasoline consumption with no compositional changes that impact fuel efficiency (i.e., no rebound effect).[14]

5 Implications for Automotive Policy Externalities

Automobile use comes with a host of negative externalities, including pollution emissions, traffic fatalities, and congestion (Parry, Walls and Harrington, 2007). In this section, we use our estimates of the fleet-size elasticities of travel distance and gasoline consumption to calculate the aggregate externality costs of removing safety inspections.

To do this, we construct counterfactual scenarios in which states that removed their safety inspections instead kept these programs in place. Namely, our preferred estimates indicate that removing safety inspections led to a 3.7% increase in fleet size on average, which we apply to all treated states. We translate this decrease in fleet size (from counterfactually keeping inspections in place) to changes in fleet travel distance and gasoline consumption using a range of elasticity estimates. We calculate the aggregate external costs resulting from these policy-induced changes in fleet travel distance and gasoline consumption using estimates of the marginal external costs associated with vehicle travel and fuel consumption from Parry, Walls and Harrington (2007, Table 2).[15,16]

Figure 7 displays the back-of-the-envelope calculation of the annual average per-state externality cost of removing safety inspections in the United States over our sample period. This figure shows that assuming a fleet-size elasticity of fleet travel distance equal to 1 may result in over-estimates of the externality costs of removing safety inspections of anywhere between 90 million dollars to several hundred million dollars per state-year (2021 USD). This calculation highlights that different assumptions regarding how a policy impacts fleet travel and gasoline consumption through

[14]The Unit Elasticity Null Hypothesis constructed to represent Alberini, Harrington and McConnell (1998) does not include fuel-efficiency compositional effects. Derivations of adjusted null hypotheses for NHTSA and USEPA, 2018a and NHTSA and USEPA, 2020 can be found in Appendix B.10.

[15]All travel externality costs were adjusted from 2005 USD to 2021 USD using the "All Items CPI-U" from the U.S. Bureau of Labor Statistics (n.d.) except for the externalities associated with gasoline consumption (as designated by Parry, Walls and Harrington (2007)), which were adjusted to 2021 USD using a GDP deflator (U.S. Bureau of Economic Analysis, n.d.). Note that

[16]A limitation of our back-of-the-envelope calculation is that externalities might not be constant over space (e.g., congestion costs in urban versus rural areas) (U.S. Federal Highway Administration, 2000) or over time (e.g., as pollutant emission standards have grown more stringent) (National Research Council, 2001, Table 1-2).

the channel of scrappage can result in dramatically different estimates of the aggregate externality costs of the policy. Our results suggest that regulatory impact analyses should consider a range of scrappage elasticities of fleet travel and gasoline consumption, including values far lower than 1.

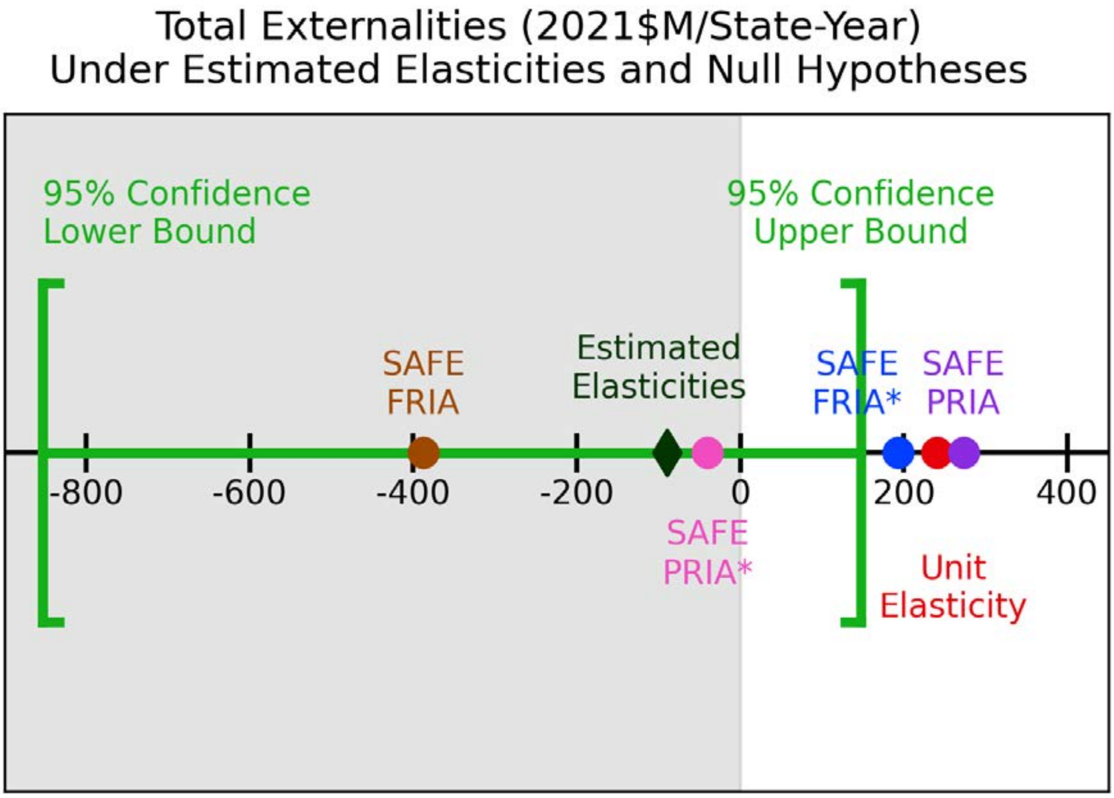
Our findings emphasize the importance of incorporating diminishing marginal increases in vehicle travel and gasoline consumption from policy-induced increases in fleet size, as in Jacobsen and van Benthem (2015), for example. Our findings also highlight that those crafting transportation models should be cognizant of how scrappage affects both travel and gasoline consumption. For example, the assumptions in the analysis of the SAFE FRIA imply a "no-rebound" fleet-size elasticity of travel closest to our point estimate. However, this same model implies fleet-size elasticities of gasoline consumption far outside of our 95% confidence sets. Overall, the assumptions in the analysis of the SAFE FRIA, excluding rebound, yield estimated externality costs similar to those from assuming fleet-size elasticities of 1 and larger than our empirical estimates suggest.

6 Conclusions

This paper leverages policy-induced variation in vehicle scrappage due to the removal of state-level safety inspection programs to estimate the fleet-size elasticities of fleet travel distance and of gasoline consumption. We estimate that the removal of safety inspection programs leads to a 3-4% increase in vehicle registrations on average. Further, we provide the first empirical estimates of the fleet-size elasticity of fleet travel distance and gasoline consumption and compare these estimates to a variety of assumptions made in previous analyses. We find that we can reject assumptions used in some prior analyses, including the assumption of unit elasticity.

Given our findings, we recommend that cost-benefit analyses of transportation policies consider a range of potential assumptions regarding how policy-induced changes in fleet size affect fleet travel distance, fleet gasoline consumption, and associated externalities. On the low end, our analysis cannot reject the possibility that policy-induced delays in vehicle scrappage have no effect or even a negative effect on fleet travel and gasoline consumption on average. On the high end, we are able to reject elasticities of travel larger than $\beta = 0.64$ and elasticities of gasoline consumption larger than

Figure 7: Annual average state-level externality costs from removing safety inspections



Notes: This figure presents the annual average per-state externality cost of removing safety inspections among treated states. For this analysis, we utilize our preferred estimate that removing safety inspections led to a 3.7% increase in fleet size, which we port into changes in fleet travel distance and gasoline consumption using a range of elasticities relating fleet size to these two outcomes. Estimates of the external costs of vehicle usage and the associated emissions from burning gasoline are from Parry, Walls and Harrington (2007, Table 2). All magnitudes are presented in 2021 USD/state-year. The points labeled "PRIA" and "FRIA" refer to the null hypotheses that represent NHTSA and USEPA, 2018a; NHTSA and USEPA, 2020 including the rebound effect. Points labeled "PRIA*" and "FRIA*" refer to the null hypotheses that represent NHTSA and USEPA, 2018a; NHTSA and USEPA, 2020 excluding the rebound effect. Similar assumptions to those made in the SAFE FRIA were utilized when analyzing the recent CAFE FRIA (NHTSA, 2022, p.71).

$\gamma = 0.33$. This is consistent with diminishing marginal fleet travel with respect to fleet size, which can be caused by vehicle scrappage inducing shifts of travel to other vehicles in the fleet or other modes of transportation.

Our findings also speak to the indirect implications of vintage-differentiated regulations more broadly (Stavins, 2006). For example, U.S. coal-fired plants built before 1972 are exempt from stricter environmental regulation (List, Millimet and McHone, 2004; Maloney and Brady, 1988; Nelson, Tietenberg and Donihue, 1993), older homes are exempt from increases in the stringency of building codes (Levinson, 2016), and older vehicles are sometimes exempt from emissions inspections requirements (Gruenspecht, 1982*b*). Some of the benefits from stricter air pollution regulation, building codes, and emissions inspections may be lost by exempting existing infrastructure from these regulations. These exemptions could increase the externality costs of the policy both by enlarging the size of the "fleet" (e.g., more coal-fired power plants, homes, and vehicles in the aforementioned examples) and by changing the composition of the fleet. Our findings highlight that the externality implications of vintage-differentiated regulation hinge not only on how the policy affects fleet size but also on how policy-induced changes in fleet size affect technology *use*. Empirical evidence on both elasticities is essential in order to assess the costs and benefits of vintage-differentiated regulations.

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Online Appendix

Externalities of Policy-Induced Scrappage

The Case of Automotive Regulations

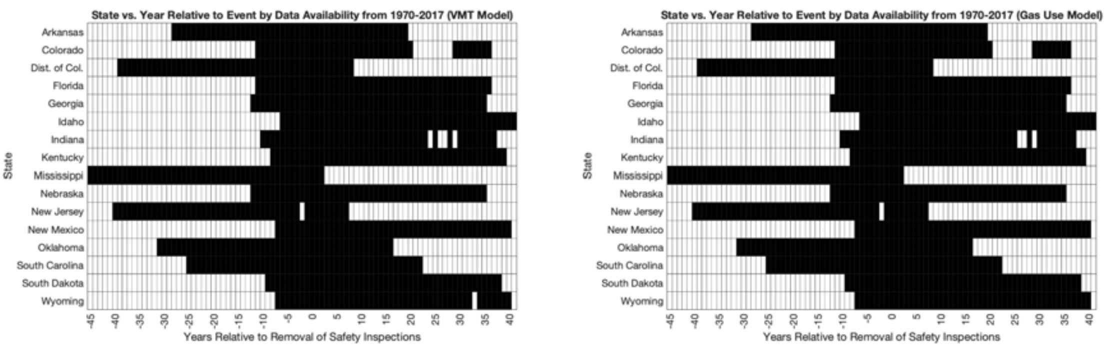
by Connor R. Forsythe, Akshaya Jha, Jeremy J. Michalek, and Kate S. Whitefoot

A Data Appendix

A.1 Data Availability

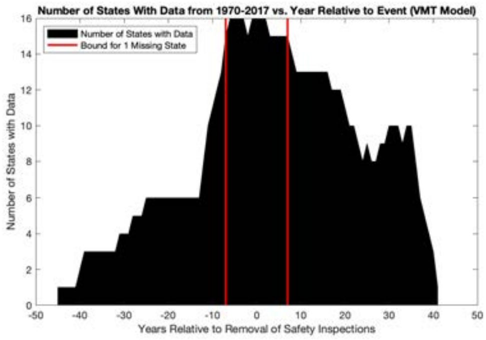
In Figure A.1, we see the data availability for treated states surrounding the removal of safety inspection programs. Figure A.2 presents the number of states with data available relative to the year in which the removal occurs. These figures show that most data for the treated states exist roughly ±10 years surrounding the removal of safety inspections. For this reason, our event study specifications focus on windows either 6 years before and after the event or 10 years before and after the event depending on specification.

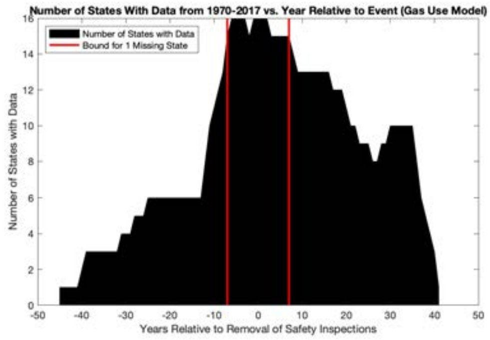
Figure A.1: Data availability for treated states relative to event year.



Notes: This figure plots the event years of data available for treated states that removed safety inspections at any point during our 1970-2017 sample period.

Figure A.2: Number of treated states with data relative to event year.





Notes: This figure plots the number of treated states that removed safety inspections at any point during our 1970-2017 sample period by event year.

A.2 Repeated Outcome Variable Data

For several data points of state-wide vehicle registrations, there were footnotes in the associated year noting that all or part of the fleet size was based on past years (U.S. Federal Highway Administration, n.d.). These data points have been removed from the analysis to prevent the use of any artificial variation.

Table A.1: States and years with repeated registration data

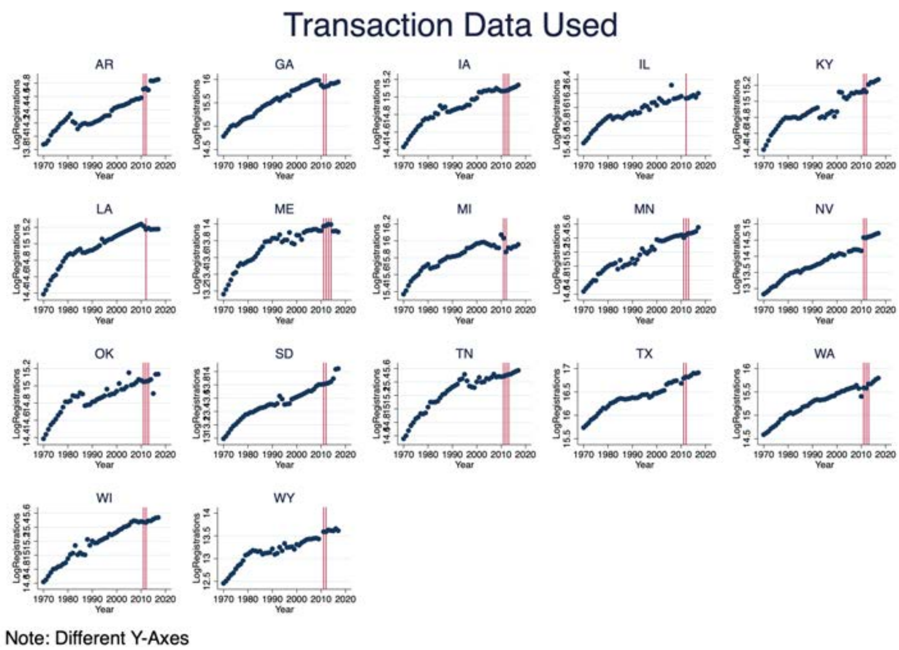
State	**Years Removed**	**Data Repeated**
CO	2006	Registration
IN	2006, 2007, 2009	Registration
MT	2005	Registration
NH	2008	Registration
TX	2009	Registration
IL	2011	Registration
NH	2012	Registration
NY	2012	Registration
MO	2003	VMT
IN	2004, 2009	VMT
NH	2004	VMT
NV	2004	VMT
NY	2005	VMT
AZ	2009	VMT
WY	2010	VMT
RI	2003, 2005	Gas Use

Notes: This table presents the states and years of data with repeated data for each of our three key outcome variables: number of vehicle registrations, vehicle-miles travelled, and gasoline usage.

A.3 Transaction Data Used

Data from certain states have noted a shift to the use of transaction data to provide an estimate of vehicle registrations (U.S. Federal Highway Administration, n.d.). States and years (depicted by red vertical lines) are shown in Figure A.3. For certain states, such as Nevada, this change seems to be persistent; for other states, such as Michigan, it appears by inspection that this was a temporary change. To control for most of these shifts, we include a dummy variable in our regression for all years after the change. For Michigan and Maine, we have separate dummy variables during the period of note and after.

Figure A.3: State-years reporting vehicles registrations using transaction data



Notes: This figure presents the years, denoted by the vertical red lines, that different states started and stopped reporting vehicle registrations using data on vehicle transactions rather than counting registrations directly.

A.4 Data Source Controls

Several states document changes in vehicle registration data sources and methods, such as changing from reporting annual registration data directly in early years to reporting annual registration estimates based on transaction data in later years. These documented changes sometimes coincide with step changes in the magnitude of reported data. Additionally, we observe some step changes in the reported data that do not coincide with documented changes in data sources or methods. Inquiries with individual state agencies were not able to resolve the source of these step changes.

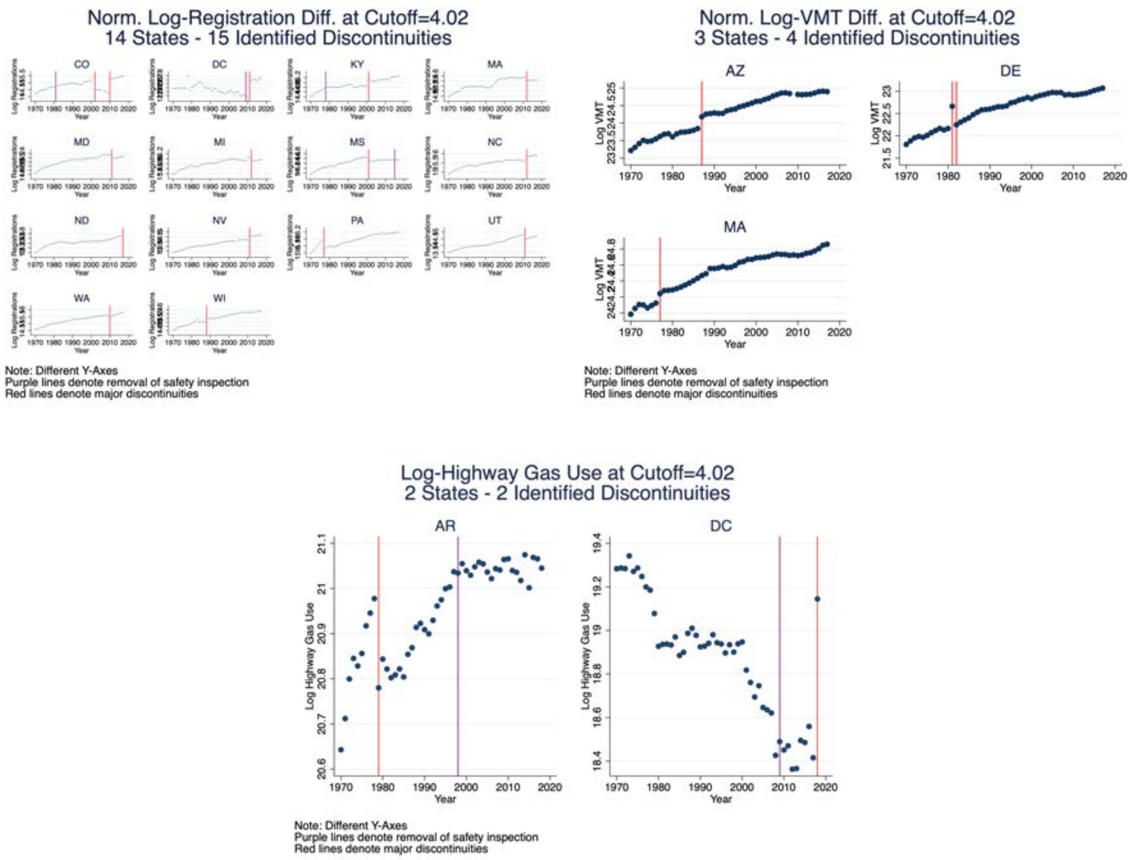
It is also the case that travel reporting has changed over time (Small and Van Dender, 2007, p.43).[q] The same is true for fuel reporting.[r] As a robustness check, we consider additional specifications that control for step changes in the reported data, which may potentially be due to changes in data sources or calculation methods. Specifically, we include controls for all step changes in the outcome variable larger than a given threshold using the absolute value of the normalized first difference in the relevant outcome (e.g., log registrations, fleet distance traveled, or gasoline consumption).

Several states exhibit large points of discontinuity in their registration and VMT data. However, several of these points of discontinuity have no documentation describing the reason for their existence. In order to identify and control for these large shifts in outcome variables, we first calculate state-level normalized differences in log fleet size, log fleet distance travelled, and log fleet gasoline use. The normalized difference in log fleet size for Colorado from 2002 and 2010 (a period where a prominent shift in data is present) are -4.029 and 4.819 respectively. As these points are exemplary of large shifts in vehicle registrations, we use 4.02 as a cutoff for identifying trend discontinuities across all outcome variables. States exhibiting such trend discontinuities are plotted in Figure A.4. In order to see how sensitive our results are to this value, we additionally use a lower value of 2.93, which is based on a discontinuity identified by inspection in Kentucky. Each trend discontinuity is controlled through a control dummy variable on each domain bounded by a noted discontinuity.

[q]This is evidenced by, for example, FHWA footnotes in the VM-2 table where Illinois changed how they estimated travel in 1996 and Texas changed their "data system" in 2014 (U.S. Federal Highway Administration, 2014).

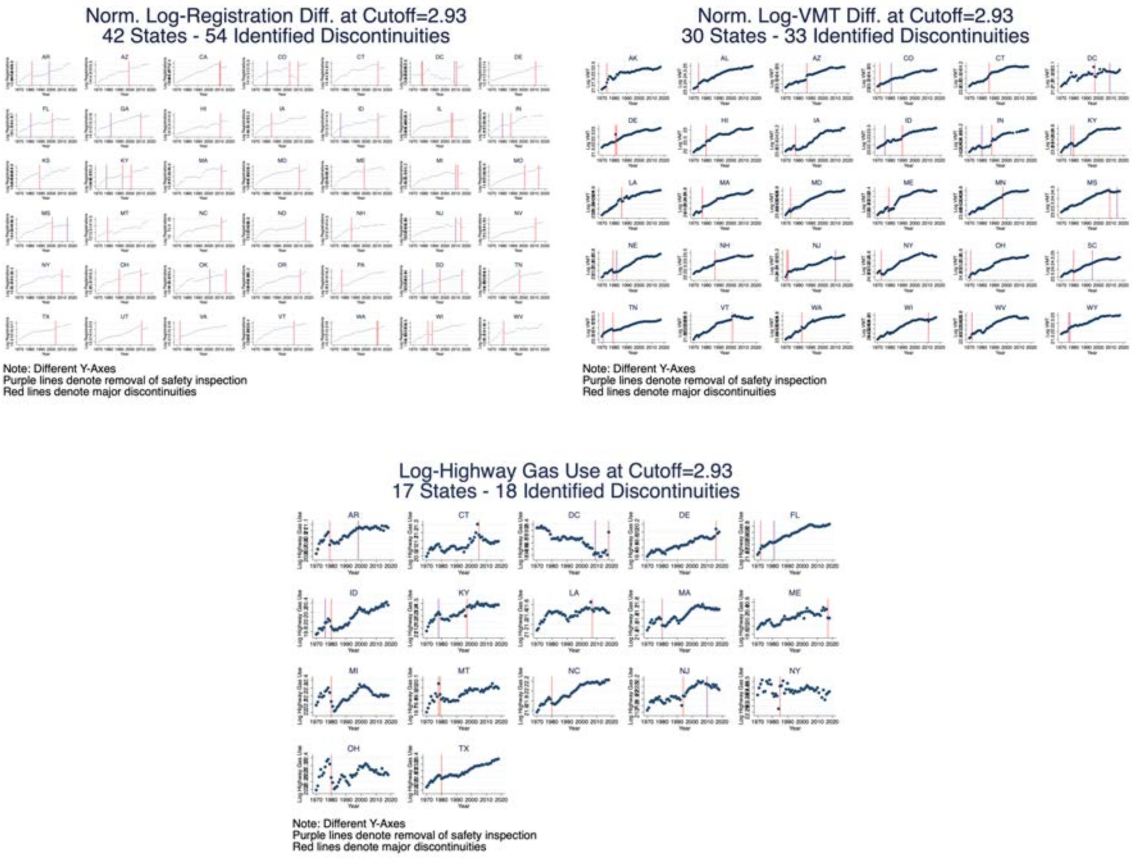
[r]FHWA notes that changes in data preparation for gasoline use may limit comparability across certain states over time (U.S. Federal Highway Administration, n.d.).

Figure A.4: Identified discontinuities under the "Large Undocumented DSC" specification



Notes: This figure presents the discontinuities in log registrations (top left panel), log VMT (top right panel) and log gasoline usage (bottom panel) identified based on a "large" cutoff for the absolute value of the normalized first difference of the outcome of 4.02.

Figure A.5: Identified discontinuities under the "Small Undocumented DSCs" specification



Notes: This figure presents the discontinuities in log registrations (top left panel), log VMT (top right panel) and log gasoline usage (bottom panel) identified based on a "small" cutoff for the absolute value of the normalized first difference of the outcome of 2.93.

B Additional Robustness Analyses

B.1 Full Results

B.1.1 Difference in Differences

Results across specifications for our first-stage difference-in-differences framework can be found in Table B.1. When additional levels of Data Source Controls are considered, the first stage point estimates move closer to zero while maintaining a similar level of statistical uncertainty. Nevertheless, we can reject the null hypothesis that there is no effect of removing safety inspections on log vehicle registrations across specifications at at least the 10% level.

Table B.1: Effect of the Removal of Safety Inspections on Log Number of Vehicle Registrations

	Dep. Var.: Log Number of Vehicle Registrations			
	(1)	(2)	(3)	(4)
Removal of Safety Inspections	0.042**	0.037**	0.028*	0.027*
	(0.017)	(0.015)	(0.016)	(0.016)
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Linear Time Trends	Y	Y	Y	Y
Common Controls	Y	Y	Y	Y
Robust F-Stat	5.766	5.874	3.216	3.045
Number of Obs.	2,424	2,424	2,424	2,424
R^2	0.997	0.997	0.998	0.998

Notes: This table presents difference-in-differences estimates of the impact of the removal of safety inspections on the log of vehicle registrations. The unit of observation is state-year. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. Standard errors, reported in parentheses, are clustered by state. * p<0.1, ** p<0.05, *** p<0.01

B.1.2 Instrumental Variables

Results across specifications for our instrumental variable framework can be found in Tables B.2 and B.3 for fleet travel and fleet gasoline consumption respectively. In Columns (3) and (4) of Tables B.2 and B.3, we include additional controls for suspected step changes in the vehicle registrations data. In Column (1), we remove all controls related to the vehicle registrations data. The point estimates remain similar across all specifications. However, in Columns (3) and (4), we lose the statistical power to reject some of the relevant null hypotheses.[s] This is unsurprising: in Specifications 3 and 4, we add a large number of additional covariates to the model to control for potential changes in data requirements. Given the similarity in point estimates across specifications, the evidence in Tables B.2 and B.3 suggests that the assumptions made in previous academic work and policy analyses imply scrappage-induced changes in fleet travel and gasoline consumption that are too large.

B.2 Parallel Trends in Log Registrations

Figure B.1 plots observed log-registration values minus predicted values for each of the four difference-in-differences specifications discussed in detail in Section 3.4. We put the residuals for never treated, not yet treated, and treated observations in gray, red, and green respectively. Although the residuals are noisy, there appears to be no particularly strong trends present that would indicate that the parallel trends or the stable unit treatment value assumptions are violated.

B.3 Event study results based on small and large estimation windows

Here, we show the results from event studies of the impact of the removal of safety inspections on log vehicle registrations for our four specifications for two event windows: $\tau \in \{\leq -6, -5, \ldots, 5, \geq 6\}$ ("small" window) and $\tau \in \{\leq -11, -10, \ldots, 10, \geq 11\}$ ("large" window). The coefficient associated with $\tau = -1$ is not included to establish the baseline as the year prior to the event.

[s]Specifically, we fail to reject the null hypothesis that $\beta = -2.5$ in Columns 3-4 and all of the null hypotheses pertaining to β in Column 4. We fail to reject the null hypothesis that $\gamma = -4.8$ in Columns 3-4. We can reject the relevant null hypotheses at at least the 10% level across all of the remaining specifications.

Table B.2: IV Estimates of the Effect of Log Number of Registrations on Log VMT

Panel A: IV Estimates and Robust 95% Confidence Intervals				
	Dep. Var.: Log Vehicle-Miles Travelled			
	(1)	(2)	(3)	(4)
Log Registrations	-0.326	-0.36	-0.40	-0.01
	[-3.49, 0.56]	[-3.69, 0.64]	(-∞, 2.20]; [3.38, ∞)	(-∞, ∞)
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Time Trends	Y	Y	Y	Y
Common Controls	Y	Y	Y	Y
Number of Obs.	2,424	2,424	2,424	2,424
Panel B: Anderson-Rubin Test Statistics and P-Values				
	$H_0 : \beta = 1.6$			
AR Stat	7.351***	7.086***	3.652*	2.059
p-value	0.007	0.008	0.056	0.151
	$H_0 : \beta = 1$			
AR Stat	6.168**	5.631**	2.992*	1.324
p-value	0.013	0.018	0.084	0.250
	$H_0 : \beta = 0.3$			
AR Stat	2.098	1.867	1.151	0.194
p-value	0.148	0.172	0.283	0.660
	$H_0 : \beta = 0$			
AR Stat	0.540	0.535	0.378	0.000
p-value	0.462	0.464	0.538	0.986
	$H_0 : \beta = -2.5$			
AR Stat	3.166*	2.995*	1.734	2.347
p-value	0.075	0.084	0.188	0.125

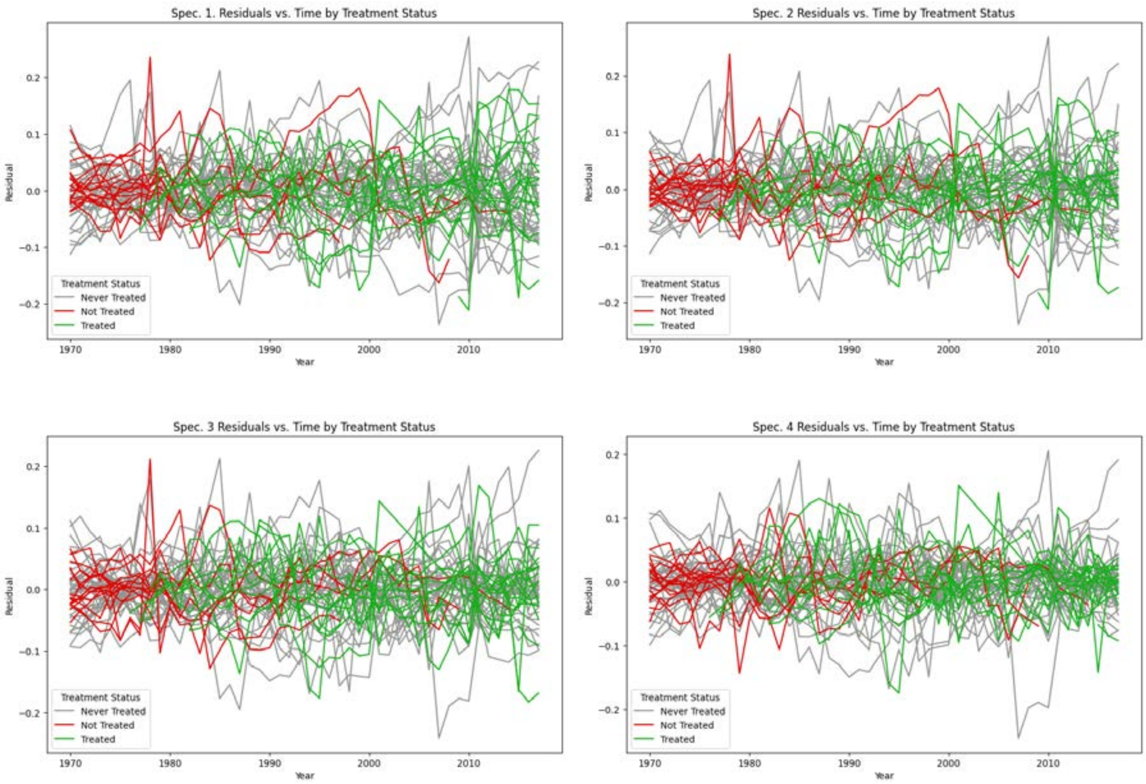
Notes: The top panel of this table presents IV estimates and 95% confidence sets robust to weak instruments of the relationship between log vehicle-miles travelled (VMT) and log vehicle registrations. The bottom panel presents Anderson-Rubin chi-squared statistics along with p-values for different null hypotheses pertaining to the parameter estimated in the top panel. The unit of observation is state-year. All regressions include state fixed effects, year fixed effects and state-specific linear time trends. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. 95% confidence sets are based on standard errors clustered by state. * p<0.1, ** p<0.05, *** p<0.01

Table B.3: IV Estimates of the Effect of Log Number of Registrations on Log Highway Gas Consumption

Panel A: IV Estimates and Robust 95% Confidence Intervals				
	Dep. Var.: Log Gasoline Consumption			
	(1)	(2)	(3)	(4)
Log Registrations	-0.420	-0.47	-0.67	-0.64
	[-1.75, 0.27]	[-1.71, 0.33]	(-∞, 0.23]; [8.59, ∞)	(-∞, 0.32]; [2.85, ∞)
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Time Trends	Y	Y	Y	Y
Common Controls	Y	Y	Y	Y
Number of Obs.	2,429	2,429	2,429	2,429
Panel B: Anderson-Rubin Test Statistics and P-Values				
	$H_0 : \gamma = 11$			
AR Stat	5.877**	6.042**	3.718*	2.806*
p-value	0.015	0.014	0.054	0.094
	$H_0 : \gamma = 9.7$			
AR Stat	5.892**	6.052**	3.777*	2.856*
p-value	0.015	0.014	0.052	0.091
	$H_0 : \gamma = 1$			
AR Stat	5.707**	5.462**	6.062**	5.266**
p-value	0.017	0.019	0.014	0.022
	$H_0 : \gamma = -4.8$			
AR Stat	5.237**	5.479**	2.284	1.673
p-value	0.022	0.019	0.131	0.196
	$H_0 : \gamma = -6.1$			
AR Stat	5.365**	5.606**	2.486	1.825
p-value	0.021	0.018	0.115	0.177

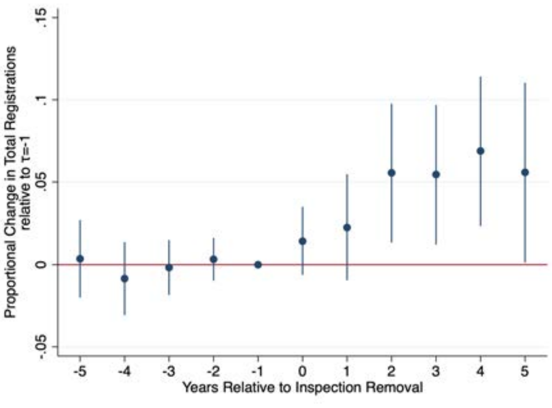
Notes: The top panel of this table presents IV estimates and 95% confidence sets robust to weak instruments of the relationship between log gasoline consumption and log vehicle registrations. The bottom panel presents Anderson-Rubin chi-squared statistics along with p-values for different null hypotheses pertaining to the parameter estimated in the top panel. The unit of observation is state-year. All regressions include state fixed effects, year fixed effects and state-specific linear time trends. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. 95% confidence sets are based on standard errors clustered by state. * p<0.1, ** p<0.05, *** p<0.01

Figure B.1: Residualized log number of registrations

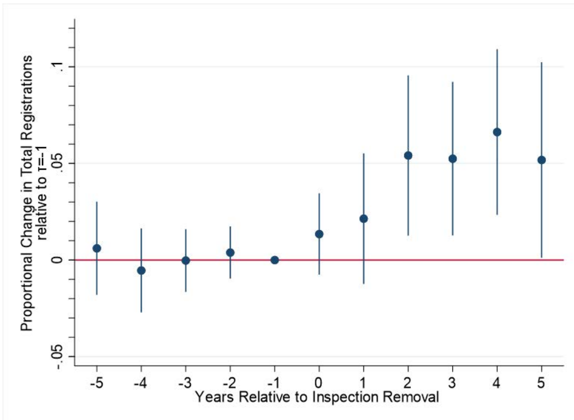


Notes: This figure presents residualized log number of vehicle registrations for each of our four difference-in-differences specifications. The four different specifications correspond to the four levels of "data source controls" for documented and undocumented changes in data reporting discussed in Section 3.4. We put the residuals for never treated, not yet treated, and treated observations in gray, red, and green respectively.

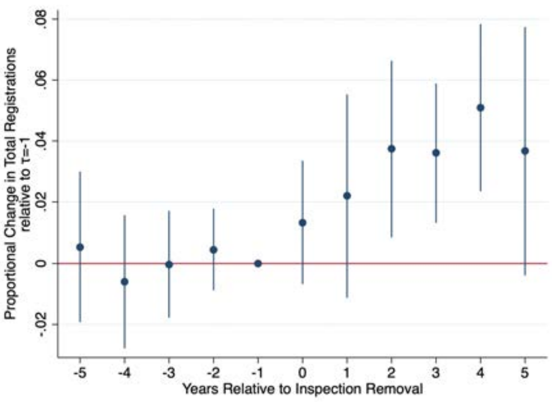
Figure B.2: Event study estimates of the impact of removing safety inspections on log vehicle registrations: "Small" window



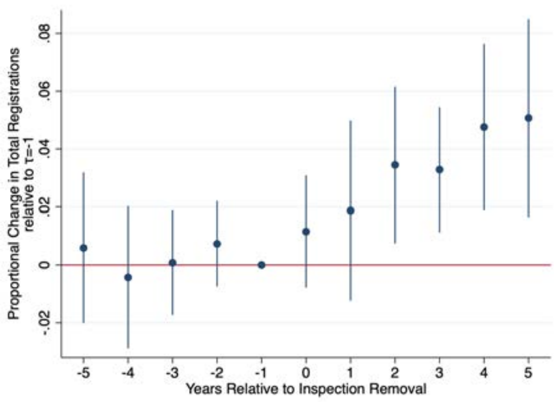
(a) Specification 1



(b) Specification 2



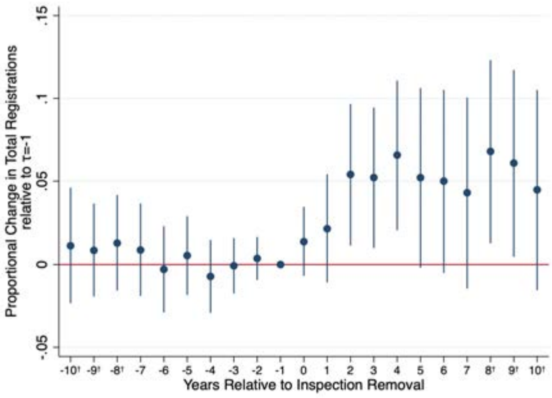
(c) Specification 3



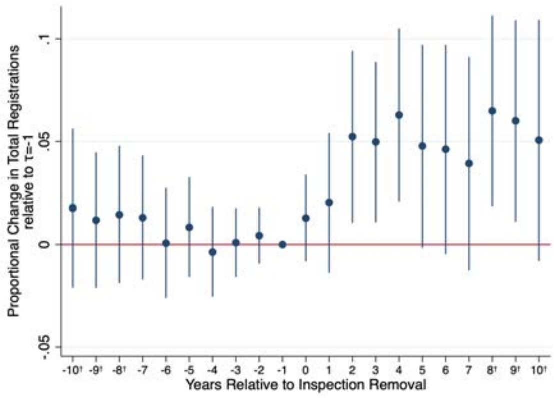
(d) Specification 4

Notes: This figure presents event study estimates of the impact of the removal of safety inspections on log vehicle registrations. The four different specifications correspond to the four levels of "data source controls" for documented and undocumented changes in data reporting discussed in Section 3.4. We also present 95% confidence intervals based on standard errors clustered by state.

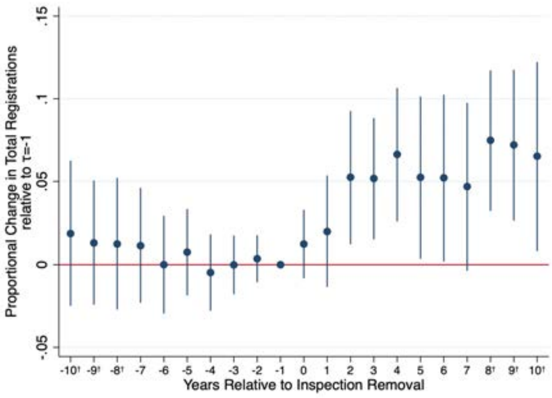
Figure B.3: Event study estimates of the impact of removing safety inspections on log vehicle registrations: "Large" window



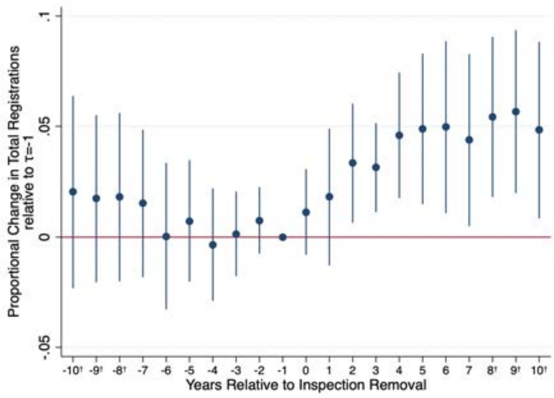
(a) Specification 1



(b) Specification 2



(c) Specification 3



(d) Specification 4

Notes: This figure presents event study estimates of the impact of the removal of safety inspections on log vehicle registrations. The four different specifications correspond to the four levels of "data source controls" for documented and undocumented changes in data reporting discussed in Section 3.4. We also present 95% confidence intervals based on standard errors clustered by state.

B.4 Difference-in-Differences and Event Study Results Without Time Trends

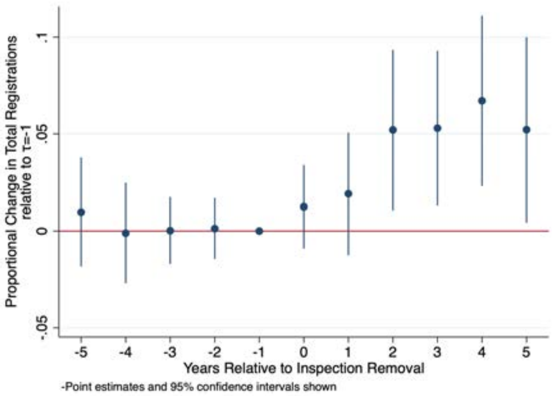
This subsection presents results from our first-stage difference-in-differences and event study models without state-specific linear time trends. We see in Table B.4 that the first-stage coefficients become slightly smaller and are not statistically significant after removing time trends. However, the event study coefficients remain similar when removing time trends (see Figures B.4 and B.5). The state-specific linear time trends therefore seem to be useful primarily for precision, through controlling for long-run trends in the outcome (i.e., more than 10 years away from the removal of safety inspections in the state).

Table B.4: Diff-in-Diff Results without Time Trends

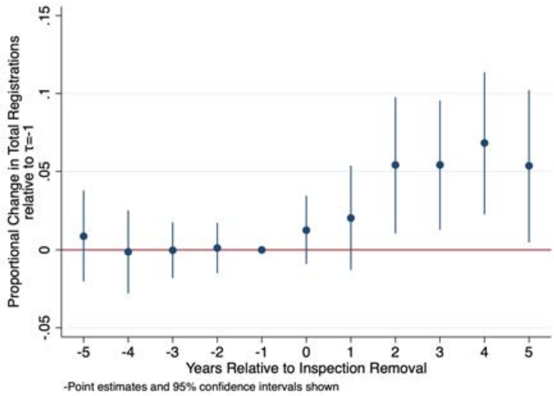
	Dep. Var.: Log Number of Vehicle Registrations			
	(1)	(2)	(3)	(4)
Removal of Safety Inspections	0.013	0.013	0.009	0.019
	(0.016)	(0.017)	(0.019)	(0.018)
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Linear Time Trends	N	N	N	N
Common Controls	Y	Y	Y	Y
Robust F-Stat	0.630	0.602	0.210	1.054
Number of Obs.	2,424	2,424	2,424	2,424
R^2	0.995	0.996	0.996	0.997

Notes: This table presents difference-in-differences estimates of the impact of the removal of safety inspections on the log of vehicle registrations. The unit of observation is state-year. In all of the specifications, we do not include state-specific linear time trends. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. Standard errors, reported in parentheses, are clustered by state. * p<0.1, ** p<0.05, *** p<0.01

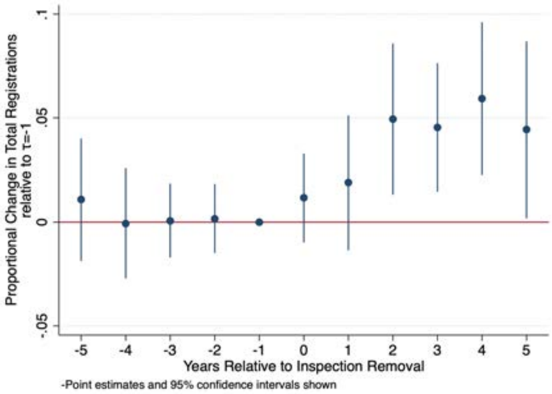
Figure B.4: Event study estimates of the impact of removing safety inspections on log vehicle registrations: “Small” window and no linear time trends



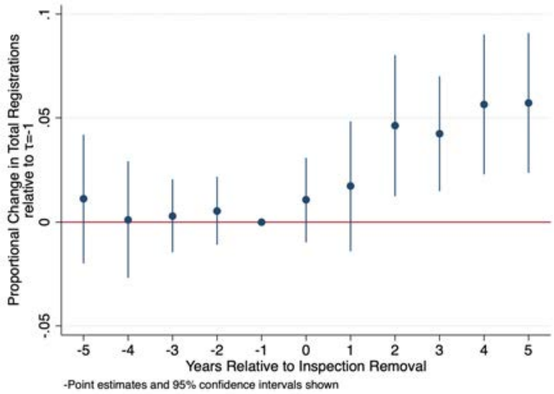
(a) Specification 1



(b) Specification 2



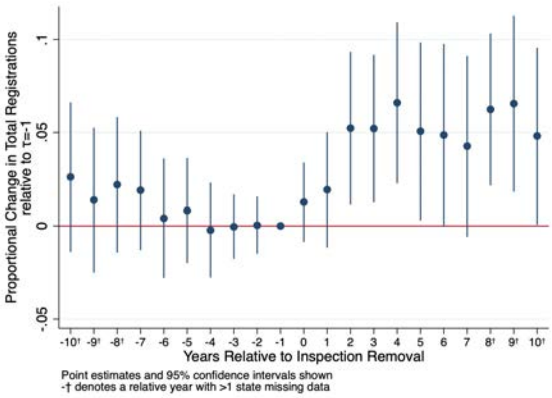
(c) Specification 3



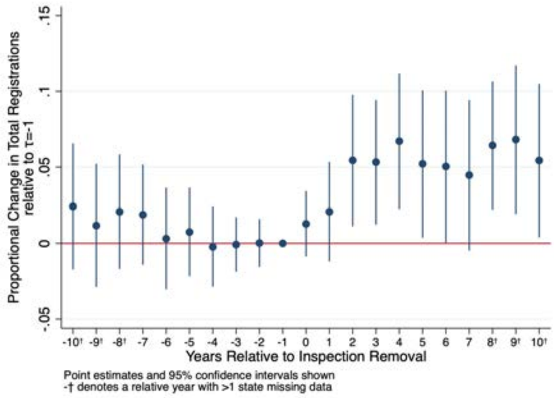
(d) Specification 4

Notes: This figure presents event study estimates of the impact of the removal of safety inspections on log vehicle registrations. The four different specifications correspond to the four levels of “data source controls” for documented and undocumented changes in data reporting discussed in Section 3.4. We also present 95% confidence intervals based on standard errors clustered by state.

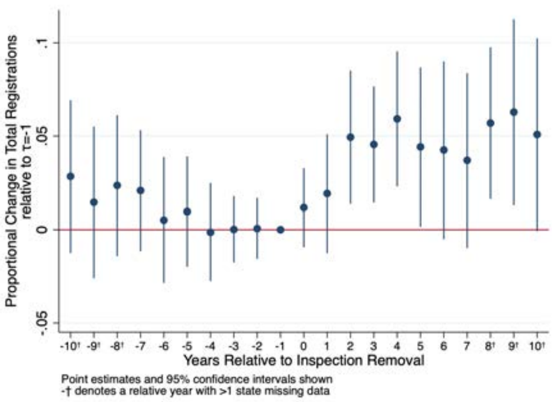
Figure B.5: Event study estimates of the impact of removing safety inspections on log vehicle registrations: "Large" window and no linear time trends



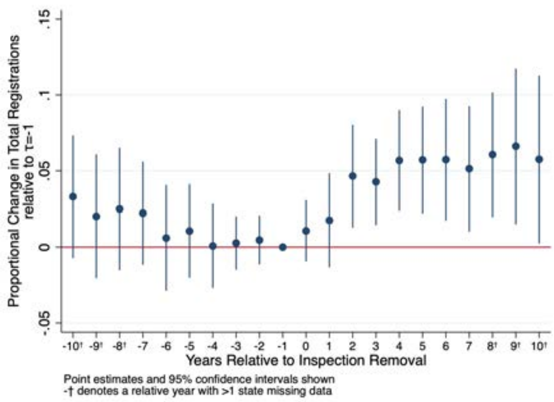
(a) Specification 1



(b) Specification 2



(c) Specification 3



(d) Specification 4

Notes: This figure presents event study estimates of the impact of the removal of safety inspections on log vehicle registrations. The four different specifications correspond to the four levels of "data source controls" for documented and undocumented changes in data reporting discussed in Section 3.4. We also present 95% confidence intervals based on standard errors clustered by state.

B.5 Leave-One-Out Analysis

The minimum and maximum p-values from "leave-one-out" analysis for our fleet-size elasticities of fleet travel distance (β) and of gasoline consumption (γ) for five null hypotheses can be seen in Tables B.5 and B.6. We performed this same analysis for our difference-in-differences model, with results presented in Table B.7. In all cases, we drop one state and perform the relevant analysis for the remaining states; this is done for all 50 states and the distribution of results across these 50 analyses is reported.

Table B.5: Leave-one-out analysis minimum and maximum p-values: Fleet Travel IV Model

	Null Value = 1.6	Null Value = 1	Null Value = 0	Null Value = 0.3	Null Value = -2.5
Max p-value	0.04	0.06	0.34	0.78	0.24
Min p-value	0.00	0.00	0.01	0.10	0.02

Notes: This table presents minimum and maximum p-values across 50 IV regressions of the effect of log fleet size on log vehicle miles-travelled (instrumenting with the removal of safety inspections). Namely, we remove one state from the analysis and estimate the model specified in Equation (3) in Section 3.3 for the remaining states; we repeat this removing each of the 50 states from the analysis.

Table B.6: Leave-one-out analysis minimum and maximum p-values: Fleet Gasoline Consumption IV Model

	Null Value = 11	Null Value = 9.7	Null Value = 1	Null Value = -4.8	Null Value = -6.1
Max p-value	0.06	0.06	0.07	0.07	0.07
Min p-value	0.00	0.00	0.01	0.00	0.00

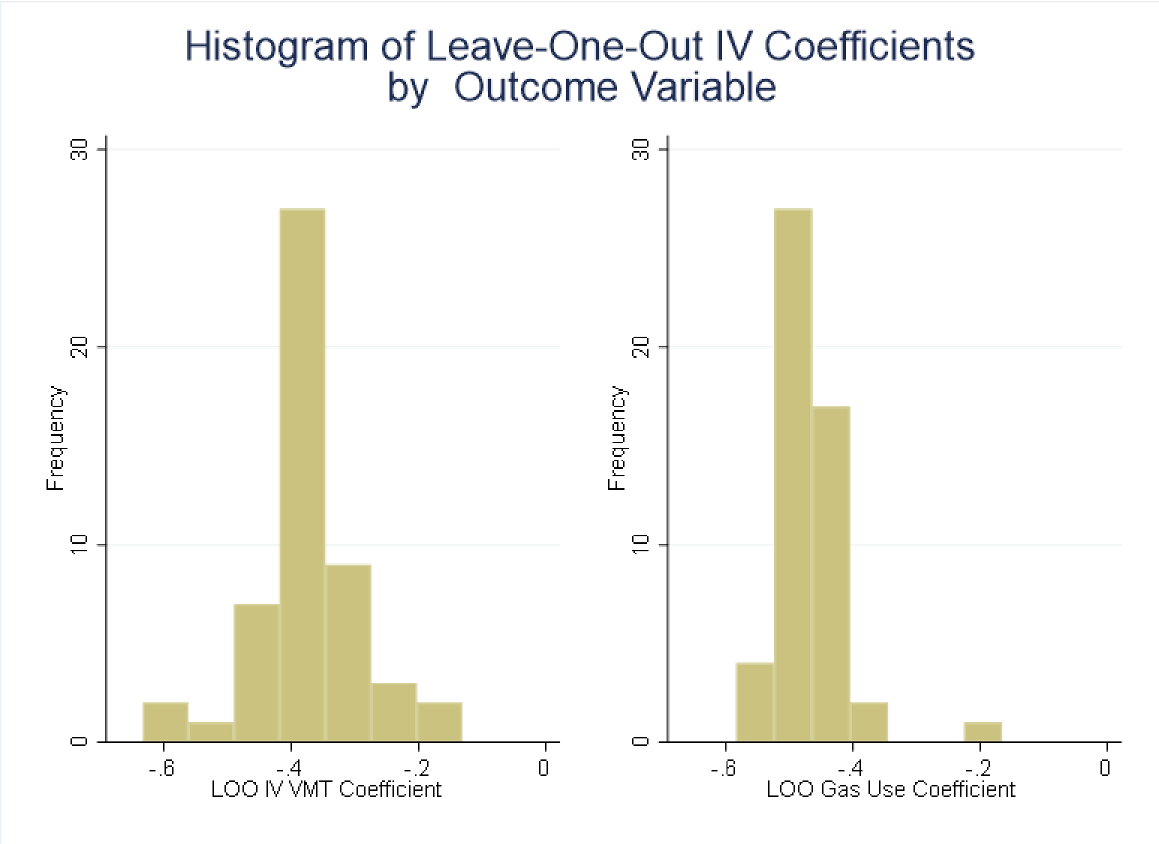
Notes: This table presents minimum and maximum p-values across 50 IV regressions of the effect of log fleet size on log gasoline usage (instrumenting with the removal of safety inspections). Namely, we remove one state from the analysis and estimate the model specified in Equation (4) in Section 3.3 for the remaining states; we repeat this removing each of the 50 states from the analysis.

Table B.7: Leave-one-out analysis minimum and maximum p-values: First-Stage Model

	Null Hypothesis Value = 0
Max p-value	0.07
Min p-value	0.00

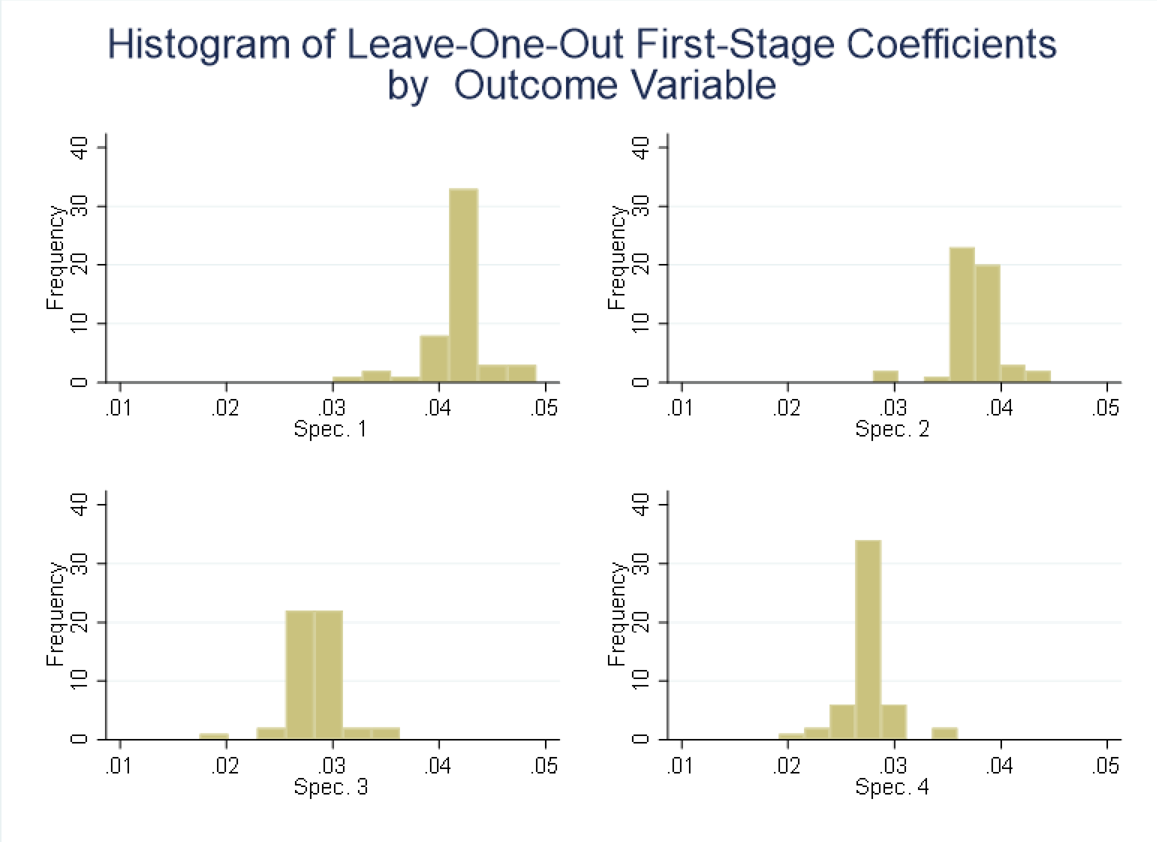
Notes: This table presents minimum and maximum p-values across 50 difference-in-differences regressions of the effect of removing safety inspections on log fleet size. Namely, we remove one state from the analysis and estimate the model specified in Equation (2) in Section 3.2 for the remaining states; we repeat this removing each of the 50 states from the analysis.

Figure B.6: Histogram of instrumental-variable coefficients from the leave-one-out analysis under the preferred specification



Notes: This figure presents histograms of estimates across 50 IV regressions of the effects of log fleet size on log vehicle miles-traveled (left panel) and log gasoline consumption (right panel). Log fleet size is instrumented with the removal of safety inspections. To do this, we remove one state from the analysis and estimate the model specified in Section 3.3 for the remaining states; we repeat this removing each of the 50 states from the analysis.

Figure B.7: Histogram of first-stage coefficients from the leave-one-out analysis across specifications



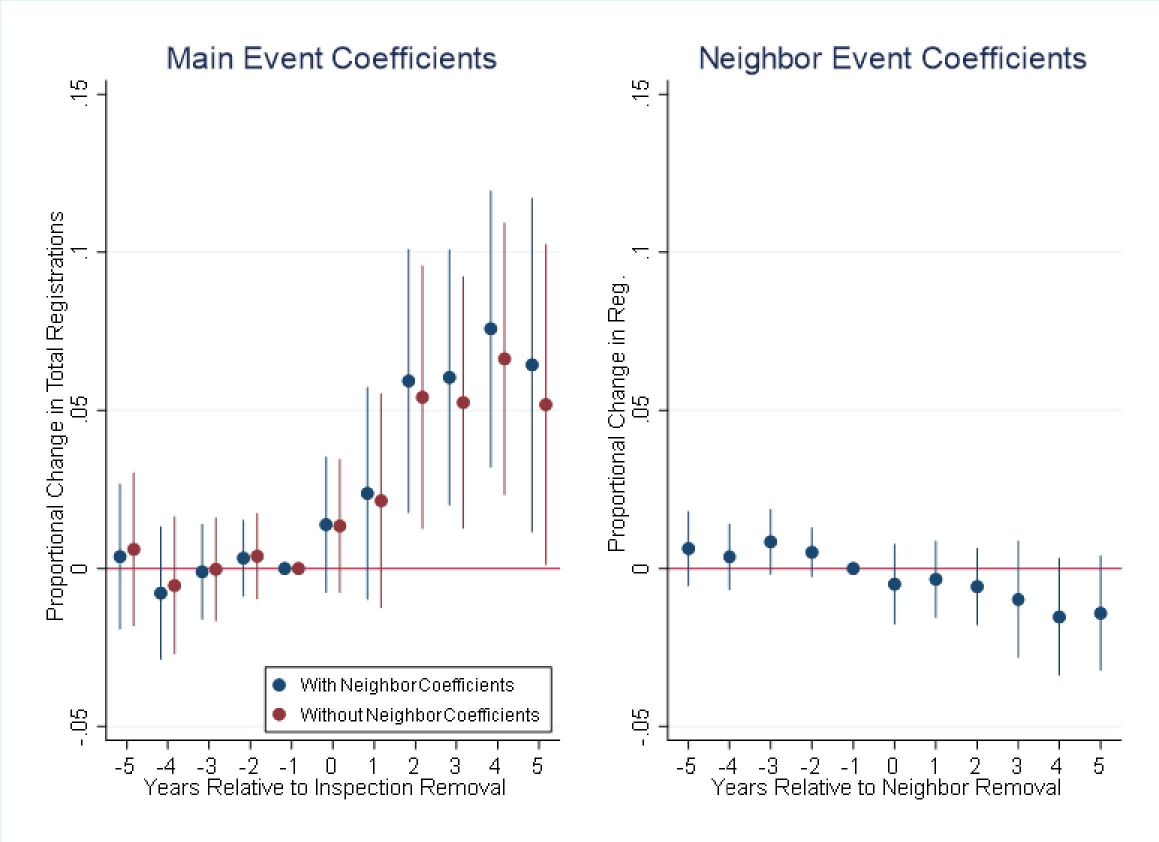
Notes: This histogram presents the distribution of estimates across 50 difference-in-differences regressions of the effect of removing safety inspections on log fleet size. Namely, we remove one state from the analysis and estimate the model specified in Equation (2) in Section 3.2 for the remaining states; we repeat this removing each of the 50 states from the analysis.

B.6 Spillover Analysis

The event study estimates corresponding to the impact of the removal of safety inspections *in neighboring states* on log vehicle registration are shown in Figure B.8. These estimates are not large and are not statistically significant either before or after the event. Moreover, the main event study estimates do not meaningfully change when controlling for the number of neighboring states that removed safety inspections. Overall, this provides evidence that treatment in one state does not substantially affect vehicle registrations in neighboring states.

B.6.1 Registrations

Figure B.8: Registration spillover results under the preferred specification



Notes: The left panel of this figure presents event study estimates of the impact of the removal of safety inspections on log vehicle registrations, controlling versus not controlling for the removal of safety inspections in *neighboring states*. In the right panel, we report event study estimates of the impact of the removal of safety inspections in *neighboring states* on log registrations; in this specification, we do not include the "main" event study dummies. In both cases, we also present 95% confidence intervals based on standard errors clustered by state.

B.7 Per-Capita Analysis

In order to test whether the results shown are a product of considering aggregate outcomes, which implicitly upweight large states, we re-estimated the models with the outcome variables being formulated in per-capita terms. The first stage becomes Equation (B.1) and the second stage equations corresponding to log fleet travel and log gasoline consumption become Equations (B.2) and (B.3), respectively. We draw the same conclusions from the estimates from the per-capita models as our primary specifications based on the aggregate outcomes.

$$\log\left(\frac{r_{it}}{p_{it}}\right) = \psi_i^{\mathrm{R}} + \tau_t^{\mathrm{R}} + \xi_i^{\mathrm{R}} \cdot t + \alpha d_{it} + \mathbf{x}_{it}^{\mathrm{R}} \boldsymbol{\alpha} + \varepsilon_{it}^{R} \tag{B.1}$$

$$\log\left(\frac{v_{it}}{p_{it}}\right) = \psi_i^{\mathrm{V}} + \tau_t^{\mathrm{V}} + \xi_i^{\mathrm{V}} \cdot t + \beta\log\left(\frac{r_{it}}{p_{it}}\right) + \mathbf{x}_{it}^{\mathrm{V}}\boldsymbol{\delta} + \varepsilon_{it}^{V} \tag{B.2}$$

$$\log\left(\frac{g_{it}}{p_{it}}\right) = \psi_i^{\mathrm{G}} + \tau_t^{\mathrm{G}} + \xi_i^{\mathrm{G}} \cdot t + \gamma\log\left(\frac{r_{it}}{p_{it}}\right) + \mathbf{x}_{it}^{\mathrm{G}}\boldsymbol{\nu} + \varepsilon_{it}^{G} \tag{B.3}$$

B.7.1 Difference-in-Differences Model

Under our preferred specification, the magnitude of the treatment effect remains similar, but is statistically significant only at the 10% confidence level.

Table B.8: Per-Capita Diff-in-Diff Results

Dep. Var.: Log Number of Vehicle Registrations per Capita	(1)	(2)	(3)	(4)
Removal of Safety Inspections	0.034*	0.031*	0.034**	0.021
	(0.018)	(0.016)	(0.016)	(0.017)
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Linear Time Trends	Y	Y	Y	Y
Common Controls	Y	Y	Y	Y
Robust F-Stat	3.618	3.562	4.759	1.600
Number of Obs.	2,424	2,424	2,424	2,424
R^2	0.923	0.935	0.944	0.960

Notes: This table presents difference-in-differences estimates of the impact of the removal of safety inspections on the log of the number of vehicle registrations per capita. The unit of observation is state-year. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. Standard errors, reported in parentheses, are clustered by state. * p<0.1, ** p<0.05, *** p<0.01

B.7.2 IV Models

In both IV models, there are significant changes to the form of the confidence sets. However, for both fleet-distance traveled and gasoline consumption, we continue to reject the unit elasticity assumption.

B.7.3 Event Study Models

There is no meaningful qualitative difference between the estimates from our primary formulation and those based on log registrations per capita (see Figure B.9).

Table B.9: IV Estimates of the Effect of Log Number of Registrations on Log VMT per Capita

Panel A: IV Estimates and Robust 95% Confidence Intervals				
	Dep. Var.: Log Vehicle-Miles Travelled per Capita			
	(1)	(2)	(3)	(4)
Log Registrations	-0.626	-0.70	-0.55	-0.61
	(-∞, 0.49]; [32.63, ∞)	(-∞, 0.56]; [28.03, ∞)	[-8.30, 0.64]	(-∞, ∞)
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Time Trends	Y	Y	Y	Y
Common Controls	Y	Y	Y	Y
Number of Obs.	2,424	2,424	2,424	2,424
Panel B: Anderson-Rubin Test Statistics and P-Values				
	$H_0 : \beta = 1.6$			
AR Stat	6.736***	6.495**	6.374**	2.260
p-value	0.009	0.011	0.012	0.133
	$H_0 : \beta = 1$			
AR Stat	6.168**	5.675**	5.257**	1.953
p-value	0.013	0.017	0.022	0.162
	$H_0 : \beta = 0.3$			
AR Stat	2.662	2.439	2.149	0.899
p-value	0.103	0.118	0.143	0.343
	$H_0 : \beta = 0$			
AR Stat	1.090	1.081	0.868	0.399
p-value	0.296	0.298	0.352	0.528
	$H_0 : \beta = -2.5$			
AR Stat	1.349	1.180	1.999	0.657
p-value	0.245	0.277	0.157	0.417

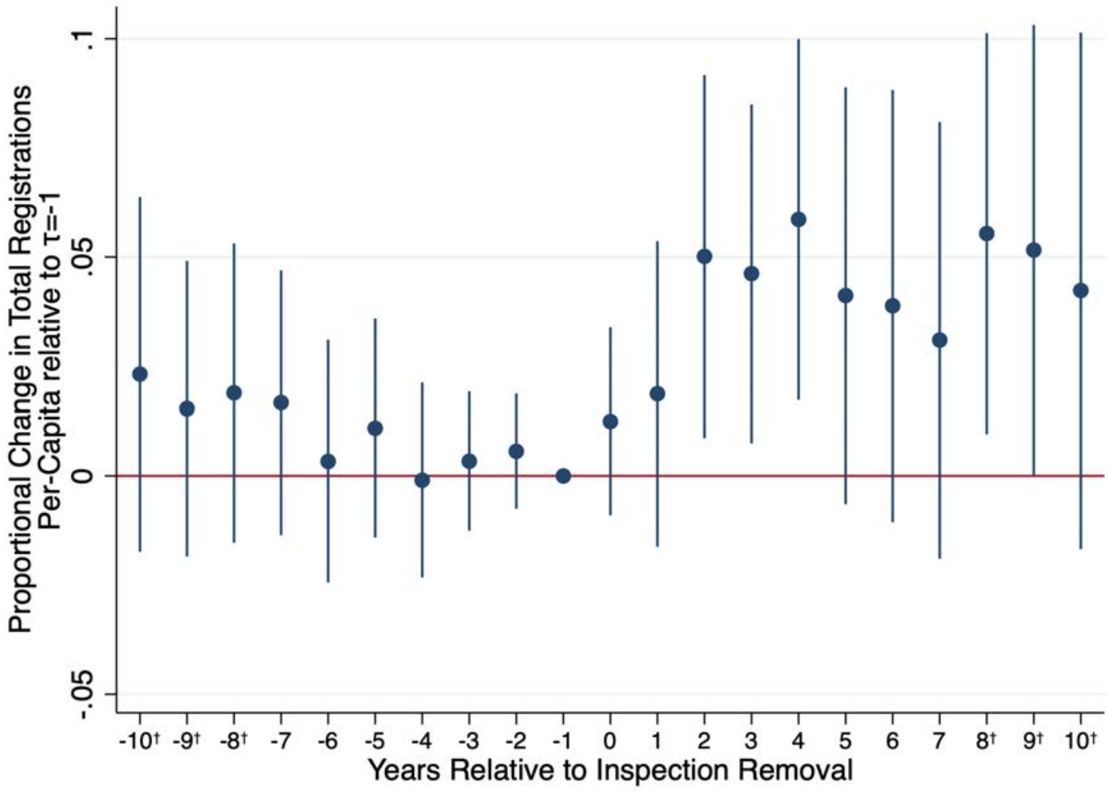
Notes: The top panel of this table presents IV estimates and 95% confidence sets robust to weak instruments of the relationship between log vehicle-miles travelled per capita and log vehicle registrations per capita. The bottom panel presents Anderson-Rubin chi-squared statistics along with p-values for different null hypotheses pertaining to the parameter estimated in the top panel. The unit of observation is state-year. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. 95% confidence sets are based on standard errors clustered by state. * p<0.1, ** p<0.05, *** p<0.01

Table B.10: IV Estimates of the Effect of Log Number of Registrations per Capita on Log Highway Gas Consumption per Capita

Panel A: IV Estimates and Robust 95% Confidence Intervals				
	Dep. Var.: Log Gasoline Consumption per Capita			
	(1)	(2)	(3)	(4)
Log Registrations	-0.737	-0.82	-0.79	-1.31
	$(-\infty, 0.11]$; $[19.76, \infty)$	$(-\infty, 0.14]$; $[19.77, \infty)$	[-5.72, 0.07]	$(-\infty, 0.12]$; $[2.28, \infty)$
Data Source Controls	None	Documented	Large Undocumented	Small Undocumented
State Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
State-Specific Time Trends	Y	Y	Y	Y
Common Controls	Y	Y	Y	Y
Number of Obs.	2,429	2,429	2,429	2,429
Panel B: Anderson-Rubin Test Statistics and P-Values				
	$H_0 : \gamma = 11$			
AR Stat	4.005**	4.009**	5.271**	1.815
p-value	0.045	0.045	0.022	0.178
	$H_0 : \gamma = 9.7$			
AR Stat	4.053**	4.058**	5.323**	1.877
p-value	0.044	0.044	0.021	0.171
	$H_0 : \gamma =1$			
AR Stat	5.707**	5.489**	6.430**	6.170**
p-value	0.017	0.019	0.011	0.013
	$H_0 : \gamma =-4.8$			
AR Stat	2.685	2.612	3.633*	0.631
p-value	0.101	0.106	0.057	0.427
	$H_0 : \gamma =-6.1$			
AR Stat	2.890*	2.833*	3.908**	0.761
p-value	0.089	0.092	0.048	0.383

Notes: The top panel of this table presents IV estimates and 95% confidence sets robust to weak instruments of the relationship between log highway gas consumption per capita and log vehicle registrations per capita. The bottom panel presents Anderson-Rubin chi-squared statistics along with p-values for different null hypotheses pertaining to the parameter estimated in the top panel. The unit of observation is state-year. The four specifications pertaining to different levels of data source controls (DSCs) are documented in Section 3.4. 95% confidence sets are based on standard errors clustered by state. * p<0.1, ** p<0.05, *** p<0.01

Figure B.9: Event study estimates of the impact of removing safety inspections on log vehicle registrations per capita



Notes: This figure presents event study estimates of the impact of the removal of safety inspections on log vehicle registrations per-capita. The specification is listed in Equation (B.1) in Section B.7. We also present 95% confidence intervals based on standard errors clustered by state.

B.8 Random Treatment Analysis

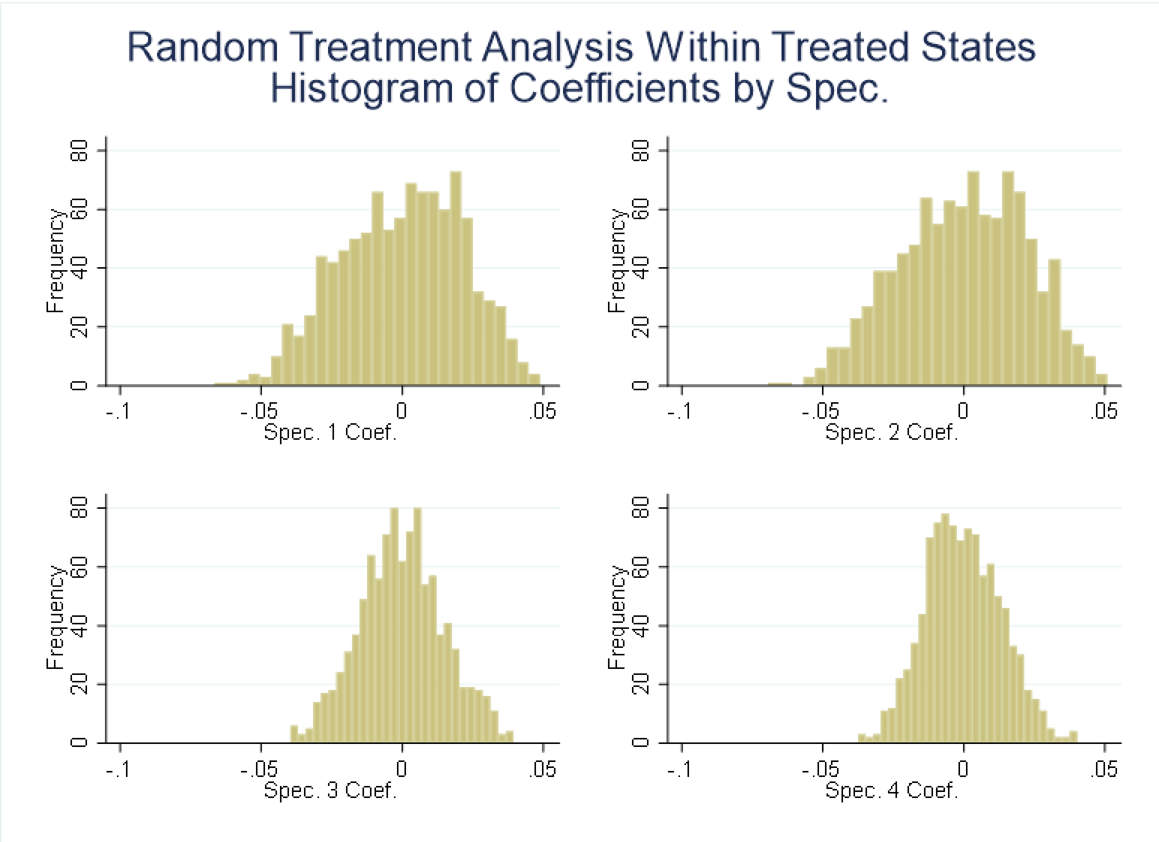
In order to assess whether our first-stage estimates are driven by some unique aspect of the treated states, we run a random treatment analysis. This first involves removing any data from after the removal of safety inspections (i.e., treated data). Then, we randomize treatment in two different ways:

- Across treated states: A year is randomly chosen as the "treatment" year between one plus the first year of data available and the last year of data available for a given state. This is repeated for every treatment state in every simulation.
- Across all states: States have a $\frac{16}{51}$ chance of being an "always treated" state, a $\frac{19}{51}$ chance of being "never treated", and a $\frac{16}{51}$ chance of being a "treated" state (the proportions of these classifications present in the data). Each classification is randomly assigned with these probabilities for each simulation. If a state

is classified as “treated”, a year is randomly chosen as the “treatment” year between the first year of data available plus one and the last year of data available for the given state. This is repeated for every state in every simulation.

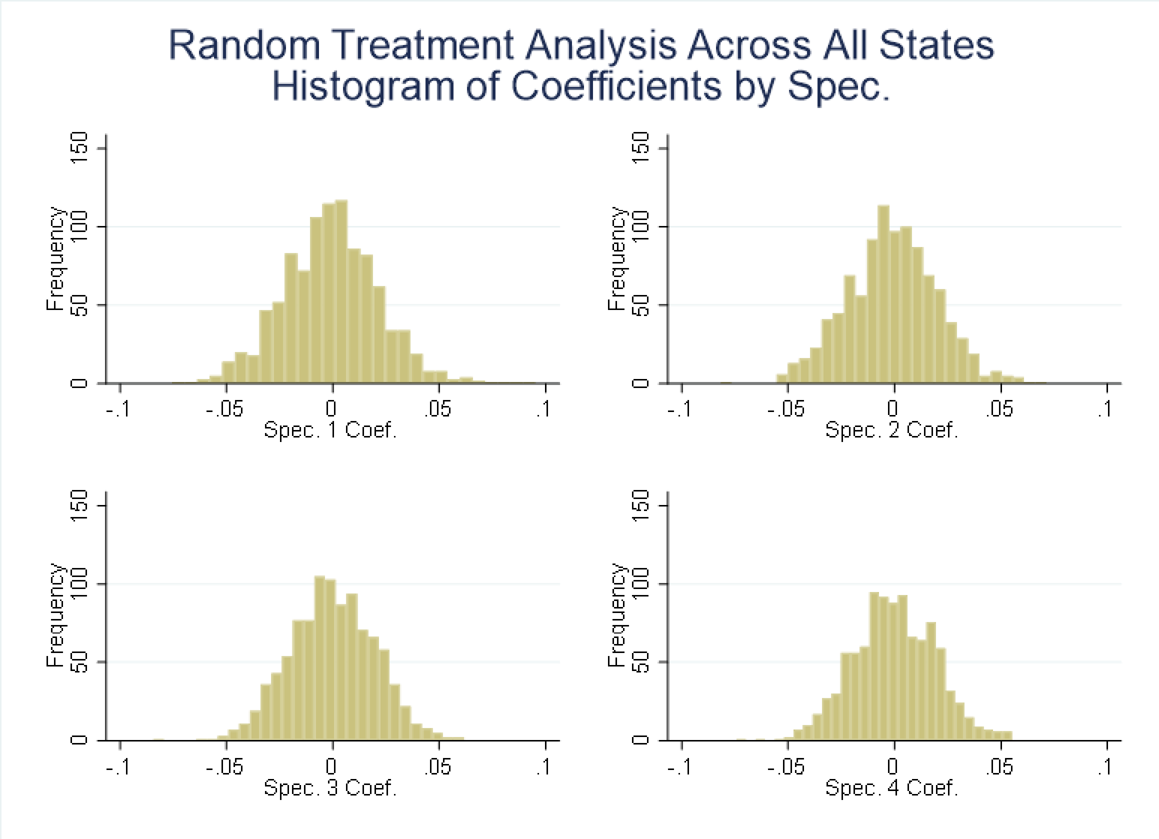
The distribution of estimated coefficients associated with the difference-in-differences estimates from 1,000 such simulations can be seen in Figures B.10 and B.11. These distributions are all centered around zero. In Table B.11, we present the rejection rates of the treatment coefficient across all specifications and randomization strategies at a significance level of 5%. Overall, rejections rates are only slightly larger than would be expected at 5% significance. This serves as evidence that our estimated effect of removing safety inspections on log number of vehicle registrations is likely not driven by idiosyncratic trends over time or across states in registrations.

Figure B.10: Distribution of placebo ATT estimates with treatment randomization across treated states



Notes: This figure presents placebo estimates of the effect of removing safety inspections on log vehicle registrations from a random treatment analysis. This first involves removing any data from after the removal of safety inspections (i.e., treated data). Then, we randomize treatment across treated states: a year is randomly chosen as the “treatment” year between one plus the first year of data available and the last year of data available for a given state. This is repeated for every treatment state in every simulation.

Figure B.11: Distribution of placebo ATT estimates with treatment randomization across all states



Notes: This figure presents placebo estimates of the effect of removing safety inspections on log vehicle registrations from a random treatment analysis. This first involves removing any data from after the removal of safety inspections (i.e., treated data). Then, we randomize treatment across all states: states have a $\frac{16}{51}$ chance of being an "always treated" state, a $\frac{19}{51}$ chance of being "never treated", and a $\frac{16}{51}$ chance of being a "treated" state (the proportions of these classifications present in the data). Each classification is randomly assigned with these probabilities for each simulation. If a state is classified as "treated", a year is randomly chosen as the "treatment" year between the first year of data available plus one and the last year of data available for the given state. This is repeated for every state in every simulation.

Table B.11: Random Treatment Simulation Rejection Rates at 5% Significance

	(1) Spec. 1	(2) Spec. 2	(3) Spec. 3	(4) Spec. 4
Rejection Rate: Treated State Randomization	0.081	0.080	0.093	0.076
Rejection Rate: All State Randomization	0.082	0.078	0.075	0.085

Notes: This figure presents placebo estimates of the effect of removing safety inspections on log vehicle registrations from two random treatment analyses. The first randomizes treatment across all states that ever removed safety inspections while the second randomizes treatment across all states. See Section B.8 for more details.

B.9 Null Hypothesis Derivation

Here we derive the fleet-size elasticities of fleet travel distance and gasoline consumption implied by the assumptions made in Alberini, Harrington and McConnell (1998).

B.9.1 Variable Definitions

We define the following five indices:

- t - Year-of-sample, which is contained in set $\mathbb{T}$.
- c - Vehicle class (e.g. passenger car, light truck, etc.), which is contained in set $\mathbb{C}$.
- m - Vehicle model (e.g. Ford Taurus, Jeep Wrangler, etc.), which is contained in set $\mathbb{M}_c$ (i.e. all models in a given class of vehicle).
- i - Vehicle vintage (e.g. 2001, 2009, etc.), which is contained in set $\mathbb{M}_c$ (i.e. all models in a given class of vehicle), which is contained in the set $\mathbb{I}$.
- s - Scenario, which is contained in set $\mathbb{S} = \{\text{With Policy Change, Without Policy Change}\} = \{1, 0\}$.

and the following variables:

- r_{stcmi} - registered (i.e. operational) fleet size in scenario s at time t, for class c, and model m.
- a_{stcmi} - Distance traveled by a single vehicle in scenario s at time t, for class c, and model m.
- v_{stcmi} - Total miles traveled by all registered vehicles of class c, model m, during time t in scenario s.
- d_{tcmi} - Percentage difference between scenarios being considered in time t, for class c, and model m; We present this as a constant, but it could be a function of some other variable (e.g. vehicle price).

Fleet-wide travel and single vehicle travel are related as follows::

$$v_{stcmi} = a_{stcmi} r_{stcmi} \tag{B.4}$$

Further, counterfactual levels of registration are:

$$r_{1,tcmi} = (1 - d_{tcmi}) r_{0,tcmi} \tag{B.5}$$

Depending on the rates of scrappage, if $r_{0,tcmi} \to 0$ faster than $r_{1,tcmi} \to 0$ (i.e. scrappage is being delayed by the policy), then $d \to -\infty$. Otherwise, $d \to 1$.

B.9.2 Derivation

We focus on the model-level fleet-size elasticity of fleet travel distance (β_{tcmi}):

$$\beta_{tcmi} = \frac{\Delta v_{tcmi}}{\Delta r_{tcmi}} \frac{r_{0,tcmi}}{v_{0,tcmi}} \tag{B.6}$$

Where:

$$\begin{aligned} \Delta r_{tcmi} &= r_{1,tcmi} - r_{0,tcmi} \\ &= (1 - d_{tcmi}) r_{0,tcmi} - r_{0,tcmi} \\ &= -d_{tcmi} r_{0,tcmi} \end{aligned} \tag{B.7}$$

If we assume that the VMT schedule is constant between scenarios (i.e. $a_{1,tcmi} = a_{0,tcmi}$), then, using Equations (B.4) and (B.7):

$$
\begin{aligned}
\Delta v_{tcmi} &= v_{1,tcmi} - v_{0,tcmi} \\
&= (a_{1,tcmi} r_{1,tcmi}) - (a_{0,tcmi} r_{0,tcmi}) \\
&= a_{0,tcmi} \left(r_{1,tcmi} - r_{0,tcmi}\right) \\
&= a_{0,tcmi} \left(\Delta r_{tcmi}\right) \\
&= a_{0,tcmi} \left(-d_{tcmi} r_{0,tcmi}\right) \\
&= -d_{tcmi} a_{0,tcmi} r_{0,tcmi} \\
&= -d_{tcmi} v_{0,tcmi}
\end{aligned}
\tag{B.8}
$$

Then, using Equations (B.7) and (B.8):

$$
\begin{aligned}
\beta_{tcmi} &= \frac{\Delta v_{tcmi}}{\Delta r_{tcmi}} \frac{r_{0,tcmi}}{v_{0,tcmi}} \\
&= \frac{-d_{tcmi} v_{0,tcmi}}{-d_{tcmi} r_{0,tcmi}} \frac{r_{0,tcmi}}{v_{0,tcmi}} \\
&= \frac{-d_{tcmi}}{-d_{tcmi}} \\
&= 1
\end{aligned}
\tag{B.9}
$$

Therefore, any model that uses a constant mileage schedule is implicitly assuming a fleet-size elasticity of travel distance equal to 1.

B.10 SAFE PRIA and FRIA Null Hypothesis Calculation

In order to establish a null hypothesis for overall fleet-size elasticities of fleet travel distance and gasoline consumption comparable to the fleet-wide implications of the assumptions made in the analyses presented in NHTSA and USEPA, 2018a and NHTSA and USEPA, 2020, we examine the results presented in the CO_2 analysis conducted by U.S. National Highway Traffic Safety Administration (2018, 2020). From these results, we can calculate effective, yearly fleet-size elasticities of travel and fuel use with Equations (B.10) and (B.11) across a variety of scenarios simulated to analyze different potential policy outcomes. We utilize Scenario 0 (Augural CAFE standards) as the baseline in all calculations, with the other scenarios serving as alternatives.

The following elasticities measure the percent changes in fleet travel distance v and gasoline consumption g per percent change in fleet size r across two scenarios (BASE and ALT):

$$\beta_y = \frac{\frac{v_y^{\text{ALT}} - v_y^{\text{BASE}}}{v_y^{\text{BASE}}}}{\frac{r_y^{\text{ALT}} - r_y^{\text{BASE}}}{r_y^{\text{BASE}}}} = \frac{v_y^{\text{ALT}} - v_y^{\text{BASE}}}{v_y^{\text{BASE}}} \left(\frac{r_y^{\text{ALT}} - r_y^{\text{BASE}}}{r_y^{\text{BASE}}} \right)^{-1} \tag{B.10}$$

$$\gamma_y = \frac{\frac{g_y^{\text{ALT}} - g_y^{\text{BASE}}}{g_y^{\text{BASE}}}}{\frac{r_y^{\text{ALT}} - r_y^{\text{BASE}}}{r_y^{\text{BASE}}}} = \frac{g_y^{\text{ALT}} - g_y^{\text{BASE}}}{g_y^{\text{BASE}}} \left(\frac{r_y^{\text{ALT}} - r_y^{\text{BASE}}}{r_y^{\text{BASE}}} \right)^{-1} \tag{B.11}$$

We use the mean of these elasticities over the calendar years 2016 to 2050[t] as a measure of the effective assumption being made in the model.[u] For the PRIA and FRIA, the elasticities are relatively stable across all scenarios we consider to be relevant.[v] Based on this, we then take the average of the elasticities across all of the relevant scenarios that account for the rebound effect to serve as our primary null hypotheses. Elasticities estimated without the rebound effect are also presented in Table B.12, as they serve as a point of comparison in our setting where compositional effects seem to be small.

Table B.12: Estimated SAFE PRIA and FRIA Elasticites

Analysis	Allowing for Rebound	β	γ
PRIA	Yes	1.6	-4.8
PRIA	No	0.3	-6.1
FRIA	Yes	-2.5	9.7
FRIA	No	0	11

Notes: This table lists the fleet size elasticities of travel distance (β) and gasoline consumption (γ) implied by the assumptions made in the analyses presented in NHTSA and USEPA, 2018a and NHTSA and USEPA, 2020. We calculate elasticities with and without incorporating the rebound effect: that scrappage leads to the purchase of newer vehicles that may be driven more than old vehicles. Details on the calculation of these elasticities is in Section B.10.

[t] This range of calendar years was presented for discussion in U.S. National Highway Traffic Safety Administration and U.S. Environmental Protection Agency (2018, Figure 8-39; 8-40).

[u] We ignore values that are undefined. Elasticity estimates are undefined if there is no difference in fleet size between the base and alternative scenario.

[v] CAFE has been in place since 1975, a large majority of the time period we study (NHTSA and USEPA, 2020, p.6). We do not consider the one MPG standard as being relevant as this modeling scenario essentially removes any CAFE regulation from being in place.

B.11 Lack of Compositonal Effects from removing safety inspections

First, we'll begin by establishing the following notation:

- v - fleet-wide travel distance.
- f - fleet-wide fuel consumption.
- c - fleet-wide fuel consumption per travel distance (i.e. inverse of fuel economy - measured in unit fuel/unit distance) weighted by travel
- r - fleet size

We define the fleet-size elasticities of travel (β), fuel consumption (γ), and fuel consumption per travel distance:

$$\begin{aligned} \beta &= \frac{\partial v}{\partial r}\frac{r}{v} \\ \gamma &= \frac{\partial f}{\partial r}\frac{r}{f} \\ \psi &= \frac{\partial c}{\partial r}\frac{r}{c} \end{aligned} \tag{B.12}$$

The following identity must hold, as described in Dimitropoulos, Oueslati and Sintek (2018, p.164) and Small and Van Dender (2007, p.27) when discussing different versions of the rebound effect:

$$f = vc \tag{B.13}$$

Equation (B.13) simply states that the fleet's fuel consumption is equal to the distance driven by the fleet multiplied by the average amount of fuel need to drive the given unit distance. With Equation (B.13), we can plug vc for f in the definition of γ in Equation (B.12) and calculate the relationship between β, γ, and ψ.

$$
\begin{aligned}
\gamma = \frac{\partial f}{\partial r}\frac{r}{f} &= \frac{\partial\,(vc)}{\partial r}\frac{r}{(vc)} \\
&= \left(\frac{\partial v}{\partial r}c + v\frac{\partial c}{\partial r}\right)\frac{r}{(vc)} \\
&= \frac{\partial v}{\partial r}c\frac{r}{(vc)} + \frac{\partial c}{\partial r}v\frac{r}{(vc)} \\
&= \frac{\partial v}{\partial r}\frac{cr}{vc} + \frac{\partial c}{\partial r}\frac{vr}{vc} \\
&= \frac{\partial v}{\partial r}\frac{r}{v} + \frac{\partial c}{\partial r}\frac{r}{c} \\
&= \beta + \psi
\end{aligned} \tag{B.14}
$$

This derivation tells us that $\gamma = \beta + \psi$. That is to say, the fleet-size elasticity of fuel consumption is equal to the fleet-size elasticity of travel plus the fleet-size elasticity of fuel consumption per travel distance.

In the case where there are no composition effects (i.e., $\psi = 0$), the fleet-size elasticities of fuel consumption and travel must be equal (i.e. $\beta - \gamma = 0$). To test this, we test whether there is a difference in the reduced form coefficients for fleet travel distance (δ) versus gasoline consumption (θ). If $\delta = \theta$, that implies that $\beta = \gamma$ since δ and θ serve as the numerators in the expressions for β and γ respectively.

We are able to empirically test $H_0 : \delta - \theta = 0$ by simultaneously estimating the reduced form equations for fleet travel distance and gasoline consumption. We calculate the difference in the two estimates, and perform a Wald test to test the null hypothesis. The resulting mean and standard error for the expression $\hat{\delta} - \hat{\theta}$ is 0.0041 and 0.023 respectively. This results in a Wald test p-value of $H_0 : \delta - \theta = 0$ of 0.859. This provides evidence that the removal of safety inspections did not lead to a statistically significant shift in efficiency composition within the fleet.

B.12 Goodman-Bacon Decomposition

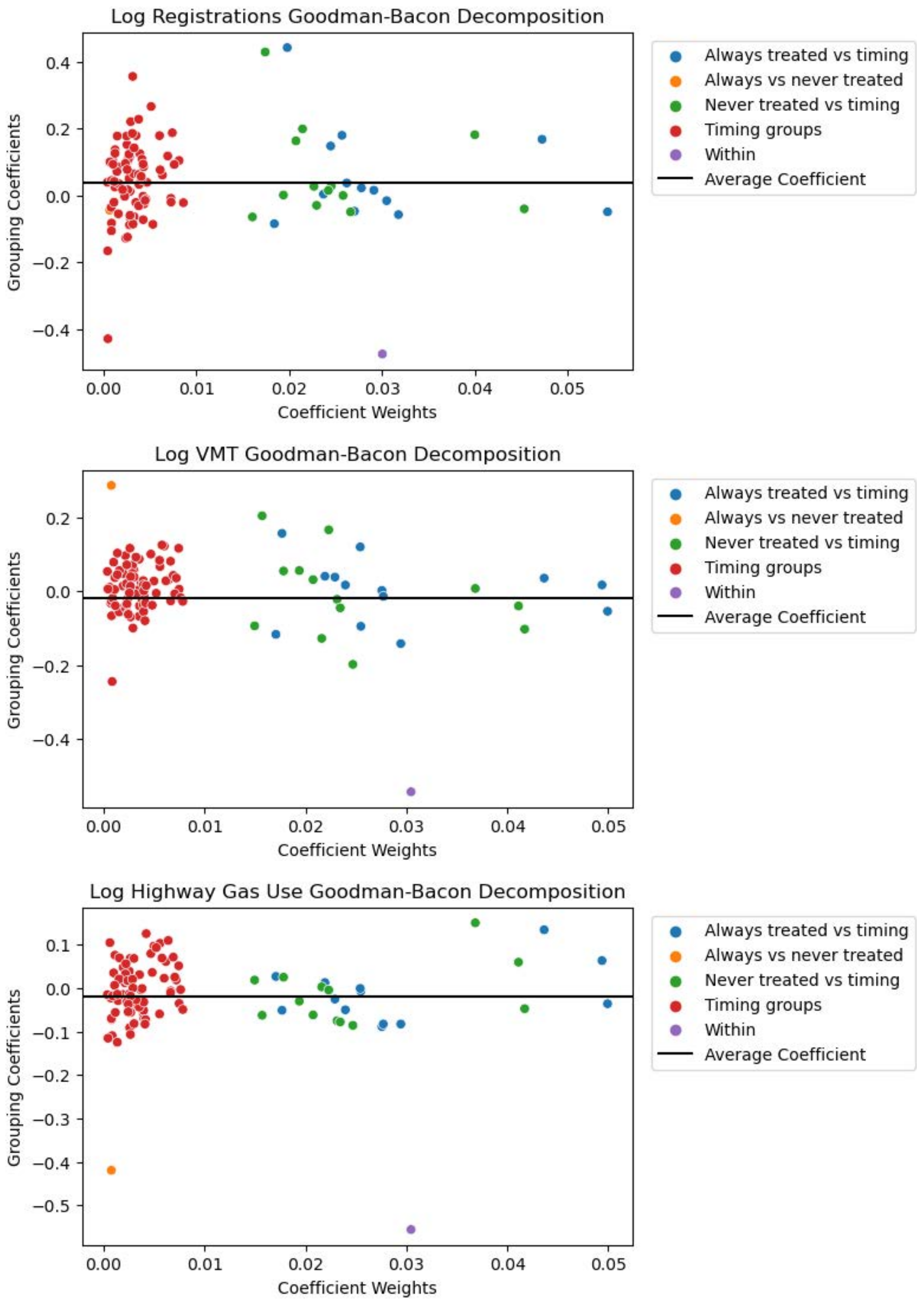
In this section, we report results from the decomposition specified in Goodman-Bacon (2021) for three equations of interest: the first-stage equation as well as the reduced-form equations for both fleet travel and fleet gasoline consumption. The Goodman-Bacon decomposition requires a balanced panel, a requirement that our dataset does not adhere to for reasons discussed in Appendix Section A.2 and Section 3.4. Therefore, for this decomposition analysis, we include all data that is removed for the travel and gasoline consumption models. For the registrations model, we remove all data for Colorado due to reported discrepancies in data collection.

The results from this additional analysis are shown in Figure B.12. By inspection, there doesn't seem to be any particular comparison across groups of entities that is unduly influencing the overall difference-in-differences coefficient. The one exception might be the "within-state" comparison, which is quite negative and receives a reasonable weight in the overall estimate.

As discussed in Baker, Larcker and Wang (2021), difference-in-differences models can produce biased estimates of the average treatment effect on the treated when the timing of treatment is staggered and treatment effects are heterogeneous across units or space. Results from Appendix Section B.3 indicate that dynamics are not a large issue in our setting; the event study coefficients "mature" to an approximately constant magnitude after two years of treatment.

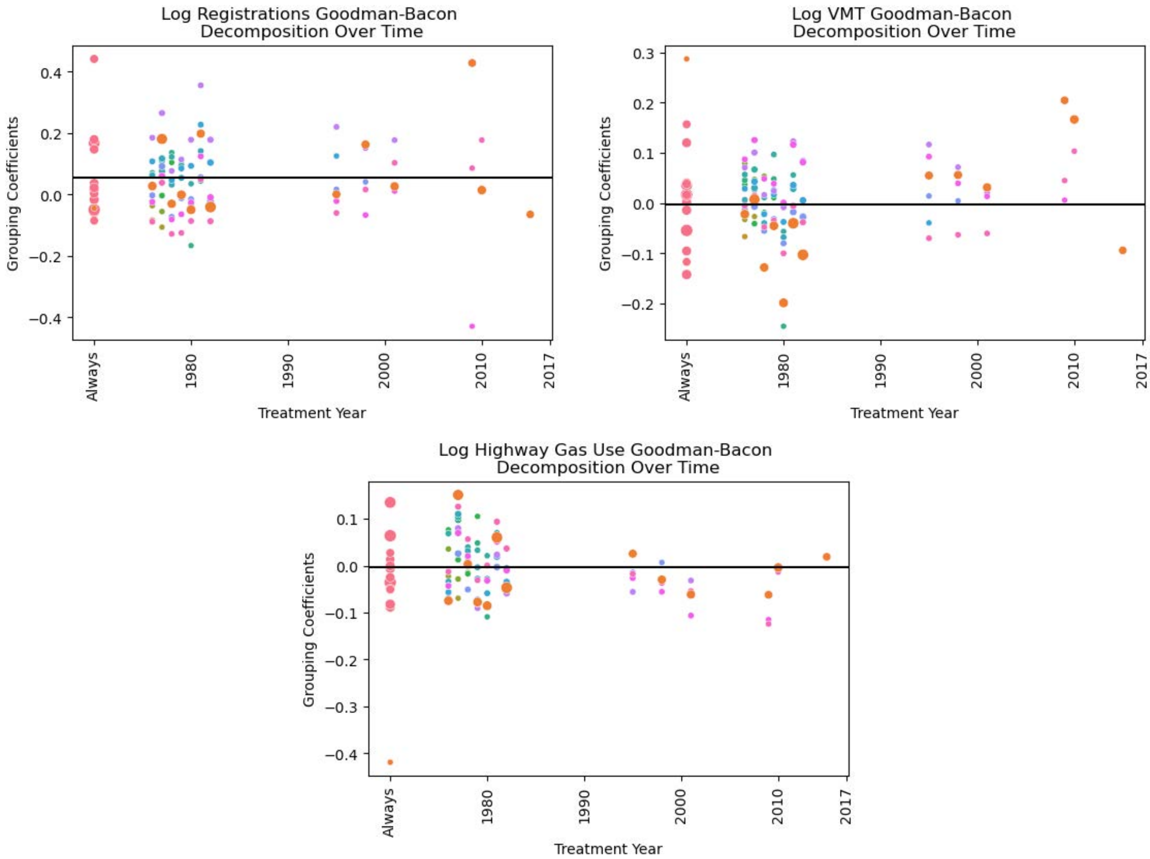
However, this does not rule out the potential for heterogeneous treatment effects over time. In order to assess this, coefficients from Figure B.12 are again plotted with their magnitude on the y-axis and the associated treatment year group on the x-axis in Figure B.13. In Figure B.13, size of the points represents the associated weight and the different colors represent the various control groupings. There seems to be no prominent trends in the average treatment effect over time, assuaging concerns that treatment effects are heterogeneous over time.

Figure B.12: Goodman-Bacon Decomposition coefficients and their weights



Notes: This decomposition calculates treatment effect estimates based on comparisons across different treatment and control groups, along with weights that specify how much each treatment effect contributes to the overall difference-in-differences estimate Goodman-Bacon (2021). The red circles denote effects estimated based on comparisons across states treated earlier versus later, the green and blue circles denote effects based on comparisons with never-treated and always-treated states respectively, and the orange and purple circles denote effects based on comparisons across always- versus never- treated states and within-state comparisons respectively. These plots were heavily influenced by the default plots built by Goodman-Bacon, Goldring and Nichols (2019), which was used to estimate coefficients.

Figure B.13: Goodman-Bacon decomposition coefficients plotted against timing of treatment



Notes: This decomposition calculates treatment effect estimates based on comparisons across different treatment and control groups, along with weights that specify how much each treatment effect contributes to the overall difference-in-differences estimate Goodman-Bacon (2021). The coefficients are plotted with their magnitude on the y-axis and the associated treatment year group on the x-axis. The size of the points represents the associated weight and the different colors represent the various control groupings; see Figure B.12 for further description.