Fleet Use Implications of Policies that Affect Light-Duty Vehicle Scrappage

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Abstract

The social welfare implications of many transportation policies – including fuel economy standards, vehicle subsidies, and vehicle inspection programs – involve changes in fleet-wide vehicle travel caused by policy-induced changes to used vehicle scrappage (e.g.: the Gruenspecht effect). While prior literature has estimated the vehicle price elasticity of scrappage, the effect of scrappage on fleet travel distance is assumed in prior analyses and has not been empirically estimated. We exploit the staggered removal of state-wide safety inspection programs across the US from 1970 to 2017 as an instrumental variable to estimate the elasticity of fleet travel distance with respect to policy-induced scrappage. Using a difference-in-difference model, we estimate that removal of safety inspections causes a 4% [95% CI: 0.7% to 7.9%] average increase in vehicle registrations. In a two-stage least-squares regression, we estimate the average elasticity of fleet travel distance and find that we can reject a common assumption in the literature that the marginal drop in fleet travel distance per vehicle scrapped is equal to average distance traveled per vehicle (elasticity of 1). We can also reject at the 94% confidence level the implied elasticity (0.54) assumed by the U.S. National Highway Traffic Safety Administration and Environmental Protection Agency's recent analysis of the light-duty fleet fuel economy and greenhouse gas standards. These results imply that effects of policies on travel-related externalities via the Gruenspecht effect or other vehicle scrappage effects may be smaller than assumed in prior analysis.[i]

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1 Introduction

Since the seminal work by Gruenspecht [1], it has been recognized that policies affecting the scrappage patterns of used vehicles can have a significant impact on social welfare via the additional travel of these vehicles and associated externalities, including emissions, accidents, and congestion [2]. Such policies include fuel efficiency standards, subsidies for new vehicle technologies, vehicle buy-back programs such as "cash-for-clunkers", vehicle safety and emission inspection programs, and any other policy that significantly impacts vehicle resale prices, operating costs, or cost of scrappage.

As one example, the U.S. National Highway Traffic Safety Administration (NHTSA) and U.S. Environmental Protection Agency (EPA) recently proposed a rollback to the Corporate Average Fuel Economy (CAFE) standards and corresponding light-duty fleet greenhouse gas (GHG) emissions standards, proposing to hold both standards constant after model year (MY) 2020 in place of their existing schedule. Their analysis justified the change in large part based on estimated lives saved due to the Gruenspecht effect: lower new vehicle prices cause price reductions for used vehicles, resulting in accelerated scrappage, reduced vehicle travel, and reduced externalities associated with vehicle travel [3]. Key assumptions implicit in the analysis are: (1) the vehicles selected for early scrappage are driven identically to other vehicles with the same attributes, and (2) when used vehicles are scrapped early, all associated vehicle travel is forgone and there is no shift of travel to other vehicles or modes [4]. A similar "identical service stream" [1, p.328] or "continual service flow" [5, p.1101] assumption – that a vehicle's additional mileage from delayed scrappage is conditional only on its attributes – is also a common assumption in the scrappage literature [1, 5].

In practice, we may expect fleet travel distance to be a concave function of fleet size; the travel behavior of vehicles selected for early or delayed scrappage may systematically differ from other vehicles; and/or some of the travel associated with scrapped vehicles may shift to other vehicles, so it is important to empirically investigate the effects of policy-induced changes to scrappage on fleet travel distance.

Much of the literature related to fleet travel distance has focused on the direct rebound effect (i.e. how changes in operating costs affect usage [6]). Many empirical measures of the rebound effect in the U.S. have been identified at the state and/or national level [7–10] and at the vehicle-level [11, 12]. However, this body of literature does not address the impact that shifts in scrappage may have on driving behavior. We focus our study on the latter.

Specifically, we estimate the elasticity of fleet travel distance associated with policy-induced changes to vehicle scrappage and compare our estimate against assumptions made in the scrappage literature as well as to agency analysis of new fuel efficiency standards. Empirical estimates of the price elasticity of scrappage are negative [13, 14], consistent with Gruenspecht's theory of scrappage [1]. However, the literature has not estimated the influence of scrappage on fleet travel distance, in part because of the difficulty of identifying changes in travel distance from exogenous shocks to vehicle scrappage. We leverage the mechanism Gruenspecht [1] and Parks [5] characterize in our identification strategy: when a vehicle's operating-value-less-repair-cost increases relative to scrappage value, and scrappage is delayed at the margin. We exploit the staggered removal of state safety inspections (thereby lowering or delaying repair costs, as well as reducing frequency/saliency of the scrappage decision – both of which act to delay vehicle scrappage) across the US from 1970-2017 as an instrumental variable to estimate the shift in state-wide vehicle registrations caused by the policy change and its effect on fleet travel distance.

Using a difference-in-difference model, we find that removal of safety inspections increases vehicle registrations by 4.3% [95% confidence interval: 0.72% to 7.89%]. Using the policy change as an instrument in a two-stage least squares model, we estimate an average elasticity of fleet travel distance with respect to policy-induced changes in vehicle registrations of -0.3 [95% AR confidence set: -4.4 to 0.6]. While our estimate is uncertain, we are nevertheless able to reject the assumption that marginal mileage is equal to average mileage (elasticity = 1), as well as weakly reject (at the 90% confidence level) the implicit elasticity of fleet travel distance from delayed scrappage in agency analysis of the CAFE and GHG light-duty fleet standards.

2 Identification Strategy

Both Gruenspecht's and Parks' model of scrappage pose that an owner will scrap vehicle i in time t if repair costs r_{it} exceed the difference between operational value v_{it} and scrappage value s_{it} [1, 5].

$$v_{it} - s_{it} < r_{it} \tag{1}$$

We use this inequality, to inform an instrument that can be used to exogenously shift fleet size by altering a term in said inequality - repair costs. At different times throughout the past century, many states and the District of Columbia (DC) have opted to introduce mandated vehicle safety inspection programs [15]. Some states have kept their safety inspection programs in place, and others have since removed the programs due in large part due to various budgetary and effectiveness concerns [15, 16]. Within the time period of data available under our preferred specification (1970-2017), there are 19 states that never adopted safety inspection programs, 16 implemented and never repealed the safety inspection programs, and 16 that implemented and removed safety inspection programs (Utah changed safety inspection requirements as of 2018 [17] but isn't deemed a "treated" state because our data only extends to 2017). Because the removal of programs reduces both the saliency of the scrappage decision and the cost of maintenance, the

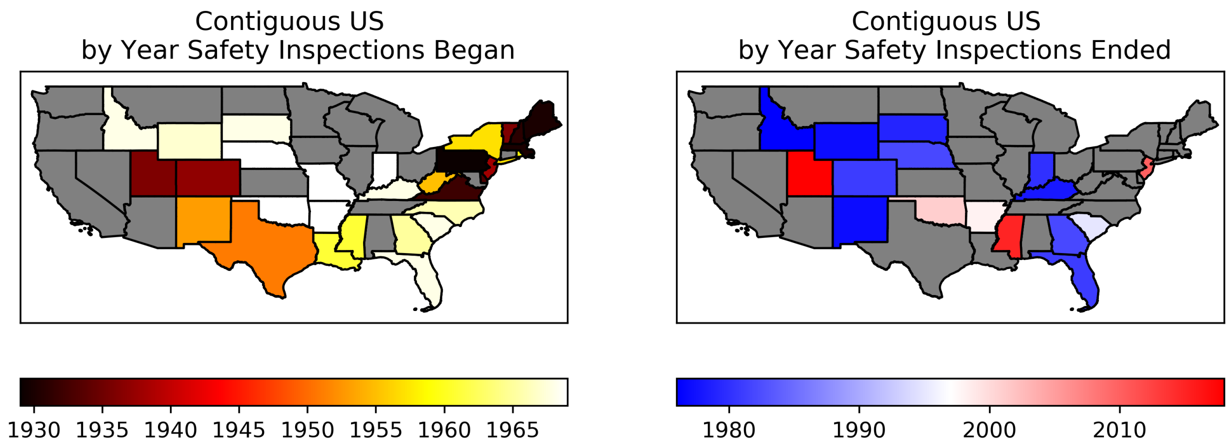

Figure 1: Safety inspections in the contiguous forty-eight U.S. states, including the year in which state-wide vehicle safety inspections began (left panel) and ended (right panel). States colored grey in the left and right panel did not began or end safety inspections, respectively, within our dataset. Alaska has never had safety inspections, and Hawaii began safety inspections in 1961 and they have yet to end [15]. The District of Columbia had safety inspection from 1939 to 2009 [15]. Program years come from the U.S. Government Accountability Office [15]. Utah removed safety inspections in 2018 [17]. State-level shape files are from the U.S. Census Bureau [18].

removal of these policies is expected to lead to a decrease in scrappage and a corresponding increase in vehicle registrations. Table 1 summarizes costs associated with inspections and inspection-related costs (including repairs). We use these policies to estimate (1) changes in fleet size (vehicle registrations) due to the policy removal and (2) marginal changes in fleet distance traveled in response to these changes in vehicle registrations.

Table 1: Summary statistics of 3.86 million records from the 2007-2016 Pennsylvania e-Safety inspection costs (more information in Fig. 5). Inspection-related costs refer to all recorded costs not denoted as a non-inspection cost for safety inspection visits (including repairs and sales tax). Units are nominal dollars.

	Mean	Std. Dev.	Median	95th Percentile	99th Percentile
Inspection-Related Costs	$87	$310	$30	$424	$957
Inspection Costs	$25	$160	$23	$41	$84

We present a two-stage least squares model to measure the elasticity of fleet travel distance in response to policy-induced delayed scrappage. In the first stage we estimate a difference-in-difference (DiD) model to estimate the causal effect of removing safety inspection programs on vehicle registrations. In the second stage, we use estimated scrappage effects to estimate the percentage change in fleet travel distance associated with a percentage increase in total registrations due to policy-induced reduced scrappage. We test this model structure under various specifications with various levels of parsimony as well as perform robustness checks and auxiliary analysis to provide evidence supporting a causal interpretation.

Table 2: Specifications and their respective controls

Variable	Specification Spec. 1	Spec. 2	Spec. 3
Log Average Fuel Price		Yes	Yes
Log Employment		Yes	Yes
Log Licensed Drivers		Yes	Yes
Log (1+Metro Income)			Yes
Log (1+Metro Population)			Yes
Log (1+Non-Metro Income)			Yes
Log (1+Non-Metro Population)			Yes
Log Road Mileage			Yes
Log State GDP			Yes
Log Total Income		Yes	
Log Total Population		Yes	
Colorado Dummy 2002-2009	Yes	Yes	Yes
Colorado Dummy 2010-2017	Yes	Yes	Yes

Note: Colorado dummies are included because of a known change vehicle registration reporting that occurred in 2010 (known via a source from U.S. Department of Transportation) and led to a shift in total registrations by roughly 2.7 million vehicles. Because we see a similar shift in reported 2002 Colorado vehicle registrations, we assume this too is a change in reporting practices. We then allow for two dummy variables to control for the two discontinuities in data.

2.1 First Stage Model

In our first stage, we estimate a traditional DiD model:

$$\log(r_{iy}) = \eta p_{iy} + \boldsymbol{\alpha}'\mathbf{x}_{iy} + \psi_i + \gamma_y + \xi_i \cdot y + \varepsilon_{iy} \tag{2}$$

In this equation, we regress the log-transformed vehicle registrations r_{iy}, indexed by state i and year y on treatment and several control variables. The treatment p_{iy} is an indicator denoting whether state i did *not* have safety inspections in year y (treatment = removal of inspections). The coefficient of interest is η, which estimates the percent change in vehicle registrations due to removal of safety inspections. The vector $\mathbf{x}_{iy}$ denotes a list of control variables that is dependent upon the specification being estimated (see Table 2). The specifications were chosen span various levels of parsimony. Fixed effects for both state ψ_i and year γ_y are included, and we allow for state-level linear time trends $\xi_i \cdot y$. These features, respectively, control for unobserved, time-invariant heterogeneity among states, national-level trends by year, and within-state linear trends that controls might not capture.

2.2 Second Stage Model

Our second stage uses estimated log vehicle registrations from Eq. (2) to predict log fleet travel distance v_{iy}.

$$\log(v_{iy}) = \beta\widehat{\log(r)}_{iy} + \boldsymbol{\delta}'\mathbf{x}_{iy} + \phi_i + \zeta_y + \omega_i \cdot y + \epsilon_{iy} \tag{3}$$

The coefficient of interest β represents the elasticity of fleet travel distance in response to policy-induced delayed scrappage. We employ the same controls $\mathbf{x}_{iy}$ used in Eq. (2), as well as state fixed effects ϕ_i, time fixed effects ζ_y, and linear time trends $\omega_i \cdot y$.

2.2.1 H_0 Decision

We adopt null hypotheses representative of assumptions made in the prior literature and in policy analyses. As mentioned, the prior literature on scrappage assumes that foregone fleet travel distance is proportional to vehicle scrappage, conditional on vehicle attributes [1, 5]. Lacking attributes of scrapped vehicles in the data, we interpret this assumption as a null hypothesis of $\beta = 1$, which assumes that the vehicles being scrapped contribute the same distance traveled as the fleet-wide average vehicle.

Additionally, we test a second null hypothesis from the 2018 EPA and NHTSA analysis of federal fleet standards by estimating the implicit elasticity assumed in the analysis by the attributes of vehicles with delayed scrappage. Since the 2018 EPA and NHTSA analysis of CAFE standards do not explicitly report an elasticity of fleet distance traveled from delayed scrappage [4], we calculate the implicitly assumed elasticity from the fleet travel distance, fleet size, and attributes of scrapped vehicles reported in the analysis. Specifically, we use the agencies' provided relationship between expected vehicle miles traveled (VMT) of an individual vehicle based on its attributes to calculate a counterfactual of changes in vehicle travel if scrapped vehicles were to remain on the road. The counterfactual assumes all vehicles scrapped in the previous year were not scrapped and are instead driven at the same average rate of those vehicles of the same model year, regulatory class (passenger car or light truck), and fuel type (gas, diesel, electricity, etc.) that are still in the fleet. We then take the log-transformed total VMT and fleet size of the respective model estimate and counterfactual estimate and compute the ratio of the differences between them:

$$H_0 : \beta_y = \frac{\Delta \log(v_y)}{\Delta \log(n_y)} = \frac{\log\left(\sum_{m\in\mathbb{M}}\sum_{c\in\mathbb{C}}\sum_{u\in\mathbb{U}} v_{mcuy}\right) - \log\left(\sum_{m\in\mathbb{M}}\sum_{c\in\mathbb{C}}\sum_{u\in\mathbb{U}} v_{mcuy}\left(1+\frac{s_{mcuy}}{n_{mcuy}}\right)\right)}{\log\left(\sum_{m\in\mathbb{M}}\sum_{c\in\mathbb{C}}\sum_{u\in\mathbb{U}} n_{mcuy}\right) - \log\left(\sum_{m\in\mathbb{M}}\sum_{c\in\mathbb{C}}\sum_{u\in\mathbb{U}} n_{mcuy} + s_{mcuy}\right)} \quad (4)$$

where n_{mcuy} is the number of vehicles in the fleet of model year m, regulatory class c, and fuel type u during calendar year y, v_{mcuy} is the fleet travel distance of the given vehicle group in year y, and s_{mcuy} denotes the number of vehicles scrapped within the given vehicle group between calendar year y and calendar year $y-1$. $\mathbb{M}, \mathbb{C}$, and $\mathbb{U}$ denote the sets of model years, regulatory classes, and fuel types, respectively. Fig. 2 plots the resulting elasticities implied in the agencies' analysis across calendar years 2017-2050. These calendar years were chosen as they are the years officially reported in the preliminary regulatory impact analysis (PRIA) [4, p.1413]. Taking the mean of the calculated values over years 2017 to 2050 results in an assumed

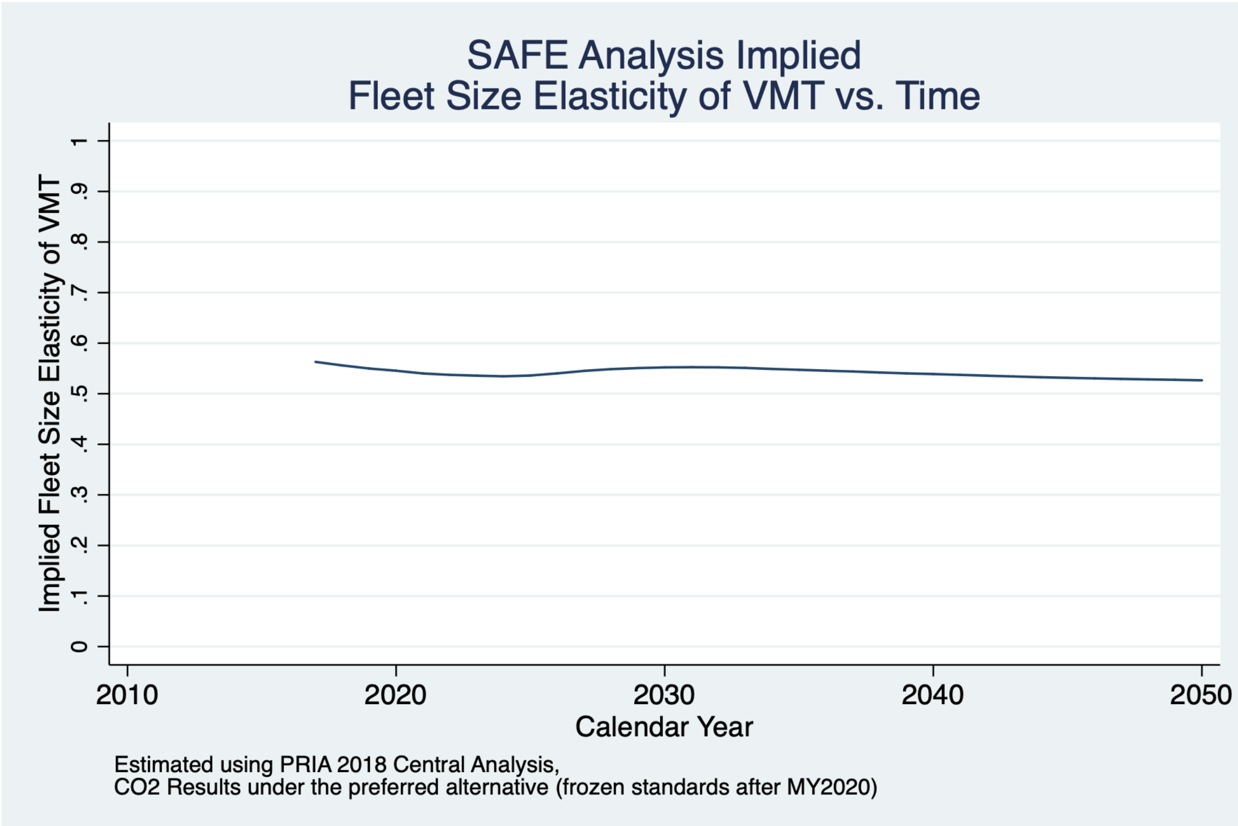

Figure 2: Estimated implicit elasticity from Eq. (4). Equation arguments come from the EPA, NHTSA 2018 central analysis using the CO_2 results under the preferred alternative [19].

null hypothesis that $\beta = 0.5415$. We use this value as our second null hypothesis.

3 Data

Data sources are all publicly available from either the U.S. Federal Highway Administration's (FHWA) Highway Statistics Series [20], the Bureau of Economic Analysis's (BEA) Regional Economic Accounts [21], the U.S. Energy Information Administration's (EIA) State Energy Data System [22], or the U.S. Bureau of Labor Statistics's (BLS) Consumer Price Index Database [23]. In Table 3, we outline the specific tables from these databases and exact sources from which we collected the data. From these sources, we have collected a panel dataset from 1967-2017 for all 50 states and the District of Columbia. These data allows us to (1) measure our variables of interest over time at annual and state levels and (2) control for various economic and road mileage changes over time that differ across states.

Vehicle registrations are reported at the state level [24]. With respect to vehicle registrations, we know through a source at the U.S. Department of Transportation that there was a shift in vehicle registration reporting practice that led to an artificial gain of roughly 2.75 million vehicles in Colorado in 2010, and we

Table 3: Primary data collected and their sources.

Data	Years Collected	Source	Table(s)	Issue Year
All Items CPI-U	1913-2019	BLS	CUUR0000SA0, CUUS0000SA0	
Average Motor Gasoline Price	1970-2017	EIA	Motor Gasoline Prices and Expenditures, 1970-2017 (Prices table)	
State-Wide Employment	1969-1997	BEA	SAEMP25S	
State-Wide Employment	1998-2017	BEA	SAEMP25N	
Gas CPI-U	1935-2019	BLS	CUUR0000SETB01, CUUS0000SETB01	
State-Wide Licensed Drivers	1967-2017	FHWA	DL-201	2017
State-Wide Metro Income	1969-2018	BEA	MAINC1	
State-Wide Metro Population	1969-2018	BEA	MAINC1	
State-Wide Non-Metro Income	1969-2018	BEA	MAINC1	
State-Wide Non-Metro Population	1969-2018	BEA	MAINC1	
State-Wide Road Mileage	1980-2017	FHWA	HM-220	2018
State-Wide Road Mileage	1967-1979	FHWA	M-1	1967-1979
State GDP	1997-2018	BEA	SAGDP2N	
State GDP	1963-1996	BEA	SAGDP2S	
State-Wide Total Income	1960-2018	BEA	SAINC1	
State-Wide Total Population	1960-2018	BEA	SAINC1	
State-Wide Vehicle Registrations	1967-1993	FHWA	MV-201	Summary to 1995
State-Wide Vehicle Registrations	1994-2017	FHWA	MV-1	1994-2017
State-wide VMT	1980-2017	FHWA	VM-202	2018
State-wide VMT	1967-1979	FHWA	VM-2	1967-1979

Note: All currency-based data except Average Motor Gasoline Price have been transformed to $2018 using the All Items CPI-U from BLS. Average Motor Gasoline Price has been transformed to $2018 using the Gas CPI-U from BLS. If available, the most up-to-date version of FHWA tables within issue year is used.

observe a similarly sized drop in 2002 (see Table 2 for more information). We control for these changes in reporting using dummy variables.

4 Results

4.1 First Stage Results

The results for the DiD study can be found in Table 4, where the coefficient corresponding to "Treatment" denotes the average percent change of state vehicle registrations due to removal of vehicle inspection policy. Note, our dataset would allow for year 1967-1970 under Spec. 1, but we use years 1970-2017 consistently for all specifications to prevent conflation when adding safety inspections programs, as many states began safety inspection programs in the late 1960s [15]. Our preferred specification is Spec. 3, as it includes controls for state-specific changes in roads built over time as well as differential controls for metro and non metro area population and income, which may have distinct mileage responses. Removal of safety inspections led to a roughly 4% increase in vehicle registrations under our preferred specification.

Table 4: Diff-in-Diff Results

Log VMT	(1) Spec. 1 b/se	(2) Spec. 2 b/se	(3) Spec. 3 b/se
Treatment	0.056	0.039*	0.043**
	(0.035)	(0.019)	(0.018)
Log Population		0.376	
		(0.248)	
Log Total Income		0.257**	
		(0.107)	
Log Mean Gas Price		-0.027	-0.030
		(0.067)	(0.064)
Log Employment		-0.038	-0.079
		(0.145)	(0.166)
Log Licensed Drivers		0.163**	0.166**
		(0.066)	(0.069)
Log (1 + Metro Population)			0.405
			(0.274)
Log (1 + Non-Metro Population)			-0.050
			(0.248)
Log (1 + Metro Income)			0.233*
			(0.137)
Log (1 + Non-Metro Income)			0.061
			(0.080)
Log State GDP			0.019
			(0.066)
Log Road Mileage			0.076
			(0.078)

Observations	2448	2448	2448
Effective F-Stat	2.607	3.972	5.832

* p <0.1, ** p <0.05, *** p <0.01

Note: All models include state and year fixed effects as well as state-level time trends. All models cluster standard errors with respect to states.

4.2 Second Stage Results

Our instrument appears to be weak, as measured by the reported effective (Montiel Olea Pflueger) F-statistics[ii] [26]. This statistic is used because it is robust to clustered standard errors [26]. Hence, we conduct an analysis for our second stage results that is robust to weak instruments, following Andrews, Stock, and Sun's recommendations when Effective F-statistics of the first stage are less than 10 [27].

We perform hypothesis testing that is robust to weak instrument bias using the Anderson-Rubin χ^2 statistic [28]. The Anderson-Rubin (AR) test was chosen for its desirable properties in the just-identified case that Andrews, Stock, and Sun review [27]. 2SLS estimates of β, as well as tests of β from Eq. (3) with respect to null hypotheses $H_0 : \beta = 1$ and $H_0 : \beta = 0.5415$, are summarized in Table 5[iii] for our preferred specification (full results available in Appendix B.3). Note, that the statistic does not report confidence intervals, but rather confidence sets. Confidence sets are made using test inversion over a grid of potential parameter values [31]. In essence, this involves testing an array of points on a grid of null-hypotheses and identifying those that fail to reject the null hypothesis at a given significance level [27]. A figure[iv] illustrating this can be seen in Fig. 3.

Table 5: Second Stage Results

Log VMT	(1) Spec. 3
Log Registrations (β)	-0.311
$\mathbf{H_0} : \beta = 1$	
AR-χ^2	5.527
P-Value	0.0187
$\mathbf{H_0} : \beta = 0.5415$	
AR-χ^2	3.655
P-Value	0.0559
AR Confidence Set (95%)	[-4.44058, .572629]
Range of Points Tested	[-5, 1]
Number of Points in Range	2500
Grid Resolution	0.00240

[ii] Effective F-statistics are calculated using the *weakivtest* Stata package [25]

[iii] Results are initially estimated using the *ivreg2* Stata package [29] and then the *weakiv* Stata package [30] (an extension of the *rivtest* package [31]) is used to robustly test the null hypotheses.

[iv] Constructed using the *weakiv* Stata package [30]

Note: All models include state and year fixed effects as well as state-level time trends.
All models cluster standard errors with respect to states.
... denotes that no bound was found within the range of points tested during test inversion

From the results, we can see that the estimated elasticity of fleet travel distance from delayed scrappage is between -4.441 and 0.573 under the preferred specification at the 95% confidence level. This confidence set shows that we cannot distinguish, within statistical uncertainty, whether the estimated elasticity is positive and small, or negative. A negative elasticity would imply that fleet travel distance decreases with increasing vehicle registrations (due to reduced scrappage), which could plausibly occur if, for example, delayed scrappage prevents trips from being transferred to other, newer vehicles that may be driven more because of the rebound effect. However, such an effect would likely be small given rebound estimates in the literature [7–12]. We anticipate that the large lower bound of the 95% confidence set is a result of statistical uncertainty from the small sample associated with state-level treatment. The upper bound of the 95% confidence set implies that fleet travel distance increases when registrations increase due to delayed scrappage, but at 57% the rate of average VMT per vehicle.

Using the Anderson-Rubin χ^2 statistic, we can reject the null hypothesis that $\beta = 1$ at the 95% level. The alternative null hypothesis that $\beta = 0.5415$ is rejected at the 94% level.

4.3 Threats to Identification and Robustness Checks

4.3.1 Parallel Trends

A key assumption in interpreting DiD results as causal is the assumption of parallel trends. Appendix B.1 shows that the residuals are noisy after controlling for the various specifications; however, the residuals do not exhibit any clear trends. As a robustness check, we estimate an event study surrounding the removal of safety inspections by state to determine whether or not we can identify a similar post-treatment shift without assuming parallel trends. The event study has a similar structure to Eq. (2) with the policy indicator replaced by a variable d_{iyt} that indicates whether or not state i in year y is t years after the event (inspection program removal).

$$\log(r_{iyt}) = \sum_t \eta_t d_{iyt} + \boldsymbol{\alpha}'\mathbf{x}_{iy} + \psi_i + \gamma_y + \xi_i \cdot y + \epsilon_{iy} \tag{5}$$

For data coverage reasons (discussed in Appendix A), we estimate the parameters for this equation when t exists on two domains: $t \in \{-6, -5, \ldots, 6\}$ and $t \in \{-11, 10, ..., 11\}$, using $t = -1$ as the reference year.

Results, summarized in Fig. 4, show no evidence of pre-trends, as all associated coefficients are not distinguishable from zero, and a post-trend increase in registrations maturing after a couple of years at

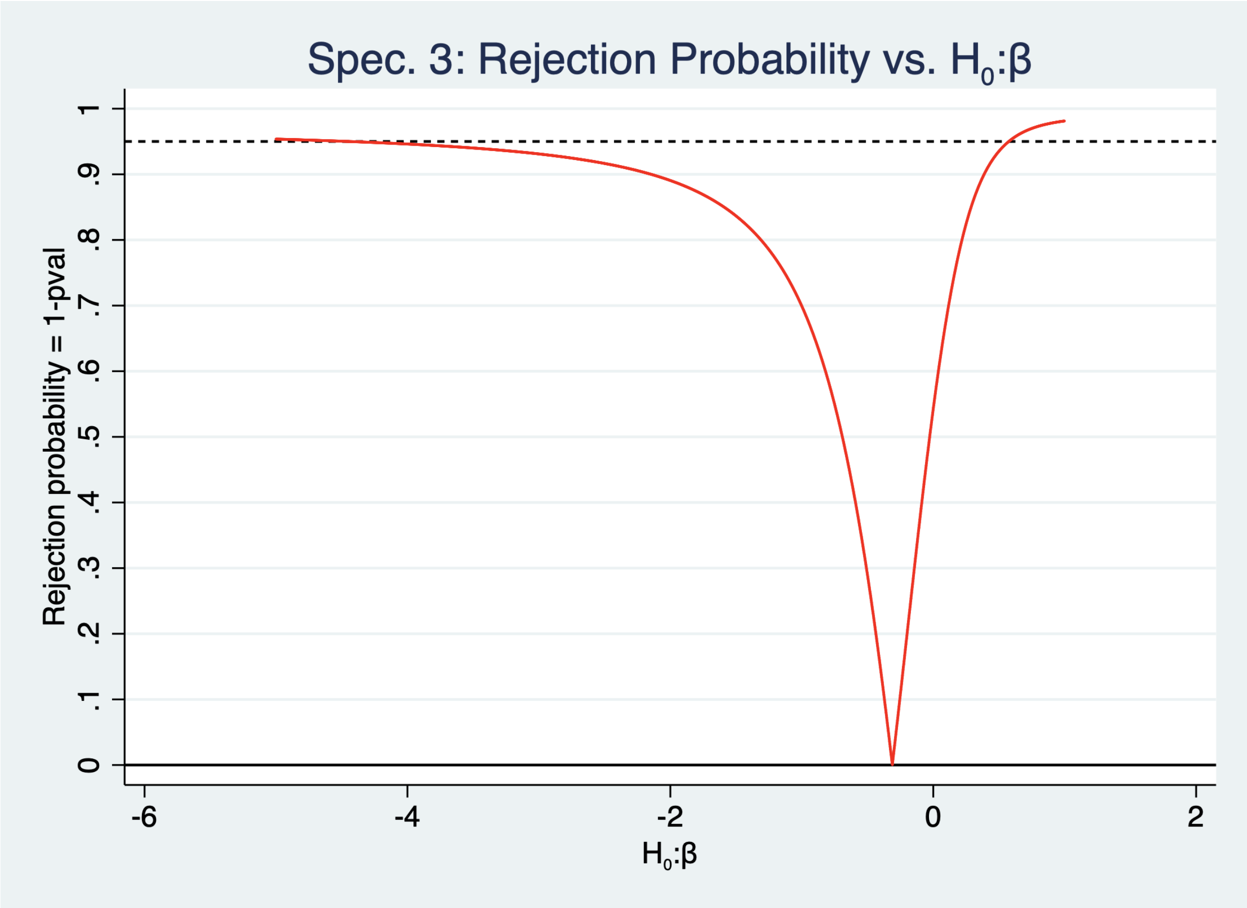

Figure 3: Rejection probabilities under various null hypotheses for the Spec. 3 version of our 2SLS model.

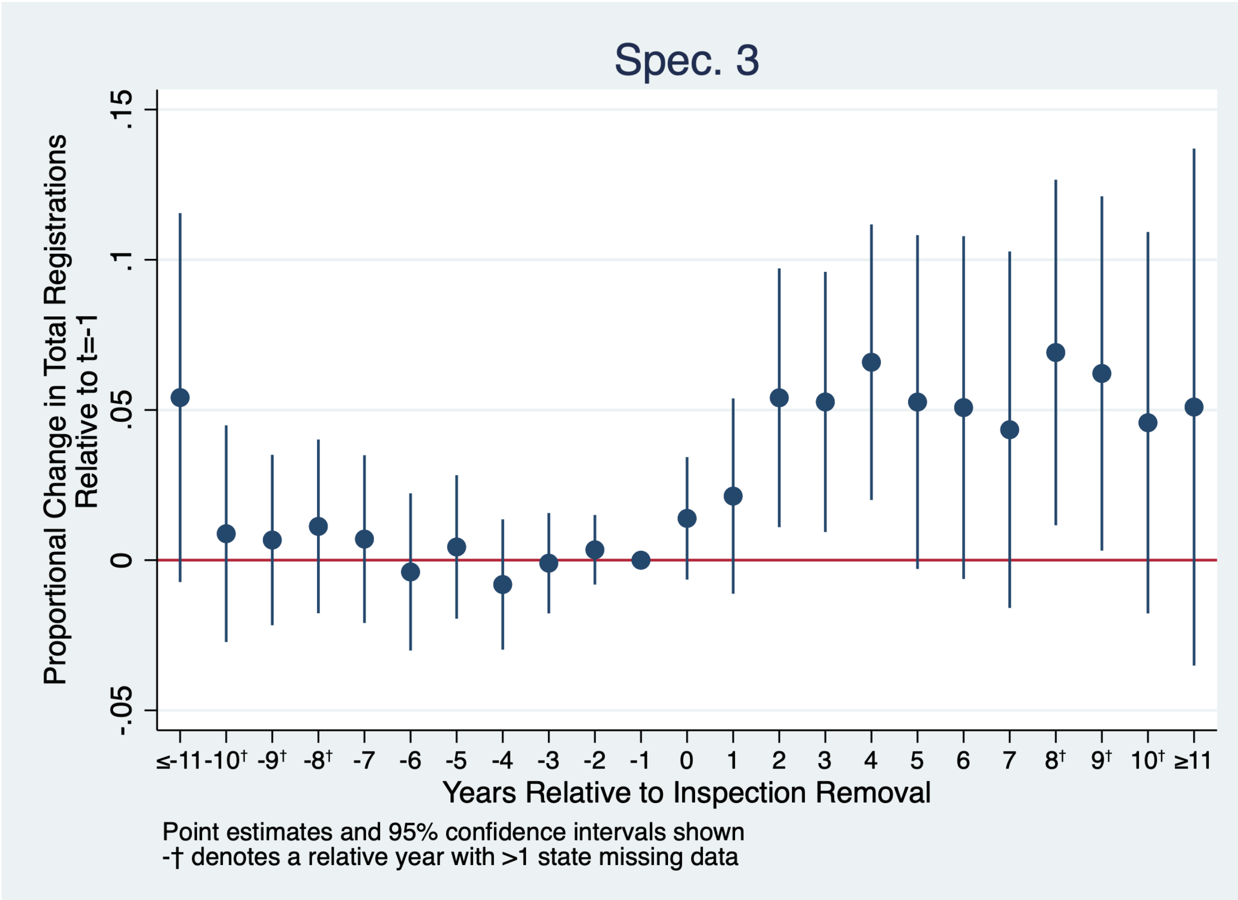

Figure 4: Event study results showing estimates of η_t values from Eq. (5) and their respective 95% confidence intervals estimated from Eq. (5) under our preferred specification and $t \in \{-11, -10, \ldots, 11\}$, using $t = -1$ as the base year. Data exist for all treated states in $t \in \{-6, -5, \ldots, 2\}$ and for no less than 15 of 16 treated states in $t \in \{-7, -6, \ldots, 7\}$. Note, -1 not included in the sets to establish baseline at the year prior to the event.

around 5%, which is consistent with our DiD findings. The two-year maturity period is consistent with inspections largely being required on an annual/biennial basis [15, 16]. We see comparable results across all versions of the event study estimates, which can be found in Appendix B.2.

4.3.2 Exogeneity of Treatment

If the repeal of state inspections is correlated with unobserved effects on registrations, our estimates will be biased. We include state-level variations over time in gas prices, licensed drivers, employment, GDP, and metro population and income relative to non-metro areas as well as state and year fixed effects to help control for unobserved effects. As discussed in Section 2, removal of these inspections varied across states from as early as the late 1970s to 2017. Early repeals of safety inspection programs were largely due to certain state funds no longer being tied to highway safety programs under the Highway Safety Act of 1976, a lack of ability to properly administer the programs, and a lack of evidence of their effectiveness in improving traffic safety [16]. More recent repeals have cited the budgetary concerns, and the lack of evidence of improvements on traffic safety [15]. None of these rationale are explicitly in response to either vehicle purchasing or driving behaviors of citizens, which provides support for the exogeneity of the event. Further, our event study, described above, finds statistically significant changes in vehicle registrations after but not before the event, suggesting that reverse causality is not a concern.

4.3.3 Exogeneity of Instrument

Necessary for the consistency of our results is the assumption that our instrument (the removal of safety inspections) does not affect fleet travel distance except through vehicle registrations. A concern regarding this assumption could be the instrument's impact on a household's fuel budget via reduced expenditure on inspection-related repairs or increased expenditure on repairs due to increased accidents. There is incongruous/limited evidence of safety inspection effectiveness with respect to reduced crashes/fatalities [15,16,32–35]. Therefore, we believe that this mechanism would not be cause for concern. For inspection-related repairs, data from Pennsylvania state safety inspections records show that, within the given sample, inspection costs are largely less than $30 (nominal $, see Fig. 5 and Table 1). Assuming this expenditure is eliminated and spread according to vehicle fuel expenditure's average share of after-taxed income, then roughly 3%-5% of these savings would go to vehicle fuel based on 2013-2018 average expenditure from the BLS Consumer Expenditure Survey [36]. This would result in approximately $1-$1.50 more spent on fuel per household, which would further be spread out across an average 1.9 vehicles in the household [36]. Because of these rather low average individual-level costs, the low cost of inspections themselves [15, 16], and the long time horizon between events (generally annually or biennially [15, 16]), we don't believe this mechanism is a particularly strong threat to exogeneity.

4.3.4 Strong Instruments in the Population

Although our analysis is robust to weak instruments in our data sample, an identifying assumption is that the instrument is strong in the population [27]. Therefore, an identifying assumption is that the safety inspections affect vehicle registrations. Although safety inspections are a small cost for vehicle households on average, the tail on the histogram shown in Fig. 5 shows that costs for some vehicle owners are hundreds or thousands of dollars. A dataset used by Jacobsen, van Benthem in their 2015 paper on paper shows that the average vehicle value (likely an upwards-biased statistic due to value being positively bounded) is less than $5,000 after age 10 and closer to $2,000 by the vehicle's late teen years [13]. This would mean that these repair costs could account for a large portion of vehicle value for a vehicle on the margin, potentially invoking the decision to scrap. Therefore, we expect that our instrument is strong in the population.

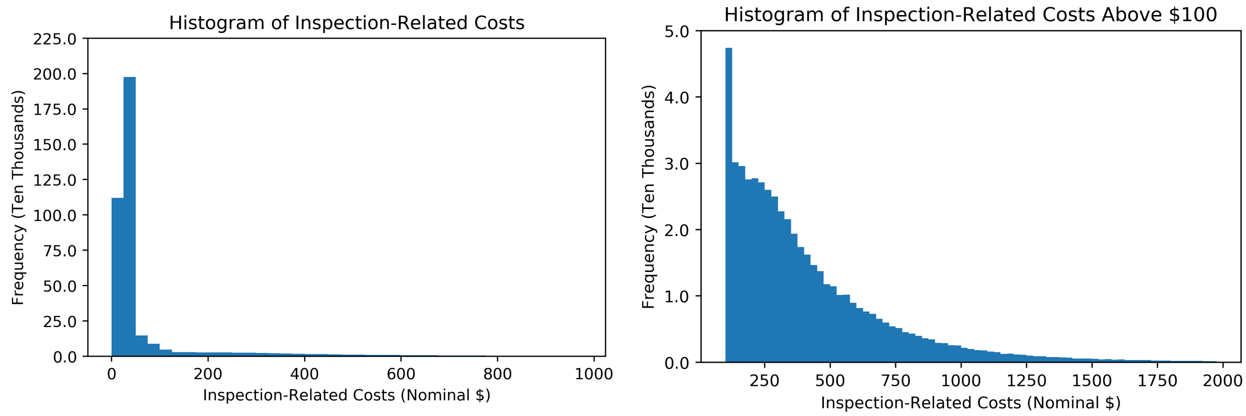

Figure 5: Data from 2007-2016 e-Safety inspection in Pennsylvania provide a sample of safety inspection costs. Values shown are all recorded costs not denoted as a non-inspection cost for safety inspection visits (repair costs and sales tax included). Costs have not been adjusted for inflation. Data points with encoding errors were not included (20 entries) as well as all data where inspection costs were larger than $50,000. This limit was chosen as it was deemed an unreasonably large repair transaction and most likely a data entry error (34 entries). We also remove entries with vehicle identification numbers (VINs) that don't satisfy the VIN check digit condition, contain disallowed characters, or don't have 17 digits (requirements outlined in [37]; 109,244 entries). For more information on the data, see Peck, et al. 2015 [38]

4.3.5 Equilibrium Shifts

Another potential threat to identification is the equilibrium shift this mechanism causes in vehicle markets. We show that removal of safety inspection programs increase vehicle registrations in each of these states. As the number of (presumably used) vehicles increases, it puts downward pressure on used vehicle prices, and because used vehicles are substitutes for new vehicles, this would cause downward pressure on new vehicle prices, increasing their sales. Because we are only capturing net changes in fleet via total registrations, this described chain of events would result in a smaller change to fleet size between years, as new vehicles "cancel out" signal from the scrapped vehicles. Assuming that new vehicles are driven more than used vehicles, the mechanism would also result in a larger change in fleet distance traveled. Combined, the data would result in a larger change in distance traveled per change in fleet size. This, we believe, would lead to an overestimate of our estimated elasticity β, making our estimate conservative. However, it is also plausible that the policy change could induce other changes to fleet composition, including changes to the types of vehicles owned and their efficiencies.

4.3.6 Potentially Non-Comparable Mechanism

Although we believe our instrument is valid, there is an unresolvable identification concern that the mechanism we leverage is not the same mechanism that CAFE or other policies invoke. The Gruenspecht effect is based upon a change in new vehicle values altering used vehicle values through the substitution effect [1]. In this study, rather, we leverage a change in repair costs and the saliency of the scrappage decision to alter vehicle scrappage patterns. If there is a systematic difference between the types of vehicles affected by the Gruenspecht effect and the mechanism used in this study, these findings may not be directly applicable in the context of other vehicle policies.

5 Conclusions and Future Work

We estimate the impact of states removing safety inspection programs on vehicle scrappage and vehicle registrations, and we leverage that shift to estimate the elasticity of fleet travel distance with respect to policy-induced scrappage. We estimate that the removal of safety inspection programs in states leads to a 4% average increase in overall vehicle registrations and that the elasticity of fleet travel distance with respect to policy-induced changes in vehicle registrations is uncertain but likely lower than values $\beta = 1$ and $\beta = 0.5415$ assumed in prior analysis.

These findings are relevant to assessing social welfare implications of any policy that affects used vehicle scrappage (e.g.: via the Gruenspecht effect). They suggest that prior literature and policy analyses assuming that travel associated with average used vehicle groups is foregone when used vehicles are scrapped may substantially overestimate the vehicle travel externality implications of these policies.

Future work will include performing a battery of robustness checks, including randomized treatment, leave-one-out, and leave-multiple-out analyses.

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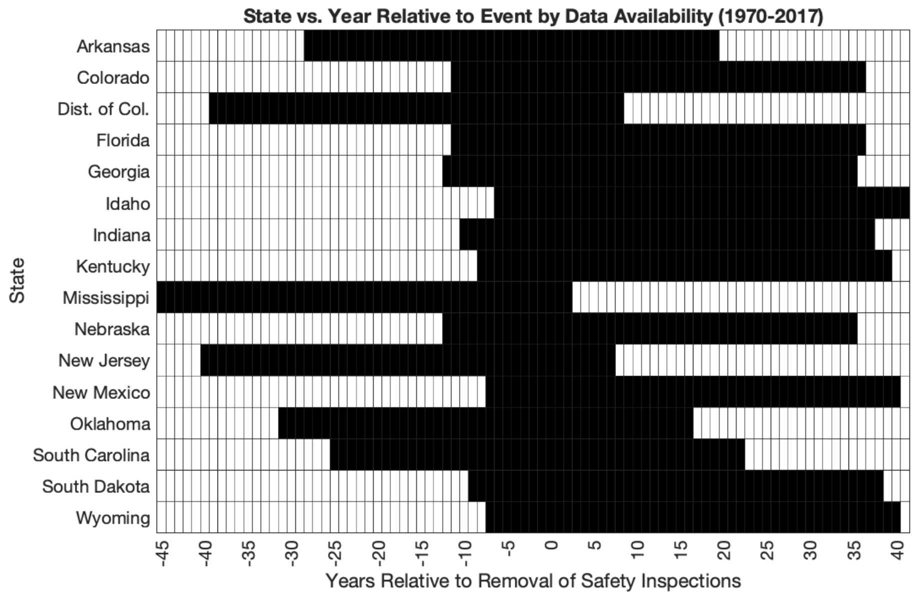

Figure A.1: Figure depicting data availability of treated states relative to the event time.

A Data Appendix

In Fig. A.1, we see the data availability for treated states surrounding the the removal of safety inspection standards. We can see that most data for the treated states exist roughly ± 10 years surrounding the events. For this reason we have chosen to sets of t for event study estimation: $t \in \{-6, -5, \ldots, 6\}$ and $t \in \{-11, -10, \ldots, 11\}$. We can also see the number states with data available relative to the year in which the event occurs in Fig. A.2.

B Model Appendix

B.1 Parallel Trends

In Fig. B.1, we see the true log-registration values less the predicted value of the diff-in-diff model under the respective specification. Although the data appears to be noisy, there also appears to be no particularly strong trends present that would indicate that would the parallel trends assumption is violated.

B.2 Full Event Study Results

Here, we show the results for an event study under the various specification both when $t \in \{-6, -5, \ldots, 6\}$ ("small" window) and $t \in \{-11, -10, \ldots, 11\}$ ("large" window). Note, -1 not included in the sets to establish baseline at the year prior to the event.

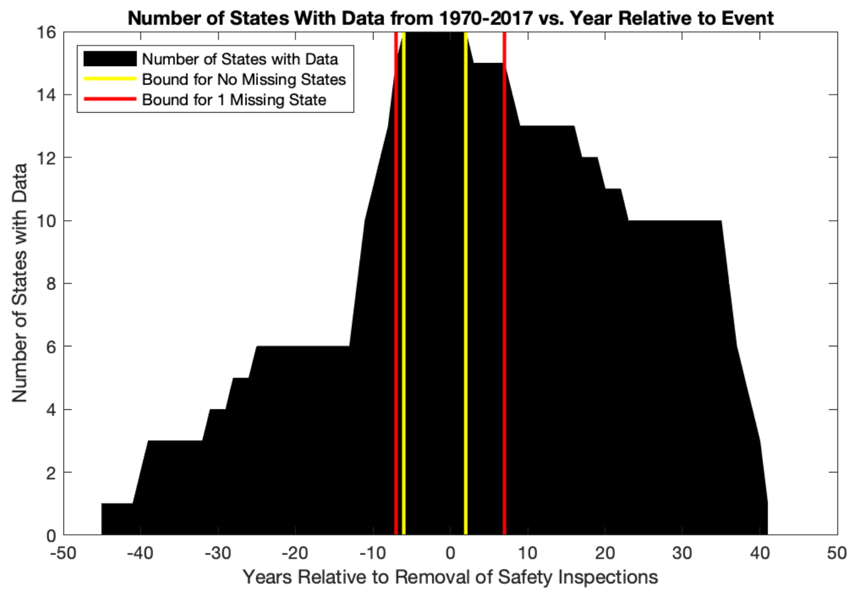

Figure A.2: Figure depicting number treated of states with data relative to the event time.

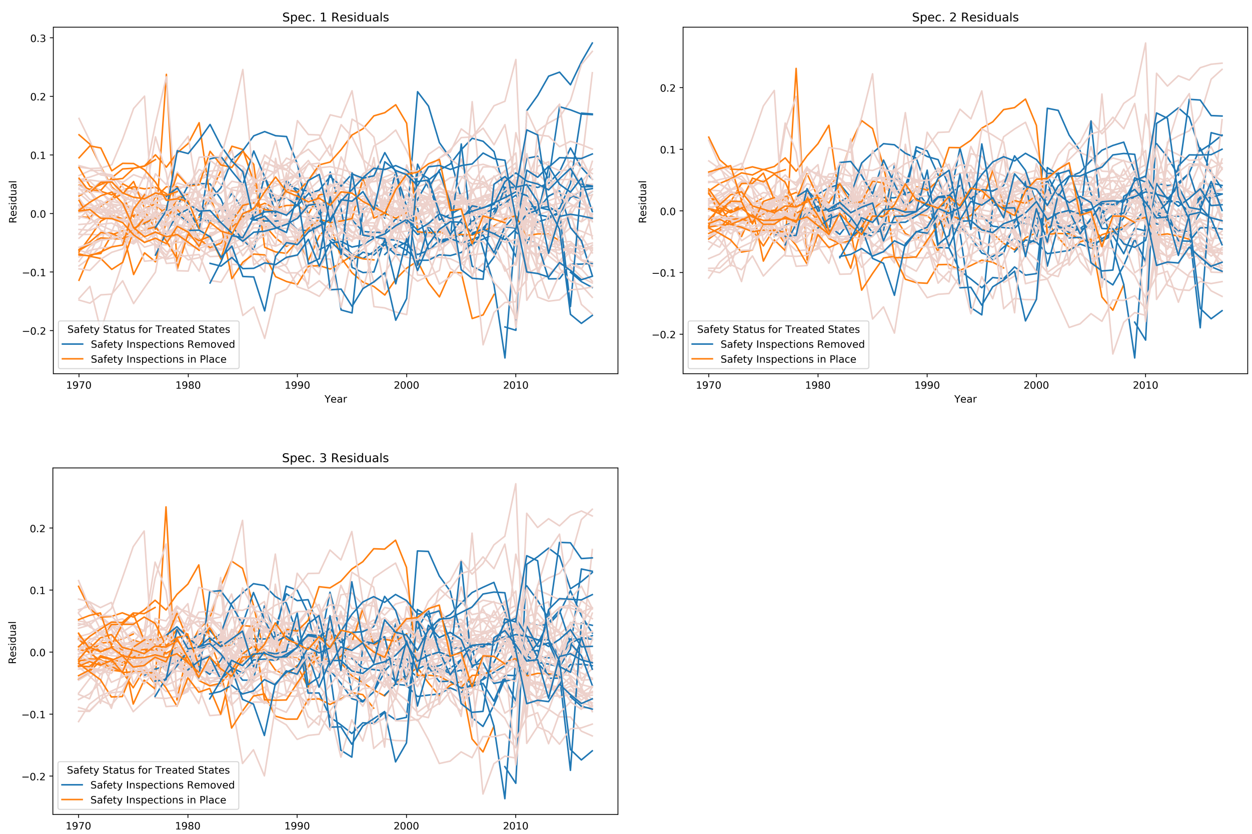

Figure B.1: True log-registration value less predicted values in the respective models.

Table B.1: Event Study Results Small Window

	(1)	(2)	(3)
	Spec. 1	Spec. 2	Spec. 3
	b/se	b/se	b/se
t $\leq$ -6	0.013	0.020	0.016
	(0.025)	(0.017)	(0.017)
t=-5	-0.008	0.005	0.003
	(0.015)	(0.012)	(0.012)
t=-4	-0.019	-0.007	-0.009
	(0.013)	(0.011)	(0.011)
t=-3	-0.009	-0.001	-0.002
	(0.011)	(0.008)	(0.008)
t=-2	0.001	0.003	0.003
	(0.006)	(0.006)	(0.006)
t=-1	0.000	0.000	0.000
	(.)	(.)	(.)
t=0	0.018**	0.014	0.014
	(0.009)	(0.010)	(0.010)
t=1	0.027*	0.022	0.022
	(0.014)	(0.016)	(0.016)
t=2	0.064**	0.054**	0.056**
	(0.028)	(0.021)	(0.021)
t=3	0.068**	0.053**	0.055**
	(0.032)	(0.022)	(0.022)
t=4	0.084**	0.067***	0.069***
	(0.035)	(0.024)	(0.023)
t=5	0.073*	0.053*	0.056**
	(0.042)	(0.028)	(0.028)
t $\geq$ 6	0.067	0.058*	0.061*
	(0.045)	(0.032)	(0.031)
Log Population		0.354	
		(0.248)	
Log Total Income		0.255**	
		(0.108)	
Log Mean Gas Price		-0.031	-0.033
		(0.067)	(0.064)
Log Employment		-0.021	-0.063
		(0.143)	(0.166)
Log Licensed Drivers		0.165**	0.167**
		(0.065)	(0.069)
Log (1 + Metro Population)			0.393
			(0.267)
Log (1 + Non-Metro Population)			-0.056
			(0.245)
Log (1 + Metro Income)			0.229*
			(0.135)
Log (1 + Non-Metro Income)			0.063
			(0.077)
Log State GDP			0.017

			(0.066)
Log Road Mileage			0.073
			(0.077)
Observations	2448	2448	2448

* p <0.1, ** p <0.05, *** p <0.01

Note: All models include state and year fixed effects as well as state-level time trends. All models cluster standard errors with respect to states.

Table B.2: Event Study Results Large Window

	(1) Spec. 1 b/se	(2) Spec. 2 b/se	(3) Spec. 3 b/se
t $\leq$ -11	0.057	0.059*	0.054*
	(0.040)	(0.032)	(0.031)
t=-10	0.005	0.013	0.009
	(0.030)	(0.019)	(0.018)
t=-9	0.009	0.009	0.007
	(0.024)	(0.014)	(0.014)
t=-8	0.016	0.017	0.011
	(0.021)	(0.015)	(0.014)
t=-7	0.001	0.012	0.007
	(0.019)	(0.014)	(0.014)
t=-6	-0.014	-0.001	-0.004
	(0.018)	(0.014)	(0.013)
t=-5	-0.006	0.006	0.004
	(0.015)	(0.012)	(0.012)
t=-4	-0.018	-0.006	-0.008
	(0.013)	(0.011)	(0.011)
t=-3	-0.008	-0.000	-0.001
	(0.011)	(0.008)	(0.008)
t=-2	0.002	0.003	0.003
	(0.006)	(0.006)	(0.006)
t=-1	0.000	0.000	0.000
	(.)	(.)	(.)
t=0	0.018**	0.014	0.014
	(0.009)	(0.010)	(0.010)
t=1	0.026*	0.021	0.021
	(0.015)	(0.016)	(0.016)
t=2	0.062**	0.053**	0.054**
	(0.028)	(0.022)	(0.021)
t=3	0.065*	0.051**	0.053**
	(0.033)	(0.022)	(0.022)
t=4	0.081**	0.064***	0.066***
	(0.036)	(0.024)	(0.023)
t=5	0.068	0.049*	0.053*
	(0.042)	(0.028)	(0.028)
t=6	0.066	0.047	0.051*

	(0.043)	(0.029)	(0.028)
t=7	0.057	0.040	0.043
	(0.047)	(0.030)	(0.030)
t=8	0.081	0.065**	0.069**
	(0.049)	(0.029)	(0.029)
t=9	0.059	0.059*	0.062**
	(0.040)	(0.030)	(0.029)
t=10	0.039	0.044	0.046
	(0.040)	(0.031)	(0.032)
t $\geq$ 11	0.053	0.050	0.051
	(0.055)	(0.044)	(0.043)
Log Population		0.356	
		(0.253)	
Log Total Income		0.252**	
		(0.109)	
Log Mean Gas Price		-0.046	-0.047
		(0.065)	(0.063)
Log Employment		-0.013	-0.049
		(0.148)	(0.169)
Log Licensed Drivers		0.158**	0.161**
		(0.065)	(0.068)
Log (1 + Metro Population)			0.391
			(0.274)
Log (1 + Non-Metro Population)			-0.058
			(0.245)
Log (1 + Metro Income)			0.225*
			(0.129)
Log (1 + Non-Metro Income)			0.063
			(0.074)
Log State GDP			0.016
			(0.066)
Log Road Mileage			0.076
			(0.077)
Observations	2448	2448	2448

* p <0.1, ** p <0.05, *** p <0.01

Note: All models include state and year fixed effects as well as state-level time trends. All models cluster standard errors with respect to states.

B.3 Full Second Stage Results

We choose not to report these results in the main body due to the need for a discussion surrounding the confidence sets. As reviewed in Andrews, Stock, and Sun 2019, confidence sets derived from the AR test can take the form of disjoint or infinite sets but maintain correct coverage [27]. These confidence sets, though potentially unintuitive, bear no weight on our ability to reject the null hypotheses shown.

Table B.3: Second Stage Results

Log VMT	(1) Spec. 1	(2) Spec. 2	(3) Spec. 3
Log Registrations (β)	0.0601	-0.512	-0.311
$\mathbf{H_0}$ **:** $\beta = 1$			
AR-χ^2	3.473	5.111	5.527
P-Value	0.0624	0.0238	0.0187
$\mathbf{H_0}$ **:** $\beta = 0.5415$			
AR-χ^2	2.027	3.797	3.655
P-Value	0.155	0.0513	0.0559
AR Confidence Set (95%)	entire grid	[... , .543017] U [22.0796, ...]	[-4.44058, .572629]
Range of Points Tested	[-10000, 10000]	[0, 23]	[-5, 1]
Number of Points in Range	2500	2500	2500
Grid Resolution	8.003	0.00920	0.00240

* p <0.1, ** p <0.05, *** p <0.01

Note: All models include state and year fixed effects as well as state-level time trends.

All models cluster standard errors with respect to states.

... denotes that no bound was found within the range of points tested during test inversion

B.4 2SLS Results Not Robust to Weak Instruments

The primary results for the traditional 2SLS[v] results can be seen in Table B.4. These results shown in Table B.4 are not robust to weak instruments, so they should be interpreted cautiously as they could be subject to weak instrument bias.

Table B.4: 2SLS Results

Log VMT	(1) Spec. 1 b/se	(2) Spec. 2 b/se	(3) Spec. 3 b/se
Log Total Registrations	0.060	-0.512	-0.311
	(0.403)	(0.613)	(0.464)
Log Population		0.466	
		(0.321)	
Log Total Income		0.293	
		(0.180)	
Log Mean Gas Price		-0.050	-0.040
		(0.080)	(0.068)
Log Employment		0.414**	0.311**
		(0.166)	(0.150)
Log Licensed Drivers		0.059	0.029
		(0.114)	(0.092)
Log (1 + Metro Population)			0.515**
			(0.234)
Log (1 + Non-Metro Population)			-0.067

[v] Estimated using the *ivreg2* Stata package [29].

			(0.147)
Log (1 + Metro Income)			-0.022
			(0.148)
Log (1 + Non-Metro Income)			0.119*
			(0.069)
Log State GDP			0.162***
			(0.054)
Log Road Mileage			0.048
			(0.077)
Observations	2448	2448	2448
$\mathbf{H_0}$ **:** $\beta = 1$			
χ^2 Value	5.433	6.086	7.975
P-Value	0.020	0.014	0.005
$\mathbf{H_0}$ **:** $\beta = 0.5415$			
χ^2 Value	1.425	2.955	3.373
P-Value	0.233	0.086	0.066

* p <0.1, ** p <0.05, *** p <0.01

Note: All models include state and year fixed effects as well as state-level time trends. All models cluster standard errors with respect to states.