CONTEMPORARY ECONOMIC POLICY

IT'S NO ACCIDENT: EVALUATING THE EFFECTIVENESS OF VEHICLE SAFETY INSPECTIONS

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An increase in technology means that vehicles are more reliable than in the past. Accordingly, states have begun to discontinue their requirements for vehicle safety inspections. To gauge the effect of such changes, we examine traffic fatality data from 2000 to 2015, with emphasis on New Jersey, which ended safety inspection requirements in 2010. Utilizing a synthetic controls approach, we conclude that ending these requirements did not result in a significant increase in the frequency or intensity of accidents due to car failure, implying that the consumer and government expenditures used for inspections could be reallocated to other areas of travel safety. (*JEL* R41, Z18, C23)

I. INTRODUCTION

As technology improves, vehicle manufacturers have taken it upon themselves to make and distribute vehicles that are far safer and more reliable than in previous years. In fact, traffic fatalities are on a steep decline in the United States, with a total of only 32,850 deaths related to motor-vehicle accidents last year as compared with 43,510 recorded in 2005.[1] As the pool of vehicles on the road gradually shifts to newer models, this decline will become more pronounced—according to consumer reports, drivers of vehicles made before 2000 are 71% more likely to die in a motor-vehicle-related accident than drivers of vehicles made after 2010 (National Highway Traffic Safety Administration 2013). With significant developments in both vehicle form and function, an accident is both less likely to occur and less likely to be fatal now than it ever has been.

Beginning in Massachusetts in 1926, states across the United States began implementing regular vehicle inspections to prevent mechanical failure on the roads. The program was nationalized by the National Traffic and Motor Vehicle Act and the Highway Safety Act of 1966, which set standards for vehicle performance across the nation and withheld federal highway funds from states failing to adopt these inspection programs. While these programs appear to have been successful at reducing traffic mortality rates as late as the 1970s (Keeler 1994), current trends toward safer and more reliable vehicles have led states to begin discontinuing these inspection[2] requirements as early as 1976, when Congress began allowing states discretion over their programs. In the last 5 years, three states have ended their programs,[3] and multiple others[4] are considering following suit.

Even amidst this movement toward deregulation, however, both state politicians and special interest groups maintain stout loyalty to the safety inspection program. When both Texas and Utah considered repealing their safety inspection programs in the 2017 legislative session, opposition ran high. "If [the repeal] is passed," said

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1. National Highway Traffic Safety Administration data.

2. Note that for the course of this paper, vehicle safety inspections refer to the inspection of a vehicle for motor defects, and is completely independent of newer and more common vehicle *emissions* testing.

3. The District of Columbia (2009), New Jersey (2010), and Mississippi (2015).

4. Including Pennsylvania, Texas, and Utah.

ABBREVIATIONS

AR: Autoregressive Model
RMPSE: Root Mean-Squared Prediction Error

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Texas Senator Eddie Lucero, Jr., "I am going to have trouble sleeping at night. Why are you willing to place yourself and Texans in danger by passing [this repeal]?"[5] Similarly, Utah Representative Jim Dunnigan claimed that many of his constituents "would drive their car until their brakes fall off and their muffler falls off and their tires fall off" and that an inspection was the only way to ensure that vehicle owners took care of potential safety concerns.[6] These claims are backed by most automobile service stations, who generally profit from performing the inspections and now claim that repealing the inspection program "will definitely result in more accidents."[7] On the other hand, those in favor of the repeal cite similar traffic fatality trends in states both with and without the inspection mandate and conclude that the inspection program is merely an indirect tax on consumers.

Evidence on both sides of the debate is largely anecdotal, and (most often on the side in favor of inspections) aims at scaring state representatives into protecting their constituents against the worst-case scenario. However, after Texas' repeal stalled in 2017, there are still 15 states requiring some form of vehicle inspection for motor defects, and the debate continues over their relevance. On average, motorists spend between \$260 million and \$600 million for every 11 million vehicles inspected,[8] and state governments spend about \$10 million a year to manage the program.[9] If the evidence in favor of vehicle safety inspections proves to be purely anecdotal, these expenditures could be reallocated in both arenas. Consumers, who would save both money and time, could choose to spend it on things of more value (one of which could well be a newer, safer vehicle); similarly, governments could focus on more effective traffic safety programs, such as distracted driving, seat belt enforcement, or drinking-and-driving programs. Additionally, a state faces nonmonetary costs: some states allow state troopers to randomly inspect vehicles they stop on roads and freeways for mechanical defects, leading to an inefficient use of police resources and time.

5. "Senate passes bill to eliminate most vehicle safety inspections." *The Texas Tribune,* May 4, 2017.

6. "Utah house votes to eliminate vehicle safety inspections." *The Salt Lake Tribune,* February 17, 2017.

7. "Some drivers cheer, others jeer as motor vehicle safety inspections disappear in N.J." *Nj.com,* July 31, 2010.

8. Analysis performed by Cambridge Systematics for Pennsylvania Department of Transportation, 2009.

9. New Jersey Motor Vehicle Commission, 2009.

A renewed investigation into vehicle safety inspections during this movement, then, is increasingly pertinent to the continued efficiency of government-sponsored safety programs. We therefore consider the impacts of a law change in New Jersey, which discontinued the practice of vehicle safety inspections in August 2010. Specifically, we analyze the effects of this policy change on both the frequency and trend of accidents due to car failure through a synthetic controls approach as well as a traditional difference-in-differences analysis. In both approaches, we conclude that discontinuing the law resulted in no significant increase in either fatalities due to car failure or the percentage of accidents due to car failure.

The rest of the paper proceeds as follows: Section II presents relevant literature about previous examinations of vehicle safety inspections, and how this approach is unique. Section III describes our data. Sections IV and V are a detailed justification and analysis of the synthetic controls approach to the problem, while Section VI explores the traditional difference-in-differences analysis. A discussion of policy implications follows in Section VII, followed by concluding remarks in Section VIII.

II. RELATED LITERATURE AND PREVIOUS FINDINGS

There have been several studies aimed at understanding the potential benefits of vehicle safety inspections, although most were published before the turn of the century (during the last flurry of inspection repeal proposals). These studies have mixed conclusions—on the one hand, earlier studies concluded that inspections were effective (Loeb 1990; Loeb and Gilad 1984; White 1986), while on the other, later studies tended to conclude that they were not (Garbacz 1987; Merrell, Poitras, and Sutter 1999; Poitras and Sutter 2002). Keeler (1994) may have a unique insight to this trend, as he posits that the effectiveness of safety inspections may be decreasing over time (probably due to the increased safety of newer vehicles). He suggests that these inspections were effective into the late 1970s, but not afterward.

Almost all of these studies rely on either time series analysis (e.g., Loeb and Gilad 1984) or panel data analysis (e.g., Poitras and Sutter 2002). In general, the assorted studies analyze trends in general traffic fatalities or traffic-related injuries, with results varying more by the year of publication than by the methodology employed, as described by Keeler. Of course, general traffic fatality trends are too broad, as they include measurements of accidents related to alcohol, speeding, and distracted driving, among other potential factors. Poitras and Sutter (2002) made a recent attempt to zero in on accidents specifically due to car failure by using the percent of older cars in use as a dependent variable, arguing that one would expect to see a measurable decrease in the number of old cars on the road in states with safety inspections as they are costlier to maintain. They conclude that the added cost of inspecting old cars does not affect the percent of old cars on the road, implying that inspections do not reduce car failure. Additionally, they attribute the persistence of these inspection programs to political transaction costs, rather than the advocacy of special- or public-interest groups.

Since the recent debates about vehicle inspection repeal, there has only been one other study published by Peck et al. (2015). This study uses traffic fatality data from Pennsylvania to conclude that cars with issues are more likely to result in fatality-causing accidents. The author then claims that mandatory safety inspections will therefore save lives but does not sufficiently justify it. Pennsylvania did not change their safety inspection regulations over the time frame of the study, making the effectiveness of the regulation impossible to measure with no proposed counterfactual. Furthermore, this study suffers from the same lack of data as previous efforts, as only trends in general traffic fatalities are analyzed, rather than fatalities due to car failure.

Our main contribution is twofold: first, we employ more precisely collected data; second, we utilize recent advances in economic analysis to more precisely answer the question of the policy's effectiveness. Our data allow us to isolate traffic fatalities specifically due to car failure, as opposed to general car accidents; additionally, the data allow us to attribute accidents to the state from which the vehicles in an accident were registered rather than where the accident took place. Finally, ours is the first study to employ the novel synthetic controls approach in understanding this particular program.

III. DATA

Data for this project come from the United States Department of Transportation's Fatality Analysis Reporting System, provided through the National Highway Traffic Safety Administration. These data provide information about all traffic accidents occurring on traffic-ways open to the public in the United States and resulting in at least one fatality[10] within 30 days of the accident. Therefore, we are restricting our analysis of vehicle safety inspections to the effect on fatality rates, rather than simply accident rates; however, this aligns better with the law's intent to reduce fatalities resulting from car failure.

We analyze data spanning the years 2000 to 2015 across all 50 states and the District of Columbia, yielding a total of 551,789 accidents and 822,049 vehicles. The data are at the person-level, including information about each person and vehicle involved in every accident, and provide a rich quantity of demographic and circumstantial evidence surrounding the crashes and resulting fatalities. Key demographics for the entire sample can be found in the Appendix[11]; more detail on the control groups (synthetic and geographic) will be given in Sections IV and VI.

Perhaps most importantly, this dataset provides information regarding the vehicle factors surrounding an accident, allowing us to pinpoint the proportion of accidents which were (at least in part) caused by car failure, problems which *should* be caught in a typical vehicle safety inspection. Given this information, we can construct an indicator for whether car failure of some kind contributed to an accident, which will allow us to investigate the effect of this law change on traffic fatalities *due to car failure* rather than overall traffic fatalities. Given that other determinants of traffic fatalities—weather, lighting, and road conditions, as well as drug/alcohol usage, speeding, or seat belt usage—are essentially uncorrelated with a vehicle's inspection status, this is a critical and novel contribution to studies of this type.

Since we are interested in the effects of law changes for vehicles of certain states, it is imperative that we look not at the number of traffic fatalities occurring within a specific state, but rather the number of fatalities due to accidents involving vehicles registered to that state. For example, an accident resulting in a fatality in Wyoming between two cars registered to Utah and Colorado ought to be attributed to both Utah and Colorado and *not* Wyoming for the

10. To qualify for inclusion, this fatality can be either a vehicle occupant or a nonmotorist; no distinction is made.

11. See Table A1 in the Appendix.

intents of our investigation. Therefore, we construct measures of (1) total traffic fatalities, (2) traffic fatalities due to car failure, and (3) the percent of accidents due to car failure based on vehicle registration rather than accident location. Each of these measures has been normalized for state population.

With each of these measures calculated, and with sufficient information on other determinants of accidents resulting in fatalities, we can proceed with our analysis by creating and/or selecting appropriate counterfactuals for our treatment group.

IV. SYNTHETIC CONTROL ANALYSIS: BASIC ANALYSIS

A. Setup

A crucial difference between this study and other studies on vehicle inspection laws rests in our state-level analysis. Specifically, our data allow us to isolate accidents based not only on in which state an accident occurred, but also on the state in which a vehicle involved in an accident was registered. This is critical because it allows us to study the effect of law changes on the vehicles affected by the law, rather than the (arguably dubious) proxy of accidents taking place in the state with the law change. However, this advantage raises the question of the effectiveness of simple comparison methods, such as a difference-in-differences approach. How can we be sure that neighboring states represent a decent comparison of traffic fatality rates and car failure trends for states whose laws have changed when it might be possible for these accidents to occur thousands of miles away?

Figure 1 shows trends in our desired measures of traffic fatalities for New Jersey comparison to a few proposed control groups. In Section VI, we will use these control groups for difference-in-differences analysis as a robustness check. However, visual inspection of the graph suggests that the treatment groups and our proposed control groups do not *closely* mirror each other before the treatment. Indeed, Table 1 contains some basic statistical information about the fit of various counterfactuals relative to New Jersey's actual trend—particularly with respect to car failure fatalities or the frequency of accidents due to car failure, note that the fit is poor.

In order to form a control which more closely mirrors the pretreatment trend in traffic fatalities (both total and those due to car failure), we will employ a synthetic controls approach.[12] That is, we will construct a synthetic New Jersey by selecting a convex combination of control states (across the United States) in order to minimize the differences between pretreatment trends in pretreatment outcomes as well as predictors of posttreatment trends, creating a better control group than a single unit alone could provide. Given this constructed control, we can then model the evolution of our dependent variables—the frequency and intensity of accidents due to car failure—across the treated group as differences in trends between real and synthetic New Jersey.

The weights are determined in what can be thought of as a two-step process. First, weights are given to different predictor variables based on their ability to predict pretreatment trends. Then, weights are given to each control unit in order to minimize the difference between the synthetic outcomes and the realized outcomes in the treatment group for the pretreatment period. One special advantage of using this two-step method is a safeguarding against "extreme counterfactuals," or counterfactuals which reside "far removed from the convex hull of the data" (Abadie, Diamond, and Hainmuller 2010; see also King and Zeng 2006). Control units are only selected to be used in the synthetic control if their assigned weight is positive; once these weights are normalized (to sum to one), it is made far less likely that results will be driven by outlier data or data radically different from the treatment data.

The synthetic control method is more data driven than other policy evaluation methods, as it constructs the proper counterfactual according to the method above from a group of proposed control states, or *donor pool*. The researcher retains only two degrees of freedom: the selection of the donor pool, and the selection of predictor variables on which to match pretreatment trends, each of which is chosen to minimize the root mean-squared prediction error (RMSPE) of the synthetic control. Our decisions along both of these dimensions are examined more carefully in Section V, which addresses potential concerns surrounding the synthetic control analysis.

This study conducts the synthetic control analysis using both state-year and state-month panels and three variables of interest: the number of traffic fatalities per capita, the number of

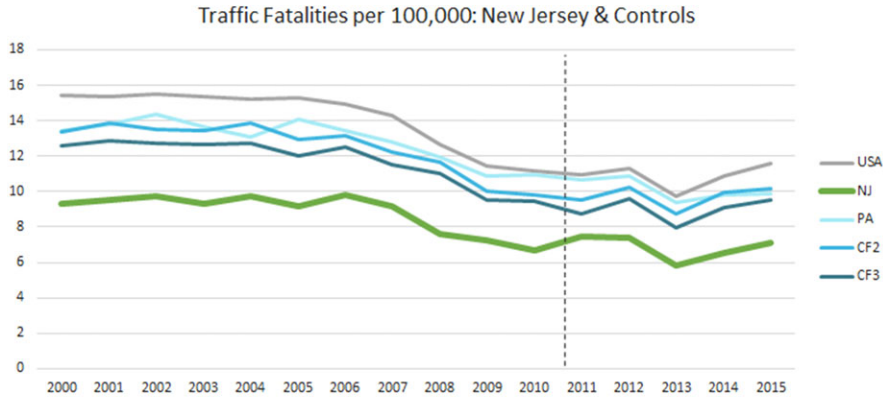
12. For a detailed theoretical motivation behind the synthetic controls approach, see Abadie, Diamond, and Hainmuller (2010).

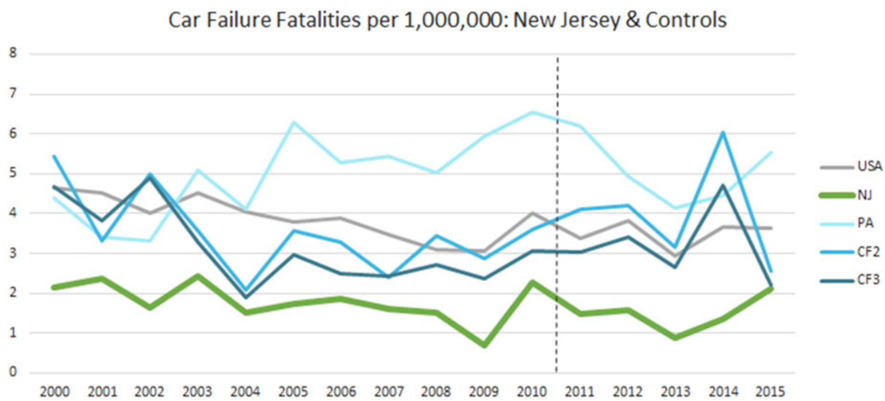
FIGURE 1

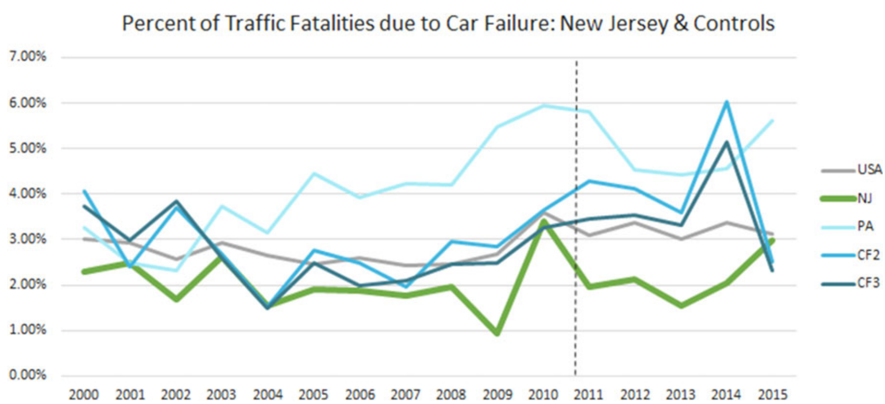
Trends in Dependent Variables for New Jersey and Proposed Geographic Controls

Proposed Controls

Control Group 1	Control Group 2	Control Group 3
Pennsylvania	Pennsylvania Delaware New York	Pennsylvania Delaware New York Maryland Connecticut







Note: The black dotted line in each graph indicates when the change in vehicle safety inspection requirements took place.

TABLE 1
Pearson's Correlation Coefficients, New Jersey and Potential Counterfactuals[a] (Measured over the Pretreatment Period, 2000–2009)

	Year-Level (N = 10)				Month-Level (N = 127)			
	Synthetic	**PA**	**CF 2**	**CF 3**	**Synthetic**	**PA**	**CF2**	**CF3**
Fatalities/capita	0.93	0.87	0.92	0.92	0.68	0.64	0.57*	0.62
Car failure fatalities/capita	0.78	−0.39***	0.35*	0.47	0.21	0.17	0.26	0.20
% of car failure accidents	0.77	−0.51***	0.25**	0.36*	0.25	0.14*	0.21	0.10***

[a]Correlation coefficients were computed for the pretreatment trends in car failure–related fatalities per capita at both the year and month level. Asterisks denote the significance of two-sample z tests between the correlation coefficient of the synthetic control and the geographic control in question.
***$p < .01$; **$p < .05$; *$p < .1$.

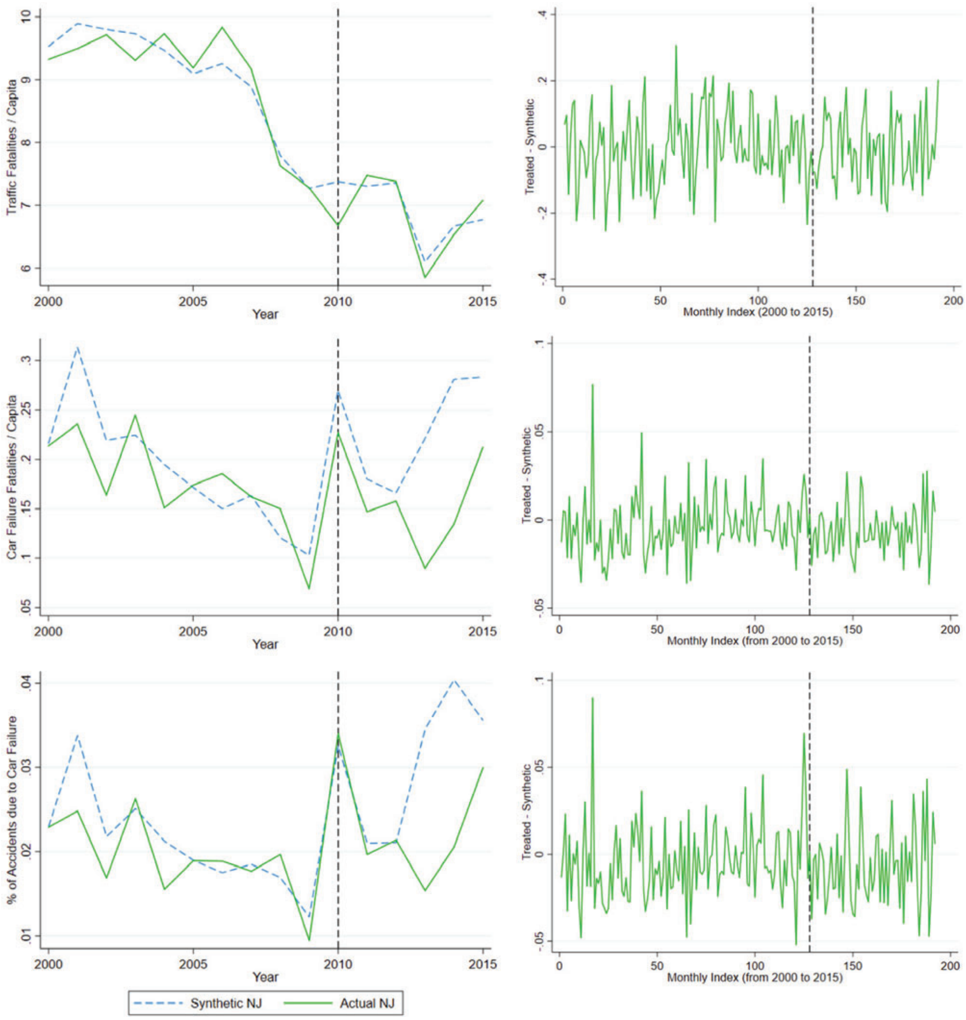
TABLE 2
Synthetic New Jersey RMSPE, State and Predictor Weights

	Traffic Fatalities/Capita		Car Failure Fatalities/Capita		% of Accidents from Car Failure	
	Year-Level	**Month-Level**	**Year-Level**	**Month-Level**	**Year-Level**	**Month-Level**
RMSPE	0.3023	0.1123	0.0385	0.0175	0.0039	0.0223
State weights						
DE	0	0	0	0	0	0
HI	0	0	0	0	0.018	0
LA	0	0	0.005	0	0	0
ME	0	0	0	0	0.005	0
MA	0.111	0.212	0.414	0.178	0.350	0.089
MO	0	0	0	0	0	0
NH	0	0	0	0	0	0
NY	0.528	0.514	0	0.529	0	0.529
NC	0	0	0	0	0	0
PA	0.071	0.164	0	0	0	0
RI	0.092	0	0.261	0	0.281	0
TX	0	0.015	0	0.129	0.036	0.245
UT	0	0	0	0	0	0
VT	0	0	0	0	0	0
VA	0.197	0.094	0.320	0.164	0.254	0.137
WV	0	0	0	0	0.058	0
Predictor weights						
Lag '00	0.04	0.00	0.07	0.29	0.40	0.003
Lag '05	0.001	0.20	0.08	0.09	0.31	0.04
Lag '09	0.94	0.09	0.003	0.07	0.00	0.00
Urban area	0.01	0.30	0.85	0.22	0.23	0.78
Poor weather	0.00	0.01	0.002	0.02	0.06	0.00
Alcohol/drugs involved	0.00	0.22	0.001	0.17	0.00	0.08
Speeding involved	0.00	0.18	0.00	0.15	0.00	0.17

traffic fatalities *due to car failure* per capita, and the frequency of accidents due to car failure. Our analysis is conducted using the *synth* package for Stata developed by Abadie, Diamond, and Hainmuller (2014). Table 2 demonstrates the weights assigned to both states in the donor pool and predictor variables for each of the six models, as well as the resulting RMSPE for each model. Figure 2 illustrates these trends graphically, with overall trends (on the left) presented at the year level and differences between New Jersey and its synthetic counterpart (on the right) presented at the month level.

In general, our donor pool consists of states which required vehicle safety inspections over the entire range of data (e.g., from 2000 to 2015). Table 3 shows a list of various demographic indicators from which we can construct our synthetic New Jersey, as well as how the synthetic control compared with geographic controls when all of these predictors were used. However, in order to minimize the error of our synthetic control, we performed sensitivity tests investigating the nonlagged predictor variables. Additionally, following the recent discussion of lagged dependent variables in synthetic control analysis (Ferman, Pinto, and Possebom 2017; Kaul et al. 2015), we performed additional sensitivity tests to determine the most appropriate number of lagged dependent variables

FIGURE 2
Trends in Traffic Fatalities for New Jersey and Synthetic New Jersey



Notes: The black dotted line in each graph indicates when the change in vehicle safety inspection requirements took place. Graphs on the left are aggregated by year to show a more holistic picture, while graphs on the right represent the differences between the treated and synthetic units at the month level.

to include in the construction of synthetic New Jersey.

The main model we will analyze utilizes lagged measure of the dependent variables of interest in the years 2000, 2005, and 2009, as well as indicators for accident type: accidents due to speeding, poor weather conditions, or substance abuse, as well as accidents occurring in an urban area.[13] In addition to the sensitivity tests mentioned above, which determined (1) the number of lagged dependent variables and (2) the selection of nonlagged predictor variables, we conducted one other sensitivity test to determine the optimal pretreatment range,[14] which suggested that the entire range of available data (2000 to 2010) was optimal. The results from these three sensitivity tests appear in the Appendix.[15]

13. These are bolded in Table 3.

14. Each test was performed as outlined in McClelland and Gault (2017).

15. For details, see Tables A2 through A5, as well as Figures A1 through A3.

TABLE 3
Description of New Jersey and Synthetic New Jersey[a]

	New Jersey	Synthetic New Jersey	Control 1[b]	Control 2	Control 3	Full Sample
Driver-level (N)	*17,203*	—	*35,182*	*66,445*	*85,939*	*822,049*
Age	42.03	41.08	42.43	42.38	42.14	40.44
	(3.72)	(3.63)	(0.23)	(0.16)	(0.14)	(0.52)
Female	0.26	0.26	0.25	0.25	0.25	0.27
	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.00)
White	0.82	0.82	0.91	0.89	0.85	0.85
	(0.07)	(0.07)	(0.00)	(0.00)	(0.00)	(0.01)
Black	0.16	0.15	0.08	0.09	0.13	0.09
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Hispanic	0.09	0.04	0.02	0.03	0.03	0.06
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Accident-level (N)	*10,470*	—	*22,013*	*42,802*	*55,377*	*551,789*
Vehicles inspected	1.00	0.74	0.92	0.89	0.71	0.39
	(0.09)	(0.07)	(0.00)	(0.00)	(0.00)	(0.01)
Average year of car	1998.02	1997.49	1999.24	1999.02	1999.12	1993.75
	(176.60)	(176.55)	(10.66)	(7.76)	(6.82)	(25.74)
Proportion in urban area	**0.72**	**0.76**	**0.31**	**0.59**	**0.58**	**0.44**
	(0.07)	(0.06)	(0.00)	(0.00)	(0.00)	(0.01)
Average # of vehicles involved	2.33	2.32	2.14	2.12	2.11	2.26
	(0.21)	(0.21)	(0.01)	(0.01)	(0.01)	(0.03)
Average # of traffic lanes	2.43	2.60	2.20	2.53	2.51	2.47
	(0.22)	(0.24)	(0.01)	(0.01)	(0.01)	(0.03)
Average speed limit	44.60	47.1	44.75	45.34	44.68	49.50
	(4.09)	(4.21)	(0.30)	(0.22)	(0.19)	(0.64)
Determinants of accident						
Proportion involving						
Inclement weather	**0.13**	**0.14**	**0.15**	**0.14**	**0.13**	**0.13**
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Drugs and/or alcohol	**0.15**	**0.15**	**0.28**	**0.23**	**0.23**	**0.21**
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Speeding	**0.60**	**0.59**	**0.55**	**0.50**	**0.51**	**0.57**
	(0.06)	(0.06)	(0.00)	(0.00)	(0.00)	(0.01)

Note: Standard errors in parentheses.
[a]All summary statistics come from the pretreatment period (e.g., 2000–2009) to show comparability. Although *t* tests are not shown, because of large sample size, all differences are statistically significance above the 99% confidence level.
[b]For information regarding the three geographic control groups, see Figure 1.

B. Analysis

In the traditional synthetic controls analysis, the impact of a change in policy is inferred from the differences between the synthetic control and treatment group data in the posttreatment data. That is, once the control has been constructed, the causal impact of the law can be obtained simply from taking the difference of the dependent variable in the treated group and the synthetic group.

Visual inspection in Figure 2 (as well as the correlation coefficients in Table 1) provides strong evidence that the synthetic control has a much better pretreatment fit than do the various proposed geographic controls. Of particular interest are the differences between the two New Jerseys following 2009 (shown on the right side of Figure 2), from which we can derive a sense of the law's impact. Fluctuations in these differences are small for each of the dependent variables and do not change significantly in size or overall slope after the treatment. Table 4 corroborates this by comparing the mean, standard deviation, and overall trend of the fluctuations across the pre- and posttreatment periods; in no case is there any significant change in the difference between treated and synthetic New Jersey after the repeal of the vehicle safety inspection program. Differences between the synthetic and real New Jersey are centered around zero consistently over time, suggesting that the policy change had no significant effect on traffic fatalities due to car failure or the frequency of accidents involving car failures.

In order to more fully place this result into context, it is important to understand how likely it is that the results obtained from this approach could be obtained purely by chance, and how this posttreatment trend compares in context. To do so, we follow the method employed by Abadie, Diamond, and Hainmuller (2010) by employing

TABLE 4
Analysis of Difference Trend between New Jersey and Synthetic New Jersey

	Pretreatment (N = 10)			Posttreatment (N = 6)		
Year-Level	**Mean**[a]	***SD***	**Slope**[b]	**Mean**	***SD***	**Slope**
Fatalities/capita	−0.01	0.32	0.05 (0.03)	−0.10	0.36	0.11 (0.08)
Car failure fatalities/capita	−0.01	0.04	0.01 (0.00)	−0.07	0.06	−0.02 (0.01)
% of car failure accidents	−0.00	0.00	0.00 (0.00)	−0.01	0.01	−0.00 (0.00)

	Pretreatment (N = 127)			Posttreatment (N = 65)		
Month-Level	**Mean**	***SD***	**Slope**	**Mean**	***SD***	**Slope**
Fatalities/capita	−0.00	0.11	0.00 (0.00)	−0.01	0.10	0.00 (0.00)
Car failure fatalities/capita	−0.00	0.02	0.00 (0.00)	−0.01	0.01	0.00 (0.00)
% of car failure accidents	−0.00	0.02	0.00 (0.00)	−0.01	0.02	0.00 (0.00)

Note: Standard errors in parentheses.
[a]Two-sample t tests (with unequal variances) were performed across these means. None of them was significant.
[b]Here, regression slopes are reported for the regression $(\mathit{Treated} - \mathit{Synthetic})_t = \beta_0 + \beta_1 * \mathit{Time}_t + \varepsilon_t$ to assess the change in the difference between actual and synthetic New Jersey as time passes. This was performed and reported individually for both pre- and posttreatment periods—none of these coefficients was significant at or above the 90% confidence level.

an iterative placebo test, answering the question "How often would we obtain results of this magnitude if we had chosen a state at random for the study instead of [New Jersey]?" That is, we act as though another state had eliminated their requirements for vehicle safety inspections, rather than New Jersey by performing identical synthetic control analysis with the policy change assigned to one of the 15 other states currently requiring vehicle safety inspections, and New Jersey relegated to the donor pool. If the placebo studies demonstrate that trends for New Jersey are significantly different from the trends in other states, we would conclude that there *is* an effect from changing the law, contrary to our initial hypothesis.

Figure 3 illustrates these results, with New Jersey's trend shown in blue against the trends of the other states in gray. Notice that the trend for New Jersey sits comfortably within the range of the other states, and has fluctuations relatively smaller in size than those of other states.[16] This holds for each of the three dependent variables in question—traffic fatalities per capita, car failure fatalities per capita, and the percent of accidents due to car failure.

These small fluctuations relative to the fluctuations of nonexistent policy changes lead us to conclude that there was no significant effect from discontinuing the vehicle safety inspection mandate in New Jersey. In addition, we conducted two other standard placebo tests as outlined in McClelland and Gault (2017): the "in-time" placebo test, in which the analysis is performed as though the law change had occurred at some point during the pretreatment period; and the "leave one out" placebo test, where the analysis is iterated again, each time stripping one state from the donor pool.[17] The results from these two placebo tests can be found in the Appendix[18]; in both tests, the result is the same. Specifically, removing the requirements for safety inspections did *not* result in an increase in either frequency or intensity of accidents due to car failure.

V. SYNTHETIC CONTROL ANALYSIS: ADDRESSING CONCERNS

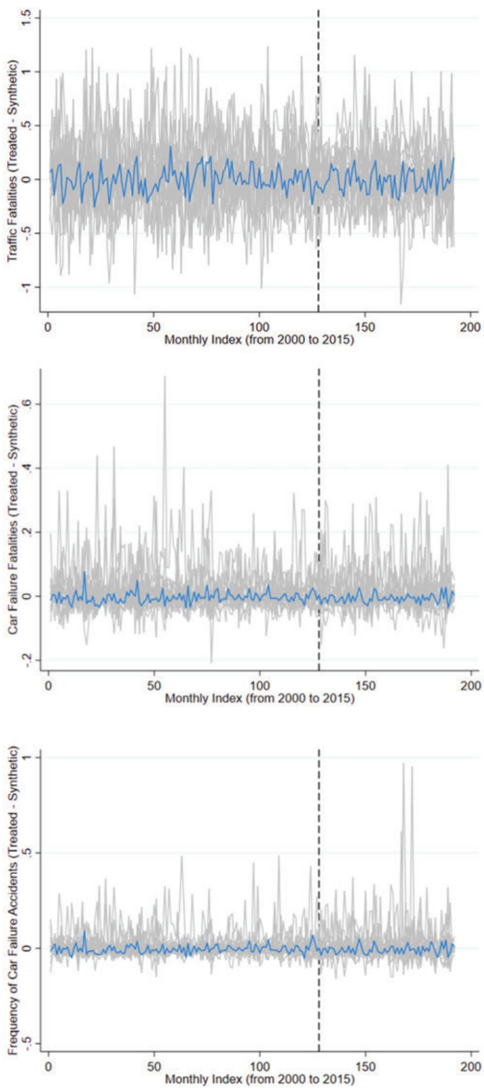
The synthetic controls analysis requires two necessary assumptions in order to draw legitimate conclusions about the policy change. First, it must be the case that there is no interference

16. For details, see Table A6 in the Appendix.

17. This test is performed in order to ensure that it is not merely one state in the donor pool driving the results.

18. See Tables A7 and A8, as well as Figures A4 and A5.

FIGURE 3
Placebo Tests for States with Vehicle Safety Inspection Laws



between units,[19] or that the number of accidents in control states is unaffected by the policy change in New Jersey. We will call this the *donor pool independence* assumption. Second, given that synthetic control analysis aims to isolate the effect of the law change, it must be that there are no other factors, either in New Jersey or its synthetic control donor states, that drive differences between the two groups posttreatment. We will call this the *trend independence* assumption. In this section, we aim to justify both of these assumptions, as well as respond to potential objections regarding our analysis. Each of the tests discussed in this section was performed across all six models—however, results are shown only for fatalities per capita due to car failure using the state-month panel. These results appear in the Appendix following the sensitivity and placebo tests described in Section IV.

A. *Donor Pool Independence*

First, we claim that the New Jersey law change did not affect trends in potential control states, thereby protecting the synthetic New Jersey from contamination. The assumption of donor pool independence would be violated if vehicles registered to New Jersey failed, causing accidents involving vehicles from neighboring states (e.g., Delaware, New York, or Pennsylvania, each of which requires safety inspections). This effect, in theory, would be proportional to the fraction of New Jersey vehicles counted in other states, a fraction which never exceeds 3%.

However, to ensure that changes in New Jersey law do not affect synthetic New Jersey in this way, we reconducted our analysis excluding neighboring states from the donor pool.[20] First, we excluded strict geographic neighbors (Delaware, New York, and Pennsylvania), followed by other close neighbors which were given high weights in the initial analysis (Massachusetts, Rhode Island, and Virginia). Finally, we excluded all six. The loss of immediate geographic neighbors did little to either the error of the synthetic control or its qualitative predictions—however, stripping all six states caused a large spike in the error associated with the control. This makes sense, given that there are only 15 states available for the donor pool, so excluding most of the Northeastern region (where most states with safety inspection programs are located) limits the ability to construct an efficient control. Even in this case, however, the overall implications of the model did not change—the policy change resulted in no significant differences to New Jersey's trend relative to its synthetic. It is therefore unlikely that geographic neighbors were influenced by the policy change enough to corrupt our initial analysis.

19. Rosenbaum (2012) provides a more detailed discussion of this assumption.

20. For details, see Figure A6 and Table A9 in the Appendix.

B. Trend Independence

Second, it may be the case that factors outside of the law change might have affected either New Jersey or its synthetic, making the lack of change posttreatment meaningless. We claim to have minimized the impact of these secondary drivers in two major ways. First, we include in the donor pool only states which require mandatory safety inspections over the entire range of data, rather than other states which have adopted or discontinued (or never required) safety inspections. This ensures that the synthetic New Jersey appears as a state which never discontinued its inspection mandate. Second, we use lagged dependent variables in our construction of synthetic New Jersey. This avoids the problem of omitting important predictors' effects, and is frequently done in this type of analysis (Athey and Imbens 2006; McClelland and Gault 2017).

Even given these strategies, more careful consideration can be given to this issue in order to ensure that our conclusions are driven only by the 2009 policy change. Of particular concern is the Cars Allowance Rebate System (colloquially known as "Cash for Clunkers"), a government-sponsored program which provided economic assistance for those trading in older, less fuel-efficient vehicles in order to purchase more fuel-efficient ones.[21] This program, which ran from July 1, 2009 to August 24, 2009, might undermine our results—as National Highway Traffic Safety Administration spokesperson Eric Bolton suggested at the time, these newer cars were probably "considerably safer than the old clunkers they are replacing," thereby counteracting the increased risk resulting from a lack of safety inspections. These contrary forces may have led to the conclusion that the repeal was ineffective.

There are several responses to this potential objection. First, there is little evidence that this program increased new vehicle sales in the long run. In fact, Mian and Sufi (2012) exploited variation in U.S. cities (by the number of clunkers) to conclude that while the program induced the sale of about 370,000 new cars in July and August 2009, these purchases were offset by a reduction in new vehicle sales in the following 10 months. Thus, when considering the entire posttreatment period, there is no reason to suspect that this program would have reduced the number of old cars on the road enough to sufficiently offset any supposed increase in risk from the elimination of inspections. Furthermore, since this program was national in scope, attributing differences between New Jersey and synthetic New Jersey to this program requires that we assume vehicle owners in New Jersey were more likely to trade in older cars than vehicle owners in our control states. However, this is false, as can be seen in figures 2 and 3 of Mian and Sufi—New Jersey did not purchase more new cars than our average control state. Therefore, we have no reason to suppose that our results are corrupted by the effects from this program.

C. Time-to-Effect

One potentially concerning detail of this analysis is the time-to-effect of the law change—that is, it is probably true that the discontinuation of vehicle safety inspections did not immediately lead to an increase in unsafe vehicles on the road, so the effects from the policy change may not appear at once.

This concern would be warranted if the difference between New Jersey and its synthetic counterpart increased over the posttreatment period. However, analysis of this difference[22] provides no evidence of growing disparities between the two groups over the 5 years following the policy change. For each of the three dependent variables at both the month- and year-levels, we perform (1) a linear regression to estimate the effect of time on the slope of the differences, (2) a Breusch-Pagan test to determine if the spread of these differences increases over the posttreatment period, and (3) a Dickey-Fuller test to ascertain if the differences between New Jersey and synthetic New Jersey are roughly stationary. In each case, we do not find enough evidence to suggest that the differences are growing over time; it therefore seems plausible to conclude that the policy change has not had an increased effect as time progressed.

VI. ROBUSTNESS CHECKS: TRADITIONAL DIFFERENCE-IN-DIFFERENCES

The synthetic controls approach to this investigation rectifies several potential issues in our study. First, as the area of investigation is not

21. Specifically, the program offered $3,500 to $4,500 vouchers to consumers trading in older cars meeting certain eligibility requirements (generally older than 25 years with mileage less than 18 miles per gallon). The vouchers could be used only to purchase vehicles newer than 5 years with mileages above 22 miles per gallon.

22. See Table A10 in the Appendix for details.

limited geographically, but is rather a pool of vehicles registered to a specific state (regardless of their locations), geographic counterfactuals were difficult to justify, given that geographic proximity is not necessarily correlated with accident likelihood. Additionally, the slight disparities between trends in both traffic mortality rates and traffic fatalities due to car failure across treatment and geographic control groups indicated that a synthetic control might provide a more believable counterfactual.

It is not entirely implausible, however, to use geographic controls for our analysis. In fact, there are a few ways through which we can justify that geographic controls are viable counterfactuals for conducting the desired analysis, even if the synthetic controls approach may be strictly preferred. By so doing, we provide an effective robustness check for our results.

We propose three different options for control groups for New Jersey based on geographic and demographic conditions. Figure 1 shows which neighboring states are included in each control, as well as trends for each of the three dependent variables of interest—the correlation coefficients in Table 1 suggest that the second and third geographic control groups match actual pretreatment trends most closely, although not as well as the synthetic control. Additionally, Table 3 compares demographic information for each proposed control with actual New Jersey; note that these demographics are closely mirrored across groups. This is advantageous because it is likely the case that demographic information is correlated with factors that increase the likelihood of a car breakdown—for example, it may be the case that lower-income individuals tend to have older cars,[23] which may lead to an increased frequency of accidents due to car failure.

Additionally, to assert that these control groups provide adequate comparison trends for New Jersey, we perform a simple pretreatment regression for trend comparisons. This regression takes the form

$$y_{it} = \beta_0 + \beta_1 * state_i + \beta_2 * year_t + \Gamma \left(state_i * year_t\right) + \varepsilon_{it},$$

where y_{it} represents the dependent variable in question (whether traffic fatalities per capita or car failure fatalities per capita) in a state i at time t, and where Γ is a vector consisting of one coefficient for each year in the pretreatment period. This regression allows us to capture deviations in trends for the proposed control groups from the treatment groups at each year in the pretreatment period. The results from this regression are reported in the Appendix[24]—we find few significant differences between the groups and actual New Jersey, particularly for the first two control groups.

Given that the control groups chosen for each state mirror trends in the treatment groups before the change in policy, we conclude that they are acceptable controls for the study. We can therefore proceed with our difference-in-differences analysis. We begin with a simple difference-in-differences model with no covariates or fixed effects, that is, a model of the form

$$y_{it} = \beta_0 + \beta_1 * nj_i + \beta_2 * post_t + \beta_3 \left(nj_i * post_t\right) + \varepsilon_{it},$$

where y_{it} is the dependent variable in question (car failure fatalities per capita or the percentage of accidents due to car failure), nj_i is an indicator for whether the observation belongs to New Jersey, the treated state, and $post_t$ is an indicator for whether the observation occurred after the change in policy. Given this setup, β_3 is an appropriate indicator of the significance and effect of the change in policy on fatality rates in the treatment group compared with our counterfactual trends. There exists strong autocorrelation in the trends of car failure fatality rates. As autocorrelation biases the size of standard errors toward 0, therefore leading to Type I errors, we are only immediately concerned with correcting for autocorrelation in the case of incorrectly assigning significance to model coefficients. While our models are concerned more with the coefficient's insignificance, we nevertheless control for autocorrelation by allowing for an autoregressive model (AR)1 disturbance in the panel data estimators.

The results for the simple models are summarized in Table 5; except for the second geographic control, all estimates of the impact of the policy change are insignificant with more than 95% confidence. In the case of the second control—Pennsylvania, Delaware, and New York—the impact of the policy change is measured to be significantly *negative*. However, for all results, the traditional difference-in-difference model provides no contradictory evidence to

23. Or not maintain their cars as well or as frequently as higher income individuals.

24. See Table A11 for details.

TABLE 5
Simple Difference-in-Differences Regression Output[a]

New Jersey Dependent Variables	Control 1: # of Car Failure Fatalities	Control 1: % of Fatalities due to Car Failure	Control 2: # of Car Failure Fatalities	Control 2: % of Fatalities due to Car Failure	Control 3: # of Car Failure Fatalities	Control 3: % of Fatalities due to Car Failure
New Jersey	−0.014*	−0.013*	−0.004*	0.004**	−0.001	0.002
	(0.008)	(0.007)	(0.003)	(0.002)	(0.001)	(0.001)
Law change	−0.003	0.007	0.013***	0.018***	0.003	0.009***
	(0.009)	(0.009)	(0.004)	(0.003)	(0.002)	(0.002)
*New Jersey * Law change*	*0.003*	*0.001*	***−0.014****	***−0.012****	*−0.004*	*−0.003*
	(0.010)	(0.009)	(0.004)	(0.003)	(0.003)	(0.002)
Observations	374	374	748	748	1,122	1,122
R^2	0.5547	0.6594	0.7336	0.7506	0.7148	0.7227

Note: Standard errors in parentheses.
[a]These models control for autocorrelation of an AR(1) form.
$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$.

results of the synthetic controls approach: the change in policy did not cause a significant increase in traffic fatalities per capita due to car failure, or in the percentage of traffic accidents due to car failure.

There are several potential explanations for the significantly negative results prevalent when estimating with the second control group. The first—and arguably most plausible—is that this choice for a demographic control is not a suitable one for the estimation. The results from estimating the model with this control group vanish when we re-estimate it with either a slightly more or less restrictive control; this lack of generalizability indicates that the choice of control group is less than perfect. While it could be the case that the change in policy resulted in *fewer* fatalities due to car failure,[25] the other empirical evidence in this paper coupled with a general intuition lead us to posit that this significance results more from a poorly chosen control than an actual effect of the law.

In order to further understand these results, we iterated the Model 8 additional times using various demographic covariates and fixed effects—a detailed list of the covariates and fixed effects included, as well as selected regression output from these iterations can be found in the Appendix.[26] In total, nine difference-in-differences models were analyzed for each state and three control groups, making a total of 27 regressions. The coefficients for the impact of the policy change were insignificant for each iteration of Controls 1 and 3, and significantly negative for each iteration of Control 2, reinforcing the conclusions drawn above.

A. *Time-to-Effect Analysis*

We can address the concern related to delayed responses to the policy change mentioned in Section V.C in this model. That is, we wish to ensure that there is not a delayed increase in traffic fatalities due to car failure resulting from the law change—to measure this, we include lagged observations of the treatment indicator in iterations of the model. We perform these iterations with lags spanning from 6 months after the law change to 5 years, with the results reported in the Appendix.[27] The results suggest no significant delayed impacts of the law change on either traffic fatalities due to car failure or the frequency of accidents involving car failure. We therefore are confident that the law change did not have a delayed effect (at least up to the maximum possible lag of 5 years, given our data).

While the difference-in-differences models are less plausible than the synthetic controls approach on account of occasional demographic differences and slight variations in pretreatment trends, these results reinforce the conclusion of the synthetic controls analysis: the removal of the vehicle safety inspection requirement in New Jersey had no significant positive effect on the number and frequency of traffic accidents due to car failure.

25. Perhaps attributable to a false sense of car security due to recurring government inspections.

26. For details, see Tables A12 and A13 in the Appendix.

27. See Table A14 for details.

VII. DISCUSSION AND POLICY RECOMMENDATIONS

Given this analysis, regular safety inspections of vehicles seem not to be an efficient use of government spending, at least given the objective of reducing traffic mortality. As noted in our summary statistics, fatalities due to car failure already account for only 3% of vehicle traffic fatalities, and the presence of a law requiring regular inspections does not appear to affect that proportion in any meaningful way.

Clearly, there are significant costs borne by citizens who must regularly inspect their vehicles, including both the monetary cost of inspection as well as the time and opportunity cost of doing so. As mentioned in Section I, motorists spend between $260 million and $600 million for every 11 million vehicles inspected in garages, an approximate cost of between $24 and $55 per vehicle; states also face significant costs maintaining these programs, as discussed in Section I. If programs requiring inspections have little safety benefits, these resources could be reallocated to reduce traffic fatalities and increase highway safety.

What, then, are acceptable substitutes for a state government's vehicle safety inspection programs? There are several alternative options, all of which may be more effective at reducing traffic fatalities than safety inspections. For example, a 2015 study by the RAND Corporation found that increasing state spending in traffic "interventions"—such as universal motorcycle helmet laws or increased seat belt enforcement—by an average of $820,000 per state could save 78 lives, prevent 8,600 injuries, and produce a financial benefit of as much as $250 million annually per state (Ecola, Batorsky, and Ringel 2015). Seat-belt enforcement has previously been found to significantly reduce traffic fatalities, and does not tend to increase careless driving (Cohen and Einav 2003). In a similar vein, Ying, Wu, and Chang found in 2013 that the presence of drinking-and-driving laws significantly reduced alcohol-related traffic fatalities by about 0.22% on average, while Nichols and Ross (1988) stated that increased legal sanctions[28] are "an integral and essential part" of reducing alcohol-related traffic fatalities. Finally, this money could be placed in infrastructure to reduce the amount of accidents due to poor road surface conditions, a significantly larger proportion of accidents resulting in fatalities across our sample.

While further analysis would need to be conducted to evaluate the relative strengths of each of these replacement programs, it seems that the money spent on vehicle safety inspections could be better spent in other areas of state government. In whichever way this reallocation is spent—in helmet laws, seat belt enforcement, drinking-and-driving enforcement, infrastructure, or other traffic interventions—a reassignment of funds can result in more lives saved and accidents prevented, as well as providing economic benefits to the state.

VIII. CONCLUSION

Through analysis of traffic fatality data from the National Highway Traffic Safety Administration, we investigate the impact of removing vehicle safety inspection requirements in New Jersey. Using both a synthetic controls approach and a traditional difference-in-differences analysis, we conclude that removing the requirements resulted in no significant increases in any of traffic fatalities per capita, traffic fatalities due specifically to car failure per capita, or the frequency of accidents due to car failure. Therefore, we conclude that vehicle safety inspections do *not* represent an efficient use of government funds, and do not appear to have any significantly mitigating effect on the role of car failure in traffic accidents.

Further research in this area might analyze other impacts of discontinuing vehicle safety inspections. For example, these law changes may have significant effects on delaying the purchases of new vehicles, or may affect how frequently or effectively vehicle owners care for their vehicles. Finally, there may be behavioral responses to this law, as drivers who feel less safe in their vehicle without the inspection requirement may drive in such a way as to reduce accidents due to factors other than car failure. Each of these may offer additional insights into the potential merits or drawbacks of requiring vehicle inspections.

As vehicle manufacturers continue to push for improved safety and reliability in their vehicles, roads will become safer and accidents will be less likely to be caused by car failure. Therefore, states should focus spending in other determinants of traffic fatalities in order to continue the push for safety across the board.

28. For example, license restrictions, and so on.

APPENDIX

TABLE A1
Description of Data, Full Sample

	Full Sample
Driver demographics	
Sample size—drivers	822,049
Age	41.42 (0.05)
Male	0.73 (0.00)
Female	0.26 (0.00)
White	0.85 (0.00)
Black	0.11 (0.00)
Hispanic	0.10 (0.00)
Vehicle demographics	
Proportion registered in	
Northeast	0.12 (0.00)
Midwest	0.21 (0.00)
South	0.47 (0.00)
West	0.20 (0.00)
Average year of car	1998 (2.20)
Proportion inspected	0.36 (0.00)
Accident demographics	
Sample size—accidents	551,789
Proportion occurring in	
Northeast	0.12 (0.00)
Midwest	0.20 (0.00)
South	0.47 (0.00)
West	0.21 (0.00)
Urban area	0.44 (0.00)
Morning	0.22 (0.00)
Afternoon	0.23 (0.00)
Evening	0.25 (0.00)
Night	0.29 (0.00)
Weekend	0.34 (0.00)
Holiday	0.07 (0.00)
Winter	0.22 (0.00)
Spring	0.24 (0.00)
Summer	0.27 (0.00)
Fall	0.26 (0.00)
Average # of fatalities	1.10 (0.00)
Average # of vehicles involved	2.09 (0.00)
Average # of traffic lanes on road	2.51 (0.00)
Average speed limit	49.93 (0.07)
Determinants of accident	
Proportion involving	
Car failure	0.03 (0.00)
Inclement weather	0.11 (0.00)
Drugs and/or alcohol	0.25 (0.00)
Speeding	0.47 (0.00)

TABLE A2
Synthetic New Jersey RMSPE across Tests

	RMSPE
Outcome lags	
2000	0.0518
2005	0.0516
2009	0.0392
2000, 2005	0.0511
2005, 2009	0.0392
2000, 2005, and 2009 (main)	**0.0385**
2000–2009 (all)	0.0377
2000–2009 (average)	0.0448
Predictor variables	
18 predictors +3 lags[a]	0.0520
17 predictors +3 lags	0.0393
16 predictors +3 lags	0.0410
14 predictors +3 lags[b]	0.0405
13 predictors +3 lags	0.0393
12 predictors +3 lags	0.0386
11 predictors +3 lags	0.0408
10 predictors +3 lags	0.0407
9 predictors +3 lags	0.0399
8 predictors +3 lags	0.0394
5 predictors +3 lags[c]	0.0394
4 predictors +3 lags (main)	**0.0385**
3 predictors +3 lags	0.0381
2 predictors +3 lags	0.0392
1 predictors +3 lags	0.0432
Lags only	0.0507
4 predictors, no lags	0.1318
3 predictors, no lags	0.1403
2 predictors, no lags	0.1676
Predictor year range	
2000–2009 (main)	**0.0385**
2002–2009	0.0381
2004–2009	0.0391
2006–2005	0.0392

[a]Eighteen total predictors were tested and eliminated in decreasing order of predictive power. These predictors are (in order from most predictive to least): urban area, drugs/alcohol involved, speeding involved, poor weather conditions, weekend, race (three categories), holiday, number of vehicles, road conditions, speed limit, age, commercial vehicle, gender (two categories), number of lanes, and vehicle model year.

[b]Here, we removed both gender categories.

[c]Here, we removed three race categories.

FIGURE A1

Synthetic New Jersey, Varying Presence of Lagged Outcome Variables

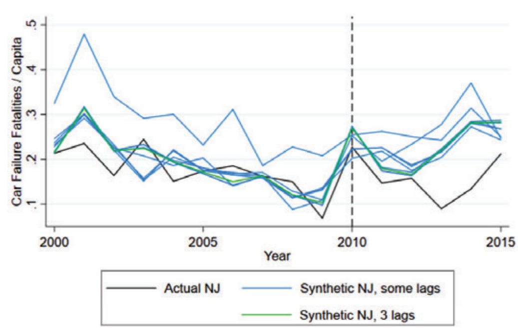


TABLE A3
Synthetic New Jersey State Weights, Varying Presence of Lagged Outcome Variables

	'00	**'05**	**'09**	**'00, '05**	**'05, '09**	**'00, '05, '09**	**'00–'09 (All)**	**'00–'09 (Average)**
RMSPE	0.0518	0.0516	0.0392	0.0511	0.0392	**0.0385**	0.0377	0.0448
DE	0	0	0	0	0	**0**	0	0
HI	0.030	0	0	0.024	0	**0**	0	0
LA	0	0	0	0	0	**0.005**	0.015	0
ME	0	0	0	0	0	**0**	0	0
MA	0.686	0.652	0.387	0.633	0.386	**0.414**	0.423	0.699
MO	0	0	0	0	0	**0**	0	0
NH	0	0	0	0	0	**0**	0	0
NY	0	0	0	0	0	**0**	0	0
NC	0	0	0	0	0	**0**	0	0
PA	0.015	0	0	0	0	**0**	0	0
RI	0	0	0.288	0	0.289	**0.261**	0.257	0
TX	0.052	0.071	0	0.059	0	**0**	0	0
UT	0	0	0	0	0	**0**	0	0
VT	0	0	0	0	0	**0**	0	0
VA	0.217	0.277	0.325	0.283	0.324	**0.320**	0.306	0.106
WV	0	0	0	0	0	**0**	0	0.194

FIGURE A2

Synthetic New Jersey, Varying Presence of Nonlagged Predictors

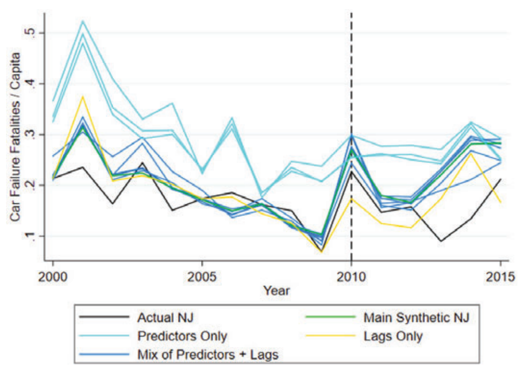


TABLE A4
Synthetic New Jersey State Weights, Varying Presence of Nonlagged Predictors

	All Predictors + Lags	**4 Predictors + Lags**	**Lags Only**	**4 Predictors Only**
RMSPE	0.0520	**0.0385**	0.0507	0.1318
DE	0	**0**	0.004	0
HI	0.025	**0**	0.011	0
LA	0	**0.005**	0.004	0
ME	0	**0**	0.006	0
MA	0	**0.414**	0.602	0.607
MO	0	**0**	0.006	0
NH	0	**0**	0.006	0
NY	0.427	**0**	0.014	0
NC	0	**0**	0.005	0

TABLE A4
Continued

	All Predictors + Lags	**4 Predictors + Lags**	**Lags Only**	**4 Predictors Only**
PA	0	**0**	0.006	0.036
RI	0.304	**0.261**	0.302	0
TX	0	**0**	0.006	0.156
UT	0	**0**	0.005	0.200
VT	0	**0**	0.002	0
VA	0.244	**0.320**	0.012	0
WV	0	**0**	0.008	0

FIGURE A3

Synthetic New Jersey, Varying Pretreatment Range

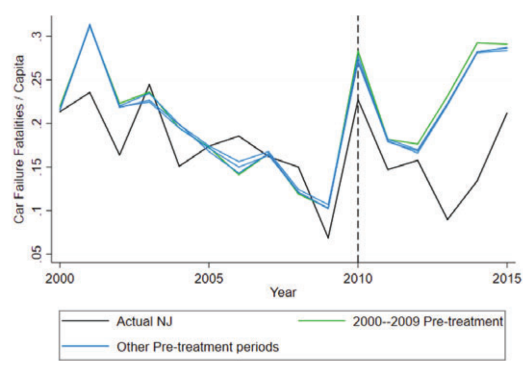


TABLE A5
Synthetic New Jersey State Weights, Varying Pretreatment Range

	2000–2009	2002–2009	2004–2009	2006–2009
RMSPE	**0.0385**	0.0381	0.0391	0.0392
DE	**0**	0	0	0
HI	**0**	0.004	0.007	0
LA	**0.005**	0.008	0	0
ME	**0**	0	0	0
MA	**0.414**	0.411	0.378	0.387
MO	**0**	0	0	0
NH	**0**	0	0	0
NY	**0**	0	0	0
NC	**0**	0	0	0
PA	**0**	0	0	0
RI	**0.261**	0.258	0.279	0.264
TX	**0**	0	0	0
UT	**0**	0	0	0
VT	**0**	0	0	0
VA	**0.320**	0.319	0.336	0.332
WV	**0**	0	0	0.017

TABLE A6
Synthetic New Jersey State Weights, "In-Time" Placebo Test

State	Typical Synthetic	2005 Placebo
DE	**0**	0
HI	**0**	0
LA	**0.005**	0
ME	**0**	0
MA	**0.414**	0.432
MO	**0**	0
NH	**0**	0
NY	**0**	0
NC	**0**	0
PA	**0**	0
RI	**0.261**	0.240
TX	**0**	0
UT	**0**	0
VT	**0**	0
VA	**0.320**	0.325
WV	**0**	0.003

FIGURE A4

Synthetic New Jersey, "In-Time" Placebo Test

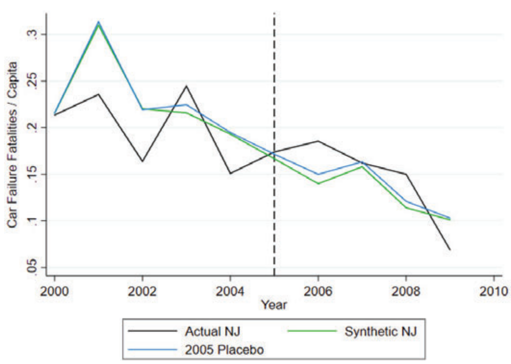


FIGURE A5

Synthetic New Jersey, "Leave One Out" Placebo Test

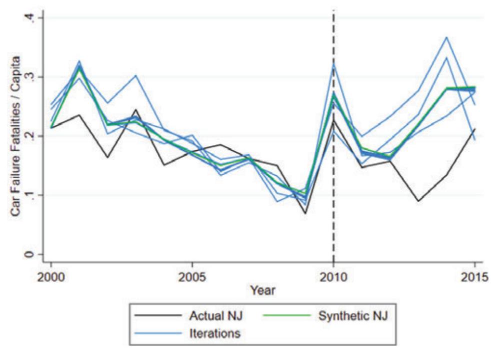


TABLE A7
Synthetic New Jersey State Weights, "Leave One Out" Placebo Test

	All States	DE	HI	LA	ME	MA	MO	NH	NY	NC	PA	RI	TX	UT	VT	VA	WV
RMSPE	**0.04**	0.04	0.04	0.04	0.04	0.06	0.04	0.04	0.04	0.04	0.05	0.05	0.04	0.04	0.04	0.04	0.04
DE	**0**	—	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
HI	**0**	0	—	0	0	0	0	0	0	0	0	0	0	0	0	0.05	0
LA	**0.01**	0	0	—	0.01	0	0	0	0	0	0	0	0	0	0.01	0	0
ME	**0**	0	0	0	—	0	0	0	0	0	0	0	0	0	0	0	0
MA	**0.41**	0.39	0.39	0.40	0.43	—	0.39	0.39	0.39	0.40	0.39	0.69	0.42	0.387	0.43	0.69	0.39
MO	**0**	0	0	0	0	0	—	0	0	0	0	0	0	0	0	0	0
NH	**0**	0	0	0	0	0	0	—	0	0	0	0	0	0	0	0	0
NY	**0**	0	0	0	0	0.34	0	0	—	0	0	0	0	0	0	0	0
NC	**0**	0	0	0	0	0	0	0	0	—	0	0	0	0	0	0	0
PA	**0**	0	0	0	0.01	0	0	0	0	0	—	0	0	0	0	0	0
RI	**0.26**	0.29	0.30	0.29	0.26	0.35	0.29	0.29	0.29	0.29	0.29	—	0.26	0.288	0.25	0.12	0.29
TX	**0**	0	0	0	0	0	0	0	0	0	0	0	—	0	0	0	0
UT	**0**	0	0	0	0	0	0	0	0	0	0	0	0	—	0	0	0
VT	**0**	0	0	0	0	0	0	0	0	0	0	0	0	0	—	0	0
VA	**0.32**	0.32	0.31	0.31	0.31	0.31	0.32	0.33	0.33	0.31	0.33	0.13	0.29	0.325	0.32	—	0.33
WV	**0**	0	0	0	0	0	0	0	0	0	0	0.12	0.02	0	0	0.15	—

FIGURE A6

Synthetic New Jersey, Excluding Geographic Neighbors

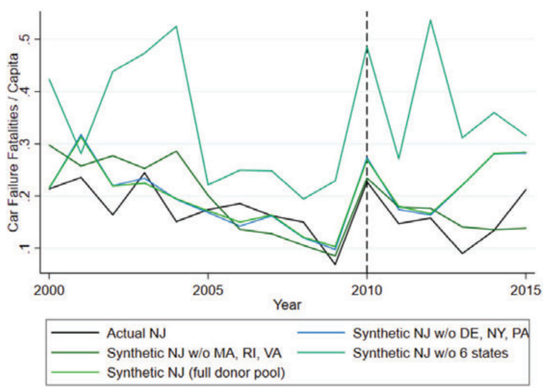


TABLE A8
Synthetic New Jersey State Weights, Excluding Geographic Neighbors

	Typical Synthetic	Without DE, NY, PA	Without MA, RI, VA	Without All 6
RMSPE	**0.0385**	0.0392	0.0672	0.1885
DE	**0**	—	0	—
HI	**0**	0	0.103	0.243
LA	**0.005**	0	0	0
ME	**0**	0	0	0.459
MA	**0.414**	0.386	—	—
MO	**0**	0	0	0
NH	**0**	0	0	0
NY	**0**	—	0.848	—
NC	**0**	0	0	0
PA	**0**	—	0	—
RI	**0.261**	0.290	—	—
TX	**0**	0	0	0
UT	**0**	0	0	0
VT	**0**	0	0	0
VA	**0.320**	0.324	—	—
WV	**0**	0	0.049	0.298

TABLE A9
Variance of Differences for Synthetic Control Placebo Test

State	Fatalities/Capita	Car Failure Fatalities/Capita	% of Car Failure Accidents
NJ	**0.012**	**0.000**	**0.000**
DE	0.197	0.007	0.003
HI	0.065	0.002	0.005
LA	0.088	0.004	0.001
ME	0.128	0.002	0.002
MA	0.015	0.000	0.001
MO	0.053	0.001	0.001
NH	0.102	0.003	0.005
NY	0.010	0.000	0.000
NC	0.038	0.001	0.001
PA	0.014	0.001	0.001
RI	0.063	0.002	0.010
TX	0.030	0.001	0.000
UT	0.065	0.003	0.003
VT	0.177	0.019	0.023
VA	0.020	0.001	0.001
WV	0.204	0.003	0.001

TABLE A10
Trend Analysis of Differences between New Jersey and Synthetic New Jersey (Measured over the Posttreatment Period, 2010–2015)

	Year Level			*Month Level*		
	Time * Post[a]	Breusch-Pagan	Dickey-Fuller	Time * Post	Breusch-Pagan	Dickey-Fuller
Fatalities/capita	−0.00	1.10	−2.78	−0.00	0.32	−7.08***
	(0.00)	(0.30)	(0.06)	(0.00)	(0.57)	(0.00)
Car failure fatalities/capita	−0.00	0.54	−1.50	−0.00*	2.22	−8.43***
	(0.00)	(0.46)	(0.54)	(0.00)	(0.14)	(0.00)
% of car failure accidents	−0.00	1.44	−1.40	−0.00	2.83	−8.24***
	(0.00)	(0.23)	(0.58)	(0.00)	(0.09)	(0.00)

Note: Standard errors in parentheses.
[a]In order to test the differences between regression slopes, we analyze the following model: $(Treated - Synthetic)_t = \beta_0 + \beta_1 * Time_t + \beta_2(Time_t * Post_t) + \varepsilon_t$ and, in this column, report the coefficient β_2, which illustrates the difference in the time trend after the treatment began.
$***p < .01$; $**p < .05$; $*p < .1$.

TABLE A11
Pretreatment Trend Comparisons for Control and Treatment Groups

	Control 1		Control 2		Control 3	
New Jersey Dependent Variables	Traffic Fatalities per Capita	Car Failure Fatalities per Capita	Traffic Fatalities per Capita	Car Failure Fatalities per Capita	Traffic Fatalities per Capita	Car Failure Fatalities per Capita
New Jersey	−0.203	0.00688	0.0230**	−0.392***	−0.221**	0.0241***
	(0.187)	(0.0204)	(0.0112)	(0.114)	(0.0872)	(0.00863)
Year	0.00915	−0.00189	−0.00291**	−0.0353***	−0.0239***	−0.00284***
	(0.0426)	(0.00464)	(0.00122)	(0.0124)	(0.00710)	(0.000703)
*New Jersey * 2000*	*0.331*	*−0.0305*	*−0.0241*	*−0.0493*	*0.0625*	*−0.0245***
	(0.433)	*(0.0472)*	*(0.0162)*	*(0.165)*	*(0.117)*	*(0.0116)*
*New Jersey * 2001*	*0.343*	*−0.0318*	*−0.0363***	*−0.00964*	*0.119*	*−0.0293****
	(0.391)	*(0.0426)*	*(0.0154)*	*(0.157)*	*(0.114)*	*(0.0113)*
*New Jersey * 2002*	*0.366*	*−0.0333*	*−0.0206*	*0.0993*	*0.193**	*−0.0208**
	(0.349)	*(0.0380)*	*(0.0144)*	*(0.147)*	*(0.110)*	*(0.0109)*
*New Jersey * 2003*	*0.318*	*−0.0214*	*−0.0244**	*0.0666*	*0.172*	*−0.0246***
	(0.308)	*(0.0335)*	*(0.0138)*	*(0.140)*	*(0.106)*	*(0.0105)*
*New Jersey * 2004*	*0.296*	*−0.0267*	*−0.0308***	*0.102*	*0.171**	*−0.0300****
	(0.267)	*(0.0291)*	*(0.0129)*	*(0.131)*	*(0.101)*	*(0.0100)*

TABLE A11
Continued

New Jersey Dependent Variables	Control 1 Traffic Fatalities per Capita	Control 1 Car Failure Fatalities per Capita	Control 2 Traffic Fatalities per Capita	Control 2 Car Failure Fatalities per Capita	Control 3 Traffic Fatalities per Capita	Control 3 Car Failure Fatalities per Capita
*New Jersey * 2005*	*0.310*	*−0.0145*	*−0.0199*	*0.167*	*0.208***	*−0.0217***
	(0.226)	*(0.0247)*	*(0.0123)*	*(0.125)*	*(0.0991)*	*(0.00982)*
*New Jersey * 2006*	*0.296*	*−0.0166*	*−0.0141*	*0.130*	*0.193***	*−0.0174**
	(0.186)	*(0.0203)*	*(0.0117)*	*(0.119)*	*(0.0963)*	*(0.00954)*
*New Jersey * 2007*	*0.234*	*−0.0163*	*−0.0183*	*0.131*	*0.191**	*−0.0156*
	(0.149)	*(0.0162)*	*(0.0114)*	*(0.116)*	*(0.0980)*	*(0.00971)*
*New Jersey * 2008*	*0.118*	*−0.0160*	*−0.0165*	*0.105*	*0.141*	*−0.0157**
	(0.114)	*(0.0125)*	*(0.0110)*	*(0.112)*	*(0.0952)*	*(0.00943)*
*New Jersey * 2009*	*0.0546*	*−0.0131*	*−0.0160*	*0.0276*	*0.0408*	*−0.0141*
	(0.0869)	*(0.00947)*	*(0.0108)*	*(0.110)*	*(0.0944)*	*(0.00935)*
Observations	256	256	512	512	768	768
R^2	0.136	0.036	0.030	0.143	0.069	0.037

Note: Standard errors in parentheses.
***$p < .01$; **$p < .05$; *$p < .1$.

TABLE A12
Iterations for the Difference-in-Differences Model[a]

Variables	Iteration 1	Iteration 2	Iteration 3	Iteration 4	Iteration 5	Iteration 6	Iteration 7	Iteration 8
Demographics	**X**					**X**		**X**
Age	X					X		X
White	X					X		X
Black	X					X		X
Hispanic	X					X		X
Gender	X					X		X
Weekend	X					X		X
Holiday	X					X		X
Morning	X					X		X
Afternoon	X					X		X
Evening	X					X		X
Night	X					X		X
Vehicle year	X					X		X
Speed limit	X					X		X
Month fixed effects		**X**		**X**	**X**	**X**		**X**
Year fixed effects				**X**	**X**	**X**		**X**
State fixed effects			**X**		**X**	**X**		**X**
Accident predictors							**X**	**X**
Inclement weather							X	X
Poor surface conditions							X	X
Poor lighting conditions							X	X
Drugs/alcohol involved							X	X
Speeding involved							X	X

New Jersey Control 2: # of Car Failure Fatalities per Capita

	Dems.	**Month FE**	**State FE**	**Month/ Year FE**	**All FE**	**All FE + Dems.**	**Accident Predictors**	**All Predictors**
New Jersey	−0.004	−0.005**	−0.0004	−0.006**	−0.001	−0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Law change	0.024***	0.013***	0.017***	0.017**	0.016**	0.013*	0.015***	0.013*
	(0.004)	(0.004)	(0.003)	(0.008)	(0.007)	(0.007)	(0.003)	(0.007)
New Jersey Law change	***−0.013******	***−0.013******	***−0.015******	***−0.013******	***−0.013******	***−0.011******	***−0.014******	***−0.012******
	(0.004)	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	748	748	748	748	748	748	748	748
R^2	0.7542	0.7399	0.7553	0.7592	0.7788	0.7897	0.7640	0.8049

Note: Standard errors in parentheses.
[a]These models control for autocorrelation of an AR(1) form.
***$p < .01$; **$p < .05$; *$p < .1$.

TABLE A13
Full Demographics and Fixed Effects Model[a]

	Control 1	Control 2	Control 3
State index	−0.002	0.001	0.001
	(0.005)	(0.003)	(0.002)
Law change	0.003	0.013*	0.007
	(0.008)	(0.007)	(0.005)
*State * Law change*	*−0.001*	***−0.012****	*−0.004**
	(0.006)	(0.004)	(0.003)
Age	−0.0001	0.000	0.00004
	(0.0002)	(0.0002)	(0.0001)
White	−0.012*	−0.002	0.007
	(0.007)	(0.008)	(0.006)
Black	−0.024***	−0.004	−0.006
	(0.008)	(0.009)	(0.007)
Hispanic	−0.020***	−0.006	−0.004
	(0.008)	(0.010)	(0.008)
Male	−0.009	0.490	−0.007
	(0.013)	(0.438)	(0.007)
Weekend	0.004	−0.013	−0.014**
	(0.010)	(0.009)	(0.006)
Afternoon	0.007	0.011	0.009
	(0.005)	(0.012)	(0.009)
Evening	0.006	0.006	0.005
	(0.005)	(0.012)	(0.009)
Night	0.005	0.003	0.005
	(0.005)	(0.012)	(0.009)
Holiday	0.0004	0.005***	0.004***
	(0.002)	(0.002)	(0.001)
Vehicle year	−0.007***	−0.003***	−0.003***
	(0.001)	(0.001)	(0.001)
Speed limit	−0.00008	−0.001**	−0.0004**
	(0.0004)	(0.0003)	(0.0002)
Inclement weather	0.010	0.006	−0.0002
	(0.019)	(0.015)	(0.011)
Poor road surface	0.009	0.006	0.010
	(0.016)	(0.013)	(0.009)
Poor road lighting	0.067***	0.041***	0.032***
	(0.009)	(0.009)	(0.006)
Drug/alcohol involved	0.006	0.026***	0.035***
	(0.009)	(0.007)	(0.006)
Speeding involved	0.033***	0.008	0.007*
	(0.005)	(0.005)	(0.004)
February	−0.001	−0.0002	−0.001
	(0.003)	(0.004)	(0.003)
March	0.001	0.0001	0.001
	(0.003)	(0.004)	(0.003)
April	0.005	0.004	0.003
	(0.003)	(0.004)	(0.003)
May	0.006*	0.004	0.004
	(0.003)	(0.004)	(0.003)
June	0.013***	0.007*	0.006*
	(0.003)	(0.004)	(0.003)
July	0.016***	0.008*	0.07**
	(0.004)	(0.004)	(0.003)
August	0.015***	0.011**	0.010***
	(0.003)	(0.004)	(0.003)
September	0.012***	0.011**	0.009***
	(0.003)	(0.004)	(0.003)
October	0.010***	0.007	0.006**
	(0.003)	(0.004)	(0.003)
November	0.010***	0.003	0.003
	(0.003)	(0.004)	(0.003)
December	0.007**	0.002	0.002
	(0.003)	(0.004)	(0.003)

TABLE A13
Continued

	Control 1	Control 2	Control 3
Pennsylvania		0.008***	0.001
		(0.002)	(0.002)
Delaware		0.007***	0.004***
		(0.002)	(0.001)
Maryland			0.002
			(0.001)
Connecticut			0.001
			(0.001)
New York			−0.0002
			(0.001)
Observations	374	748	1,122
R^2	0.8546	0.8049	0.7880

Note: Standard errors in parentheses.
[a]Covariates not shown: year fixed effects (14) and AR(1) autocorrelation.
$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$.

TABLE A14
Difference-in-Differences Models Using Lagged Treatment Indicators[a]

Length of Lag	Fatalities/ Capita	Car Failure Fatalities/ Capita	% of Car Failure Accidents
6 months	0.05**	0.01	0.00
	(0.02)	(0.00)	(0.00)
12 months	0.04	−0.00	−0.01
	(0.02)	0.00	(0.00)
18 months	0.05*	0.00	0.00
	(0.02)	(0.00)	(0.00)
24 months	0.05	−0.00	−0.00
	(0.03)	0.00	(0.00)
30 months	0.05*	0.00	0.00
	(0.03)	(0.00)	(0.00)
36 months	0.05	−0.00	−0.00
	(0.03)	(0.00)	(0.00)
42 months	0.05	0.01*	0.01
	(0.03)	(0.01)	(0.00)
48 months	0.03	−0.00	−0.00
	(0.04)	(0.01)	(0.01)
54 months	−0.01	0.01	0.00
	(0.05)	(0.01)	(0.01)
60 months	0.13*	0.01	0.00
	(0.07)	(0.01)	(0.01)

Note: Standard errors in parentheses.
Note: [a]Each cell reports the coefficient for the *New Jersey * Lagged Treatment* dummy variable—that is, significant coefficients indicate a delayed treatment effect of the law change. Each of the estimated models includes month and year fixed effects, and controls for an AR(1) process.
$^{***}p < .001$; $^{**}p < .01$; $^{*}p < .05$.

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