Do Safety Inspections Improve Safety? Evidence from the Safety Inspection Program for Commercial Motor Vehicles

Yuanning Liang*

July 2020

[Preliminary. Please do not cite.]

Abstract

Regulatory efforts to reduce public hazards could be undermined when agents exhibit risk compensating behaviors. This paper evaluates the North American Standard Inspection Program, a national safety regulation for commercial vehicles. I quantify changes in road safety due to this regulation for the past two decades, analyze the causes of those changes, and suggest possible ways of improving the regulatory design. I compile a comprehensive database on inspections and crashes for 23 million trucks ever inspected from 1996 to 2018. Linking inspection and crash history using each truck's unique vehicle identification number (VIN), I implement an event study that tracks a given truck's crash rate shortly before and after inspection. The main finding is a sharp, 43.5% *increase* in crash rate immediately following an inspection, and the effect lasts for at least 14 days. Further analysis points to a Peltzman-style explanation: since a truck is less likely to be re-inspected shortly after an initial inspection, the driver conducts fewer pre-trip checks and drives more recklessly after an inspection, which leads to more crashes. The increase of crash due to drivers' compensating behaviors costs around 1.6 billion dollars to the society each year. Through a comparison of different regulatory designs across states, this paper proposes alternative policy designs that could achieve crash reduction through randomizing the inspection schedule.

Keywords: Enforcement and compliance; Peltzman effect; road safety; trucking industry

---

*Liang: The Dyson School of Applied Economics and Management, Cornell University (email: yl2544@cornell.edu). I am very grateful to the suggestions and guidance by my committee members, Shanjun Li, Cathy Kling, Crocker Liu, and Andrew Waxman. I also thank Todd Gerarden, Matt Khan, Ivan Rudik, Eric Zou, and seminar participants at the AERE 2020 Virtual Conference, the 2019 University of Colorado Boulder Environmental Resource Economics Workshop, the 2019 National Tax Association Annual Conference, the 2019 Southern Economic Association Annual Meetings, the Cornell Dyson School Sustainable Environment, Energy and Resource Economics Research Seminar for helpful comments.

1 Introduction

The effectiveness of regulation relies on the design of regulatory enforcements. Enforcement mechanisms adopted by regulators often aim to increase the deterrent effect from either increasing the probability of detection (Duflo et al. (2018)), or increasing sanction severity through fines or imprisonment (Muehlenbachs et al. (2016), Levitt and Porter (2001)). However, one overlooked dimension of effective enforcement is people's learning about enforcements and abilities to respond to regulations when the enforcement schedule is predictable. There are many examples of predictable enforcements. The police choose a fixed "hot spot" to crack down crimes (Banerjee et al. (2019)), Environmental Protection Agency conducts more inspections on target groups (Blundell et al. (2018)), and insurance companies use repeated audit strategies (Okat (2016)). In this paper, I look at the scenario when regulators tend to avoid conducting repetitive enforcements on recently checked agents to save regulatory efforts. Such predictability could undermine the overall effectiveness of the regulation if people choose to reduce compliance correspondingly. Hence, understanding the regulatory enforcement designs and people's compliance response is crucial for informing policy design.

This paper provides new insights to the study of regulatory enforcement and compliance by evaluating the effectiveness of a road safety regulation. There is an extended discussion in the literature on how behavioral responses of drivers to automobile regulations can counteract the intent of the policies (Peltzman (1975), Cohen and Einav (2003), Evans and Graham (1991)), namely, the Peltzman effect. The argument was first pushed forward by Sam Peltzman in 1975 that points out the offsetting effect of drivers' responses to the seat belt law in reducing the highway death rate. Since motor vehicle traffic crash is the leading cause of death in the US[1], it is of vital importance to critically evaluate the regulatory efficacy on road safety.

This paper looks at the North American Standard Inspection Program, a national safety regulation for commercial vehicles. I quantify changes in road safety due to this regulation for the past two decades, analyze the causes of those changes, and suggest possible ways of improving the regulatory design. In particular, this paper provides direct evidences to the existence of drivers' offsetting responses to road safety regulations using a large scale data in the US trucking industry.

The US trucking industry represents a multi-billion sector that transports the nation's freights and provides essential services to passengers across the North America. The value of shipments transported by truck accounts for 79.3% from all modes of transportation. But it is also a major source of safety externality (Muehlenbachs et al. (2017)): every year, more than 130,000 people are involved in large truck crash accidents, and about 5,000 people die from those accidents.[2] The Department of Transportation (DOT) implemented a national safety inspection program for

[1] From NHTSA's National Center for Statistics and Analysis.

[2] From National Highway Traffic Safety Administration (NHTSA) calculation.

all commercial motor vehicles[3] (CMV) to ensure both vehicles and drivers are safe for travel. The inspections are random checks performed at the weigh stations along the highways by law enforcement personnel. All trucks have to enter the weigh stations when they are open. While the program has been in place for decades, there is a dearth of empirical study on what the program actually does to road safety in the US.

Despite the potential benefits to road safety, the CMV safety inspection program has attracted a lot of controversy on whether its benefits have exceeded its costs. First, the benefit of the program is unclear due to a lack of empirical evidence in how much deaths it has prevented. Second, the cost of enforcement by the regulators and the cost of compliance from the private trucking companies are enormous. Every year, 7,000 certified roadside inspectors throughout the US conduct approximately 3.5 million inspections. But the enforcement agencies are already questioning the program's efficacy as it "no longer leading to annual increases in the industry-wide level of compliance with safety regulations" (GAO (2005)). In the private sector, an anecdotal evidence shows that for each in-and-out at the inspection stations, trucks lose \$2.5 per minute they spend at the stations[4]. Although huge efforts have been made in conducting the inspections, without an enhancement in safety there is no rationale for the regulation.

This paper evaluates the effectiveness of the roadside safety inspection program using the most comprehensive data files on trucks, roadside safety inspections, and crash accidents ever complied. The set of inspection files are obtained from the Department of Transportation's national network of highway weigh stations and patrol inspectors. It contains a complete inspection record of more than 23 million trucks that ever received an inspection in all 50 states from 1996 to 2018. The crash files are collected from the state police crash reports in the same period covering all accidents involving CMVs. The two sets of file have an advantage over other accident analysis files (e.g. Fatality Analysis Reporting System (FARS), NHTSA State Data System) since they record the full vehicle identification number (VIN) for trucks involved. Linking inspection and crash history using each truck's unique VIN, the data allows me to identify the trucks involved in crashes with their inspection records so that I can implement an event study research design that tracks a given truck's crash rate shortly before and after it receives an inspection.

The main finding of this paper is a sharp, 43.5% *increase* in crash rate immediately following an inspection, and the effect lasts for at least 14 days. In the longer term, the increase in crash rate can be detected up to 12 months after inspections. There is no compensating reduction in crash

[3]Section 204 of the Motor Carrier Safety Act of 1984 (MCSA) (Pub. L. 98-554, Title II, 98 Stat. 2832, at 2833) defined a "commercial motor vehicle" as one having a gross vehicle weight rating (GVWR) of 10,001 pounds or more; designed to transport more than 15 passengers, including the driver; or transporting hazardous materials in quantities requiring the vehicle to be placarded.

[4]The estimates are reported from Doug Johnson, director of marketing for Drivewyze, which offers a pre-clearance weigh station bypass service. The company analyzed around 13 million individual "site visits" across the US between September and October of 2015.

after 12 months. The current inspection design leads to 1803 additional crashes per year, costing roughly \$1.6 billion.

Why do truck inspections increase crash rates? This paper then explores potential mechanisms underlying the finding. I find evidences that a truck is much less likely to be re-inspected for at least a quarter following an initial inspection. Correspondingly, I find a larger increase in single-vehicle crashes, such as non-collision crashes or collision with unmovable objects, which are caused by drivers' behaviors, comparing to multi-vehicle crashes. Moreover, the number of crashes due to reckless driving or a lack of vehicle maintenance increases after inspections. This suggests a Peltzman-style explanation: knowing that the truck will not be re-inspected in the near term, the driver might conduct fewer pre-trip checks at the truck, and drive more recklessly on the road, which offsets the potential benefits of the safety program.

To illustrate the driver's decision making process, I develop a utility maximization model on driver's traffic safety behaviors following the framework developed by Peltzman (1975) and Blomquist (1986). The main result of this model suggests that, when drivers expect zero re-inspection probability following an inspection, they exert less private safety effort (e.g., drive faster, do less maintenance) comparing to the case when the re-inspection probability stays the same regardless of the truck's inspection status. As a result, the total number of crashes is larger when the re-inspection probability is zero.

Empirically, I provide several tests to the identification assumption, and discuss important confounding factors which I can rule-out. Throughout the analysis, the validity of the event study framework relies on the identification assumption that, for the same truck, the trend in crash rate should continue in the absence of an inspection[5]. First, I show that there is no pretrend in crash in the 14 days preceding the inspection. Second, I perform a placebo test which randomly assigns inspections to trucks in the sample. The test shows that the placebo inspections do not lead to an increase in crash. So the increase in crash found in the observed sample can only be caused by the observed inspections. Third, I can rule out that changes in weather pattern or traffic condition explain the increase in crash after an inspection. Fourth, I indirectly test for selection of inspections based on the probability of a truck being on road. Lastly, I eliminate the possibility of reverse causality by dropping trucks that crash 18 hours before inspection.

On the effect size, there is little heterogeneity across different inspection outcomes. I find that the effect of an inspection on crash is 46.5% for trucks that receive at least one violation[6] (41% of all inspections) versus 42.6% for trucks that do not receive any violations (38% of all inspections). There is also limited heterogeneity in effect sizes across different firm characteristics. The changes

[5] Gray and Shimshack (2011) discusses the endogeneity of selected inspections, here I resolve the issue by including individual truck fixed effects.

[6] Those violations exclude the out-of-service violations, which is 20% of all inspections.

in crash due to inspections are quite comparable for large or small firms[7], firms carrying different types of cargo, and firms doing more interstate or intrastate businesses. The similarity in effect sizes across various dimensions shows that the compliance behaviors from drivers or firms are comparable when the re-inspection probability is low. But I do find a strong spatial heterogeneity in effect sizes across different commuting zones. This spatial heterogeneity is largely driven by the differences in the enforcement strategies across regions.

This paper seeks to answer a few policy relevant questions with the data: How should the states design the inspection strategy considering drivers' offsetting behaviors to safety regulations? Would it be better to inspect trucks on a random frequency? In order to answer these questions, I explore the heterogeneity in the inspection schedules across states, and find that there is a greater crash reduction in states with more unpredictable schedules. In addition, I explore the extent to which the inspectors rely on time passed since last inspection to determine which trucks are chosen for inspection today. I find that states inspect trucks on a greater randomness achieve better results in terms of crash control[8]. Exploration of these questions help shed light on a better design of the national safety inspection network.

This paper contributes to the literature in the following four ways. First, this paper contributes to a broad literature on regulatory efficacy through periodic or random inspections beyond auto safety regulations. This paper illustrates that predictable enforcement hinders the regulatory effect as drivers exhibit compensating behaviors after inspections. The literature find different regulatory designs optimal under various contexts. Blundell et al. (2018) evaluate the benefits of EPA's dynamic enforcement strategy on high priority violators of Clean Air Act and Amendments, Duflo et al. (2018) show that discretion in targeting of inspections helps enforcement of pollution regulation in India, Oliva (2015) evaluates the effectiveness of vehicle emission inspections in Mexico City and finds prevalent corruptions, Mookherjee and Png (1989) and Okat (2016) both find that an auditor benefits from randomizing the choice of auditing methodology over time, Muehlenbachs et al. (2016) find stricter enforcement from larger inspection teams than more inspections on oil and gas platforms, Banerjee et al. (2019) show better effect of anti-drunk driving crackdown at random checkpoints in India, Shimshack (2014) reviews theoretical models and empirical evidences of environmental regulatory enforcements.

Second, this paper highlights the importance of critically evaluating road safety regulations, which contributes to a growing literature on road safety for all motorists and pedestrians sharing the road. Graham et al. (2015) find a 5–23% increases in crash rates with shale-gas drilling due to increased heavy truck traffic, Muehlenbachs et al. (2017) estimate the externality of the presence

[7]The sizes of carrier companies are measured using their inventories, which are the total number of trucks or drivers in the company.

[8]Within sample estimates of effect sizes are still positive for all states, but one could extrapolate from the estimates that greater crash reduction could happen if states have chosen to inspect trucks on a greater randomness.

of a truck on the road which increases the rate of accidents using car insurance rates. Beyond large trucks, Li (2012) and Anderson and Auffhammer (2014) find that the presence of heavier vehicles leads to more severe accidents. Edlin and Karaca-Mandic (2006) estimate that the correcting Pigouvian tax for auto accident externalities is over $220 billion per year nationally between 1987 to 1995. Van Benthem (2015) discusses the optimal speed limit law, and Ashenfelter and Greenstone (2004) evaluate the consequences of raising the speed limits which then inform the value of statistical life.

Third, this paper builds on an extensive literature that study people's offsetting behavioral responses to safety regulations. On road safety, the literature initially focused on theoretical works (Peltzman (1975), Blomquist (1988)). Despite extensive empirical studies afterwards, for example, regarding seat belt use (Cohen and Einav (2003), Evans and Graham (1991), Lv et al. (2015)) and other safety regulations like the point-record driving license (Benedettini and Nicita (2012)), controversies remain about whether drivers' responses offset the regulatory effect and to what extent. This paper provides direct evidence to the Peltzman effect. More broadly, the debate of the Peltzman effect also exists in the context of product safety regulation (Viscusi (1984)), occupational health and safety regulations (Viscusi (1986)), soft drink taxes (Fletcher et al. (2010)), and cigarette taxes (Adda and Cornaglia (2006)).

Fourth, this is the first study that looks at the impact of the CMV safety inspection program on crash using best data available from all states during a long time frame of 1996 to 2018. Since the DOT only collected detailed inspection record from 1989 onwards, and data in the earlier period before 1996 was missing for many of the data entries[9], this paper uses all data available at the most granular level (individual truck level) for the research question asked. Previous studies of this question use either data from a few states only (Loeb and Gilad (1984), Kwigizile et al. (2016)), or less granular data at county level (Keeler (1994)), or data from longer than 10 years ago (GAO (2005)).

The paper proceeds as follows. Section 2 reviews the institution details of the trucking industry and the CMV safety inspection program. Section 3 provides a utility maximization model of driver's safety behaviors. Section 4 describes the data and sample used in the paper. Section 5 presents the empirical framework, main findings of the paper, and identification challenges. Section 6 analyzes the mechanism behind the increase of crash after inspection. Section 7 examines the heterogeneity of the impact of inspection on crashes. Section 8 discusses alternative regulatory designs that improve safety. Section 9 concludes.

[9]The data requested from DOT is from 1989 to 2018, but the data quality before 1996 is significantly poorer. For example, all records of VIN are missing for truck inspected before 1996, the record of inspection facility are also largely missing before 1994, and all inspection records in the year 1995 was not available. So this paper uses data starting from 1996 for consistency of data availability and quality.

2 Regulatory framework

2.1 The trucking industry

Commercial motor vehicles (CMV), or trucks and buses for simplicity, compose a huge and important sector in the United States. It is the lifeblood of the US economy. In 2016, the total value of shipment transported by truck within the US was $700 billion, which accounted for 79.3% from all modes of transportation. Among the 269 million total registered vehicles, 11 million (4% of the total) were trucks[10], and 1 million (0.4% of the total) were buses. Also in 2016, there were 3,174 billion vehicle miles traveled (VMT) by all motor vehicles. Large trucks traveled 287.9 billion miles (9.1% of the total), and buses traveled 16.3 billion miles (0.5% of the total). The trucking industry is also important because of the significant number of employment it provides. There were 6 million truck and bus drivers, and 543,061 active motor carrier companies operating in the US in 2017.[11].

2.2 CMV safety

However, despite the fact that the trucking sector promotes transport of goods and passengers, it poses a huge cost on human lives. Every year in the US, more than 130,000 people are involved in CMV-related accidents, more than 60,000 people injured from those accidents, and more than 5,000 people died from those crashes[12]. Large trucks and buses together accounted for 7% of the total number of vehicle fatalities in the US in 2016. Over the past 10 years, the number of fatalities involving large trucks increased 17%, while the VMT only increased 10%.[13]

Since truck safety has significant implications for both truck drivers and other motorists sharing the road, safety is a major consideration for the trucking industry. The Federal Motor Carrier Safety Administration (FMCSA) is the major regulatory agency to improve safety and prevent CMV-related accidents. The agency was established as a separate administration within the US DOT on January 1, 2000, pursuant to the Motor Carrier Safety Improvement Act of 1999. It assumed almost all of the responsibilities and personnel of the Federal Highway Administration's Office of Motor Carriers established in 1966. The federal government spends over $600 million per year on the truck safety program under the Fixing America's Surface Transportation (FAST) Act. Approximately 7,000 certified roadside inspectors throughout the United States assist the agency by conducting approximately 3.5 million roadside inspections per year. The inspections are known

[10]Trucks include single-unit trucks (8.7 million) and combination trucks (2.8 million).
[11]From Federal Motor Carrier Safety Administration (FMCSA) 2018 Pocket Guide to Large Truck and Bus Statistics.
[12]From National Highway Traffic Safety Administration (NHTSA) calculation.
[13]Statistics sources are from NHTSA and Bureau of Transportation Statistics (BTS). The percentage increase of crash is calculated by author.

as the North American Standard Inspection Program.

2.3 Safety inspection program

CMV Roadside Safety Inspections are random checks performed at fixed weigh stations or mobile locations along the major highways to ensure both vehicles and drivers are safe for travel. Approximately 95% of all inspections are conducted by inspectors at each state DOT, with the remainder conducted by federal inspectors. Although it is a federal regulation, each state DOT has its own way of handling roadside inspections. Some mainly perform safety inspections at weigh stations along major highways (California), some focus on border inspections (Michigan), and some have agents patrolling the highways and can pull over any truck at any time (New York), most states have a mixed strategy that composes all. The strategy adopted by the states also change over time, for example, Pennsylvania used to have more pull-over inspections from before 2015, but starting in 2016, the number of weigh station inspections increased dramatically.

2.3.1 The weigh station inspections

At weigh stations, all trucks passing by have to enter for inspections if the weigh stations are indicated open[14]. I self-recorded a video showing that it is indeed the case that all trucks in sight complied, the video can be viewed upon request. Figure A.1 shows the procedures of an inspection: the first picture in Figure A.1 shows a line of trucks waiting at the ramp to enter the weigh station for inspections. Once entered, all trucks drive on a scale to get weighted (Figure A.1b). While they are weighted, the inspectors are watching and listening. If they see or hear anything wrong with the driver or the vehicle, they will inspect the truck. The inspectors will also examine the truck's company Safety Measurement System (SMS) score reported on the monitors to determine whether the trucks will receive a closer inspection or not (Figure A.1c). Therefore, all trucks have to enter the weigh station, but only a selected number of trucks will go through the "real" inspections, others are allowed to proceed immediately (just like the truck in the video). The inspections in the data files used in this study include only those "real" inspections.

What is it that determines whether a truck is going to be inspected? First, as mentioned previously, if the inspector suspects any problem with the truck or the driver based on the her observations on site, the truck will get inspected. Second, trucks can also be selected if they have bad safety histories. The SMS assesses a carrier's performance calculated from information collected from inspections, crash records, company reviews and violations from investigations. Carriers will be targeted for inspections if their SMS scores exceed the intervention threshold (the higher the

[14]Only a small number of carriers with good safety scores are allowed to participate in Weigh Station By-Pass system. The bypass system on board will show whether the truck need to enter into the weigh station for inspection or not.

safety score, the worse the carrier). This scoring system implies that trucks belong to carriers that have worse inspection and crash records will be receiving more inspections.[15] Inspectors will also focus less on vehicles recently passed an inspection so that inspection efforts are not duplicated for trucks already inspected. The selection of truck inspection based on recent inspection histories is a critical detail in the main mechanism behind the findings of this paper.

During the inspection (Figure A.1d), there are six types of Behavior Analysis and Safety Improvement Category (BASIC) inspections conducted depending on the level of scrutiny decided on site. The inspections cover both the driver and the vehicle. On the driver's side, inspections cover unsafe driving behaviors, hours-of-service compliance, controlled substances/alcohol consumption, and driver fitness. On the vehicle's side, the inspections cover all aspects of vehicle maintenance and cargo securement. For hazardous material carriers, inspections also cover the hazmat compliance.

2.3.2 The inspection outcomes

Different violations are given for problems found during the inspections based on criteria created by Commercial Vehicle Safety Alliance (CVSA). Multiple violations could be given for a truck if multiple issues were found. Among all inspections conducted at weigh stations, there are 41% that result in at least one violation but not an out-of-service violation. For those violations, 40% are assigned to drivers, 75% are assigned to vehicles, and 1% for hazmat carriers.Driver violations could incur a higher fine than vehicle violations in general. Driver violations could even result in suspension of license which deters the drivers more than vehicle violations. During inspections, trucks with violations that do not result in the vehicle to be placed out-of-service are still allowed to proceed for operation, but any violations or defects noted at the time of inspection must be corrected within 15 days of receiving the violation.

There are 20% of inspections result in out-of-service violations. A vehicle being placed out-of-service (OOS) during inspections indicates that the vehicle or driver has made severe violations which present imminent hazards to the public. Also the vehicle may not be driven again until all necessary repairs are made and all the violations are corrected.

There are 38% inspection that do not find any violations. If during an thorough vehicle inspection, there are no critical violations assigned to the vehicle itself, then the vehicle could be issued a CVSA decal. A decal is a colored sticker featuring the year and quarter in which the inspection was performed. Generally, a vehicle displaying a valid CVSA decal will not be re-inspected during the three-month time frame in which the decal is valid. Note that the CVSA decal is only valid for the vehicle, so if the driver displays any violations, he or she could still receive an inspection. Among

[15] There is a recent debate on the fairness of the scoring system for small carrier companies since they have very few records.

all inspected vehicles, 16.6% receive a decal.

The main takeaway from looking at the inspection outcomes is the following. Since more than 60% of inspections result in some sort of violations. The inspectors have done a reasonably well job in detecting problematic trucks for inspection. However, the deterrence effect of the inspection is likely to be weak. The intervention threshold on SMS score is around 70-80%, which means that only the worse 20-30% trucks in rank are going to be targeted for more inspections. Because the OOS rate is 20%, those targeted carriers are also likely to be those that receive OOS violations during inspections. Most trucks only receive non-OOS violations, so they are not alerted against the violations. In section 7, I analyze the effect of inspection on crash depending on different violations received during inspections.

3 A model of driver's safety behaviors

In this section, I present a theoretical model on driver's traffic safety behaviors following the framework developed by Peltzman (1975) and Blomquist (1986). I extend their models by incorporating driver's expectations on the probability of getting another inspection following an initial one, which is the driving mechanism for the theoretical results. In this model, a driver balances between reduced risk and increased cost from private safety expenditures and the compliance costs to the inspection regulation. Drivers can increase private safety effort through adopting safer driving behaviors, moderate traveling speed, and vehicle maintenance. Compliance costs to the inspection regulation includes fines received from violations and time spent at the inspection stations.

In particular, I compare driver's safety behaviors under two inspection designs that vary in their re-inspection probability: in the first design, inspectors tend to choose trucks that are not inspected for a long time for inspection, so the re-inspection probability drops to zero for trucks recently inspected; in the second design, the re-inspection probability stays the same regardless of the trucks inspection history. Drivers form different expectations on the re-inspection probability depending on the inspection designs[16].

The main result of this model suggests that, when drivers expect zero re-inspection probability following an initial inspection, they exert *less* private safety effort comparing to the counterfactual scenario when the re-inspection probability stays the same regardless of the truck's inspection status. As a result, the total number of crashes is *larger* when the re-inspection rate is zero.

[16]Here I frame the question as having inspectors choose trucks for inspections based on different criteria under two types of inspections designs, which makes drivers form different expectations on their own probability of getting an inspection. In other words, in this model I assume that drivers' expectations about their probability of getting inspections are consistent with the true probability.

3.1 Model setup

The model simplifies the driver's problem in the following ways: first, drivers are assumed to receive no direct utility from driving itself; second, drivers are risk neutral so that utility maximizing drivers maximize their expected income; third, this model presents driver's optimal choices and ignores the resulting effects on non-drivers.

In the model, drivers maximize their expected income by choosing the optimal private effort e in reducing accident loss and avoiding regulatory fines.

$$\max_e E = \beta S_1 + (1-\beta)S_2, \tag{1}$$

where E = the expected income for a given driving mileage, β = probability of inspection, and S_1, S_2 = net income when encountering an inspection and no inspection, respectively. If the driver gets an inspection then the driver's net income is

$$S_1 = p(e,v)(I - D(e,v) - L) + (1 - p(e,v))(I - D(e,v)), \tag{2}$$

where I = gross income for driving a given mileage in the limit where the effort devoted is zero and no inspection happens. $p = p(e,v)$ is the probability of crash, which is defined as a function of private effort e and the inspection v. The inspection v is specified by $v = v(e)$, which can be thought as the compliance cost to inspections that depends on private effort e. $D(e,v)$ represents the cost from private effort e, like vehicle maintenance costs and longer traveling time due to slower speed, as well as the compliance costs to inspection v. Examples of the compliance costs include direct monetary losses from fines, and indirect losses from wages forgone due to time spent at the inspection stations because the majority of truck drivers earn per-mileage wages. For simplicity, I define $D(e,v) = e + v$. L = the driver's loss from a crash.

If the driver does not get an inspection, the driver's net income is

$$S_2 = p(e,0)(I - D(e,0) - L) + (1 - p(e,0))(I - D(e,0)), \tag{3}$$

The difference between S_1 and S_2 is the probability of crash and the compliance costs to inspection. In S_2, since there is no inspection, $p = p(e,0)$ and $D(e,v) = D(e,0) = e$.

Combining equation 1, 2, 3 and rearranging terms, the expected income is the following:

$$E = \beta[I - e - v - p(e,v)L] + (1-\beta)[I - e - p(e,0)L] \tag{4}$$

The analysis below is framed by comparing two scenarios corresponding to the two inspection designs mentioned previously: in the first scenario, a truck driver receives an inspection which significantly lowers the probability that the driver gets another inspection within a long time. The empirical evidence found in section 6.1 suggests that the re-inspection probability within 1-week after inspection is only 1.3%. So here I assume that the re-inspection probability drops to zero after an initial inspection. In the second scenario, a truck driver receives an inspection which does not affect the probability that the driver gets another inspection, in other words, the probability of getting an inspection is the same for all drivers regardless of their recent inspection status.

In the first scenario, the first order condition for optimal private safety effort can be simplified to:

$$\left.\frac{\partial p}{\partial e}\right|_{e_0} = -\frac{1}{L} \tag{5}$$

where e_0 is the driver's optimal private effort when the re-inspection rate is zero for trucks recently inspected.

In the second scenario, the first order condition becomes

$$\left.\frac{\partial p}{\partial e}\right|_{e_1} = -\frac{1}{L} - \beta\frac{\partial v}{\partial e}\left(\frac{1}{L} + \frac{\partial p}{\partial v}\right) \tag{6}$$

where e_1 is the driver's optimal private effort when the re-inspection rate stays constant.

Assumption.

1. The probability of crash is bounded between 0 and 1, and decreases in driver's private effort e and inspection v: $\frac{\partial p}{\partial e} < 0$, $\frac{\partial p}{\partial v} < 0$, $\frac{\partial^2 p}{\partial e^2} > 0$, $\frac{\partial^2 p}{\partial v^2} > 0$, $\frac{\partial^2 p}{\partial e \partial v} > 0$. In addition, value of inspection $v = v(e)$ (or violations from inspections) decreases in private effort e: $\frac{\partial v}{\partial e} < 0$.

2. Since optimal fines are determined by incorporating loss from accidents to both the truck driver and the other vehicle involved. Drivers prefer not to be inspected because the benefits of inspection in reducing the expected private loss from accident is smaller than the cost of compliance on the margin: $-\frac{\partial p}{\partial v}L < 1$.

Proposition. *Under the assumptions, I derive the following two results from comparing the two scenarios,*

1. *Drivers exert* ***less*** *private efforts when the re-inspection rate drops to zero following an initial inspection (first scenario) comparing to the case when the re-inspection rate stays the same regardless of the truck's inspection status (second scenario).*

2. *As a result, the total loss from crashes (or the total number of crashes) is* ***larger*** *under the first scenario comparing to the second scenario.*

Proof. See Appendix A for the proof of the propositions. □

3.2 The empirical design according to the theoretical model

The optimal testing design of the propositions would involve a comparison of the number of crashes and drivers' private efforts when drivers expect the re-inspection rate to drop to zero after an initial inspection versus a counterfactual state when drivers expect the inspection (or re-inspection) rate to stay constant. The empirical analysis in this paper provides tests by comparing crash rate in the pre-inspection period with that in the post-inspection period.

In the pre-inspection period, which is the 14 days before an inspection, since the truck has not yet received an inspection, the driver expects the probability of getting an inspection in the next few days to be positive and constant. This is consistent with the counterfactual scenario in the theoretical model where the inspection rate stays constant. In the post-inspection period, which is the 14 days after an inspection, since the truck just gets an inspection, the driver expects no inspection in the near term so the re-inspection rate drops to 0.

The empirical analysis below indeed shows that the re-inspection probability drops significantly after an initial inspection. The number of crashes due to reckless driving behaviors increases significantly in the post-inspection period relative to the pre-inspection period. This is consistent with the two predictions of the theoretical model, when drivers expect the re-inspection probability to drop significantly, they exert less private efforts (adopting risky driving behaviors) which then lead to more crashes.

4 Data description

In this research, I compile a comprehensive database on trucks, roadside safety inspections, and crash accidents in every state across the U.S. from 1989 to 2018. The set of files are requested from the Federal Motor Carrier Safety Administration (FMCSA). FMCSA maintains a complete record of inspection and crash accidents for commercial motor carriers (truck & bus) and hazardous material shippers. The records are electronically transmitted from the states to the FMCSA using a crash reporting system (SAFETYNET). I also observe the firm characteristics of the currently active carriers in the company census data. In addition to the data files obtained from FMCSA, I use fatal crash data from Fatality Analysis Reporting System (FARS), and accident data directly from the Texas DOT. Other supplementary data sets I use to create the crash external condition covariates include highway traffic volume and weather records.

Inspection files The inspection records are collected by state and federal inspectors at the highway weigh stations and roadside pull-over inspections. The inspection files contain information on the time, location, inspection facility, outcome of inspections for more than 63 million inspections from 1989 to 2018. There are 23 million trucks recorded that received inspections.

Figure A.4 shows the total number of inspections and the average number of inspections per truck across years. Figure A.5 in the appendix shows the number of trucks and buses grow at a rate of 3% and 2% respectively per year. As the total number of CMV increases, the total number of inspections conducted has been relatively constant in the recent decade. So the number of inspections that any given truck could receive decreases. For weigh station inspections, Figure A.2 and A.3 in the appendix show that the inspection program covers the whole U.S., but the states vary a lot in their inspection intensities.

The inspection data files also contain information of each truck's unique VIN, license plate number and the carrier company that the truck belongs to. Those information allow me to link each truck's inspection record with other records of the same truck or the same company[17].

Crash files The main crash files I use from FMCSA are collected from state police crash reports. The crash files contain information on the time, location, number of injuries and fatalities, accident event type, VIN, carrier company ID for all trucks ever involved in crash accidents from 1989 to 2018. Figure 1 shows the annual total number of CMV crash accidents increases over time, and the trend is procyclical to the general economy. While the vehicle miles travelled by CMVs[18] exhibits a much mild increase.

The crash files also record the number of vehicles involved as well as the circumstances of the crashes, which allows me to infer the factors contributing to a crash accident. This is the key information that allows me to tease out the mechanism of the findings in the paper.

Other sources of crash records In addition to the crash files from FMCSA, I explore two other sources of crash records. One is the Fatality Analysis Reporting System (FARS) which include all fatal crash records maintained by NHTSA, the other one is the crash records directly obtained from Texas DOT.

The FARS data used in the paper covers from 2000 to 2017. There were in total 472 fatal crashes identified for the same trucks inspected within 14 days before and after the inspection. In addition to the same record entries in FMCSA, FARS also have records for the factors contributed to the accidents, travel speed prior to the accident, driver demographics, and driver's previous traffic convictions. The crash data maintained by Texas Department of Transportation is available from 2010 to 2017 to the public. There were 1,487,842 inspections conducted in Texas. During the 28-day event window around those inspections, 756 crashes occurred in Texas for the trucks that received inspections.

There are both advantages and disadvantages for using the crash data set maintained by FMCSA over the other two sources of crash records. The advantages are two folds: First, FARS data set only contains the first 12-digit of VIN for privacy purposes, but the FMCSA has access to the

[17] In case that the VIN is missing for a vehicle, I use the license plate number as the identifier.
[18] The vehicle miles travelled by CMVs is obtained from the Bureau of Transportation Statistics.

full (17-digit) VIN, which is critical to link to the truck inspection data files[19]. So I can directly compare the crash rate for the same truck before and after it receives an inspection, which is the key of identification. Second, the FMCSA crash files contain all truck crash accidents including non-injury or non-fatal ones. Since I am interested in road safety in general, crashes that result in property damage or/and life loss are both my focuses. As for Texas DOT crash data, although it has full record on all types of crashes, the limitation is that it only records own state crashes.

The disadvantage of using FMCSA crash records alone is that they do not contain detailed entries on the factors contributing to each crash accident, such as driver-related factors, like, speeding, changing lanes recklessly or driving while intoxicated; or vehicle-related factors, like, no brake or malfunctioning lights; or in cases that truck drivers are not at fault. Both FARS crash data and Texas crash data contain detailed information on those crash contributing factors. Therefore, I use them as supplementary data sets that allow me to examine the causes of accidents. They also provide more robustness to my findings with multiple sources of data.

Carrier census data I also request the carrier company census file from FMCSA. This data file is a snapshot of all active operating carrier companies in the U.S. at the time of my request in October 2018. The company census file contains information on the address, registration, type of cargo transports, number of vehicles owned and drivers employed. This data file is useful in particular to look at the heterogeneity analysis among different carrier companies. In section 7, I compare the impact of an inspection on crashes for large versus small companies defined by their inventories: the power units and drivers, as well as for companies carrying different cargo types and doing interstate or intrastate businesses. Table 1 panel D shows that there were 1,669,661 active companies registered as of October 2018. On average, each carrier company employs 5 drivers and possess 21 trucks. Note that a median size carrier company only have 1 driver and 1 truck, so half of the carrier companies are very small, but there are also a small number of giant carrier companies in the industry.

Other data sets Supplementary data sets that I put together for the analysis include the traffic monitoring and traffic volume data from Federal Highway Administration (FHWA), the daily weather records from Global Historical Climate Network Daily (GHCN-Daily). Traffic monitoring data contains the traffic volume records by vehicle class at hourly frequency for the participating states from 2012 to 2018. This data set allows me to directly observe truck traffic volume, and use traffic volume of other types of vehicles as controls for the analysis. GHCN-Daily weather data provides weather records including maximum and minimum temperature, total daily precipitation, snowfall, and snow depth. I use this data set to test whether adverse weather conditions lead to more crashes after inspections.

[19] The last 6 digits of VIN are the serial number of a vehicle. So they are critical in identifying a vehicle.

4.1 Sample construction

I summarize the key variables used in the paper in Table 1. Panel A of Table 1 shows the summary statistics of inspection files. There are 69,549,512 inspections recorded for 23 million trucks in total from 1996 to 2018 across the whole U.S. Among them, nearly half of the inspections are conducted at fixed weigh stations. The weigh station inspections are my main focus of this paper. On average, a truck is inspected 3 times in its lifetime, but only 32% of all trucks get a re-inspection. On average, there are 790 trucks passing through a given inspection county at the inspection hour.

The inspection and crash files are the two main data sets combined to create an inspection-truck-crash daily panel, tracing all crashes happened to the same truck inspected within the time frame of interest. More specifically, since there are 11 million trucks inspected at the fixed weigh stations, it would involve too much computational burden to create a balanced panel for each truck at the daily level for 23 years. The number of observations would be larger than 50 billion[20]. Instead, I construct a 28-day event window around each inspection for any given truck so that I could compare the 14 days before with the 14 days after the inspection. Then for each inspection, I find all crash accidents for the same truck that receives the inspection within the 28-day event window using the VIN in both files[21]. As shown in panel B of Table 1, there are 842,830,408 observations in the panel constructed. There are on average 6.39 crashes per 100,000 trucks inspected in a day. Among those crashes, there are 58.5% crashes result in any injuries, and 3.8% result in fatalities. For all trucks in the sample, a truck on average experiences 0.005 crashes in its lifetime.

In addition to the daily event panel I described in the previous paragraph, I also construct a monthly event panel in order to analyze the long term impact of inspections. The monthly event panel is constructed similarly as the daily panel, but the unit of observation is a month. The time frame of interest here is 12 months before to 24 months after inspection. Table 1 panel C shows the summary statistics of the monthly event panel. There are on average 160 crashes per 100,000 trucks inspected in a month.

5 The impact of inspection on truck crashes

5.1 The econometric framework

This section describes two econometric specifications and the identification assumption required to consistently estimate the effects of an inspection on crash. The first specification is an event study, which allows for the effect of an inspection to vary over the 14 days afterwards and tests the zero-pretend assumption. In the second specification, I estimate the effect using a post-inspection

[20] A truck appears in my sample for 10 years on average.

[21] In case that the VIN is missing for a vehicle, I use the license plate number as the identifier for matching.

indicator for time after the inspection to test for differences in crash 14 days before and after the inspection, which efficiently estimates the average effect size of one inspection on crashes up to 14 days later.

In the event study framework, I regress the number of crashes for the same truck that receives the inspection on a set of inspection indicators from 14 days before to 14 days after inspection, controlling for individual truck fixed effects, year, month, and day-of-week fixed effects. I further control for whether there are other inspections happened before or after the event window using two indicators.

Specifically, the regression equation I use to estimate the impact of an inspection on crash is the following:

$$Crash_{it} = \sum_{\tau=-14,\tau\neq-1}^{13} \beta_\tau Insp_{it}^{\tau} + \beta_{-15} Insp_{it}^{-15} + \beta_{14} Insp_{it}^{14} + u_i + \eta_t + \varepsilon_{it}, \tag{7}$$

where $Crash_{it}$ is the number of crashes made by truck i at time t[22]. $Insp_{it}^{\tau}$ is an inspection indicator, $Insp_{it}^{\tau} = 1$ if as of time t, truck i experiences an inspection τ days ago. Since inspection happens at event day 0 ($\tau = 0$), the 28 days event window is between event day τ = [-14,13]. $Insp_{it}^{-15}$ and $Insp_{it}^{14}$ are indicators equal to 1 if there is any inspection for truck i that happens before or after the 28-day event window, respectively, and 0 otherwise. u_i is the individual truck fixed effect. Each truck is identified using its VIN primarily, or using the license plate number if VIN is missing [23]. η_t includes year, month and day-of-week fixed effects. It is important to control for the day-of-week fixed effects because the inspection schedules largely follow a day-of-week pattern that more inspections are conducted during weekdays, which is consistent with truck traffic being the most during weekdays and less during weekends. I combine the daily inspection indicators into 2-day bins to increase the power of estimation, such as (-14,-13), (-12,-11), ..., (-2,-1), (0,1), (2,3), ..., (12,13) relative to the day of inspection at day 0. The effect of an inspection on crash accidents happening in the two-day bin (-2,-1) is normalized to 0.

In order to estimate the average effect of an inspection on crash throughout the 14 days after the inspection, I use the following econometric framework which regress the number of crashes on a post-inspection indicator, also controlling for outside event window inspections, individual truck fixed effects, year, month, and day-of-week fixed effects. The estimation equation is the following:

[22]Most of the values of $Crash_{it}$ are 0 or 1 at the daily level because a truck normally only crash once in any given day in the estimation sample.

[23]The sample period of this paper is from 1996 to 2018, some trucks may change license plate number in between, so VIN is the best identifier for the same vehicle. However, VIN is missing for many inspection records from 1996 to 2009, so license plate numbers are used to identify a truck if VIN is missing. After 2010, 90% of the inspections have VIN records.

$$Crash_{it} = \gamma post\text{-}insp_{it} + \beta_{-15} Insp_{it}^{-15} + \beta_{14} Insp_{it}^{14} + u_i + \eta_t + \varepsilon_{it}, \tag{8}$$

where $post\text{-}insp_{it}$ is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. The other variables used in this framework is the same as in equation 7.

Throughout the analysis, both econometric frameworks rely on the identification assumption that, for the same truck, the trend in crash should continue in the absence of an inspection. Under this assumption, any difference in crash accidents is caused by the inspection alone. We could then compare the crash rate before and after the inspection to estimate the impact of an inspection on crash. In section 5.3, I discuss why this assumption holds by addressing all potential confounders in this event study framework. It is important to explicitly discuss the confounders here because inspections do not happen randomly at any given time or among all trucks, nor are the inspections distributed identically and independently for a truck each time it enters a weigh station.

5.2 Changes in crash after inspections

The event study framework described above allows me to flexibly estimate the effect of an inspection on crash beginning with the 14$^{\text{th}}$ day prior to the inspection and ending with the 14$^{\text{th}}$ day after the inspection.

The result is summarized in Figure 2. I find that crashes involving trucks *increase* by 43.5% immediately following an inspection relative to a mean of 6.4 crashes per 100,000 trucks inspected per day, and the increase lasts for at least 2 weeks after the inspection. I illustrate the level shift in the number of crashes before and after the inspection in Figure **??** by fitting two horizontal lines using the average of percentage changes respectively. The standard errors are clustered at the truck level. The figure also shows that there is almost no pre-trend during the 14 days before the inspection which proves that the identification assumption is valid.

In order to gauge the effect of an inspection on crash more concisely, I estimate equation 8 using 1 post-inspection indicator instead of 14 leads and lags indicators. Table 2 column 1 shows the regression result which confirms the finding from the event study. It shows that the increase represents 2.8 more crashes per 100,000 trucks inspected in a day comparing to the pre-inspection level, which is 43.5% relative to the daily average crash rate (6.4 crashes per 100,000 trucks).

5.3 Tests for identification

The identification assumption for both econometric frameworks is that, for the same truck, the trend in crash should continue in the absence of an inspection. In this section, I will discuss how I rule out all potential confounders to identification. There are three concerns regarding the identification assumption: first, omitted variables that are correlated to both inspection and crash, in this case, weather conditions and traffic conditions; second, possibility of reverse causality between inspection and crash; third, sample selection issues. Lastly, I implement a placebo test in order to eliminate concerns regarding unobserved covariates for which I cannot test directly.

5.3.1 Potential confounders: weather and traffic

I can rule out weather and traffic conditions as confounders to the identification. Since the inspection schedule is likely to be planned ahead by the inspection personnel, the confounders are factors that could potentially affect the inspection schedule and are also correlated with the probability of crash. Weather conditions, or traffic conditions, are potential confounders because adverse weather conditions, or high traffic volume, are going to cause more crash accidents. At the same time, if inspections are also scheduled at times when crash probability is high in the local area to guarantee that trucks on the road are safer after inspections, then the observed increase of crash following inspections might be caused by inspections scheduled right before heavy rains or rush hours. The direct way of dealing with those two potential confounders is to include them as covariates in the regressions for all 28 days in the event window following each truck's travel path. However, since I do not observe the exact location of the trucks except for the time of inspections and the time of crash if any, I cannot directly control for the weather and traffic. Instead, I use the following ways to rule out the two confounders.

I rule out that weather conditions are confounders by showing that the inspections are not chosen at times of worse weather conditions (rain, or snow), on the contrary, the inspections are chosen at days with better weather conditions. Therefore, even if adverse weather conditions do lead to higher probabilities of crash accidents, the observed *increase* in crash after an inspection are not caused by adverse weather conditions. More specifically, imagine that the inspectors operating a weigh station at Tompkins county in New York state are deciding on what day they are going to do inspections according to the weather forecast for the entire next week.[24] I test if inspections are chosen at worse weather conditions by looking at an event study of inspections on weather conditions of the county where the weigh station is located at, for instance, a rain indicator for Tompkins county. Figure A.6 in the appendix shows the relationship between inspections and the probability of raining. There is a 1.5% drop in the probability of raining on the day of the inspection ($t = 0$) comparing to days before or after. Since it is certain that inspections do not cause rains, the relationship has to be the other way round, that inspections are chosen at days with less

[24] In this exercise, I pre-assume that weather conditions are going to affect the inspection schedules. If the assumption does not hold, which means that inspection schedules are set as stone so weather conditions are not correlated with inspection schedules, then the weather confounders can be ruled out as well.

raining probabilities. The slightly positive coefficients on days before inspection also suggest that inspectors choose to do inspections on a sunny day following rainy days. The results are similar when looking at snowy days. I do not find that temperatures affect the inspection schedule.

I can also rule out that higher traffic volume is the leading cause of the increase in crash after an inspection in the following ways. First, I control for the traffic conditions in the inspection county at the inspection hour as shown in Table B.1. The first column of Table B.1 in the appendix prints the baseline estimation using equation 8 for the sample period 2012-2018.[25] In order to measure traffic conditions more comprehensively, I look at both the truck counts (in column 2) and the percentage of trucks out of all motor vehicles (in column 3). The post-inspection coefficients in column 2 and 3 are almost the same as in column 1, which indicates that the effect of inspection on crash stays the same after controlling for traffic volume. It is consistent with our priori that the coefficient on percentage of truck (pct_truck) is significantly positive in column 3, which indicates that higher truck traffic does lead to more crash accidents, but higher traffic is not correlated with inspections. Second, I interact traffic conditions with the post-inspection indicator to see whether the effect size depends on traffic volume or not. Column 4 and 5 use the truck counts and the percentage of truck traffic to measure traffic condition, respectively. Neither of the coefficients on the interaction term is significant at 5% level. [26] Thus, we can also rule out that traffic conditions affect the effect of inspection on crash.

5.3.2 Eliminate the reverse causality

In order for the identification assumption to hold, inspections induced by crashes should be excluded in the estimation sample, in other words, there has to be no reverse causality. The reason to consider this particular case is because that it is not uncommon that after a truck crashes, the police officer would call an inspector nearby to come also inspect the truck. In this case, there is reverse causality since it is the crash that invites the inspection. I drop all inspections that happen within 18 hours after crashes to eliminate that concern.

In Figure A.7 in the appendix, the increase in crash on day -1 without dropping any inspections reveals the fact that a crash could invite an inspection, which causes a reverse causality problem. The rationale of solving the problem by dropping inspections right after crashes is the following. In principle, an inspection happening on day 0 should not have any differential effect on crash accidents one day or two days before. Even if the truck drivers anticipate that an inspection is going to happen in the near term, they would not be able to know for sure whether there will be an inspection 1 or 2 or 3 days later. Thus their behaviors in the days approaching the inspection

[25] Since The traffic volume data is available from 2012 onwards, so the event study period is 2012-2018 in this case.

[26] The interaction term in column 4 is significant at the 10% level. But considering the large sample size in this study, 5% or 1% level is a better benchmark for significant relationships.

should be similar. I vary the length of the time interval during which the inspections are dropped after crashes to see what is the proper time frame, and decided to use 18-hour as the proper time frame since the coefficient on the (-2,-1) bin is consistent with that on (-4,-3) bin. Figure A.7 shows that no other coefficients are affected when varying the length of the time interval except for the coefficients on day (-2, -1) and (0, 1) that drop as the length gets longer.[27].

5.3.3 Sample selection issues

I address several concerns related to sample selections. One concern is about whether the truck is equally likely to be on the road at any given day during the time frame of interest. Since I only see the truck when it receives an inspection on event day 0 but not on any other days, I am assuming that the truck is equally likely to be on the road before and after the inspection. Here is an example that challenges this assumption. If a truck had a serious crash 10 days before inspection, then it probably has to be off the road for repairing for a week after the accident, so it is less likely to be on the road before inspection. If the probability of crash is mean reverting, then I would see an increase of crash after inspection caused by mean reversion.

In order to address this concern, I perform a robustness test that only looks at no-injury or nonfatal crash accidents so trucks who have a serious crash then stop operating for a while are dropped. Table B.2 in the appendix shows that, for this subsample of trucks, there is still a 43.9% increase in crash after inspection. Therefore, the truck selection issue or the mean reversion concern here does not bias the main result.

Another concern related to sample selection is regarding the inspection selection process that determines which truck gets inspected when all enter the weigh station. In order to increase the probability of detection, the design of the regulation indicates that trucks appear to be problematic or have worse histories (inspection, crash, company review records) will receive more inspections. As a result, trucks appear more often in my sample are likely to be those having potentially higher probability of crash. It is not a threat to identification since all analysis in this paper focus on comparing the same truck before and after inspection, not across trucks with different baseline crash rates or other unobserved characteristics. The correct way of interpreting the estimation result is how inspections affect crashes for those trucks inspected, not for any given truck on the highway. Nevertheless, since this study looks at the effect of the current inspection program, the result from this analysis is still the relevant margin to focus on.

[27]In Figure A.7, I normalize the coefficient on day (-14,-13) to be 0 in order to compare across samples

5.3.4 The placebo test

In order to provide additional evidence supporting the identification assumption and eliminate other unobservable confounders for which I cannot test directly, I perform a placebo test that reshuffles the inspections for a randomly selected group representing 0.2% of all trucks ever inspected.[28] The placebo test illustrates that no other confounders could generate such an increase of crash after inspection except for the inspections themselves, which is exactly the identification assumption of the event study.

I randomly selected 20,311 trucks from all trucks inspected from 1996 to 2018. For each truck selected, I reshuffle the inspections it receives during its operating years for 500 times. Then I estimate 500 event studies using the same framework described in equation 7 and equation 8, and compare the estimated effect sizes in the 500 placebo tests with the observed effect size using the real sample. The results are shown in Figure 3 and 4. Figure 3 shows that the observed post-inspection coefficient lies outside of the 95% confidence interval of the distribution of the coefficients from the 500 placebo tests. Figure 4 compares each lead and lag coefficient in the 28-day event window between the observed real sample and the daily average of the 500 placebo tests, which is almost a flat line. So Figure 4 shows that the samples in the placebo tests would not generate an increase of crash right after inspections. Only the observed inspections could lead to such an increase in crash.

6 The mechanism

After eliminating several potential confounders, this section discusses the mechanism behind the finding, which suggests a Peltzman-style explanation for the increase of crash accidents after inspection: knowing that the truck will not be re-inspected in the near term, right after receiving an inspection, the driver conducts risky driving behaviors that offset the potential benefits of the safety inspection program. Section 6.1 describe two pieces of evidence linked to the mechanism. Section 6.2 provide important supporting evidences of changes in drivers' behaviors that contribute to more crashes.

6.1 Peltzman effect

6.1.1 Re-inspection probability

First, I find evidences that a truck is much less likely to be re-inspected for at least a quarter following an initial inspection. Figure 5 shows that the re-inspection probability for a given truck

[28] There are a large number of trucks in the full sample, so I randomly select a smaller but sufficient sample to save the computation time.

is only 1.3% within the first week after receiving an inspection, and the probability is only 8.9% within the first quarter (13-weeks).[29] As mentioned in section 2, when a truck enters the weigh station for inspection, the inspectors will review its inspection and crash history to look for trucks that have a bad history or have not been inspected for a long time. Therefore trucks recently inspected with relatively good records are not going to get another inspection, which is reasonable for the regulators to save efforts on trucks not yet inspected. The re-inspection probability shown in Figure 5 also reflects that trucks recently inspected are indeed inspected with much less probability. Thus, for a truck driver, he or she learns that they are very less likely to get another inspection once they pass the current inspection.

6.1.2 Multi- vs single-vehicle crashes

Correspondingly, I find a larger increase in single-vehicle crashes following an inspection comparing to multi-vehicle crashes. It suggests that the reason for the increase of crash is attributable to the truck drivers. Single-vehicle crashes include all noncollision crashes (18.2%), collision involving parked motor vehicle (2.06%), fixed object (6.8%), and all other crash types involving only the truck itself. Together, they account for 34.9% of all crashes. Multi-vehicle crashes are collisions with other motor vehicles in transport (63.6% of all crashes).

As shown in Figure 6, by estimating the same framework using equation 7 with different crash categories, single-vehicle crashes increased by 74.4% following an inspection, and multi-vehicle crashes increased by 26.8%. Both effects occur immediately on the day of inspection and last for at least 2 weeks after the inspection. There is no pre-trend in both cases. Table 2 column 2 and 3 prints the regression results by estimating equation 8 for single- and multi-vehicle crashes. There are on average 2.23 single-vehicle crashes and 4.06 multi-vehicle crashes per 100,000 trucks inspected in a day. So the baseline probability of a single-vehicle crash is smaller than that of a multi-vehicle crash. In order to compare the size of the inspection effects between the two types of crashes, in Figure 6, I plot the percentage change in each type of crash using the change in the number of crashes with respect to the average number of the corresponding crash accidents. Figure A.8 in the appendix breaks down the single-vehicle crash categories into non-collision ran-off road, non-collision overturn (rollover), non-collision cargo loss or shift, non-collision equipment failure (brake failure, blown tires, etc.), and collision involving fixed objects. The figure shows that, for most single-vehicle crash categories, the increase of crash are all larger than the multi-vehicle crashes.

[29] For all trucks inspected in the sample, I generate a list of inspection history for each one of them. I then calculate the time interval between the current inspection and the closest next inspection in the future for a given truck. If the truck is not re-inspected after the last inspection observed in the sample, I replace the time interval to be longer than 1 year. Then for all trucks in the sample, I calculate the percentage of them that were re-inspected within 1 to 13 weeks to draw Figure 5.

I compare the difference in the factors contributing to the single-vehicle crashes versus the multi-vehicle crashes. The factors contributing to a single-vehicle crash can only be related to the drivers' driving behaviors, or equipment failures of the trucks which are also due to low maintenance efforts of the drivers. While factors contributing to a multi-vehicle crash is either related to the truck side or the side of the other vehicle involved in the accident. Thus, a larger percentage increase in single-vehicle crashes following an inspection indicates that the reason for the increase of crash accidents is attributable to driver's behaviors following an inspection.

Furthermore, I find that the increase in single-vehicle crashes is even higher when the external conditions are worse. It manifests that the increase is caused by drivers paying less attention to external conditions. Crash external conditions include road surface conditions, weather conditions and light conditions. Table B.3 in the appendix compares the regression results estimated using crashes under normal conditions (40%) versus crash under adverse conditions (55.8%). Column 2 shows that crashes under normal conditions increase by 61.2% following an inspection, and column 3 shows that crashes under adverse conditions increase by 84.7% following an inspection. Since when external conditions are bad, drivers need to pay more attention to the traffic than usual in order to prevent an accident. The larger increase in single-vehicle crash accidents under adverse conditions reveals that drivers are driving less carefully after they have had an inspection, which then leads to even more crashes when the external conditions are worse.

To summarize, first, I find evidence that a truck is much less likely to be re-inspected for at least a quarter following an initial inspection. Correspondingly, I find a larger increase in single-vehicle crashes, which is more likely to be related to driver's behavior, comparing to multi-vehicle crashes. Among single-vehicle crashes, I find an even larger an increase for crashes under adverse external conditions, which highlights the drivers' change of behaviors. All evidences indicate that, knowing that the truck will not be re-inspected in the near term, the driver might conduct fewer pre-trip checks at the vehicle, and drive more recklessly on the road for longer hours, which offset the potential benefits of the safety program.

6.2 Factors contributing to crashes from other crash data sources

Using two supplementary crash data sets, I find several supporting evidences to the argument of the Peltzman effect. Section brings in FARS (Fatality Analysis Reporting System) data maintained by NHTSA, and section brings in crash data maintained directly by the Texas Department of Transportation (TxDOT). Both data sets have the advantage of identifying the exact factors contributing to crashes, which allows me to tease out what causes the increase of crash after inspections. In addition, I find very comparable results to the main findings using these two separately maintained data sources, so that it further provides robustness to my main findings.

6.2.1 FARS

FARS data contains information on the exact factors contributing to fatal crashes, as well as the demographics of the drivers involved in the accidents, which are not included in the FMCSA crash files. Therefore, using FARS data from 2000 to 2017, I can analyze the different factors contributing to fatal crash accidents involving CMVs, for example, whether the truck driver was speeding, or the lights of the vehicles were not functioning, or the truck driver was not at fault at all.

First, Figure 7 shows that there is a large increase in the number of fatal crashes involving CMVs after inspections. Since crashes resulting in fatalities are rare events, there are in total 472 fatal crashes identified within the time frame of interest around inspections[30]. I present the analytic data using bar charts instead of performing event study regressions. Figure 7 shows that there are on average 15 fatal crashes for each two-day bin before inspections. The number increases to 52 after inspections.

Second, when analyzing the factors contributing to the fatal crashes, I find that, post inspections, there are increases in crashes for which the truck analyzing is having violation convictions. Generally speaking, the contributing factors to crashes could be related to either the truck or the other vehicle/pedestrian in the accident, or sometimes, both. On the truck side, it could be related to the driver, or any equipment failures, or other factors including the passengers in the vehicle. Figure 8 shows that there are increases in crashes due to drivers' reckless driving behaviors, including speeding and driving while intoxicated, and due to a lack of vehicle maintenance. Furthermore, Figure A.9 in the appendix shows suggestive evidence that there is an increase in the travel speed prior to the accidents for crashes happen after inspections, from 30.5 mph before inspections to 54 mph after inspections[31].

Third, Table B.4 in the appendix compares driver demographics for those involved in fatal crashes 14 days before and after an inspection. The table shows that the average age, sex, and previous moving violation convictions for the truck drivers are similar before and after, only the percentage of drivers that do not have a valid CDL (Commercial Driver's License) is slightly higher for crashes happen after inspection. It suggests that the type of drivers operating the trucks are quite comparable before and after inspections.

[30] There are 472 fatal crashes identified in FARS data within (-14,13) days of an inspection for the same truck. Since FARS data only report 12-digit VIN, the matching of trucks in FARS and the inspection file is done by using the characteristics of fatal crashes in the FMCSA crash file.

[31] Only 52% of crashes have records on the travel speed prior to the occurrence of the crashes. The shaded area of Figure A.9 shows 25 to 75 percentile of the distribution of travel speed within each 2-day bin. The dots are the median of the travel speed. There are even fewer fatal crashes happening before inspections that have travel speed recorded, so the confidence interval is wider.

6.2.2 Texas DOT crash data

In this section, I provide additional supporting evidences using data containing all types of crashes obtained from the Texas Department of Transportation (TxDOT). TxDOT maintains a set of crash files under the Crash Records Information System collected from the Texas Peace Officer's crash reports from 2010, to present. The benefits of using this set of files for supplementary analysis are three-fold: first, CRIS includes non-injury, injury and fatal crashes; second, there are detailed description of the factors contributed to each crash accident; third, the crash records contain the VIN. In order to match the federal recordable[32] crash accidents in FMCSA, which is used in the main finding, I use the VIN and date of crash to find crash records in CRIS that matches up with those in FMCSA.

Figure 9 shows that there is a 39% increase in truck crashes after inspections relative to the baseline crash rate, which is comparable to the average effect size using the data from the whole US. Here I use the exact same framework in equation 7 for all inspections and crashes happening in Texas. There are 1,487,842 inspections used in this analysis. Note that the effect size is attenuated due to the reason that I can only observe crashes happened within Texas for trucks inspected in Texas, therefore, crashes happened outside of Texas for the trucks inspected within Texas are not included in the analysis.

Figure 10 shows that there is an increase in crashes due to driver-related factors after inspections. Among those crashes, those having a speeding violation also exhibit a significant increase. Figure 11 serves as a falsification test which shows that there is no increase [1.63e-06 (P = 0.063)] in the number of crashes that are *not* due to truck related factors.

In summary, the FARS data and the TxDOT data provide additional evidences of drivers' offsetting behaviors to the safety regulation. First, from FARS, the driver demographics do not change for crashes happen before and after the inspections. It validates the identification assumption that drivers who operate the truck do not switch, so controlling for individual truck fixed effect is sufficient. Second, from both data sets, there is an increase in the number of crashes due to inspections, which supports the main finding of this paper. Since FARS data analyzes fatal crashes, the finding also makes the current study even more imperative. Third, from analyzing the factors contributing to crashes in both data sets, I find that, after inspections, drivers indeed drive more recklessly (speeding, DWI) and perform fewer vehicle maintenance checks. So the mechanism presented in the previous section is validated with facts in this section.

[32] According to the crash file documentation by FMSCA, a federal "recordable" crash has occurred when at least one person dies, or at least one person experiences bodily injury which requires immediate medical treatment away from the scene of the crash, or a vehicle is towed away.

7 Heterogeneity in effects of inspections on crashes

The findings reported above in section 5 show that, on average, for a truck, there is a 43.5% increase of crash accidents after an inspection. I then illustrate in section 6 that the increase of crash is due to drivers' compensating behaviors after inspections since they do not expect to get re-inspected for a long time after an initial inspection. In this section, I discuss how the effects vary when trucks receive different inspection outcomes, or have different firm characteristics, or get inspected in different states which implement heterogeneous inspection strategies.

7.1 Effects on crash by inspection outcomes

In order to see how the effects vary when trucks receive different inspection outcomes, I separate the full sample into subgroups of inspections with heterogeneous violations. I first split the inspection sample into two groups: those that do not find any violation (38%), and those that result in violations but not out-of-service violations (41%). I do not include trucks that receive OOS violations (20%) in either group because those trucks have to be either repaired at scene or parked immediately, they cannot have any crashes after inspections in principle.[33] In Table 3, column 1 and 2 show that the two groups exhibit similar increase in crashes after inspections. The effect of an inspection on crashes for trucks that do not receive any violations is 42.6%, while the effect for trucks that receive some violations is 46.5%. The average crash rate for the two groups are also similar. The result indicates that truck drivers are not responding differently after inspections depending on whether or not they receive violations, which result in similar increases in crash accidents. In other words, as long as the violations do not put the truck out-of-service, the driver would continue to drive the truck. However, the driver would drive less carefully since the truck was just inspected and would not be inspected again in a very long time.

Next, for inspections that result in violations, I further split the sample into trucks that receive driver violations (40%) and trucks that receive vehicle violations (75%)[34]. Examples of driver violations are false log book, noncompliance with hours-of-service regulation, consumption of alcohol or controlled substances, and speeding. Examples of vehicle violations are brake or tires violations. Column 3 and 4 of Table 3 compare the impact of an inspection on crashes for those two violation categories. The effect size for trucks that receive driver violations is 52.9%, while the effect for trucks that receive vehicle violations is 42.8%. The average crash rate for trucks that receive driver violations is also larger than those receive vehicle violations. The result indicates that inspections are useful in detecting worse behaving drivers and giving them citations, however, inspections fail

[33]It is the regulation that trucks receive out-of-service violations have to stop service, however, in my data, I still observe a large number of crashes for those trucks after inspections. So they probably disregard the OOS citation and continue to operate, which is a serious crime.

[34]Some inspections issue both driver and vehicle violations.

to deter future reckless driving behaviors and lead to a lower re-inspection probability for trucks already inspected, which contributes to more crashes.

7.2 Effects on crash by firm characteristics

Depending on the management practices adopted by different firms, truck drivers are likely to be facing different constraints and motivations when driving on the road. In order to examine how the effect of an inspection varies by firm characteristics, I perform a heterogeneity analysis by estimating equation 8 using different firms. The carrier/shipper census file contains information on the type of cargo transports, number of vehicles owned and drivers employed for 1.6 million active firms as of October 2018. I look at the firm characteristics in the following four facets.

7.2.1 Firm size

First, I look at how trucks belong to large versus small firms respond differently to inspections. The firm characteristics in terms of the driver and vehicle inventory are shown in Table 1 panel D. There is a lot of heterogeneity in firm size in the trucking industry. The largest carrier firm owns more than 500,000 power units and more than 100,000 drivers. But 50% of carriers only have 1 power unit and 1 driver. I define a large firm as one having more than the median number of power units (or drivers) among all inspected firms, and small firm are the rest of the sample. So I can split the full sample into two equally sized sub-samples. A large firm has more than 48 power units (or more than 47 drivers)[35]. Panel A in Table 4 compares the effect of an inspection on crashes for trucks belong to firms with large or small number of power units. The result shows that the increase in crash from smaller firms is slightly smaller: the effect size for large firms is 43.68%, for small firms is 38.68%. The average crash rate for the two types of firms are almost the same. Panel B in Table 4 shows that defining firm sizes using the number of drivers instead of power units gives a similar result. In addition, I look at the 1-truck-1-driver firms in column 3 of Panel A. It shows that drivers in this type of firms respond in the same way. The reason to highlight this group is because, throughout the paper, I cannot identify the driver-vehicle pair as I have no information on drivers' identities. So I cannot see if drivers switch after inspections. If drivers do switch, it would not be the same driver before and after the inspection which creates problem for identification. Since 1-truck-1-driver firms cannot switch drivers, and I find that the effect size of this group is similar to that of the full sample. The concern could be eliminated.

Overall, the impact of an inspection on crash across different firms sizes are quite comparable. However, it does not suggest that large and small firms are responding same to the safety regulation.

[35]This number is larger than the number of power units for a median firm in the carrier census file is because large firms with more power units are more likely to receive inspections.

There are as many reasons why large firms exhibit compensating behaviors as small firms. For 1-truck-1-driver carriers, the drivers' earning are tied with how much work they choose to finish. So they have more incentive to drive longer once their perceived inspection probability is lower. Since they are often self-operated, their behaviors are entirely unmonitored. Moreover, small companies have less resources to spend on driver training, vehicle maintenance, and efficiency enhancing technology adoption in general. On the other hand, a large carrier, like Fedex, would potentially have the principal-agent problem (Baker and Hubbard (2004)) that suggests the drivers would not drive in the best manner to preserve the truck's value. Thus, drivers in large versus small firms are living in quite different worlds with different profit structure as well as liabilities, which then affect their risk preferences of driving. In this paper, since my ability to tease out the different constraints firms are facing is limited, I do not find the effect size from the two types of firms to be quite different, but that does not suggest that they are the same when facing inspections.

7.2.2 Firm business – inter- vs. intra-state

Second, I look at how carrier firms respond differently to inspections depending on their major business types, that is, whether they hire interstate or intrastate drivers. Drivers on interstate versus intrastate routes are of particular interest in this paper because, depending on the route, they face different time constraints. More specifically, if an interstate driver encounters an inspection which could take from 20 minutes to 1 hour plus the time waiting in the queue of entering the weigh station, although it delays the schedule on the day of inspection, the driver could still manage to make up for the time loss on the following days before the delivery time. On the other hand, long-haul drivers could respond by driving for long hours. They could then violate the hour-of-service regulation that mandates the 60/70-hour limit which says drivers may not drive after 60/70 hours on duty in 7/8 consecutive days. If an intrastate driver who is on a tight delivery schedule within the day since it is shorter distance, the delay caused by the inspection could possibly result in the driver speeding in order to be on time for delivery. Panel C in Table 4 compares firms that hire drivers only for interstate routes, only for intrastate routes, and for both. The result shows that the effect for interstate only firms is 41.96%, for intrastate only firms is 38.02%, and for firms do both business is 38.13%. The average crash rate for intrastate only firms is slightly smaller probably because they are familiar with local routes they operate.

7.2.3 Firm type – truck or bus

Third, I look at how carrier firms respond differently to inspections depending on whether their main inventory is trucks or buses. Both trucks and buses are commercial motor vehicles regulated under the same safety inspection program. However, since trucks transport cargo while buses transport human, the protocol that binds each type of carriers is different. Examples include the hours-of-service regulation for bus drivers is stricter than that for truck drivers, and most of the bus drivers do not run long-distance route across the U.S., but long-distance route can be very common for inter-state truck drivers. There are in total 1.4 million carrier firms that only have trucks, and 41,000 that only have buses. Panel D in Table 4 compares between firms that only operate trucks and firms that only operate buses. The result shows that the effect of an inspection on crashes for truck-only firms is 40.67%, while for bus-only firms is 41.90%. Both type of firms respond similarly to inspections except that bus-only firms have a slightly smaller average crash rate.

7.2.4 Cargo carried

Fourth, I look at how carrier firms respond differently to inspections depending on the type of cargo they transport. I summarize the 30 types of cargo into general freight, chemicals, food and beverage, paper product, building materials, metal sheet, heavy duty commodities, and passengers and livestock. Such categorization makes sure that there are enough observations within each category. The result is shown in Table B.5. In general, trucks transport different types of cargo exhibit very comparable effects of inspection on crashes.

To summarize, all the findings reported above show that there is very *little* heterogeneity in the effect sizes in terms of different firm characteristics, including the firm size, interstate or intrastate commerce, and transported cargo. This result indicates that driver's compensating behaviors exist for all types of drivers after they receive an inspection. Although drivers could potentially face different constraints and incentives, they all face a small re-inspection probability following the current inspection, so they tend to be less careful afterwards which resulted in an increase of accidents.

7.3 Spatial distribution of effect sizes

There is a large variation in the effect of an inspection on crashes across different geographical areas of the U.S. The spatial heterogeneity in effect sizes reveals differences in the regulatory designs across the states. Although the inspection program is conducted across the whole nation, each state DOT chooses its own way of handling the enforcement strategy. The differences lie in the intensity and the type of inspections, the trucks selected, the inspection schedules, and the location of inspections. For example, Figure A.3 shows that Wisconsin, Illinois, and Missouri focus more on the state borders, while New York and Pennsylvania have inspections more spread out. The inspection strategy also varies across time. Figure A.10 shows that California implemented many roadside inspections during 2009 to 2015, but more fixed station inspections in other years.

While Pennsylvania used to have small number of fixed station inspections before 2015, but made a significant shift in the type of inspections conducted with very little change in the total number of inspections in 2015. In section 8, I illustrate in detail how differences in inspection schedule and truck selection across states affect the effect sizes.

Moreover, the vehicle miles traveled (VMT) by truck in each state varies a lot. For example, the VMT by truck in 2017 in Texas is around 27,000 million miles; while it is 1,200 million miles in Montana. So 1 more crash due to drivers' behavioral responses to inspection shows up as a greater concern in Montana where the truck traffic is much lighter than that in Texas.

Therefore, in order to compare the effect of an inspection on crash across space, I calculate the VMT adjusted effect size for each commuting zone, then draw a map in Figure 12. There are in total 709 commuting zones that delineate the local economies for the whole nation. The VMT adjusted effect sizes are computed in the following way. I first estimate the effect size of an inspection on crash using subsamples of inspections conducted in each commuting zone. Next, I weigh the effect size in each commuting zone by the standardized annual VMT by trucks in the corresponding state. I standardize the annual VMT of states to be mean one and standard deviation one. So the mean and standard deviation of the weighted effect sizes are still the same as the unweighted ones.

The spatial heterogeneity analysis presented in Figure 12 shows a large variation of the effects on crash from different inspection policy designs across the states. Most areas in the map exhibit positive effect sizes, indicating that there is an increase of crash after inspections. In the next setion, I explore several factors in the regulatory design that may have resulted in this spatial difference in effect sizes.

8 Policy recommendation

The econometric evidence described in the previous section shows that drivers' compensating behaviors to the safety inspection regulation result in an increase of crash after inspections. The effect size of the increase in crash is 43.5% relative to the baseline crash rate in 14 days after an inspection, and it is statistically significant and robust.

In this section, I explore whether the impact of inspection is economically significant by quantifying the magnitude of the cost of crash due to behavioral responses to the regulation. In order to estimate the total increase in crash following an inspection, I look at the overall effect of inspection on crashes in the longer term, which is 24 months after inspections. Next, I explore several alternative inspection policy options that could reduce the increase of crash or even achieve crash reduction. Specifically, I look at the choices made by the inspectors in conducting inspections across different states. I show that if the inspections are designed to be less predictable for the truck drivers, the inspection program could achieve better results in crash prevention.

8.1 Quantify the overall effect on crash

In this section, I implement a monthly event study that enables me to estimate the impact of an inspection on truck crashes in the longer term.

In the monthly event study, I use a similar framework as in equation 7 and 8 while changing the unit of time from 1 day to 1 month. The event window is from 12 months before to 24 months after an inspection (or inspections) in month 0 for any given truck. Again, I control for individual truck fixed effects, year and month fixed effects. The specific estimation equation is the following:

$$Crash_{it} = \sum_{\tau=-12,\tau\neq-1}^{24} \beta_{\tau} Insp_{it}^{\tau} + u_i + \eta_t + \varepsilon_{it}, \tag{9}$$

In order to compare crash rate before and after an inspection using the estimation equation above, an implicit assumption is that the truck has to be operating during the 36-month time window. Otherwise, I might observe a reduction in crash a few months after inspection which was just caused by truck exiting the market. In this event study, I use only the inspections that happen in the middle of any given truck's life. Specifically, only inspections that occurred 12 months after the first inspection and 24 months before the last inspection for any given truck are included in the estimation sample. So the estimation result will not be affected by trucks enter or exit the market.

The result of the monthly event study is shown in the top panel of Figure 13. There is a significant increase of crash during the first two months after an inspection/inspections in event month 0. After month 2, the increase of crash gradually declines. In the longer term, the increase of crash can be detected up to the 12^{th} month after the inspection. The crash rate goes back to the baseline level after 12 months and remains at the baseline until the 24^{th} months, which is the end of the event window. In the lower panel of Figure 13, I plot the probability of re-inspection for any given truck from month 1 to 24 after the inspection. The probability of re-inspection is only 4.0% in the first month after inspection, and it grows over time. The probability of re-inspection within 24 months after inspection is 25.1%. The correspondence between the top and lower panel of Figure 13 shows that as the re-inspection probability increases over time, which means that drivers face higher probability of inspection, the increase of crash due to driver's compensating behaviors to the inspection regulation decreases.

The total effect of one inspection on crash is calculated by adding up the first 12 lag coefficients estimated from equation 9. Since the crash rate goes back to the baseline after the 12^{th} month, variation in crash after month 12 are mostly noise that should not be counted. The total effect is 0.00103 additional crashes. There are on average 1.75 million inspections conducted at the weigh stations every year. Therefore, the total number of crashes caused by the behavioral responses of the inspection program is roughly 1803 (=0.00103*1.75 million). According to the National

Safety Council's cost calculation of motor vehicle injuries in 2018, the weighted average cost of a motor vehicle accident was around \$0.9 million. Hence, the total loss due to drivers' compensating responses to the current inspection regulatory design is roughly \$1.6 billion per year.

In order to prevent the huge loss resulting from the imperfect design of the inspection program, I provide two alternative policy options both aiming to increase drivers' expected re-inspection probability from increasing the randomness in inspections.

8.2 Predictability in inspection schedule

In the first alternative inspection policy, I explore how the predictability in inspection time schedule affects drivers' behavioral responses. On this margin, I find that states with most unpredictable inspection schedules could achieve crash reduction after inspections; while states with very predictable schedule have large increases in crash after inspection.

More specifically, I look at the variation in the day-of-week inspection schedule across states. In the example given in Figure 14, I compare the number of inspections done in each day of the four weeks in county A and B. The average number of inspections per day is the same (mean = 58) for both counties. County A has a fixed day-of-week inspection schedule, meaning that county A is conducting 10 inspections every Sunday, 60 every Monday, 80 every Tuesday, etc.; while county B has a random day-of-week inspection schedule so that the number of inspection done on Sunday this week is different from the next Sunday, this Monday is different from the next Monday, etc. Therefore, in county A, it would be very easy for truck drivers who travel the same route frequently to predict whether they will receive an inspection on a given day of the week. But it would be hard for them to predict in county B. As a result, drivers would always be cautious when driving in county B since they are uncertain of the inspection intensity on a particular day, but they could driver more recklessly or maintain their vehicles less carefully in county A if they know that they are very less likely to get an inspection today, or even for the rest of the week.

In order to measure the predictability of the day-of-week inspection schedule, I develop a predictability index for each state in the following way. I first demean the number of inspections conducted ($Insp_{ct}$) in a county c at a given day t using an interactive fixed effects estimator that controls for variations within in county$\times$day-of-week, county$\times$year, day-of-week$\times$year, and county$\times$day-of-week$\times$year as shown in the following equation.

$$Insp_{ct} = \alpha_c + \gamma_{dow} + \eta_{year} + \theta_{c\times dow} + \mu_{c\times yr} + \lambda_{dow\times yr} + \omega_{c\times dow\times yr} + \varepsilon_{ct}, \tag{10}$$

where α_c, γ_{dow}, η_{year} are the county, day-of-week, and year fixed effects, $\theta_{c\times dow}, \mu_{c\times yr}, \lambda_{dow\times yr}$ are the corresponding three-way fixed effects, and $\omega_{c\times dow\times yr}$ is the fully interactive fixed effect. I then define the predictability index as the variance of the residual, ε_{ct}, at the state×year level. So

$$pred_{s,yr} = var(\varepsilon_{ct}) \tag{11}$$

In the 2-county example in Figure 14, county A and B both have 58 inspections per week in total. Using the predictability index defined above, county A has a predictability index of 0, county B has a predictability index of 32. The lower *pred_index*, the easier the drivers could forecast an inspection. The state×year predictability index is the average for all county indices in the given state. Figure 15 shows the yearly average of the state×year predictability index across all states.

I estimate the effect of inspection on crash depending on the differences in inspection schedule predictability in the following way.

$$\begin{aligned} Crash_{it} =& \beta\, post\_insp_{it} + \gamma post\_insp_{it} \times std\_pred_{s,yr} + \theta std\_pred_{s,yr} \\ &+ \beta_{-15} Insp_{it}^{-15} + \beta_{14} Insp_{it}^{14} + u_i + \eta_t + \varepsilon_{it} \end{aligned} \tag{12}$$

where $std\_pred_{s,yr}$ is the standardized predictability index for state s in year yr. $std\_pred_{s,yr}$ is mean 0, standard deviation 1. Note that equation 12 is a variation of equation 8 by adding the interaction term $post\_insp_{it} \times std\_pred_{s,yr}$ and the main effect of $std\_pred_{s,yr}$. Thus the coefficient of interest that estimates the effect of inspection by different inspection schedule is γ. The estimation result of equation 12 is in Panel A of Table 6.

Using the coefficient estimates from Table 6, Panel A of Table 7 shows that for states with highly predictable inspection schedule, or lower predictability index, the effect of inspection on crash is 51%; for states with highly **un**predictable inspection schedule, or higher predictability index, the effect of inspection on crash is -7%. One standard deviation increase in the predictability index *drops* the effect size by 9%. In other words, states who have adopted highly unpredictable inspection schedules do achieve crash reduction after the inspections. Note that estimated effect sizes in Table 7 are based on the current sample, hence one could extrapolate from the estimates that greater crash reduction could be achieved if states have adopted inspection schedules with even more unpredictability than the states now.

8.3 Randomness in inspection selection

Another policy variation I explore is the extent to which the inspectors rely on past inspection records to determine which trucks are chosen for inspections, in other words, whether trucks recently inspected are still getting inspections as if they were selected randomly. In section 6.1, I highlight the fact that, on average, the probability of re-inspection is very low for trucks recently inspected in the current situation (see Figure 5), therefore, drivers not expecting to receive another inspection in the near future tend to drive more recklessly on the road, which creates more crash accidents. Therefore, I compare states with longer versus shorter time interval to previous inspection see if this variation in inspection selection changes the impact of the inspection program.

More specifically, in order to find out the variation in inspection selection based on past inspection records across different states, I calculate the average number of days passed since last inspection (in any state) for any given truck inspected in state s in year yr. I call that number the re-inspection time interval, $Dtime_{s,yr}$. Figure 16 shows that states vary a lot in this aspect: Texas has an average re-inspection interval of 3-months, but Michigan has an average re-inspection interval longer than 1 year. Therefore, trucks recently inspected now travelling in Michigan today can almost be assured that they are not going to be re-inspected this time, but trucks travelling in Texas still face a large chance of re-inspection even if they were just inspected. As a result, the increase in crash due to drivers' offsetting behaviors after inspections could be less severe in Texas than in Michigan.

I estimate the effect of inspection on crash by different re-inspection time interval using equation 13,

$$\begin{aligned} Crash_{it} = &\beta post\_insp_{it} + \gamma post\_insp_{it} \times std\_Dtime_{s,yr} + \theta std\_Dtime_{s,yr} \\ &+ \beta_{-15} Insp_{it}^{-15} + \beta_{14} Insp_{it}^{14} + u_i + \eta_t + \varepsilon_{it} \end{aligned} \tag{13}$$

where $std\_Dtime_{s,yr}$ is the re-inspection time interval calculated for state s in year yr, standardized to mean 0 and standard deviation 1. In this equation, the interaction term $post\_insp_{it} \times std\_Dtime_{s,yr}$ estimates the effect of inspection depending on the length of the re-inspection time interval. The estimated result is in Panel B of Table 6.

Panel B in Table 7 shows that for states with very short re-inspection time intervals which are less than 1 month, the average effect size is 25%; and for states whose re-inspection time intervals are longer than 1 year, the average effect size is 58%. One standard deviation increase in the interval (5-month) *rises* the effect size by 10%. Again, all above estimations of effect sizes are based on the current sample, hence one could extrapolate from the estimates that greater crash reduction could happen if states could have chosen to inspect trucks at greater randomness.

9 Conclusion

This paper evaluates the effectiveness of the CMV roadside safety inspection program using the most comprehensive data files on trucks, inspections, and crashes ever complied from 1996 to 2018. Linking inspection and crash history using each truck's unique VIN, the data allows me to implement an event study research design that tracks a given truck's crash rate shortly before and after it receives an inspection.

I find that there is a sharp, 43.5% *increase* in crash rate immediately following an inspection, and the effect lasts for at least 14 days. In the longer term, the increase in crash persists for 12 months after inspections, so the inspection program accounts for 1803 additional crashes in a year. On the effect size, there is little heterogeneity across different inspection outcomes, such as having violation convictions or no violations at all. There is also little heterogeneity in effect sizes between large and small carriers/shippers, or interstate and intrastate firms. But I do find a strong spatial heterogeneity in effect sizes across different commuting zones due to regional differences in the inspection program designs.

The increase in crash rates after inspections is attributable to the truck drivers' risk compensating behaviors to safety regulations. I find evidences that a truck is much less likely to be re-inspected for at least a quarter following an initial inspection. Correspondingly, I find a larger increase in single-vehicle crashes, which is related to driver's behaviors, comparing to multi-vehicle crashes. Moreover, the number of crashes due to speeding or reckless driving increases after inspections. So knowing that the truck will not be re-inspected in the near term, the driver might conduct fewer pre-trip checks at the truck, and drive more recklessly on the road, which offset the potential benefits of the safety program.

This paper makes several suggestions to the implementation of the inspection program according to the findings. First, states should adopt a more random inspection schedule so that drivers cannot anticipate on which day they will get an inspection. Second, states should assign same inspection probability to trucks recently inspected with trucks that have not been inspected in a while. Third, since perfect monitoring is impossible from enforcement actions on the regulators' side, self-monitoring through the carrier companies needs to be promoted at the same time. The on-board computer system (Baker and Hubbard (2004)) is widely adopted by carrier companies to communicate with the truck drivers. It can also be used to monitor the truck's driving behaviors, such as the speed, driving hours, etc., which can be used as a self-monitoring mechanism to enforce better driving behaviors both before and after inspections. I would be interested in conducting a field experiment, like what Gosnell et al. (2020) did for commercial airline captains, to explore the effectiveness of several management practices on truck drivers' performances.

Overall, this paper provides a compelling evidence for drivers' compensating behaviors under predictable enforcements by evaluating the effectiveness of the CMV safety inspection regulation in improving road safety. However, this paper is not concluding on the notion that the inspection program should be abandoned as it leads to more crashes. After all, it is worth mentioning that there are 20% of CMVs put out-of-service after the inspections. Without the inspection program, those 20% CMVs, potentially more dangerous, would remain on the road and lead to more serious safety problems. Therefore, the findings of the paper show that the current enforcement mechanism of

CMV inspections is sub-optimal, and needs to be improved. The paper hopes to call upon attentions from the public on this nationwide safety program.

References

Adda, Jerome and Francesca Cornaglia, "Taxes, cigarette consumption, and smoking intensity," *American Economic Review*, 2006, *96* (4), 1013–1028.

Anderson, Michael L and Maximilian Auffhammer, "Pounds that kill: The external costs of vehicle weight," *Review of Economic Studies*, 2014, *81* (2), 535–571.

Ashenfelter, Orley and Michael Greenstone, "Using mandated speed limits to measure the value of a statistical life," *Journal of political Economy*, 2004, *112* (S1), S226–S267.

Baker, George P and Thomas N Hubbard, "Contractibility and asset ownership: On-board computers and governance in US trucking," *The Quarterly Journal of Economics*, 2004, *119* (4), 1443–1479.

Banerjee, Abhijit, Esther Duflo, Daniel Keniston, and Nina Singh, "The efficient deployment of police resources: theory and new evidence from a randomized drunk driving crackdown in India," Technical Report, National Bureau of Economic Research 2019.

Benedettini, Simona and Antonio Nicita, "The costs of avoiding accidents: Selective compliance and the 'Peltzman effect'in Italy," *International Review of Law and Economics*, 2012, *32* (2), 256–270.

Benthem, Arthur Van, "What is the optimal speed limit on freeways?," *Journal of Public Economics*, 2015, *124*, 44–62.

Blomquist, Glenn, "A utility maximization model of driver traffic safety behavior," *Accident Analysis & Prevention*, 1986, *18* (5), 371–375.

_ , *The Regulation of Motor Vehicle and Traffic Safety* 01 1988.

Blundell, Wesley, Gautam Gowrisankaran, and Ashley Langer, "Escalation of Scrutiny: The Gains from Dynamic Enforcement of Environmental Regulations," Technical Report, National Bureau of Economic Research 2018.

Cohen, Alma and Liran Einav, "The effects of mandatory seat belt laws on driving behavior and traffic fatalities," *Review of Economics and Statistics*, 2003, *85* (4), 828–843.

Duflo, Esther, Michael Greenstone, Rohini Pande, and Nicholas Ryan, "The value of regulatory discretion: Estimates from environmental inspections in India," *Econometrica*, 2018, *86* (6), 2123–2160.

Edlin, Aaron S and Pinar Karaca-Mandic, "The accident externality from driving," *Journal of Political Economy*, 2006, *114* (5), 931–955.

Evans, William N and John D Graham, “Risk reduction or risk compensation? The case of mandatory safety-belt use laws,” *Journal of Risk and Uncertainty*, 1991, *4* (1), 61–73.

Fletcher, Jason M, David E Frisvold, and Nathan Tefft, “The effects of soft drink taxes on child and adolescent consumption and weight outcomes,” *Journal of Public Economics*, 2010, *94* (11-12), 967–974.

GAO, US, “LARGE TRUCK SAFETY: Federal Enforcement Efforts Have Been Stronger Since 2000, but Oversight of State Grants Needs Improvement,” *GAO-06-156*, 2005.

Gosnell, Greer K, John A List, and Robert D Metcalfe, “The impact of management practices on employee productivity: A field experiment with airline captains,” *Journal of Political Economy*, 2020, *128* (4), 000–000.

Graham, Jove, Jennifer Irving, Xiaoqin Tang, Stephen Sellers, Joshua Crisp, Daniel Horwitz, Lucija Muehlenbachs, Alan Krupnick, and David Carey, “Increased traffic accident rates associated with shale gas drilling in Pennsylvania,” *Accident Analysis & Prevention*, 2015, *74*, 203–209.

Gray, Wayne B and Jay P Shimshack, “The effectiveness of environmental monitoring and enforcement: A review of the empirical evidence,” *Review of Environmental Economics and Policy*, 2011, *5* (1), 3–24.

Keeler, Theodore E, “Highway safety, economic behavior, and driving environment,” *The American Economic Review*, 1994, *84* (3), 684–693.

Kwigizile, Valerian, Andrew H Ceifetz, Jun-Seok Oh, Joyce L Yassin, and Jason Firman, “Benefit-Cost Analysis of Fixed Weigh Stations: The Michigan Case,” 2016, pp. 16–4434.

Levitt, Steven D and Jack Porter, “How dangerous are drinking drivers?,” *Journal of political Economy*, 2001, *109* (6), 1198–1237.

Li, Shanjun, “Traffic safety and vehicle choice: quantifying the effects of the ‘arms race’on American roads,” *Journal of Applied Econometrics*, 2012, *27* (1), 34–62.

Loeb, Peter D and Benjamin Gilad, “The efficacy and cost-effectiveness of vehicle inspection: a state specific analysis using time series data,” *Journal of Transport Economics and Policy*, 1984, pp. 145–164.

Lv, Jinpeng, Dominique Lord, Yunlong Zhang, and Zhi Chen, “Investigating Peltzman effects in adopting mandatory seat belt laws in the US: Evidence from non-occupant fatalities,” *Transport Policy*, 2015, *44*, 58–64.

Mookherjee, Dilip and Ivan Png, “Optimal auditing, insurance, and redistribution,” *The Quar-*

terly Journal of Economics, 1989, *104* (2), 399–415.

Muehlenbachs, Lucija, Stefan Staubli, and Mark A Cohen, "The impact of team inspections on enforcement and deterrence," *Journal of the Association of Environmental and Resource Economists*, 2016, *3* (1), 159–204.

_ , _ , and Ziyan Chu, "The accident externality from trucking," 2017.

Okat, Deniz, "Deterring fraud by looking away," *The RAND Journal of Economics*, 2016, *47* (3), 734–747.

Oliva, Paulina, "Environmental regulations and corruption: Automobile emissions in Mexico City," *Journal of Political Economy*, 2015, *123* (3), 686–724.

Peltzman, Sam, "The effects of automobile safety regulation," *Journal of political Economy*, 1975, *83* (4), 677–725.

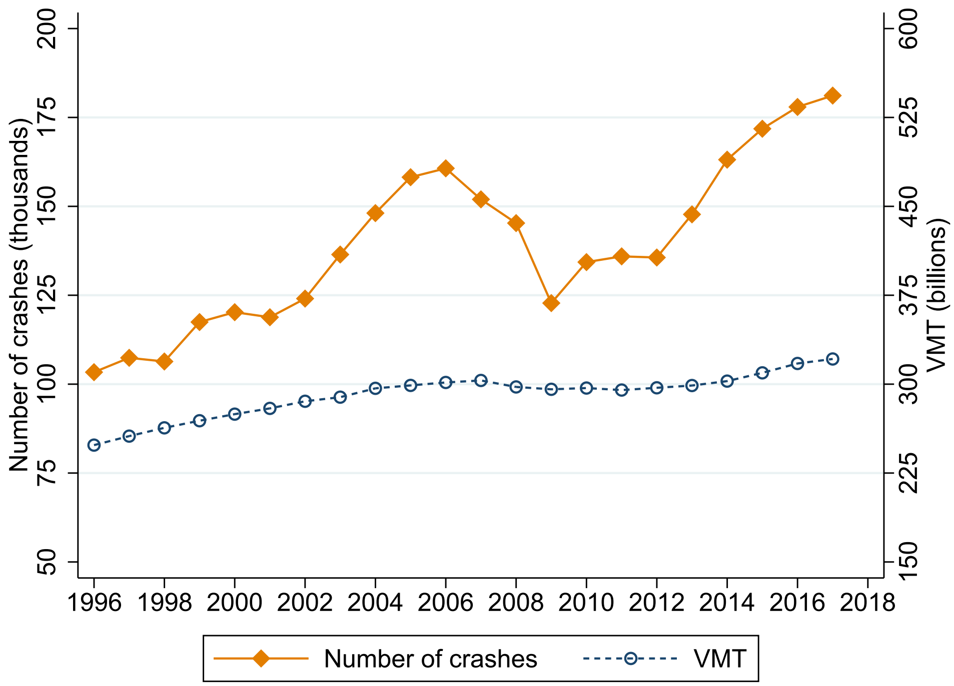
Shimshack, Jay P, "The economics of environmental monitoring and enforcement: A review," *Annual Review of Resource Economics*, 2014, *6*, 339–60.

Viscusi, W Kip, "The lulling effect: the impact of child-resistant packaging on aspirin and analgesic ingestions," *The American Economic Review*, 1984, *74* (2), 324–327.

_ , "The impact of occupational safety and health regulation, 1973-1983," *The RAND Journal of Economics*, 1986, pp. 567–580.

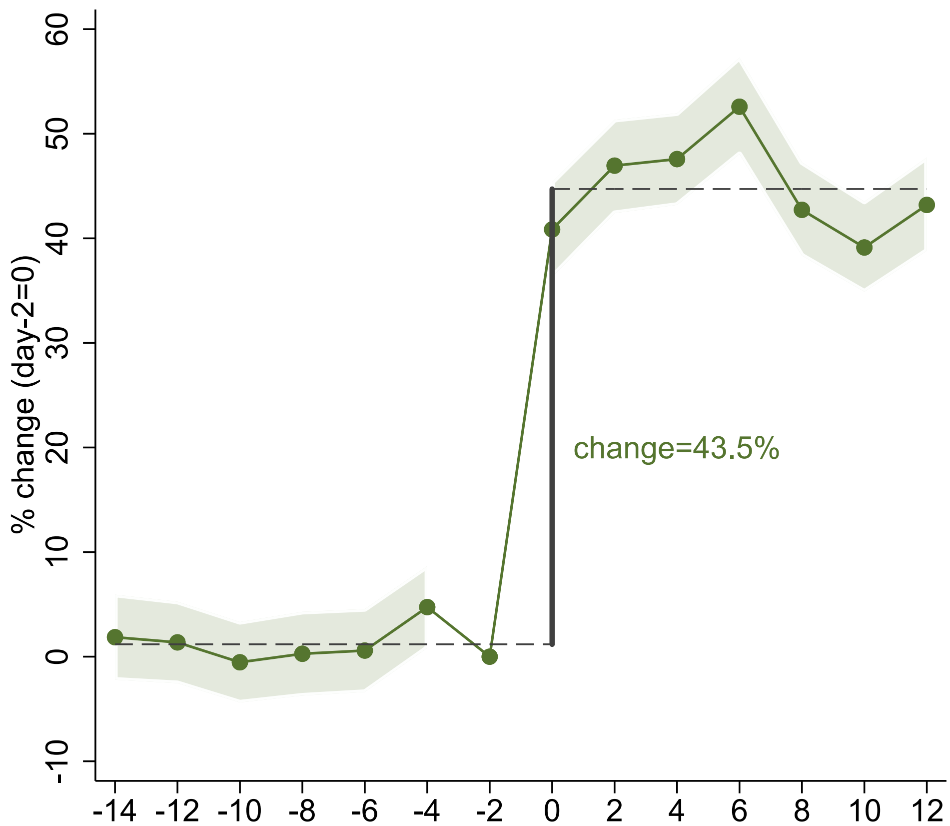
Tables & Figures

Figure 1: The number of CMV crashes vs. VMT over time



Note: The left y-axis is the number of truck crashes in thousands. The number of crashes for the whole US are aggregated using the FMCSA crash data for all commercial motor vehicles, which is the same data used in the analysis. The right y-axis is the annual vehicle miles traveled (VMT) by CMVs, which is obtained from the Bureau of Transportation Statistics.

Figure 2: Event study: the impact of an inspection on truck crashes



Note: The coefficients plotted in this figure are estimated using equation 7 where I regress the number of crash accidents for the same truck that receives the inspection on a set of inspection indicators from 14 days before an inspection to 14 days after inspection, controlling for individual truck fixed effects, year, month, and day-of-week fixed effects. The shaded area is the 95% confidence interval for the estimates. The standard errors are clustered at the truck level. I combine the daily inspection indicators into 2-day bins to increase the power of estimation, such as (-14,-13), (-12,-11), ..., (-2,-1), (0,1), (2,3), ..., (12,13) relative to the day of inspection at day 0. The effect of an inspection on crash accidents happening in the two-day bin (-2,-1) is normalized to 0.

This figure shows that the number of crash accidents involving trucks increases immediately following an inspection, and the increase lasts for at least 2 weeks after the inspection within the event window studied. I illustrate the level shift in crash accidents before and after the inspection in Figure 2 by fitting two horizontal lines using the average of percentage changes respectively. The level difference between the two dashed lines represents the increase in the crash accidents. The increase in crash accidents is 43.5% relative to the daily average crash rate (6.4 crashes per 100,000 trucks). The figure also shows that there is almost no pre-trend during the 14 days before the inspection which proves that the identification assumption is valid.

Figure 3: Placebo test: the effect sizes

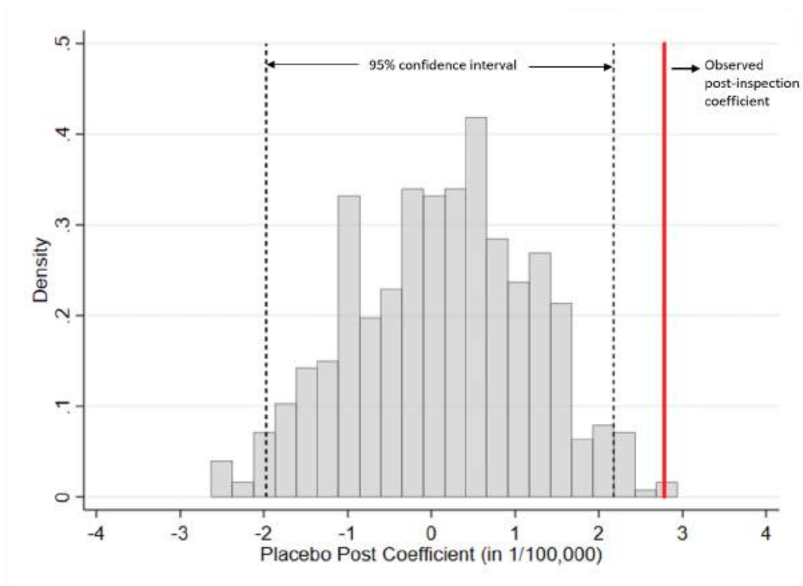
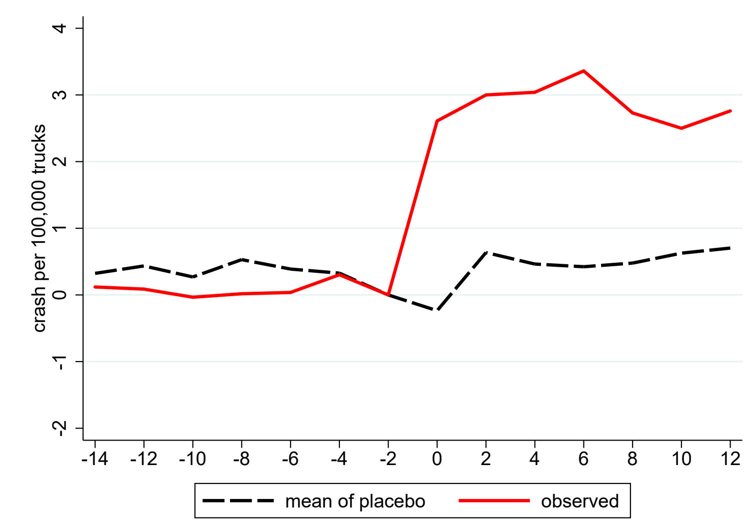
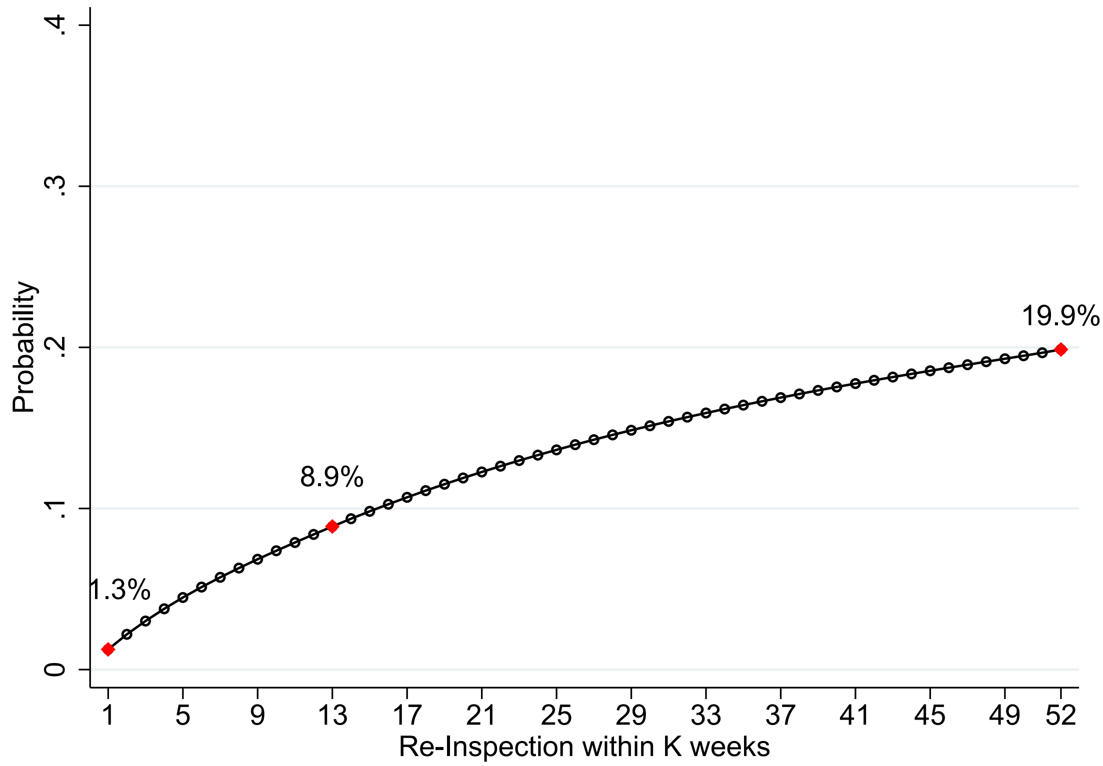


Figure 4: Placebo test: compare 14 days before and after an inspection



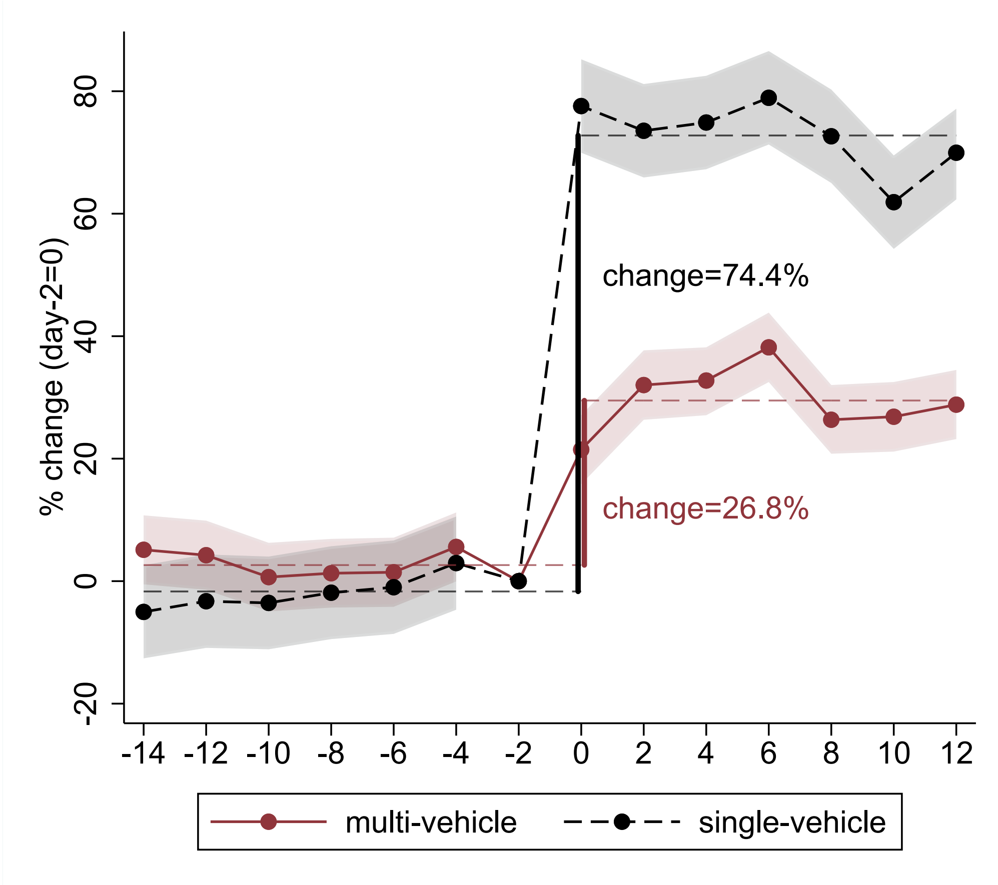
Note: This two figures show the results of a placebo test that conducts 500 random re-shuffling of inspections for each of the randomly selected 20,311 trucks, which represents 0.2% of all trucks ever inspected from 1996 to 2018. Using the subsample, I compare the estimated effect sizes in the 500 placebo tests with the observed effect size using the real sample. Figure 3 shows that the observed post-inspection coefficient lies outside of the 95% confidence interval of the distribution of the coefficients from the 500 placebo tests. Figure 4 compares each lead and lag coefficient in the 28-day event window between the observed real sample and the daily average of the 500 placebo tests, which is almost a flat line. The results show that the samples in the placebo tests would not generate an increase of crash right after inspections. Only the observed inspections could lead to such an increase in crash.

Figure 5: Mechanism: re-inspection probability



Note: This figure shows that the re-inspection probability for a given truck is only 1.3% within the first week after receiving an inspection, and the probability is only 8.9% within the first quarter (13-weeks). It grows up to 19.9% within a year after an inspection. For all trucks inspected in the sample, I generate a list of inspection history for each one of them. I then calculate the time interval between the current inspection and the closest next inspection in the future for a given truck. If the truck is not re-inspected after the last inspection observed in the sample, I replace the time interval to be longer than 1 year. Then for all trucks in the sample, I calculate the percentage of them that were re-inspected within 1 to 52 weeks to plot this graph.

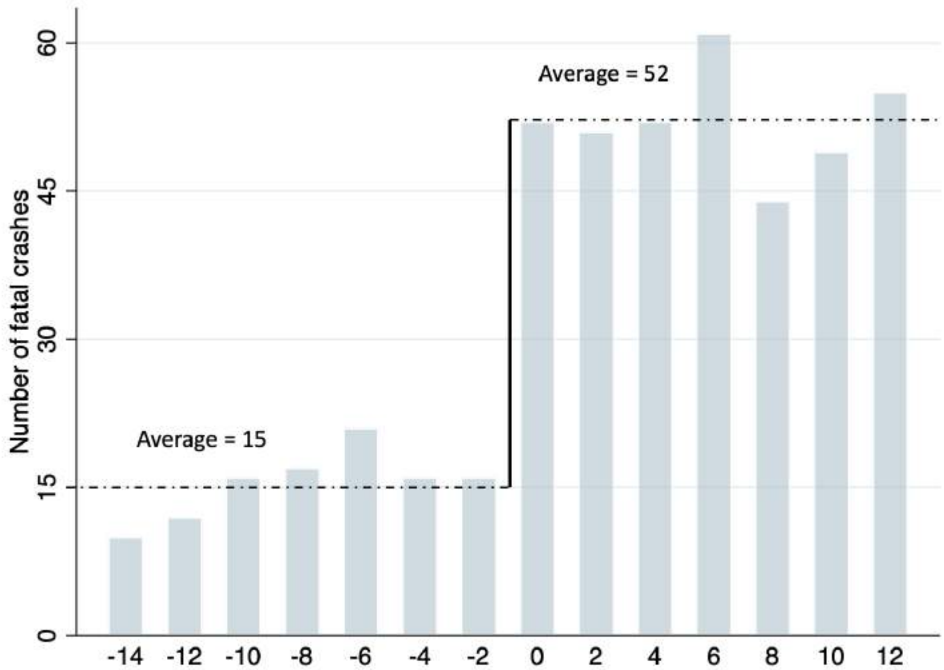
Figure 6: Mechanism: comparing effect sizes in single- vs multi-vehicle crashes



Note: This figure shows that, by estimating equation 7 with different crash variables, single-vehicle crashes increased by 74.4% following an inspection, and multi-vehicle crashes increased by 26.8%. Both effects occur immediately on the day of inspection and last for at least 2 weeks after the inspection. There is no pre-trend for both cases. The standard errors are clustered at the truck level.

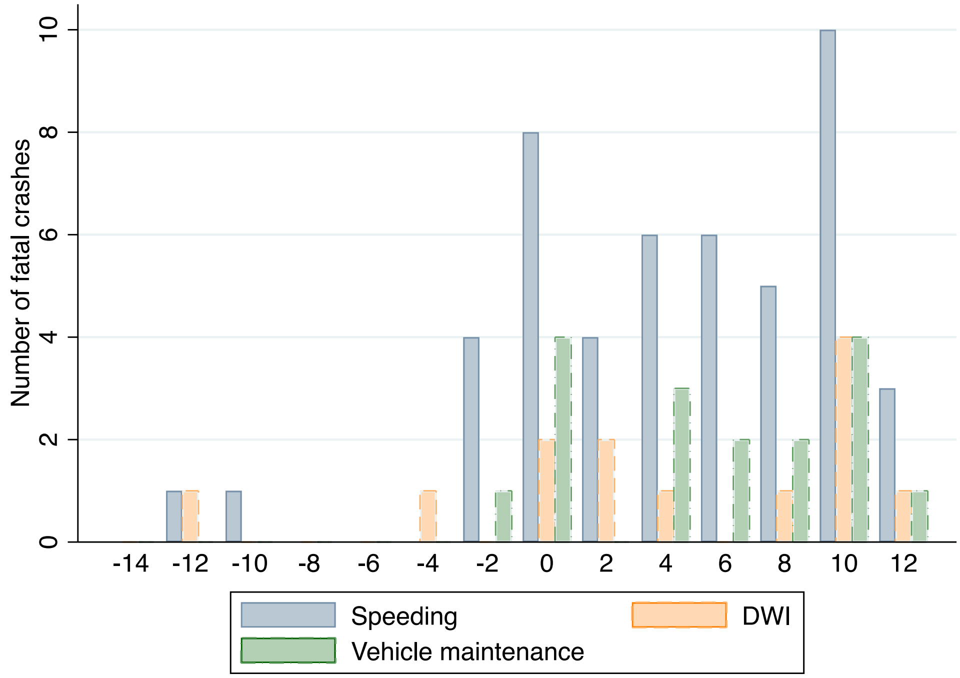
Multi-vehicle crashes include collision with motor vehicles in transport (63.6% of all crashes). Single-vehicle crashes include all noncollision crashes (18.2%), collision involving parked motor vehicle (2.06%), fixed object (6.8%), and all other crash types involving only the truck itself. Together, single-vehicle crashes account for 34.9% of all crashes. There are on average 2.23 single- vehicle crashes versus 4.06 multi-vehicle crashes per 100,000 trucks inspected in a day. In order to compare the effect size of an inspection between those two types of crash accidents, I plot the percentage change in both types of crash accidents using the change in the number of crashes with respect to the corresponding average number of crashes.

Figure 7: Additional evidence: increase in **fatal** crashes



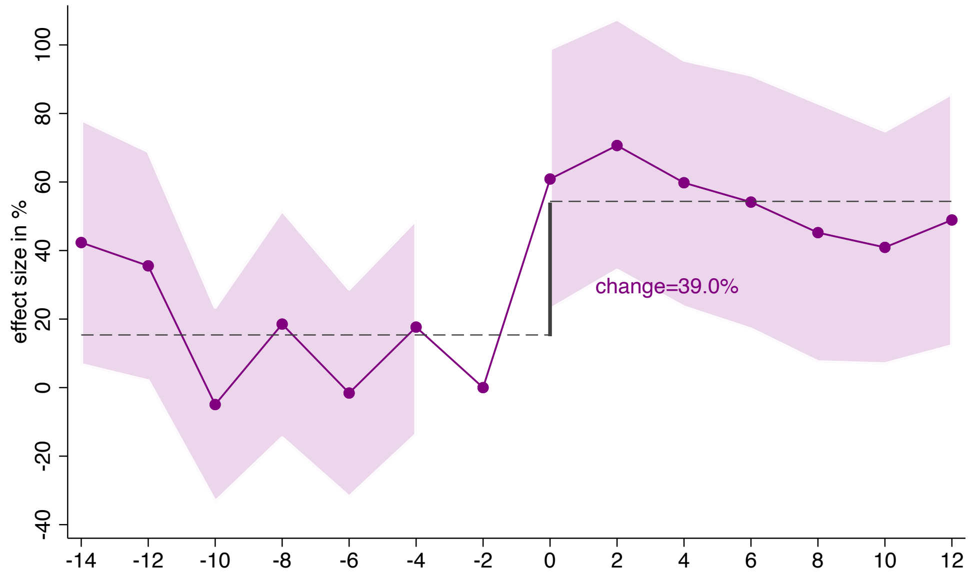
Note: In this figure, I plot the raw number of fatal crashes involving CMVs around inspection days normalized at event time 0. In the FARS sample, there are on average 15 fatal crashes happen for each 2-day bin before inspections (average 7.5 crashes per day). After inspections, there are on average 52 fatal crashes happen for each 2-day bin (average 26 crashes per day).

Figure 8: Additional evidence: increase in **fatal** crashes due to **truck** violations



Note: This figure looks at the number of fatal accidents for which the truck crashing is having violation convictions. I plot the number of crashes for each two-day bin around the inspection day normalized at day 0. It shows that, post inspections, there are increase in crashes due to drivers' reckless driving behaviors, including speeding and driving while intoxicated, and due to lack of vehicle maintenance.

Figure 9: Additional evidence: the impact of an inspection on crashes in Texas



Note: This figure uses crash data obtained from the Texas Department of Transportation (TxDOT) from 2010 to 2018. This set of crash files contain all types of crash, including non-injury and nonfatal crashes. This figure uses the exact same econometric framework in equation 7 for all inspections and crashes happening in Texas. There are 1,487,842 inspections in this analysis. It shows that there is a 39% increase in truck crashes after inspections in Texas, which is almost the same as the average effect size using the data from the whole US.

Figure 10: Additional evidence: increase in **driver-related** crashes

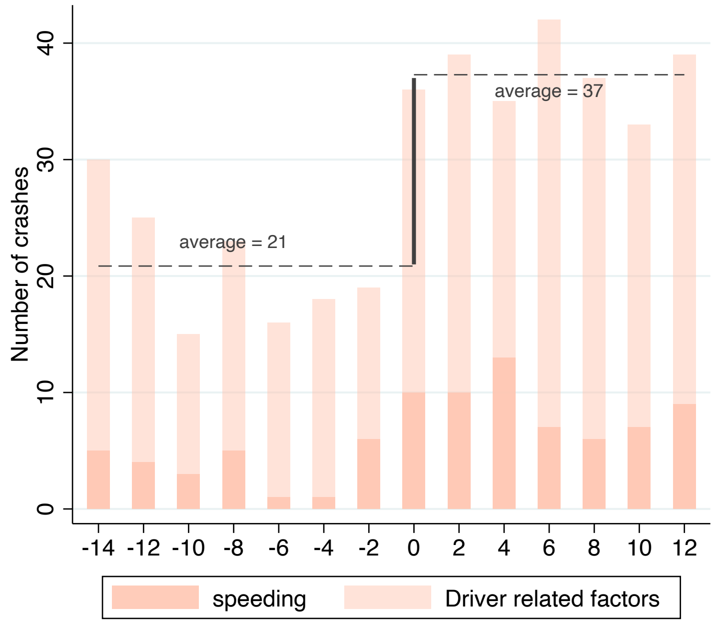
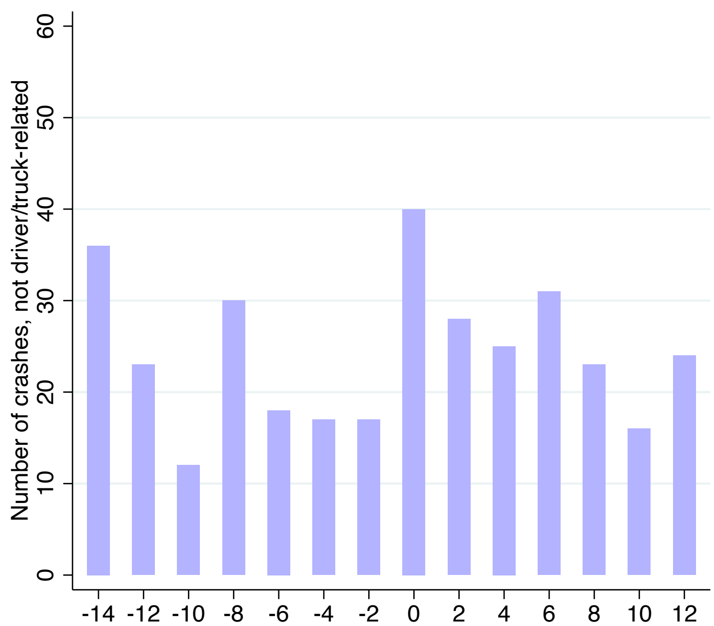
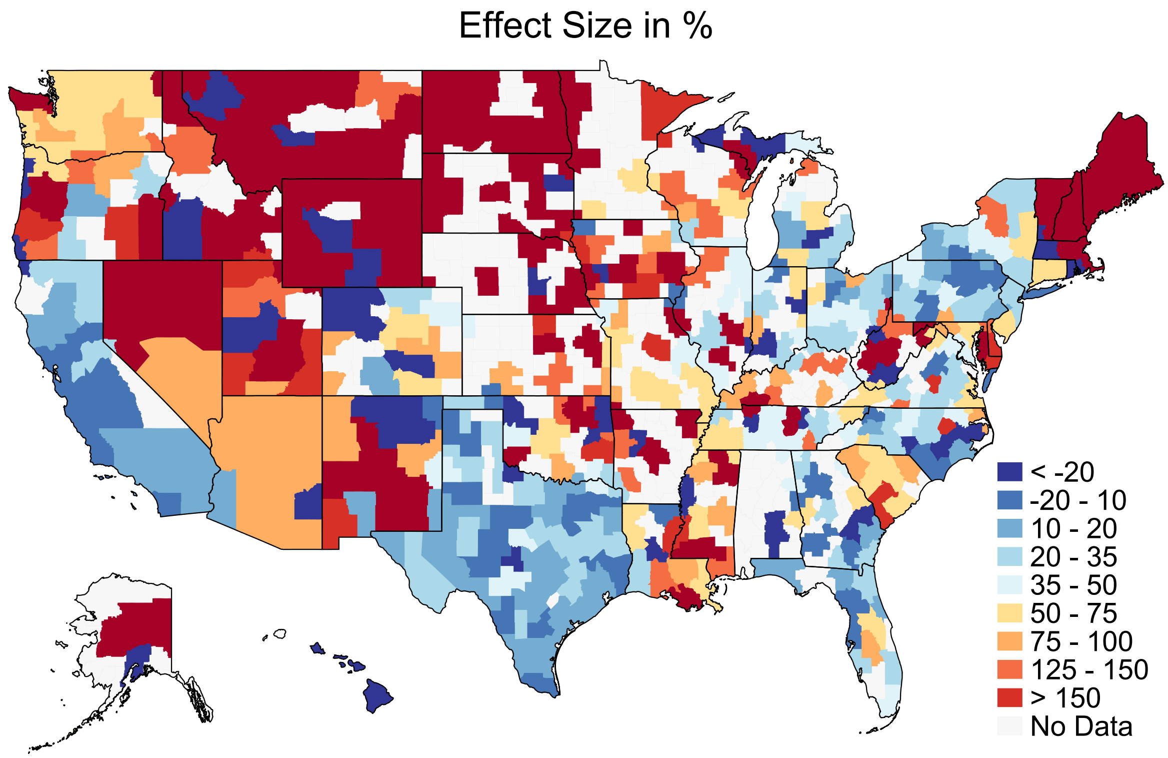


Figure 11: Additional evidence: no effect on other crashes (not driver related)



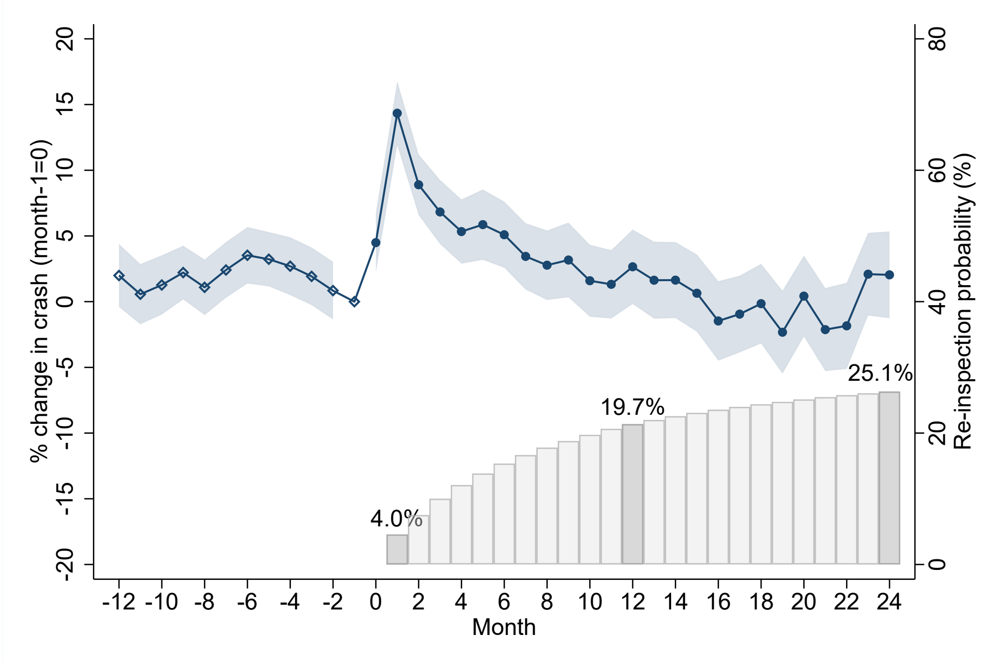
Note: Figure 10 shows that, before inspections, there are on average 21 crashes due to driver-related factors for each 2-day bin, while 37 after inspections. Among those crashes, crashes that have a speeding violation also exhibit a significant increase. Figure 11 shows that there is no increase in the number of crashes which are *not* due to driver behavior related factors. Test for difference in average crash before and after inspection: 1.63e-06 (P = 0.063).

Figure 12: Heterogeneity in effect sizes across commuting zones



Note: This figure presents the spatial heterogeneity on the effect size of an inspection on crashes for all commuting zones in the US. The effect sizes plotted on the map are the VMT adjusted effect sizes. The VMT adjusted effect sizes are computed in the following way. I first estimate the effect size of an inspection on crash using subsamples of inspections conducted in each commuting zone. Next, I weigh the effect size in each commuting zone by the standardized annual VMT by trucks in the corresponding state. The annual VMT of states are standardized to mean one and standard deviation one.

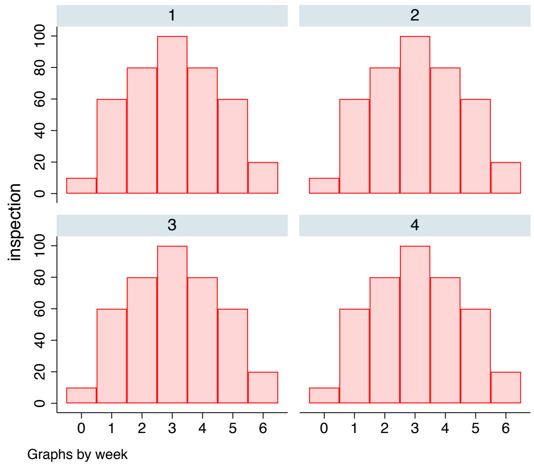
Figure 13: In longer term: the impact of an inspection on truck crashes



Note: In the upper panel, this plot shows the estimation result of the monthly event study that looks at the impact of inspections on truck crashes in the 12 months before to 24 months after an inspection (or inspections) in month 0 for any given truck. Here I use the same research framework as in equation 7 while changing the unit of time from 1 day to 1 month. It shows that, in the longer term, the increase of crash can be detected up to the 12^{th} month after the inspection. The crash rate goes back to the baseline level after 12 months and remains at the baseline until the 24^{th} months, which is the end of the event window.

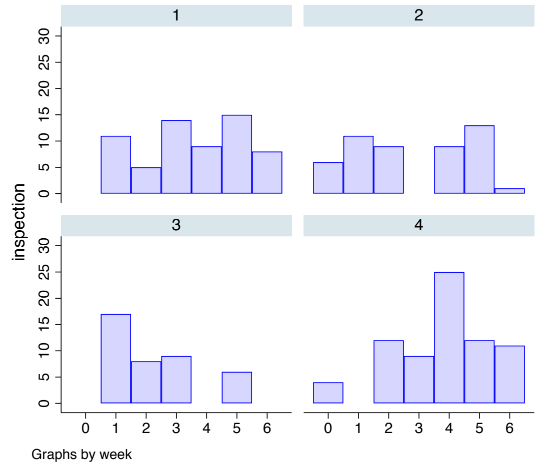
In the lower panel, I plot the probability of re-inspection for any given truck from month 1 to 24 after the inspection. The correspondence between the top and lower panel shows that as the re-inspection probability increases over time, which means that drivers face higher probability of inspection, the increase of crash due to driver's compensating behaviors to the inspection regulation decreases.

Figure 14: Policy option 1: the variation in the day-of-week inspection schedule



County A

mean = 58, *pred_index* = 0

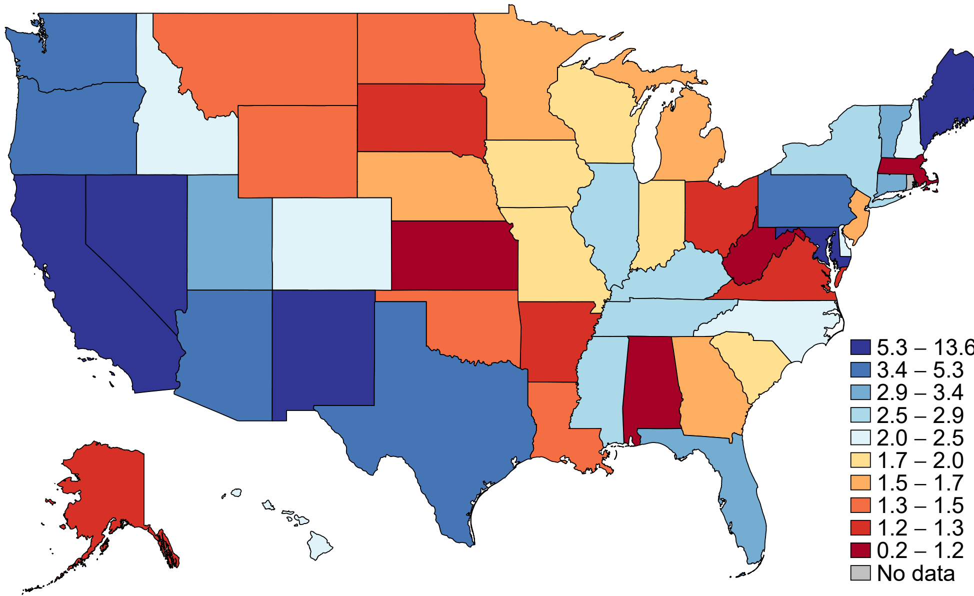


County B

mean = 58, *pred_index* = 32

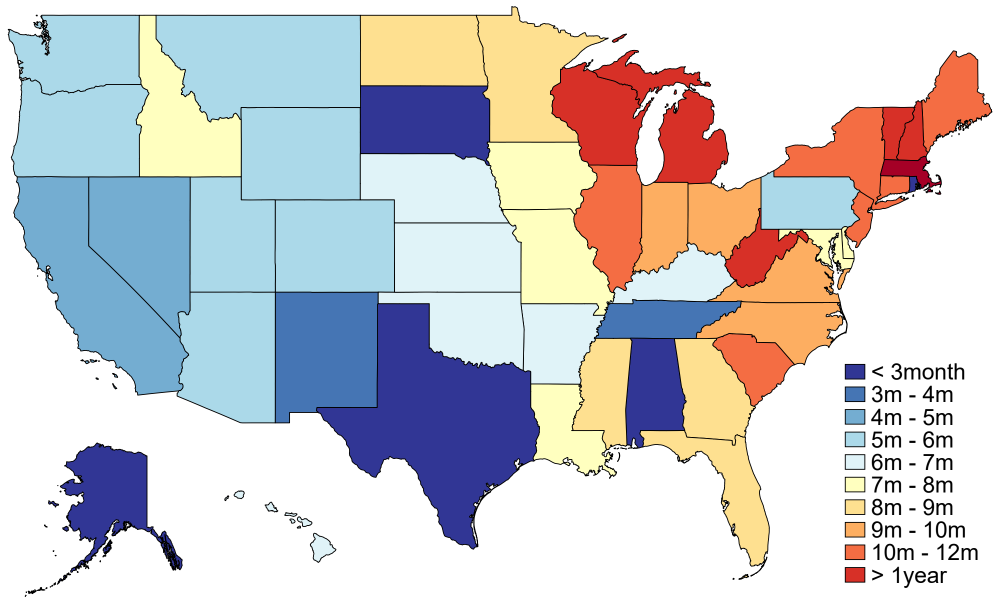
Note: This set of figures shows an example of two counties that have very different day-of-week inspection schedule. In particular, the figures compares the the number of inspections done in each day of the four weeks in county A and B. The average number of inspections per day is 58 for both counties. County A has a fixed day-of-week inspection schedule, meaning that county A is conducting 10 inspections every Sunday, 60 every Monday, 80 every Tuesday, etc.; while county B has a random day-of-week inspection schedule so that the number of inspection done on Sunday this week is different from the next Sunday, this Monday is different from the next Monday, etc. In order to measure the different predictability of the day-of-week inspection schedule, as in county A and B, I develop a predictability index for each state, called *pred_index* following equation 10 and 11. So the lower *pred_index*, the easier the drivers could forecast an inspection. Therefore, in county A, it would be very easy for truck drivers who travel the same route frequently to predict whether they will receive an inspection on a given day of the week. But it would be hard for them to predict in county B. As a result, drivers would always be cautious when driving in county B since they are uncertain of the inspection intensity on a particular day, but they could behave more recklessly when driving or maintain their vehicles less carefully in county A if they know that they are very less likely to get an inspection today, or even for the rest of the week.

Figure 15: Policy option 1: the variation in the predictability index



Note: This figure shows the yearly average of the state×year predictability index (*pred_index*) across all states. The *pred_index* measures the different predictability of the day-of-week inspection schedule, as shown in the example of county A and B in Figure 14. The index is estimated following equation 10 and 11. The lower *pred_index*, the easier the drivers could forecast an inspection. In this figure, warmer colors indicate a lower *pred_index* and a higher predictability in inspection schedule, while cooler colors indicate a higher *pred_index* and a lower predictability.

Figure 16: Policy option 2: the time interval between consecutive inspections



Note: This figure shows the variation of the average re-inspection time interval for trucks inspected in a state. More specifically, for each truck inspected in state *s* in year *yr*, I calculate the number of days passed since the last inspection received for the same truck, then take the average for all trucks that were inspected in state *s* in year *yr*. Cooler colors in this figure indicate a shorter re-inspection interval, and warmer colors indicate a longer re-inspection interval. For example, Texas has an average re-inspection interval of 3-months, but Michigan has an average re-inspection interval that is longer than 1 year.

Table 1: Summary Statistics

	(1) Observations	(2) Mean	(3) SD	(4) Min.	(5) Med.	(6) Max.
Panel A: Inspection file, 1996 to 2018						
Total inspections	69,549,512	-	-	-	-	-
Weigh stations inspections	30,101,086	-	-	-	-	-
Trucks ever inspected	23,078,901	-	-	-	-	-
OOS violations	6,158,151	1.45	1.02	1	1	56
Driver violations (not OOS)	4,953,818	1.43	0.94	1	1	80
Vehicle violations (not OOS)	9,322,306	2.34	2.05	1	2	85
Truck volume	3,354,901	790.98	1957.23	1	410	1,280,036
Panel B: Daily event study panel: (-14,13) days of inspection						
Crashes	842,830,408	6.39E-05	0.008	0	0	2
Injuries	842,830,408	3.74E-05	0.024	0	0	630
Fatalities	842,830,408	2.41E-06	0.002	0	0	10
Inspections per truck	11,017,905	2.73	34.99	1	1	114,360
Crashes per truck	11,017,905	0.005	0.133	0	0	204
Panel C: Monthly event study panel: (-12,24) months of inspection						
Crashes	191,668,177	0.0016	0.058	0	0	32
Injuries	191,668,177	0.0009	0.0508	0	0	49
Fatalities	191,668,177	0.00005	0.008	0	0	11
Panel D: Carrier companies						
No. of trucks	1,669,661	20.91	3420.75	0	1	2,699,990
No. of drivers	1,669,661	5.15	225.72	0	1	110,690

Note: In Panel A, *Total inspections* is the total number of inspections conducted in the whole sample from 1996 to 2018; *Weigh station inspections* are those conducted at the weigh stations, as opposed to at roadside. *Trucks ever inspected* is the number of trucks that ever receive an inspection. *OOS violations* is the number of out-of-service (OOS) violations found in inspections that result in at least one out-of-service (OOS) violation. *Driver violations (not OOS)* is the number of driver violations (but not OOS violations) found in inspections that result in at least one driver violation, similarly with *Vehicle violations (not OOS)*. *Truck volume* is the total number of trucks that pass through the inspection county at the inspection hour.
In Panel B, *crashes* are the number of crashes for a given truck in a day within the event window, similarly with *injuries* and *fatalities*. *Inspections per truck* is the number of weigh station inspections received for each truck in the sample. The number of observations is the number of trucks ever inspected at weigh stations. *Crashes per truck* is the number of crashes involving those trucks.
In Panel C, *crashes* are the number of crashes for a given truck in a month within the event window, similarly with *injuries* and *fatalities*.
In Panel D, *No. of trucks* is the number of trucks owned by each carrier company recorded in the company census file, which is a snapshot of all active carrier (and shipper) companies as of October 2018, similarly with *No. of drivers*. It also shows that there are 1,669,661 active companies registered as of October 2018.

Table 2: The impact of an inspection on truck crashes

	(1) Baseline (all crashes) (per 100,000 trucks)	(2) Single-vehicle crashes (per 100,000 trucks)	(3) Multi-vehicle crashes (per 100,000 trucks)
post_insp	2.78^{***}	1.66^{***}	1.09^{***}
	(0.05)	(0.03)	(0.04)
Average crash rate	6.39	2.23	4.06
Effect size	43.50%	74.44%	26.85%
Observations	842,830,408	842,830,408	842,830,408

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All three columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14 days before to 14 days after inspection. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. Standard errors are clustered at the truck level. Column 1 looks at the impact of an inspection on all kinds of crashes. Column 2 looks at single-vehicle crashes only, other crashes are dropped in the estimation. Column 3 looks at multi-vehicle crashes only, other crashes are dropped in the estimation. The average crash rate is calculated using the number of any kinds of crashes, or only single-, or multi-vehicle crashes per 100,000 trucks inspected. All regressions dropped inspections that happen within 18 hours after crashes.

Table 3: The impact of an inspection on crashes by inspection outcomes

Dependent var: number of crashes (per 100,000 trucks)	(1) No violation	(2) Any violation	(3) Driver violation	(4) Vehicle violation
post_insp	2.78***	2.90***	4.04***	2.42***
	(0.09)	(0.08)	(0.15)	(0.09)
Average crash rate	6.52	6.23	7.64	5.65
Effect size	42.64%	46.55%	52.88%	42.83%
Observations	323,231,972	347,170,208	138,706,904	261,024,568

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before and after. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. All regressions dropped inspections that happen within 18 hours after crashes. Standard errors are clustered at the truck level. Column 1 looks at the impact of an inspection on crashes for trucks that do not receive any violation from the inspection, which accounts for 38% of all inspections. Column 2 looks at trucks that do receive violations but not out-of-service violations (41%). Among trucks that receive violations, column 3 and 4 further break down to driver-related violations and vehicle-related violations. Note that there are trucks that receive both driver and vehicle violations in one inspection.

Table 4: The impact of an inspection on crashes by firm characteristics

Number of crashes (per 100,000 trucks)	(1)	(2)	(3)
Panel A: Firms size, measured by the number of vehicles			
	Large	**Small**	**1-truck-1-driver**
post_insp	3.35^{***}	2.82^{***}	2.88^{***}
	(0.11)	(0.11)	(0.26)
Average crash rate	7.67	7.29	6.87
Effect size	43.68%	38.68%	41.92%
Observations	236,224,044	240,011,464	39,294,248
Panel B: Firms size, measured by the number of drivers			
	Large	**Small**	
post_insp	3.33^{***}	2.84^{***}	
	(0.11)	(0.11)	
Average crash rate	7.68	7.29	
Effect size	43.36%	38.96%	
Observations	237,548,472	238,687,064	
Panel C: Firms type, measured by inter- or intra-state business			
	Interstate only	**Intrastate only**	**Both inter- & intrastate**
post_insp	3.29^{***}	2.30^{***}	2.49^{***}
	(0.09)	(0.26)	(0.17)
Average crash rate	7.84	6.05	6.53
Effect size	41.96%	38.02%	38.13%
Observations	357,311,780	33,435,220	84,399,140
Panel D: Firms type, measured by the number of trucks or buses			
	Truck only	**Bus only**	
post_insp	3.03^{***}	2.51^{***}	
	(0.08)	(0.62)	
Average crash rate	7.45	5.99	
Effect size	40.67%	41.90%	
Observations	450,253,664	6,089,860	

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All four panels use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before and after. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. All regressions dropped inspections that happen within 18 hours after crashes. Panel A and B both compare large firms with small firms. I measure company sizes according to their inventories in two ways: the number of vehicles and the number of drivers. I define a large company as one having more than the median number of power units (or drivers) among all companies in the inspection sample, and small company are the rest of the sample. The median is 48 power units or 47 drivers. I also look at firms with 1 truck and 1 driver only. Panel C compares firms that hire drivers to drive interstate, or intrastate, or both routes. Panel D compares firms that own trucks or buses as their main inventory.

Table 5: The impact of an inspection on crashes by carrier's cargo types

Type of Cargo	post_insp (per 100,000 trucks)	S.E.	Avg crash rate	Effect size	No. of Obs
General freight	3.27***	(0.10)	7.71	42.41%	336,706,076
Chemicals	3.49***	(0.18)	7.72	45.21%	95,883,424
Food and beverage	3.38***	(0.13)	7.87	42.95%	185,007,088
Paper products	3.34***	(0.14)	7.84	42.60%	151,570,972
Building materials	3.04***	(0.14)	7.49	40.59%	143,361,876
Metal: sheets, coils, rolls	3.15***	(0.17)	7.81	40.33%	109,749,304
Heavy duty commodities	2.93***	(0.11)	7.25	40.41%	236,750,864

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before and after. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. All regressions dropped inspections that happen within 18 hours after crashes. This table looks at the impact of inspections on crashes of different carrier companies depending on the type of cargo they transport. I summarized the 30 types of cargo that the carrier/shipper companies transport into general freight, chemicals, food and beverage, paper product, building materials, metal sheet, and heavy duty commodities. Such categorization makes sure that there are enough observations within each category.

Table 6: The impact of an inspection on crashes under two alternative regulatory designs

Panel A: Inspection schedule, equation 12	
post_insp	2.78***
	(0.05)
std. $pred_{s,yr}$	-0.09
	(0.06)
post_insp × std. $pred_{s,yr}$	-0.58***
	(0.05)
Average crash rate	6.39
Observations	842,160,116

Panel B: Randomness in selection, equation 13	
post_insp	2.78***
	(0.05)
std. $Dtime_{s,yr}$	-0.14**
	(0.07)
post_insp × std. $Dtime_{s,yr}$	0.66***
	(0.05)
Average crash rate	6.39
Observations	842,158,632

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Both panels use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before and after. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. $std\_pred_{s,yr}$ is the predictability index of inspection schedule for state s in year yr, standardized to mean 0 and standard deviation 1. $std\_Dtime_{s,yr}$ is the average re-inspection time interval calculated for state s in year yr, standardized to mean 0 and standard deviation 1. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. All regressions dropped inspections that happen within 18 hours after crashes.

Table 7: Policy counterfactuals by two regulation designs

Panel A: Inspection schedule	Effect size
Highly predictable (low $pred_{s,yr}$)	51%
Highly **Un**predictable (high $pred_{s,yr}$)	-7%
1 S.D. increase in $pred_{s,yr}$	-9%

Panel B: Randomness in selection	Effect size
Short re-inspection interval (1-month)	25%
Long re-inspection interval (> 1 year)	62%
1 S.D. (5-month) increase in the interval	10%

Notes: The effect sizes in this table are inferred from the estimates in Table 6. $pred_{s,yr}$ is the predictability index of inspection schedule for state s in year yr. $Dtime_{s,yr}$ is the average re-inspection time interval calculated for state s in year yr. In the first row of Panel A, to calculate the effect size for states with highly predictable inspection schedule, I use the lowest 1^{st} percentile in the distribution of $pred_{s,yr}$. In the second row of Panel A, for the case of highly unpredictable inspection schedule, I use the 99^{th} percentile in the distribution of $pred_{s,yr}$. In the first row of Panel B, to calculate the effect size for states with shortest re-inspection interval, I use 1-month as the re-inspection interval, which is the lowest 1^{st} percentile in the distribution. In the second row of Panel B, I use 1-year as the long re-inspection interval, which is the 95^{th} percentile in the distribution.

Appendices

Figure A.1: An inspection at a weigh station

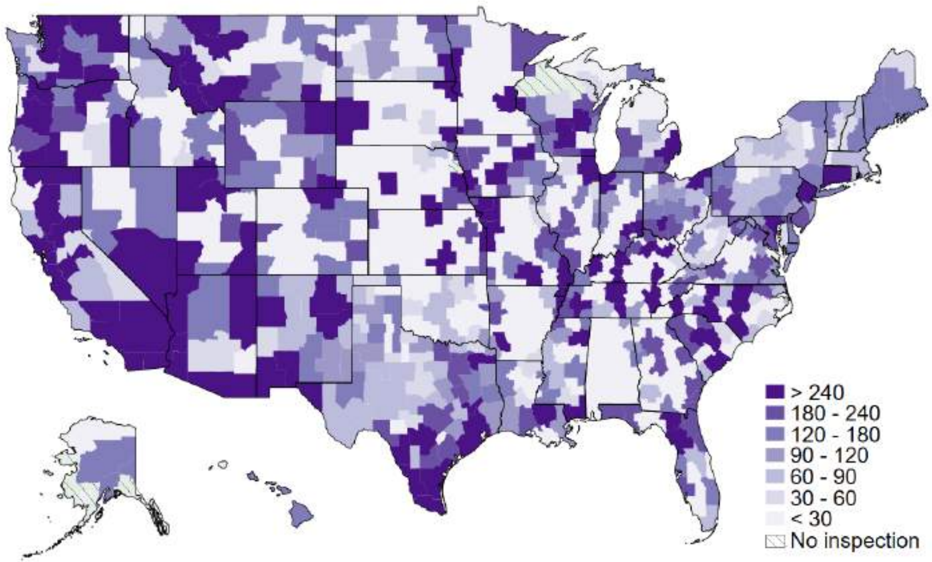
(a) step1: enter the weigh station

(b) step2: get weighted

(c) step3: the inspector chooses

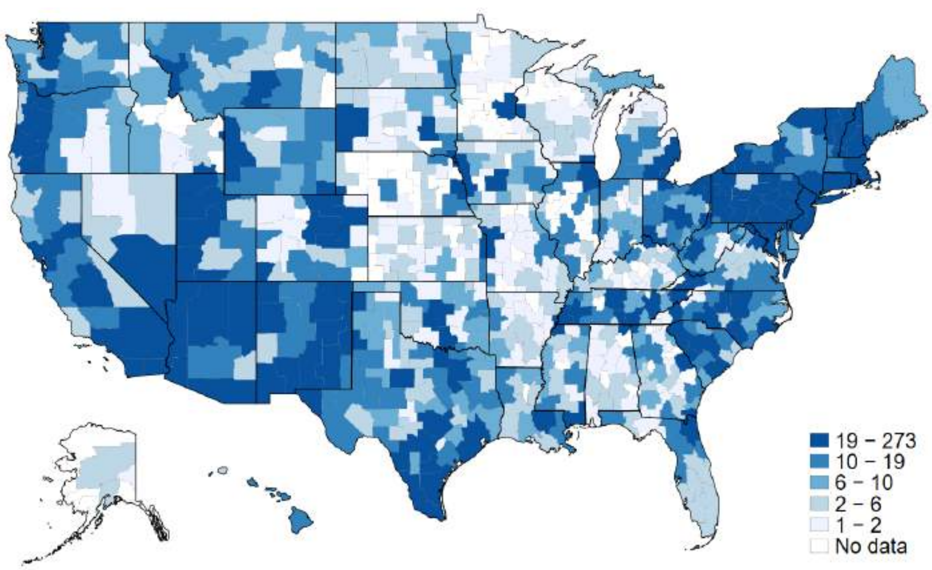
(d) step4: closer inspection

Figure A.2: The days of inspection per year by commuting zone



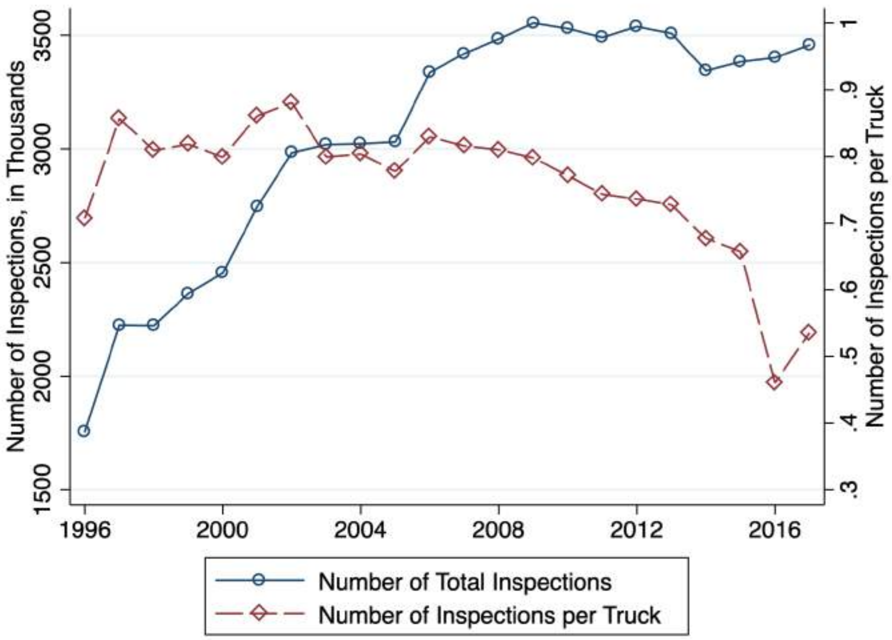
Note: From author's calculation based on the inspections conducted at the fixed weigh stations, excluding the roadside inspections. Data are aggregated to the 709 commuting zones in the US.

Figure A.3: The number of weigh stations by commuting zone



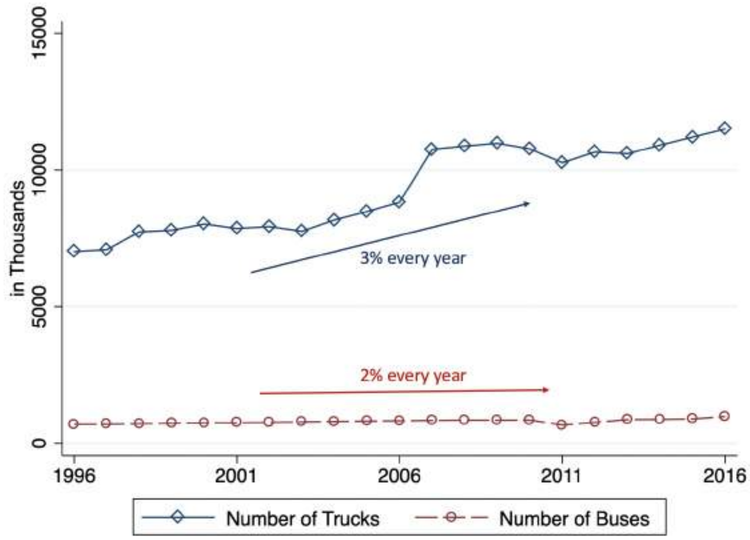
Note: From author's calculation based on the inspections conducted at the fixed weigh stations. Data are aggregated to the 709 commuting zones in the US.

Figure A.4: The number of inspections over time



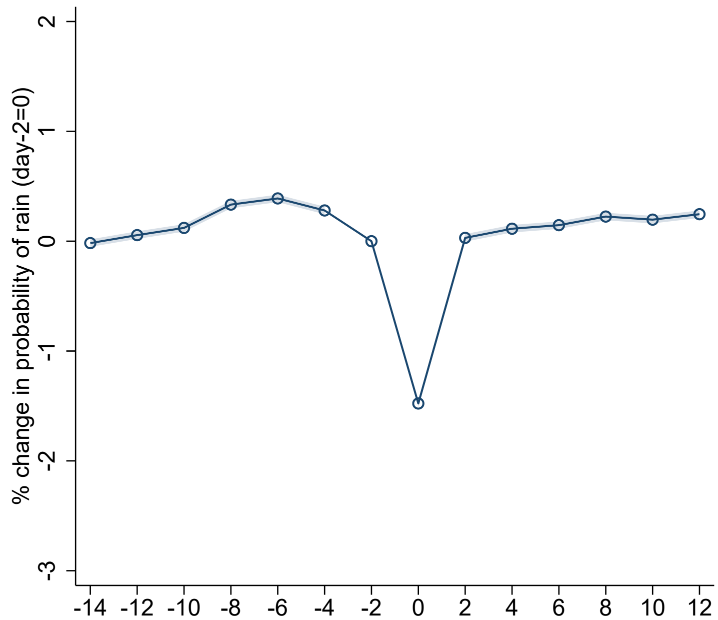
Note: From author's calculation based on all inspection records, including fixed weigh station inspections and roadside inspections. The y-axis on the left shows the total number of inspections in the US per year, while the y-axis on the right shows the number of inspections per truck. The number of inspections per truck declines over time despite the increase in the total number of inspections since the number of vehicles increase much faster.

Figure A.5: The number of trucks and buses over time



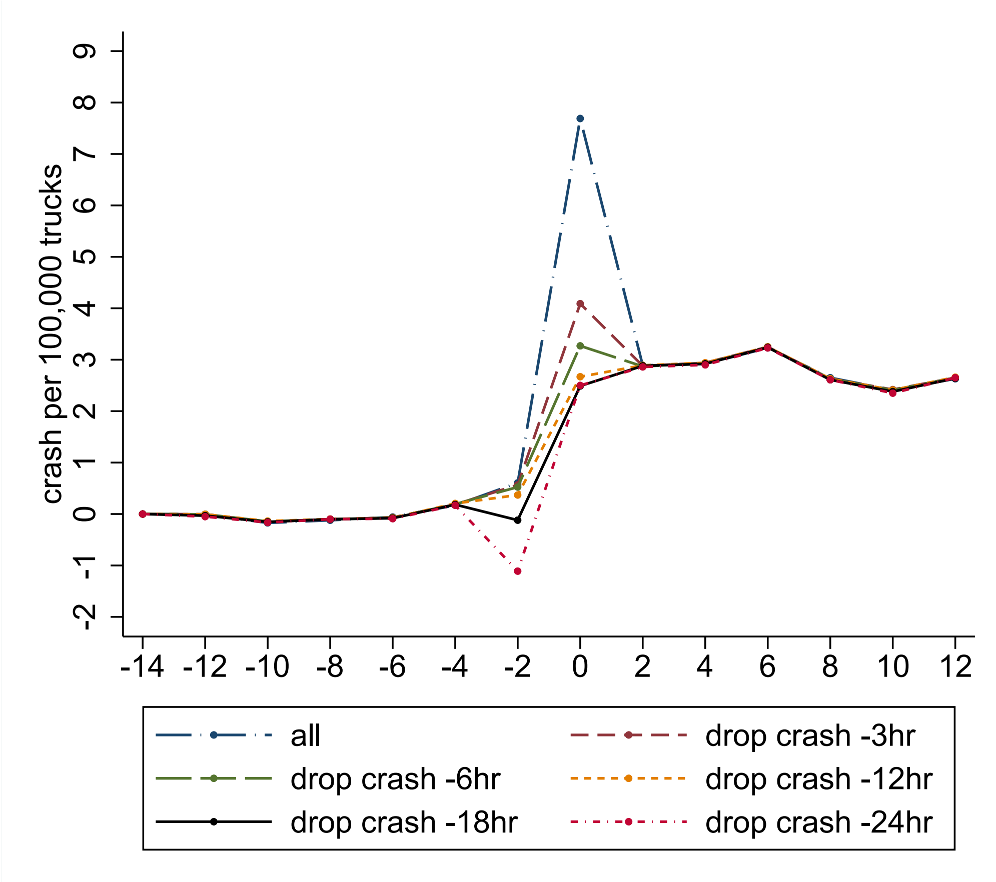
Note: The number of trucks and buses are collected from the Bureau of Transportation Statistics. Data for 2007-17 were calculated using a new methodology developed by FHWA. Data for these years are based on new categories and are not comparable to previous years. So the rate of increase are calculated separately for years before 2007 and after 2007 and then taken the mean. They average at 3% per year over the period.

Figure A.6: Ruling out weather condition as a confounder



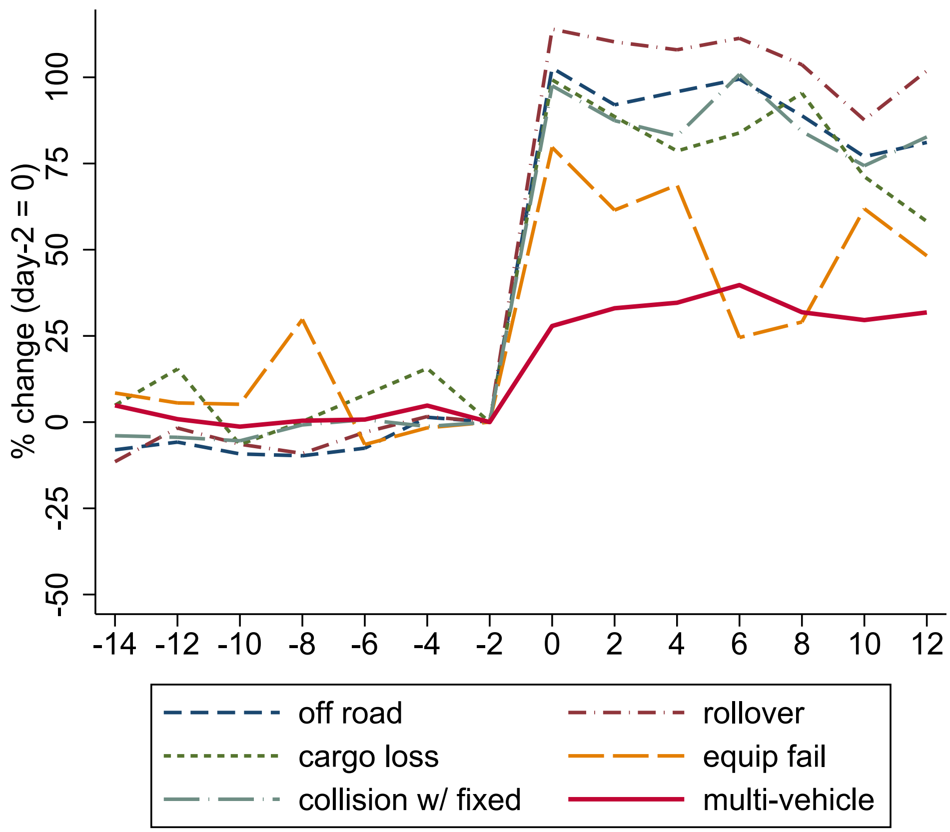
Note: This figure shows the relationship between inspections and a rain indicator in the county where the inspections take place. I rule out that weather conditions are confounders by showing that the inspections are not chosen at times of worse weather conditions (rain), on the contrary, the inspections are chosen at days with better weather conditions. The coefficients plotted in this figure are estimated using equation 7 where I regress a rain indicator of the county where the inspection takes place on a set of inspection indicators from 14 days before an inspection to 14 days after inspection, controlling for individual truck fixed effects, year, month, and day-of-week fixed effects. The shaded area is the 95% confidence interval for the estimates. The effect of an inspection on crash accidents happening in the two-day bin (-2,-1) is normalized to 0.

Figure A.7: Eliminating the reverse causality



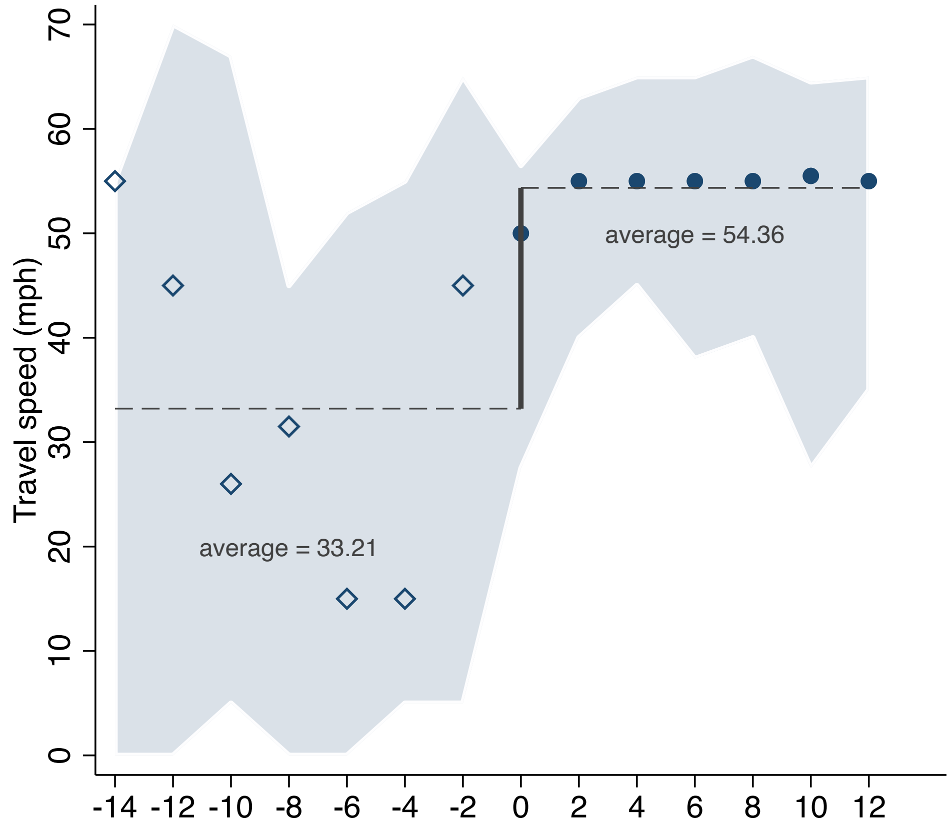
Note: This figure shows that by varying the length of the time interval during which the inspections are dropped after crash accidents, the coefficients on two-day bin (-2, -1) and (0, 1) drop as the length gets longer, but it does not affect any other coefficients. The first line marked as "all" is the full sample without dropping an inspections. Then I look at the choice of dropping inspections within 3, 6, 12, 18, and 24 hours of crash. I decide to use 18-hour as the proper time frame since the coefficient on the (-2,-1) bin is consistent with that on (-4,-3) bin. I normalize the effect of an inspection on crashes on day (-14,-13) to be 0 in order to compare across samples. The coefficients plotted in this figure are estimated using equation 7 where I regress the number of crash accidents for the same truck that receives the inspection on a set of inspection indicators from 14 days before an inspection to 14 days after inspection, controlling for individual truck fixed effects, year, month, and day-of-week fixed effects.

Figure A.8: Comparing effect sizes in different crash categories



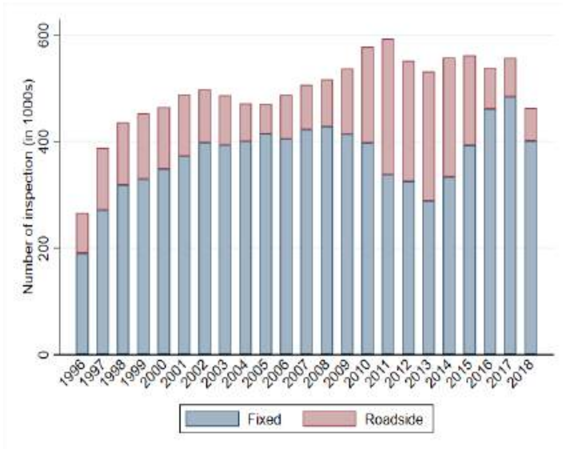
Note: This figure complements figure 6 by further breaking down the single-vehicle crash group into detailed crash categories. First 5 categories in the figure are all single-vehicle crashes: truck run off the road, truck rollover, cargo loss, equipment failure, and collision with fixed objects. Multi-vehicle crashes include all collisions with moving motor vehicles. The vertical axis is the percentage change relative to each of the mean crash rate. The coefficients plotted in this figure are estimated using equation 7 where I regress the number of crash accidents for the same truck that receives the inspection on a set of inspection indicators from 14 days before an inspection to 14 days after inspection, controlling for individual truck fixed effects, year, month, and day-of-week fixed effects. The effect of an inspection on crash accidents happening in the two-day bin (-2,-1) is normalized to 0.

Figure A.9: Travel speed prior to the occurrence of the fatal crashes

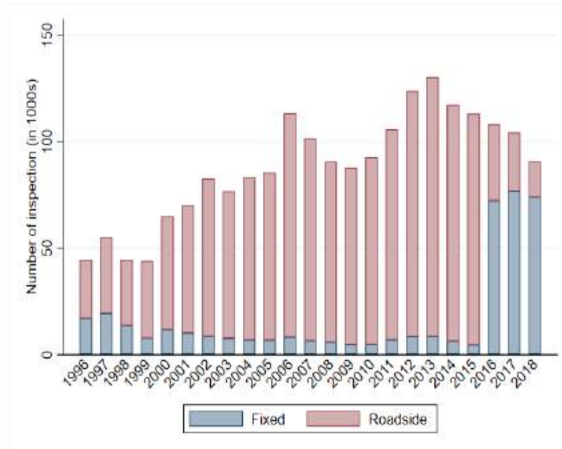


Note: In the FARS sample, there are 472 fatal crashes that occur [-14,13] days for trucks that receive inspections. Only 246 of those crashes have records on the travel speed prior to the occurrence of the crashes. The dots in the plot are the median of the travel speed for all crashes happen each day within the time frame of interest. The shaded area of the plot shows 25 to 75 percentile of the distribution of travel speed within each 2-day bin. Since there are even fewer fatal crashes that have travel speed recorded and happen before inspections, so the confidence interval is wider. This figure gives suggestive evidence that there is an increase in the travel speed for crashes happen after inspections, from 30.5 mph before to 54 mph after inspections.

Figure A.10: Number of fixed vs roadside inspections across years



California



Pennsylvania

Note: This figure compares the different inspection strategies between California and Pennsylvania, highlighting the difference in the number of fixed vs roadside inspections. It shows that California implemented many roadside inspections during 2009 to 2015, but more fixed station inspections in other years. While Pennsylvania used to have small number of fixed station inspections, but made a significant shift in the type of inspections conducted at year 2015.

Table B.1: The impact of an inspection on crashes, controlling for the traffic volume

Dependent var: number of crashes (per 100,000 trucks)	(1)	(2)	(3)	(4)	(5)
post_insp	2.96^{***}	2.96^{***}	2.96^{***}	3.70^{***}	3.07^{***}
	(0.193)	(0.194)	(0.193)	(0.269)	(0.396)
truck_count		-.00002		$.0014^{***}$	
		(.00006)		(.0001)	
pct_truck			2.20^{**}		.935
			(1.07)		(1.62)
post_insp×truck_count				$-.0003^{*}$	
				(.0002)	
post_insp×pct_truck					-2.69
					(1.81)
Observations	96,320,300	96,462,478	96,329,947	96,441,835	96,309,902

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14 days before to 14 days after inspection. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. *truck_count* is the number of trucks passed by in the same hour and county as when the inspection happened. *pct_truck* is the percentage of truck volume out of all motor vehicles in the same hour and county as the inspection. The sample period in this exercise is from 2012-2018 since the traffic volume data is available from 2012 onwards. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. Standard errors are clustered at the truck level. All regressions dropped inspections that happen within 18 hours after crashes.

Table B.2: The impact of an inspection on no-injury truck crashes

	(1) Baseline (all crashes) (per 100,000 trucks)	(2) No-injury & nonfatal crashes (per 100,000 trucks)
post_insp	2.78***	1.56***
	(0.05)	(0.04)
Average crash rate	6.39	3.55
Effect size	43.50%	43.94%
Observations	842,830,408	842,185,848

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Both columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before to 14 days after inspection. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. Standard errors are clustered at the truck level. All regressions dropped inspections that happen within 18 hours after crashes. Column 1 is the baseline estimation using all kinds of crashes. Column 2 only looks at no-injury and nonfatal crashes, other crashes are dropped in the estimation. The average crash rate is calculated using the number of crashes per 100,000 trucks inspected.

Table B.3: The impact of an inspection on crashes under different crash external conditions

Dependent var: number of crashes (per 100,000 trucks)	(1) All single-vehicle crashes	(2) Normal conditions	(3) Adverse conditions
post_insp	1.66^{***}	0.55^{***}	1.05^{***}
	(0.03)	(0.02)	(0.02)
Average crash rate	2.23	0.89	1.24
Effect size	74.44%	61.21%	84.68%
Observations	842,830,408	842,830,408	842,830,408

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All three columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before to 14 days after inspection. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. Standard errors are clustered at the truck level. All regressions dropped inspections that happen within 18 hours after crashes. Crash external conditions include road surface conditions, weather conditions and light conditions. Column 1 prints the baseline estimation using all single-vehicle crashes. Column 2 selects single-vehicle crashes under normal external conditions (all three external conditions are normal). Column 3 selects single-vehicle crashes under adverse external conditions (any one of the three external conditions is worse).

Table B.4: Demographics for drivers involved in fatal crashes

	(1) before inspections	(2) after inspections
Sex (% male)	97.15%	96.87%
	(.0459)	(.0341)
Age	44.63	43.17
	(11.45)	(12.32)
No valid CDL (%)	10.59%	15.03%
	(.12)	(.06)
No. of prev. convictions	0.86	0.86
	(1.07)	(1.27)

Note: This table summarizes the characteristics for drivers involved in the fatal crashes 14 days before and after an inspection. Standard deviations in parentheses. In the FARS sample, there are on average 7.5 crashes per day before inspections. After inspections, there are on average 26 crashes per day. *Sex* is the percentage of male drivers involved. *Age* is the average age for all drivers involved. *No valid CDL* is the percentage of drivers involved with invalid commercial driver license (CDL). *No. of prev. convictions* is the average number of previous moving convictions for drivers involved.

Table B.5: The impact of an inspection on crashes by carrier's cargo types

Type of Cargo	post_insp (per 100,000 trucks)	S.E.	Avg crash rate	Effect size	No. of Obs
General freight	3.27***	(0.10)	7.71	42.41%	336,706,076
Chemicals	3.49***	(0.18)	7.72	45.21%	95,883,424
Food and beverage	3.38***	(0.13)	7.87	42.95%	185,007,088
Paper products	3.34***	(0.14)	7.84	42.60%	151,570,972
Building materials	3.04***	(0.14)	7.49	40.59%	143,361,876
Metal: sheets, coils, rolls	3.15***	(0.17)	7.81	40.33%	109,749,304
Heavy duty commodities	2.93***	(0.11)	7.25	40.41%	236,750,864

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All columns use the same estimation framework following equation 8, which looks at the impact of an inspection on crashes by comparing 14-days before and after. *post_insp* is a post-inspection indicator, it equals to 0 before an inspection happen on truck i, and it equals to 1 on and after the inspection. All regressions include dummies for inspection happened outside of the time window of interest, as well as individual truck, year, month, and day-of-week fixed effects. All regressions dropped inspections that happen within 18 hours after crashes. This table looks at the impact of inspections on crashes of different carrier companies depending on the type of cargo they transport. I summarized the 30 types of cargo that the carrier/shipper companies transport into general freight, chemicals, food and beverage, paper product, building materials, metal sheet, and heavy duty commodities. Such categorization makes sure that there are enough observations within each category.

A Appendix c. Proof of Proposition

In this section, I present a theoretical model on driver's traffic safety behaviors following the framework developed by Peltzman (1975) and Blomquist (1986). I use the model to demonstrate that drivers exert less private safety effort when they expect the re-inspection probability drops to zero following an initial inspection comparing to the case when the re-inspection probability stays the same regardless of the truck's inspection status. As a result, the total number of crashes is larger.

In the model, drivers maximize their expected income by choosing the optimal private effort e in reducing accident loss.

$$\max_{e} E = \beta S_1 + (1-\beta) S_2, \tag{14}$$

where E = the expected income for a given driving mileage, β = probability of inspections, and S_1, S_2 = net income when encountering an inspection and no inspection, respectively. If the driver gets an inspection then the driver's net income is

$$S_1 = p(e,v)(I - D(e,v) - L) + (1 - p(e,v))(I - D(e,v)), \tag{15}$$

where I = gross income in the limit where the effort devoted to driving a given mileage is zero and no inspection happens. $p = p(e,v)$ is the probability of a crash accident, which is defined as a function of private effort e and the inspection v. The inspection v is specified by $v = v(e)$, which can be treated as the compliance cost to inspections that depends on private effort e. $D(e,v)$ represents the cost from private effort e and the compliance costs to inspection v. For simplicity, I define $D(e,v) = e + v$. L = the loss from an accident.

If the driver does not get an inspection, the driver's net income is

$$S_2 = p(e,0)(I - D(e,0) - L) + (1 - p(e,0))(I - D(e,0)), \tag{16}$$

In S_2, since there is no inspection, $p = p(e,0)$ and $D(e,v) = D(e,0) = e$.

Combining equation 14, 15, 16 and rearranging terms, the expected income is the following:

$$E = \beta[I - e - v - p(e,v)L] + (1-\beta)[I - e - p(e,0)L] \tag{17}$$

The first order condition for optimal private safety effort can be simplified to:

$$\frac{\partial E}{\partial e} = -1 - \beta \frac{\partial v}{\partial e} - L[\frac{\partial p}{\partial e} + \beta \frac{\partial p}{\partial v} \frac{\partial v}{\partial e}] = 0 \tag{18}$$

Assumption.

1. The probability of an accident is bounded between 0 and 1, and decreases in driver's private effort e and inspection v: $\frac{\partial p}{\partial e} < 0, \frac{\partial p}{\partial v} < 0, \frac{\partial^2 p}{\partial e^2} > 0, \frac{\partial^2 p}{\partial v^2} > 0, \frac{\partial^2 p}{\partial e \partial v} > 0$. In addition, value of inspection $v = v(e)$ (or violations from inspections) decreases in private effort e: $\frac{\partial v}{\partial e} < 0$.

2. Drivers prefer not to be inspected because the reduction in expected loss from accident is smaller than the increase in compliance costs on the margin: $-\frac{\partial p}{\partial v}L < 1$.

Consider two scenarios: in the first scenario, a truck driver receives an inspection which significantly lowers the probability that the driver gets another inspection within a long time. Here I assume that the re-inspection probability drops to zero. In the second scenario, a truck driver receives an inspection which does not affect the probability that the driver gets another inspection, in other words, the probability of getting an inspection is the same for all drivers regardless of their recent inspection status.

Proposition. *Under the assumptions, we can derive the following two results from comparing the two scenarios,*

1. *Drivers exert less private effort when the re-inspection rate drops to zero following an initial inspection (first scenario) comparing to the case when the re-inspection rate stays the same regardless of the truck's inspection status (second scenario).*
2. *As a result, the total loss (or the total number of crashes) is larger under the first scenario comparing to the second scenario.*

Proof. In the first scenario, the first order condition in equation 18 becomes

$$\left.\frac{\partial p}{\partial e}\right|_{e_0} = -\frac{1}{L} \tag{19}$$

where e_0 is the driver's optimal private effort when the re-inspection rate is zero for trucks recently inspected.

In the second scenario, the first order condition in equation 18 becomes

$$\left.\frac{\partial p}{\partial e}\right|_{e_1} = -\frac{1}{L} - \beta\frac{\partial v}{\partial e}\left(\frac{1}{L} + \frac{\partial p}{\partial v}\right) \tag{20}$$

Under assumption 1, we know that $\frac{\partial v}{\partial e} < 0$. Therefore, the relative magnitude of $\left.\frac{\partial p}{\partial e}\right|_{e_0}$ and $\left.\frac{\partial p}{\partial e}\right|_{e_1}$ depends on the sign of $\frac{1}{L} + \frac{\partial p}{\partial v}$.

Under assumption 2, drivers prefer not to be inspected because the reduction in expected loss from accident is smaller than the increase in compliance costs on the margin: $-\frac{\partial p}{\partial v}L < 1$. Therefore, we have $\frac{1}{L} + \frac{\partial p}{\partial v} > 0$. It implies that $\left.\frac{\partial p}{\partial e}\right|_{e_0} < \left.\frac{\partial p}{\partial e}\right|_{e_1}$. Also from assumption 1, we know $\frac{\partial^2 p}{\partial e^2} > 0$, therefore, $e_0 < e_1$.

So far, we prove proposition 1 that says when the inspection is not effective in reducing the probability of crashes, then if driver does not expect to receive another inspection following an initial inspection, then the driver's private effort e_0 is going to be smaller than e_1, which is the private effort exerted if the driver knows that the rate of inspection is constant and does not depend on recent inspection status.

Now we go on to derive proposition 2. Suppose there are in total n truck drivers in the society, the total loss from crash accidents, or equivalently, the total number of crashes depends on driver's private effort in reducing crashes e, and the inspection v. Since $v = v(e)$ is a function of driver's private effort, the total number of crashes then depends on private efforts exerted by all the truck drivers in the society. From proposition 1, we know that drivers' efforts depend on their believes on the re-inspection rate. Therefore, the total number of crashes depends on drivers' believes on the re-inspection rate.

In order to compare the total number of crashes under the two scenarios discussed above, we assume that the inspectors are always choosing βn out of n trucks for inspection, the difference lies in what kind of trucks they choose to inspect.

In particular, in the first scenario, if inspectors are always choosing trucks that have not received an inspection in a long time for inspections, then drivers who have recently received an inspection expect the re-inspection rate to be zero immediately following an inspection. Therefore, their private effort is e_0. Those drivers compose βn out of n drivers in total. The total number of crashes made by those drivers is

$$n\beta p(e_0, 0)L$$

There are $(1-\beta)n$ trucks that have not received an inspection recently. For them, the probability of getting an inspection is therefore $\frac{\beta}{1-\beta}$. Therefore, their private effort is e_1. The total number of crashes made by those drivers is

$$n(1-\beta)[\frac{\beta}{1-\beta}p(e_1, v(e_1))L + (1-\frac{\beta}{1-\beta})p(e_1, 0)L]$$

Adding up the number of crashes made by the two types of drivers, the total number of crashes is

$$n(1-\beta)[\frac{\beta}{1-\beta}p(e_1, v(e_1))L + (1-\frac{\beta}{1-\beta})p(e_1, 0)L] + n\beta p(e_0, 0)L \tag{21}$$

In the second scenario, the inspector chooses trucks randomly regardless of their recent inspection status, then all drivers face the same probability of getting an inspection, hence all of them exert private effort e_1. The total number of crashes is

$$n[\beta p(e_1, v(e_1))L + (1-\beta)p(e_1, 0)L] \tag{22}$$

Taking the difference between equation 21 and 22,

$$\begin{aligned}
&nL[\beta p(e_1, v(e_1)) + (1-2\beta)p(e_1, 0) + \beta p(e_0, 0) - \beta p(e_1, v(e_1)) - (1-\beta)p(e_1, 0)] \\
=&nL[(1-2\beta-1+\beta)p(e_1, 0) + \beta p(e_0, 0)] \\
=&nL\beta[p(e_0, 0) - p(e_1, 0)] > 0
\end{aligned}$$

From the assumption, we know that $\frac{\partial p}{\partial e} < 0$, since $e_0 < e_1$, $p(e_0, 0) - p(e_1, 0) > 0$. The difference between equation 21 and 22 is unambiguously positive given $e_0 < e_1$. The result implies that if driver does not expect to receive another inspection following an initial inspection, the driver's private effort e_0 is smaller than e_1, which is the private effort exerted if the driver knows that the rate of inspection is constant and does not depend on recent inspection status, then the total number of crashes in the society is larger. So we prove proposition 2.

$\square$