Competition and Misconduct in Certification Markets: A Case for Smog-Check Monopolies*

Nano Barahona
UC Berkeley and NBER

Juan-Pablo Montero
PUC-Chile and Aalto University

Pedro Skorin
MIT

April 1, 2026

Abstract: Vehicle inspections—commonly known as smog and safety checks—are often delegated to private agents, much like other quality certification markets. When these agents compete, they face incentives to misreport quality—especially when consumers do not internalize the external costs of misreporting. Theory and evidence from Chile's concentrated vehicle-inspection markets show that these incentives respond to a coordination problem so severe that it can only be resolved by delegating each market to a single agent. Doing so compromises neither service quality nor the ex-ante competition for the market, while delivering substantial and permanent reductions in vehicle emissions. An alternative to monopoly delegation is to allow multiple agents in the market while restricting consumer choice—assigning consumers to specific stations but permitting them to switch either by trading "location allowances" or by paying a "switching tax."

Keywords: competition, quality misreporting, smog checks, air pollution, coordination games.
JEL Classification: C72, D43, L51, Q58

*Emails: nanobk@berkeley.edu, jmontero@uc.cl, and pskorin@mit.edu. We are grateful to José Figueroa, J.D. Salas, and Nine von Dessauer for research assistance; Isaiah Andrews, Felipe Balmaceda, Ignacio Cuesta, Francisco Gallego, Andrés Gómez-Lobo, Larry Goulder, Tomas Guanziroli, Enrique Ide, Matti Liski, Juan Carlos Muñoz, Pauli Murto, and audiences at Aalto University, MIT, PUC-Chile, Stanford, TSE, UC Berkeley, UNAB, U. Andes, U. Montevideo, and U. Pacífico for helpful comments and discussions; and several officials at Chile's Ministry of Transportation and Telecommunications (MTT), in particular, Carola Jorquera, for access to many of the databases used in the study and comments. The opinions expressed in the paper, however, are not necessarily shared by the MTT. Montero also acknowledges financial support from Fondecyt grant No. 1250007 and a research fellowship from UNAB.

1. Introduction

Consumers base many of their decisions on third-party quality disclosures—when choosing a restaurant (Jin and Leslie, 2003), selecting a healthcare provider (Dranove et al., 2003), investing in corporate bonds (Bolton et al., 2012), or undergoing a vehicle inspection (Hubbard, 1998; Oliva, 2015). As noted by Dranove and Jin (2010), certifiers may have incentives to misreport quality—for example, by issuing fake certificates—especially when externalities are present, and consumers do not fully internalize the costs of misreporting. Vehicle inspections, commonly known as smog and safety checks, illustrate this problem. Drivers often care more about passing the inspection than about the true condition of their cars, making fake certificates privately valuable but socially costly.[1]

In many jurisdictions—including most U.S. states and numerous countries—vehicle inspections are delegated to private agents that compete to attract drivers, potentially amplifying incentives for misreporting, that is, for passing more cars than they should. The goal of this paper is to study these incentives using both theory and evidence from Chile's vehicle inspection markets.

According to our theory, duopoly markets exhibit strong strategic complementarities: in equilibrium, both firms either report truthfully or misreport, regardless of parameter values.[2] In principle, both firms should have every incentive to coordinate (or tacitly collude, if necessary) on truthful reporting, since they share a fixed pool of consumers in any case and misreporting entails an extra cost due to the risk of losing the concession to operate in the market.[3] Yet, the evidence tells a different story: misreporting emerges as soon as competition is introduced. These findings convey a clear policy message: the incentive problem is so severe that it can only be resolved by keeping the competition *for* the market but not *in* the market, that is, by delegating each market to a single agent.

Chile's vehicle inspection markets provide an ideal setting to study how market structure shapes misconduct. First, they consist of well-defined local markets with few competitors: about half of the more than 50 markets in our sample are served by a single firm, one-fourth by two firms, and the remainder by three or more firms. Second, entry into these markets is regulated by the government, generating sharp increases in competition over short periods of time. These features allow us to estimate how changes in market structure affect misreporting at different levels of competition, much like in Bresnahan and Reiss (1991).

[1] This is especially true for smog checks (emissions inspections) and less so for safety checks, where consumers may learn about mechanical problems that affect accident risk. Similar problems have been documented among industrial plants required to report emissions through third-party auditors (Duflo et al., 2013), and in the forestry and fishing industries, where firms seek certification of environmentally responsible practices (Isman, 2024).

[2] This strategic complementarity weakens as the number of firms increases, turning actions from strategic complements into substitutes. We observe a similar transition, for example, when the cost function becomes more convex in a pricing game (Vives, 2001) or when congestion rises in a bar (Karp et al., 2007).

[3] Coordination should be interpreted broadly, encompassing both the selection among multiple one-shot equilibria in a static setting and the sustaining of tacit collusion in a repeated-interaction setting.

Through competitive public tenders, the Ministry of Transportation and Telecommunications (MTT) determines the number of competitors in each market at any given time. These tenders also set the prices that winning firms are authorized to charge for inspections. Between 2013 and 2024, the MTT issued tenders for 59 concessions in our sample of markets, covering 129 inspection stations in total.[4] In most cases, these tenders were intended to renew existing concessions—whether awarded to incumbents or new entrants—without altering the number of competitors in the market. In 13 markets, or roughly 25% of our sample, however, the tenders were explicitly designed to increase the number of suppliers, primarily in response to growth in the vehicle fleet.

We estimate the effect of competition on misreporting by exploiting variation in market structure induced by stations' new entry through public tenders. We use data from over 15 million inspections of vehicles ten years and older—each identified by a unique, non-transferable license plate—to estimate a linear probability model of inspection approval.[5] Identification relies on within-vehicle and within-firm variation around the timing of market structure changes, using a staggered event-study design.

We find that, on average, the entry of a competitor increases the probability that a vehicle passes its overall inspection—including both smog and safety checks—at an incumbent firm by nearly 10 percentage points. Pass rates for smog and safety inspections each rise by about 5 percentage points. These are not small effects: before entry, the average smog check pass rate is 90%, so a 5-point increase implies that markets facing an additional competitor reject only half as many polluting cars as before. Although smaller, these increases in pass rates are already visible at least two quarters before entry—effectively known a year in advance—suggesting that firms begin building a reputation for "easy" passes to attract future customers (Dranove and Jin, 2010).

Since entry occurred across markets with varying market structures—from monopolies to markets with two or more competitors—we examine whether the increase in passing rates depends on the number of incumbents at the time of entry. We find that the effect is strongest in monopoly markets. Misreporting emerges sharply as soon as competition is introduced. Adding more competitors appears to increase misreporting further, but only marginally and at a decreasing rate. Consistent with our theory, we find some support for an inverted-U relationship between competition and misreporting: beyond a point, more competition may reduce misreporting as the demand-boost effect of misreporting is dominated by the cost-saving effect of truthful reporting, at least for one or more firms.

These findings appear to be in sharp contrast with existing studies of vehicle-inspection markets in the U.S., which find little evidence that competition affects misreporting. Using

[4]No firm can be awarded more than one concession per market. However, a firm may own multiple inspection stations within a market, and may also operate across multiple markets.

[5]Vehicles older than 10 years, for which misreporting can have a meaningful impact, represent up to 50% of the total fleet and account for more than 80% of its emissions. In many jurisdictions, these are the vehicles subject to annual inspections; newer vehicles are inspected less frequently and are sometimes exempt for the first few years. In any case, results are robust to including the entire fleet (see Online Appendix A, Figure A.8).

data from California, Hubbard (1998) shows that sharing a nine-digit ZIP code with one or more competitors increases the probability that a car passes inspection by only 1.8 percentage points—from 78.4% to 80.2% on average.[6] Similarly, Bennett et al. (2013), studying New York, find an even smaller effect: an additional competitor within 0.2 miles increases the passing probability by less than 0.1 percentage points, from a baseline rate of 93%. Our effects are much larger. Nonetheless, both their results and ours are fully consistent with our theory: once markets are sufficiently competitive, many firms engage in misreporting by passing too many cars, so adding further competition has little incremental effect. In other words, these U.S. studies capture already highly competitive environments and thus may fail to identify the effects of moving from highly concentrated markets—ideally monopolies—to less concentrated ones.[7]

Next, we examine whether higher approval rates have meaningful consequences for air quality and road safety—the two externalities that vehicle inspections are designed to regulate. Using hourly air pollution data from state-run monitoring stations, we employ a similar staggered event-study design to estimate the impact of misreporting—triggered by additional competition—on air quality.[8] We find that stations in markets experiencing the entry of a new competitor report increases in air pollution of about 20%. This provides strong evidence that vehicles failing their inspections undergo long-lasting repairs.[9]

Results are not nearly as conclusive when it comes to traffic accidents. Using a comprehensive database of police reports—which includes all police-reported accidents between 2015 and 2024, along with detailed information on vehicle license plates and possible causes (e.g., mechanical failures such as brake problems)—we find no evidence that additional competition affects accident rates. One interpretation is that consumers learn about their vehicle's condition even when a safety check passes the vehicle despite underlying mechanical issues, prompting them to make repairs later or drive more cautiously until the vehicle is fixed. Moreover, traffic accidents are influenced by a wide range of factors and conditions (Edlin and Karaca-Mandic, 2006), which likely reduces the relative importance of safety inspections in determining accident outcomes.

A valid concern with limited competition *in* the market is that consumers may experience a

[6] There are more than 3.4 million nine-digit ZIP codes in California, distributed across 58 counties, or roughly 60,000 per county on average.

[7] Indeed, when we apply the approach of Bennett et al. (2013) to Santiago—a market with multiple competitors—we find virtually no effect of competition on passing rates, mirroring their results for New York (see Online Appendix B). Yet there is clear evidence of misconduct in Chile: as illustrated in Figure A.3 of Online Appendix A, von Dessauer (2019) documents significant bunching in smog-check readings, with many readings clustered close to zero or just below the emission limits. Similar patterns appear in Hubbard (1998), who reports that rejection rates in California are more than twice as high in inspections conducted by state officials as in those by private firms—consistent with our findings if state inspectors behave like private monopolies. Oliva (2015) provides further evidence of misreporting in the highly competitive markets of Mexico City.

[8] We focus on fine particulate matter (PM2.5)—to which vehicles are a major contributor (Rivera et al., 2024; Rizzi and De La Maza, 2017)—because many of the monitoring stations located outside Santiago only keep records of it.

[9] We arrive at the same conclusion when examining whether a rejection today affects the probability of passing next year's inspection. Using data from cars inspected only in monopoly markets—to avoid switchers—we find that a rejection today increases the probability of passing the inspection the following year by 10 percentage points—a substantial effect, given that the unconditional passing rate is close to 90%.

decline in service quality. To explore this possibility, we exploit two data sources: vehicle inspection times and enforcement agents' detailed weekly reports documenting problems across a range of quality indicators, including inadequate cleaning, missing signage, temporarily closed inspection lines, and problems entering the station. Our staggered event-study design provides no evidence that increased competition improves these quality indicators, with the sole exception of inspection times, which decline by an average of 26%. It is difficult to determine, however, whether this reduction reflects a genuine improvement in service or instead less diligent inspections—consistent with the emergence of misconduct discussed above.[10]

A natural policy response to limit misreporting is stricter enforcement. In practice, however, enforcement is already intensive and becomes less effective when firms adopt similar noncompliance strategies (Alé-Chilet et al., 2025).[11] While imposing sufficiently large sanctions can, in theory, ensure honesty (Becker, 1968), there are practical limits to how severe such penalties can be (Cropper and Oates, 1992). In our context, it seems unlikely that any punishment stronger than revoking the concession could be credibly enforced. Both our theory and empirical evidence therefore point to delegating each market to a single operator as a compelling alternative.

We use our estimated parameters to conduct a back-of-the-envelope calculation quantifying the potential benefits of monopoly delegation for environmental outcomes. We find that, relative to the monopoly-delegation benchmark, the current market structure leads to permanently higher vehicle emissions—by 31.3% in Santiago and 18.6% in the rest of the country. Eliminating the smog-check program altogether would result in even larger increases in emissions, of 55.5% and 49.1% in Santiago and the rest of the country, respectively.

Many U.S. states—including Florida, Kentucky, Michigan, and South Carolina—do not require vehicle inspections. One possible explanation is that authorities in these states view inspections as having limited gains on air quality and traffic safety relative to their cost.[12] Combining benefit and cost estimates, we find that the benefit–cost ratio under the current program is 3.6 in Santiago and 2.6 in the rest of the country, whereas moving to monopoly delegation would raise this ratio to 5.9 in Santiago and 3.3 elsewhere.[13]

Another potential concern with monopoly delegation is that competition *for* the market may weaken once competition *in* the market is eliminated, leading public tenders to clear at higher

[10]Even if we attribute the entire decline in inspection times to faster service, value-of-time estimates from the transportation literature (Small et al., 2024) suggest that moving to monopoly delegation would raise consumers' inspection costs by less than 7%—about $3 (all currency in this paper is in 2023 U.S. dollars)—far too little to offset the substantial environmental benefits of monopoly delegation.

[11]As discussed by Alé-Chilet et al. (2025) in the context of compliance with vehicle manufacturing norms, the expected cost of misconduct is likely to decline because either responsibility becomes diffused, firms put "skin in the game," or detection is less likely, especially when enforcement agents cannot screen firms based on distinct passing rates.

[12]Support for this laissez-faire stance, at least regarding smog checks, comes from Sanders and Sandler (2020), who, using data from California, find that repairs of vehicles failing their initial inspections have no measurable impact on air pollution.

[13]The smaller gains from monopoly delegation outside Santiago reflect the fact that many of these markets are already served by monopolies.

prices. We find this unlikely for three reasons. First, using all bidding offers—both from winners and losers—in every public tender issued since 2013, we find that larger tenders attract more participants and yield lower bids, ruling out diseconomies of scale. Second, our theoretical model implies that if bidders anticipate an equilibrium in which all firms misreport ex post, their bids would be higher, not lower. Third, from a technological (or informational) standpoint, Anton and Yao (1992) show that under full information, monopoly delegation—i.e., a winner-take-all auction—is strictly superior for the government. Since the technology for inspecting cars is fairly standard, bidders are likely to be well informed about each other's costs. Moreover, in Chile and other jurisdictions, this information has been further reinforced by a long history of repeated bidding rounds.

An important factor that strengthens the case for monopoly delegation is that it is already in place in some jurisdictions, including Ireland, Portugal, and Spain.[14] However, if monopoly delegation is not politically feasible in markets large enough to support multiple competitors, an alternative is to allow more agents into the market while restricting consumer choice by assigning each consumer to a specific provider. To accommodate unanticipated location or preference shocks, consumers could be allowed to switch providers—either, following Coase (1960), by trading "location allowances," or, following Pigou (1920), by paying a "switching tax." In the absence of transaction costs, the former replicates the monopoly-delegation outcome (while the switching tax never does).

There is an extensive literature looking at the connection between competition and misconduct (Rose-Ackerman, 1975; Shleifer and Vishny, 1993; Ades and Di Tella, 1999; Shleifer, 2004; Thanassoulis, 2023). These studies generally show that competition can either discipline or exacerbate misconduct depending on the underlying incentives. Within vehicle-inspection markets, Hubbard (1998) and Bennett et al. (2013) find limited evidence that competition affects misreporting, suggesting that once markets are already competitive, additional entry has little effect. We contribute to this literature by showing that the relationship between competition and misconduct is highly nonlinear, as misreporting emerges sharply when markets transition from monopoly to duopoly. Our results provide a unified explanation for the small effects found in highly competitive U.S. markets and highlight monopoly delegation as a viable mechanism to restore truthful reporting.

We also contribute to a broader literature evaluating the effectiveness of environmental policies in reducing emissions—including emissions trading programs (Ellerman et al., 2000; Fowlie et al., 2012; Greenstone et al., 2025), regulatory inspections of industrial plants (Hanna and Oliva, 2010; Duflo et al., 2018), vehicle air-pollution standards (Greenstone and Hanna, 2014; Jacobsen et al.,

[14]Although the monopoly-delegation aspect is not always emphasized, Spain's vehicle inspection program has been cited as a model by international organizations such as the Inter-American Development Bank (IDB, 2023). Gómez et al. (2022) estimate the environmental and safety benefits of Spain's program in 2021 at $1,032 million and $577 million, respectively.

2023), driving restrictions (Davis, 2008; Gallego et al., 2013), and smog checks (Sanders and Sandler, 2020). While the latter conclude that smog checks in California have little effect on air quality, we show, using a different empirical design and data, that these inspections are an effective tool for containing emissions, and even more so when delegated to a single agent.

Finally, our paper touches on the tension between competition *for* and *in* the market, a theme raised more than 160 years ago by Chadwick (1859) in the context of public transport in Paris and London. He also advocated monopoly delegation, for reasons later formalized by Gómez-Lobo (2007). But the idea extends well beyond transport; for example, to the health insurance markets studied by Cuesta and Tebaldi (2025), where monopoly delegation limits inefficiencies from adverse selection but may reduce variety, whereas in our setting it mitigates misreporting but may come at the cost of lower service quality.

The rest of the paper is organized as follows. Section 2 presents the model. Sections 3 and 4 cover the empirical analysis, describing the data and the econometric results, respectively. We provide policy recommendations in Section 5, and conclude in Section 6.

2. A Model of Competition with Misreporting

We develop a simple yet sufficiently general model that captures key features of vehicle inspection markets, particularly the possibility that stations misreport vehicle quality by passing cars that should fail inspection. The framework applies equally well to both smog and safety checks. In addition to producing testable implications to be examined in Section 4, the model also serves to guide the policy analysis in Section 5.

To isolate the effect of competition on misreporting, we hold the market size fixed by keeping the vehicle fleet and the number of inspection lines, l, constant. These lines are established by the regulator and allocated evenly among $n \geq 2$ firms. Our goal is to understand how the equilibrium changes as the number of firms or stations varies, while keeping the installed inspection capacity constant. In other words, we vary only the number of independent firms owning and operating that capacity. We use the terms firm and station interchangeably.

2.1. The duopoly case

To start, consider two symmetric firms, each operating $l/2$ inspection lines under a common price cap $\bar{p}$, set in an earlier (i.e., procurement) stage and equal to the maximum price a station can charge for an inspection. We return at the end of the section to how this cap is determined.

There is a continuum of individuals of mass one, each owning a car that must be inspected. Cars differ in the expected cost of passing a proper inspection, denoted by θ, defined as the probability of failing the inspection times the corresponding repair cost. To facilitate the exposition, and without much loss of generality, we assume θ to be uniformly distributed over the unit interval.[15] Thus, an individual who takes her car θ to firm $i \in \{1, 2\}$ expects to obtain utility

$$u_{\theta i} = \theta m_i - p_i - \gamma q_i$$

where $m_i \in \{0, 1\}$ identifies whether the firm engages in misreporting ($m = 1$) or not ($m = 0$), $p_i \leq \bar{p}$ is the price charged by the firm for each inspection, $\gamma > 0$ captures the disutility of waiting in line for the inspection, and q_i is the fraction of individuals visiting one of the firm's inspection lines (visitors split evenly between the different firm's lines, so $Q_i = q_i l/2$ is i's total demand). Visiting a misreporting station yields expected savings of fixing the car equal to θ, which must be balanced against visiting a possibly less crowded station.

Firms decide simultaneously their prices and whether to misreport (M) or report truthfully (T). To simplify the exposition we normalize the variable cost of running an inspection to zero, so p_i can be interpreted as the markup over variable costs. Many years of evidence have shown that firms have no incentives to price below the cap, which is ensured by assuming that $\bar{p} \leq 2\gamma/l$.[16]

Consumers observe stations' decisions before they decide which one to visit. Thus, the payoff of a station that plays M when its rival also plays M is given by

$$\pi(M, M) = (\bar{p} - c)Q(M, M) = (\bar{p} - c)/2$$

where $Q(\cdot, \cdot)$ denotes the firm's total demand and $c \in (0, \bar{p})$ is the per-unit cost of cheating or misreporting. All consumers are indifferent between the two stations when they behave the same and charge the same price. The cost $cQ(M, M)$ can be interpreted as the probability of being caught misreporting times a penalty that is proportional to the damage inflicted. The latter, in turn, can be assumed proportional to the demand served, $Q(M, M)$, which is consistent with the maximum sanction that a non-compliant station can face: the revocation of its concession and the associated forgone revenues.[17]

If, on the other hand, both firms choose to report truthfully, the payoff of each is

$$\pi(T, T) = \bar{p}Q(T, T) = \bar{p}/2.$$

[15] In the proofs below, we let $\theta \in [0, \infty)$ be distributed according to a general cumulative distribution function, with the usual regularity properties. This general formulation also serves to accommodate the possibility that some individuals partially internalize their cars' externalities, which can be captured by reducing their actual values of θ.

[16] The MTT's website (`https://www.prt.cl/Paginas/TarifasyHorariosPRT.aspx`) contains the price caps for the 146 stations currently in operation. Stations' posted prices (on their own websites) match these caps for all but five stations, with deviations of 1–5%. We attribute these minor differences to pending updates rather than undercutting the cap.

[17] See Branco and Villas-Boas (2015) for a similar formulation of cheating costs. Note also that imposing sufficiently large sanctions *à la* Becker (1968) can ensure honesty, but, as documented by Cropper and Oates (1992), there is often a limit to how large such penalties can be in practice.

Consumers again are indifferent between the two inspection stations. Clearly, it is best for the two stations to report truthfully than to misreport. It is what a monopoly—owner of both stations—would do.[18]

Another possibility is that stations may specialize. Since misreporting can be interpreted as offering a product of higher (private) quality, although of lower social quality, one can imagine stations following different strategies in an effort to differentiate from each other, much in the spirit of Shaked and Sutton (1982). Note that observing specialization could be particularly helpful for government enforcement, as it enables officials to pay closer attention to markets with stations with distinct pass rates.[19]

In case stations follow different strategies, their payoffs are:

$$\pi(M,T) = (\bar{p} - c)Q(M,T) \text{ and } \pi(T,M) = \bar{p}Q(T,M),$$

respectively, where $Q(M,T) = 1 - Q(T,M) = 1 - \tilde{\theta}$, and $\tilde{\theta}$ is the consumer who is indifferent between visiting either station. From her indifference condition

$$0 - \gamma\frac{\tilde{\theta}}{l/2} - \bar{p} = \tilde{\theta} - \gamma\frac{1-\tilde{\theta}}{l/2} - \bar{p}$$

we obtain

$$\tilde{\theta} = \frac{2\gamma}{l + 4\gamma}. \tag{1}$$

From the payoffs $\pi(M,T)$ and $\pi(T,M)$, we see that playing M when the rival plays T involves a tradeoff: more demand but higher costs. A similar tradeoff arises when playing T while the rival plays M: lower costs but less demand. For specialization to arise in equilibrium, these tradeoffs must resolve in favor of the demand-boost effect of misreporting for one player and in favor of the cost-saving effect of truthful reporting for the other. Formally, the following two conditions must hold:

$$\pi(M,T) \geq \pi(T,T) \Leftrightarrow c \leq \bar{p}l/(2l + 4\gamma),$$

[18] One cannot exclude the possibility that a worker in a monopoly station might eventually, without the owner's knowledge, approve a failing car in exchange for a bribe. In our model, playing $m = 0$ is equivalent to the station's owner exerting effort to prevent such cheating, whereas $m = 1$ corresponds to exerting no such effort. In fact, our model can be rephrased as one in which workers in different stations compete for consumer bribes by setting their fees, while owners decide whether to exert effort to prevent this from happening.

[19] Since we are fundamentally interested in preventing stations from misconduct, we model it as a binary choice, that is, as an extensive-margin rather than an intensive-margin decision. Unlike with prices, consumers need to form binary perceptions about whether a station offers fake certificates or not. While it is true that some stations may misreport more than others, any station that chooses to engage in misreporting must differentiate itself sufficiently from honest counterparts for consumers to be aware of it. The same applies to quality differentiation more generally. Our empirical analysis tends to support this binary choice, although with some intensive-margin adjustments in the form of lower inspection times (here lower γ). From a pure modeling perspective, our binary assumption can be microfounded if the cost of misreporting c is independent of the extent of misreporting, in which case firms would exert the maximum extent that for simplicity we normalize to approving all cars, i.e., setting $m = 1$. Without loss of generality, this maximum amount could be any $m \in (0, 1]$. To accommodate intensive-margin adjustments, we could extend the model by allowing c to increase with inspection negligence aimed at reducing inspection times. See Online Appendix F for such extension.

and

$$\pi(T, M) \geq \pi(M, M) \Leftrightarrow c \geq \bar{p}l/(l + 4\gamma).$$

Put together, these conditions require

$$\bar{p}l/(l + 4\gamma) \leq c \leq \bar{p}l/(2l + 4\gamma)$$

which clearly cannot hold.

Proposition 1. *A specialization (pure-strategy) equilibrium in which one station misreports and the other reports truthfully cannot exist.*

Proofs of propositions are provided in Online Appendix E where appropriate. The proofs are written for a general distribution of types, extending the results beyond the uniform case. In Shaked and Sutton (1982), "quality" differentiation serves to soften price competition. Here, prices are kept at their cap level, so that mechanism is muted. Having discarded the specialization equilibrium, it is clear that both firms would be better off if both report truthfully. Unfortunately, this is not guaranteed.

Proposition 2. *Let* $\underline{c} \equiv \bar{p}l/2(l + 2\gamma)$ *and* $\bar{c} \equiv \bar{p}l/(l + 4\gamma) > \underline{c}$*. The equilibrium of the misreporting game depends on the cost of misreporting, c, as follows:*

(i) if $c > \underline{c}$*, it is an equilibrium for both firms to report truthfully (i.e., to play T);*

(ii) if $c < \bar{c}$*, it is an equilibrium for both firms to misreport (i.e., to play M); and*

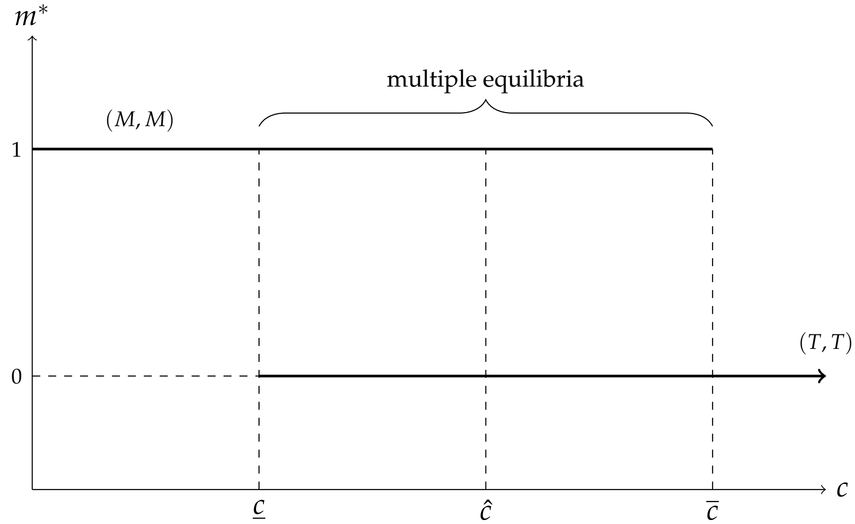
(iii) if $c \in [\underline{c}, \bar{c}]$*, there are two pure-strategy Nash equilibria—both stations misreport and both report truthfully—together with a mixed-strategy equilibrium.*

As shown in Figure 1, much of the misreporting game turns out to be a coordination game. Within the multiplicity region, if station $i \in \{1, 2\}$ plays $m \in \{M, T\}$, station $-i$ is better off playing m than $-m$. This strategic complementarity explains both the multiplicity of equilibria and the absence of specialization. Outside the multiplicity region, it is a dominant strategy to play M, when $c < \underline{c}$, or T, when $c > \bar{c}$. The reason misreporting becomes harder to prevent as $\bar{p}$ increases or γ/l decreases is intuitive: higher margins and easier-to-attract demand make the temptation to cheat stronger.[20]

An obvious solution to stop misreporting is to delegate all smog and safety checks to a single firm. Another option is to increase enforcement, ideally to a level where firms perceive the cost of cheating to be at least $\bar{c}$. However, this approach has clear limits, both in Chile and elsewhere. Despite frequent inspections—enforcement agents visit each station roughly once a week—detecting cheating is difficult because inspectors cannot directly observe test manipulation

[20] Note that what matters for demand elasticity is the ratio γ/l.

Figure 1: Multiple equilibria in the duopoly game



Notes. The figure summarizes the equilibrium outcome of the two-player misreporting game. Both firms either misreport (play M) or report truthfully (play T). There is a wide range of misreporting costs c that accepts both equilibria.

and firms often engage in similar practices simultaneously.[21] In Section 5 we discuss alternative policy mechanisms that can achieve outcomes comparable to monopoly delegation that do not require additional enforcement.

While the duopoly setting does not support a specialization equilibrium in pure strategies, it does support a specialization outcome within the multiplicity region if firms play mixed strategies. As illustrated by Harsanyi and Selten (1988) in an analogous setting, however, the mixed-strategy equilibrium in Proposition 2 is a poor predictor of how firms may actually play. It displays discontinuities around the thresholds $\underline{c}$ and $\bar{c}$ that defy intuition.[22] A way to address this problem is by adopting the concept of risk dominance introduced precisely by Harsanyi and Selten (1988). Risk dominance formalizes the idea that when players are uncertain about which equilibrium will prevail, they form expectations and coordinate on the equilibrium that entails the lowest strategic risk.

Proposition 3. *It is a risk-dominant strategy, as defined by Harsanyi and Selten (1988), for each station to misreport (i.e., play M) if $c < \hat{c} \equiv 2\bar{p}l/(3l + 8\gamma)$, and to report truthfully (i.e., play T) otherwise.*

[21] Alé-Chilet et al. (2025) argue that when firms adopt similar non-compliance strategies, the expected cost of cheating or misreporting is likely to fall—whether because responsibility is diffused, firms have "skin in the game," or the probability of detection is lower, particularly as enforcement agents are unable to screen firms with distinct pass rates. Formally, if we let the cost of misreporting be endogenous to firms' strategies—in particular, if $c(T, T) = c(T, M) = 0 < c(M, M) < c(M, T) = c$—then $\bar{c}$ would shift to the right, making the equilibrium (M, M) even more prevalent.

[22] For example, if c falls slightly below $\underline{c}$ it is a dominant strategy for firms to play M, but as soon as c goes slightly above $\underline{c}$, the mixed-strategy equilibrium predicts that firms play T with almost certainty. The inconsistency arises from the way the mixed-strategy equilibrium is constructed. For player i to make j indifferent between playing M and T when c is slightly above $\underline{c}$, i must place a high probability on playing T.

Finding the cutoff $\hat{c} \in (\underline{c}, \bar{c})$ in our symmetric game is relatively simple. At the cutoff, a firm is indifferent between playing T and M given that its rival is equally likely to play T than M.[23] Above the cutoff, a player finds it safer (or less risky) to play T than M given that its rival is also more likely to play T than M. Note that this "cutoff" strategy is not that different from the "switching" strategy of global games, pioneered by Carlsson and van Damme (1993).[24]

According to Propositions 1, 2, and 3, duopoly markets leave no room for specialization: in equilibrium, both firms follow the same strategy, either M or T. One implication is that, at the bidding stage, firms will submit identical bid prices, consistent with our assumption of equal price caps. A more fundamental implication of the absence of specialization is that firms should have every incentive to coordinate (or collude, if necessary) on truthful reporting, which is the Pareto-dominant outcome. Whether this no-specialization outcome extends to environments with more players, and how it is shaped by the interplay between the demand-boosting effect of misreporting and the cost-saving effect of truthful reporting, is something we explore in the next section.

2.2. Extending the misreporting game to more players

Consider now $n \geq 2$ symmetric firms competing in the market, each of them operating l/n inspection lines. Again, assume that all stations compete under the common price cap $\bar{p}$ and that they have no incentive to price below it, which is ensured by imposing $\bar{p} \leq n\gamma/l(n-1)$.

When all stations play T, the payoff of each of them is $\bar{p}/n$, and when they all play M is $(\bar{p} - c)/n$. Consider now the case in which $1 \leq k < n$ of the stations play M and the remaining $n-k$ play T. The payoff of each of the stations that play T is, with some abuse of notation, equal to

$$\pi(T, M^k, T^{n-k-1}) = \bar{p}\tilde{\theta}_k/(n-k) \tag{2}$$

where

$$\tilde{\theta}_k = \frac{\gamma n(n-k)}{lk(n-k) + \gamma n^2}$$

denotes the consumer who is indifferent as to which station to visit. Note that $\tilde{\theta}_k$ reduces to (1) for $n = 2$ and $k = 1$. On the other hand, the payoff of each of the $k \geq 1$ stations that play M is given by

$$\pi(M, M^{k-1}, T^{n-k}) = (\bar{p} - c)(1 - \tilde{\theta}_k)/k. \tag{3}$$

[23]Formally, playing T risk-dominates playing M if the Nash-product of the equilibrium (T, T) is greater than that of (M, M), that is, if $(\pi(T, T) - \pi(M, T))^2 > (\pi(M, M) - \pi(T, M))^2$, which reduces to $c > \hat{c}$. Note that if we allow the cost of misreporting to be endogenous to firms' strategies, the cutoff $\hat{c}$ in Figure 1 would also shift to the right, making the (M, M) equilibrium again more prevalent.

[24]Our misreporting game would have the exact same structure of the game in Carlsson and van Damme (1993) if we let c be imperfectly known to the stations, which only get a private (and independent) signal of its true value. In that game, a station would switch from playing M to playing T as soon as its private signal about the true value of c crosses the threshold $\hat{c}$. This close connection is not surprising, as explained by Morris and Shin (2003).

From these payoffs we can establish the following result.

Proposition 4. *Suppose there are $n \geq 2$ firms in the market:*

(i) if $c > \underline{c}(n) \equiv \bar{p}l(n-1)^2/n(l(n-1)+\gamma n)$, it is an equilibrium for all firms to report truthfully (i.e., to play T); and

(ii) if $c < \bar{c}(n) \equiv \bar{p}l(n-1)/(l(n-1)+\gamma n^2)$, it is an equilibrium for all firms to misreport (i.e., to play M).

There are several points to highlight in the proposition. The first, and much anticipated, is that $\underline{c}(n)$ increases in n, meaning that the truthful-reporting outcome—where all firms play T—becomes harder to sustain as the number of competitors grows. In theory, the truthful-reporting outcome could be sustained in equilibrium under a large number of competitors with a sufficiently high level of enforcement c, since $\lim_{n\to\infty} \underline{c}(n) = \bar{p}l/(l+\gamma) < \bar{p}$.[25]

The second point, probably less anticipated, is that $\bar{c}(n)$ decreases in n. Notably, $\lim_{n\to\infty} \bar{c}(n) = 0$, which means that in highly competitive environments one or more firms may prefer to report truthfully even if enforcement is very weak. When n is small, the demand-boost effect of misreporting (i.e., $1/n > \tilde{\theta}_{n-1}$) dominates the cost-saving effect of truthful reporting (i.e., $\bar{p} > \bar{p} - c$); but as n grows large, the demand-boost effect vanishes and so the cost-saving effect dominates.

The evolution of $\underline{c}(n)$ and $\bar{c}(n)$ as a function of n paints an interesting possibility of an inverted-U relationship between competition and the aggregate level of misreporting. At very low levels of competition, misreporting may be limited or absent; as the number of firms increases, misreporting can rise and reach a peak when all firms choose to misreport. Beyond that point, further increases in competition may again induce one or more firms to report truthfully. Interestingly, our empirical results show some of this inverted-U pattern.[26]

That $\bar{c}(n)$ decreases in n raises another issue. Since $\bar{c}(n)$ can fall below $\underline{c}(n)$ already at $n = 3$, the strategic complementarity (or the no-specialization) found in the duopoly setting does not necessarily extend to settings with more players. Strategic complementarity requires $\underline{c}(n) < \bar{c}(n)$, or equivalently

$$\gamma < \hat{\gamma}(n) \equiv \frac{l(n-1)}{n^2(n-2)} \tag{4}$$

The cutoff $\hat{\gamma}(n)$ may not even exist if it falls below $\bar{p}l(n-1)/n$—the no-undercutting pricing constraint—ruling out any strategic complementarity whatsoever.

[25] When c approaches $\bar{p}$, it becomes almost certain that any cheating will be detected and punished with concession revocation. Whether this level of enforcement is feasible—particularly regarding the severity of the punishment—is a practical matter.

[26] An example of an inverted-U relationship is illustrated below as the number of competitors increases from one to two to three.

Ours is not the first game in which actions may change from strategic complements to strategic substitutes as a function of some relevant parameter; for example, congestion in a bar (Karp et al., 2007) or the convexity of the cost function in a pricing game (Vives, 2001). In either case, attracting (resp. serving) demand becomes more difficult (resp. costlier). A similar logic applies to our misreporting game: The demand of a firm that follows its rivals' actions shrinks with higher n and/or γ/l. In any case, the risk-dominant equilibrium characterized in Proposition 3 for the duopoly case extends to more agents, but only if $\gamma < \hat{\gamma}(n)$, in which case it is a risk-dominant strategy for each station to play M when

$$c < \bar{p}\frac{ln(n-1)}{l(n^2-1)+2\gamma n^2} \equiv \hat{c}(n) \in (\underline{c}(n), \bar{c}(n))$$

and to play T otherwise.

The last point to highlight in Proposition 4 is that when $\bar{c}(n) \leq \underline{c}(n)$, the characterization of the equilibrium departs significantly from that of the duopoly setting. Specialization, where some firms play T while others play M, is very likely to arise. This certainly makes coordination (or collusion) in truthful reporting much more difficult to achieve. As an illustration, using eqs. (2) and (3), we characterize in Figure 2 the equilibrium for $n = 3$ across different values of $\gamma > \hat{\gamma}(3)$ and c.

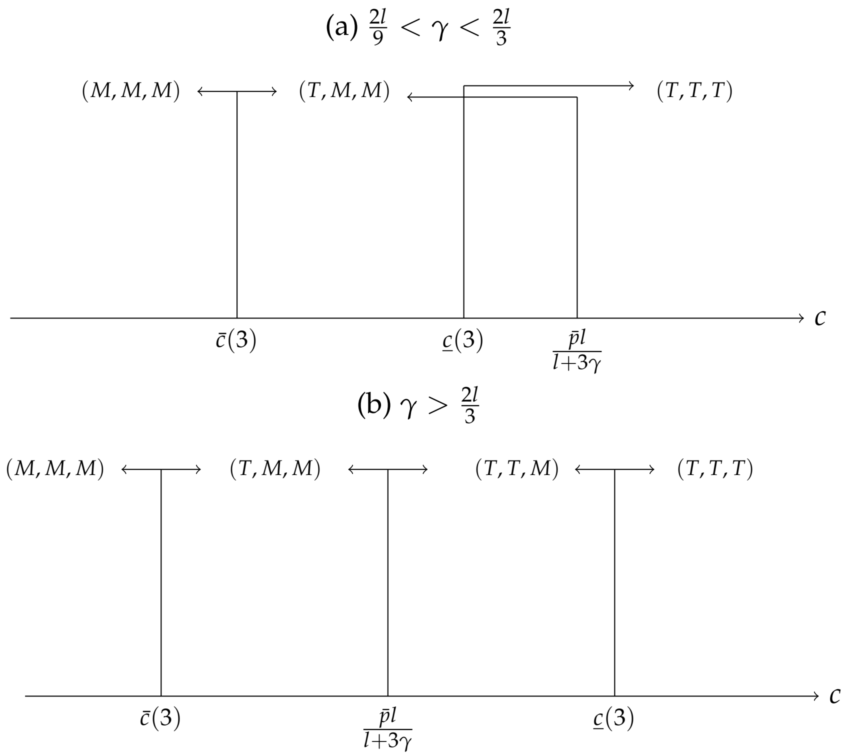
If $\gamma = \hat{\gamma}(3) = 2l/9$, the enforcement thresholds coincide, $\underline{c}(3) = \bar{c}(3)$, and the equilibrium is simple: all three stations play M whenever $c < \underline{c}(3)$, and all play T otherwise. However, if $\gamma > \hat{\gamma}(3)$ and $\bar{c}(3) = 2\bar{p}l/(2l+9\gamma) < 4\bar{p}l/(6l+9\gamma) = \underline{c}(3)$, specialization may arise in equilibrium depending on the cost of cheating. As depicted in the figure, (T, M, M) is an equilibrium for any $c \in [\bar{c}(3), \bar{p}l/(l+3\gamma)]$. In panel (a) of the figure, when $\gamma \in (2l/9, 2l/3)$, this specialization equilibrium coexists with (T, T, T) for any $c \in [\underline{c}(3), \bar{p}l/(l+3\gamma)]$. This multiplicity disappears in panel (b), as soon as $\gamma > 2l/3$, in which case the second specialization outcome, (T, T, M), may emerge in equilibrium. This happens when $c \in [\bar{p}l/(l+3\gamma), \underline{c}(3)]$.

Figure 2 also serves to illustrate the possibility of an inverted-U relationship between competition and misreporting. A monopolist ($n = 1$) always plays T. Now suppose that $\gamma > l$ and

$$c \in \left(\frac{2\bar{p}l}{2l+9\gamma}, \frac{2\bar{p}l}{3l+8\gamma}\right).$$

The upper bound implies that in the duopoly game ($n = 2$), misreporting is risk-dominant, so both firms play M. The lower bound, together with $\gamma > l$, places the three-player game in panel (b) of Figure 2, where (T, M, M) is the equilibrium. Hence, as the number of competitors increases from one to two to three, equilibrium misreporting first rises and then falls.

Figure 2: Equilibrium in a three-player market



Notes. The figure summarizes the different equilibria that can emerge in a three-player misreporting game as a function of the congestion parameter γ and the misreporting cost c. Panel (a) illustrates the possibility of multiple of equilibria when γ is sufficiently low and Panel (b) illustrates that such possibility vanishes as we consider higher values of γ.

2.3. Implications of the theory

Given the absence of specialization in duopoly markets, one of the implications of our theory is that firms in those markets should have every incentive to coordinate (or collude, if necessary) on truthful reporting, since they will be splitting a fixed pool of consumers in any case and misreporting entails an additional cost—–the risk of losing the concession to operate in the market. In that sense, there is hope—at least in theory—that truthful reporting could be sustained under competition, provided the number of firms in the market is sufficiently small. Ultimately, this is an empirical matter.

Our theory also has implications for what to expect in terms of how misreporting could evolve as the number of competitors in the market increases. If, on the one hand, we observe truthful reporting in duopoly markets, this evolution should be gradual, giving rise to an inverted-U relationship. Adding competitors should induce some firms to misreport. At some level of competition, all firms misreport. Beyond that point, further increases in competition may induce one or more firms to return to truthful reporting. This occurs when the demand-boost effect of misreporting becomes dominated by the cost-saving effect of truthful reporting for at least one firm, while the majority of firms continue to misreport.

If, on the other hand, we already observe misreporting in duopoly markets, the evolution should be slightly different. All firms continue to misreport until competition becomes sufficiently intense that at least one or more firms choose to return to truthful reporting, while the majority continue misreporting. We can recover the inverted-U relationship described above, but only if we extend our theory to allow for intensive-margin adjustments in the level of misreporting, not only extensive-margin adjustments as considered so far. This is done in Online Appendix F, where we allow the cheating cost c to increase with the misreporting effort used to boost demand (e.g., conducting faster and thus more negligent inspections). The extended model shows conditions under which misreporting—measured by higher pass rates—increases with the number of (misreporting) competitors, but at a decreasing rate. When competition is sufficiently intense, one or more firms adjust their extensive margins, returning to truthful reporting. In the following sections we take these implications to the data.

3. Setting and Data

Chile's vehicle inspection markets provide an ideal setting to study the relationship between competition and firm reporting behavior, and to examine the model's implications, for three reasons. First, they are well-defined markets with few competitors, particularly outside Santiago. Second, the substantial increase in the number of inspection stations over the last decade generates useful variation in market structure. Third, detailed inspection data allow us to track cars over time and control for potential selection into stations. In what follows, we first describe the regulatory framework governing these markets and then present the data sources, along with descriptive statistics, used in the empirical analysis.

3.1. Inspections and markets

As in many countries, all vehicles in Chile must undergo a technical inspection—a combination of smog and safety checks—at government-approved stations operated by private agents. Vehicles are classified as light or heavy, depending on their weight and use. Light vehicles include passenger cars, motorcycles, and small vans, while heavy vehicles include trucks, buses, and vehicles used for the commercial transport of goods and passengers. Light vehicles—exempt from inspection during the first two years after manufacture—must undergo inspection once a year, whereas heavy vehicles are required to be inspected twice a year.

Drivers are free to visit any station, with inspection prices ranging from \$12 to \$30 for light vehicles and from \$13 to \$47 for heavy vehicles (all currency in this paper is in 2023 U.S. dollars).[27]

[27]More details can be found at https://www.prt.cl/Paginas/TarifasyHorariosPRT.aspx.

The inspection process begins with a documentation check, during which station operators collect and record vehicle information. This is followed by a safety inspection that includes visual checks as well as tests of lights, brakes, steering, suspension, and overall structural integrity.[28] For vehicles powered by gasoline, diesel, compressed natural gas, or liquefied petroleum gas, the inspection concludes with a smog check to verify compliance with emission standards, which vary by vehicle type, model year, and pollutant. A car that fails to pass any of the tests has two weeks to return for a reinspection. A small fee may apply to cover the cost of some inputs other than labor used during the reinspection. We treat any inspection conducted after a rejection, whether at the same station or at another, as a reinspection.

Stations can only conduct vehicle inspections, following rigid, standardized procedures that leave little scope for differentiation beyond location. They are prohibited from engaging in any other economic activity, including vehicle repairs or the sale of auto parts. They are not even allowed to advertise in the media, run promotions, or display advertising materials of any kind.

Entry and exit in these markets are determined by the government. Entry occurs through public tenders for concession rights, organized by the Ministry of Transportation and Telecommunications (MTT). With few exceptions, concessions are granted for ten years. Participating firms submit both the technical details of their proposed project and the price to be charged for each inspection. Tenders are organized at the regional level, and each may cover one or more concessions.[29] Each concession, in turn, may comprise one or more inspection stations. For each station, the tender specifies the municipality and the number of operating lines required. Concessions often group stations in different municipalities—for example, combining one in a densely populated area with another in a more rural location.

When bidding, firms must propose a single uniform price to be applied across all stations within the same concession. To select the winner, the government first excludes bidders that fail to meet a minimum technical score. Among the remaining firms, the concession is awarded to the bidder offering the lowest price. However, no firm can be awarded more than one concession per tender within the same region. While a firm may operate several stations, the rules are designed to limit concentration: if a firm submits the lowest bid for multiple concessions in the same region, it is granted only the first, and the second is awarded to the next eligible bidder with the lowest price. The bid price, adjusted annually for inflation and other price indices, serves as a cap; however, according to the MTT, no firm has ever charged below this level.[30] After winning a tender, a firm has up to 24 months to begin operations, although extensions to this deadline may be granted.

There are several measures in place to ensure stations comply with existing norms when

[28] Inspection centers also perform noise tests, mainly on motorcycles and modified vehicles, and turning radius tests on heavy-duty and large vehicles.

[29] The country is divided into 16 regions, which constitute its first-level administrative divisions. The second level consists of 346 municipalities.

[30] See footnote 16.

carrying out the inspections. A large number of state enforcement agents visit stations on a weekly basis. These visits are unannounced, and agents are not only periodically evaluated but also regularly rotated to prevent the development of long-term relationships. During their visits, inspectors check that testing machines are properly calibrated and functioning, and that stations are clean, orderly, and with appropriate signage in place. In some cases, inspectors follow vehicles through the entire inspection process.

In the early 2010s, the MTT introduced an online monitoring system that automatically transmitted inspection data to it in real time. By 2015, 75% of stations were connected to the system, and by 2024, full adoption had been achieved. In addition to improving data reliability, the system provided detailed information, including specific reasons for rejection and raw emissions-test data. In case of misconduct, the MTT may impose sanctions ranging from warnings and fines to concession termination.[31]

Table 1 reports the number of stations, operating firms, inspections, and vehicles inspected in selected years for the entire country. Between 2015 and 2023, the number of inspected vehicles increased by 50%, reflecting steady growth in the vehicle fleet. To accommodate this increase, the MTT expanded the number of stations and operating lines through new procurement processes. By 2023, 33 firms owned 150 stations nationwide, 15% more than in 2015. These firms vary substantially in size, ranging from small local operators with a single station to international corporations managing more than ten stations across multiple regions.

Table 1: Evolution of vehicle inspections

Year	**2015**	**2019**	**2023**
Number of stations	130	147	150
Number of firms	29	31	33
Number of inspections	5,313,173	6,372,052	7,460,483
Number of vehicles	3,375,850	4,280,344	5,140,039

Notes. The table reports the number of stations, firms, inspections, and vehicles inspected in selected years.

Following Neilson (2025), we define a vehicle-inspection market as the aggregation of municipalities whose urban areas lie less than two kilometers apart. Urban areas are identified by the government as contiguous settlements with at least 2,000 inhabitants, basic infrastructure, and urban amenities. We exclude from our sample all municipalities in Santiago, Chile's capital, and home to more than seven million people.[32] Figure A.2 of Online Appendix A shows the geographical distribution of the more than 50 markets created under this definition. The

[31]Over the past ten years, the MTT has terminated three concessions for issuing certificates without inspecting vehicles, and another four for failing to commence operations by the mandated deadline. See Table A.4 of Online Appendix A for details.

[32]As Neilson (2025) also notes in his definition of school markets, defining relevant markets within such a large city is not straightforward. It possibly requires incorporating traffic patterns, as done in Houde (2012) for gasoline retail, for example.

distribution closely mirrors population density, with more markets in the central part of the country.

Panel A of Table 2 reports statistics for our sample of markets in 2015, 2019, and 2023, including the number of firms, stations, and inspected vehicles (Panel B reports the corresponding statistics for Santiago). The variables show an upward trend, including the number of competitors in some markets—variation that we exploit in our empirical analysis. Between 2013 and 2024, the MTT issued tenders for 59 concessions outside Santiago, covering a total of 129 inspection stations. In most cases, these tenders were intended to renew existing concessions—whether granted to incumbents or new firms—without changing the number of competitors in the market. In 13 of the 63 markets in our sample, however, the tenders were explicitly designed to increase the number of competitors. The number of markets varies over time due to the opening and closing of stations (monopolies) in more remote, rural areas of the country.

Table 2: Structure of vehicle inspection markets

Year	**2015**	**2019**	**2023**
Panel A: Markets outside Santiago			
Number of markets with one firm	30	33	38
Number of markets with two firms	16	12	13
Number of markets with three or more firms	3	9	8
Total number of markets	49	54	59
Number of firms	24	29	31
Number of stations	96	111	107
Number of vehicles	2,232,820	2,798,243	3,384,648
Median number of vehicles per market	31,293	34,066	39,662
10th percentile of vehicles per market	12,336	8,022	11,638
90th percentile of vehicles per market	100,000	113,666	137,609
Panel B: Santiago			
Number of firms	10	8	10
Number of stations	34	36	43
Number of vehicles	1,168,872	1,512,073	1,793,126

Notes. The table summarizes the structure of the vehicle inspection market for selected years. Panel A reports figures for markets outside Santiago, including the number of markets by active firms, as well as total firms, stations, and inspected vehicles. It also shows the median, 10th percentile, and 90th percentile of vehicles per market. Panel B presents aggregate figures for Santiago.

3.2. Data

To conduct our analysis, we assemble a comprehensive dataset that combines multiple administrative sources from the Chilean government. These sources cover the full chain of vehicle inspections, regulations, environmental outcomes, and road safety. In what follows, we describe the construction and scope of each dataset.

Vehicle inspections: We use administrative data from the MTT covering all vehicle inspections conducted nationwide between January 2015 and June 2024. The dataset contains more than 60 million records, each of which contains detailed information about the inspection, including the date and result, station identifier, vehicle's license plate, brand, model, year of manufacture, inspection time and emission readings. For most stations, it also reports the specific reason for rejection, when applicable. Because each car is identified throughout its lifetime by a unique, non-transferable license plate, we can reliably track its inspection history over time.

Procurement auctions: We use official records from the MTT on all government-run procurement auctions for vehicle-inspection concessions between 2013 and 2024. These data include detailed information on each concession, such as the number of operating lines, the municipalities served, the structure of the station network, the identity of all bidders, and the full set of submitted bids, identifying the winning bids.

Enforcement: We use monitoring data from the MTT covering all government audits of vehicle-inspection stations carried out between 2014 and 2025, comprising 76,255 reports. Each report includes the date of the visit, the station audited, and a free-text paragraph summarizing the inspector's observations. These narratives vary considerably in tone, style, and length. To systematically analyze them, we apply a large language model[33] to extract and classify content. The model classified observations into five groups: inspection line not in operation, access problems at the station, inadequate cleaning, missing signage, and malfunctioning machine.

Emissions: We use hourly records of fine particles (PM2.5) from the national air-quality monitoring network, operated by the Ministry of the Environment.[34] These monitors are located across a wide range of urban and suburban areas and provide consistent temporal coverage over the years. By matching monitoring data to inspection-station locations and dates, we examine how inspection-related behaviors—such as changes in pass rates or the entry of new firms—correlate with local pollution outcomes.

Road accidents: We use comprehensive administrative records from the national police on all reported traffic accidents between 2015 and 2024. Each record includes the time and location of the traffic accident, the number of vehicles involved, the number of injuries and deaths, the officially

[33]We use OpenAI's GPT-4o, accessed in May 2025, to identify emergent categories in the inspector narratives and classify each report accordingly.

[34]Like Rivera et al. (2024), our focus on PM2.5 responds to the fact that most monitoring stations outside Santiago do not keep records of other local pollutants such as carbon monoxide (CO) and nitrogen oxides (NO_x).

recorded cause, and the license plates of the vehicles involved.[35]

Table A.3 of Online Appendix A reports summary statistics for each of the datasets described above. The table includes the main variables used in the analysis, such as pass rates, auction bids, enforcement records, emissions across different regions of the country, and accident counts.

4. Empirical Analysis

In this section, we examine the main implications of the model developed in Section 2. We begin by analyzing the relationship between competition and misreporting. Next, we assess whether this relationship has meaningful consequences by studying its effects on air quality and road safety—the two externalities that vehicle inspections are designed to regulate. Finally, we investigate whether competition influences other margins of behavior, such as service quality and inspection time.

4.1. The effects of competition on passing rates

To study how competition shapes misreporting, we exploit within-station variation in the number of competitors over time, driven by exogenous firm entries into relatively concentrated markets. Because inspection procedures are standardized and license plates can be tracked across inspections, pass rates should remain constant once we control for vehicle characteristics—unless competition changes firm behavior. The wide range of market structures—with stations facing anywhere from none to more than six competitors—also allows us to test for potential non-linear effects of competition on pass rates.

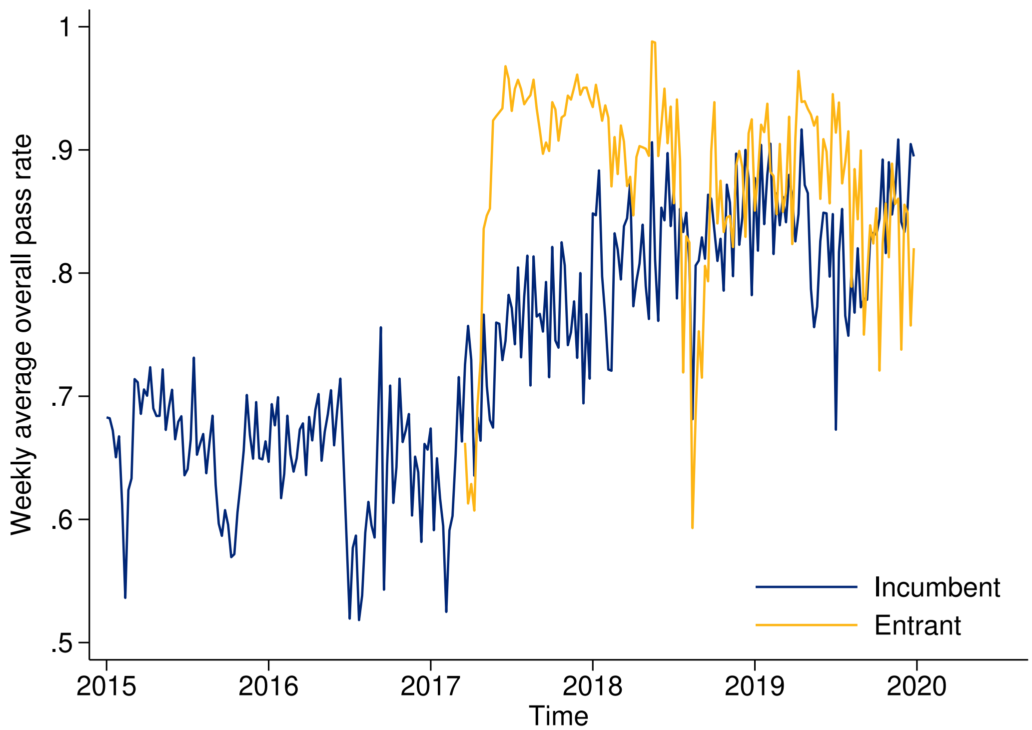
For the period between 2015 and 2024, we identify 13 markets in which the government authorized the entry of additional firms, thereby increasing the number of active competitors. Table A.2 of Online Appendix A lists these markets, with the number of firms before and after each change and the date when it happened, and Figure A.2 of the same appendix shows that these 13 markets are well distributed across the country. Our strategy is to analyze the evolution of pass rates at incumbent stations before and after these changes in market structure.

If stations follow standardized procedures and vehicle characteristics remain constant, pass rates should not change when a new competitor enters the market. Even if the composition of inspected vehicles shifts, comparing the same vehicle at the same station before and after entry provides a clean test. Under these assumptions, an increase in pass rates points to a behavioral response to competitive pressure. To illustrate our data, Figure 3 presents the evolution of pass rates in Vallenar, a small, isolated town north of Santiago. The figure shows raw weekly pass rates, without controlling for vehicle characteristics, before and after the entry of a new competitor.

[35] For accidents involving more than one vehicle, the data do not identify which vehicle was responsible. Thus, we can only observe who was involved and not who caused the accident.

Following entry, the incumbent's pass rates rose sharply, with both stations converging to a higher level.

Figure 3: An incumbent monopoly's reaction to competition



Notes. The figure plots the weekly average pass rate of first inspections in Vallenar, a small town 337 miles north of Santiago. To reduce noise, we exclude weeks with fewer than 50 inspected vehicles, which removes the entrant firm's first three weeks of operation.

To systematically quantify the effect of competition on pass rates, we estimate a linear probability model of vehicle approval outcomes, exploiting within-vehicle and within-station variation around the timing of market structure changes. Our econometric specification is a staggered event-study design centered on the entry of a new competitor into the market. We define quarter 0 as the period when a new competitor begins operating, and analyze a window of 8 quarters before and 12 quarters after the event.[36] This horizon covers nearly all treated markets for the full interval.

We estimate the following model:

$$y_{ijt} = \delta_i + \delta_j + \delta_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k\, z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \theta_{v(i,t)T(t)} + \phi_j \times t + \varepsilon_{ijt}, \quad (5)$$

where y_{ijt} is a binary indicator equal to 1 if vehicle i is approved during an inspection at station j

[36] The time between the announcement of a new tender and the actual start of operations is often two years. As a result, stations have a substantial window to adjust their pass rates around the time of the announcement, so there is no reason to expect an immediate response at the announcement date but rather closer at the actual entry date.

at month t. We include fixed effects for license plate, station, and month, denoted by δ_i, δ_j, and δ_t, respectively. License-plate fixed effects are central to the design, ensuring that changes in average pass rates are not driven by shifts in the composition of vehicles. We also include vintage-by-year fixed effects, $\theta_{v(i,t)T(t)}$, since older vehicles are more likely to fail inspections, where $v(i,t)$ is the vintage of vehicle i at time t and $T(t)$ is the year. Finally, we add station-specific linear trends, $\phi_j \times t$, to account for (i) potential machine deterioration over time, which could influence pass rates, and (ii) station-specific market dynamics not captured by standard time fixed effects.

The coefficients of interest are β_k, which trace the dynamic effect of increased competition on approval rates. The variable $z_{j,q(t)-k}$ is an indicator equal to 1 if quarter $q(t)$ is k quarters away from the actual entry of a new competitor in station j's market. To allow for potential anticipation effects, we normalize $k = -4$ (four quarters before entry) as the reference period, so that each β_k captures the change in pass rates relative to this baseline. The indicators $z_{j,q(t)\leq -9}$ and $z_{j,q(t)\geq 13}$ bin all periods more than eight quarters before and more than twelve quarters after entry, respectively. We cluster standard errors at the market level.[37]

We restrict the regression sample to first inspections, excluding re-inspections, which are mechanically tied to prior outcomes and may capture compliance efforts rather than initial station behavior. We also limit the sample to vehicles more than ten years old, since younger cars are either exempt or much less likely to fail inspection. We focus on inspections at incumbent stations in markets that experienced an increase in competition, explicitly excluding entrants from both the treatment and control groups. The control group consists of inspections from all stations in non-treated markets outside Santiago.

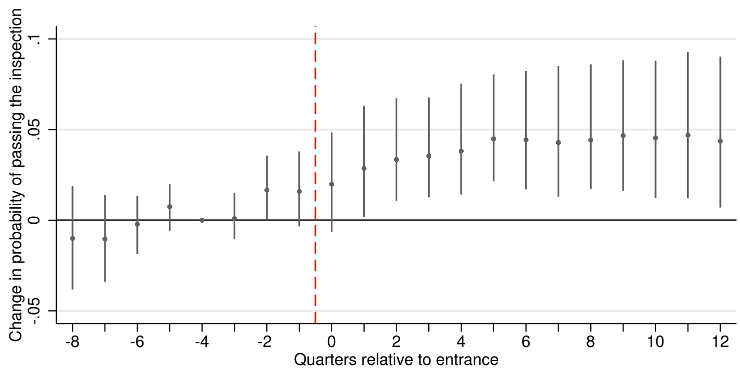
Figure 4 presents event-study estimates for smog checks, safety checks, and overall inspections. Panel (a) shows the effects for smog checks, where we estimate an average increase of 5 percentage points in pass rates following an increase in competition. Since, on average, vehicles in treated stations had a 90% pass rate one year before entry, a 5 percentage point increase is equivalent to a 50% reduction in smog-check rejections. The effect starts about two quarters before the actual entry and stabilizes roughly one year after entry. Panel (b) shows the effects for safety checks, where we estimate a similar 5 percentage point increase, also beginning two quarters before entry. Finally, Panel (c) presents the effects on overall inspections, showing a 10 percentage point increase in pass rates following entry.

As a robustness exercise, we also estimate eq. (5) including inspections from both incumbent and entrant stations in the treatment group. For this specification, we further explore heterogeneity by distinguishing between "loyals"—vehicles that stayed with an incumbent—and "switchers"—vehicles that moved to an entrant station at least once three years after the entrance. In both exercises, the control group consists of all stations in inspections from non-treated markets outside

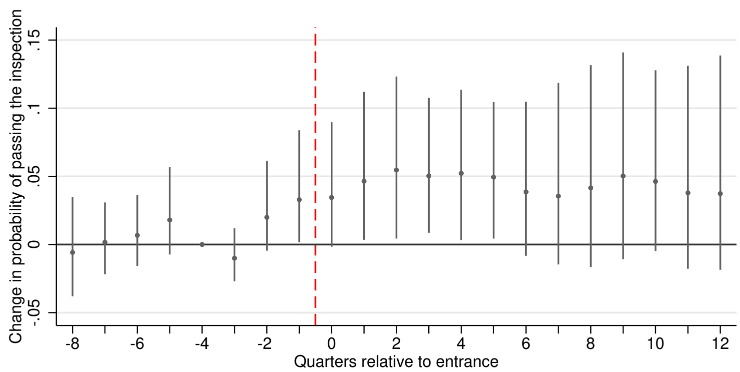
[37]The estimation sample includes 13 treated markets and 49 control markets. Because the number of treated clusters is relatively small, we conduct inference using wild cluster bootstrap procedures, which improve the finite-sample reliability of standard errors and test statistics in settings with few clusters (Cameron et al., 2008).

Figure 4: Competition effect on pass rates

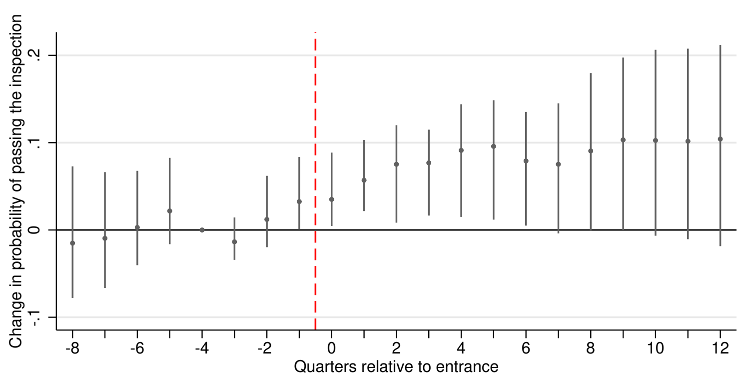
(a) Smog Checks



(b) Safety Checks



(c) Overall Inspection



Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago over vehicles manufactured at least 10 years before the inspection time, excluding entrant stations. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions.

Santiago. Figure A.4 of Online Appendix A presents the estimates when we include incumbent and entrant stations in the treatment group. The results are similar to the previous exercise, which is expected since controlling for station fixed effects should absorb the behavior of entrant stations.

We also examine whether there is heterogeneity between vehicles that continued inspecting at incumbent stations ("loyal" vehicles) and those that switched to the entrant ("switchers"). Figures A.5 and A.6 of Online Appendix A show the estimates treating loyals and switchers as the treated groups, respectively. The point estimates of both regressions are similar, though switchers exhibit wider confidence intervals. Because they account for only about 20% of vehicles in treated markets, the smaller sample size likely increases the estimation noise. Nonetheless, the similar effects for loyal and switching customers suggest that entrant firms behaved similarly to incumbents, providing little evidence of specialization in the market.

Finally, we report three alternative specifications in Online Appendix A. In Figure A.7, we present results using the same specification as in Equation 5, but excluding the station-specific linear trend component. In this case, the estimates are noticeably noisier and quantitatively smaller. This pattern suggests that, over a ten-year panel, a specification with only national time fixed effects may leave important station-level low-frequency variation unaccounted for, such as equipment deterioration or gradual changes in local conditions. In Figures A.9 and A.10, we re-estimate the baseline event-study using the Callaway and Sant'Anna (2021) and Sun and Abraham (2021) estimators, respectively. The estimated effects remain quantitatively similar to those in Figure 4, providing further support for the baseline findings.

4.1.1. How does the extent of competition affect misreporting?

Proposition 4 from Section 2 predicts that the likelihood of misreporting—i.e., the emergence of an all-M equilibrium—increases with the number of competing stations. An all-T equilibrium may still be sustained in a two-station game but may break down as competition intensifies. To test these predictions, we empirically examine how misreporting varies with the number of competing firms and whether misconduct emerges only once competition passes a certain threshold.

Unlike the previous subsection, where we analyzed competitors' entry as discrete events, here we regress pass rates directly on the number of firms in the market around entry. Controlling for station and vehicle fixed effects, we exploit within-station and within-vehicle variation generated by changes in the number of competitors, driven by the 13 entry events that increased market competition. We estimate effects separately by the number of competitors, allowing us to test whether moving from one to two firms may have a different impact than, for example, moving from two to three.

We estimate the following model:

$$y_{ijt} = \delta_i + \delta_j + \delta_t + \sum_k \beta_k \mathbb{1}\{\text{\# of competitors} = k\}_{jt} + \theta_{v(i,t)T(t)} + \phi_j \times t + \varepsilon_{ijt}, \quad (6)$$

where y_{ijt} is an indicator equal to 1 if vehicle i is approved at station j in quarter t. We include vehicle, station, and quarter fixed effects, as well as vintage-by-year fixed effects and station-specific linear trends. The coefficients of interest are β_k, which compare pass rates across different levels of competition k for the same vehicle at the same station. We normalize $k = 1$ (monopoly markets) as the reference category, so that each β_k captures the change in pass rates relative to monopoly markets. As in equation (5), standard errors are clustered at the market level.

Figure 5 reports the estimated values of β_k, which capture the causal effect of moving from monopoly to a k-firm market on pass rates. We find a large and statistically significant increase when markets transition from monopoly to duopoly. Beyond that point, the marginal effects of additional competitors are much smaller. There is even some support for an inverted-U relationship, as anticipated in Proposition 4. Nevertheless, much of what we find is reminiscent of Bresnahan and Reiss (1991), who document a sharp change in outcomes when moving from monopoly to duopoly, followed by more modest effects as more competitors enter.

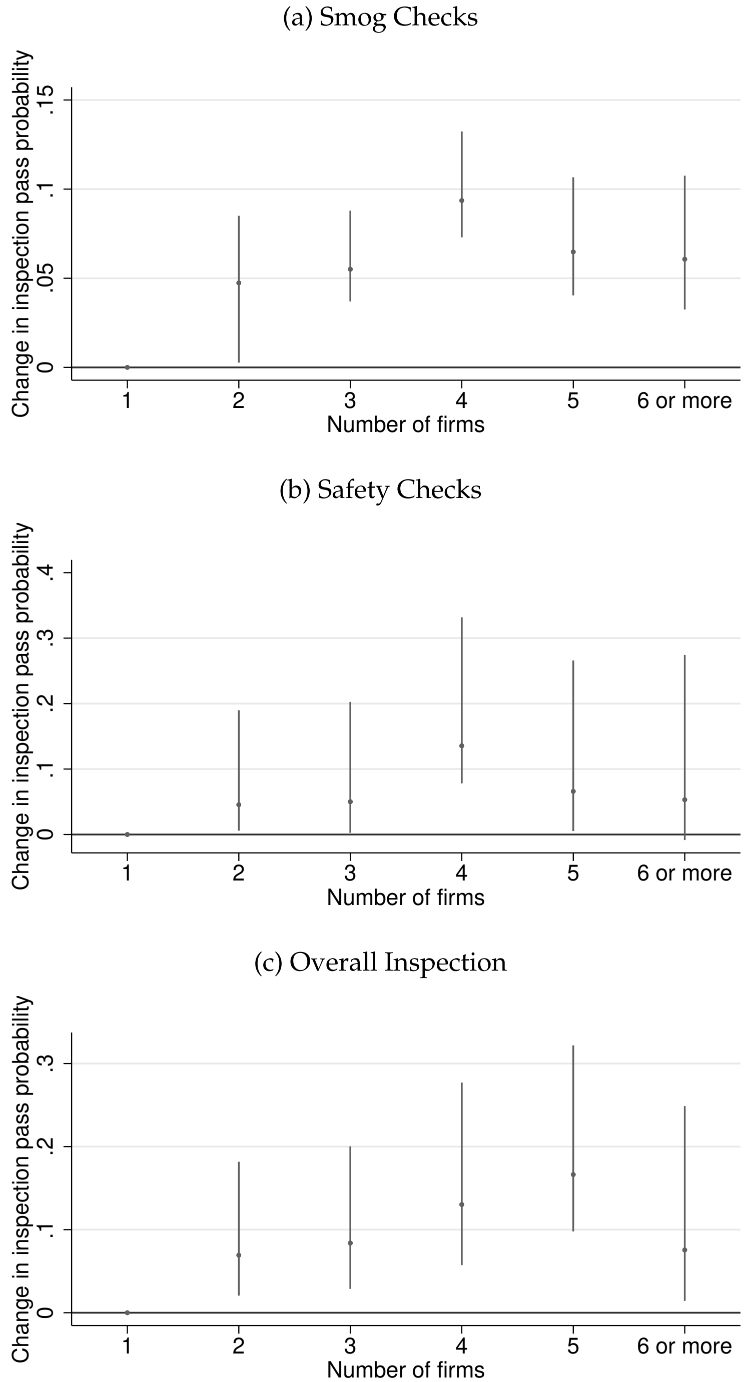
The limited effects of additional competition in markets with four or more competitors help explain why previous studies have found only small impacts of competition on pass rates. Drawing on evidence from California, Hubbard (1998) shows that when an inspection station shares its nine-digit zip code with at least one rival, the pass rate rises only slightly——by about 1.8 percentage points, from 78.4% to 80.2%. Similarly, using data from the state of New York, Bennett et al. (2013) find an even smaller effect: the presence of one more competitor within 0.2 miles increases pass rates by less than 0.1 percentage points, from an already high baseline of 93%. These geographic definitions may not capture the full extent of the relevant market, which may already be highly competitive. In such environments, additional entry is unlikely to generate meaningful changes in conduct.

To assess whether our results parallel those in these already highly competitive markets, we replicate the identification strategy of Bennett et al. (2013) for Santiago in Online Appendix B. Specifically, we examine the relationship between pass rates and the number of nearby stations within a given radius. Since a 0.2-mile range is too narrow in our context, we instead use three distances: 1, 2, and 3 kilometers. Figure B.1 of Online Appendix B, reports the estimated coefficients. The coefficient for the 1-kilometer radius is positive but statistically insignificant, while for the 2- and 3-kilometer radii we find no detectable effect of competition. Our estimates reject effects larger than 3 percentage points.

4.2. The effects of competition on air pollution and traffic accidents

We have shown that competition leads to higher pass rates, so a natural question is whether these shifts in passing behavior translate into meaningful impacts on the two outcomes that inspections are designed to regulate: local air pollution and road safety. We examine these impacts separately, using data on ambient concentrations of fine particulates (PM2.5) and on traffic

Figure 5: Fixed-effect model with number of competitors



Notes. The plot shows how different levels of competition affect smog check pass rates. We focus on first inspections conducted outside of Santiago for vehicles that are more than 10 years old at the time of inspection, excluding entrant stations. The bars represent 95% confidence intervals, calculated using wild bootstrap with 1,000 repetitions. We estimate that smog and safety check pass rates increase by 5 percentage points when moving from a monopoly to a duopoly.

accidents, respectively.[38]

[38]It is important to distinguish between local and global air pollution. Smog checks are particularly designed to regulate the former, not the latter, which is closely link to fuel efficiency. In addition to PM2.5, local pollutants include

4.2.1. Air pollution

To examine impacts on air quality, we adopt a similar identification strategy as the one used for pass rates. Specifically, we study how PM2.5 concentrations recorded by monitoring stations installed in different markets evolve with changes in competition. We estimate the effect of entry on logarithmic PM2.5 concentrations using the following event-study regression:

$$\ln(PM_{ijt}) = \gamma_{ij} + \gamma_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \phi_j \times t + \omega'_{ijt}\theta_j + \varepsilon_{ijt}, \quad (7)$$

where $\ln(PM_{ijt})$ denotes the logarithm of PM2.5 concentration at hour i, day t, and monitoring station j. We include hour-by-station fixed effects, γ_{ij}, and day fixed effects, γ_t, to account for systematic variation in pollution levels within stations and across calendar days. We also add station-specific linear trends, $\phi_j \times t$, to capture station-level changes over time. Finally, we control for weather conditions using the vector ω_{ijt}, which includes temperature, precipitation, wind speed, and wind direction measured at hour i, day t, and station j. The vector θ_j collects station-specific coefficients on these variables, allowing weather effects to vary across monitoring stations. This specification flexibly accounts for heterogeneous station-level responses to local meteorological conditions. The coefficients of interest are β_k, which trace the dynamic effect of increased competition on PM2.5 concentrations. The variable $z_{j,q(t)-k}$ is an indicator equal to 1 if quarter $q(t)$, the quarter of day t, is k quarters away from the entry of a new competitor in the market of monitoring station j. To allow for potential anticipation effects, we normalize $k = -4$ (four quarters before entry) as the reference period, so that each β_k captures the change in PM2.5 relative to this baseline. The indicators $z_{j,q(t)\leq -9}$ and $z_{j,q(t)\geq 13}$ bin all periods more than eight quarters before and more than twelve quarters after entry, respectively. We cluster standard errors at the market level.

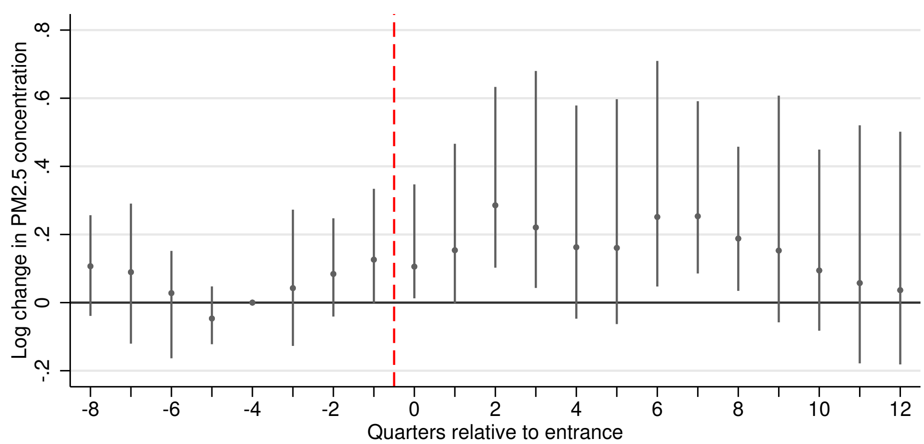
Before turning to the results, it is important to note that PM2.5 concentrations are the result not only of vehicle emissions but also of industrial activity and, most prominently in our context, of residential heating. In southern Chile, a large share of households rely on wood-burning stoves for heating, and as shown by Álamos et al. (2022), residential sources account for more than 95% of total PM2.5 emissions in much of southern cities. To address this concern, we exclude from the analysis all monitoring stations located south of the Biobío region. Figure A.11 of Online Appendix A, which uses 0.01 × 0.01 gridded emissions data from Álamos et al. (2022), supports this decision. The figure shows that the Biobío region serves as a clear threshold latitude, beyond which the vehicle-related share of PM2.5 falls sharply. This pattern indicates that, south of Biobío, vehicle emissions contribute only marginally to overall PM2.5 levels. Excluding southern Chile leaves an

carbon monoxide (CO), hydrocarbons (HC) and nitrogen oxides (NO_x). These local pollutants, unlike global pollutants such as carbon dioxide (CO_2), are characterized as having a local impact, at the city level, that lasts for a short time, sometimes only a few hours. The adverse health effects of these local pollutants are well documented (e.g., Currie and Neidell, 2005).

estimation sample of 17 markets, 7 of which saw an increase in the number of firms. For this analysis, we also include Santiago as a control group, given that it contains a large number of air-quality monitoring stations.

Figure 6 depicts the estimated β_k coefficients from equation (7). We find that PM2.5 concentrations increase by about 20% in markets that saw an increase in the number of competitors. The confidence intervals are relatively wide, which likely reflects heterogeneity in the contribution of vehicle emissions to overall pollution across markets. The effect appears to begin prior to period 0, consistent with the anticipation patterns observed in pass rates, suggesting that changes in inspection behavior may have started influencing local air quality before actual entry. After two years, the estimates lose statistical significance, with coefficients converging toward zero.[39]

Figure 6: The impact of competition on air quality



Notes. The plot shows the log change in PM2.5 concentrations across all regions north of the Biobío region. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions. The effect becomes statistically significant one quarter before the entry of competition and reaches an increase of about 20% over the following two years.

Based on the work of Sanders and Sandler (2020), one could have expected smog checks to have virtually no effect on air pollution.[40] Our results tell otherwise, that smog checks can lead to persistent reductions in vehicle emissions. In Figure C.1 of Online Appendix C we further test for this possibility by examining whether repairs following a rejection improve outcomes in subsequent inspections. We find that previously rejected vehicles are significantly more likely to pass later inspections, confirming that smog checks lead to long-lasting repairs rather than short-lived ones.

[39] An alternative explanation for the rise in PM2.5 is that higher pass rates generate more traffic by by attracting vehicles from elsewhere seeking an "easy" pass. This is unlikely for two reasons. First, as shown in Figure A.12 of Online Appendix A, we find no significant increase in the number of inspections in markets experiencing entry. And second, even if some vehicles came from other markets seeking an "easy" pass, the additional traffic this generates is negligible, given that vehicles are inspected only once a year.

[40] They find approved reinspections to affect air pollution only for very old vehicles—those from model year 1985 or earlier—which are virtually no longer seen on the road.

4.2.2. Traffic accidents

As with smog checks, safety checks also saw their pass rates systematically increase with competition. To study any impact on traffic accidents, we use a comprehensive database with all police-reported accidents between 2015 and 2024 following the same identification strategy used above. A key feature of our dataset is that it includes the license plates of all vehicles involved in each accident, so we can link each accident to the vehicle's most recent inspection within the two years prior to the incident. If the last inspection occurred more than two years before the accident, we assign a missing value. With this approach, we are able to match approximately 75% of accident records to a specific inspection station.[41] This matching procedure allows us to map accidents to specific inspection stations and, by extension, to specific markets.

To assess whether increased competition leads to more accidents, we adopt the same staggered event-study design used thus far. We aggregate accident outcomes at the market-week level and examine how different accident-related metrics evolve around the time of entry. Specifically, we analyze four outcomes: (i) the log of the total number of vehicles involved in accidents, (ii) an indicator for whether at least one accident in the market was attributed to mechanical or brake failure, (iii) an indicator for whether at least one accident resulted in serious injuries, and (iv) an indicator for whether at least one accident resulted in a fatality. Because the database does not identify which vehicle caused the accident, outcomes are defined in terms of whether at least one vehicle from the market was involved. If two vehicles from different markets are involved in the same accident, both markets are assigned that accident.

We estimate the following specification:

$$y_{jt} = \delta_j + \delta_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \phi_j \times t + \varepsilon_{jt}, \tag{8}$$

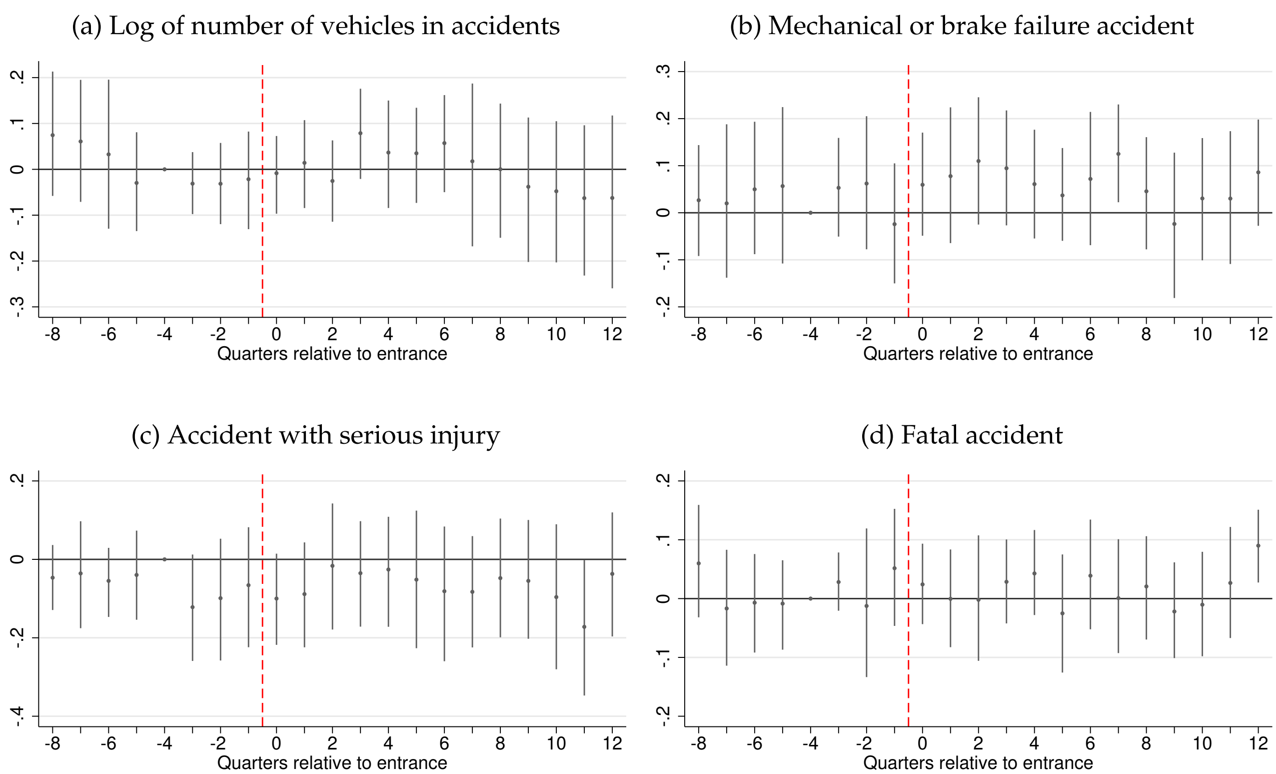
where y_{jt} represents the outcome of interest in market j at week t. We include market and time fixed effects, denoted by δ_j and δ_t, respectively, along with market-specific linear trends, $\phi_j \times t$. The coefficients of interest are β_k, which trace the dynamic effect of increased competition on each outcome. The variable $z_{j,q(t)-k}$ is an indicator equal to 1 if quarter $q(t)$, the quarter corresponding to time t, is k quarters away from the entry of a new competitor in market j. To allow for potential

[41] Three main factors explain the incomplete match. First, some mismatches result from misspellings or inconsistencies in license plates; for instance, certain entries contain fewer than the standard six characters, reflecting measurement error. Restricting the sample to six-character license plates raises the match rate to 77%. Second, vehicles less than two years old are not required to undergo inspection and thus cannot be matched. Restricting to vehicles older than two years with valid six-character plates increases the match rate further to 86%. Third, a share of vehicles on the road do not undergo inspections at all. Among vehicles older than two years, 14% could not be matched to an inspection record in the two years before their accident. This residual likely reflects both vehicles that were not inspected and remaining measurement error in the police reports or the matching procedure. For reference, the MTT estimates that roughly 10% of vehicles circulate without valid inspections. If non-compliant vehicles are more likely to be involved in accidents, the share of unmatched accident records could exceed the overall non-compliance rate, so a figure around 14% is not implausible.

anticipation effects, we normalize $k = -4$ (four quarters before entry) as the reference period, so that each β_k captures the change in accident outcomes relative to this baseline. The indicators $z_{j,q(t) \leq -9}$ and $z_{j,q(t) \geq 13}$ bin all periods more than eight quarters before and more than twelve quarters after entry, respectively. Standard errors are clustered at the market level.

We plot the estimated β_k coefficients in Figure 7. We find no systematic effects of increased competition on accident rates. Unlike in smog checks, consumers may care, at least to some extent, about their vehicle's safety. One interpretation of our results is that consumers learn about the condition of their cars even when a safety check passes the vehicle despite underlying mechanical issues, prompting them to make repairs later or drive more cautiously until the vehicle is fixed. Moreover, traffic accidents are influenced by a wide range of factors and conditions (Edlin and Karaca-Mandic, 2006), which may reduce the relative importance of safety inspections in determining accident outcomes.

Figure 7: The impact of competition on traffic accidents



Notes. The plots show the estimated coefficients of Equation (8) using different outcomes. In Panel (a), we show the change in the log of the total number of vehicles involved in accidents; in Panel (b), on an indicator for whether at least one accident in the market was attributed to mechanical or brake failure; in Panel (c), on an indicator for whether at least one accident resulted in serious injuries; and in Panel (d), on an indicator for whether at least one accident resulted in a fatality. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions.

4.3. The effects of competition on service quality

A natural concern with lack of competition is that consumers may experience a decline in service quality. We explore this possibility using enforcement agents' weekly reports, which document a range of quality issues, from inadequate cleaning and missing signage to temporarily closed inspection lines and problems entering the station. We estimate the following specification:

$$y_{jt} = \delta_j + \delta_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \phi_j \times t + \varepsilon_{jt}, \tag{9}$$

where y_{jt} represents a dummy equal to one if the enforcement agents' weekly report in station j at week t contained a complaint about one of the selected quality issues. Using a large language model, we classified the issues into five categories: inspection line not in operation, inadequate cleaning, missing signage, problem entering the station, and malfunctioning machine. We include station and time fixed effects, denoted by δ_j and δ_t, respectively, along with station-specific linear trends, $\phi_j \times t$. The coefficients of interest are β_k, which trace the dynamic effect of increased competition on each outcome. The variable $z_{j,q(t)-k}$ is an indicator equal to 1 if quarter $q(t)$, the quarter corresponding to time t, is k quarters away from the entry of a new competitor in market of station j. To allow for potential anticipation effects, we normalize $k = -4$ (four quarters before entry) as the reference period, so that each β_k captures the change in service quality outcomes relative to this baseline. The indicators $z_{j,q(t)\leq -9}$ and $z_{j,q(t)\geq 13}$ bin all periods more than eight quarters before and more than twelve quarters after entry, respectively. Standard errors are clustered at the market level.

As shown in Figure 8, which presents β_k for the five different quality categories, our event-study estimates provide no evidence that increased competition improves these quality indicators. If anything, competition appears to increase the probability of receiving a warning related to problems entering the station.

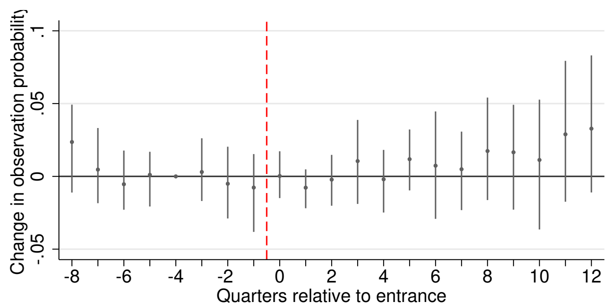
We also examine whether increased competition enhances service quality by reducing inspection turnaround times. To test that, we estimate

$$\ln(\tau_{jt}) = \delta_j + \delta_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \phi_j \times t + \varepsilon_{jt}, \tag{10}$$

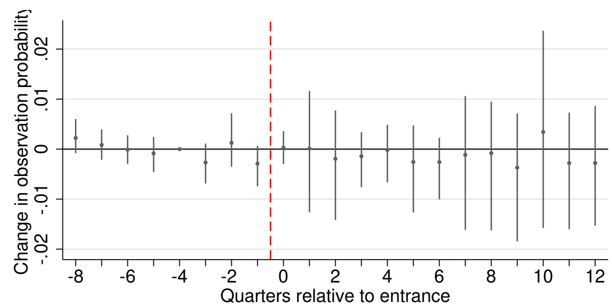
where $\ln(\tau_{jt})$ represents the log of inspection time in minutes. Following previous specifications, we include station and time fixed effects, denoted by δ_j and δ_t, respectively, along with station-specific linear trends, $\phi_j \times t$. The coefficients of interest are β_k, which trace the dynamic effect of increased competition on each outcome. The variable $z_{j,q(t)-k}$ is an indicator equal to 1 if quarter $q(t)$, the quarter corresponding to time t, is k quarters away from the entry of a new competitor in the market of station j. To allow for potential anticipation effects, we normalize $k = -4$ (four quarters before

Figure 8: Effect of competition on the probability of receiving an enforcement warning on different issues

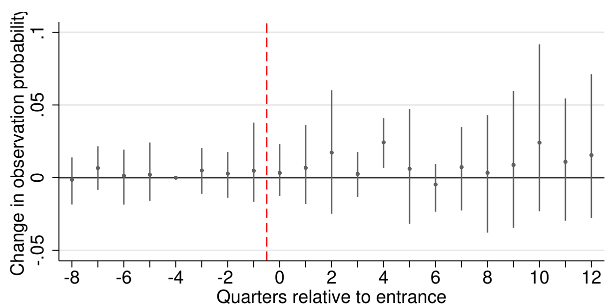
(a) Inspection line not in operation



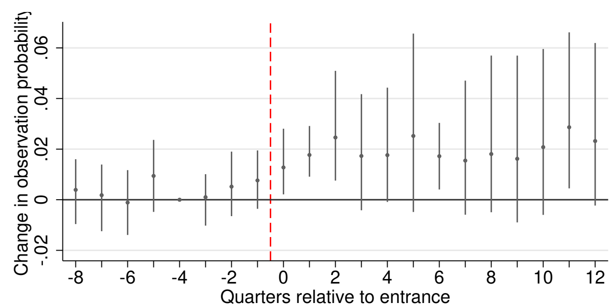
(b) Inadequate cleaning



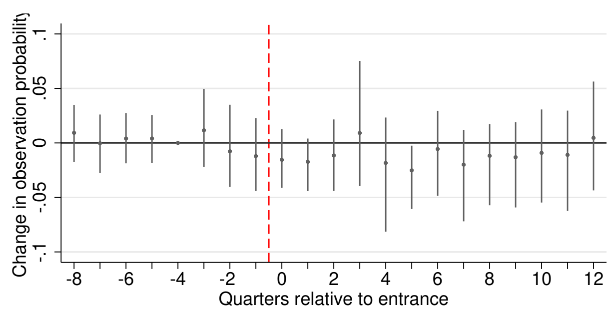
(c) Missing signage



(d) Problem entering the station



(e) Malfunctioning machine



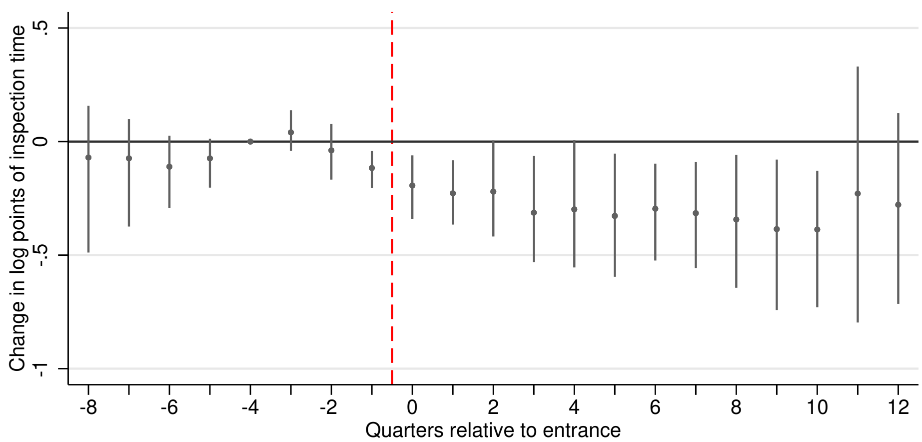
Notes. The plot shows the percentage-point change in the probability that a government inspector files an observation in a given week. Each panel focuses on a different issue. Panel (a) reports an inspection line not in operation; Panel (b), inadequate cleaning; Panel (c), missing signage; Panel (d), problem entering the station; and Panel (e), a malfunctioning machine. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions. For Panels (b), (c), (d), and (e), competition does not seem to significantly affect the probability that a station receives an observation. For plot (a), which reports observations about access problems, competition slightly increases the observation probability.

entry) as the reference period, so that each β_k captures the change in accident outcomes relative to this baseline. The indicators $z_{j,q(t)\leq-9}$ and $z_{j,q(t)\geq13}$ bin all periods more than eight quarters before and more than twelve quarters after entry, respectively. Standard errors are clustered at the market level.

We report the estimated coefficients in Figure 9. We find an average decrease of 26% in inspection times. Before the entry of a new competitor, the average inspection in treated markets lasted about 25 minutes, implying an average reduction of roughly 6.5 minutes. Consistent with the findings on passing rates, these results also provide evidence of anticipatory adjustments prior to entry.

However, it is difficult to determine from the figure whether the decline in inspection times reflects a genuine effort to improve service quality in response to greater competition or instead less diligent inspections, consistent with the misconduct documented earlier. In fact, declines in inspection times before entry actually occurs are consistent with either explanation. There is likely some of both effects at play. In any case, if we attribute the entire decline in inspection times to service improvement (as we do in the cost estimates of the following section), moving to monopoly delegation would raise drivers' inspection times by an equivalent of only \$3 per inspection.[42]

Figure 9: Effect of competition on inspection time



Notes. The plot depicts the effect of new entrants on log inspection time. The dashed red line marks the entry event, with the 4-quarter lag chosen as the baseline to account for potential anticipation effects. The analysis focuses exclusively on first inspections conducted outside Santiago. Vertical bars show 95% confidence intervals, computed using a wild bootstrap with 1,000 replications. Approximately one year after entry, log inspection time falls by 0.3, corresponding to a 26% reduction. Given that the average inspection time in treated markets was 25 minutes four quarters before entry, this translates into a decrease of about 6.5 minutes. The results also indicate evidence of anticipatory adjustments prior to entry.

5. Policy Implications

The previous analysis shows that increased competition in vehicle inspection markets can induce firms to misreport vehicle quality by passing cars that should fail their inspections. As a result, polluting vehicles remain on the road, raising overall emissions by roughly 20%. A natural policy

[42]See footnote 50 for details on the \$3 figure.

response to limit misreporting is stricter enforcement. In practice, however, enforcement is already intensive and becomes less effective when firms adopt similar noncompliance strategies (Alé-Chilet et al., 2025). While imposing sufficiently large sanctions can, in theory, ensure honesty (Becker, 1968), there are practical limits to how severe these penalties can be (Cropper and Oates, 1992). In our case, it seems unlikely that any sanction stronger than revoking the concession could be credibly enforced.

To curb misreporting, both our theory and evidence point to delegating each market to a single operator as a compelling alternative. In this section, we use our estimates to quantify the potential benefits of monopoly delegation for environmental externalities and consumer welfare.

5.1. How much can monopoly delegation reduce vehicle emissions?

To quantify the environmental externalities generated by competition, we combine our causal estimates of the impact of competition on pass rates with information on the vehicle fleet's age distribution, emission rates, and driving patterns. We divide vehicles into five age bins, $a \in \{1, 2, 3, 4, 5\}$, corresponding to 0–5 years, 5–10 years, 10–15 years, 15–20 years, and 20 years or older. Each bin exhibits distinct driving patterns, denoted by d_a, reflecting that newer vehicles tend to be driven more intensively.[43] On the supply side, inspection markets, denoted by j, are classified by the current number of active firms, with $m \in \{1, 2, 3, 4\}$ representing monopoly, duopoly, triopoly, and markets with four or more firms.

Let e_a^0 and e_a^1 represent the emissions rate of a vehicle in good and poor condition, respectively, with $e_a^1 > e_a^0$. Under *monopoly delegation*, all cars are assumed to be in good condition, meaning the condition currently observed in monopoly markets. Vehicles in poor condition are always rejected, repaired, and subsequently returned to good condition. Under this benchmark, the average emissions rate for age group a is simply

$$\bar{e}_{a,m}^{\text{monop}} = e_a^0 \tag{11}$$

regardless of the original market structure m.

Competition increases approval rates. For each (a, m) cell, we let $\Delta\theta_{am}$ denote the distortion in approvals induced by competition relative to the monopoly baseline. An additional fraction $\Delta\theta_{am}$ of the entire fleet is approved under competition, consisting of vehicles that should have been rejected and are instead allowed to remain on the road in poor condition. Then, the resulting average emissions rate in market m for age group a is

$$\bar{e}_{a,m}^{\text{comp}} \;=\; (1 - \Delta\theta_{am})\, e_a^0 \;+\; \Delta\theta_{am}\, e_a^1. \tag{12}$$

[43] We compute d_a using mileage records from inspections. Specifically, we track the same vehicle (identified by license plate) across different inspections, calculate its annual driving rate, and then average these values within each age group.

The gap between $\bar{e}^{\text{comp}}_{a,m}$ and $\bar{e}^{\text{monop}}_{a,m}$ captures the environmental externality of competition-driven misreporting relative to perfectly enforced inspections.

Finally, for comparison, we also consider a third counterfactual: *no smog checks*. In this case, emission rates are given by

$$\bar{e}^{\text{no}}_{a,m} \;=\; p_{am} e^0_a \;+\; (1 - p_{am}) e^1_a. \tag{13}$$

where p_{am} denotes the probability that a vehicle from age bin a in a market with structure m is in good condition, and $1 - p_{am}$ denotes the probability that is in poor condition. We quantify $\bar{e}^{\text{monop}}_{a,m}$, $\bar{e}^{\text{comp}}_{a,m}$, and $\bar{e}^{\text{no}}_{a,m}$ by estimating the key parameters $\Delta\theta_{am}$, p_{am}, e^0_a, and e^1_a. The distortion $\Delta\theta_{am}$ is obtained by estimating the regression model in equation (6), separately for each age group a, with results reported in Table 3.

Table 3: Competition effects by vehicle age ($\Delta\theta_{am}$)

Vehicle Age Group a	Duopoly ($m = 2$)	Triopoly ($m = 3$)	4+ firms ($m = 4$)
0–5 years	0.54	0.67	0.61
5–10 years	1.21	1.55	1.84
10–15 years	2.41	2.74	3.79
15–20 years	5.98	6.76	9.11
20+ years	5.62	6.82	9.51

Notes. Entries are estimated approval-rate changes $\Delta\theta_{am}$ (percentage points) from the same pass-rate regression in equation (6), measured relative to monopoly markets ($m = 1$), but filtering by vehicle age groups. In general, we find greater effects for older vehicles and for more concentrated markets.

To recover the underlying share of vehicles in good condition, p_{am}, we invert observed pass rates. Let p^{obs}_{am} denote the observed pass rate (on first inspections) in cell (a, m) under the current competitive regime. Since $\Delta\theta_{am}$ measures excess approvals induced by competition relative to the monopoly baseline, the implied approval probability absent misreporting is

$$p_{am} \;=\; p^{\text{obs}}_{am} - \Delta\theta_{am}. \tag{14}$$

For example, if $p^{\text{obs}}_{am} = 0.93$ and $\Delta\theta_{am} = 0.03$, the estimated share of vehicles in good condition is $p_{am} = 0.90$.[44]

Finally, to measure emission rates, e^0_a and e^1_a, we compute average ASM–2525 hydrocarbon (HC)

[44] This calculation has an important limitation: it relies on observed pass rates given the current fleet composition, and abstracts from behavioral responses in vehicle choice. In reality, eliminating inspections would likely increase the share of high-emitting vehicles, as consumers would face no incentive to purchase or maintain cleaner cars. Our estimates therefore understate the true environmental cost of removing the smog-check program, since they assume a fixed fleet relative to the current scenario.

readings under monopoly inspections, conditional on rejection status within each age group:[45]

$$e_a^0 = \mathbb{E}[\text{HC} \mid \text{approved}, a] \qquad e_a^1 = \mathbb{E}[\text{HC} \mid \text{rejected}, a]. \tag{15}$$

We compute total emissions separately for Santiago and the rest of the country. We take Santiago as a single market, already at the maximum level of competition (i.e., $m = 4$). For the rest of the country, we use each local market's 2023 competition status, $m_j \in \{1, 2, 3, 4\}$, and aggregate across markets and age groups. Formally, let N_{aj} be the number of vehicles of age group a in market j, d_a the average miles driven by a car of age group a, and let m_j be market j's 2023 competition level. We denote Santiago by $j = \text{SCL}$ and the set of markets in the rest of the country by RCL, so $j \in \text{RCL}$ denotes a market in that set. For a generic scenario $s \in \{\text{comp}, \text{monop}, \text{no}\}$, define

$$E^s_{\text{SCL}} \;=\; \sum_a N_{a,\text{SCL}}\, d_a\, \bar{e}^s_{a,\,m=4} \qquad E^s_{\text{RCL}} \;=\; \sum_{j \in \text{RCL}} \sum_a N_{aj}\, d_a\, \bar{e}^s_{a,\,m_j} \tag{16}$$

as total emissions in Santiago and the rest of the country, respectively.

Table 4 reports increases in expected emissions, relative to the monopoly-delegation benchmark, for two scenarios: the status quo and the termination of smog checks. Figures are larger in Santiago partly because several markets outside Santiago are already served by monopolies. Our results show that the regulator can achieve substantial reductions in emissions by delegating each market to a single operator.

Table 4: Vehicle emissions under different scenarios

	Current scenario vs. monopoly delegation	No smog-checks vs. monopoly delegation
Santiago	31.3%	55.5%
Rest of the country	18.6%	49.1%

Notes. Values report percent differences in expected emissions relative to the monopoly-delegation benchmark. Santiago is evaluated under high competition, while for the rest of the country we aggregate local markets using current structures with vehicle-weighted averages. We construct each counterfactual scenario following the methodology described in the text.

[45] HC are a very good proxy for (local) vehicle emissions. As explained by Barahona et al. (2020), HC are not only a key precursor of PM2.5 but they also account for the largest share (82%) of a vehicle's external polluting cost among the local pollutants measured at inspection, including carbon monoxide (CO) and nitrogen oxides (NO_x). An ASM–2525 reading measures a vehicle's exhaust emissions during an Acceleration Simulation Mode (ASM) test on a chassis dynamometer and is reported in our dataset. We focus on monopoly firms to minimize the risk of using distorted emission readings.

5.2. Do vehicle inspections pass the benefit-cost test?

Many U.S. states—including Florida, Kentucky, Michigan, and South Carolina—do not require vehicle inspections.[46] One possible explanation is that authorities in these states view inspections as having limited effects on air quality and traffic safety relative to their cost.

The analysis that follows focuses exclusively on smog checks, as safety checks appear less relevant in our analysis. To estimate the benefits of the smog-check program, we analyze Santiago (SCL) separately from the rest of the country (RCL). The harm from vehicle emissions in region $r \in \{\text{SCL}, \text{RCL}\}$ under scenario $s \in \{\text{comp}, \text{monop}, \text{no}\}$ can be expressed as

$$H^s_{\text{SCL}} = h_{\text{SCL}} E^s_{\text{SCL}} \;=\; k^s_{\text{SCL}} \sum_a N_{a,\text{SCL}}\, d_a \qquad H^s_{\text{RCL}} = h_{\text{RCL}} E^s_{\text{RCL}} \;=\; k^s_{\text{RCL}} \sum_{j \in \text{RCL}} \sum_a N_{aj}\, d_a\,, \tag{17}$$

where h_r is a constant that converts emissions into dollars and k^s_r is a function that captures the harm per mile of a representative car for a given scenario s.

We borrow the value of $k^{\text{comp}}_{\text{SCL}}$ from Rizzi and De La Maza (2017), who estimate it at ¢7.1 per mile. Plugging $k^{\text{comp}}_{\text{SCL}}$, $N_{a,SCL}$, and d_a into equation (17), we recover $H^{\text{comp}}_{\text{SCL}}$. Then, using the value of E^{comp}_s computed in equation (16), we estimate h_{SCL}.

Having estimated h_{SCL}, we compute H^s_{SCL} for the other two counterfactuals of interest, $s =$ no and $s =$ monop, using the corresponding values of E^s_{SCL} from equation (16). The benefit of maintaining the current smog-check program in Santiago, rather than eliminating it, is given by $H^{\text{no}}_{\text{SCL}} - H^{\text{comp}}_{\text{SCL}}$, while the additional benefit of transitioning from the current system to monopoly delegation equals $H^{\text{comp}}_{\text{SCL}} - H^{\text{monop}}_{\text{SCL}}$.

It remains to compute similar figures for the rest of the country. Rizzi and De La Maza (2017) do not provide an estimate of k^s_r (or h_r for that matter) for the rest of the country. They note, however, that h_r increases with both the number of people exposed to pollution and prevailing pollution levels. Thus, as a first approximation, variation in the product of these two factors should account for differences in the values of h_r across regions. According to this criteria, $h_{\text{RCL}} = 0.59 h_{\text{SCL}}$.[47] Combining the values of h_r with the emission figures that produced Table 4 and the vehicle numbers of Table 2, we can obtain environmental costs per vehicle-year under different smog-check scenarios. These costs are reported in the first column of Table 5.

On the other hand, the cost of implementing vehicle inspections comprises several components,

[46] Figure A.1 of Online Appendix A provides a map with information about each state.

[47] We approximate the exposed population as residents of municipalities with an air-quality monitoring station: 35 municipalities in Santiago (7.4 million people) and 37 municipalities in the rest of the country (5.7 million people). On the other hand, monitoring stations in Santiago report an annual average concentration of PM2.5 of 24.0 μg/m^3 versus 18.5 μg/m^3 in the rest of the country. From these numbers we obtain $0.59 = (5.7 \times 18.5)/(7.4 \times 24)$. Note that this number would remain largely unchanged if the population living in municipalities outside Santiago without monitoring stations (6.3 million)—while still subject to the smog-check program—were exposed to lower level of pollution, of around 10 μg/m^3 on average.

Table 5: Welfare analysis per vehicle

	Environmental Costs	Inspection, Repair, Time and Administrative Costs	Total Costs
Panel A: Santiago			
Status Quo	967.9	49.8	1017.7
Monopoly Delegation	737.3	69.2	806.5
No Smog-Checks	1146.4	0.0	1146.4
Panel B: Rest of the country			
Status Quo	550.9	53.8	604.7
Monopoly Delegation	464.3	68.5	532.8
No Smog-Checks	692.3	0.0	692.3

Notes. Entries report estimated welfare costs by scenario and region, expressed in 2023 U.S. dollars. Environmental Costs correspond to damages from emissions. Inspection, Repair, Time and Adm. Costs include operational, waiting time, repair expenditures and administration costs. Total Costs are the sum of the two components. Using the vehicle numbers of Table 2, the total environmental cost under no smog checks would be $2.1 billion in Santiago and $2.3 billion outside the capital.

including drivers' time, repair expenses, and the fixed and variable costs of performing inspections. We assume these costs are the same in Santiago as in the rest of the country. There is also an administrative cost borne by the MTT to organize and enforce the program. Although we lack direct information on these administrative costs, looking at MTT's available budget lines we estimate them at $2 per vehicle-year.[48]

Under the assumption of competitive prices in the public tenders, we set the cost of conducting each inspection at $20, corresponding to the national average inspection price. A fraction of vehicles require reinspection—whose frequency, as reported above, depends on market structure—and since reinspections cover only the items that failed in the initial test, we assume their cost equals half that of a full inspection.

Cars that fail their first inspection must also be repaired. Since the MTT does not record repair costs, we borrow estimates from California, as reported by Sanders and Sandler (2020). After adjusting their figures for inflation and accounting for differences in labor costs, we arrive at an average repair cost of $350 per vehicle.[49] In addition, as a conservative measure, we estimate that individuals in monopoly markets spend an average of two hours per inspection—one hour

[48]While not reported as a stand-alone budget line, these costs fall under "Programa 05" ("Fiscalización y Control") of the MTT's transportation secretariat, which totaled 14,579 million pesos in 2023 (about $17.4 million). Informally, we were told that roughly 60% of Programa 05 budget goes toward covering the administrative costs of running and enforcing smog and safety checks, implying a cost of $2.1 per vehicle-year.

[49]Labor costs account for roughly one-third of total repair expenses. According to the Federal Reserve Bank of St. Louis, average hourly earnings in California's manufacturing sector in 2024 are about $30.5 per hour. The equivalent average hourly earnings in Chile, based on data from the Central Bank of Chile, are roughly $5.6 per hour. The gap is smaller for low-skilled workers earning minimum wages—about 3.5 times higher in California than in Chile—so we adopt a factor of 4. This adjustment ensures that our repair cost estimate reflects local conditions.

traveling to and from the station, 30 minutes waiting, and 30 minutes for the inspection itself. Following standard practice in the transportation economics literature (e.g., Small et al., 2024), we value time at the average hourly wage of a representative car owner, approximately \$12, which implies \$24 in time costs per inspection.[50]

The overall costs of implementing these inspections under different smog-check scenarios are summarized in the second column of Table 5. These costs are small relative to their corresponding environmental gains. In fact, the benefit-cost ratio of the smog-check program in its current form is 3.6 in Santiago—the ratio of 1146.4 minus 967.9 over 49.8—and 2.6 in the rest of the country. Moving to monopoly delegation would raise this ratio to 5.9 in Santiago and to 3.3 elsewhere. Again, the smaller gains from monopoly delegation outside Santiago reflect the fact that many of its markets are already served by monopolies.

5.3. Alternatives to monopoly delegation

A potential concern with monopoly delegation is that competition *for* the market may weaken once competition *in* the market is eliminated, leading public tenders to clear at higher prices. We find this unlikely for three reasons. The first follows directly from our theory: if bidders anticipate a misconduct equilibrium ex post, their bids should be higher—not lower—because they would incorporate the expected costs of cheating.

The second reason is technological—or, more precisely, informational. According to Anton and Yao (1992), the use of split-award auctions—which in our context corresponds to dividing a market among two or more bidders—is justified only when bidders have limited information about each other's costs. Under full information, monopoly delegation, that is, a winner-take-all format, would be strictly superior for the government. The logic is that, with complete information, bidders can escape the Bertrand outcome in split-award auctions by non-cooperatively coordinating on higher bids for portions of the market.[51] The technology for vehicle inspections is relatively standardized, so we should expect bidders to be reasonably well informed about one another's costs. Moreover, in Chile and other jurisdictions, this information availability is further reinforced by a long history of public tenders and bidding data.

The third reason is empirical. Using all bids from both winners and losers in tenders since 2013, we find that larger tenders have attracted not only more participants but also lower bids, ruling out diseconomies of scale (see Online Appendix D). While some concessions have been awarded to local operators bidding for only part of a market, this occurs solely because, under current

[50] We impute the same two-hour time cost to reinspections, assuming that the additional trip to the repair shop is roughly offset by shorter inspection and queuing times. Furthermore, for the Status Quo scenario, we decrease the time cost at the station by 26% using estimates from Figure 9. This lowers total time costs per inspection by \$3, from \$24 to \$21 $(= 12 + (1 - 0.26) \times 12)$.

[51] Anton and Yao (1992) formally show this for dual sourcing with two bidders. It remains unclear how quickly this coordination effect weakens as the number of bidders increases.

allocation rules, a market cannot be fully assigned to a single provider even when it submits the most competitive offer. Under monopoly delegation, this restriction would no longer apply.

If, for any reason, monopoly delegation is not politically feasible in markets large enough to sustain two or more competitors, an alternative is to allow multiple agents to operate while restricting consumer choice—that is, by assigning each consumer to a specific firm.[52] In the duopoly setting of Section 2, where consumers are indifferent between stations—as long as both follow the same strategy—this alternative would replicate the monopoly-delegation outcome. In practice, however, even if consumers are assigned to their nearest stations, unforeseen factors may shift their preferences toward another provider. In such cases, allowing limited flexibility—so that consumers can switch from their originally designated station—can improve welfare. In Online Appendix G, we extend our model by introducing station-specific demand shocks.

To illustrate the extended model, suppose consumers are evenly split between the two stations. If the cost of cheating is sufficiently high, the planner can safely allow consumers to switch stations without inducing misreporting. When the cost is lower, however, allowing switching may prompt firms to misreport in order to attract drivers seeking higher pass rates. The planner could eliminate this problem by restricting switching altogether, but doing so may impose excessive costs on consumers. Allowing switching, on the other hand, introduces an externality—the risk of triggering misconduct. The planner therefore faces a mechanism design problem balancing consumer flexibility and enforcement integrity.

One can approach this problem from two classical angles: Coase (1960) and Pigou (1920). In general—absent transaction costs and assuming complete (aggregate) information on the planner's side—these two approaches are equivalent (Baumol and Oates, 1988). In our setting, however, they are not. Following Coase (1960) is equivalent to issuing "location allowances" to individuals, granting them the right to visit a specific station. Anyone wishing to switch stations would need to trade their location allowance with someone assigned to the other station. In the absence of transaction costs, this constitutes the optimal mechanism. A market for location allowances would emerge and clear at zero—or any positive—price, ensuring that exactly half of all individuals switch and that each station continues to serve half of the market, regardless of cheating costs.[53] In the absence of transaction costs, trading location allowances replicates the monopoly-delegation outcome.

If transaction costs are significant, the alternative is to follow Pigou (1920) and introduce a "switching tax" that internalizes the externality associated with potential misconduct.[54] In Online Appendix G, we show that there exists a tax level, $\tau(c)$, that ensures full honesty for a given cost of cheating c, while still granting consumers some flexibility to switch. Unlike location allowances, a

[52] According to officials at the MTT, restricting consumer choice is not straightforward to implement and could even be deemed unconstitutional unless it is clearly demonstrated that such a restriction yields sufficient public benefit.

[53] Prices would be exactly zero if a car exits the market after a fatal crash but its owner retains the location allowance.

[54] A switching tax strictly dominates the alternative of allowing a random fraction of consumers to switch freely.

switching tax never replicates the monopoly-delegation outcome.

6. Concluding Remarks

In this paper, we make a strong case for delegating each vehicle-inspection market to a single firm as a viable option to prevent firms from misreporting the environmental and safety qualities of the cars they inspect. This is remarkable because, according to our theory, duopoly markets exhibit strong strategic complementarities: in equilibrium both firms either report truthfully or misreport. Hence, firms should have every incentive to coordinate (or collude, if necessary) on truthful reporting, since they will be splitting a fixed pool of consumers anyway and misreporting carries an additional cost, the risk of losing the concession to operate in the market. Yet the evidence shows otherwise: misreporting emerges as soon as competition is introduced.

Our recommendation to delegate each market to a single provider is no different from what Chadwick (1859) proposed more than 160 years ago to improve public transport in London and Paris: keep the competition *for* the market but not *in* the market. A factor that should help advance the case for monopoly delegation is that it already exists in some jurisdictions, including Ireland, Portugal, and Spain (see Online Appendix A, Table A.1). Although the monopoly-delegation aspect is not explicitly highlighted, Spain's vehicle inspection program has been cited as a model to follow by international organizations such as the Inter-American Development Bank (IDB, 2023).[55]

If, for any reason, monopoly delegation is not politically feasible in markets with the scale to support two or more competitors, an alternative is to allow more firms into the market while restricting consumer choice to some extent—enough to prevent firms from engaging in misreporting. We have suggested ways to achieve this, such as through the trading of location allowances or the payment of a switching tax. The convenience and exact implementation of these mechanisms, including the number of firms in the market, require further analysis that goes beyond the scope of this paper. An application of these ideas to Santiago or another large city would be valuable.

References

Ades, Alberto and Rafael Di Tella, "Rents, Competition, and Corruption," *American Economic Review*, 1999, *89* (4), 982–993.

Álamos, Nicolás, Nicolás Huneeus, Mariel Opazo, Mauricio Osses, Sebastián Puja, Nicolás Pantoja, Hugo Denier van der Gon, Alejandra Schueftan, René Reyes, and Rubén Calvo,

[55] Gómez et al. (2022), for example, estimate the environmental and safety benefits of Spain's program in 2021 at $1,032 and $577 million, respectively.

"High-resolution Inventory of Atmospheric Emissions from Transport, Industrial, Energy, Mining and Residential Activities in Chile," *Earth System Science Data*, 2022, *14* (1), 361–379.

Alé-Chilet, Jorge, Cuicui Chen, Jing Li, and Mathias Reynaert, "Colluding Against Environmental Regulation," *The Review of Economic Studies*, 2025, p. rdaf024.

Anton, James J. and Dennis A. Yao, "Coordination in Split Award Auctions," *The Quarterly Journal of Economics*, 1992, *107* (2), 681–707.

Barahona, Nano, Francisco A. Gallego, and Juan-Pablo Montero, "Vintage-Specific Driving Restrictions," *The Review of Economic Studies*, 2020, *87* (4), 1646–1682.

Baumol, William J. and Wallace E. Oates, *The Theory of Environmental Policy*, Cambridge University Press, 1988.

Becker, Gary S., "Crime and Punishment: An Economic Approach," *Journal of Political Economy*, 1968, *76* (2), 169–217.

Bennett, Victor Manuel, Lamar Pierce, Jason A Snyder, and Michael W Toffel, "Customer-driven Misconduct: How Competition Corrupts Business Practices," *Management Science*, 2013, *59* (8), 1725–1742.

Bolton, Patrick, Xavier Freixas, and Joel Shapiro, "The Credit Ratings Game," *The Journal of Finance*, 2012, *67* (1), 85–111.

Branco, Fernando and J. Miguel Villas-Boas, "Competitive Vices," *Journal of Marketing Research*, 2015, *52* (6), 801–816.

Bresnahan, Timothy F. and Peter C. Reiss, "Entry and Competition in Concentrated Markets," *Journal of Political Economy*, 1991, *99* (5), 977–1009.

Callaway, Brantly and Pedro HC Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of econometrics*, 2021, *225* (2), 200–230.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-based Improvements for Inference with Clustered Errors," *The Review of Economics and Statistics*, 2008, *90* (3), 414–427.

Carlsson, Hans and Eric van Damme, "Global Games and Equilibrium Selection," *Econometrica*, 1993, *61* (5), 989–1018.

Chadwick, Edwin, "Results of Different Principles of Legislation and Administration in Europe; of Competition for the Field, as Compared with Competition within the Field, of Service," *Journal of the Statistical Society of London*, 1859, *22* (3), 381–420.

Coase, Ronald H., "The Problem of Social Cost," *The Journal of Law and Economics*, 1960, *3*, 1–44.

Cropper, Maureen L and Wallace E Oates, "Environmental Economics: A Survey," *Journal of Economic Literature*, 1992, *30* (2), 675–740.

Cuesta, José I. and Pietro Tebaldi, "Scoring Auctions vs. Managed Competition in Markets with Adverse Selection," *Mimeo*, 2025.

Currie, Janet and Matthew Neidell, "Air Pollution and Infant Health: What Can We Learn from California's Recent Experience?," *The Quarterly Journal of Economics*, 2005, *120* (3), 1003–1030.

Davis, Lucas W., "The Effect of Driving Restrictions on Air Quality in Mexico City," *Journal of Political Economy*, 2008, *116* (1), 38–81.

Dranove, David and Ginger Z. Jin, "Quality Disclosure and Certification: Theory and Practice," *Journal of Economic Literature*, December 2010, *48* (4), 935–63.

_ , Daniel Kessler, Mark McClellan, and Mark Satterthwaite, "Is More Information Better? The Effects of "Report Cards" on Health Care Providers," *Journal of Political Economy*, 2003, *111* (3), 555–588.

Duflo, Esther, Michael Greenstone, Rohini Pande, and Nicholas Ryan, "Truth-telling by Third-party Auditors and the Response of Polluting Firms: Experimental Evidence from India," *The Quarterly Journal of Economics*, 2013, *128* (4), 1499–1545.

_ , _ , _ , and _ , "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.

Ellerman, A. Denny, Paul Joskow, Richard Schmalensee, Juan-Pablo Montero, and Elizabeth Bailey, *Markets for Clean Air: The U.S. Acid Rain Program*, Cambridge University Press, Cambridge, UK., 2000.

Fowlie, Meredith, Stephen P. Holland, and Erin T. Mansur, "What Do Emissions Markets Deliver and to Whom? Evidence from Southern California's NOx Trading Program," *American Economic Review*, 2012, *102* (2), 965–993.

Gallego, Francisco, Juan-Pablo Montero, and Christian Salas, "The Effect of Transport Policies on Car Use: Evidence from Latin American Cities," *Journal of Public Economics*, 2013, *107*, 47–62.

Greenstone, Michael and Rema Hanna, "Environmental Regulations, Air and Water Pollution, and Infant Mortality in India," *American Economic Review*, 2014, *104* (10), 3038–3072.

—, **Rohini Pande, Nicholas Ryan, and Anant Sudarshan**, "Can Pollution Markets Work in Developing Countries? Experimental Evidence from India," *The Quarterly Journal of Economics*, 2025, *140* (2), 1003–1060.

Gómez, Ana María, José Luis San Román, and Vicente Diaz, "Contribución de la Inspección Técnica de Vehículos (ITV) a la Seguridad Vial y al Medioambiente," Technical Report ISBN 978-84-09-42580-8, Universidad Carlos III de Madrid 2022.

Gómez-Lobo, Andrés, "Why Competition Does Not Work in Urban Bus Markets: Some New Wheels for Some Old Ideas," *Journal of Transport Economics and Policy*, 2007, *41* (2), 283–308.

Hanna, Rema N. and Paulina Oliva, "The Impact of Inspections on Plant-level Air Emissions," *The BE Journal of Economic Analysis & Policy*, 2010, *10* (1), 1–29.

Harsanyi, John C. and Reinhard Selten, *A General Theory of Equilibrium Selection in Games*, The MIT Press, 1988.

Houde, Jean-François, "Spatial Differentiation and Vertical Mergers in Retail Markets for Gasoline," *American Economic Review*, 2012, *102* (5), 2147–82.

Hubbard, Thomas N., "An Empirical Examination of Moral Hazard in the Vehicle Inspection Market," *The RAND Journal of Economics*, 1998, pp. 406–426.

IDB, "Road Safety in Latin America and the Caribbean: After a Decade of Action Prospects for Safer Mobility," Technical Report IDB-MG-988, Inter-American Development Bank 2023.

Isman, Johanna, "Auditor Leniency and Participation in Voluntary Forest Certification." PhD dissertation, Toulouse School of Economics 2024.

Jacobsen, Mark R., James M. Sallee, Joseph S Shapiro, and Arthur A Van Benthem, "Regulating Untaxable Externalities: Are Vehicle Air Pollution Standards Effective and Efficient?," *The Quarterly Journal of Economics*, 2023, *138* (3), 1907–1976.

Jin, Ginger Z. and Phillip Leslie, "The Effect of Information on Product Quality: Evidence from Restaurant Hygiene Grade Cards," *The Quarterly Journal of Economics*, 2003, *118* (2), 409–451.

Karp, Larry, In Ho Lee, and Robin Mason, "A Global Game with Strategic Substitutes and Complements," *Games and Economic Behavior*, 2007, *60* (1), 155–175.

Morris, Stephen and Hyun Song Shin, *Global Games: Theory and Applications* Econometric Society Monographs, Cambridge University Press, 2003.

Neilson, Christopher A., "Targeted Vouchers, Competition among Schools, and the Academic Achievement of Poor Students," *Working Paper, Yale University*, 2025.

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

Pigou, Arthur C., *The Economics of Welfare*, MacMillan and Co., London., 1920.

Rivera, Nathaly M., J. Cristobal Ruiz-Tagle, and Elisheba Spiller, "The Health Benefits of Solar Power Generation: Evidence from Chile," *Journal of Environmental Economics and Management*, 2024, *126*, 102999.

Rizzi, Luis I. and Cristóbal De La Maza, "The External Costs of Private versus Public Road Transport in the Metropolitan Area of Santiago, Chile," *Transportation Research Part A: Policy and Practice*, 2017, *98*, 123–140.

Rose-Ackerman, Susan, "The Economics of Corruption," *Journal of Public Economics*, 1975, *4* (2), 187–203.

Sanders, Nicholas J. and Ryan Sandler, "Technology and the Effectiveness of Regulatory Programs over Time: Vehicle Emissions and Smog Checks with a Changing Fleet," *Journal of the Association of Environmental and Resource Economists*, 2020, *7* (3), 587–618.

Shaked, Avner and John Sutton, "Relaxing Price Competition Through Product Differentiation," *The Review of Economic Studies*, 1982, *49* (1), 3–13.

Shleifer, Andrei, "Does Competition Destroy Ethical Behavior?," *American Economic Review*, 2004, *94* (2), 414–418.

_ **and Robert W Vishny**, "Corruption," *The Quarterly Journal of Economics*, 1993, *108* (3), 599–617.

Small, Kenneth J., Erik T. Verhoef, and Robin Lindsey, *The Economics of Urban Transportation*, 3rd ed., Routledge, 2024.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of econometrics*, 2021, *225* (2), 175–199.

Thanassoulis, John, "Competition and Misconduct," *Journal of Finance*, 2023, *78* (4), 2277–2327.

Vives, Xavier, *Oligopoly Pricing: Old Ideas and New Tools*, The MIT Press, 2001.

von Dessauer, Cristine, "Behind the Mist of Corruption: A Case for Reconsidering Smog Checks," Master's thesis, Pontificia Universidad Catolica de Chile 2019.

Online Appendix for:

Competition and Misconduct in Certification Markets: A Case for Smog-Check Monopolies

(Not for publication)

Nano Barahona
UC Berkeley and NBER

Juan-Pablo Montero
PUC-Chile and Aalto University

Pedro Skorin
MIT

April 1, 2026

Appendix A: Additional tables and figures

Table A.1: Smog and safety checks around the world

Location	Form of entry	Market concentration	Type of checks	Pricing rule	Charges re-inspection?
California, U.S.	Accreditation	Competitive	Smog only	Unregulated	Unregulated
New York, U.S.	Accreditation	Competitive	Both	Ceiling (gov.)	No (under cond.)
Chile	Public tender	Monop. and comp.	Both	Tender price	No (under cond.)
Mexico	Public tender	Monop. and comp.	Smog only	Fixed (gov.)	No (under cond.)
Australia	Accreditation	Competitive	Safety only	Free pricing	Free to decide
Japan	Accreditation	Competitive	Both	Free pricing	Free to decide
Singapore	Public tender	Competitive	Both	Fixed (gov.)	Yes
Portugal	Public tender	Monopolies	Both	Fixed (gov.)	No (under cond.)
Spain	Public tender	Mostly monop.	Both	Fixed (gov.)	No (under cond.)
Ireland	Public tender	National monop.	Both	Tender price	No (under cond.)
France	Accreditation	Competitive	Both	Ceiling (gov.)	No (under cond.)
U.K.	Accreditation	Competitive	Both	Ceiling (gov.)	Free to decide
Germany	Accreditation	Monop. and comp.	Both	Regulated	Yes
Sweden	Accreditation	Competitive	Both	Ceiling (gov.)	Yes

Notes. The table compares the institutional design of vehicle inspection systems across selected countries in 2025, highlighting how entry rules, market structure, pricing, and reinspection policies differ. "Public tender" indicates that firms obtain market rights through government bidding, while "Accreditation" allows qualified stations to enter freely. "Monop." (monopoly) means one operator per market, whereas "Comp." (competitive) denotes multiple stations. "Both" refers to systems that combine smog and safety checks. "Ceiling (gov.)" and "Fixed (gov.)" indicate government-regulated prices; "Tender price" is the winning bid from the tender, and "Free pricing" allows stations to set their own fees. "No (under cond.)" means reinspections are free under certain conditions (e.g., same station or short return period), while "Yes" indicates that a reinspection fee is always charged.

Table A.2: Markets that experienced entry of new firms

Market	**Firms before entry**	**Firms after entry**	**Entry date(s)**
Los Andes	1	2	July 2019
Vallenar	1	2	March 2017
Coquimbo	2	3	April 2016
Copiapó	2	3	February 2017
Iquique	2	3	March 2021
Talca	2	3	March 2021
Temuco	2	3	January 2017
Puerto Montt	2	3	April 2017
Rancagua	2	3	February 2018
Valdivia	2	3	December 2016
Valparaíso	2	3-8*	August 2018
Concepción	3	5	February 2022
Los Angeles	3	5	June 2018

Notes. The table lists markets that transitioned from one or few competitors to having one or more additional competitors. "Firms before entry" and "Firms after entry" refer to the number of active stations immediately before and after the indicated entry date(s). In Valparaíso, multiple entry events occurred starting in August 2018, and by 2024 the market included eight active firms. For the event-study analysis, we focus on the first entry observed in each market.

Table A.3: Descriptive statistics

Variable	(1) N	(2) Mean	(3) SD	(4) p10	(5) p50	(6) p90
A. Vehicular inspections						
Smog inspection result	30,613,309	0.94	0.23	1	1	1
Safety inspection result	30,613,309	0.79	0.41	0	1	1
Overall inspection result	30,641,014	0.74	0.44	0	1	1
Year vintage	30,543,522	11.53	8.25	3	10	23
Inspection time	27,525,119	22.03	38.77	6	14	39
B. Procurement auctions						
Number of comunas attended	59	2.19	0.43	2	2	3
Number of bidders	59	2.86	1.24	1	3	4
Winner bid (in 2023 USD)	59	18.26	4.38	14	18	26
Number of lines (per station)	129	2.90	0.84	2	3	4
C. Enforcement						
Type 1 observation per visit	73,935	0.022	0.15	0	0	0
Type 2 observation per visit	73,935	0.013	0.11	0	0	0
Type 3 observation per visit	73,935	0.003	0.06	0	0	0
Type 4 observation per visit	73,935	0.016	0.13	0	0	0
Type 5 observation per visit	73,935	0.049	0.22	0	0	0
Number of visits per month	16,032	4.61	2.29	2	4	7
D. Emissions						
PM2.5 concentration	4,597,102	24.19	39.65	4	14	50.5
PM2.5 concentration in the North	422,673	11.33	7.96	4	10	20
PM2.5 concentration in the Center	2,574,030	24.22	27.49	5	16	51
PM2.5 concentration in the South	1,600,399	27.54	56.83	3	11	61
E. Road accidents						
Number of vehicles in accidents per week	29,153	28.28	74.96	2	11	53
Mechanical or brakes failure accident	29,153	0.22	0.41	0	0	1
Accident with serious injuries	29,153	0.39	0.49	0	0	1
Fatal accident	29,153	0.20	0.68	0	0	1

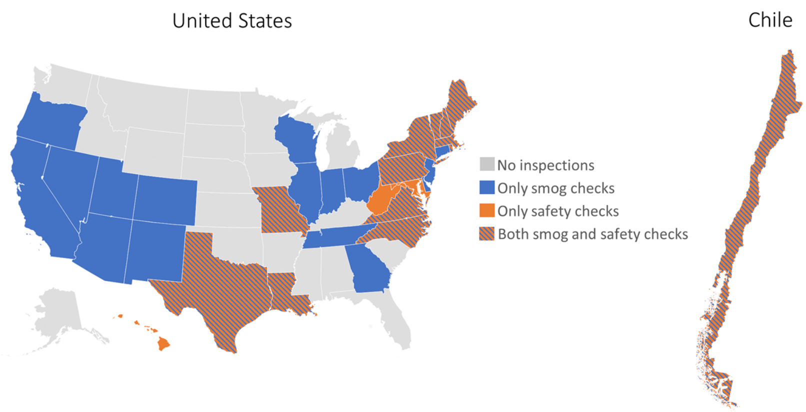
Notes. Panel A reports descriptive statistics for first inspections only, excluding reinspections and the Santiago market. Panel B presents variables related to the concession tender process. Panel C summarizes enforcement data, where the model classified each inspection visit into five mutually exclusive categories: (1) inspection line not in operation, (2) access problems at the station, (3) inadequate cleaning, (4) missing signage, and (5) malfunctioning machine. Panel D groups observations into macrozones following the classification established by the *Ministerio del Medio Ambiente* (2024) resolution. Finally, Panel E reports road-accident variables aggregated at the market-week level; variables *Mechanical or brakes failure accident*, *Accident with serious injuries*, and *Fatal accident* represent dummies of whether any accident involved the specified content at the weekly level.

Table A.4: Summary of concession revoked, 2015–2024

Year	**Region**	**Reason for Concession Revocation**
2017	Los Lagos	Did not start operations within the deadline
2017	Santiago	Did not start operations within the deadline
2018	Ñuble	Did not start operations within the deadline
2019	Atacama	Issued certificates without inspection
2022	Araucanía	Issued certificates without inspection
2023	Biobío	Did not start operations within the deadline
2023	Maule	Issued certificates without inspection

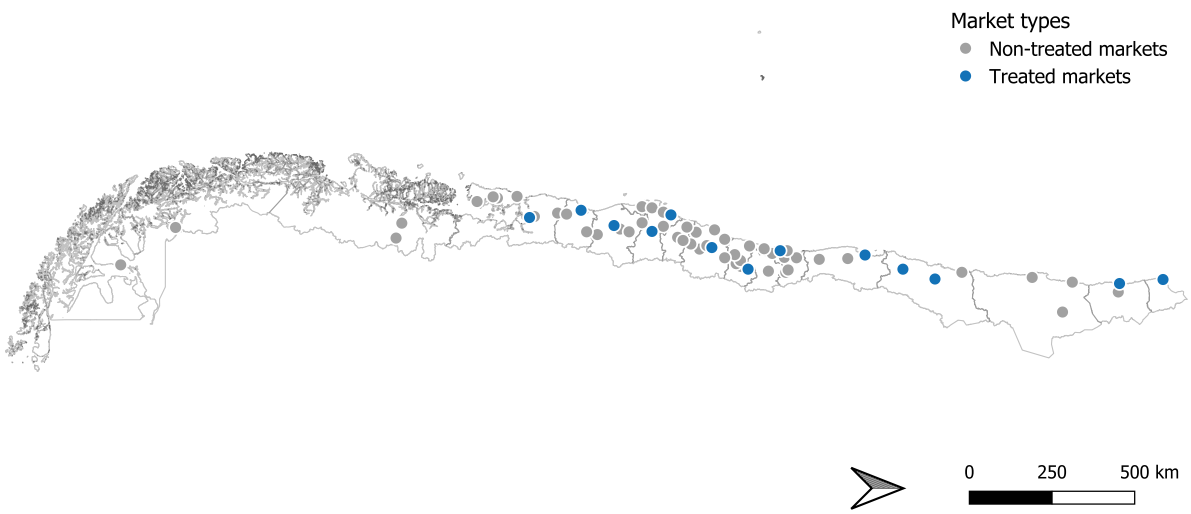
Notes. The table summarizes all cases of concession revocation between 2015 and 2024. Revocations occurred either because firms failed to begin operations within the contractual deadline or because they were found issuing inspection certificates without conducting the required tests.

Figure A.1: Smog and safety vehicle inspections in the U.S. and Chile



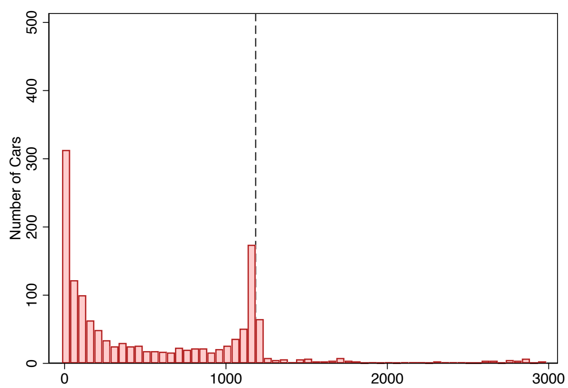
Notes. This plot shows the presence of smog and safety checks in the United States and Chile as of December 2024. Chile conducts both smog and safety checks nationwide, while the United States varies by state—some have only smog checks, others only safety checks, some have neither, and some require both inspections.

Figure A.2: Smog-check market distribution

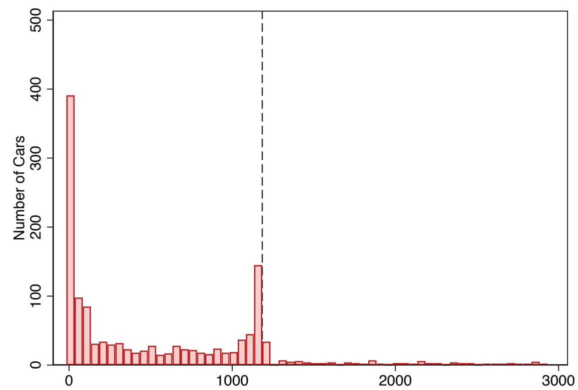


Notes. The plot shows the distribution of smog check markets in Chile. Blue circles represent markets that experienced an increase in the number of different competitors during our sample period. Gray circles represent other markets that were active during the same period.

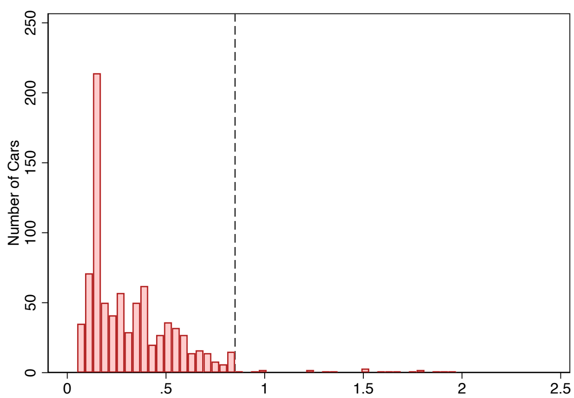
Figure A.3: Bunching evidence using emission test results



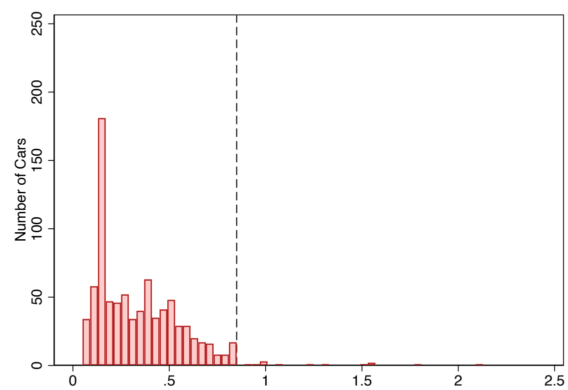
(a) NO 5015 test results in 2016



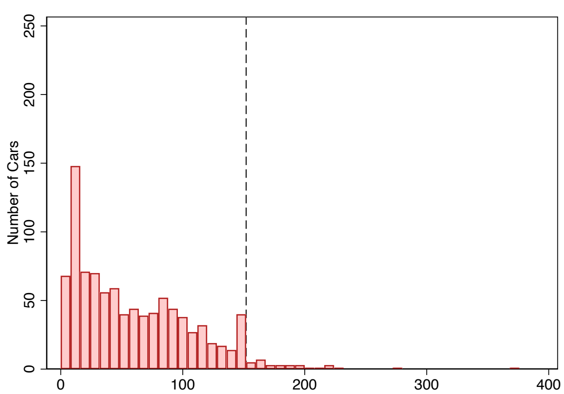
(b) NO 5015 test results in 2017



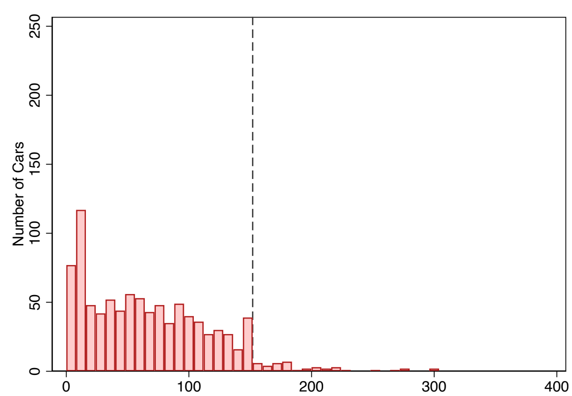
(c) CO 5015 test results in 2016



(d) CO 5015 test results in 2017



(e) HC 5015 test results for 2016

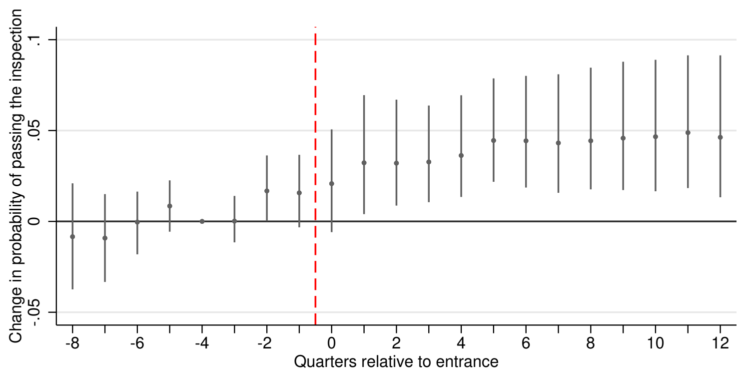


(f) HC 5015 test results for 2017

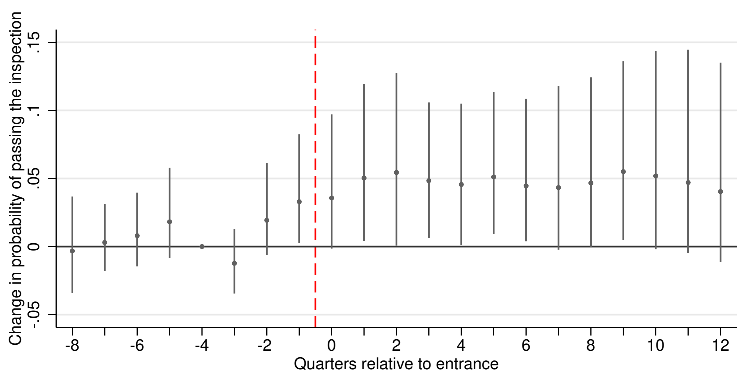
Notes. This figure presents evidence from von Dessauer (2019) on bunching in vehicle emissions in Chile. The panels display the distributions of NO, CO, and HC emissions for two vehicle models, the 1998 Hyundai Accent and the 1998 Suzuki Esteem, using data from all inspection stations with available test results in 2016 and 2017. Because emission thresholds vary across vehicles, focusing on specific models allows for a clearer comparison around well-defined cutoffs. The distributions show excess mass both just below the relevant thresholds and near zero, a pattern consistent with possible misconduct at some inspection stations.

Figure A.4: Competition effect on pass rates with entrants

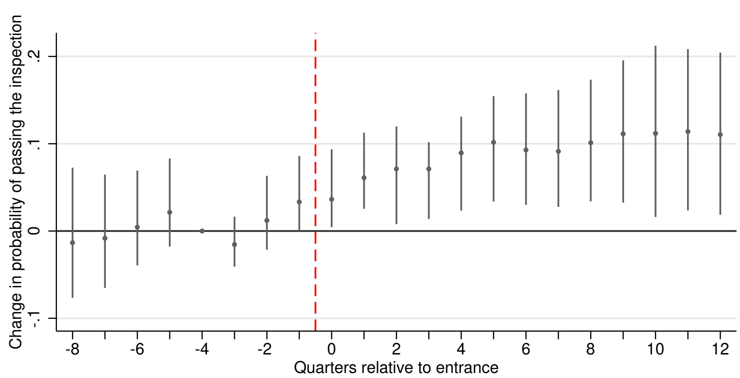
(a) Smog Checks



(b) Safety Checks



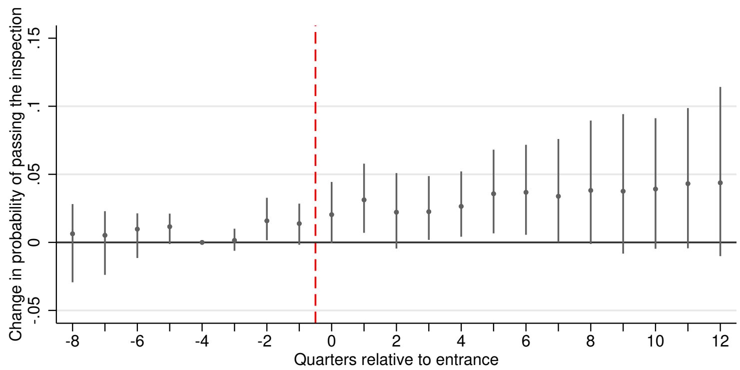
(c) Overall Inspection



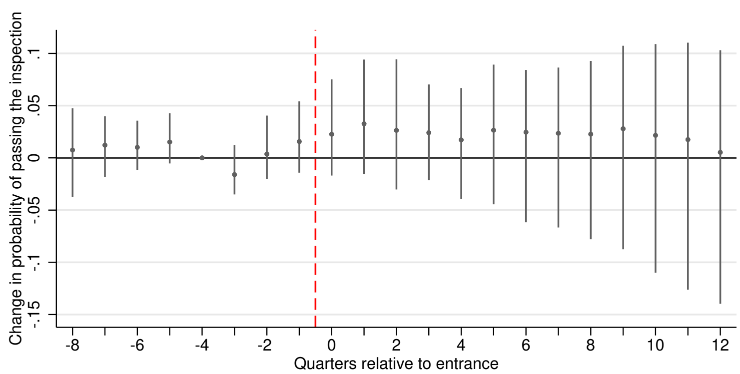
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago over vehicles manufactured at least 10 years before the inspection time, including entrant stations in the sample. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions.

Figure A.5: Competition effects on inspection pass rates: switchers

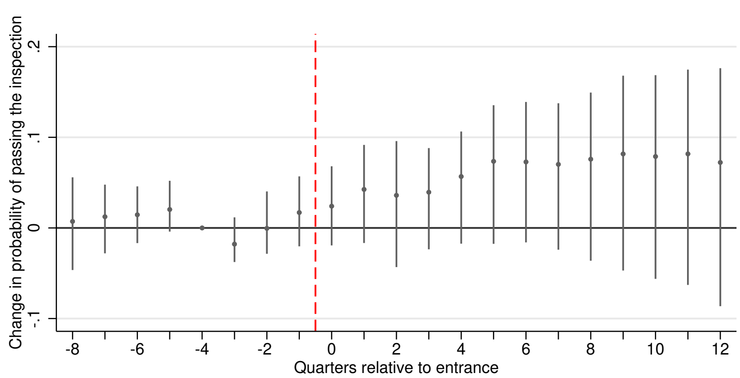
(a) Smog Checks



(b) Safety Checks



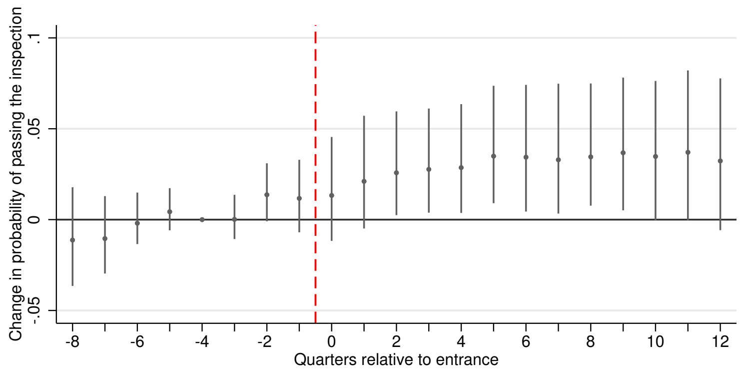
(c) Overall Inspection



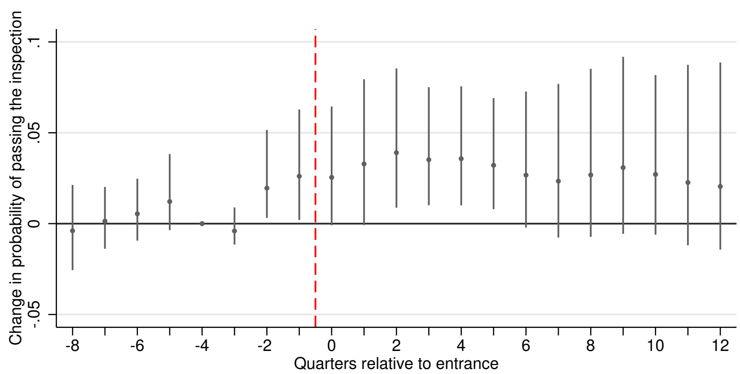
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago over vehicles manufactured at least 10 years before the inspection time. Also, for treated markets, we restrict the sample to switcher vehicles, defined as vehicles who at least once in the two years after the treatment entrance went to the entrant station. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions.

Figure A.6: Competition effects on inspection pass rates: loyals

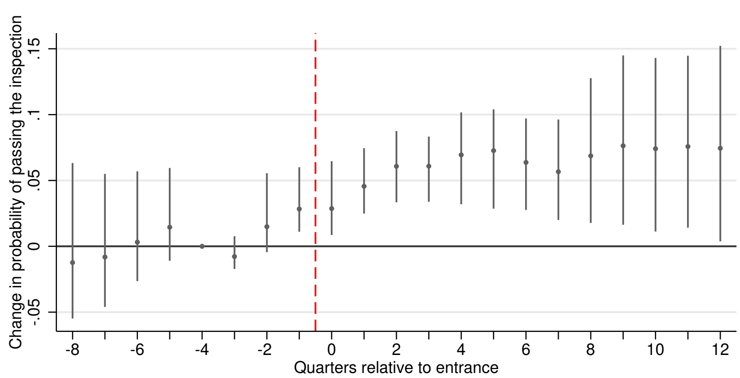
(a) Smog Checks



(b) Safety Checks



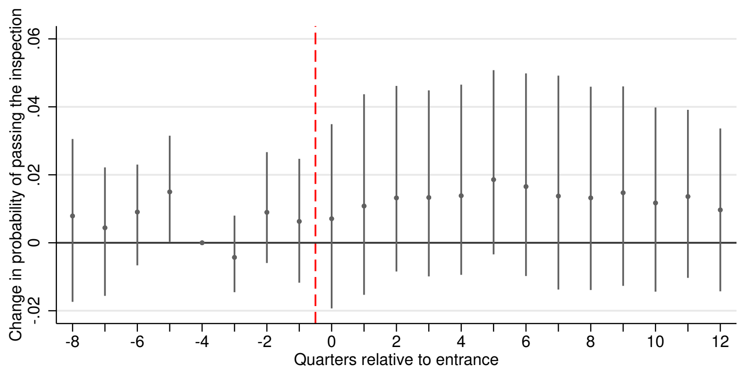
(c) Overall Inspection



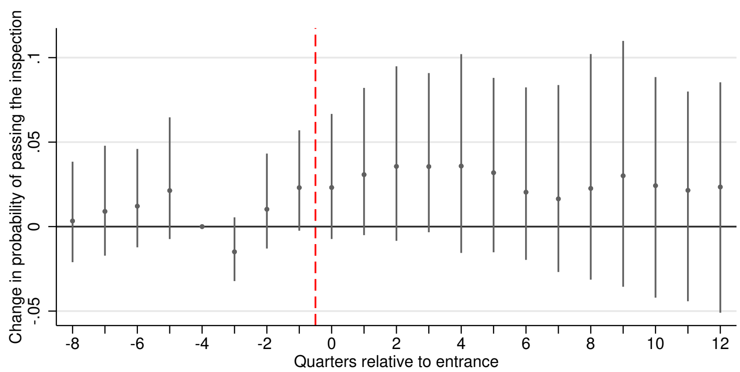
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago over vehicles manufactured at least 10 years before the inspection time. Also, for treated markets, we restrict the sample to loyal vehicles, defined as vehicles who never in the two years after the treatment entrance went to the entrant station. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions.

Figure A.7: Competition effects on inspection pass rates: no linear trends

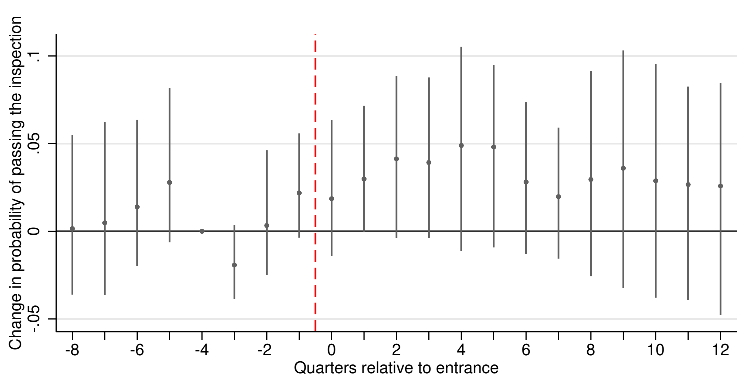
(a) Smog Checks



(b) Safety Checks



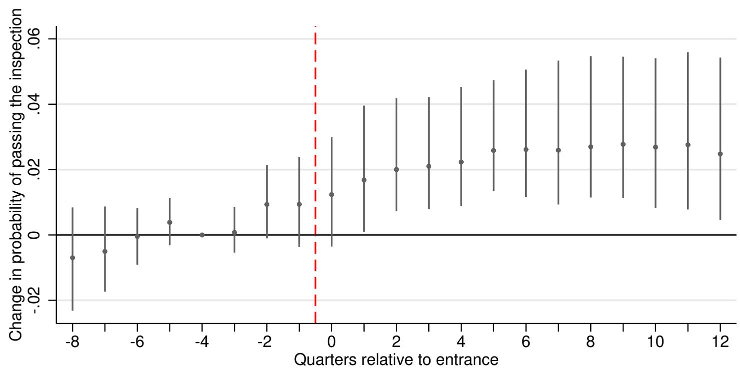
(c) Overall Inspection



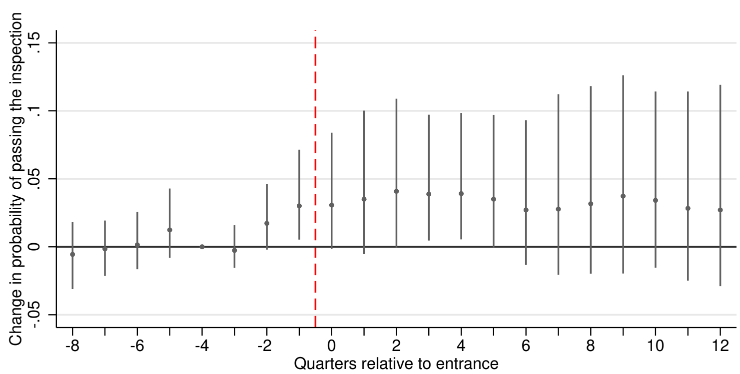
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates using equation 5 without the specific-station linear trends. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago over vehicles manufactured at least 10 years before the inspection time. The bars represent 95% confidence intervals calculated using wild bootstrap with 1000 repetitions.

Figure A.8: Competition effects on inspection pass rates: vehicles of all ages

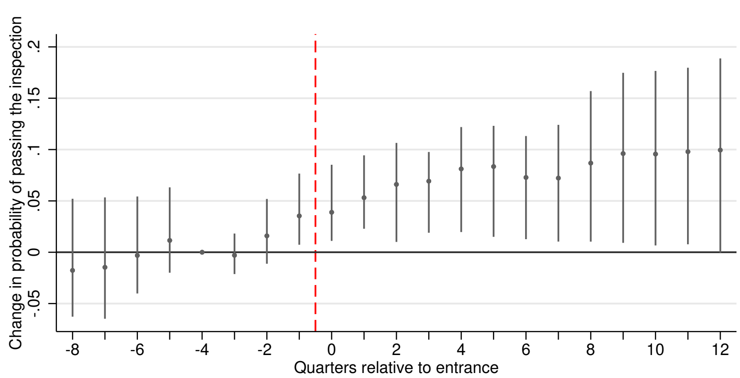
(a) Smog Checks



(b) Safety Checks



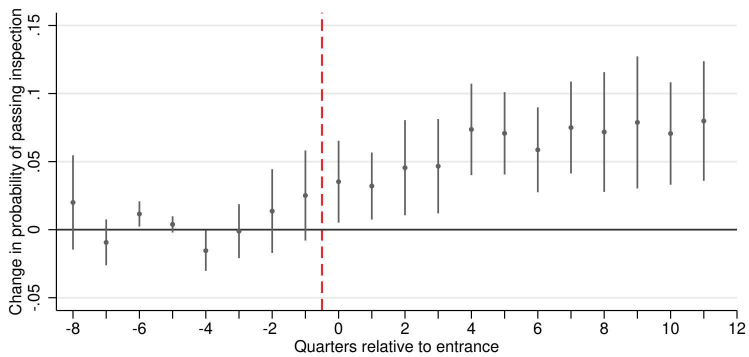
(c) Overall Inspection



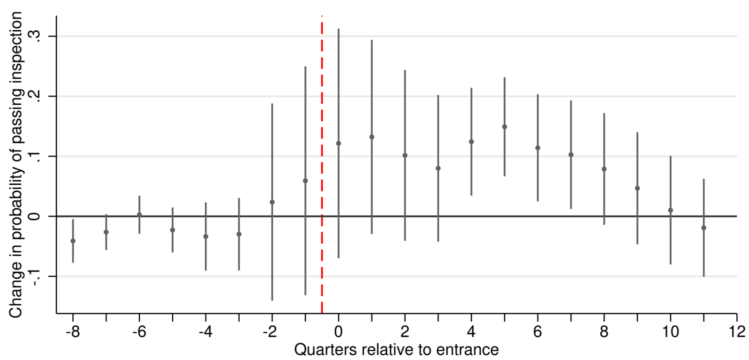
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago, excluding entrant stations in the sample. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions.

Figure A.9: Competition effects on inspection pass rates: Callaway-Sant'Anna Estimator

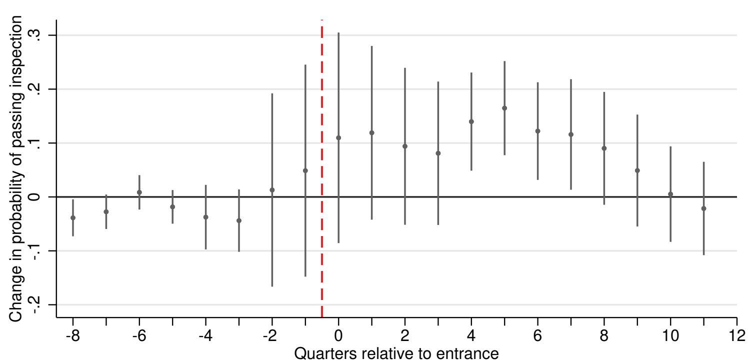
(a) Smog Checks



(b) Safety Checks



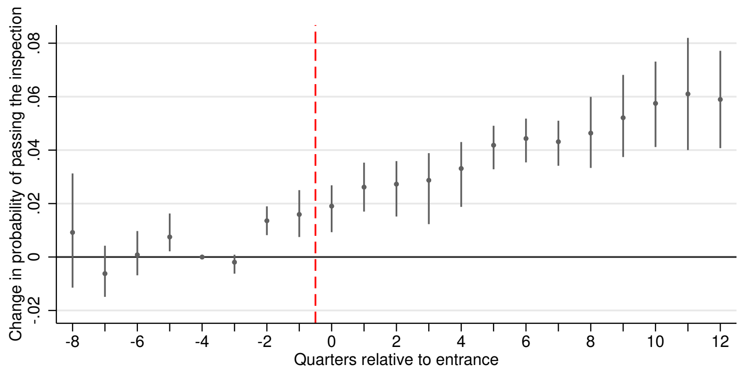
(c) Overall Inspection



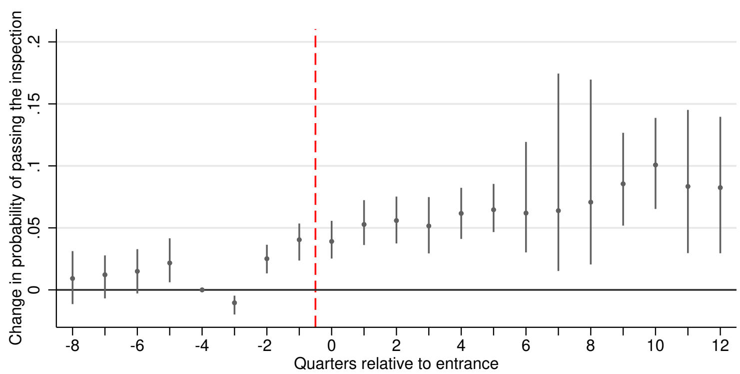
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. We use the repeated cross-sectional version of the estimator in Callaway and Sant'Anna (2021), defining treatment cohorts at the quarterly level. Since this specification does not accommodate the same high-dimensional license-plate fixed effects used in our baseline model, we instead control flexibly for vehicle composition by including fixed effects for the 50 most common brands in the data, interacted with 5-year manufacturing-year bins. The dashed red bar marks the time of entry, and we use the fourth quarter before entry as the base period to allow for anticipation effects. The sample is restricted to first inspections at incumbent stations outside Santiago, excluding entrant stations. Bars represent 95% confidence intervals computed using the wild bootstrap with 1,000 repetitions.

Figure A.10: Competition effects on inspection pass rates: Sun-Abraham Estimator

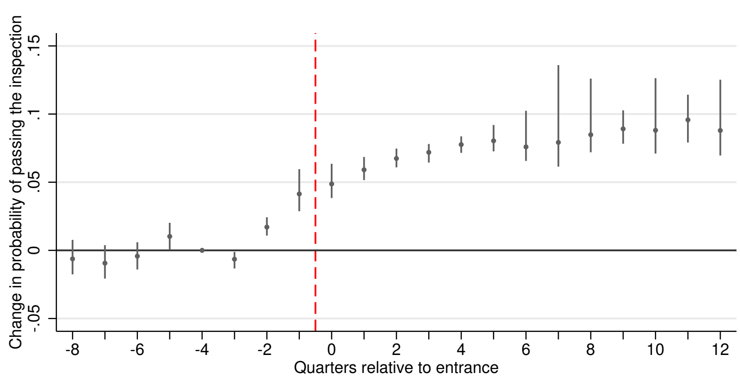
(a) Smog Checks



(b) Safety Checks

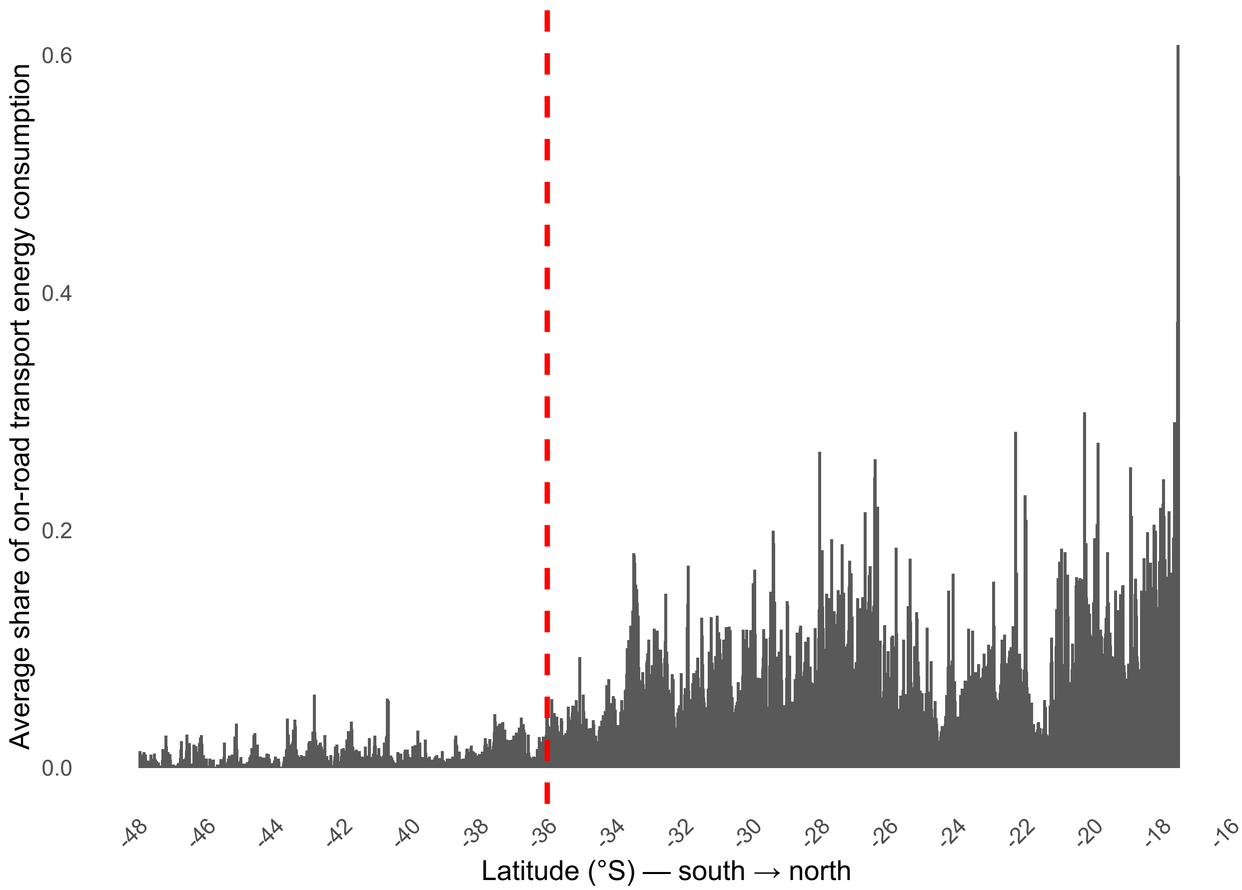


(c) Overall Inspection



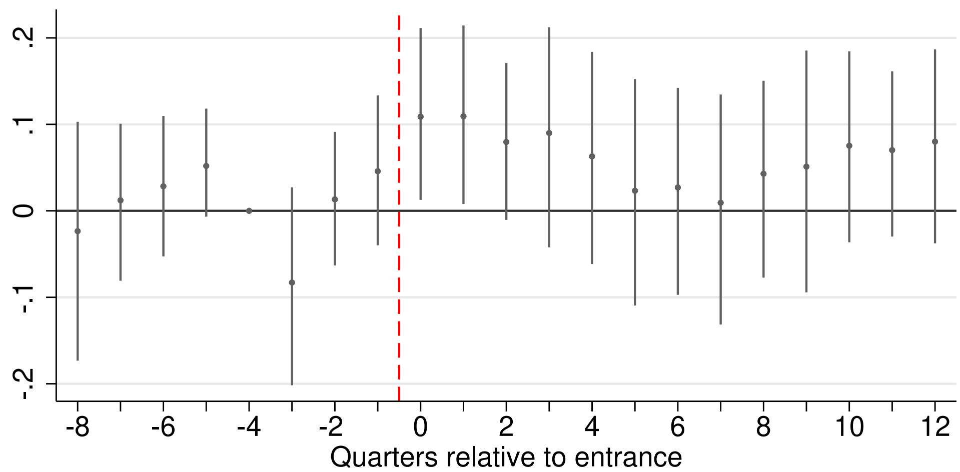
Notes. The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. We use the estimator in Sun and Abraham (2021), defining treatment cohorts at the monthly level. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside Santiago, excluding entrant stations in the sample. The bars represent 95% confidence intervals calculated using wild bootstrap with 1,000 repetitions fixing the cohort weights from the full sample level.

Figure A.11: Average share of on-road transport energy consumption PM2.5 by latitude



Notes. The plot shows the average share of on-road transport energy consumption PM2.5 relative to total PM2.5, by latitude. We use data from 2018 of Álamos et al. (2022). It covers latitudes from -17 to -48, encompassing the vast majority of Chile's densely populated territory. The red dashed line marks latitude -36, which closely approximates the location of the Biobío region. To the south of this line, the share of transport-related PM2.5 drops sharply, averaging below 1%.

Figure A.12: Change in log points of total market inspections



Notes. The figure presents an event-study of the log number of total first inspections per month in all markets except Santiago, using increases in the number of competitors as the event. The specification includes time and market fixed effects, market-by-month fixed effects to absorb seasonality, and market-specific linear trends. Standard errors are clustered at the market level, and inference is based on the wild bootstrap with 1,000 repetitions. We find no evidence of a systematic increase in the number of inspections following entry by new competitors.

Appendix B: Effects of additional competition on pass rates in Santiago

Santiago is by far the largest market in the country, with a much greater number of stations than other markets. For this reason, we treat the market as highly competitive throughout our sample period. To measure the effect of competition under these circumstances, we follow the approach in Bennett et al. (2013) and examine how station pass rates respond to a higher number of competing stations within a given radius.

Bennett et al. (2013) study smog check facilities in the state of New York. Unlike Chile, New York has a very large number of inspection stations (in the thousands), and inspections are performed at privately owned facilities that commonly include auto repair shops or garages, gas stations with service bays, and dealership service departments. To assess the effect of local market concentration on pass rates, the authors regress pass rates on the number of stations within a specified radius around each station. Because they do not observe license plates, they mitigate potential composition effects by controlling for available vehicle characteristics: brand, model, model year, and odometer.

In their main specification, the authors use a radius of 0.2 miles (approximately 0.32 kilometers) to capture local market concentration. They find that one additional facility within 0.2 miles is associated with a 0.07 percentage point increase in the probability of passing the inspection. Given that, in their data, roughly 7% of vehicles fail the test, this implies that an additional facility within 0.2 miles reduces the annual number of rejections by 1%.

To replicate this exercise in our setting, we adapt the analysis by considering the number of stations within radii of 1, 2, and 3 kilometers of each station. Stations in Santiago are more dispersed, so a 0.32-kilometer radius provides too little variation in local market concentration. For each radius specification, we estimate the following regression:

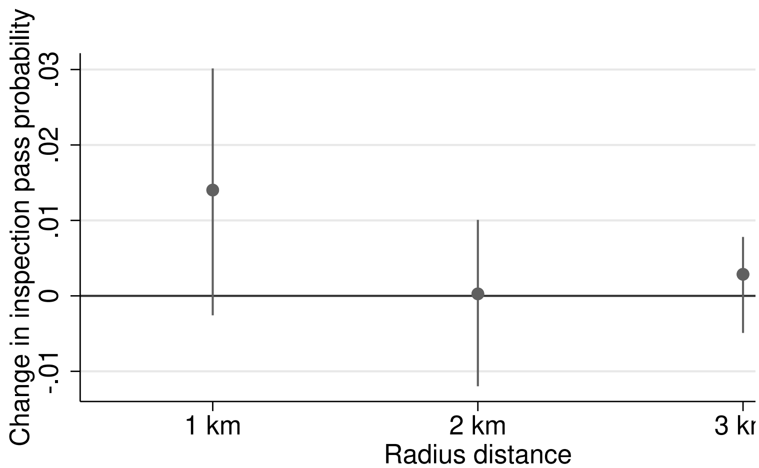
$$y_{ijt} = \delta_i + \delta_j + \delta_t + \beta_k c_{kjt} + \theta_{v(i,t)T(t)} + \phi_j \times t + \varepsilon_{ijt}, \tag{B.1}$$

where y_{ijt} is an indicator equal to 1 if vehicle i is approved at station j in month t. We include vehicle, station, and month fixed effects, as well as vintage-by-year fixed effects and station-specific linear trends. The variable c_{kjt} denotes the number of competing stations within a radius of k kilometers of station j in month t, where $k \in \{1,2,3\}$. The coefficient of interest is β_k, which captures the marginal effect of an additional station within k kilometers. The sample is restricted to first inspections conducted at stations in the Metropolitan Region of Santiago. In Figure B.1, we report the estimates of β_k for two samples: one restricting attention to vehicles more than 10 years old, and another including all vehicles.

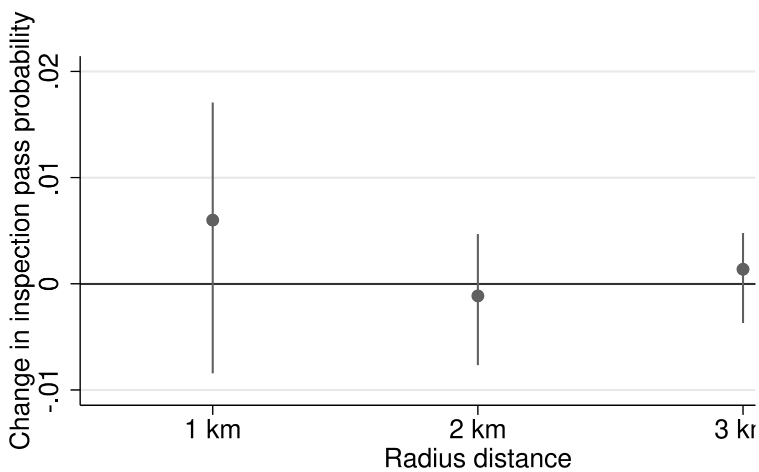
In neither panel do we find evidence that changes in local market concentration are associated with higher pass rates. Using the 1-kilometer radius, the estimated β_k is positive but statistically insignificant. We interpret these results as suggesting that the smog check market in Santiago is already highly competitive, so marginal entry of an additional nearby station does not appear to

Figure B.1: Replication of competition effect measured by the number of stations inside a radius in Santiago

Panel (a): Vehicles more than 10 years old



Panel (b): All vehicles



Notes. This figure plots the estimated coefficients on the number of stations within 1, 2, and 3 km of a given inspection site from regressions of pass rates. The specifications include station, vehicle, and month fixed effects, as well as controls for vehicle age and station-specific linear trends. The analysis is restricted to Santiago: panel (a) focuses on vehicles older than 10 years, while panel (b) includes all vehicles. For the 1 km radius, the estimated coefficient is positive but statistically insignificant. At 2 and 3 km, we find no evidence that additional nearby stations affect pass rates. Standard errors are clustered at the station level and are computed using the wild bootstrap with 1,000 repetitions.

increase pass rates.

Appendix C: Long-lasting effects of smog checks on inspection results

An important question for assessing the effectiveness of the smog check program is whether repairs operate only in the short run, lasting a few weeks or months, or instead generate longer-lasting reductions in emissions. In this section, we use the fact that we can track vehicles by license plate across inspections to study whether receiving a rejection increases the probability of passing in a subsequent inspection.

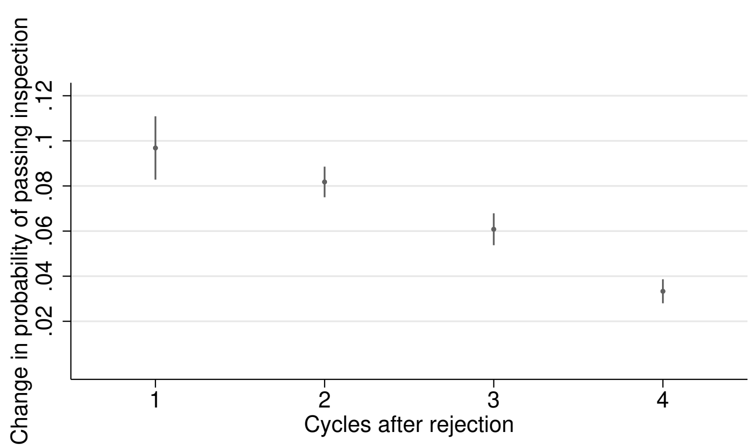
The rationale is straightforward. If rejections do not affect the probability of passing in a later inspection, then repairs have only transitory effects that do not persist beyond a year. Conversely, if a rejection increases future pass rates, we interpret this as evidence that repairs produce longer-lasting corrections in high-emission vehicles. We estimate the following regression:

$$y_{ic\tau} = \delta_i + \delta_{j(i,c)} + \delta_{t(i,c)} + \beta_\tau \, \text{Rejected}_{ic} + \theta_{v(i,c)T(i,c)} + \phi_{j(i,c)} \times t(i,c) + \varepsilon_{ic\tau}, \tag{C.1}$$

where $y_{ic\tau}$ is an indicator equal to 1 if vehicle i is approved τ cycles after its inspection in base cycle c. The treatment variable Rejected_{ic} equals 1 if vehicle i was rejected in cycle c. The station $j(i,c)$ and month $t(i,c)$ denote, respectively, the station and calendar month associated with the base inspection of vehicle i in cycle c. We include vehicle fixed effects, base-station fixed effects, and base-month fixed effects, as well as vintage-by-year fixed effects $\theta_{v(i,c)T(i,c)}$ and base-station-specific linear trends $\phi_{j(i,c)} \times t(i,c)$. The coefficient of interest is β_τ, which captures the persistence of the effect of an initial rejection τ cycles ahead. We estimate four specifications with $\tau \in \{1, 2, 3, 4\}$.

A potential concern is that β_τ may reflect not repairs following a rejection, but rather learning or search, for instance, drivers may switch to stations that misreport outcomes. To mitigate this concern, we restrict the sample to monopoly markets.

Figure C.1: Long-lasting effects of smog checks



Notes. The plot shows the change in the probability of passing an inspection conditional on being rejected τ cycles earlier. We restrict the sample to monopolistic markets and bootstrap cluster standard errors at the market level with 1,000 repetitions.

Figure C.1 presents our estimates of β_τ. We find that a rejection in cycle c increases the probability of passing the next inspection by 12 percentage points. This effect remains positive even four cycles later, with pass rates higher by about 4 percentage points. We interpret these results as evidence that repairs are not merely short-term adjustments to pass the current inspection, but instead generate longer-lasting reductions in high-emission behavior.

Appendix D: Testing for diseconomies of scale using auction data

In this section, we test whether station scale, measured by the number of operating lines, affects bidders' behavior. The goal is to assess whether the market exhibits diseconomies of scale, which would represent a potential drawback of monopoly delegation.

In Chile, tenders are organized by concessions, each of which may include one or more stations, and the tender documents specify the number of operating lines required at each station. It is not unusual for a concession to include stations located in different, but nearby, comunas. Bidders submit a single price for the entire concession, implying that all stations operated by the winning bidder within that concession charge the same price. We treat the number of operating lines as a proxy for operational scale. In our sample, stations have between one and five operating lines; thus, bidders competing for concessions that include stations with four or five lines likely anticipate operating at a larger scale than bidders competing for stations with one or two lines. By examining how bids vary with the number of lines, we can assess the presence (or absence) of diseconomies of scale. A positive relationship between the number of lines and bids would be consistent with diseconomies of scale.

Because of the joint-bidding structure, we implement two complementary approaches. First, we compare bids under the assumption that each station corresponds to a separate auction: we relate bids to each station's number of operating lines and assign the same bid to all stations within a given concession. Second, we aggregate to the concession level by summing operating lines across all stations in the concession. In both cases, we estimate the following regression:

$$\ln(b_{irt}) = \sum_m \beta_m \, \text{number of lines}_{m,irt} + \delta_t + \gamma_r + \alpha_i + \varepsilon_{irt}, \tag{D.1}$$

where $\ln(b_{irt})$ is the log bid (in 2023 prices) submitted by firm i in region r in year t. We include firm, region, and year fixed effects. The coefficient of interest is β_m, which loads on indicators for the number of operating lines: each dummy equals 1 if the bid corresponds to a station (or concession) with a given number of lines. Table D.1 reports the number of stations and concessions in each line category. To avoid comparisons based on very small cells, we group some line counts in the regressions.

Figure D.1 reports the estimates of β_m from the station-level specification, with stations with 1–2 lines as the omitted category. The coefficient for stations with three lines is negative but not statistically significant. For stations with four or five lines, the estimated coefficient is around -0.04, implying bids that are about 4% lower than for stations with fewer lines. Figure D.2 reports the estimates from the concession-level specification, where the omitted category is concessions with five or fewer total lines. The remaining coefficients are negative, with only the estimate for seven lines statistically significant at the 5% level. Taken together, the two analyses are consistent with

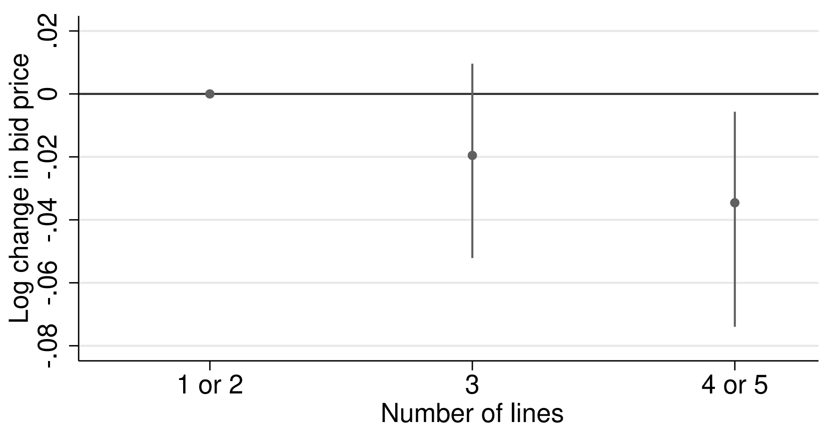
Table D.1: Distribution of number of lines

Lines	Stations	Concessions
1	17	0
2	98	0
3	170	5
4	88	4
5	7	27
6	0	51
7	0	42
8	0	25
9	0	6
10	0	9
Total	380	169

Notes. The table presents the distribution of stations and concessions in our sample by number of lines. For the concessions data, we sum the lines across all stations within each concession.

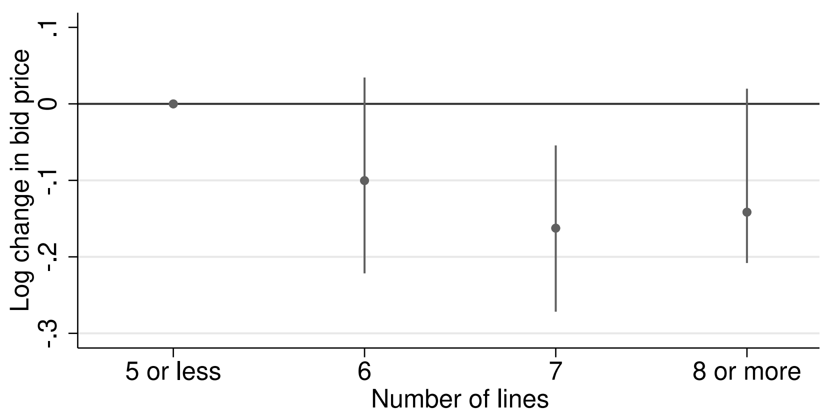
economies of scale, with bids decreasing as the number of lines increases. We therefore find no evidence of diseconomies of scale.

Figure D.1: Effect of scale on auction bids at the station level



Notes. The figure shows the log change in bid prices for different numbers of operating lines at the station level. Bids are adjusted using a deflator to account for auctions held in different years. Standard errors are clustered to the auction level and confidence intervals are computed using wild bootstrap with 1000 repetitions.

Figure D.2: Effect of scale on auction bids at the concession level



Notes. The figure shows the log change in bid prices for different numbers of operating lines at the concession level. Bids are adjusted using a deflator to account for auctions held in different years. Standard errors are clustered to the auction level and confidence intervals are computed using wild bootstrap with 1000 repetitions.

Appendix E: Proofs of selected propositions

Proofs for most propositions follow directly from the text, aside from a few exceptions, which are covered here. To save on notation we have normalized the number of inspection lines to two units, i.e., $l = 2$. We also let $\theta \in [0, \infty)$ be distributed according to the cumulative distribution function $F(\theta)$, with the usual regularity properties (more on this below).

Proof of Proposition 1. The individual $\tilde{\theta}$, who is indifferent between visiting the misreporting station and the truthful-reporting one, is given by the indifference condition

$$-\gamma F(\tilde{\theta}) = \tilde{\theta} - \gamma(1 - F(\tilde{\theta})) \tag{E.1}$$

which implies, quite intuitively, that $F(\tilde{\theta}) < 1/2$; that is, fewer than half of the drivers visit the truthful-reporting firm. An specialization equilibrium requires

$$\pi(M, T) \geq \pi(T, T) \Leftrightarrow (\bar{p} - c)(1 - F(\tilde{\theta})) \geq \bar{p}/2$$

and

$$\pi(T, M) \geq \pi(M, M) \Leftrightarrow \bar{p}F(\tilde{\theta}) \geq (\bar{p} - c)/2$$

to hold. Rearranging, these inequalities require

$$\frac{1 - 2F(\tilde{\theta})}{2(1 - F(\tilde{\theta}))} \geq 1 - 2F(\tilde{\theta})$$

to hold, which clearly doesn't, whether $F(\tilde{\theta}) < 1/2$ or not.

Proof of Proposition 2. Proofs to parts (i) and (ii) are straightforward except for the condition that ensures that no firm wants to price below the cap $\bar{p}$, i.e., $\bar{p} < \gamma$. Since firms simultaneously decide $p_i \leq \bar{p}$ and $m_i \in \{M, T\}$, there are four potential deviations to consider.

Consider first the case in which firm 1 deviates from an equilibrium that involves choosing M and pricing at the cap $\bar{p}$ by simultaneously choosing T and $p_1 < \bar{p}$. We want to rule out that if firm 1 were to deviate, pricing below the price cap is never optimal. The payoff from this deviation is given by

$$\pi_1(T, M) = p_1 F(\tilde{\theta})$$

where $\tilde{\theta} = \tilde{\theta}(p_1, \bar{p})$ is the type of the indifferent driver, which is given by the indifference condition

$$-\gamma F(\tilde{\theta}) - p_1 = \tilde{\theta} - \gamma(1 - F(\tilde{\theta})) - \bar{p}$$

Pricing below the cap is not profitable for firm 1 as long as

$$\left.\frac{\partial \pi_1(T,M)}{\partial p_1}\right|_{p_1=\bar{p}} \geq 0 \tag{E.2}$$

and

$$\left.\frac{\partial \pi_1^2(T,M)}{\partial p_1^2}\right|_{p_1\leq\bar{p}} < 0 \tag{E.3}$$

The first-order-condition (E.2) requires

$$\bar{p} \leq F(\tilde{\theta})\left(2\gamma + \frac{1}{f(\tilde{\theta})}\right) \tag{E.4}$$

where $f(\theta) = F'(\theta)$. Note that (E.4) reduces to $\bar{p} \leq \gamma$ for the Uniform distribution.

On the other hand, the second-order-condition (E.3) requires

$$\bar{p}f'(\theta) < 2f(\theta)(1+2\gamma f(\theta))^2 \tag{E.5}$$

for $\theta \geq \tilde{\theta}(\bar{p},\bar{p})$, which is immediate for the Uniform distribution since $f'(\theta) = 0$ for all $\theta \in [0,1]$. Condition (E.5) also holds trivially for any distribution with a nonincreasing density (i.e., $f'(\theta) < 0$) on $[0,\infty)$ like the Exponential and Pareto distributions. Using (E.4) and $\gamma > 0$ a simpler and sufficient condition for (E.5) to hold is

$$F(\theta)f'(\theta) \leq 2\left[f(\theta)\right]^2$$

which can be used to show that (E.5) also holds for the Normal distribution.

Consider now the case in which firm 1 deviates by pricing below the cap while continuing to misreport quality together with firm 2. In this case drivers split between the two stations according to

$$-\gamma Q_1 - p_1 = -\gamma Q_2 - \bar{p}$$

where $Q_1 + Q_2 = 1$. Solving we obtain that

$$Q_1(p_1,\bar{p}) = \frac{1}{2} + \frac{\bar{p}-p_1}{2\gamma} = 1 - Q_2(\bar{p},p_1)$$

Pricing below the cap is not profitable for firm 1 as long as (we can ignore the second-order condition since it holds automatically in this case)

$$\left.\frac{\partial \pi_1(M,M)}{\partial p_1}\right|_{p_1=\bar{p}} \geq 0 \tag{E.6}$$

where $\pi_1(M,M) = (p_1 - c)Q_1(p_1,\bar{p})$. Condition (E.6) reduces to $\bar{p} - c \leq \gamma$ for any $F(\theta)$. This implies that, for more general distributions, the relevant no-deviation condition, at least for the first two cases considered so far, is the more stringent of (E.4) and (E.6).

It remains to consider two other potential deviations, now from an equilibrium in which firms price at the cap and play T. Proceeding as above, it is straightforward to show that pricing below the cap is even less profitable than before, regardless of whether the deviation also involves playing M.

For part (iii), let β be the probability that a firm will play M and $1-\beta$ the probability that will play T. Anticipating that, both players must be indifferent between playing M and T, that is,

$$\pi(M) = \pi(T)$$

where $\pi(M) = \beta\pi(M,M) + (1-\beta)\pi(M,T)$ and $\pi(T) = \beta\pi(T,M) + (1-\beta)\pi(T,T)$. Solving for β as a function of $c \in [\underline{c}, \bar{c}]$ yields

$$\beta(c) = \frac{2c - \bar{p} + 2(\bar{p} - c)F(\tilde{\theta})}{c(1 - 2F(\tilde{\theta}))} \in [0,1]$$

where $\tilde{\theta}$ is given by equation (E.1),

$$\underline{c} = \frac{\bar{p}(1 - 2F(\tilde{\theta}))}{2(1 - F(\tilde{\theta}))},$$

and

$$\bar{c} = \bar{p}(1 - 2F(\tilde{\theta})).$$

Note that $\beta(\underline{c}) = 0$ and $\beta(\bar{c}) = 1$.

Appendix F: Intensive margin of misconduct

Consider $n \geq 2$ firms, all of them misreporting. Suppose that one of them, denoted by i, can make its station more attractive to visit by reducing its congestion from γ to $\gamma - \Delta_i$, at the cost of increasing its cheating cost to $c(\Delta_i) < \bar{p}$. We restrict attention to effort levels such that

$$0 \leq \Delta_i < \gamma \qquad \text{and} \qquad c(\Delta_i) < \bar{p},$$

so that cheating effort remains feasible and profits remain strictly positive. We assume throughout that

$$c'(\Delta) > 0, \qquad c''(\Delta) > 0, \qquad c'''(\Delta) = 0.$$

The first condition says that greater cheating effort is more costly, the second ensures that the firm's problem is locally concave at an interior optimum, and the third is imposed solely for tractability.

If each of the remaining $n-1$ firms exerts effort Δ_{-i}, firm i solves

$$\Delta_i^*(\Delta_{-i}) = \arg\max_{\Delta_i} \big(p - c(\Delta_i)\big) D_i(\Delta_i, \Delta_{-i}),$$

where

$$D_i(\Delta_i, \Delta_{-i}) = \frac{\gamma - \Delta_{-i}}{n\gamma - (n-1)\Delta_i - \Delta_{-i}}$$

is the fraction of consumers visiting station i. Note that if $\Delta_i = \Delta_{-i}$, then $D_i = 1/n$. We want to find conditions under which (equilibrium) cheating effort $\Delta(n)$ increases with the number of misreporting competitors, but at a decreasing rate.[1]

Proposition F1. *A sufficient condition for equilibrium cheating effort $\Delta(n)$ to satisfy*

$$\Delta'(n) > 0 \qquad \textit{and} \qquad \Delta''(n) < 0$$

for all $n \geq 2$ is that the cheating cost function is sufficiently convex

$$c'(\Delta) < c''(\Delta)(\gamma - \Delta) \tag{F.1}$$

Proof. The first-order condition for firm i is

$$(n-1)\big(p - c(\Delta_i)\big) = c'(\Delta_i)\Big(n\gamma - (n-1)\Delta_i - \Delta_{-i}\Big).$$

[1] Note that in our model, where misreporting plants reject no car, an increase in cheating effort would not translate into higher pass rates. The latter, however, is straightforward to obtain by letting m depend on cheating effort.

Imposing symmetry, $\Delta_i = \Delta_{-i} = \Delta(n)$, yields

$$p - c(\Delta(n)) = \frac{n}{n-1} c'(\Delta(n))\big(\gamma - \Delta(n)\big). \tag{F.2}$$

To study how equilibrium cheating effort varies with the number of firms, define

$$F(\Delta, n) \equiv p - c(\Delta) - \frac{n}{n-1} c'(\Delta)(\gamma - \Delta).$$

Then the symmetric equilibrium $\Delta(n)$ satisfies

$$F(\Delta(n), n) = 0.$$

By the implicit function theorem,

$$\Delta'(n) = -\frac{F_n}{F_\Delta} = -\frac{c'(\Delta)(\gamma - \Delta)}{(n-1)\big[c'(\Delta) - nc''(\Delta)(\gamma - \Delta)\big]}. \tag{F.3}$$

Since $c'(\Delta) > 0$ and $\gamma - \Delta > 0$, a sufficient condition for $\Delta'(n) > 0$ is $c'(\Delta) < nc''(\Delta)(\gamma - \Delta)$, which follows immediately from the stronger condition (F.1) since $n \geq 2$.

We now turn to the second derivative. Differentiating the identity

$$F(\Delta(n), n) = 0$$

once more gives

$$\Delta''(n) = -\frac{F_{\Delta\Delta}(\Delta'(n))^2 + 2F_{\Delta n}\Delta'(n) + F_{nn}}{F_\Delta},$$

Substituting (F.3) into the expression for $\Delta''(n)$, using $c'''(\Delta) = 0$, and simplifying yields

$$\Delta''(n) = \frac{2c'(\Delta)(\gamma - \Delta)\big[c'(\Delta) - c''(\Delta)(\gamma - \Delta)\big]\big[c'(\Delta) - nc''(\Delta)(\gamma - \Delta)\big] + (n+1)c''(\Delta)\big(c'(\Delta)\big)^2(\gamma - \Delta)^2}{(n-1)^2\big[c'(\Delta) - nc''(\Delta)(\gamma - \Delta)\big]^3}.$$

It follows from condition (F.1) that $\Delta''(n) < 0$, implying that equilibrium cheating effort increases with the number of misreporting firms, but at a decreasing rate. □

Note that a sufficiently convex cost function is necessary to ensure that $c(\Delta_i) < \bar{p}$ for all values of Δ_i.

Appendix G: Restricting consumer choice

In the duopoly setting of Section 2, where consumers are indifferent between stations—as long as both follow the same strategy—restricting consumer choice to a single provider would replicate the monopoly-delegation outcome. In practice, however, even if consumers are assigned to their nearest stations, unforeseen factors may shift their preferences toward another provider. In such cases, allowing limited flexibility—so that consumers can switch from their originally designated station—can improve welfare.

We explore this by introducing shocks to the duopoly model of Section 2. To simplify notation, let us consider just two inspection lines, $l = 2$, so that a consumer who visits station $i \in \{1, 2\}$ obtains

$$u_{\theta i} = \theta m_i + \mu \epsilon_i - \gamma Q_i - \bar{p}$$

where $\epsilon_i = 1 - \epsilon_{j \neq i}$ is a preference shock for station i, which we assume uniformly distributed over the unit interval, and $0 < \mu < \min\{\gamma/(1+2\gamma), 1/2(1+2\gamma)\}$. Note that the consumer with $\epsilon_1 = \epsilon_2 = 1/2$ perceives no differentiation between the two stations.

Assume consumers are split evenly between the two stations. If the cost of cheating, c, is sufficiently high, at least above $\hat{c}$, the planner can safely let consumers switch stations without triggering misconduct. In this case, half of consumers would switch stations: those whose shocks for their originally-designated station is below $1/2$. The problem for the planner is what to do when c is not that high. One option is to restrict switching altogether, but this may be too costly. Allowing some switching, however, introduces an externality in addition to the pollution externality, the possibility of triggering misconduct. The planner thus faces a mechanism design problem.

One way to approach this problem is to follow Coase (1960) and issue "location" allowances to individuals for visiting a specific station. Anyone wishing to switch stations would need to trade his location allowance with someone assigned to the other station. In absence of transaction costs, this mechanism replicates the monopoly-delegation outcome. A market for location allowances will develop and clear at a zero price, or at any positive price for that matter, ensuring that 50% of individuals switch and that each station serves exactly half of the market, regardless of c.

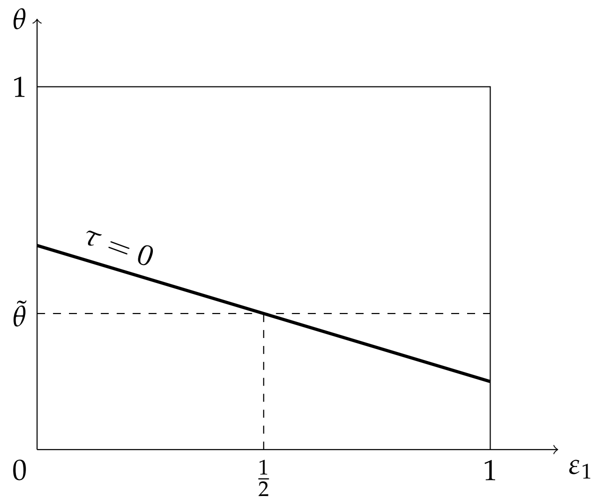
Proposition G2. *In the absence of transaction costs, the optimal mechanism follows Coase (1960): issue location allowances and let individuals freely trade them.*

If transaction costs are expected to be significant, the alternative is to follow Pigou (1920) and introduce a "switching" tax that internalizes the possibility of triggering misconduct. Let τ be the tax that a consumer must pay to the government if she decides to switch stations (the entire tax collection is returned back to society, including drivers and non-drivers, in a lump-sum fashion). A consumer would switch from station i to j whenever

$$\theta m_j + \mu\epsilon_j - \gamma Q_j - \bar{p} - \tau > \theta m_i + \mu\epsilon_i - \gamma Q_i - \bar{p}$$

The payoffs when both stations follow the same strategy, either misreport (M) of report truthfully (T), are as before, no matter τ. The latter only affects the number of consumers who switch, from 50%, when $\tau = 0$, to none, when $\tau \geq \mu$. Changes in payoffs arise only when stations follow different strategies. As depicted in Figure G.1, the payoffs when $\tau = 0$ are exactly as before, with $\tilde{\theta}$ given by (1). The figure assumes that station 1 plays M while station 2 plays T. Station 1 retains all the consumers assigned to it who lie above the "$\tau = 0$" line, that is, those for whom $\theta + 2\mu\epsilon_1 \geq 2\gamma Q_1 + \mu - \gamma$ (recall that $Q_2 = 1 - Q_1$). However, it loses all consumers assigned to it who lie below this line to station 2, but in return gains all consumers assigned to station 2 who lie above the same line. As a result, station 1 continues to serve a fraction $Q_1 = 1 - \tilde{\theta}$ of consumers, as before.

Figure G.1: Consumers assigned to station 1 and their switching decisions



Notes. The figure depicts the switching decision of a consumer type (θ, ϵ_1) who was originally assigned to station 1, the misreporting station, when the switching tax is zero, $\tau = 0$. If her type lies on the line "$\tau = 0$", she is indifferent where to go for the inspection, whether to remain with station 1 or to switch to the honest station (station 2). If her type lies above line "$\tau = 0$", she is strictly better off staying with station 1, and if her type lies below line "$\tau = 0$", she is strictly better off switching to station 2.

Consider now a positive but small τ. There are two changes relative to the case of $\tau = 0$. First, the fraction of consumers leaving station 1 is smaller now, only those for whom $\theta + 2\mu\epsilon_1 \leq 2\gamma Q_1 + \mu - \gamma - \tau$. Second, the fraction of consumers arriving from station 2 is also smaller, only those for whom $\theta + 2\mu\epsilon_1 \geq 2\gamma Q_1 + \mu - \gamma + \tau$. These two changes exactly offset each other as long as $\tau \leq \underline{\tau}$, in which case station 1 continues to serve the same fraction $1 - \tilde{\theta}$ of consumers, i.e., as in the baseline duopoly model.

Once $\tau > \underline{\tau}$, however, the fraction leaving station 1 exceeds the fraction arriving from station 2, resulting in a net loss of consumers. This loss increases with τ until no consumer switches stations, which occurs when $\tau = \bar{\tau} > \underline{\tau}$ (the thresholds $\underline{\tau}$ and $\bar{\tau}$ will be determined shortly). Thus, setting $\tau \geq \bar{\tau}$ is equivalent to prohibiting switching. This may be the only option when $c = 0$, but would be socially too costly when $c > 0$ but too low to prevent misconduct.[2] There is a cheaper way to ensure honesty.

Proposition G3. *There exists some $\tau \in (\underline{\tau}, \bar{\tau})$, where $\underline{\tau} \equiv \gamma/(1+2\gamma) - \mu$ and $\bar{\tau} \equiv 1 + \mu$, which ensures truthful reporting from both firms for a given cost of cheating c while giving consumers some flexibility to switch.*

The exact value of τ depends not only on the value of underlying parameters such as μ and γ, but also on whether the regulator aims to implement truthful reporting in dominant strategies or merely in risk-dominant ones, the latter requiring a lower τ. It could also be the case, when is optimal to set $\tau \in (\mu, 1 + \mu)$, that we do not observe any switching on path, but we would off path, in case of a deviation to play M. In any case, it is clear that a uniform tax is not the best tax mechanism, since the regulator could improve upon it; for example, by conditioning it on whether the car fails the first inspection. Still, trading location allowances is strictly superior when transaction costs are negligible.

[2]Since $\epsilon_2 - \epsilon_1 = 1 - 2\epsilon_1$, the welfare loss of prohibiting switching when both stations are known (or expected) to report truthfully amounts to $\int_0^{1/2} \mu(1 - 2\epsilon)\, d\epsilon = \mu/4$.