# Competition and Misconduct in Certification Markets with Externalities[*]

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**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 suggest that these incentives are significant: misreporting emerges as soon as competition is introduced. We find that delegating each market to a single agent proves effective in reducing approval rates, without compromising service quality or the ex-ante competition for the market, while delivering substantial and permanent reductions in vehicle emissions.

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# 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 theory and evidence from Chile's concentrated vehicle-inspection markets.

According to our theory, duopoly markets exhibit strong strategic complementarities: in equilibrium, both firms either report truthfully or misreport for a wide range 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 and misreporting entails an extra cost (e.g., the risk of losing the concession to operate in the market). Yet the evidence tells a different story: misreporting emerges as soon as competition is introduced, causing substantial and persistent increases in air pollution.

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 63 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).

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

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[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. Vives (2001) offers a similar transition in a pricing game as the cost function becomes more convex.

2024, the MTT issued tenders for 59 concessions in our sample of markets, covering 129 inspection stations in total.[3] 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—roughly 20% 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.[4] 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. We find some support for an inverted-U relationship between competition and misreporting: beyond a point, more competition may reduce misreporting.[5]

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 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

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[3] 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. Note that concessions restrict firms to inspection services only: under no circumstances are stations allowed to offer repair services or sell auto parts.

[4] 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).

[5] This is consistent with our model, in which misreporting plays a dual role: it boosts a firm's demand at the cost of being caught under non-compliance. At higher levels of competition the demand-boost effect becomes dominated at least for one firm.

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 markets experiencing the entry of a new competitor exhibit increases in air pollution of about 20%, consistent with higher vehicle emissions induced by the higher pass rates documented above.[9] Importantly, this provides strong evidence that vehicles failing their inspections undergo long-lasting repairs.[10]

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

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[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 estimate that vehicle emissions increased by 36% in markets experiencing the entry of new competitors, which, given the pass-through rates of 30–37% documented in Rizzi and De La Maza (2017) and Barraza et al. (2017), should translate into increases in PM2.5 concentrations of around 12%. These pass-through rates could be larger, however, given that industrial emissions and wood burning have declined significantly in recent years.

[10] 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%.

the relative importance of safety inspections in determining accident outcomes.

A natural policy response to limit misreporting is stricter enforcement. In practice, however, enforcement is already intensive and probably 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 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 suggest that, even without stronger sanctions, delegating each market to a single operator would achieve lower pass rates and pollution levels.

A valid concern with monopoly delegation is that consumers may experience a 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.[11] The same evidence speaks to a mirror-image concern: that monopolists might misreport in the opposite direction, over-rejecting vehicles to economize on inputs or avoid costly mistakes. That monopoly stations spend more time inspecting, not less, cuts against this possibility; and because they can neither charge for reinspections nor offer repair services, they have little incentive to over-reject.

In addition to the previous concerns, one could argue 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. 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 theory and evidence indicate that if bidders anticipate a misreporting equilibrium ex post, their bids would be higher, not lower.[12] 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—yields higher revenue 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

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[11] 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.

[12] This is because misreporting is fundamentally an incentive problem in our setting—triggered by competition—rather than a selection problem, as in, for example, Diwan et al. (2026). For more on this see Online Appendix C.

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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.[13] 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.[14]

Monopoly delegation is not merely hypothetical: it is already in place in some jurisdictions, including Ireland and some regions in Spain.[15] 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 show that monopoly delegation can restore truthful reporting.

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[13] 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.

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

[15] 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.

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., 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 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 describes the market for vehicle inspections and presents our model of misreporting. Sections 3 and 4 cover the empirical analysis, describing the data and the econometric results, respectively. Section 5 discusses policy considerations, and Section 6 concludes.

## 2. The Market for Vehicle Inspections

Chile's vehicle inspection markets provide an ideal setting to study the relationship between competition and firm reporting behavior. On the one hand, they are well-defined markets with few competitors, particularly in towns and cities outside Santiago. On the other hand, the increase in the number of inspection stations over the last decade generates useful variation in market structure. To understand the workings of these markets, we describe the regulatory environment and develop a stylized model of the incentives faced by inspection stations and drivers. We use the terms firm and station interchangeably.

### 2.1. The regulatory environment

As in many countries and jurisdictions, vehicles in Chile must undergo periodic technical inspections—a combination of smog and safety checks—to ensure that those in circulation meet the minimum safety and environmental standards established by law. These inspections are carried out at government-approved stations operated by private agents. The government agency in charge of running and enforcing the inspection program is the Ministry of Transportation and

Telecommunications (MTT).

The structure of each market is determined by the MTT. Many of these markets are too small to support more than one provider, which, according to the MTT, explains why about half of the 63 markets in our sample are served by a single firm, one-fourth by two firms, and the remainder by three or more firms. Through competitive public tenders, the MTT determines both the identity and number of inspecting firms operating in each market at any given time. These tenders also establish the price caps that winning firms are authorized to charge for each inspection. Although firms are free to charge below the cap, years of evidence show that they never do so.[16]

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. 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 vehicle that fails any of the tests may return for reinspection within two weeks at no extra cost.

Unlike in some other jurisdictions, under Chilean law inspection firms can only conduct vehicle inspections and must follow rigid, standardized procedures that leave little scope for differentiation. They are prohibited from engaging in any other economic activity, including vehicle repairs or the sale of auto parts, whether directly or indirectly through third parties. They are not even allowed to advertise in the media, run promotions, or display advertising materials of any kind.

To ensure that stations comply with the law—failing vehicles that do not meet minimum safety and environmental standards—the MTT has several enforcement measures in place. A large number of government inspectors visit stations on a weekly basis. These visits are random, and inspectors are not only periodically evaluated but also regularly rotated to prevent the development of long-term relationships with station personnel. During their visits, inspectors check that testing machines are properly calibrated and functioning, and that stations are clean, orderly, and have appropriate signage in place. In some cases, inspectors follow vehicles through the entire inspection process. To improve data reliability, the MTT introduced an online monitoring system in the early 2010s that automatically transmits inspection data to the ministry in real time, including the specific reasons for rejection. By 2015, 75% of stations were connected to the system, and by 2024, full adoption had been achieved.

In cases of station misconduct, the MTT can impose sanctions ranging from warnings and fines to concession termination. Because each concession is governed by a separate contract, termination

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[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.

applies only to the market in which the misconduct occurred. Thus, a conglomerate operating in several markets that is found to be in non-compliance in one particular market must relinquish only the concession for that market. Over the past ten years, the MTT has terminated three concessions for issuing certificates without inspecting vehicles.[17]

It is useful to distinguish between detecting suspicious behavior and enforcing sanctions. The regulator may observe unusually high approval rates that help flag suspicious behavior, but such evidence is unlikely by itself to provide sufficient grounds for concession revocation. Revocation requires harder evidence, such as issuing certificates for cars that were not inspected, as has occurred in the past; approving cars that leave the station in evidently poor condition; or manipulating testing equipment. In practice, the difficulty of establishing definitive proof of misreporting limits the effectiveness of enforcement, even when statistical patterns suggest non-compliance. As we formalize below, this enforcement friction—the gap between suspecting and proving misconduct—plays a central role in shaping firms' incentives to misreport.

## 2.2. A model of misreporting

While the regulator's objective in designing and overseeing these markets is clear—to ensure compliance with safety and environmental standards—the incentives faced by drivers and firms are less straightforward. From the regulator's perspective, the key concern is misreporting: vehicles that should fail inspection are instead approved and remain on the road unrepaired. For drivers, however, the consequences of misreporting are more nuanced. Although drivers arguably care little about their vehicles' emissions, they likely do care about safety. Even if a vehicle improperly passes inspection, the driver may still learn about underlying safety problems during the process and choose to address them independently. This means that misreporting may have less of an impact on safety than on air quality, making the emissions externality the central policy problem. We develop a model to illustrate how competition among inspection stations shapes the extent of this misreporting.

### 2.2.1. The duopoly case

To fix ideas, consider two symmetric firms, each operating $l/2$ inspection lines under a common price cap $\bar{p}$, set in an earlier procurement stage. 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 $\theta$, defined as the probability of failing the inspection times the corresponding repair cost. To facilitate the exposition, we assume $\theta$ to be uniformly distributed over the unit

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[17] Another four concessions were terminated for failing to commence operations by the mandated deadline. See Table A.4 of Online Appendix A for details.

interval.[18] An individual who takes her car $\theta$ to firm $i \in \{1, 2\}$ expects to obtain utility

$$u_{\theta i} = \theta\beta_i - p_i - \gamma q_i \tag{1}$$

where $\beta_i \in \{0, 1\}$ identifies whether the firm reports truthfully ($\beta = 0$) or misreports ($\beta = 1$),[19] $p_i \leq \bar{p}$ is the price charged by the firm, $\gamma > 0$ captures the disutility of waiting in line, and $q_i$ is the fraction of individuals visiting one of the firm's inspection lines (there is no difference across a firm's inspection lines, so its overall demand is simply $Q_i = lq_i/2$). Visiting a misreporting station yields expected savings of $\theta$ from avoiding repairs, which must be balanced against visiting a possibly more crowded station.

Firms simultaneously choose their prices and whether to misreport ($M$) or report truthfully ($T$). We normalize the variable cost of running a proper inspection to zero, so $p_i$ can be interpreted as the markup over variable costs. In principle this markup can increase with misreporting if it saves on some of these variable costs. Many years of evidence show that firms have no incentives to price below the cap, regardless of their conduct, which is ensured by assuming $\bar{p} \leq 2\gamma/l$.[20]

To build intuition, we begin by assuming that consumers observe stations' reporting decisions before choosing which one to visit; we return to this assumption at the end of the section and formally relax it in Online Appendix D. When both firms follow the same strategy and charge the same price, all consumers are indifferent between them. Thus, the payoff of each firm when both report truthfully is $\pi(T, T) = \bar{p}Q(T, T) = \bar{p}/2$, while if both misreport their payoffs are $\pi(M, M) = (\bar{p} - c)Q(M, M) = (\bar{p} - c)/2$, where $c \in (0, \bar{p})$ is the per-unit cost of misreporting, capturing the enforcement friction described above. The cost $cQ(M, M)$ can be interpreted as the probability of being caught misreporting times a penalty proportional to the damage inflicted, consistent with the maximum sanction a non-compliant station can face: the revocation of its concession and the associated forgone revenues.[21] Clearly, both firms are better off reporting truthfully.

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). If stations follow

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[18] In Online Appendix D we present the results for a general distribution of types, $\theta \in [0, \infty)$.

[19] For simplicity, we assume that a firm that chooses to misreport approves all vehicles that would otherwise have failed a proper inspection. We relax this assumption in Online Appendix D.

[20] See Online Appendix D for a formal derivation. We adopt this assumption not only because it is consistent with reality, but also because it simplifies the exposition. Relaxing it does not qualitatively affect the results that follow, as also shown in Online Appendix D.

[21] Formally, if $\phi$ is the probability of losing the concession and $s$ denotes any input savings from misreporting (including bribes or "tips", if any), then $c = \phi(\bar{p} + s) - s$, so that $(\bar{p} - c)Q(M, \cdot) = (1 - \phi)(\bar{p} + s)Q(M, \cdot) + \phi \cdot 0$. Note that $c > 0$ requires $\phi(\bar{p} + s) > s$. 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.

different strategies, their payoffs are

$$\pi(M,T) = (\bar{p} - c)Q(M,T) \quad \text{and} \quad \pi(T,M) = \bar{p}Q(T,M), \tag{2}$$

where $Q(M,T) = 1 - Q(T,M) = 1 - \tilde{\theta}$, and $\tilde{\theta}$ is the consumer indifferent between the two stations. From her indifference condition, we obtain

$$\tilde{\theta} = \frac{2\gamma}{l + 4\gamma}. \tag{3}$$

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:

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

and

$$\pi(T,M) \geq \pi(M,M) \Leftrightarrow c \geq \bar{p}l/(l+4\gamma). \tag{5}$$

Put together, these conditions require $\bar{p}l/(l+4\gamma) \leq \bar{p}l/[2(l+2\gamma)]$, which clearly cannot hold.[22]

In Shaked and Sutton (1982), "quality" differentiation serves to soften price competition. Here, prices are kept at their cap level, so in principle that mechanism is muted. However, as shown in Online Appendix D, even if the cap were removed and firms were free to choose prices, that mechanism would not be enough to overturn the strong strategic complementarity in reporting choices, regardless of $c$ and $\gamma$. Having discarded a specialization equilibrium, it is clear that both firms would be better off if both report truthfully. Unfortunately, this is not guaranteed as the next proposition shows. In what follows, we focus on pure-strategy equilibria.[23]

**Proposition 1.** *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* $c \in [\underline{c}, \bar{c}]$, *two Nash equilibria coexist: either both stations misreport or both report truthfully.*

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[22] As shown in Online Appendix D, this result extends to asymmetric firms—for example, firms that differ in their costs of misreporting or in the number of inspection lines they operate—provided the asymmetry is not too large.

[23] The misreporting game admits mixed-strategy equilibria for some parameter values, but, as explained in Online Appendix D and illustrated by Harsanyi and Selten (1988) in an analogous setting, they are poor predictors of how firms may actually play.

The proposition reveals a coordination problem. Since the two firms split a fixed pool of consumers and follow the same strategy in equilibrium, both would be better off reporting truthfully: misreporting attracts no additional customers but entails a cost—the risk of losing the concession to operate in the market. Individually, however, a firm that misreports while its rival reports truthfully captures additional demand, so each is tempted to cheat. When misreporting is sufficiently cheap, i.e., $c \leq \underline{c}$, this temptation is strictly dominant and both misreport in equilibrium. As cheating costs rise, truthful reporting can be sustained in equilibrium, becoming strictly dominant only when $c \geq \bar{c}$. But even when $c < \bar{c}$, there is hope that firms may be able to coordinate (or tacitly collude, if necessary) on the Pareto-optimal outcome of truthful reporting. Ultimately, this is an empirical matter.

### 2.2.2. Extending the misreporting game to more players

Our theory also has implications for how misreporting could evolve as the number of competitors in the market, $n \geq 2$, increases. 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. In other words, we vary only the number of independent firms owning and operating that capacity. Again, we consider symmetric firms and 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 assuming that $\bar{p} < n\gamma/(n-1)l$.

When all stations play $T$, the payoff of each firm is $\bar{p}/n$, and when they all play $M$ is $(\bar{p}-c)/n$. Consider now the case in which $k$ of the stations play $M$ and the remaining $n-k$ play $T$, with $1 \leq k < n$. The payoff of each truthful-reporting station is

$$\pi(T, M^k, T^{n-k-1}) = \bar{p}\,\tilde{\theta}_k/(n-k) \tag{6}$$

where

$$\tilde{\theta}_k = \frac{\gamma n(n-k)}{lk(n-k)+\gamma n^2} \tag{7}$$

denotes the consumer indifferent between visiting a misreporting station and a truthful-reporting one. The payoff of each of the $k$ misreporting stations is

$$\pi(M, M^{k-1}, T^{n-k}) = (\bar{p}-c)(1-\tilde{\theta}_k)/k. \tag{8}$$

From these payoffs we can establish the following result.

**Proposition 2.** *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. 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. 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 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 exhibit some of this inverted-U pattern. As formally discussed in Online Appendix D, this inverted-U pattern can be amplified by intensive-margin adjustments: as competition intensifies, misreporting firms may also reduce inspection effort, approving more vehicles through faster and more negligent inspections in order to boost demand, but at the cost of greater exposure to sanctions.

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—and hence the no-specialization result—found in the duopoly setting does not necessarily extend to markets with more players.[24] When this occurs, the equilibrium characterization can depart significantly from the duopoly case. In particular, specialization, with some firms playing $T$ and others playing $M$, may arise (see Online Appendix D for an illustration). This makes coordination, or collusion, on truthful reporting much more difficult to achieve.

For tractability, the baseline model assumes that consumers perfectly observe whether a station misreports. In practice, both consumers and the regulator receive noisy signals about a station's leniency. Consumers may learn about a station's reputation through their own experience, through interactions with other drivers, or by observing patterns in pass rates across stations.[25] Similarly, the regulator may flag suspicious behavior based on unusually high approval rates, but faces its own informational frictions in proving misconduct, as discussed above. In Online Appendix D,

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[24] Ours is not the first game in which actions may change from strategic complements to strategic substitutes as a function of some relevant parameter; examples include congestion in a bar (Karp et al., 2007) and the convexity of the cost function in a pricing game (Vives, 2001).

[25] The evidence in Online Appendix E suggests that learning from own experience is relevant. In markets with two or more stations, more than 30% of drivers switch inspection stations between inspections, and the probability of switching increases by 5.1 percentage points after a rejection.

we extend the model to incorporate these features by introducing a probability $\lambda < 1$ that a consumer learns of a firm's misreporting reputation and a probability $\beta < 1$ that a non-compliant car is approved by a misreporting station. The key implications of our theory—summarized in Propositions 1 and 2—carry over to this more general setting. We now take these implications to the data.

## 3. Data

This section describes the data and characterizes the markets used in the empirical analysis. 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 to extract and classify content.[26] 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.[27] These monitors are located across a wide

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

[27] Like Rivera et al. (2024), our focus on PM2.5 responds to the fact that most monitoring stations outside Santiago do

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 recorded cause, and the license plates of the vehicles involved.[28]

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 markets.

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.[29] We exclude from our sample all municipalities in Santiago, Chile's capital, and home to more than seven million people.[30] Figure A.2 of Online Appendix A shows the geographical distribution of the 63 markets created under this definition. The 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

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not keep records of other local pollutants such as carbon monoxide (CO) and nitrogen oxides ($NO_x$).

[28] 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.

[29] Urban areas are identified by the government as contiguous settlements with at least 2,000 inhabitants, basic infrastructure, and urban amenities.

[30] 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.

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.

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. In the next section, we use these data to study how competition among inspection stations affects pass rates and its consequences for air quality and road safety.

## 4. Empirical Analysis

In this section, we start by analyzing the relationship between competition and pass rates. 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

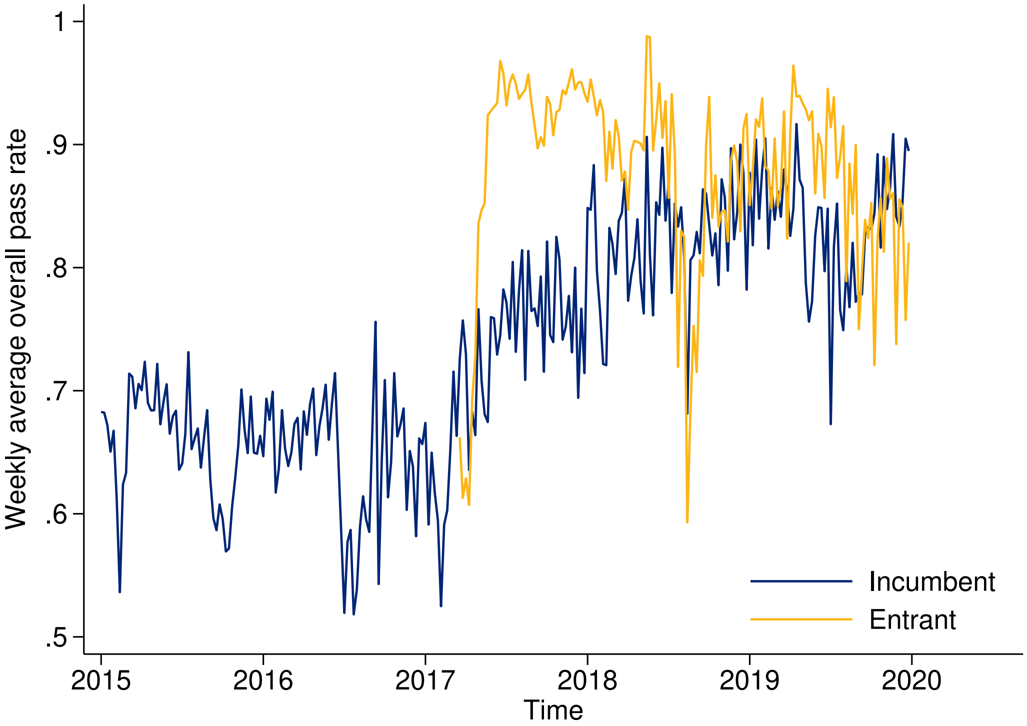
To study how competition shapes firms' incentives to approve vehicle inspections, we exploit within-station variation in the number of competitors over time, driven by exogenous firm entries into relatively concentrated markets. Because inspection procedures are standardized and license plates can be 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 1 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. Following entry, the incumbent's pass rates rose sharply, with both stations converging to a higher level.

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

Figure 1: 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.

quarters before and 12 quarters after the event.[31] This horizon covers nearly all treated markets for the full interval.

We estimate the following model:

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

where $y_{ijt}$ is a binary indicator equal to 1 if vehicle $i$ is approved during an inspection at station $j$ at month $t$. We include fixed effects for license plate, station, and month, denoted by $\delta_i$, $\delta_j$, and $\delta_t$, 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.

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[31] 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.

The coefficients of interest are $\beta_k$, 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 $\beta_k$ 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.[32]

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 2 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. (9) 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 within three years after entry. In both exercises, the control group consists of all stations in inspections from non-treated markets outside 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

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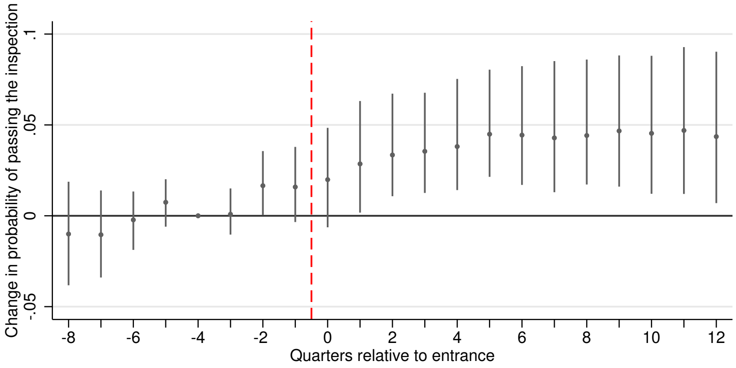
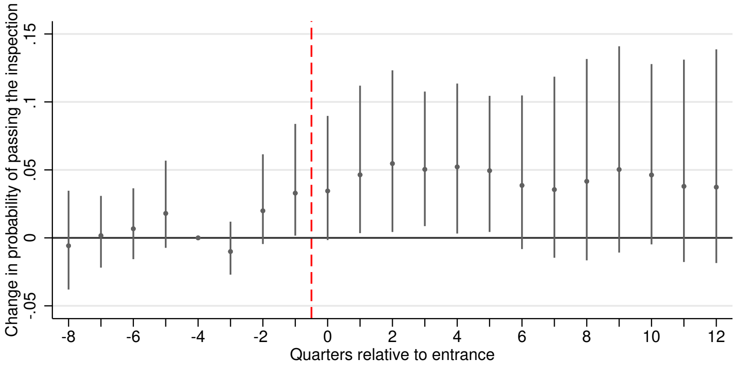
[32] The estimation sample includes 13 treated markets and 49 control markets. Because the number of treated clusters is relatively small, we conduct inference using wild cluster bootstrap procedures, which improve the finite-sample reliability of standard errors and test statistics in settings with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

Figure 2: Competition effect on pass rates

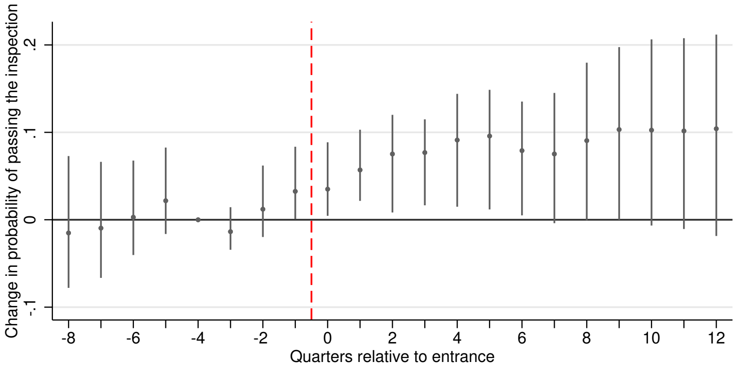
(a) Smog Checks



(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.

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 9, 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 2, providing further support for the baseline findings.

### 4.1.1. How does the extent of competition affect firms' conduct?

Unlike the previous analysis, 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 on firm's conduct than, for example, moving from two to three.

We estimate the following model:

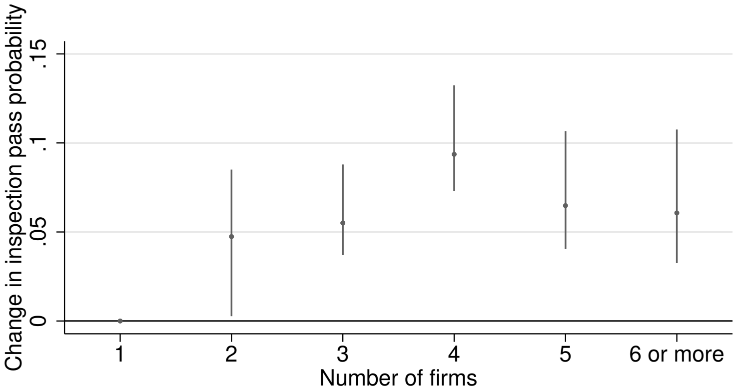
$$y_{ijt} = \delta_i + \delta_j + \delta_t + \sum_k \beta_k \mathbb{1}\{\text{\# of competitors} = k\}_{jt} + \theta_{v(i,t)T(t)} + \phi_j \times t + \varepsilon_{ijt}, \tag{10}$$

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 $\beta_k$, 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 $\beta_k$ captures the change in pass rates relative to monopoly markets. As in equation (9), standard errors are clustered at the market level.

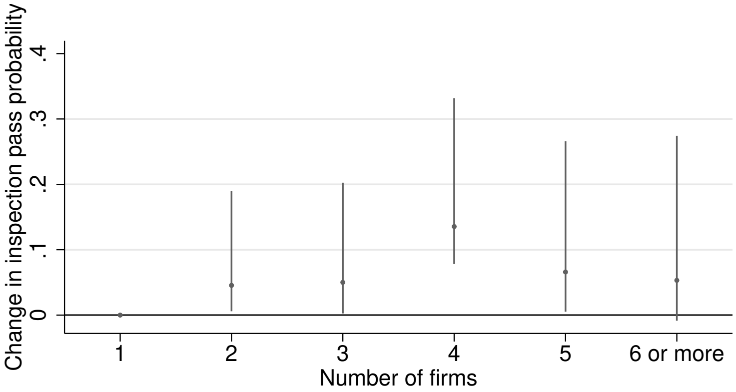
Figure 3 reports the estimated values of $\beta_k$, which capture the causal effect of moving from monopoly to a $k$-firm market on pass rates. We find a large and statistically significant increase when markets transition from monopoly to duopoly. Beyond that point, the marginal effects of additional competitors are much smaller. There is even some support for an inverted-U relationship. 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.

Figure 3: Fixed-effect model with number of competitors

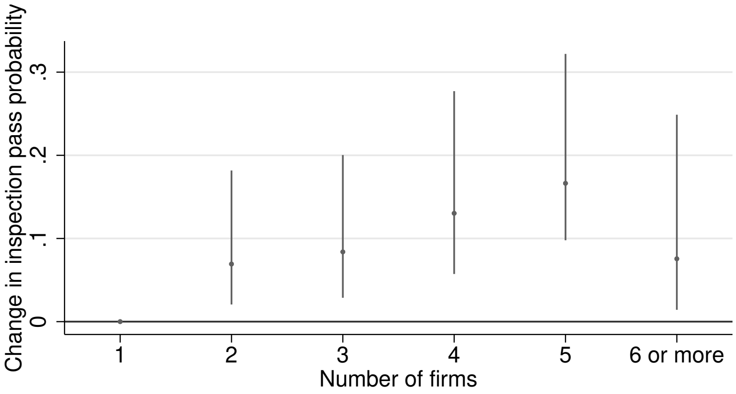
(a) Smog Checks



(b) Safety Checks



(c) Overall Inspection



*Notes.* The plot shows how different levels of competition affect smog check (a), safety check (b), and overall (c) 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.

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 accidents, respectively.[33]

#### 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:

$$\ln(PM_{ijt}) = \gamma_{ij} + \gamma_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \phi_j \times t + \omega'_{ijt}\theta_j + \varepsilon_{ijt}, \quad (11)$$

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[33] 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 linked to fuel efficiency. In addition to PM2.5, local pollutants include 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).

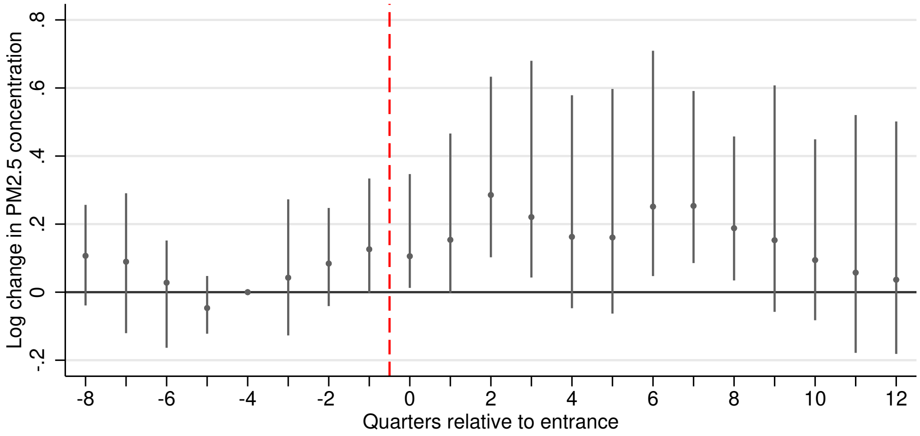
where $\ln(PM_{ijt})$ denotes the logarithm of PM2.5 concentration at hour $i$, day $t$, and monitoring station $j$. We include hour-by-station fixed effects, $\gamma_{ij}$, and day fixed effects, $\gamma_t$, to account for systematic variation in pollution levels within stations and across calendar 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 $\omega_{ijt}$, which includes temperature, precipitation, wind speed, and wind direction measured at hour $i$, day $t$, and station $j$. The vector $\theta_j$ 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 $\beta_k$, 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 $\beta_k$ 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 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 4 depicts the estimated $\beta_k$ coefficients from equation (11). 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. Note that the increase in air pollution shown in Figure 4 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.[34]

To assess whether the rise in PM2.5 is consistent with the implied change in vehicle emissions, we use the inspection emissions data for the 7 markets studied in Figure 4, where the number of firms increased. This approach examines the share of vehicles that would have been rejected before entry but were approved after entry, and uses the emissions values of rejected and approved vehicles to estimate the resulting change in emissions. The details of the estimation are in Online Appendix F. In this "bottom-up approach", we estimate a 36% increase in vehicle emissions over the period. Obviously, vehicle emissions are not the only source of PM2.5. Using data from Santiago, Rizzi and De La Maza (2017) and Barraza et al. (2017) estimate that vehicles account for between 30% and 37% of total PM2.5. This implies that a 36% increase in vehicle emissions would translate into an 11% to 13% increase in total PM2.5.[35]

Figure 4: 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.[36] Our results tell otherwise, that smog checks can lead to persistent reductions in vehicle emissions. In Figure G.1 of Online Appendix G we further test for this possibility by examining whether repairs following a rejection improve outcomes in subsequent

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[34] An alternative explanation for the rise in PM2.5 is that higher pass rates generate more traffic 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.

[35] The passthrough rates estimated by Rizzi and De La Maza (2017) and Barraza et al. (2017) are likely to be higher now, given that industrial emissions and wood burning have declined significantly in recent years.

[36] 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.

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.

#### 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.[37] 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:

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

where $y_{jt}$ represents the outcome of interest in market $j$ at week $t$. We include market and time fixed effects, denoted by $\delta_j$ and $\delta_t$, respectively, along with market-specific linear trends, $\phi_j \times t$.

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[37] 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.

The coefficients of interest are $\beta_k$, 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 anticipation effects, we normalize $k = -4$ (four quarters before entry) as the reference period, so that each $\beta_k$ 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 $\beta_k$ coefficients in Figure 5. 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.

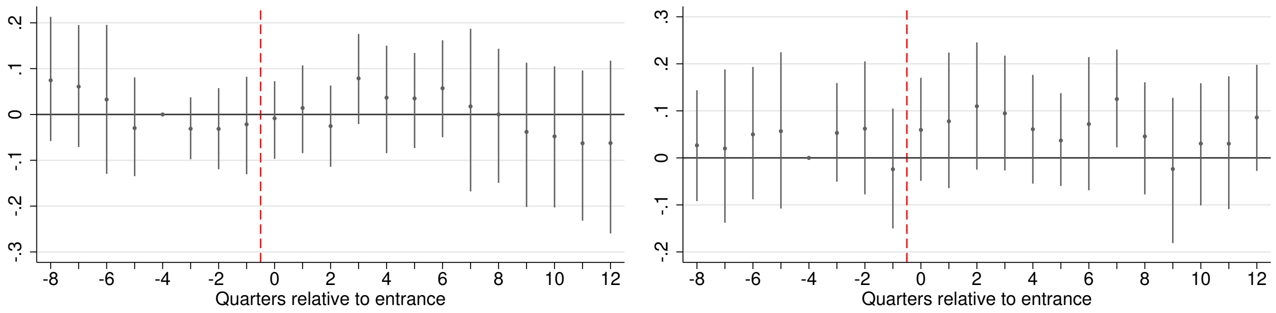
### 4.3. The effects of competition on service quality

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:

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

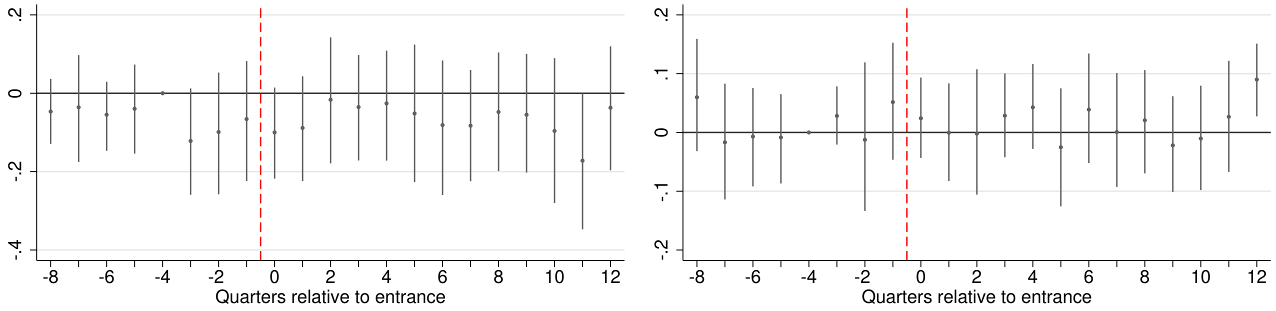
where $y_{jt}$ represents a dummy equal to one if the enforcement agents' weekly report in station $j$ at week $t$ contained a complaint about one of the selected quality issues. Using a large language model, we classified the issues into five categories: inspection line not in operation, inadequate cleaning, missing signage, problem entering the station, and malfunctioning machine. We include station and time fixed effects, denoted by $\delta_j$ and $\delta_t$, respectively, along with station-specific linear trends, $\phi_j \times t$. The coefficients of interest are $\beta_k$, 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 $\beta_k$ 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.

Figure 5: The impact of competition on traffic accidents



(a) Log of number of vehicles in accidents

(b) Mechanical or brake failure accident



(c) Accident with serious injury

(d) Fatal accident

*Notes.* The plots show the estimated coefficients of Equation (12) 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.

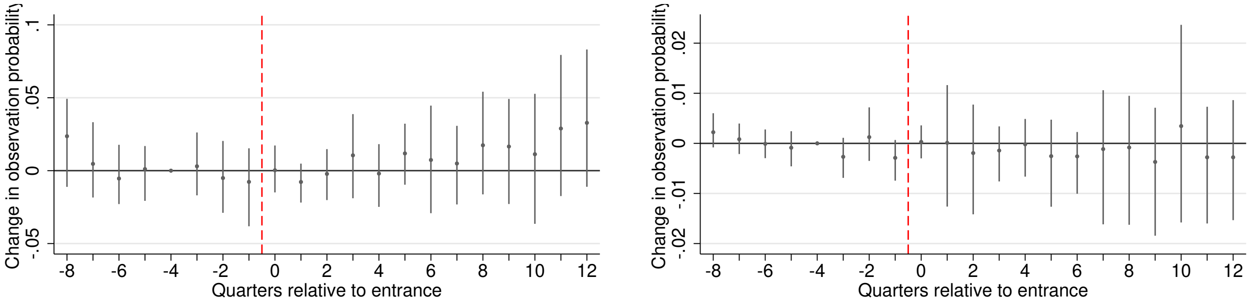
As shown in Figure 6, which presents $\beta_k$ for the five different quality categories, our event-study estimates provide no evidence that increased competition improves these quality indicators. If anything, competition appears to increase the probability of receiving a warning related to problems entering the station.

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

$$\ln(\tau_{jt}) = \delta_j + \delta_t + \beta_{\leq -9}\, z_{j,q(t)\leq -9} + \sum_{k=-8}^{12} \beta_k z_{j,q(t)-k} + \beta_{\geq 13}\, z_{j,q(t)\geq 13} + \phi_j \times t + \varepsilon_{jt}, \tag{14}$$

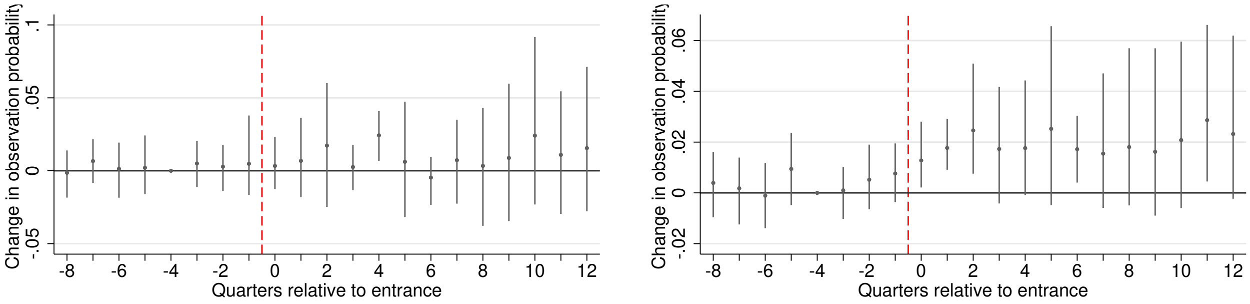
where $\ln(\tau_{jt})$ represents the log of inspection time in minutes. Following previous specifications, we include station and time fixed effects, denoted by $\delta_j$ and $\delta_t$, respectively, along with station-specific linear trends, $\phi_j \times t$. The coefficients of interest are $\beta_k$, 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

Figure 6: Effect of competition on the probability of receiving an enforcement warning on different issues



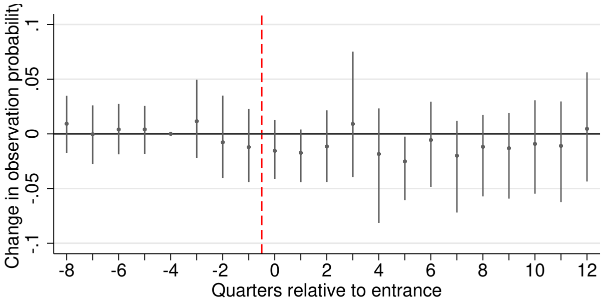
(a) Inspection line not in operation

(b) Inadequate cleaning



(c) Missing signage

(d) Problem entering the station



(e) Malfunctioning machine

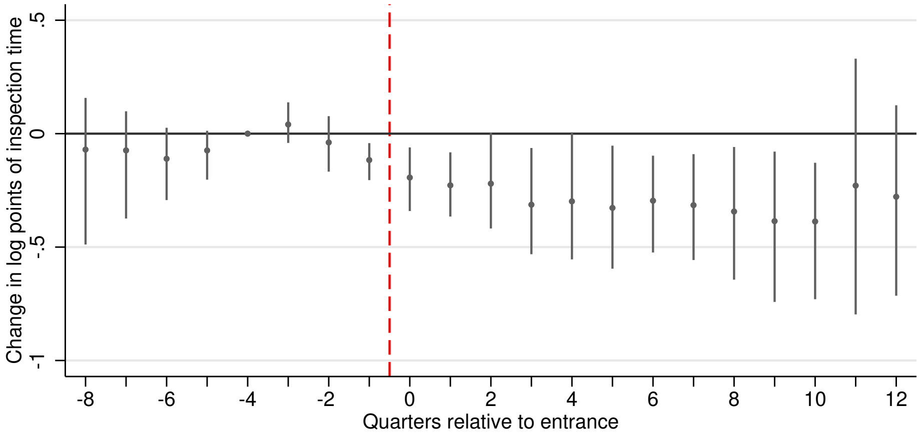
*Notes.* The plot shows the percentage-point change in the probability that a government inspector files an observation in a given week. Each panel focuses on a different issue. Panel (a) reports an inspection line not in operation; Panel (b), inadequate cleaning; Panel (c), missing signage; Panel (d), 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.

of station $j$. To allow for potential anticipation effects, we normalize $k = -4$ (four quarters before entry) as the reference period, so that each $\beta_k$ 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 report the estimated coefficients in Figure 7. 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.[38]

Figure 7: 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

We have shown that introducing competition leads vehicle-inspection stations to approve more cars than they would under a monopoly structure. As a result, polluting vehicles remain on the road

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[38] See footnote 47 for details on the $3 figure.

unrepaired, raising overall air pollution. A natural policy response to limit this misreporting is stricter enforcement. In practice, however, enforcement is already intensive and probably becomes less effective when firms adopt similar noncompliance strategies (Alé-Chilet et al., 2025). While imposing sufficiently large sanctions can, in theory, ensure compliance (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, if at all.

To curb misreporting, both our theory and evidence indicate that delegating each market to a single operator would lower pass rates and emissions. One potential concern with this option is that monopoly stations may also have incentives to misreport, but in the opposite direction. Since a monopolist's demand—and hence its revenue—is largely fixed, it may have incentives to save on inputs and operate under tighter capacity constraints. And to avoid costly mistakes, it may over-diagnose failures. If so, some competition could, in principle, move pass rates closer to their correct levels.

We find this over-rejection possibility unlikely for two reasons. On the one hand, stations cannot charge extra for reinspections and can neither repair cars nor sell auto parts, so if anything they would be more lenient. On the other hand, as documented in Section 4, monopoly stations spend more time, not less, inspecting vehicles than firms in more competitive environments. In what follows, 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.[39] 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

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[39] 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.

average emissions rate for age group $a$ is simply

$$\bar{e}_{a,m}^{\text{monop}} = e_a^0 \tag{15}$$

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

$$\bar{e}_{a,m}^{\text{comp}} \;=\; (1 - \Delta\theta_{am})\, e_a^0 \;+\; \Delta\theta_{am}\, e_a^1. \tag{16}$$

The gap between $\bar{e}_{a,m}^{\text{comp}}$ and $\bar{e}_{a,m}^{\text{monop}}$ 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

$$\bar{e}_{a,m}^{\text{no}} \;=\; p_{am} e_a^0 \;+\; (1 - p_{am}) e_a^1. \tag{17}$$

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 $\bar{e}_{a,m}^{\text{monop}}$, $\bar{e}_{a,m}^{\text{comp}}$, and $\bar{e}_{a,m}^{\text{no}}$ by estimating the key parameters $\Delta\theta_{am}$, $p_{am}$, $e_a^0$, and $e_a^1$. The distortion $\Delta\theta_{am}$ is obtained by estimating the regression model in equation (10), 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 (10), 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 $p_{am}^{\text{obs}}$ 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

$$p_{am} \;=\; p_{am}^{\text{obs}} - \Delta\theta_{am}. \tag{18}$$

For example, if $p_{am}^{\text{obs}} = 0.93$ and $\Delta\theta_{am} = 0.03$, the estimated share of vehicles in good condition is $p_{am} = 0.90$.[40]

Finally, we proxy relative emission rates using hydrocarbon (HC) readings. In particular, we compute $e_a^0$ and $e_a^1$ using average ASM–2525 HC readings under monopoly inspections, conditional on rejection status within each age group:[41]

$$e_a^0 = \mathbb{E}[\text{HC} \mid \text{approved}, a] \qquad e_a^1 = \mathbb{E}[\text{HC} \mid \text{rejected}, a]. \tag{19}$$

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

$$E^s_{\text{SCL}} \;=\; \sum_a N_{a,\text{SCL}}\, d_a\, \bar{e}^s_{a,\,m=4} \qquad E^s_{\text{RCL}} \;=\; \sum_{j \in \text{RCL}} \sum_a N_{aj}\, d_a\, \bar{e}^s_{a,\,m_j} \tag{20}$$

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 imply that delegating each market to a single operator would substantially reduce emissions.

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[40] 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.

[41] Beyond the fact that we are interested in relative values, HC readings happen to be a very good proxy for (local) vehicle emissions. As explained by Barahona et al. (2020), HC emissions 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 ($\text{NO}_x$). 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.

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.

### 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.[42] 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

$$H^s_{\text{SCL}} = h_{\text{SCL}} E^s_{\text{SCL}} \; = \; k^s_{\text{SCL}} \sum_a N_{a,\text{SCL}}\, d_a \qquad H^s_{\text{RCL}} = h_{\text{RCL}} E^s_{\text{RCL}} \; = \; k^s_{\text{RCL}} \sum_{j \in \text{RCL}} \sum_a N_{aj}\, d_a \,, \tag{21}$$

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.[43] Plugging $k^{\text{comp}}_{\text{SCL}}$, $N_{a,SCL}$, and $d_a$ into equation (21), we recover $H^{\text{comp}}_{\text{SCL}}$. Then, using the value of $E^{comp}_s$ computed in equation (20), we estimate $h_{\text{SCL}}$.

Having estimated $h_{\text{SCL}}$, we compute $H^s_{\text{SCL}}$ for the other two counterfactuals of interest, $s =$ no and $s =$ monop, using the corresponding values of $E^s_{\text{SCL}}$ from equation (20). 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)

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

[43] Note that this figure captures the harm from all local pollutants, not just HC. Although it is more than double the figure reported by Holland et al. (2016) for Los Angeles, ¢3.3 per mile after excluding $CO_2$ damages and converting the estimate to 2023 dollars, the difference is not that surprising given that annual PM2.5 concentrations in Santiago are almost 80% higher than in Los Angeles, according to IQAir's 2024 World Air Quality Report. In addition, the average passenger car in Santiago is about 9 years old, compared with about 7 years in Los Angeles, and more polluting than the Ford Focus model used as the reference in their paper.

do not provide an estimate of $k_r^s$ (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, $h_{\text{RCL}} = 0.59 h_{\text{SCL}}$.[44] 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.

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.

On the other hand, the cost of implementing vehicle inspections comprises several components, 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.[45]

Under the assumption of competitive prices in the public tenders, we set the cost of conducting

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[44] 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 $\mu$g/m$^3$ versus 18.5 $\mu$g/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 $\mu$g/m$^3$ on average.

[45] 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.

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.[46] In addition, as a conservative measure, we estimate that individuals in monopoly markets spend an average of two hours per inspection—one hour 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.[47]

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. Concerns with monopoly delegation

Beyond the possibility of over-rejection discussed above, monopoly delegation raises two further concerns. The first is that consumers may experience lower service quality once competitive pressure disappears. We find little support for this: as documented in Section 4, increased competition does not improve the quality indicators recorded in enforcement agents' weekly reports, and the one margin it does affect—inspection times, which fall by 26%—is as consistent with less diligent inspections as with genuine service improvements. Even attributing the entire reduction to service, moving to monopoly delegation would raise drivers' inspection costs by less than $3 per inspection, far too little to offset the environmental benefits quantified above.

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[46] 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.

[47] 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 7. This lowers total time costs per inspection by $3, from $24 to $21 ($= 12 + (1 - 0.26) \times 12$).

The second concern 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 yield higher revenue 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.[48] 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 C). While some concessions have been awarded to local operators bidding for only part of a market, this occurs solely because, under current 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.

### 5.4. Alternatives to monopoly delegation

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.[49] 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 H, we extend our model by introducing station-specific demand shocks.

To illustrate the extended model, suppose consumers are evenly split between the two stations.

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[48] 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.

[49] 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.

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.[50] 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.[51] In Online Appendix H, 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 switching tax never replicates the monopoly-delegation outcome.

## 6. Concluding Remarks

We have shown that delegating each vehicle-inspection market to a single firm eliminates the competitive pressure that drives firms to misreport the environmental and safety qualities of the cars they inspect. This is remarkable because, according to our theory, duopoly markets exhibit strong strategic complementarities, so firms should have every incentive to coordinate (or tacitly collude, if necessary) on truthful reporting. Yet the evidence shows otherwise: misreporting emerges as soon as competition is introduced.

Notably, monopoly delegation is not merely hypothetical: it already exists in some jurisdictions, including Ireland and some regions in Spain. If, for any reason, monopoly delegation is not politically feasible in markets with the scale to support two or more competitors, an alternative

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

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

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 is a natural direction for future research.

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# Online Appendix for:

## Competition and Misconduct in Certification Markets with Externalities

(Not for publication)

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# 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 | Monop. and comp. | Both | Fixed (gov.) | No (under cond.) |
| Spain | Public tender | Monop. and comp. | 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)<br>N | (2)<br>Mean | (3)<br>SD | (4)<br>p10 | (5)<br>p50 | (6)<br>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 |

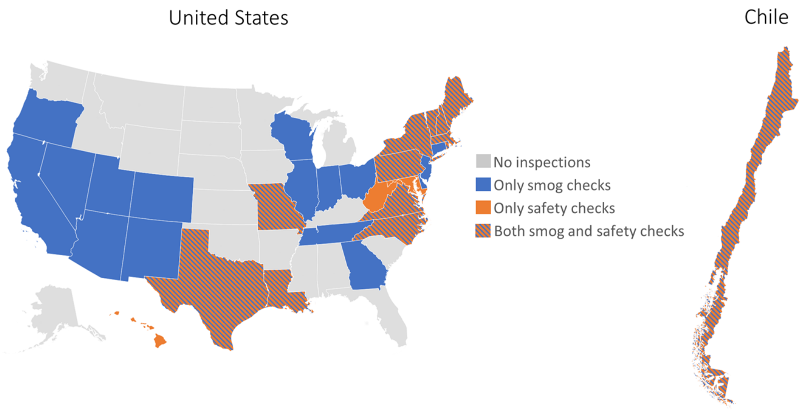
*Notes.* Panel A reports descriptive statistics for first inspections only, excluding reinspections and the Santiago market. Panel B presents variables related to the concession tender process. Panel C summarizes enforcement data, where the model classified each inspection visit into five mutually exclusive 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 |

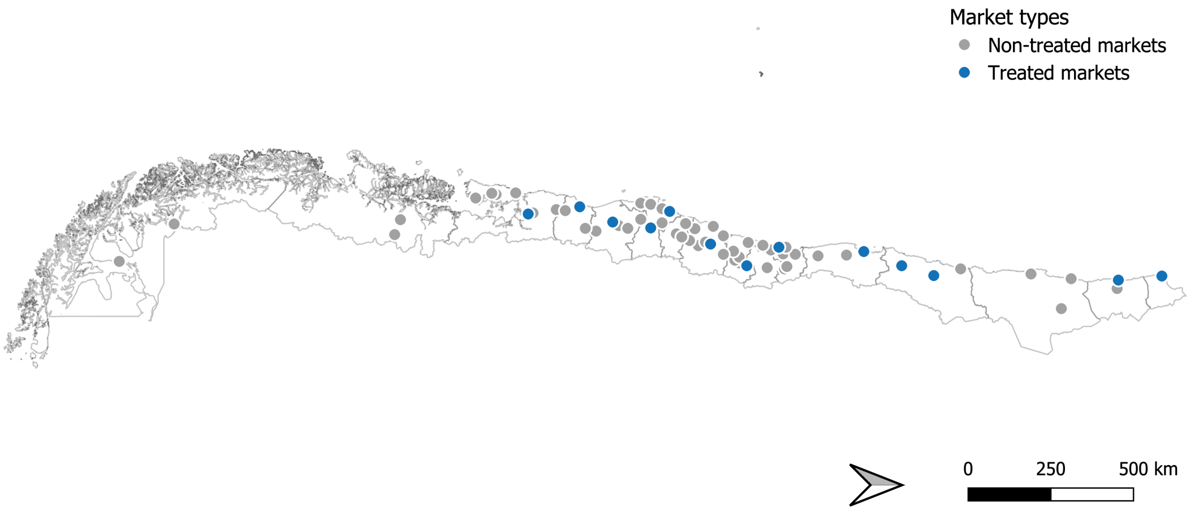
*Notes.* The table summarizes all cases of concession revocation between 2015 and 2024. Revocations occurred either because firms failed to begin operations within the contractual deadline or because they were found issuing inspection certificates without conducting the required tests.

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



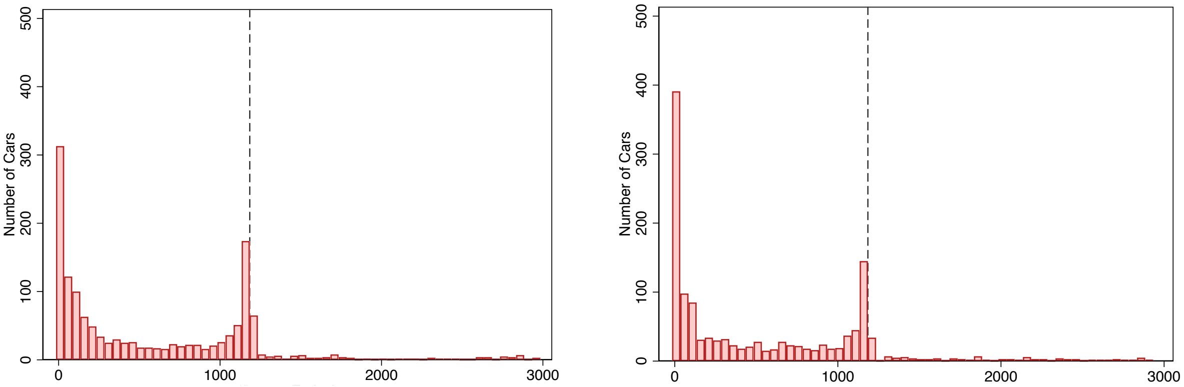
*Notes.* This plot shows the presence of smog and safety checks in the United States and Chile as of December 2024. Chile conducts both smog and safety checks nationwide, while the United States varies by state—some have only smog checks, others only safety checks, some have neither, and some require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

Figure A.2: Smog-check market distribution

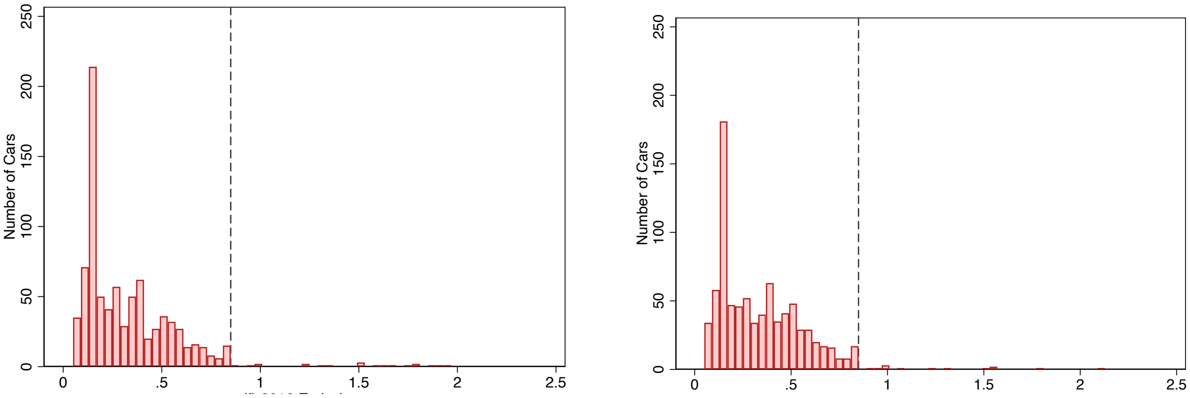


*Notes.* The plot shows the distribution of smog check markets in Chile. Blue circles represent markets that experienced an increase in the number of different competitors during our sample period. Gray circles represent other markets that were active during the same period.

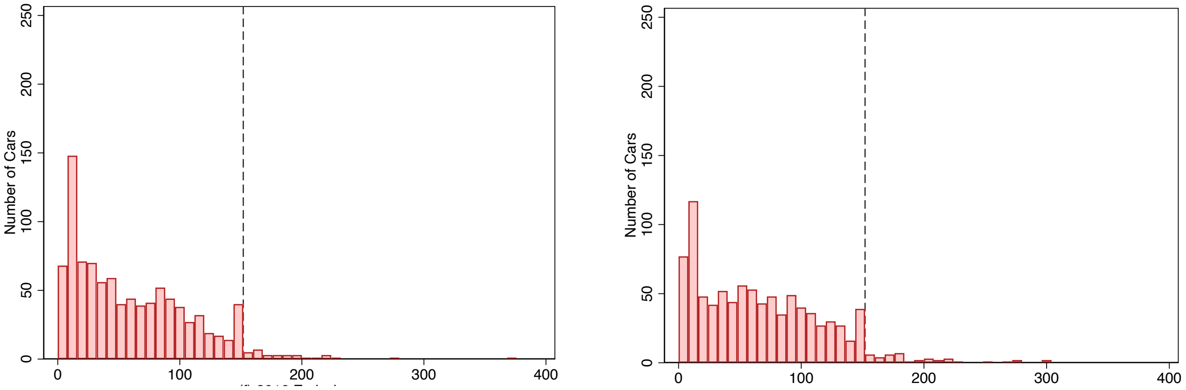
Figure A.3: Bunching evidence using emission test results



(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

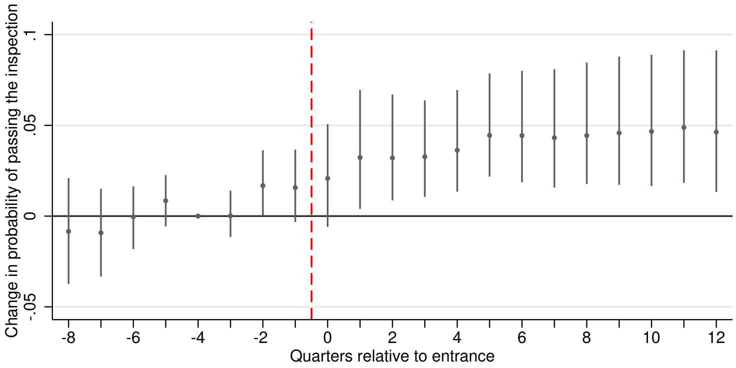


(e) HC 5015 test results for 2016 (f) HC 5015 test results for 2017

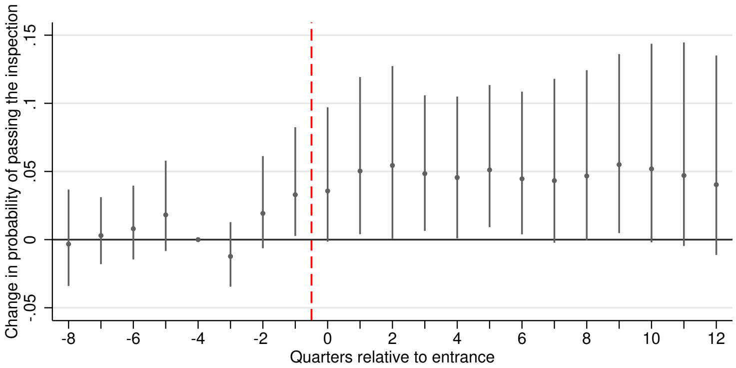
*Notes.* This figure presents evidence from von Dessauer (2019) on bunching in vehicle emissions in Chile. The panels display the distributions of NO, CO, and HC emissions for two vehicle models, the 1998 Hyundai Accent and the 1998 Suzuki Esteem, using data from all inspection stations with available 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

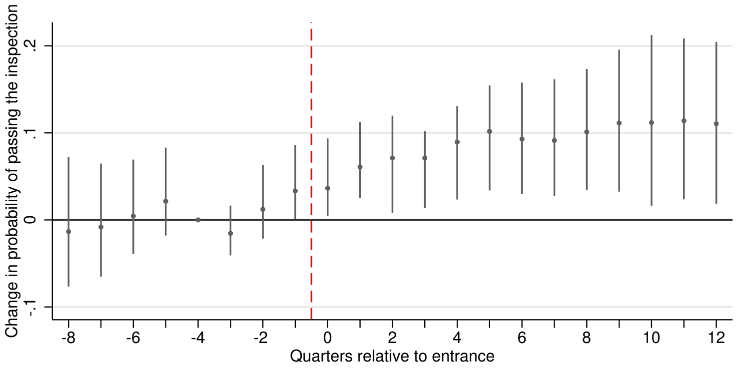
(a) Smog Checks



(b) Safety Checks



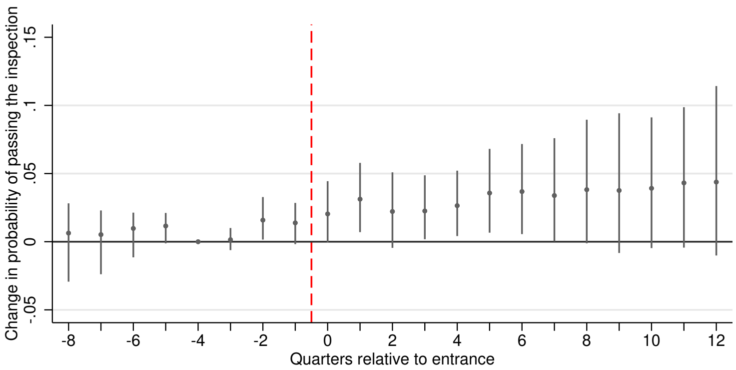
(c) Overall Inspection



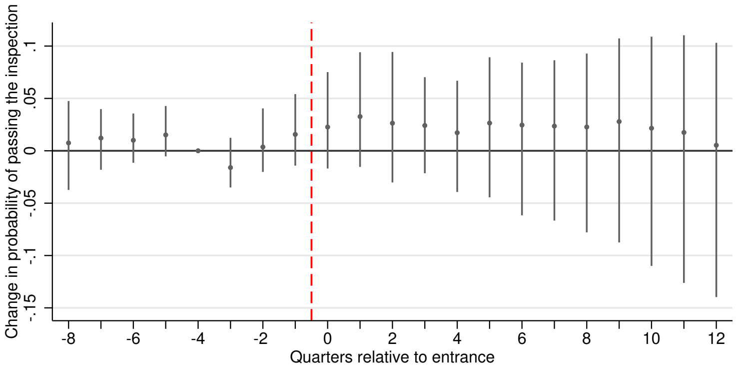
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside 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

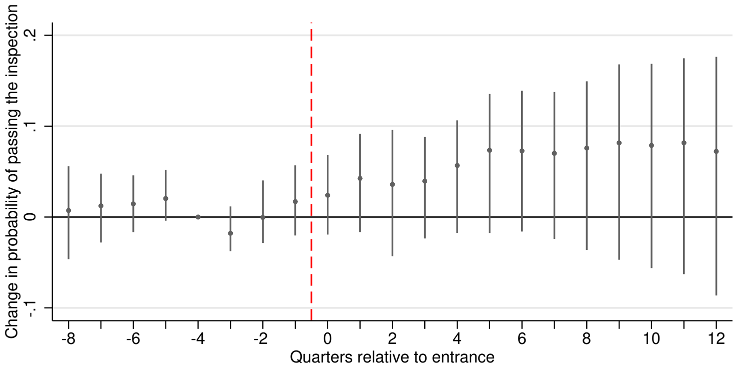
(a) Smog Checks



(b) Safety Checks



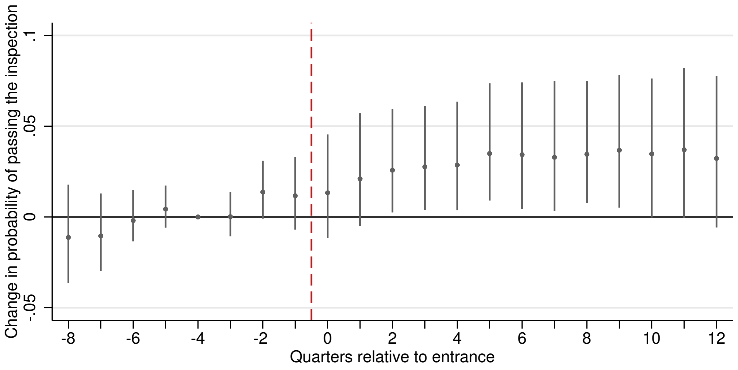
(c) Overall Inspection



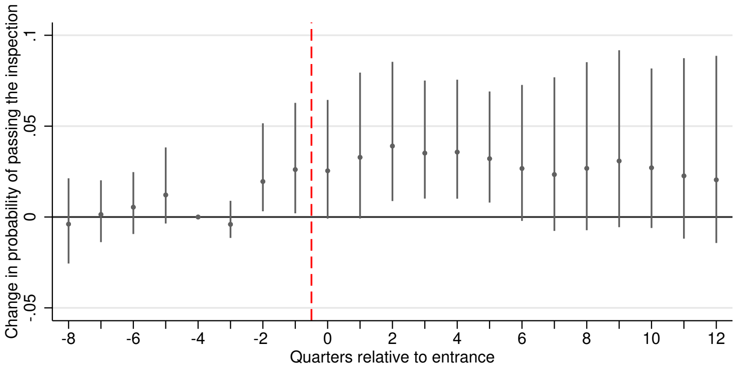
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside 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 three 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

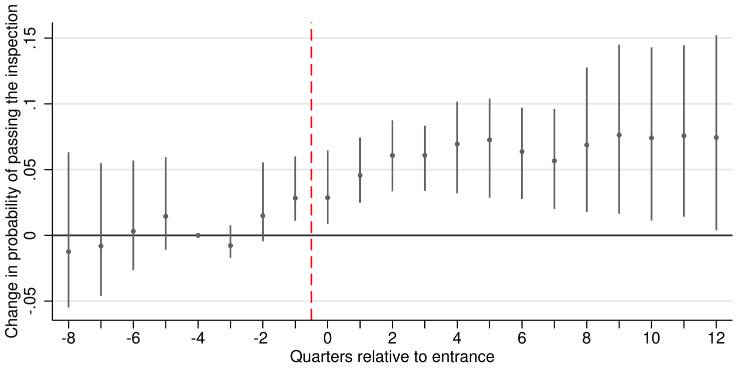
(a) Smog Checks



(b) Safety Checks



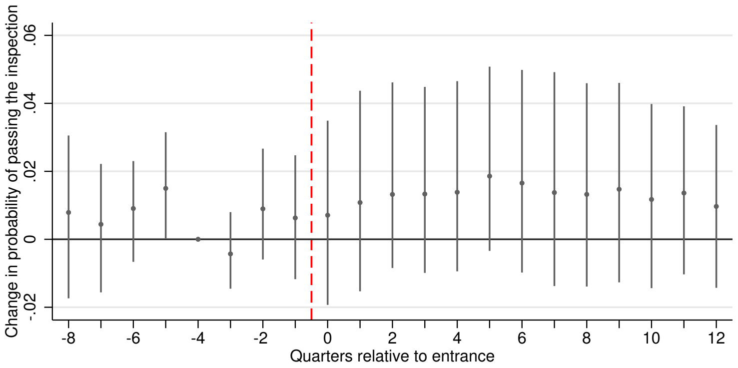
(c) Overall Inspection



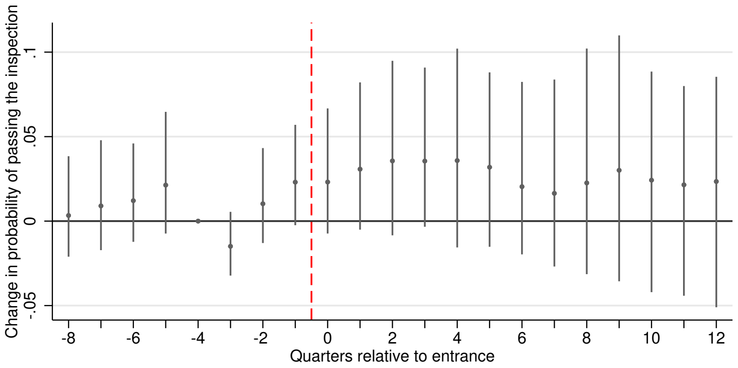
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside 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 three 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

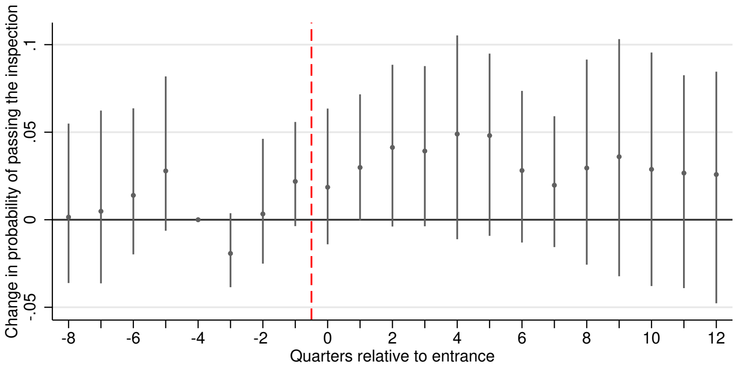
(a) Smog Checks



(b) Safety Checks



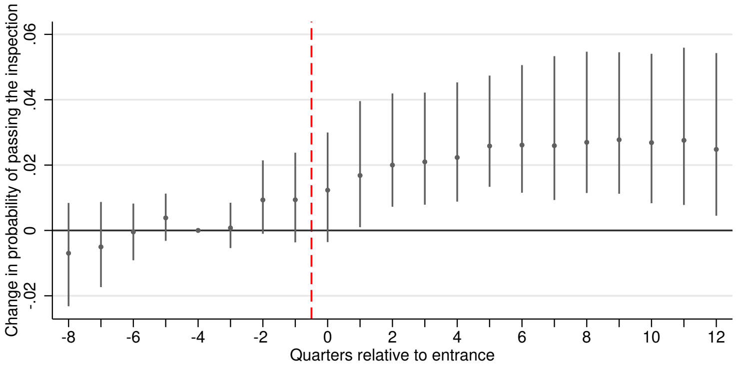
(c) Overall Inspection



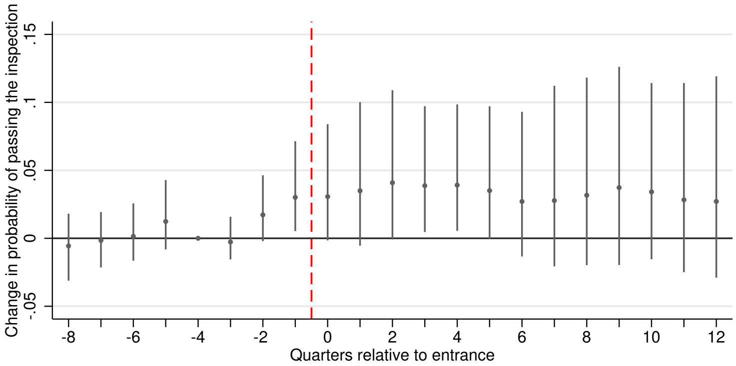
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates using equation 9 without the specific-station linear trends. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. 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 1,000 repetitions.

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

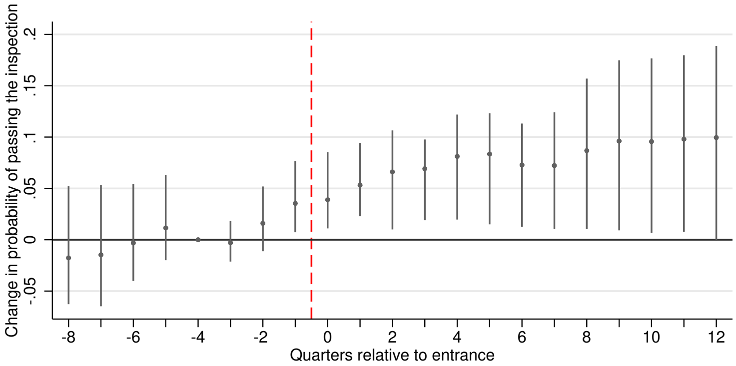
(a) Smog Checks



(b) Safety Checks



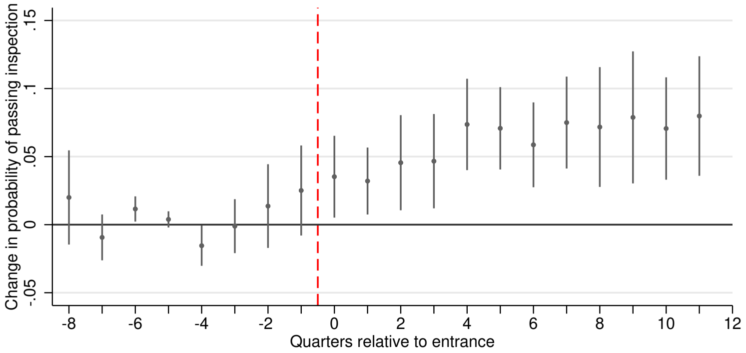
(c) Overall Inspection



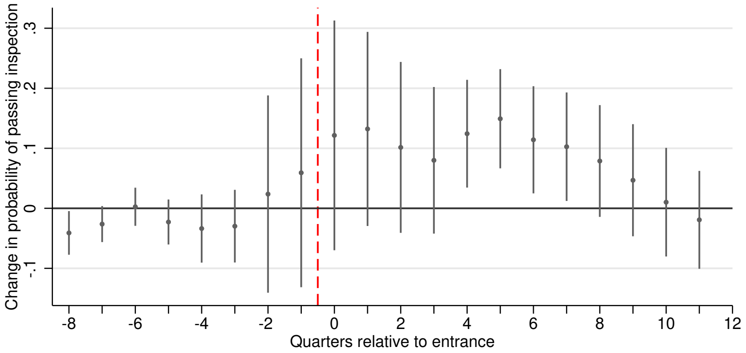
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to allow for potential anticipation effects. We focus only on first inspections in stations outside 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

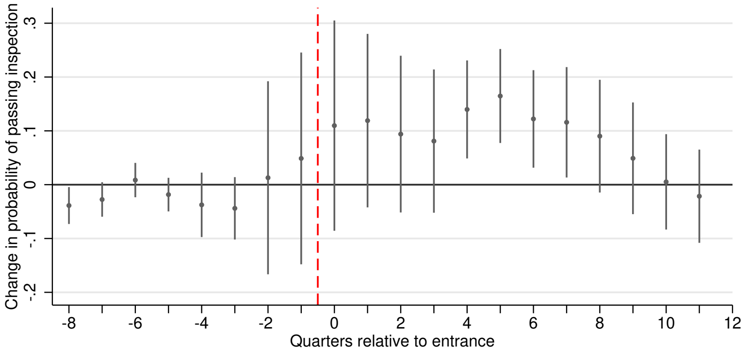
(a) Smog Checks



(b) Safety Checks



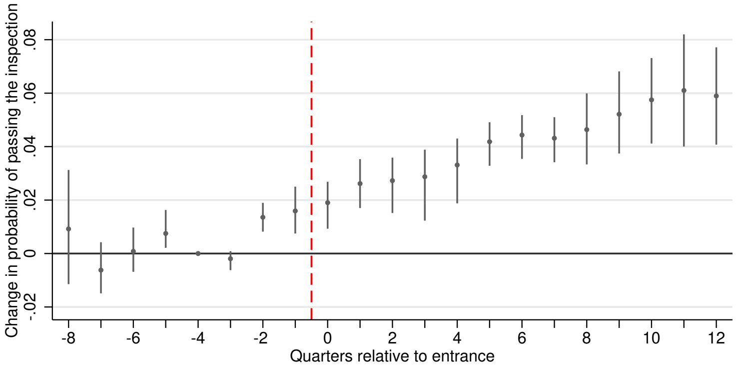
(c) Overall Inspection



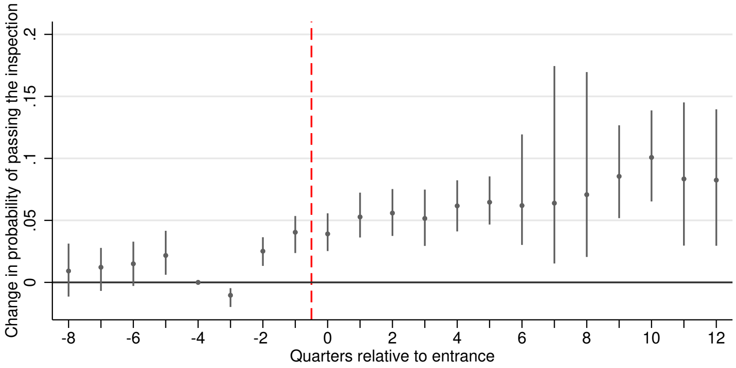
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. We use the repeated cross-sectional version of the estimator in Callaway and Sant'Anna (2021), defining treatment cohorts at the quarterly level. Since this specification does not accommodate 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

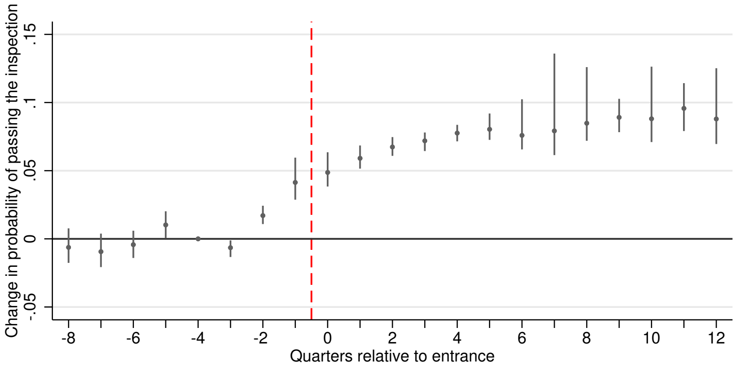
(a) Smog Checks



(b) Safety Checks

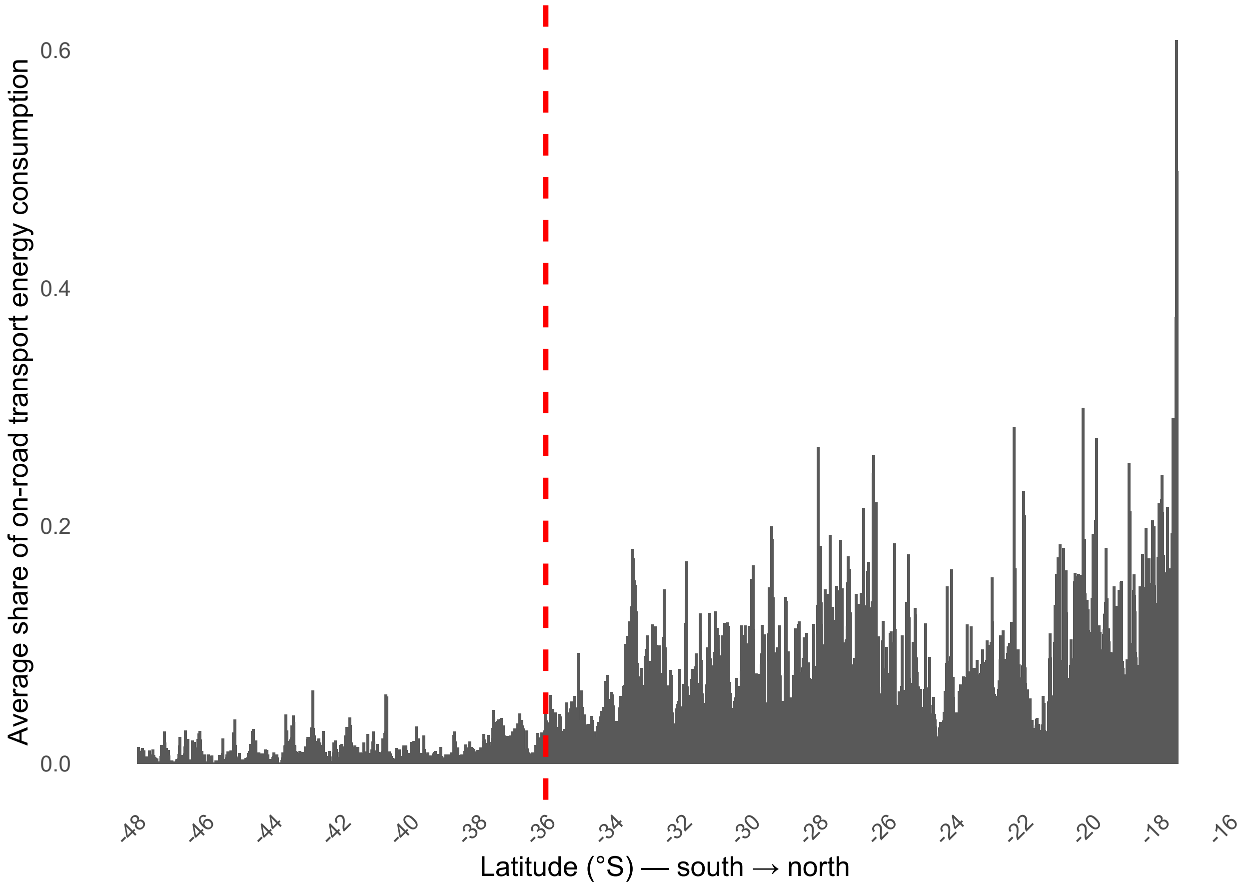


(c) Overall Inspection



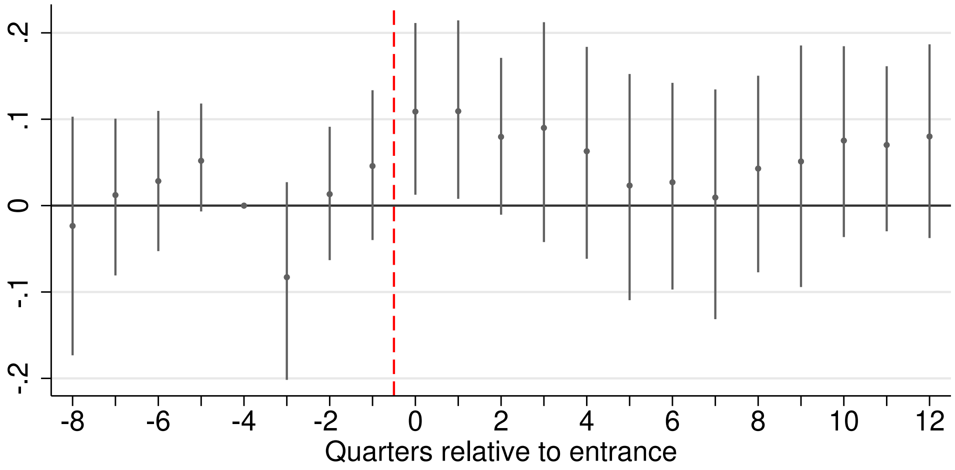
*Notes.* The plot shows the effect of entry on smog check (a), safety check (b), and overall (c) pass rates. We use the estimator in Sun and Abraham (2021), defining treatment cohorts at the monthly level. The dashed red bar marks the time of entry, and we use the 4-quarter lag as the base period to 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:

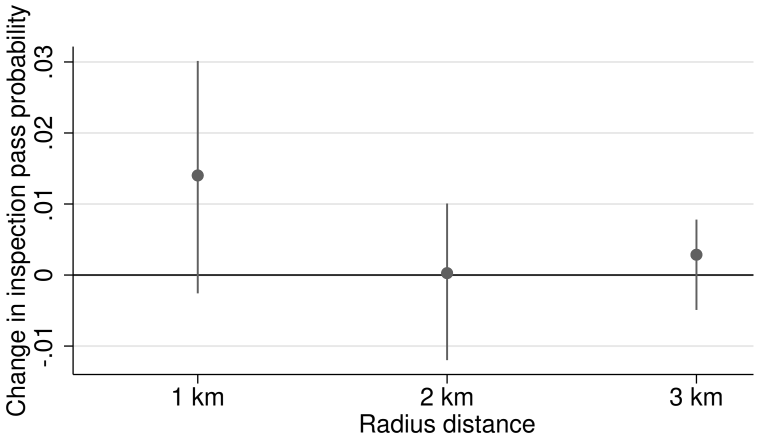
$$y_{ijt} = \delta_i + \delta_j + \delta_t + \beta_k c_{kjt} + \theta_{v(i,t)T(t)} + \phi_j \times t + \varepsilon_{ijt}, \tag{B.1}$$

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 $\beta_k$, 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 $\beta_k$ for two samples: one restricting attention to vehicles more than 10 years old, and another including all vehicles.

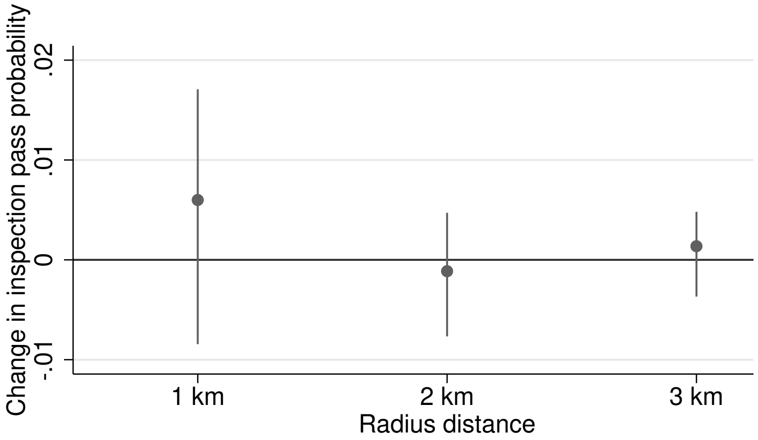
In neither panel do we find evidence that changes in local market concentration are associated with higher pass rates. Using the 1-kilometer radius, the estimated $\beta_k$ is positive but statistically insignificant. We interpret these results as suggesting that the smog check market in Santiago is 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: Testing for adverse selection and diseconomies of scale using auction data

In this section, we first test whether a higher level of competition in the market induces adverse selection by pushing legitimate firms out in favor of less compliant ones; and second, we test whether station scale, measured by the number of operating lines, affects bidders' behavior. The second test assesses 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 location (in terms of comuna) of the stations and 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.

## C.1. Testing for adverse selection

A potential explanation for the increase in leniency induced by competition is adverse selection. If firms expect competition to induce more leniency, the tender process may reduce margins to such an extent that compliant firms are no longer selected in the auction. This mechanism is studied in health care by Diwan et al. (2026). To test whether adverse selection is relevant in our case, we check whether stations' bids differ systematically when they expect to face more competition in the market.

We create a competition variable equal to one if the market has more than one firm in operation three years after the bidding process. 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 market and competition level and assign the same bid to all stations within a given concession. Second, we aggregate to the concession level by taking the mean value of the competition dummy across the markets of all stations in the concession. In both cases, we estimate the following regression:

$$\ln(b_{irt}) = \beta \, Comp_{rt} + \delta_t + \gamma_r + \alpha_i + \varepsilon_{irt}, \tag{C.1}$$

where $\ln(b_{irt})$ is the log bid (in 2023 prices) submitted by firm $i$ in region $r$ in year $t$, and $Comp_{rt}$ is the station-market competition dummy in the first specification and the competition concession-level mean in the second. We include firm, region, and year fixed effects. The coefficient of interest is $\beta$. In Table C.1, we present the results for both regressions. At the station level, the result is close to zero and non-significant. At the concession level, it is positive but non-significant. The estimates suggest that firms do not systematically bid lower when facing more competition in the market, indicating that adverse selection through the exclusion of compliant firms is unlikely to be a driving force in our setting.

Table C.1: Testing for adverse selection

| | Station level | Concession level |
|---|---|---|
| Competition | -0.003 | 0.090 |
| Wild bootstrap 95% CI | [-0.028, 0.017] | [-0.199, 0.262] |
| Observations | 379 | 147 |

*Notes.* The first column presents the estimates at the station level, while the second uses the concession-level aggregation. In both, we add firm, region, and year fixed effects. Standard errors are clustered at the tender level and we use wild bootstraps with 1,000 repetitions. Results suggest no evidence of adverse selection through more aggressive bidding when firms face more competition.

## C.2. Testing for diseconomies of scale

We now test whether markets exhibit diseconomies of scale. To do so, 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.

We again 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:

$$\ln(b_{irt}) = \sum_{m} \beta_m \text{ number of lines}_{m,irt} + \delta_t + \gamma_r + \alpha_i + \varepsilon_{irt}, \tag{C.2}$$

where $\ln(b_{irt})$ is the log bid (in 2023 prices) submitted by firm $i$ in region $r$ in year $t$, and number of $\text{lines}_{m,irt}$ is measured at the station level in the first specification and as the total across stations in the concession in the second. We include firm, region, and year fixed effects. The coefficient of interest is $\beta_m$, 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 C.2 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.

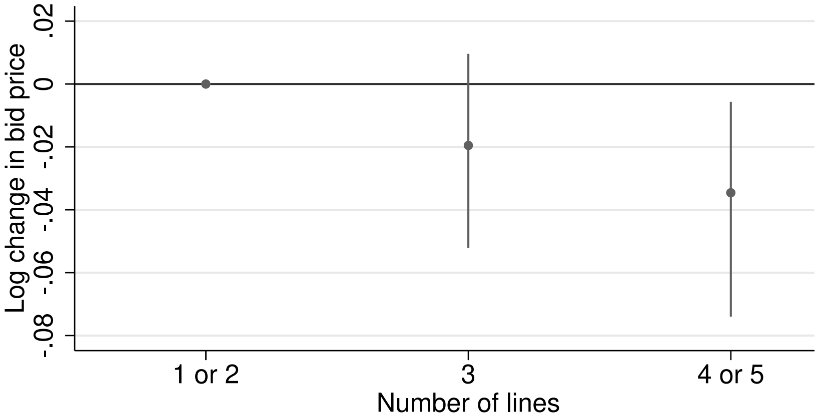
Table C.2: Distribution of number of lines

| 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.

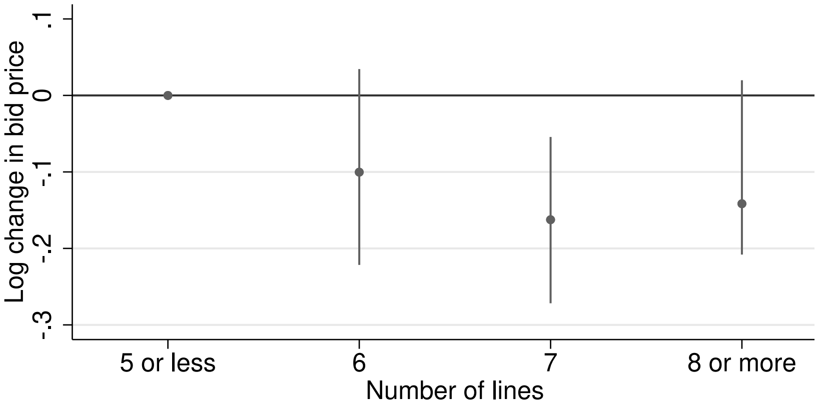
Figure C.1 reports the estimates of $\beta_m$ from the station-level specification, with stations with 1–2 lines as the omitted category. The coefficient for stations with three lines is negative but not statistically significant. For stations with four or five lines, the estimated coefficient is around $-0.04$, implying bids that are about 4% lower than for stations with 1 or 2 lines. Figure C.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 economies of scale, with bids decreasing as the number of lines increases. We therefore find no evidence of diseconomies of scale.

Figure C.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 at the auction level and confidence intervals are computed using wild bootstrap with 1,000 repetitions.

Figure C.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 at the auction level and confidence intervals are computed using wild bootstrap with 1,000 repetitions.

# Appendix D: Additional results for the misreporting model

We provide additional results for the misreporting model. First, we derive the condition that ensures that firms have no incentives to price below the cap in the duopoly game. We also show that results do not change when the cap is not binding. Extending the analysis to $n > 2$ firms is straightforward, so it is omitted. Second, we discuss the existence of a mixed-strategy equilibria in the duopoly game. Again, the discussion can be easily extended to the case of more firms. Third, we discuss the conditions under which the no-specialization result found for the duopoly game remains under firms' asymmetries, whether regarding misreporting costs or inspection capacities. Fourth, we show the emergence of specialization in the three-player case, which in turn helps illustrate the possibility of an inverted-U relationship between competition and misreporting. Fifth, we extend the baseline model along two dimensions: (i) we let the distribution of types to follow a general cumulative distribution function $F(\theta)$, $\theta \in [0, \infty)$, and (ii) we allow for imperfect consumer information about firms' misreporting behavior and partial approval of non-compliant vehicles. We close the section by examining how competition may also affect the intensive margin of misreporting, that is, the fraction of non-compliant vehicles approved by a misreporting firm.

## D.1. Incentives to price below the cap

We study two cases; first the baseline case when firms price at the cap in equilibrium, and second the case in which the cap is not binding.

### D.1.1. Binding cap

We obtain conditions to rule out price deviations below the cap for the duopoly case where car types are uniformly distributed over the unit interval. Since each firm operates $l/2$ inspection lines, the congestion cost of serving demand $Q_i$ is $(2\gamma/l)Q_i$. To simplify notation let $l = 2$.

First suppose both firms follow the same reporting strategy. If firm $i$ charges $p_i \leq \bar{p}$ while its rival charges $\bar{p}$, demand is determined by $-\gamma Q_i - p_i = -\gamma(1 - Q_i) - \bar{p}$, so

$$Q_i(p_i, \bar{p}) = \frac{1}{2} + \frac{\bar{p} - p_i}{2\gamma}.$$

If both firms report truthfully, firm $i$'s payoff is $p_i Q_i(p_i, \bar{p})$, and

$$\left.\frac{\partial p_i Q_i(p_i, \bar{p})}{\partial p_i}\right|_{p_i = \bar{p}} = \frac{1}{2} - \frac{\bar{p}}{2\gamma}.$$

Thus, since the payoff is concave in $p_i$, charging below the cap is not profitable whenever $\bar{p} \leq \gamma$. If, on the other hand, both firms misreport, the payoff is $(p_i - c)Q_i(p_i, \bar{p})$, and the corresponding derivative at the cap is

$$\left.\frac{\partial(p_i-c)Q_i(p_i,\bar{p})}{\partial p_i}\right|_{p_i=\bar{p}}=\frac{1}{2}-\frac{\bar{p}-c}{2\gamma},$$

which is also nonnegative whenever $\bar{p}\leq\gamma$, since $c>0$.

Now consider deviations that also change the reporting choice. Suppose firm $i$ reports truthfully and charges $p_i\leq\bar{p}$, while its rival misreports and charges $\bar{p}$. Since $\theta$ is uniformly distributed, firm $i$'s demand is the indifferent type $\tilde{\theta}$, defined by

$$-\gamma\tilde{\theta}-p_i=\tilde{\theta}-\gamma(1-\tilde{\theta})-\bar{p},$$

so firm $i$'s demand is

$$\tilde{\theta}(p_i,\bar{p})=\frac{\gamma+\bar{p}-p_i}{1+2\gamma}.$$

And from its payoff $\pi_i(T,M)=p_i\tilde{\theta}(p_i,\bar{p})$, we have that

$$\left.\frac{\partial\pi_i(T,M)}{\partial p_i}\right|_{p_i=\bar{p}}=\frac{\gamma-\bar{p}}{1+2\gamma},$$

so this deviation is not profitable whenever $\bar{p}\leq\gamma$.

Finally, suppose firm $i$ misreports and charges $p_i\leq\bar{p}$, while its rival reports truthfully and charges $\bar{p}$. The indifferent type satisfies

$$\tilde{\theta}-\gamma(1-\tilde{\theta})-p_i=-\gamma\tilde{\theta}-\bar{p},$$

so firm $i$'s demand is

$$1-\tilde{\theta}(p_i,\bar{p})=\frac{1+\gamma+\bar{p}-p_i}{1+2\gamma},$$

And from its payoff $\pi_i(M,T)=(p_i-c)(1-\tilde{\theta}(p_i,\bar{p}))$, we have that

$$\left.\frac{\partial\pi_i(M,T)}{\partial p_i}\right|_{p_i=\bar{p}}=\frac{1+\gamma+c-\bar{p}}{1+2\gamma},$$

which is nonnegative whenever $\bar{p}\leq\gamma$.

### D.1.2. A non-binding cap

We extend the previous analysis to the case in which the cap is not binding, or more generally, when there is no cap altogether. We will show that the no-specialization result remains, and so does Proposition 1. Suppose, toward a contradiction, that there is a specialization equilibrium in which one firm misreports and the other reports truthfully. We continue focusing on pure-strategy equilibria. Let the misreporting firm charge $p_M$, and the truthful reporting firm $p_T$. Let $\hat{\theta}$ denote the consumer who is indifferent between the two firms, so that $Q(T,M)=\hat{\theta}=1-Q(M,T)$, where $\hat{\theta}$

is given by the indifference condition

$$\hat{\theta} - \gamma(1-\hat{\theta}) - p_M = -\gamma\hat{\theta} - p_T.$$

Because the cap is not binding (the case in which it could be binding for just one firm is an intermediate case with no new implications), equilibrium prices can be found from the first order conditions that result from firms' maximization problems, $\max_{p_T}\ p_T\hat{\theta}$ and $\max_{p_M}\ (p_M - c)(1-\hat{\theta})$, respectively. In equilibrium both firms serve some demand, i.e., $\hat{\theta} < 1$. Solving we obtain,

$$p_T = \frac{1+3\gamma+c}{3},$$

and

$$p_M = \frac{2+3\gamma+2c}{3}.$$

Given that its product is of higher (private) quality, it is not surprising that the misreporting firm is charging a higher price.

Now compute the optimal deviations from this equilibrium candidate. First consider the deviation of the misreporting firm. If it deviates to truthful reporting, it is optimal to simultaneously change its price to $r$ and obtain a deviation payoff of

$$r\left(\frac{1}{2} + \frac{p_T - r}{2\gamma}\right),$$

which is maximized at $r = r^*(p_T) = (p_T + \gamma)/2$. Thus, the no-deviation condition for the misreporting firm reduces to

$$8\gamma(2+3\gamma-c)^2 \geq (1+2\gamma)(1+6\gamma+c)^2.$$

Note that $2+3\gamma-c > 0$ from the condition that both firms serve some demand in equilibrium.

Consider now the truthful reporting firm. If it deviates to misreport, it does so by also charging $r = r^*(p_M) = (p_M + \gamma + c)/2$. Thus, the no-deviation condition for the truthful reporting firm reduces to

$$8\gamma(1+3\gamma+c)^2 \geq (1+2\gamma)(2+6\gamma-c)^2.$$

Again, note that $2+6\gamma-c > 0$ from the condition that both firms serve some demand in equilibrium. For both no-deviation conditions to hold, there must exist a value of $c$ such that

$$2+6\gamma-K(1+3\gamma) \leq c(1+K) \leq K(2+3\gamma)-(1+6\gamma).$$

where $K = \sqrt{8\gamma/(1+2\gamma)}$, or more simply, that

$$(1+4\gamma)^2 \leq 8\gamma(1+2\gamma).$$

which clearly cannot hold for any $\gamma > 0$. Therefore, there is no specialization equilibrium even when firms are free to choose prices.

### D.2. Mixed strategies

It is clear that the duopoly game admits an equilibrium in mixed strategies within the multiplicity region $[\underline{c}, \bar{c}]$. As illustrated by Harsanyi and Selten (1988) in an analogous setting, however, this equilibrium is a poor predictor of how firms may actually play. It displays discontinuities around the thresholds $\underline{c}$ and $\bar{c}$ that defy intuition. 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 $T$. 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.

In our case, 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. 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 $M$.[1] 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).

### D.3. Firms' asymmetries

Here we discuss the conditions under which the no-specialization result found for the duopoly game remains under firms' asymmetries, whether regarding misreporting costs or inspection capacities. First, allow firms to have different misreporting costs, and label them so that $c_L \leq c_H$. The only candidate specialization equilibrium is one in which the low-cost firm plays $M$ and the high-cost firm plays $T$. So consider the candidate equilibrium $(M_L, T_H)$. As before, the indifferent

---

[1] 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 $c > \hat{c}$.

consumer is

$$\tilde{\theta} = \frac{2\gamma}{l + 4\gamma},$$

For $(M_L, T_H)$ to be an equilibrium, the low-cost firm must prefer misreporting to deviating to truthful reporting:

$$(\bar{p} - c_L)(1 - \tilde{\theta}) \geq \frac{\bar{p}}{2},$$

and the high-cost firm must prefer truthful reporting to deviating to misreporting:

$$\bar{p}\tilde{\theta} \geq \frac{\bar{p} - c_H}{2}.$$

These two conditions are equivalent to

$$c_L \leq \frac{\bar{p}l}{2l + 4\gamma} \qquad \text{and} \qquad c_H \geq \frac{\bar{p}l}{l + 4\gamma}.$$

Hence, a sufficient condition to rule out specialization is

$$c_H - c_L < \frac{\bar{p}l}{l + 4\gamma} - \frac{\bar{p}l}{2l + 4\gamma} = \frac{\bar{p}l^2}{(l + 4\gamma)(2l + 4\gamma)}.$$

Allow now firms to have different inspection capacities, while keeping the misreporting cost equal across firms. Firm $L$ owns a fraction $\alpha < 1/2$ of the inspection lines, while firm $H$ owns the remaining fraction $1 - \alpha$. Thus, if firm $i$ serves demand $Q_i$, its congestion cost is

$$\frac{\gamma Q_L}{\alpha l} \quad \text{for firm } L, \qquad \text{and} \qquad \frac{\gamma Q_H}{(1 - \alpha)l} \quad \text{for firm } H.$$

The only specialization equilibrium that can potentially arise is one in which the small-capacity firm misreports and the large-capacity firm reports truthfully. Consider then the candidate equilibrium $(M_L, T_H)$. The indifferent consumer satisfies

$$-\frac{\gamma\tilde{\theta}}{(1 - \alpha)l} = \tilde{\theta} - \frac{\gamma(1 - \tilde{\theta})}{\alpha l},$$

so

$$\tilde{\theta} = \frac{\gamma(1 - \alpha)}{\gamma + l\alpha(1 - \alpha)}.$$

Hence,

$$Q_H = \tilde{\theta}, \qquad Q_L = 1 - \tilde{\theta} = \frac{\alpha[\gamma + l(1 - \alpha)]}{\gamma + l\alpha(1 - \alpha)}.$$

For $(M_L, T_H)$ to be an equilibrium, firm $L$ must prefer misreporting to deviating to truthful reporting,

$$(\bar{p} - c)Q_L \geq \bar{p}\alpha,$$

and firm $H$ must prefer truthful reporting to deviating to misreporting,

$$\bar{p}Q_H \geq (\bar{p} - c)(1 - \alpha).$$

These two conditions are equivalent to

$$\frac{\bar{p}l\alpha(1-\alpha)}{\gamma + l\alpha(1-\alpha)} \leq c \leq \frac{\bar{p}l(1-\alpha)^2}{\gamma + l(1-\alpha)}.$$

Therefore, no-specialization remains as long as

$$\gamma(1 - 2\alpha) < l\alpha^2(1 - \alpha).$$

### D.4. Equilibrium with three players

Figure D.1 illustrates the equilibrium for $n = 3$ across different values of $c$ and $\gamma > 2l/9$. If $\gamma \leq 2l/9$, $\underline{c}(3) \leq \bar{c}(3)$, and the equilibrium characterization follows that of the duopoly case: all three stations play $M$ whenever $c < \bar{c}(3)$ and $T$ whenever $c > \underline{c}(3)$, with both outcomes coexisting between the thresholds. However, if $\gamma > 2l/9$ 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 D.1 also illustrates the possibility of an inverted-U relationship between competition and misreporting. A monopolist ($n = 1$) always plays $T$. Now suppose that $\gamma > 2l$ and

$$c \in \left(\frac{2\bar{p}l}{2l + 9\gamma}, \frac{\bar{p}l}{2l + 4\gamma}\right).$$

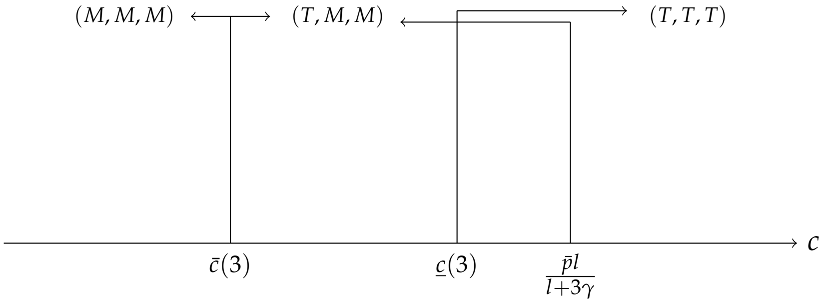
The upper bound implies that, in the duopoly game ($n = 2$), misreporting is a dominant strategy, so both firms play $M$. The lower bound, together with $\gamma > 2l$, places the three-player game in panel (b) of the figure, where $(T, M, M)$ is the equilibrium. Hence, as the number of competitors increases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

### D.5. General distribution, learning and partial misconduct

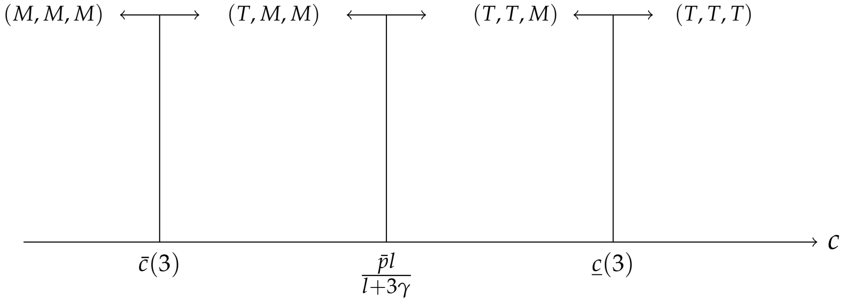
Here we extend the baseline model of Section 2 along two dimensions. First, we generalize the distribution of types from the uniform to a general cumulative distribution function $F(\theta)$, with $\theta \in [0, \infty)$. Second, we allow for imperfect consumer information about firms' misreporting behavior

Figure D.1: Equilibrium in a three-player market

(a) $\frac{2l}{9} < \gamma < \frac{2l}{3}$



(b) $\gamma > \frac{2l}{3}$



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

and partial approval of non-compliant vehicles. Specifically, we let $\lambda \in (0, 1]$ denote the probability that a consumer learns of a firm's misreporting reputation and $\beta \in (0, 1]$ denote the fraction of non-compliant cars that a misreporting firm approves.[2] The baseline model in the main text corresponds to the special case $\lambda = \beta = 1$ with $F$ uniform on $[0, 1]$. We use this extended model to characterize Propositions 1 and 2.

Consider first the duopoly case. Consumers who do not learn about misreporting split equally across firms. If one firm plays $M$ and the other plays $T$, demands are

$$Q(M, T) = \frac{1-\lambda}{2} + \lambda[1 - F(\tilde{\theta})], \qquad Q(T, M) = \frac{1-\lambda}{2} + \lambda F(\tilde{\theta}),$$

where the indifferent consumer satisfies

$$\beta\tilde{\theta} = \gamma\left[Q(M, T) - Q(T, M)\right] = \lambda\gamma\left[1 - 2F(\tilde{\theta})\right].$$

---

[2] The regulator may learn of a station's misreporting reputation, as consumers may, or observe unusually high approval rates that help flag suspicious behavior. Such evidence, however, is unlikely by itself to provide sufficient grounds for concession revocation. Revocation would require harder evidence, such as issuing certificates for cars that were not inspected, as has occurred in the past; approving cars that leave the station in evidently poor condition; or manipulating testing equipment.

Hence $F(\tilde{\theta}) < 1/2$, so a misreporting firm attracts more demand than a truthful-reporting firm whenever the other firm reports truthfully.

The no-specialization result continues to hold. A candidate specialization equilibrium requires

$$(p-c)Q(M,T) \geq \frac{p}{2} \qquad \text{and} \qquad pQ(T,M) \geq \frac{p-c}{2}.$$

These two inequalities are equivalent to

$$c \leq \underline{c} \equiv \frac{p\lambda[1-2F(\tilde{\theta})]}{1+\lambda[1-2F(\tilde{\theta})]} \qquad \text{and} \qquad c \geq \bar{c} \equiv p\lambda[1-2F(\tilde{\theta})].$$

Since $\underline{c} < \bar{c}$, the two conditions cannot hold simultaneously. Thus, as in the baseline model, a pure-strategy equilibrium in which one firm misreports and the other reports truthfully cannot exist.

The equilibrium characterization in Proposition 1 is also unchanged, except that the cutoffs are now given by the expressions above. Truthful reporting by both firms is an equilibrium if $c \geq \underline{c}$, whereas misreporting by both firms is an equilibrium if $c \leq \bar{c}$. Therefore, when $c \in [\underline{c}, \bar{c}]$, both symmetric equilibria coexist.

The same logic extends to $n \geq 2$ firms. Suppose $k \in \{1, \ldots, n-1\}$ firms play $M$ and the remaining $n-k$ play $T$. With total inspection capacity fixed, the indifferent consumer satisfies

$$\beta\tilde{\theta}_k = \frac{\gamma n \lambda}{2}\left[\frac{1-F(\tilde{\theta}_k)}{k} - \frac{F(\tilde{\theta}_k)}{n-k}\right],$$

or equivalently

$$\tilde{\theta}_k = \frac{\gamma n \lambda}{2\beta}\frac{n-k-nF(\tilde{\theta}_k)}{k(n-k)}.$$

The demand of each truthful-reporting firm is

$$Q(T, M^k, T^{n-k-1}) = \frac{1-\lambda}{n} + \frac{\lambda F(\tilde{\theta}_k)}{n-k},$$

and the demand of each misreporting firm is

$$Q(M, M^{k-1}, T^{n-k}) = \frac{1-\lambda}{n} + \frac{\lambda[1-F(\tilde{\theta}_k)]}{k}.$$

It follows that the all-truthful equilibrium is sustained whenever

$$c \geq \underline{c}(n) \equiv \frac{p\lambda\left[n(1-F(\tilde{\theta}_1))-1\right]}{1+\lambda\left[n(1-F(\tilde{\theta}_1))-1\right]},$$

while the all-misreporting equilibrium is sustained whenever

$$c \le \bar{c}(n) \equiv p\lambda \left[1 - nF(\tilde{\theta}_{n-1})\right].$$

Thus, the general model preserves the structure of the baseline results. The parameters $F$, $\lambda$, and $\beta$ affect the location of the cutoffs, but not the underlying strategic logic: misreporting boosts demand when rivals report truthfully, while truthful reporting saves the expected cost of sanctions when rivals misreport.

## D.6. Intensive margin of misconduct

Consider $n \ge 2$ misreporting firms. To save on notation normalize the inspection capacity to $l = 2$. Firm $i$ chooses $\beta_i$, while its $n-1$ rivals choose a common value $\beta$. Misreporting costs are given by $c(\beta_i)$, with $c'(\beta) > 0$, $c''(\beta) \ge 0$, and $c(\beta) < \bar{p}$. We show that the symmetric equilibrium intensity $\beta(n)$ increases with the number of firms.

Let $Q_i$ denote firm $i$'s demand. If firm $i$ chooses $\beta_i$ and its rivals choose $\beta$, then its demand is given by

$$Q_i(\beta_i, \beta; n) = \frac{1-\lambda}{n} + \lambda\left[1 - F(\theta_i)\right],$$

and that of each of its rivals by

$$Q_j(\beta_i, \beta; n) = \frac{1-\lambda}{n} + \lambda \frac{F(\theta_i)}{n-1},$$

where $\theta_i$ is the consumer indifferent between firm $i$ and one of its rivals:

$$\beta_i\theta_i - \frac{\gamma n}{2} Q_i = \beta\theta_i - \frac{\gamma n}{2} Q_j,$$

or equivalently

$$(\beta_i - \beta)\theta_i = \frac{\gamma n \lambda}{2(n-1)}\left[n - 1 - nF(\theta_i)\right]. \tag{D.1}$$

At a symmetric profile, $\beta_i = \beta$, equation (D.1) implies

$$F(\theta_n) = 1 - \frac{1}{n}, \qquad \theta_n = F^{-1}\left(1 - \frac{1}{n}\right),$$

and each firm serves $1/n$ of the market. Implicitly differentiating (D.1) with respect to $\beta_i$, and evaluating at symmetry, gives

$$\left.\frac{\partial \theta_i}{\partial \beta_i}\right|_{\beta_i=\beta} = -\frac{2(n-1)\theta_n}{\gamma n^2 \lambda f(\theta_n)}.$$

Therefore,

$$\left.\frac{\partial Q_i}{\partial \beta_i}\right|_{\beta_i=\beta} = -\lambda f(\theta_n)\left.\frac{\partial \theta_i}{\partial \beta_i}\right|_{\beta_i=\beta} = \frac{2(n-1)\theta_n}{\gamma n^2}. \tag{D.2}$$

Firm $i$'s payoff is

$$\pi_i(\beta_i, \beta; n) = [\bar{p} - c(\beta_i)]\, Q_i(\beta_i, \beta; n).$$

Using (D.2), the symmetric first-order condition becomes

$$\frac{\bar{p} - c(\beta(n))}{c'(\beta(n))} = \frac{\gamma n}{2(n-1)\theta_n}.$$

Define

$$H(\beta) \equiv \frac{\bar{p} - c(\beta)}{c'(\beta)}, \qquad A(n) \equiv \frac{\gamma n}{2(n-1)F^{-1}(1 - 1/n)}.$$

Then

$$H(\beta(n)) = A(n). \tag{D.3}$$

Since $c' > 0$, $c'' \geq 0$, and $c(\beta) < \bar{p}$,

$$H'(\beta) = -1 - \frac{[\bar{p} - c(\beta)]c''(\beta)}{[c'(\beta)]^2} < 0.$$

Moreover, because

$$\theta_n' = \frac{1}{n^2 f(\theta_n)} > 0,$$

we have that

$$\frac{A'(n)}{A(n)} = \frac{1}{n} - \frac{1}{n-1} - \frac{\theta_n'}{\theta_n} < 0,$$

so $A'(n) < 0$.

Differentiating (D.3) with respect to $n$ yields $H'(\beta(n))\beta'(n) = A'(n)$, and therefore $\beta'(n) > 0$.

## Appendix E: Consumer learning from inspection outcomes

In this section we explore how consumers learn about station misconduct by examining their station-switching dynamics across inspection cycles. Specifically, we ask whether being rejected at a station induces a vehicle to inspect at a different station in the following cycle.

As a baseline, Table E.1 shows that roughly 33% of approved vehicles switch stations between consecutive cycles, indicating substantial exploration across stations within a market.[3] Switching rates also vary by vehicle age: among approved vehicles, older cars switch less often than younger ones, consistent with either greater loyalty to a familiar station or a more developed information set built up over previous inspections. The pattern reverses, however, after a rejection: the share of switchers rises substantially more for older vehicles than for younger ones. This is consistent with older drivers caring more about being approved—and therefore searching more actively for alternatives—or being better informed about the local set of stations, whether through past personal experience or through larger informal networks.

Table E.1: Baseline switcher rates

| | Approved at $t$ | | | Rejected at $t$ | | |
|---|---|---|---|---|---|---|
| | All | Age $> 10$ | Age $\leq 10$ | All | Age $> 10$ | Age $\leq 10$ |
| Switching rate | 0.334 | 0.312 | 0.349 | 0.386 | 0.380 | 0.393 |
| Observations | 12,900,004 | 5,153,033 | 7,746,971 | 4,364,965 | 2,356,145 | 2,008,820 |

*Notes.* The table reports switching rates—the share of vehicles that inspect at a different station in the following cycle—across six samples. Columns 1–3 restrict to vehicles approved at their time-$t$ inspection, and columns 4–6 to vehicles rejected. Within each group, the first column includes all vehicles, the second restricts to vehicles older than 10 years, and the third to vehicles aged 10 years or less.

We then test whether this exploration is triggered by a rejection more rigorously with a regression. To do so, we estimate the following:

$$\text{Switcher}_{ijt} = \delta_i + \delta_j + \delta_t + \beta\,\text{Rejected}_{ijt} + \theta_{v(i,t)\,T(t)} + \phi_j \times t + \varepsilon_{ijt}, \tag{E.1}$$

where $\text{Switcher}_{ijt}$ is an indicator equal to one if vehicle $i$, inspected at station $j$ at time $t$, moves to a different station in the following cycle. We include fixed effects for the inspection date, the vehicle, and the original station, together with vintage-specific fixed effects and station-specific linear time trends. The coefficient of interest is $\beta$, on $\text{Rejected}_{ijt}$, an indicator equal to one if the vehicle was rejected in its current inspection. Table E.2 reports the results.

The main finding is that a rejection significantly raises the probability that a vehicle inspects at a different station the following cycle. We interpret this as evidence of own-experience learning:

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[3] We restrict the analysis to non-monopolistic markets, where switching is a meaningful choice, and exclude cases in which the original station does not operate in the following cycle, where switching would be mechanical.

Table E.2: Effect of rejection on the probability of switching stations

| | All vehicles | Age $> 10$ | Age $\leq 10$ |
|---|---|---|---|
| Rejected | 0.051 | 0.058 | 0.041 |
| 95% CI | [0.045, 0.057] | [0.051, 0.067] | [0.036, 0.047] |
| Observations | 16,465,391 | 7,020,573 | 9,077,242 |

*Notes.* The table reports estimates of $\beta$ from equation (E.1), where the dependent variable is an indicator for whether the vehicle inspects at a different station in the following cycle. Column 1 uses all vehicles; column 2 restricts to vehicles older than 10 years and column 3 to vehicles aged 10 years or less. The sample excludes monopolistic markets and cases in which the original station does not operate in the following cycle. Standard errors are clustered at the market level, and 95% confidence intervals are computed via the wild cluster bootstrap with 1,000 replications.

a personal rejection prompts consumers to search for alternative providers. The heterogeneity analysis shows that older vehicles respond more strongly than younger ones, consistent with either a stronger interest in passing future inspections or a richer informal network through which to identify lenient stations.

Taken together, these results suggest that own-experience learning is at work, but that the signal each consumer receives is noisy and uneven across vehicles. This informational friction limits both the extent to which consumer choice disciplines lenient stations and the regulator's ability to identify and sanction misconduct from market-level outcomes.

# Appendix F: A bottom-up estimate of competition-induced vehicle emissions

In Figure 4, we find that competition led to an increase of approximately 20% in PM2.5 at monitoring stations near entry events. To verify that the competition effect on pass rates found in Figure 2 can account for this rise, we propose a complementary analysis: we look at how rejection rates and emissions of inspected vehicles change before and after entry, and project the predicted change in vehicle pollution. We can refer to this alternative estimation of changes in ambient pollution concentrations as a "bottom-up" estimation, since we use vehicle-level data to infer such changes.

Figure 4 is based on 7 markets that experienced an increase in competition between 2016 and 2022. These markets had different pre-entry competition levels: the entrant joined an existing monopoly ($m_j^{\text{pre}} = 1$), duopoly ($m_j^{\text{pre}} = 2$), or triopoly ($m_j^{\text{pre}} = 3$). We denote this set of markets by $J^*$ and write $t_j^{\text{entry}}$ for the month of entry in market $j$. We classify vehicles into five age groups $a \in \{1, 2, 3, 4, 5\}$, corresponding to 0–5, 5–10, 10–15, 15–20, and 20 or more years of age. For each market $j$ and age group $a$, we measure the change in pass rates caused by entry as the difference between the average first-inspection pass rate over the five quarters before entry and the five quarters after the market has had time to settle,

$$\widetilde{\Delta\theta}_{aj} \;=\; \bar{p}_{aj}^{\text{post}} - \bar{p}_{aj}^{\text{pre}}, \tag{F.1}$$

where $\bar{p}_{aj}^{\text{pre}}$ averages observed pass rates over the window $[t_j^{\text{entry}} - 8,\; t_j^{\text{entry}} - 4]$ and $\bar{p}_{aj}^{\text{post}}$ over $[t_j^{\text{entry}} + 8,\; t_j^{\text{entry}} + 12]$. We start the post-entry window two years after entry so the market has time to settle into its new equilibrium, and skip the year just before entry to avoid contamination from incumbents that may have already adjusted their behavior in anticipation.

To turn this change in pass rates into a change in emissions, we use the same ingredients as in Section 5: driving patterns $d_a$, the emission rates of vehicles in good ($e_a^0$) and poor ($e_a^1$) condition, and the cross-sectional distortion estimates $\Delta\theta_{a,m}$ from Table 3.[4] The cross-sectional estimates give us each market's baseline leniency before entry: a market that was a duopoly pre-entry already had a share $\Delta\theta_{a,2}$ of vehicles approved that should have been rejected. Entry adds another $\widetilde{\Delta\theta}_{aj}$ on top of that, without removing the vehicles that were already wrongly approved. The pre- and post-entry average emissions in market $j$ are then

$$\bar{e}_{aj}^{\text{pre}} \;=\; \big(1 - \Delta\theta_{a,m_j^{\text{pre}}}\big)\, e_a^0 \;+\; \Delta\theta_{a,m_j^{\text{pre}}}\, e_a^1, \tag{F.2}$$

$$\bar{e}_{aj}^{\text{post}} \;=\; \big(1 - \Delta\theta_{a,m_j^{\text{pre}}} - \widetilde{\Delta\theta}_{aj}\big)\, e_a^0 \;+\; \big(\Delta\theta_{a,m_j^{\text{pre}}} + \widetilde{\Delta\theta}_{aj}\big)\, e_a^1. \tag{F.3}$$

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[4] Variable $d_a$ is the average annual mileage of an age-$a$ vehicle, computed by tracking the same vehicle across consecutive inspections; $e_a^0$ and $e_a^1$ are the average ASM–2525 HC readings of approved and rejected vehicles in monopoly markets, respectively, capturing emission rates in good and poor condition; and $\Delta\theta_{a,m}$ is the additional share of vehicles approved in a market with structure $m$ relative to monopoly, estimated from Equation 10 in the text.

We use the post-entry fleet $N_{aj}^{\text{post}}$ for both calculations, so the comparison reflects how the same fleet would have emitted under the pre- and post-entry approval rates. The predicted percentage increase in fleet HC emissions for market $j$ is then

$$\%\Delta E_j \;=\; \frac{\sum_a N_{aj}^{\text{post}}\, d_a\, \widetilde{\Delta\theta}_{aj}\, (e_a^1 - e_a^0)}{\sum_a N_{aj}^{\text{post}}\, d_a\, \bar{e}_{aj}^{\text{pre}}} \times 100. \tag{F.4}$$

Intuitively, we are comparing the average emission level before and after entry, weighted by the share of vehicles in each vintage and their average annual mileage. To obtain a single number across the seven markets, we average the per-market results:

$$\overline{\%\Delta E} \;=\; \frac{1}{|J^*|} \sum_{j \in J^*} \%\Delta E_j, \tag{F.5}$$

which gives a predicted HC increase of roughly 36%.

To translate this HC increase into a predicted PM2.5 increase, we use the simple identity

$$\%\Delta \text{PM}_{2.5}^{\text{ambient}} \;=\; s_V \cdot \%\Delta E_V, \tag{F.6}$$

where $s_V$ is the share of ambient PM2.5 coming from vehicles in the local airshed and $\%\Delta E_V$ is the percentage increase in vehicle-source PM2.5. Using data from Santiago, Rizzi and De La Maza (2017) and Barraza et al. (2017) estimate that vehicles account for between 30% and 37% of total PM2.5. Assuming HC moves one-to-one with vehicle PM2.5, our 36% predicted HC increase combined with a vehicle share between 30% and 37% implies an ambient PM2.5 increase between 11% and 13%. This sits below the central estimate of roughly 20% in Figure 4, but well within its confidence intervals. The fact that two independent approaches—the cross-sectional regressions in equation (10) and the within-market entry comparisons here—deliver consistent results supports the idea that pass-rate leniency is the channel through which competition worsens local air quality.

## Appendix G: 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.

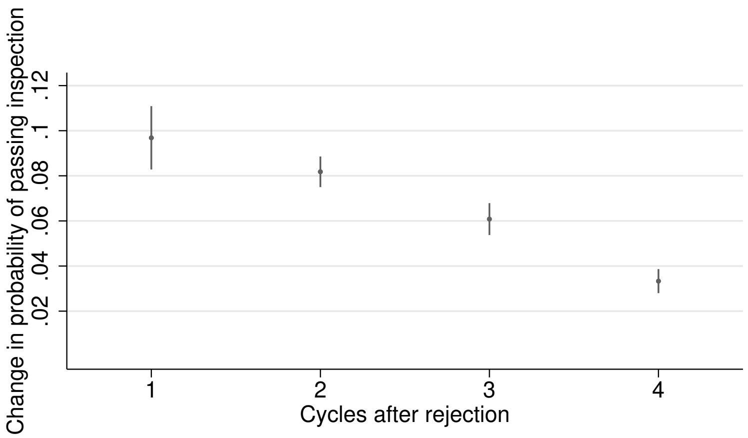
The rationale is straightforward. If rejections do not affect the probability of passing in a later inspection, then repairs have only transitory effects that do not persist beyond a year. Conversely, if a rejection increases future pass rates, we interpret this as evidence that repairs produce longer-lasting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

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

where $y_{ic\tau}$ is an indicator equal to 1 if vehicle $i$ is approved $\tau$ cycles after its inspection in base cycle $c$. The treatment variable $\text{Rejected}_{ic}$ 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 $\beta_\tau$, which captures the persistence of the effect of an initial rejection $\tau$ cycles ahead. We estimate four specifications with $\tau \in \{1,2,3,4\}$.

A potential concern is that $\beta_\tau$ 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 G.1: Long-lasting effects of smog checks



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

Figure G.1 presents our estimates of $\beta_\tau$. 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 H: 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

$$u_{\theta i} = \theta m_i + \mu \epsilon_i - \gamma Q_i - \bar{p} \tag{H.1}$$

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 $c$.

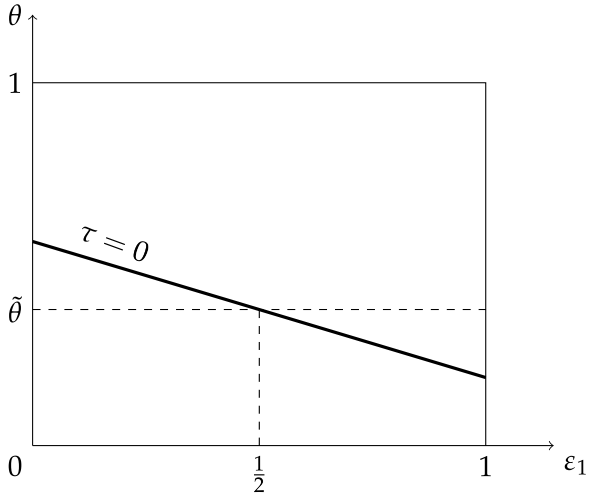
**Proposition H1.** *In the absence of transaction costs, the optimal mechanism follows Coase (1960): issue location allowances and let individuals freely trade them.*

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 $\tau$ 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

$$\theta m_j + \mu \epsilon_j - \gamma Q_j - \bar{p} - \tau > \theta m_i + \mu \epsilon_i - \gamma Q_i - \bar{p} \tag{H.2}$$

The payoffs when both stations follow the same strategy, either misreport ($M$) or report truthfully ($T$), are as before, no matter $\tau$. 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 H.1, the payoffs when $\tau = 0$ are exactly as before, with $\tilde{\theta}$ given by (3). 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 H.1: Consumers assigned to station 1 and their switching decisions



*Notes.* The figure depicts the switching decision of a consumer type $(\theta, \epsilon_1)$ 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 $\tau$. 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 $\tau$ 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.[5] There is a cheaper way to ensure honesty.

**Proposition H2.** *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 $\tau$ depends not only on the value of underlying parameters such as $\mu$ and $\gamma$, 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 $\tau$. 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 optimal 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 strictly dominates, in welfare terms, when transaction costs are negligible.

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[5] 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 $\int_0^{1/2} \mu(1 - 2\epsilon)\, d\epsilon = \mu/4$.