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Impacts of vehicle restrictions on urban traffic speeds and transit ridership: an empirical analysis using high-frequency data

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ABSTRACT

The impacts of vehicle restrictions in Santiago, Chile, on the average speeds of vehicle traffic and public buses as well as on transit card validations are quantified using novel high-frequency data, coming from millions of records from a ride-hailing service and the city's public transport system. The restrictions were in force on weekdays during May through August between 7:30 am and 9 pm in urban districts, and applied on a given day to 20 % of the stock of vehicles registered before September 2011. The quality, frequency and spatial coverage of the data we use allow us to estimate not only classic methods like before-and-after and differences-in-differences, but also triple differences, which allows for a higher number of control variables. All these methods arrived at the conclusion that the restrictions produce small increases in speed vehicle traffic (between 3.3 % and 4.2 %) and bus speeds (between 1.8 % and 2.3 %); no increases in the use of public transport were detected. Three likely reasons for the size of the effects are the low percentage of vehicles subject to the restrictions on any given day (approximately 6.9 %), the tendency for frequent drivers to be from higher-income groups and thus to own newer vehicles, and widespread violation of the restrictions due to weak enforcement with low fines.

1. Introduction

Traffic congestion in cities is an endemic and long-standing problem with substantial costs: according to the Urban Mobility Report (Texas Transportation Institute, 2021), Americans spent an extra 8.7 billion hours traveling due to congestion in 2019, resulting in an additional purchase of 3.5 billion gallons of fuel, for a total congestion cost of 190 billion USD. Moreover, traffic congestion worsens the levels of emissions of an already critical sector: in Europe, transport accounted for 25.9 % of the EU's total greenhouse emissions in 2019 (European Environment Agency, 2022), while in 2017, transportation activities accounted for the largest portion (29 %) of total US greenhouse gas emissions (Hockstad and Hanel, 2018).

Although theoretical contributions studying congestion pricing strategies date at least back to Vickrey (1959), real-world implementations have been limited, mainly due to public and political resistance (Noordegraaf et al., 2014; Santos et al., 2010). Consequently, second-best policies, often involving restrictions, have been adopted to facilitate practical implementation (Soto et al., 2023). In this context, policies that restrict certain license plates from circulating on some days have been particularly popular in Latin America, starting with the *Hoy no circula* program implemented in Mexico City in 1989. These type of road rationing policies have been viewed in conflicting ways: while they offer an easy, flexible, and cost-effective means to potentially reduce congestion in the short-

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term, there is also evidence suggesting these measures can increase the number of vehicles in circulation in the long-term, since many households buy an additional car to cope with the restriction (Davis, 2008).

The present article aims to quantify the impacts of a vehicle restriction system applied in Santiago, Chile during 2023, based on the last digit of the license plate number. The restrictions were in force Monday to Friday except holidays from 7:30 am to 9 pm between May 1st and August 31st. Only those vehicles registered before September 1st, 2011 were subject to the measure. It was imposed each day in rotation to two different digits and thus affected 20 % of the vehicles so registered on any given day of application. We quantify the impacts of this policy on the average speeds of Santiago's vehicle traffic and of buses in the city's public bus system as well as on the number of validations of the integrated (Bus & Metro) public transport system of the city.

To study the vehicle speeds, we use a novel data source, coming from a ride-hailing system. In particular, we consider a full real-world dataset of ride-hailing trips, consisting of millions of records. The main hypothesis of our work is that ride-hailing speeds are representative of private vehicle speeds. Although there are differences between commercial and personal vehicle use, in terms of interaction with urban infrastructure and dynamic behavior, platform vehicles like Uber are highly comparable to private cars. Therefore, in analyses of urban mobility, vehicle speed, or response to restrictions, these vehicles are an excellent proxy for the behavior of the private vehicle fleet.

Additionally, to estimate the effect on the public transport system, we use full disaggregated data for the period, namely, GPS and smart card data. The quality, frequency and spatial coverage of the data we use allow us to estimate not only classic methods like before-and-after and differences-in-differences, but also triple differences, which allows for a higher number of control variables. With these methods, reliable comparisons could be made of periods before and after the coming into force of the restrictions, times within and outside of restriction hours, and districts within and outside of the restrictions' area of application. We believe that our work shows that ride-hailing data may prove a valuable asset for transportation planning. Additionally, to the best of our knowledge, our work is the only one that considers trip-by-trip data in a citywide context to study the impact of vehicle restrictions.

The results indicated that the vehicle restrictions generated only minor changes in vehicle speeds (vehicle traffic and public buses) and no increase in transit use. We therefore conclude that the measure did not have the effects in terms of less congestion and greater public transport ridership the authorities promoting it had expected. We believe there are three reasons for these disappointing outcomes:

1) The number of vehicles affected was small. Data provided by the Chilean Automobile Association from historical records up to and including 2023 suggest that a modest 34.7 % of the total stock of vehicles in Santiago were registered before September 1st, 2011. Since the daily restrictions only affected 20 % of vehicles so registered, on any given day of application only 6.9 % of the total were subject to the measure.
2) Drivers who use their vehicles every day tend to be persons in the higher income strata who may be presumed to drive newer cars (in the same line as the works of Van Wee et al., 2000; Zachariadis et al., 2001), that is, cars registered after September 1st, 2011. In practice, then, those truly affected by the restrictions constituted rather less than 6.9 % of all Santiago drivers.
3) Drivers ignored the restrictions. If the likelihood of being caught and the fine levied for violations are both low, some drivers will no doubt be willing to take the risk. This is especially so since enforcement tends to be weak.

The remainder of this article is organized in three sections. Section 2 contains an extensive and up-to-date survey of the specialized literature on vehicle restrictions in various cities worldwide as well as some background on the operation of the system applied in recent years in Santiago. Section 3 introduces the data for the present study and the econometric model with its various specifications used to estimate the variables of interest. The results of the estimates and their interpretation are also discussed. Section 4 presents our conclusions.

2. Vehicle restrictions around the world and survey of empirical studies

2.1. Vehicle restrictions implementation in different cities

Vehicle restrictions are a type of regulatory policy on the use of vehicles in urban areas that can be traced back at least to the 1970 s, with early documented implementations in Buenos Aires, Argentina, where half of all vehicles were prevented from entering the city centre based on whether the last digit of the licence plate was even or odd. The method was also used in Caracas, Venezuela and Athens, Greece in the 1980 s (Bull, 2003).

Santiago established one of the first systematic and enduring vehicle restriction programs in Latin America in 1986 following severe pollution episodes (Mahendra, 2008). The policy initially prohibited vehicles on emergency pollution days only, affecting 20 % of the fleet daily based on license plate endings. The restrictions became seasonal, applied generally April through August (De Grange & Troncoso, 2011), expanding to 40 % of vehicles during high pollution episodes by 2008 (Cantillo & Ortúzar, 2014). A critical evolution occurred in 2001 when catalytic converter-equipped vehicles gained exemptions, fundamentally altering the policy's scope (Mahendra, 2008).

The concept spread rapidly across Latin America in the following decades. Examples of vehicle restriction systems in Latin America include the "parked-car day" program in Caracas, Venezuela, between 1979 and 1990 (Olivar, 2021); the "day without a car" program in Mexico City starting in 1989 (Tovar, 1995), which was later adopted by the Mexican cities of Pachuca, Puebla, and Toluca; the "Rodizio" vehicle restriction in Sao Paulo, Brazil, in 1997 (Bull, 2003); and the "Pico y Placa" system in Bogotá in 1998 (Cantillo & Ortúzar, 2014). Similarly, the city of San José, Costa Rica in 2005 (Osakwe, 2010) implemented restrictions to entry to the city center during rush hours. In most cases, the authorities' primary justification for introducing vehicle restrictions was traffic congestion, although environmental pollution was also considered.

Outside of Latin America, Asia presents multiple examples of vehicle restrictions. Jakarta's "three-in-one" policy (1992–2016) required a minimum of three passengers during rush hours, before transitioning to odd–even restrictions in 2018 (Hanna et al., 2017; Yudhistira et al., 2025). In China, Beijing implemented temporary restrictions during the 2008 Olympics, then adopted a permanent driving restriction policy (Sun et al., 2014). The concept spread to numerous other Chinese cities with varying policy designs (Li et al., 2018; Zhang et al., 2022).

In Europe, implementations remained largely episodic or zone-based. Paris applied temporary restrictions during pollution emergencies (2014, 2015) (Blackman et al., 2018), Athens implemented permanent odd–even restrictions since 1982 (Chaziris & Yannis, 2023), and Madrid developed staged responses through its NO2 Protocol (Romero et al., 2019) alongside low emission zones (Lebrusán & Toutouh, 2021).

In summary, vehicle restrictions have been applied worldwide across diverse urban contexts, evolving from simple license plate-based access controls to more sophisticated policy instruments. Contemporary implementations show increased sophistication, incorporating exemptions for cleaner vehicles, dynamic pricing elements, or complementary measures, while maintaining the basic concept of restricting vehicle access based on various criteria depending on local conditions and policy objectives.

2.2. Empirical results and analyses

This section reviews empirical evidence on vehicle restriction policies, focusing on three main outcomes: impacts on traffic conditions, effects on public transport usage, and behavioral responses that may undermine policy effectiveness. These themes structure the discussion and provide context for the contributions of the present study.

2.2.1. Impact on traffic conditions

Vehicle restriction policies have been implemented worldwide to address urban congestion and air pollution, with mixed findings regarding their effectiveness that vary significantly across cities and policy designs. Studies from China generally report positive but modest traffic improvements, with Li and Guo (2016) finding traffic volume reductions of 20–40 % and speed gains of 10–20 % during Beijing's 2008 Olympic restrictions, while Liu et al. (2018) documented speed increases of up to 23 % during peak hours when Langfang shifted from one-day-per-week to odd–even policies. Yang et al. (2018) and Lin et al. (2022) reported substantial travel time savings in Beijing (up to 57 % during evening peaks) and average speed increases of 12 % in Zhengzhou, particularly during congested periods. Li and Yang (2023) found that Chengdu's policy tightening resulted in a 4.3 % decrease in the Air Quality Index (AQI), a composite measure of air pollution, providing evidence from a major Chinese city with minimal confounding policies.

Evidence from Jakarta shows similar patterns, with Hanna et al. (2017) demonstrating the reverse effect when high-occupancy vehicle restrictions were removed, finding delays increased by up to 87 % on affected roads with negative spillovers to surrounding areas and non-peak hours. Also in Jakarta, Yudhistira et al. (2025) found that introducing odd–even restrictions reduced per-kilometer travel times by 3 %, with stronger effects on weekends and evening peaks, while Chaziris and Yannis (2023) documented statistically significant speed increases during morning peak hours when Athens reinstated restrictions after COVID-19 suspension, with no persistent spillover effects.

In contrast, Latin American studies reveal limited or counterproductive effects. Eskeland and Feyzioglu (1995), Davis (2008), and de Grange and Troncoso (2011) found that Mexico City and Santiago restrictions failed to achieve sustained traffic reductions, with Davis (2008) documenting increased vehicle ownership and weekend driving, while de Grange and Troncoso (2011) reported no measurable impact on traffic volumes from permanent restrictions and only 5.5 % reductions during temporary pollution episodes.

Air quality outcomes, often used as proxies for traffic activity changes, show similarly mixed results. While studies by Viard and Fu (2015), Carrillo et al. (2016), Pu et al. (2015), Sun et al. (2022), and Meng (2022) found measurable pollution reductions ranging from 3.6-33.6 % for various pollutants, others documented mixed, limited, or no improvements. Barahona et al. (2020) found 20–27 % CO emission reductions using vintage-specific restrictions in Santiago. Green et al. (2020) found significant reductions in CO, PM10, and NO but substantial increases in NO2 concentrations due to increased diesel vehicle usage in London's congestion charge zone. Davis (2008) and Gallego et al. (2013a) found that initial air quality gains in Mexico City were reversed as households acquired additional vehicles, while Sun et al. (2014) and Chen et al. (2021) found no significant effects or increases in PM concentrations despite traffic improvements, and Zhang et al. (2017) documented increases in NO2 and O3 concentrations in Bogotá, suggesting complex relationships between traffic volumes and emissions.

Overall, while some studies report substantial short-term congestion relief, especially under stricter enforcement or during major events, the literature suggests that vehicle restrictions produce modest improvements in traffic speeds that often fall short of their theoretical potential, with these limited gains frequently attributed to low compliance rates, behavioral adaptations, and enforcement challenges.

2.2.2. Impact on public transport use

Vehicle restriction policies are often implemented with the expectation that they will encourage modal shift from private vehicles to public transportation, though empirical evidence shows mixed results that vary considerably across transport modes and urban contexts. Several studies document measurable increases in public transport use, with Yang et al. (2018) finding that Beijing's restrictions generated roughly one additional bus passenger trip for every four banned cars, while Lin et al. (2022) reported more substantial modal shifts in Zhengzhou with subway ridership increasing by 11.5 % and bus ridership by 14.2 % when restrictions were tightened. Zhang et al. (2019) found that driving restrictions increased public transport passenger volume by 5–25 % across six Chinese cities, with dual policies showing larger effects of 20–30 %, and Chaziris and Yannis (2023) documented substantial metro ridership increases of 80–780 additional hourly validations per station in Athens.

However, other studies report important limitations and variations across transport modes. Yang et al. (2018) noted that rail ridership in Beijing showed no significant response despite new subway lines opening, while Zhang et al. (2019) found that although absolute ridership increased, the proportion of public transport trips remained unchanged, indicating restrictions did not fundamentally alter mode choice patterns. Studies from Latin America show even more limited effects, with de Grange and Troncoso (2011) finding only a 3.1 % Metro ridership increase in Santiago with no significant bus response, and Guerra and Reyes (2022) documenting that most Mexico City households simply shuffle trips to unrestricted days rather than switching modes.

The differential response between transport modes appears linked to service quality and accessibility. Romero et al. (2019) found modest modal shifts in Madrid with bus and metro ridership increasing by 1.9–11.6 %, but analysis of ticket types revealed no increase in occasional transit use, suggesting limited shift from regular car users. Cheng et al. (2020) found that areas with better public transportation coverage and land-use diversity in Xi'an showed stronger resilience to restrictions, while policy design features significantly influence outcomes. Gonzalez et al. (2022) showed that exemptions for cleaner vehicles in Madrid encouraged vehicle purchases rather than public transport use, creating perverse incentives for continued car dependency among those who can afford newer vehicles.

Interestingly, restrictions often increase demand for ride-hailing and taxi services rather than traditional public transport. Lin et al. (2022) found ride-hailing services experienced the largest demand increase (28.1 %) in Zhengzhou, indicating preferences for car-like mobility even when private vehicles are restricted. This suggests that restrictions may simply shift demand between different forms of motorized transport rather than promoting truly sustainable modes, with overall evidence indicating that while restrictions generate some modal shift, effects are typically modest and highly dependent on the quality and accessibility of available alternatives.

2.2.3. Behavioral adaptation and enforcement

Vehicle restriction policies often fail to achieve their intended long-term objectives due to various behavioral adaptations and enforcement challenges that fundamentally undermine policy effectiveness over time. The most documented and significant response is the purchase of additional vehicles to circumvent restrictions. Multiple studies from Mexico City demonstrate this pattern clearly: Gallego et al. (2013b) found that middle-income households purchased second cars sufficient to reverse initial air quality improvements within 12.5 months, while Davis (2008) documented the city's transformation from a net used car exporter to importer, with households acquiring older, more polluting vehicles. Eskeland and Feyzioglu (1995) provided early quantitative evidence that approximately 10 % of car-owning households adjusted their vehicle ownership in response to restrictions, while Chen et al. (2021) found similar vehicle acquisition patterns across multiple Chinese cities. However, Gonzalez et al. (2021) found 39 % and 33 % lower car ownership probability in Madrid's policy areas.

High violation rates represent another permanent challenge across different contexts. Wang et al. (2014) found that 47.8 % of regulated car owners in Beijing violated restriction rules, while Liu et al. (2018) documented illegal travel rates increasing from 8.12 % to 17.90 % when Langfang tightened restrictions from one-day-per-week to odd–even policies. Guerra and Reyes (2022) found that regular drivers in Mexico City employ multiple non-compliance strategies, including driving on less-policed peripheral roads and paying bribes, with enforcement patterns creating spatial inequalities in compliance.

Temporal and spatial substitution effects further reduce policy effectiveness. Studies by Hu et al. (2020) in Ningbo, Qin et al. (2023) in Shanghai, and Gu et al. (2017) in Beijing all document how drivers shift their travel times to unrestricted hours or use alternative routes rather than abandoning car trips. Qin et al. (2023) found CO concentrations increasing by 15–20 % during off-peak hours, while Hu et al. (2020) showed that 87 % of restricted drivers simply bypassed restricted roads rather than switching modes. Lu et al. (2022) documented similar spillover effects in Shanghai with elevated expressway restrictions.

The effectiveness of restriction policies consistently diminishes over time due to these cumulative adaptations. Yang et al. (2018) observed that while Beijing's restrictions remained effective for traffic management over five years, only 13 % of restricted demand shifted to public transit, with most moving to taxis and other motorized alternatives. Cantillo and Ortúzar (2014) argued that these adaptation patterns explain why license plate restrictions across Latin American cities generally fail in the long term. Moreover, behavioral responses vary significantly by income, with Guerra and Reyes (2022) and Soto et al. (2023) showing that wealthier households more easily purchase exempt vehicles or pay exemption charges, while lower-income households face greater mobility constraints, creating equity concerns alongside effectiveness issues.

In Table 1 we present a comparative summary of the main studies on vehicle restrictions, their scope, general methodology, main effects and the evaluation proposed by the respective authors in relation to this transport policy, ordered by publication date. As shown in Table 1, studies differ in the spatial extent of the restrictions evaluated. To explore whether area size influences the perceived effectiveness of these policies, we grouped the cases into two categories: limited zone restrictions (city center, corridor, or partial city) and large zone restrictions (citywide or metropolitan area). Among the 23 studies classified as limited-zone interventions, 43 % were assessed positively, 43 % received mixed evaluations, and only 13 % were negative. In contrast, of the 16 studies implementing large-zone restrictions, just 25 % were positive, while 38 % were mixed and 38 % negative. This pattern suggests that restrictions focused on smaller, denser areas may be more enforceable and generate more noticeable effects, leading to more favorable evaluations. Broader-scale implementations, by contrast, may suffer from diluted impacts, enforcement difficulties, and behavioral adaptation, contributing to less favourable assessments.

Table 1
Summary of empirical studies on urban vehicle restriction policies.

Study	City and Country	Policy Type	Restricted Area	Horizon	Method	Main Findings	Assessment
Eskeland & Feyzioglu (1995)	Mexico City, Mexico	License-plate	Metropolitan area	Long-term	Descriptive comparison	Traffic up after initial drop	Negative
Davis (2008)	Mexico City, Mexico	License-plate	Metropolitan area	Long-term	Regression discontinuity	No air quality improvement	Negative
de Grange & Troncoso (2011)	Santiago, Chile	License-plate	Citywide	Short-term	Descriptive comparison	Traffic down modestly, Metro use up	Mixed
Gallego et al. (2013a)	Mexico City, Mexico	License-plate	Citywide	Long-term	Descriptive comparison	Short-term drop, long-term increase	Negative
Gallego et al. (2013b)	Mexico City, Mexico	License-plate	Citywide	Long-term	Difference-in-differences	Initial drop, long-run increase	Negative
Sun et al. (2014)	Beijing, China	License-plate	City Center	Long-term	Descriptive comparison	Traffic improved, air quality unchanged	Mixed
Wang et al. (2014)	Beijing, China	License-plate	Partial city	Short-term	Descriptive comparison	No significant car use reduction; high violations	Negative
Cantillo & Ortúzar (2014)	4 Latin American Cities	License-plate	Citywide	Long-term	Descriptive comparison	Short-term benefits fade, situation worsens	Negative
Viard & Fu (2015)	Beijing, China	License-plate	Citywide	Long-term	Regression discontinuityDifference-in-differences	Air quality up, flexible work participation down	Positive
Pu et al. (2015)	Hangzhou, China	License-plate	City center	Short-term	Descriptive comparisonSimulation	Traffic and emissions down modestly	Positive
Li & Guo (2016)	Beijing, China	License-plate	Citywide	Short-term	Descriptive comparison	Traffic down modestly, speeds up moderately	Positive
Carrillo et al. (2016)	Quito, Ecuador	License-plate	City center	Medium-term	Difference-in-differences	Traffic and pollution down modestly	Positive
Hanna et al. (2017)	Jakarta, Indonesia	Other	City center	Short-term	Descriptive comparison	Congestion up substantially citywide	Positive
Gu et al. (2017)	Beijing, China	License-plate	Partial city	Medium-term	Descriptive comparison	Traffic down modestly	Mixed
Zhang et al. (2017)	Bogotá, Colombia	License-plate	Citywide	Long-term	Regression discontinuity	NO2 and O3 up, NO down	Mixed
Liu et al. (2018)	Langfang, China	License-plate	City center	Short-term	Regression discontinuity	Traffic down modestly, speed up significantly	Positive
Yang et al. (2018)	Beijing, China	License-plate	City center	Long-term	Descriptive comparison	Congestion down significantly in restricted hours	Positive
Li et al. (2018)	Shanghai, China	Area	Partial city	Short-term	Regression discontinuity	Effects vary by design	Mixed
Zhang et al. (2019)	6 Chinese cities	License-plate	Not specified	Long-term	Panel regression	Modest modal shift to public transport	Mixed
Romero et al. (2019)	Madrid, Spain	License-plate	City center	Medium-term	Choice modeling	Limited modal shift	Mixed
Cheng et al. (2020)	Xi'an, China	License-plate	Citywide	Short-term	Descriptive comparison	Taxi use up; areas with good transit more resilient	Mixed
Barahona et al. (2020)	Santiago, Chile	License-plate	Citywide	Long-term	Regression discontinuity	Fleet shifted to cleaner vehicles	Positive
Hu et al. (2020)	Ningbo, China	License-plate	Corridor	Short-term	Descriptive comparison	Traffic down modestly, most drivers bypassed restrictions	Mixed
Chen et al. (2020)	11 Chinese cities	License-plate	Partial City	Long-term	Difference-in-differences	No PM10 reduction	Negative
Green et al. (2020)	London, UK	Congestion pricing	City center	Long-term	Difference-in-differences	Most pollutants down, NO2 up	Mixed
Chen et al. (2021)	173 Chinese cities	License-plate	Citywide	Long-term	Difference-in-differences	Restrictions increased air pollution	Negative
Lebrusán & Toutouh (2021)	Madrid, Spain	Low-emission zone	City center	Medium-term	Descriptive comparison	Air quality up significantly	Positive
Gonzalez et al. (2021)	Madrid, Spain	Low-emission zone	City center	Short-term	Choice modelling	Car ownership down in policy areas	Positive

(continued on next page)

Table 1 (*continued*)

Study	City and Country	Policy Type	Restricted Area	Horizon	Method	Main Findings	Assessment
Zhang et al. (2022)	32 Chinese cities	License-plate	Citywide	Long-term	Difference-in-differences	Air pollution down modestly	Positive
Guerra & Reyes (2022)	Mexico City, Mexico	License-plate	Metropolitan area	Long-term	Descriptive comparison	Driving reduction less than intended	Mixed
Lin et al. (2022)	Zhengzhou, China	License-plate	Citywide	Short-term	Regression discontinuity	Traffic improved significantly, vehicle reduction limited	Mixed
Meng (2022)	54 Chinese cities	License-plate	City center	Short-term	Regression discontinuity	Effects vary by implementation	Mixed
Lu et al. (2022)	Shanghai, China	Area	Partial city	Short-term	Regression discontinuity	Gas pollutants down significantly, particulate matter unchanged	Mixed
Sun et al. (2022)	15 Chinese cities	License-plate	Partial City	Long-term	Difference-in-differences	Both policies improved air quality, intermittent better	Positive
Gonzalez et al. (2022)	Madrid, Spain	Low-emission zone	City center	Long-term	Choice modeling	Modal shift encouraged, clean car owners stayed car-dependent	Mixed
Li & Yang (2023)	Chengdu, China	License-plate	Partial city	Medium-term	Difference-in-differences	Air quality up modestly	Positive
Chaziris & Yannis (2023)	Athens, Greece	License-plate	City center	Short-term	Descriptive comparison	Traffic improved in restricted area, transit use up	Positive
Soto et al. (2023)	Cali, Colombia	License-plate	Citywide	Medium-term	Choice modeling	Revenue up, transit contribution minimal	Mixed
Qin et al. (2023)	Shanghai, China	License-plate	City center	Short-term	Difference-in-differences	No improvement in area, spillovers worsened nearby	Negative
Yudhistira et al. (2025)	Jakarta, Indonesia	License-plate	Partial city	Short-term	Regression discontinuity	Traffic down modestly	Mixed

2.3. *Contribution of this study*

This study contributes to the empirical literature on vehicle restriction policies in several ways.

First, we introduce the use of trip-level ride-hailing data as a novel proxy for private vehicle traffic conditions. While most previous studies have relied on aggregate traffic counts, air quality measurements, or limited survey data to infer the effects of vehicle restrictions on private traffic, our approach leverages millions of individual trip records from 30,528 drivers on Santiago's ride-hailing platform. Extensive empirical evidence supports the validity of using this type of data as proxies for general traffic conditions. Castro et al. (2012) demonstrated that taxi GPS trajectories can predict city-wide traffic patterns with remarkable accuracy, while O'Keeffe et al. (2019) showed that even small taxi fleets (10 vehicles) possess substantial sensing power. Studies by Qing and Hao (2018), Meng et al. (2017), and Wang et al. (2017) have consistently found high correlations between taxi-derived speeds and overall traffic conditions across different urban contexts, validating this methodological approach for traffic monitoring and congestion estimation. Importantly, our analysis considers only speeds during active passenger trips, excluding deadheading (empty vehicle travel), which ensures that our speed measurements reflect purposeful, destination-oriented travel that closely mirrors private vehicle behaviors. Our granular, trip-level data provides direct, real-time measurements of vehicle speeds across the urban network, offering high precision in assessing restriction impacts compared to traditional aggregate measures.

Second, we provide a multi-modal evaluation using triple difference methodology. Unlike most previous studies that focus on a single outcome measure, our analysis simultaneously examines three key variables: private vehicle speeds (through ride-hailing data), public bus speeds (through GPS records), and public transport demand (through smart card validations). This multi-modal approach allows us to capture the full spectrum of transportation system responses to vehicle restrictions, including potential modal substitution effects and system-wide efficiency changes that single-outcome studies might miss. We apply the triple difference estimator in a high-frequency, urban mobility context. While difference-in-differences methods are common in transport policy evaluation, the application of triple differences to vehicle restriction analysis is rare, particularly with the spatial and temporal granularity our data permits. The DDD approach allows us to control for both time-varying citywide factors and district-specific trends simultaneously, providing more robust causal identification than conventional before-and-after or difference-in-differences approaches.

Finally, the integration of ride-hailing trip data, bus GPS records, and smart card validations illustrates how the growing availability of digital urban data can enhance the precision and scope of policy evaluation studies. This data fusion approach opens new possibilities for real-time policy monitoring and evidence-based transportation planning that extends well beyond vehicle restriction policies.

3. Data, models and results

3.1. Data

Our estimates of the impact of vehicle restrictions on Santiago's traffic conditions and use of the city's public transport employed information in three different analytical samples drawn from different databases. The time period covered by the sample data consisted of representative working days, excluding those on which special events were held. The 2023 version of the restrictions began on May 2, which fell on a Tuesday. That meant the International Workers' Day holiday the day before resulted in a long weekend, in turn meaning that both the previous week and the shortened week that followed may have been atypical. We therefore chose as our before-and-after periods the three midweek days (Tuesday, Wednesday, Thursday) from, respectively, the week before and the week after those two possibly unrepresentative weeks. Thus, the before-period was April 18, 19 and 20 while the after-period was May 9, 10 and 11. Both weeks considered correspond to the two weeks closest (before and after) to the start of the restriction, during which no special events occurred. Furthermore, the midweek days were chosen because these are the days with the greatest stability in terms of urban traffic flows (see, for example, Schmidt, 1996). No events were reported that could have affected the normal functioning of the city for those days, such as strikes, demonstrations, mass events or public transportation massive failures. Additionally, our DDD methodology (presented in Section 3.2) helps control for any remaining time-varying factors that might differ between periods, as such city-wide influences would affect both restricted and unrestricted districts similarly, allowing us to isolate the restriction-specific effect. Fig. 1 illustrates the selected sample.

We believe the selection of this short timeframe is methodologically appropriate for several reasons. First, the literature shows that behavioral responses to vehicle restrictions usually manifest immediately upon implementation rather than requiring adjustment periods (de Grange & Troncoso, 2011; Hanna et al., 2017; Liu et al., 2018). Second, measuring close to the policy implementation allows us to better isolate the restriction effects from other time-varying confounding factors. Finally, empirical evidence consistently shows that when vehicle restrictions have effects, these are strongest in the immediate post-implementation period and typically weaken over time due to behavioral adaptations (Bull, 2003; Davis, 2008; Gallego et al., 2013a), making our timeframe conservative for detecting any policy benefits.

The first analytical sample consisted of hundreds of thousands of trip records, conducted by 30,528 drivers, for the two periods provided by the local Uber ride-hailing service. This formed the basis for our estimates of the restrictions' effect on the average speed of vehicle traffic on the road network. Average individual trip speed was the unit of analysis.

Table 2 describes the variables contained in the ride-hailing dataset. Each trip record included origin and destination coordinates, trip distance, and timestamps for trip start and completion, from which we calculated average trip speed as distance divided by travel time.

These records allowed trips initiated in districts subject to the restrictions to be distinguished from trips initiated in surrounding districts that, while being part of the same metropolitan area with regular commuting flows between them, are not subject to the restriction policy. The districts in both categories, together constituting the entire Greater Santiago Region, are shown on the map in Fig. 2. The records also differentiated trips made during the restricted hours (7:30 am to 9 pm weekdays as noted earlier) from those made outside of them. The temporal information enabled us to distinguish between trips occurring during restricted hours and those outside these periods. As Figs. 3 and 4 show, the ride-hailing trips considered cover most of the restricted area. Moreover, there are a considerable number of trips (37,613) that start and finish in unrestricted areas, thus serving as controls.

The second analytical sample contained millions of Santiago public bus GPS records. From these data we estimated the effect of the restrictions on the buses average speed. The unit of analysis in this case was their average speed every 30 s during the period of analysis. The data was available for hours both within and outside of the restricted periods but only in the urban districts of the city, that is, those subject to the restrictions.

Finally, the third analytical sample was made up of the records of numbers of transit card validations, or in effect, the number of the number of travel stages in the public transport system. Santiago's public transportation system (called Red) integrates bus and Metro modes, so that a trip can consist of more than one leg of travel with no (or very little) additional payment. The unit of analysis is the

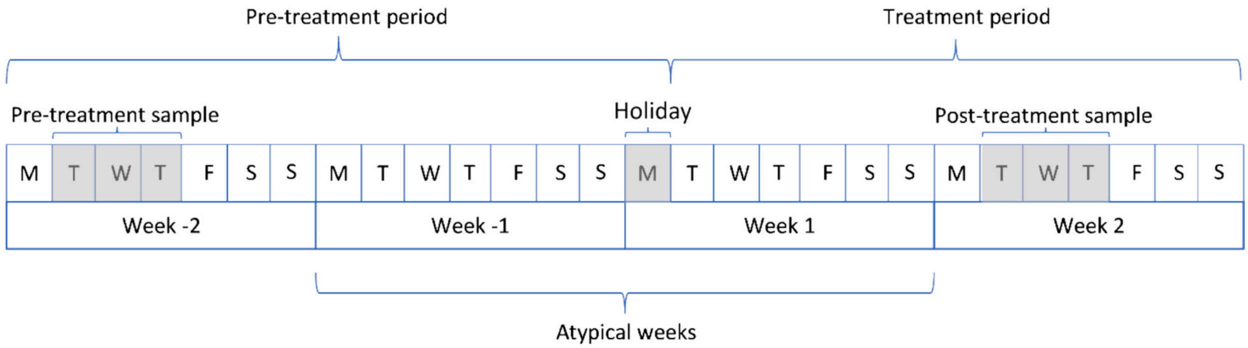

Fig. 1. Illustration of selected sample.

Table 2
Ride-hailing dataset. Each row corresponds to a completed trip.

Variable	Description
Origin Latitude	Geographical position associated with the origin of the trip.
Origin Longitude	Geographical position associated with the origin of the trip.
Destination Latitude	Geographical position associated with the destination of the trip.
Destination Longitude	Geographical position associated with the destination of the trip.
Distance	Route distance of the trip.
Timestamp Begin	Date and time of the trip start.
Timestamp Dropoff	Date and time of the trip end.

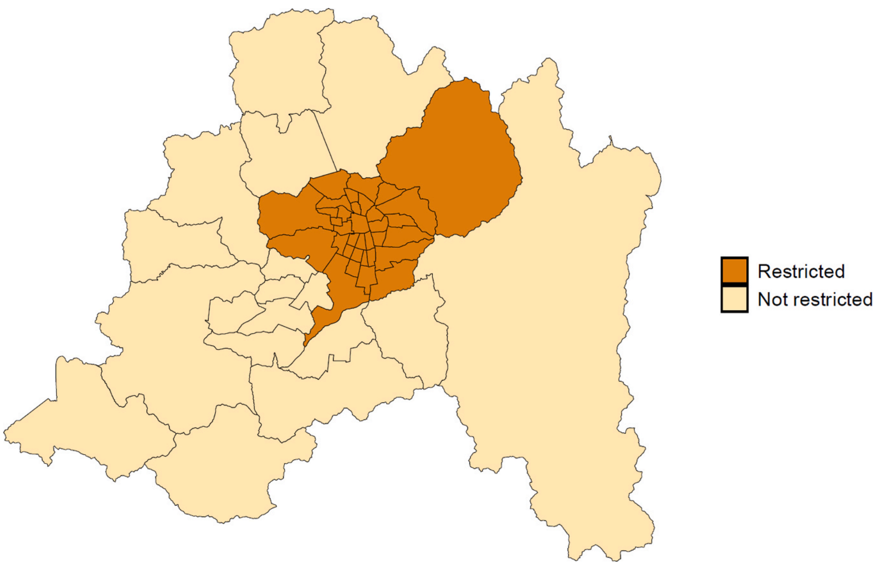

Fig. 2. Vehicle Restriction Category of Greater Santiago Region Districts.

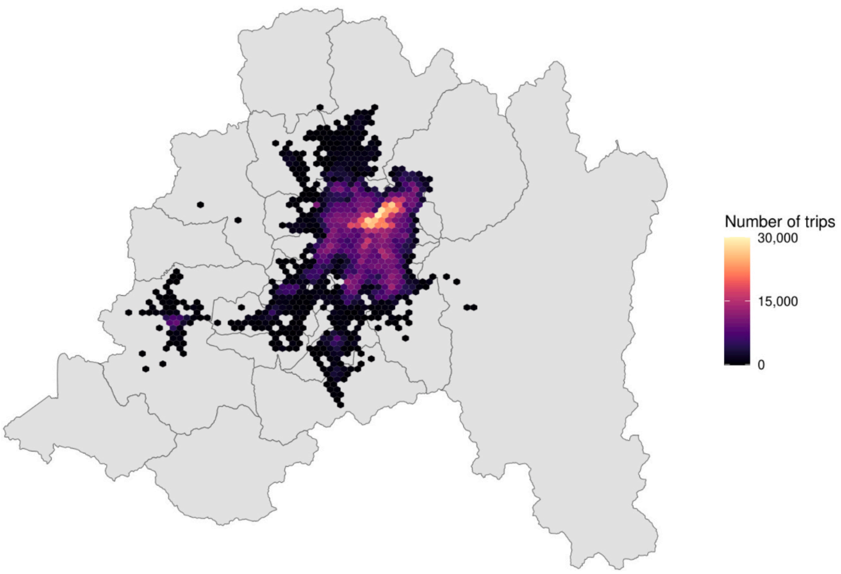

Fig. 3. Distribution of origins – Ride-hailing trips.

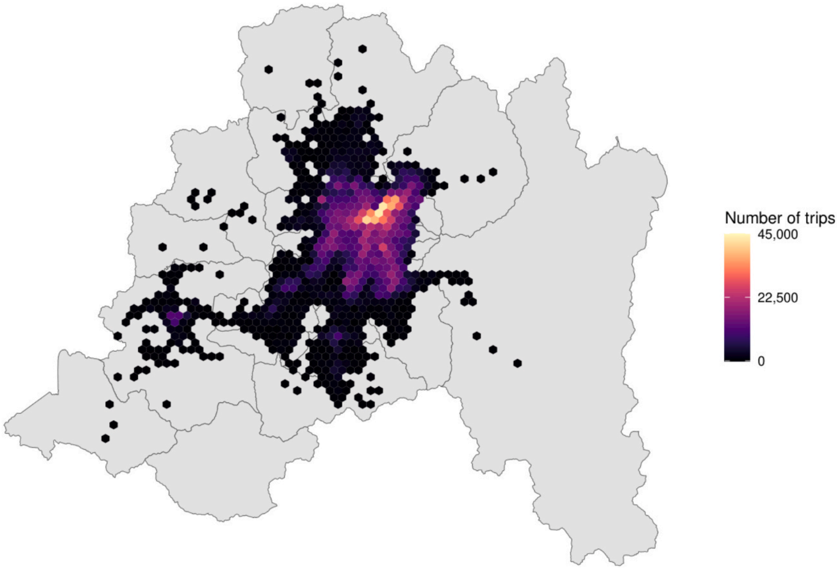

Fig. 4. Distribution of destinations – Ride-hailing trips.

transit card used to pay the fare and the information collected was the number of validations in the before-and-after three-day periods, distinguishing between the restricted and unrestricted hours. Note here that unlike the ride-hailing service, the Santiago public transport system does not operate outside of the restricted districts so no data on buses or metro in the surrounding districts was available for the study.

The descriptive data relating to the bus and ride-hailing speeds calculated from the three analytical samples broken down by before-and-after calendar periods, restricted and unrestricted hours, and (for ride hailing only) restricted and unrestricted districts are set out in Table 3. Note that in the case of card validations, the sample is constructed as a balanced panel that includes all cards observed in either period, assigning zero validations when applicable. In contrast, the ride-hailing and bus speed samples are not panels, as individual drivers or buses are not tracked over time. Additionally, it should be noted that our ride-hailing speed measurements reflect the average speed for entire trips originating in each district type, including any portions that may occur in a different district type. While this approach may introduce some measurement considerations due to the mixing of restricted and unrestricted areas within individual trips, robustness checks using only trips that both originate and terminate within the same district type yield similar results (see Appendix).

Table 3
Descriptive statistics.

Variable	Restricted districts			
	Before		After	
	Non-affected hours	Affected hours	Non-affected hours	Affected hours
Ride Hailing Speed (Km/h)				
Mean	30.04	23.20	30.10	24.03
Std. Dev.	11.59	9.37	11.50	9.58
No. of observations	94,038	433,621	85,672	411,886
Bus Speed (Km/h)				
Mean	19.75	15.78	19.78	16.10
Std. Dev.	14.35	12.98	14.28	13.08
No. of observations	5,707,217	21,126,727	5,696,232	20,930,896
Transit card validations (number per card)				
Mean	0.635	3.323	0.614	3.272
Std. Dev.	1.733	4.143	1.661	4.065
No. of observations	3,355,053	3,355,053	3,355,053	3,355,053
	Unrestricted districts			
Ride Hailing Speed (Km/h)				
Mean	30.66	26.36	31.41	27.02
Std. Dev.	11.82	12.14	11.67	12.40
No. of observations	2,706	16,004	2,659	16,244

3.2. Models

Estimation of the vehicle restrictions' impact on average ride-hailing vehicle and bus speeds and on metro/bus validations (i.e., passenger trips) was performed using three different methodological approaches, depending on the type of data available. The first two approaches, before-and-after (BA) and differences-in-differences (DD), have been widely utilized in empirical evaluations of transport policies and projects (see, among others works, Romano & Sampaio (2023) on road concessions, and Wang et al. (2023) and Feng et al. (2023) on high-speed trains). The third approach, triple difference (DDD), was the subject of a recent in-depth treatment by Olden and Møen (2022). It is more robust to the assumption of parallel trends than DD to have a causal interpretation.

In the impact analysis literature (Khandker et al., 2009), the effect of a treatment on the treated group (TT) is measured in terms of a result variable or output, the treatment in the present case being the vehicle restrictions. The BA indicator compares the average results for the treated units before and after the treatment, the DD indicator compares the BA change in the treated units to the change in the untreated ones, and the DDD indicator compares the double difference between the populations subject to and not subject to the treatment.

The three indicators can be implemented as estimators in regression models. Thus, BA is estimated by

$$y_{it} = \beta + \delta t + \varepsilon_{it} \tag{1}$$

where y_{it} is the output variable of individual i in period t (where $t = 0$ in the period before treatment and $t = 1$ in the period after treatment), β is a parameter and δ is the BA estimator for TT. Equation (1) is estimated using only TT observations, which in our case are the observations from restricted districts during restricted hours. So that the estimator is consistent, we assume that y_{it} is trendless.

As for the DD estimator, it uses the y_{it} information for unrestricted hours to correct for the existence of a trend in the latter. To ensure this estimator is consistent, we must assume that the trends in restricted and unrestricted hours are parallel in the absence of the restrictions. The DD estimator for TT can then be estimated by

$$y_{it} = \beta_0 + \beta_1 t + \beta_2 D_i + \delta(t \times D_i) + \varepsilon_{it} \tag{2}$$

where $D_i = 0$ in unrestricted hours $\mathrm{D}_i = 1$ in restricted hours. The βs are parameters and δ is the DD estimator of TT. Equation (2) is estimated using observations from restricted districts during both restricted and unrestricted hours.

In the case of ride-hailing vehicle speeds, since we have data from the surrounding districts where restrictions do not apply we can estimate the DDD model, which requires that only one of the trends (for hours or districts) be parallel (Olden and Møen, 2022). This estimator is obtained from the following equation:

$$y_{it} = \beta_0 + \beta_1 t + \beta_2 D_i + \beta_3 Z_i + \beta_4 (t \times D_i) + \beta_5 (t \times Z_i) + \beta_6 (D_i \times Z_i) + \delta(t \times D_i \times Z_i) + \varepsilon_{it} \tag{3}$$

where $Z_i = 0$ for unrestricted districts and $Z_i = 1$ otherwise, the βs are parameters and δ is the DDD estimator for TT. The estimation of Equation (3) requires the data used in Equation (2) and observations for restricted and unrestricted hours in unrestricted districts.

Equations (1), (2) and (3) were all estimated using least squares while the standard errors were estimated with the Huber-White heteroscedasticity-robust matrix estimator (Huber, 1967; White, 1980). Either panel or cross-sectional data could have been used, but in our case we had unbalanced panel data for the three output variables. We were thus able to include fixed effects by individual in the regression models as control variables to correct bias due to any violations of the trendless assumption in the case of BA or of the parallel trend assumption in the case of DD and DDD (Abadie, 2005). For example, Equation (1) would become

$$y_{it} = \beta + \delta t + \gamma' X_{it} + \varepsilon_{it} \tag{4}$$

where X_{it} is the control variable vector and γ' is the transposed vector of parameters. The incorporation of variable controls for the DD and DDD estimators would be similar.

Table 4
Impact of restrictions on ride-hailing vehicle speed.

Estimator	Without CV	With CV
BA	0.8226***	0.8829***
	(0.0206)	(0.0215)
No. Obs.	845,507	845,507
R-Squared	0.002	0.200
DD	0.7595***	0.823***
	(0.0583)	(0.0518)
No. Obs.	1,025,217	1,025,217
R-Squared	0.060	0.296
DDD	0.8598**	0.9714***
	(0.3535)	(0.3379)
No. Obs.	1,062,830	1,062,830
R-Squared	0.060	0.298

Standard errors in parenthesis. ** indicates significance at the 5% level and *** at the 1%.

What was used as the control variables depended on the particular sample. For ride-hailing vehicle speeds, it was a set of dummies (individual effects) for each driver; for the bus speeds, a set of dummies (individual effects) for each bus; and for the number of rider validations, a set of dummies (individual effects) for each transit card. To avoid having to estimate large sets of parameters (one for each dummy variable), we estimated the equations with control variables using least squares with dummy variables (LSDV) given the large number of the latter and calculated the heteroscedasticity-robust standard errors.

3.3. Results

To simplify the presentation of the results, in what follows we show only the estimates of the δ coefficient in Equations (1), (2) and (3) with and without control variables (individual effects) for TT. We also report the R-squared of the regression and the number of observations for each different treatment, subsample or output variable.

The results for the BA, DD and DDD estimators of the impact of vehicle restrictions on ride-hailing vehicle speed are displayed in Table 4. All of the estimates are significant at 5 %, indicating slight increases in speed ranging from 0.76 to 0.97 Km/h (that is, between 3.3 % and 4.2 % relative to the average speed in the period before, during the affected hours). This is consistent with a reduction in the number of private vehicles on the road network.

The estimates in Table 4 consider a trip to operate in a restricted or unrestricted district based on trip origin rather than destination. Trips originating in one type of district and terminating in another could decrease the magnitude of the treatment effect estimate. To check whether these trips affect the results, we replicate the estimation in Table 4 using only trips that originate and terminate in the same type of district and the estimates of the constraint effect are largely unchanged; the estimates and descriptive statistics of the data are shown in Appendix.

The results for the BA and DD estimators of the impact of vehicle restrictions on bus speed are set out in Table 5. As with the ride-hailing case, the effect is positive and significant but the size is considerably smaller, varying between 0.28 and 0.36 km/h (that is, between 1.8 % and 2.3 % relative to the average bus speed in the period before, during the affected hours).

Finally, the results for the BA and DD estimators of the impact of vehicle restrictions on transit validations are given in Table 6. Unexpectedly, the estimator has a negative sign and is statistically significant. The value of the DD estimator with and without control variables is −0.0302 validations per transit. Considering an average of 3.3 validations per card before the restriction during the affected hours, this represents a 0.9 % decrease in the number of validations per card. Due to the effect of replacing private vehicles with public transportation, a slight increase in this figure was expected. One possible explanation for this estimate is that, due to the short period of time to adapt, some activities were canceled or postponed, which would temporarily result in fewer trips. Another possible effect is that people who used public transportation due to the restriction made trips with fewer stops on average, which would decrease the average number of validations per card.

Table 4, Table 5 and Table 6 reveal a consistent pattern across all outcome measures and estimation methods: Santiago's vehicle restrictions generated statistically significant but modest effects. For ride-hailing vehicles, speed increases ranged from 0.76 to 0.97 km/h (3.3–4.2 % relative to baseline speeds during restricted hours), while public buses experienced smaller gains of 0.28–0.36 km/h (1.8–2.3 %). To contextualize these magnitudes, the observed speed improvements translate to relatively minor travel time savings. For a typical 10-kilometer urban trip, the 3.3–4.2 % speed increase would reduce travel time by approximately 1–2 min during peak congestion periods. While statistically robust across all estimation methods, these gains are more limited than might have been expected when implementing the restrictions. The modest magnitude of these effects reflects three key factors that influenced the policy's effectiveness.

The limited magnitude of these effects can be understood through three key factors that influenced the policy's effectiveness. First, the vintage-specific restriction design created a coverage limitation: as Fig. 5 shows, only 34.7 % of Santiago's vehicle fleet was registered before September 2011 and thus subject to restrictions, with the daily rotation system further limiting impact to 6.9 % of the total fleet on any given day. Second, this design structure meant that higher-income frequent users, who contribute disproportionately to congestion and are most likely to own post-2011 exempt vehicles, were less affected by the restrictions. Third, enforcement challenges with relatively modest fines (approximately $67 USD) may have enabled some degree of non-compliance, though direct violation data are unavailable. These factors help explain why Santiago's effects are more modest compared to international examples and why the policy had limited success in generating modal shift to public transport.

The consistency of results across our three estimation approaches (BA, DD, and DDD) strengthens confidence in these findings, with

Table 5
Impact of restrictions on bus speed.

Estimator	Without CV	With CV
BA	0.32***	0.359***
	(0.004)	(0.0041)
No. Obs.	42,057,623	42,057,623
R-Squared	0.000	0.020
DD	0.2897***	0.2855***
	(0.0094)	(0.0093)
No. Obs.	53,461,072	53,461,072
R-Squared	0.014	0.033

Standard errors in parenthesis. *** indicates significance at the 1% level.

Table 6
Impact of restrictions on transit validations (trip stages).

Estimator	Without CV	With CV
BA	−0.0513***	−0.0513***
	(0.0032)	(0.0025)
No. Obs.	6,710,106	6,710,106
R-Squared	0.000	0.359
DD	−0.0302***	−0.0302***
	(0.0034)	(0.0031)
No. Obs.	13,420,212	13,420,212
R-Squared	0.153	0.485

Standard errors in parenthesis. * indicates significance at the 10% level and *** at the 1%.

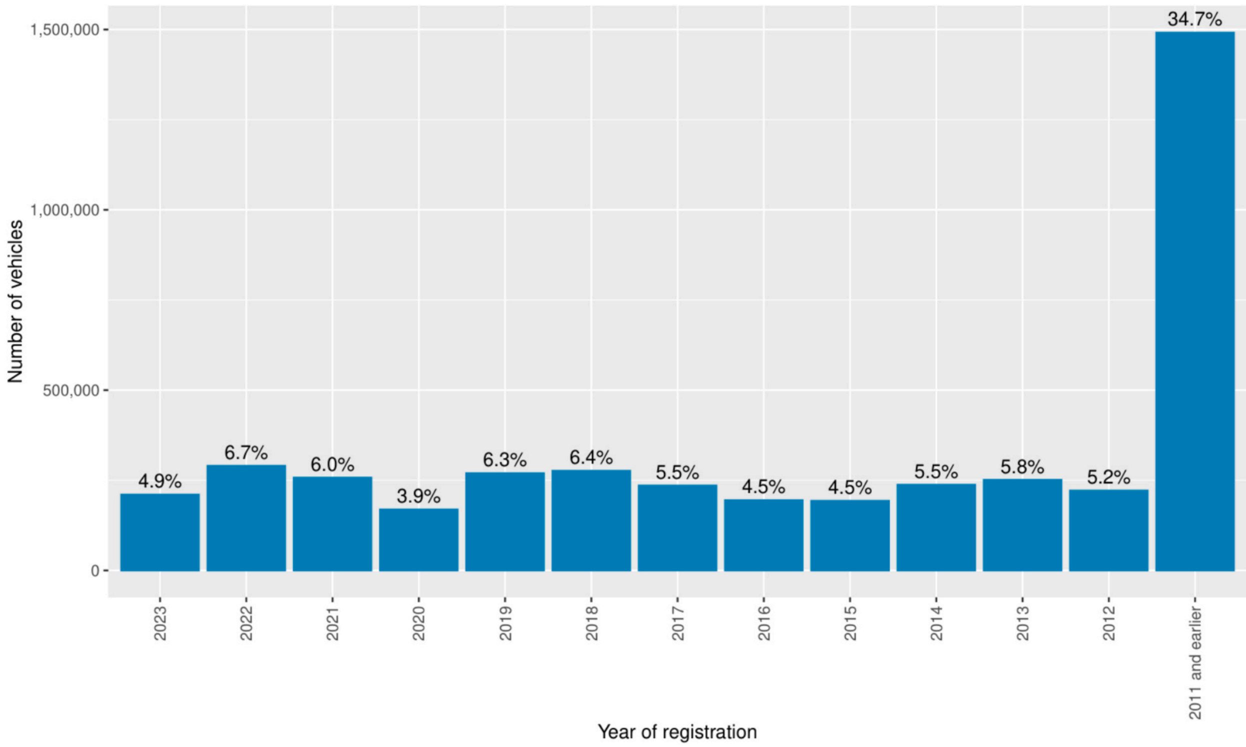

Fig. 5. Stock of Santiago Vehicles by Year of Registration).
Source: National Automotive Association of Chile (www.anac.cl

the similarity between DD and DDD estimates (0.76–0.82 km/h) suggesting that our identification strategy successfully isolates the restriction effect from other time-varying factors. The slightly higher DDD estimate (0.97 km/h) may reflect more precise control for both temporal and spatial confounders, though all methods converge on substantively similar conclusions about the modest magnitude of effects.

The differential impact across transport modes provides important insights into Santiago's urban mobility dynamics, with private vehicles (represented by ride-hailing) experiencing larger speed gains (3.3–4.2 %) compared to public buses (1.8–2.3 %) despite both operating on the same road network. This pattern likely reflects several factors: ride-hailing vehicles may be more responsive to marginal changes in traffic density due to their flexible routing and real-time optimization algorithms, allowing them to better exploit slightly less congested conditions; buses operate on fixed routes that may include segments less affected by the restrictions, such as dedicated bus lanes or peripheral areas with lower restriction compliance; and buses make frequent stops for passenger boarding, meaning that network-level speed improvements translate less directly to overall travel speeds.

Perhaps our most striking finding is the unexpected 0.9 % decrease in transit card validations during restricted hours, directly challenging fundamental assumptions about restriction policy mechanisms. This result stands in sharp contrast to successful modal shift examples from other cities: Zhang et al. (2019) found public transport ridership increases of 5–25 % across six Chinese cities, Lin et al. (2022) documented subway ridership gains of 11.5 % and bus ridership increases of 14.2 % in Zhengzhou, and even Chaziris and Yannis (2023) observed substantial metro ridership increases of 80–780 additional hourly validations per station in Athens. Several explanations merit consideration: the short implementation timeframe may have led to temporary activity suppression rather than immediate modal substitution, as successful policies typically require adjustment periods; some restricted drivers may have shifted to non-peak hours rather than switching to public transport, as documented by Qin et al. (2023) who found emissions increased by 15–20 % during off-peak hours in Shanghai due to temporal substitution. Finally, the availability of ride-hailing services may have provided an alternative to both private car use and public transport, as Lin et al. (2022) found ride-hailing services experienced the largest demand increase when Zhengzhou tightened vehicle restrictions, indicating preferences for car-like mobility even when private vehicles are restricted.

Santiago's 3.3–4.2 % speed improvements fall within the modest range typically observed in metropolitan-scale policies but are considerably smaller than effects achieved under more stringent implementations. For comparison, Liu et al. (2018) found speed increases of 23 % during peak hours when Langfang, China shifted from one-day-per-week to odd–even restrictions, while Yang et al. (2018) documented travel time savings of 34 % during morning peaks and 57 % during evening peaks in Beijing's restricted hours. Li and Guo (2016) observed 10–20 % speed increases during Beijing's Olympic restrictions affecting over 50 % of vehicles. Even Jakarta's removal of high-occupancy vehicle restrictions produced larger effects in reverse, with Hanna et al. (2017) documenting delay increases of 46–87 % when the policy was eliminated. Santiago's modest effects align more closely with other Latin American experiences: Davis (2008) found no measurable traffic improvements from Mexico City's restrictions after behavioral adaptations, while de Grange and Troncoso (2011) documented only 5.5 % traffic reductions during Santiago's own pre-emergency restrictions in 2008.

Our results are also consistent with the general pattern observed in our literature analysis: among studies of large-zone restrictions (citywide or metropolitan), only 25 % received positive assessments compared to 43 % for limited-zone interventions. This pattern suggests that the spatial scope of restrictions significantly influences their effectiveness, with citywide implementations like Santiago's facing greater enforcement challenges, more opportunities for spatial substitution, and diluted impacts across the broader urban network compared to targeted interventions in dense central areas.

Synthesizing across all outcome measures, Santiago's 2023 vehicle restrictions achieved limited success, generating measurable but modest traffic improvements while failing to advance broader sustainable mobility goals through modal shift to public transport. This aligns with findings from other metropolitan-scale implementations and provides evidence of limitations and challenges of license-plate based policies, as previously reported across multiple Latin American cities (Cantillo & Ortúzar, 2014).

4. Conclusions

This study provides an empirical evaluation of Santiago's 2023 vehicle restriction policy using novel high-frequency data from ride-hailing platforms, public bus GPS systems, and transit card validations. Our analysis employs multiple econometric approaches, namely before-and-after comparisons, difference-in-differences, and triple difference estimation, to isolate the causal effects of restrictions on traffic speeds, bus performance, and public transport utilization. The use of triple difference estimation with spatial and temporal controls, enabled by our detailed datasets, allows for more precise identification of policy effects by controlling for both spatial and temporal confounders.

Our findings reveal that while Santiago's vehicle restrictions generated statistically significant effects across all outcome measures, the practical impacts were modest and fell well short of expectations. Private vehicle speeds (measured through ride-hailing data) increased by 3.3–4.2 %, translating to minimal travel time savings of 1–2 min on typical urban trips. Public bus speeds improved by an even smaller margin (1.8–2.3 %), suggesting limited network-wide benefits. Most strikingly, transit card validations decreased by 0.9 % during restricted hours, directly contradicting the modal shift objectives and highlighting fundamental limitations in restriction design. These limited effects stem from three critical design characteristics that systematically undermined policy effectiveness. First, the vintage-specific restriction structure created a severe coverage problem, affecting only 6.9 % of Santiago's vehicle fleet on any given day. Second, by targeting older vehicles, the policy exempted higher-income frequent drivers who contribute disproportionately to congestion. Third, weak enforcement mechanisms with modest fines likely enabled substantial non-compliance, though direct violation data remain unavailable.

Our results contribute to evidence questioning the effectiveness of license plate-based restrictions in large metropolitan contexts. Santiago's experience aligns with unsuccessful implementations documented in other Latin American cities (Cantillo & Ortúzar, 2014), contrasting sharply with more targeted interventions in smaller, denser areas such as Quito's successful Pico y Placa program (Carrillo et al., 2016). Our literature analysis shows only 25 % positive assessments for large-zone restrictions versus 43 % for limited-zone policies, suggesting that spatial scope fundamentally determines policy effectiveness. The findings suggest that vehicle restriction measures like Santiago's can have limited effects if not designed appropriately: effective restrictions require targeting a large share of vehicles within a spatially concentrated area, combined with strong enforcement and consideration of socioeconomic factors. Going forward, transport demand management policies should consider more efficient instruments such as congestion pricing, public transport incentives, and integrated urban mobility strategies that combine multiple policy tools.

Finally, this study demonstrates the analytical potential of high-frequency digital mobility data, including ride-hailing trips, bus GPS records, and transit card validations, for transportation research, enabling more precise measurement of policy impacts than traditional aggregate indicators. However, several limitations warrant acknowledgment: our analysis focuses on short-term effects, the ride-hailing data we use represents a subset of private vehicle traffic, and we cannot assess broader outcomes such as air quality impacts. Future research should extend the evaluation timeframe to capture longer-term behavioral adaptations and integrate multiple mobility data sources for more complete understanding of restriction impacts.

CRediT authorship contribution statement

Louis de Grange: Writing – review & editing, Writing – original draft, Visualization, Validation, Data curation. **Raúl Pezoa:** Writing – review & editing, Writing – original draft, Visualization, Validation, Data curation. **Rodrigo Troncoso:** Writing – review & editing, Writing – original draft, Formal analysis.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix

Estimates of the effect of restriction on ride-hailing vehicle speeds using only trips that begin and end in districts of the same type (restricted or unrestricted).

Table A1
Descriptive statistics.

Variable	Restricted districts			
	Before		After	
	Non-affected hours	Affected hours	Non-affected hours	Affected hours
Ride Hailing Speed (Km/h)				
Mean	29.87	23.06	29.92	23.88
Std. Dev.	11.41	9.17	11.29	9.38
No. of observations	93,182	429,818	84,876	408,428
	Unrestricted districts			
Ride Hailing Speed (Km/h)				
Mean	28.40	23.04	29.05	23.74
Std. Dev.	8.91	8.73	8.77	8.87
No. of observations	2,259	12,962	2,214	13,333

Table A2
Impact of restrictions on ride-hailing vehicle speed.

Estimator	Without CV	With CV
BA	0.8215***	0.8795***
	(0.0203)	(0.0212)
No. Obs.	838,246	838,246
R-Squared	0.002	0.197
DD	0.7721***	0.8337***
	(0.0575)	(0.0511)
No. Obs.	1,016,304	1,016,304
R-Squared	0.061	0.296
DDD	0.7185**	0.8005***
	(0.2915)	(0.2977)
No. Obs.	1,047,072	1,047,072
R-Squared	0.061	0.293

Standard errors in parenthesis. ** indicates significance at the 5 % level and *** at the 1 %.

Data availability

Data will be made available on request.

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