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The effect of transport policies on car use: Evidence from Latin American cities ☆

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ABSTRACT

In an effort to reduce air pollution and congestion, Latin American cities have experimented with different policies to persuade drivers to give up their cars in favor of public transport. This paper looks at two of such policies: the driving restriction program introduced in Mexico City in November of 1989—Hoy-No-Circula (HNC)—and the public transport reform carried out in Santiago in February of 2007—Transantiago (TS). Based on hourly concentration records of carbon monoxide, which comes primarily from vehicles exhaust, we find that household responses to both HNC and TS have been not only ultimately unfortunate—more cars on the road and higher pollution levels—but also remarkably similar in two important aspects: on how policy responses vary widely among income groups and on how fast households adjust their stock of vehicles, when they do.

1. Introduction

Air pollution and congestion remain serious problems in many cities around the world, particularly in emerging economies because of the steady increase in car use. Latin American cities have experimented with different transport policies to deal with these problems (EIU, 2010). In November of 1989, for example, authorities in Mexico City introduced a program, Hoy-No-Circula (HNC), that restricted drivers from using their vehicles one weekday per week. More recently, in February of 2007, authorities in the city of Santiago-Chile embarked in a transportation reform, Transantiago (TS), that completely reorganized the bus routing and scheduling system of the entire city. Given the complexity of transport dynamics in any large city, drastic interventions like these represent unique opportunities to improve our understanding of how households of different income levels respond to these policies. This paper is an empirical attempt at evaluating these responses; particularly, the long-run responses, that is, whether and how fast households adjust their stock of vehicles.

Since the two policies share similarities but also important differences, it helps for the reliability and applicability of our methodology to consider both policies and not just one. The two policies amount to one-time drastic interventions like no other in the region: they changed the relative prices of transport options at once and overnight in cities that exhibit great income disparity. While it is true that both policies are implemented almost 18 years apart and in cities that differ in various aspects, the main difference is in the type of policy and in the way households are expected to respond to them in the long run. By lowering the value of owning a car, a driving restriction like HNC produces opposite reactions among households, so its overall long run effect is ambiguous. For some it may be optimal to give up their cars or postpone the purchase of a first car while for others it may be optimal to buy a second car in order to continue driving each and every day of the week. Conversely, a transportation reform like TS does not suffer from such ambiguity. A reform that increases the relative price of using public transport across the city should make the use and purchase of a car more attractive to all households (and the opposite if the reform decreases the price of using public transport). In the specific case of TS, we know from different sources that TS failed to improve public transport, instead it led to longer travel times for the vast majority of riders,

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thus increasing the cost of using public transport for almost all. This lack of ambiguity in the direction of the cost change also provides a test of our methodology. Failing to find a long-run increase in car use would be a major setback for our methodology, not only as applied to TS but also to HNC.

We are not the first to look at whether HNC and TS succeeded or not in persuading drivers to give up their cars in favor of public transport. There is already an understanding that these policies did not work as intended. (e.g., Onursal and Gautam, 1997; GDF, 2004) in part because compliance with the program was near universal (e.g., Eskeland and Feyzioglu, 1997). Most agree, however, that over the longer term it led to an increase in the number of vehicles on the road (e.g., Eskeland and Feyzioglu, 1997; Onursal and Gautam, 1997; Molina and Molina, 2002; Davis, 2008). TS's new routing and scheduling system, on other hand, was expected to deliver significant reductions in car use (DICTUC, 2009). Its actual implementation, however, has been recognized as a major policy failure resulting in a significant and permanent increase in the cost of using public transport from the very first day (e.g., Briones, 2009; Muñoz et al., 2009).[1]

The contribution of this paper is to characterize and quantify how households adjust to these policy shocks in the short- and long-run and how that adjustment varies across income groups.[2] Understanding these effects is important for policy evaluation and design. For example, accounting for differential location effects within a large city can be particularly relevant for quantifying health costs associated to non-uniformly mixed pollutants (e.g., carbon monoxide, ozone, particulates), that is, pollutants for which the location of the polluting sources matter. And these costs can be substantial as reported elsewhere (e.g., Currie and Neidell, 2005). Likewise it can help better quantify policy effects across income groups and time (e.g., we find HNC to have desirable effects in middle-income groups but only in the short-run).

In the absence of a direct measure of car driving, our empirical evaluation is based on a by-product of car usage: hourly observations of concentration of carbon monoxide (CO), which are recorded by a network of monitoring stations distributed over the two cities (stations also keep records of other pollutants and weather variables). As argued in detail in Section 4 of the paper, CO is a good proxy for vehicle use, particularly at (morning) peak hours, compared to alternative candidates like hourly records of traffic flows and of other pollutants. Mobile sources, and light-duty vehicles in particular, are by far the main emitters of CO—97% and 94%, respectively, at the time HNC and TS were implemented.

Our empirical strategy is to compare CO records at peak hours two years before and after policy implementation (while controlling for day-of-the-week and month-of-the-year fixed effects, economic activity and meteorological conditions). Our strategy handles well the daily volatility in the data, which is common in pollution time series,[3] because we do not estimate the impact of the policy on any particular day (e.g., right after policy implementation) but rather over a period of several months. A potential problem with our approach, however, is that there may be still time-varying (economic and/or weather) factors we cannot control for, so past CO records may not build a good counterfactual of how CO would have evolved in the absence of the policy. In other words, the effects of these time-varying omitted factors on CO would be entangled with those of the policy. In addition, there may be an endogeneity problem if the introduction of the policy is correlated with an increasing trend of congestion and pollution. One way to get around both of these "long-run" concerns is to focus exclusively on the short-run impact of the policy using a regression discontinuity design (RDD), as done by Davis (2008) for HNC, while hoping for the daily volatility to be less of a problem. We also run a RD exercise in the paper but instead using the RD estimator of Imbens and Kalyanaraman (2012) which is based on local linear regressions on either side of the threshold (i.e., time of policy implementation).

We are often, however, less interested in the immediate or short-run effect of a policy than in its long-run effect. Indeed, according to Litman (2011) it may take 1 to 3 years for some policies (e.g., new transit service) to reach its full potential. We then implement two sets of exercises to deal with the above long-run concerns. First, we run a difference-in-difference regression with a comparable pollution data from a monitoring station in a city that was not subject to the policy shock but did follow a similar trend in air pollution before the policy was enacted. There is no such city for Mexico City—because lack of data—but fortunately there is one for Santiago; a small one with a monitoring station that exhibits an ex-ante CO trend that makes it indistinguishable, in relative terms, from the CO trends of any of the monitoring stations in Santiago. For HNC, we run alternative "control" exercises. One is a falsification test that computes the effect of a placebo shock implemented two years before the actual policy. The other looks at policy impacts on Saturdays and Sundays, i.e., when HNC is not active. These impacts provide both a falsification test for short run effects, since HNC should have little impact than except perhaps for a most modest amount of intertemporal substitution, and a robustness check for long-run effects, since any adjustment in vehicle capacity should also be reflected on car use in days in which the policy is not active. The second set of exercises exploits the existence of a network of monitoring stations in each city to identify municipalities in which the transport policy (either TS or HNC) should have no or limited effects. Estimates from these municipalities would then provide a good idea of potential time-varying omitted factors that could be affecting other municipalities in the city. More generally, the empirical estimates from all individual stations should tell us whether the policy effects we find across income groups are theoretically sound.

Empirical results at the *city level* for HNC show for morning-peak hours a reduction in CO concentration in the range of 5–13% for the first month. This short-run result is in line with the perception of high compliance with the program and with the initial announcement that the program would only last for three months (GDF, 2004). For the long run we find an increase in CO of 11%, which is reached about 12 months after implementation. Interestingly, estimates for Saturdays and Sundays (also at morning-peak hours) show little reduction in the short-run and a significant net increase in the long run close to 20%.[4] Most of the increase in CO prompted by HNC is less due to extra traffic from additional cars than to the fact that these additional cars pollute way above the fleet average. As for TS, we find virtually no impact on CO in the short run, consistent with the near-zero elasticities of Litman (2011), and a city-level increase of 27% in the long run, which is reached about 7 months after implementation.[5] In this case, the increase in CO is a combination of more cars (from households of different income levels) and more congestion.

The above city-level figures mask a wide heterogeneity in responses. Looking at CO records from individual monitoring stations, we find HNC to have its largest impact in middle-income groups, where some households that own a car are likely to buy a second (and older) one to bypass the restriction, and much lower in high- and low-income neighborhoods but for different reasons. High-income households have

[1] The Economist (Feb 7th, 2008) referred to TS as "…a model of how not to reform public transport."

[2] In this paper, short-run and immediate impact are used interchangeably and long-run is the time it takes (most) agents to adjust their stock of vehicles as a response to a policy shock. There may be longer-run effects (e.g., inter and intra city migration of people and commercial activity) but our methodology is not well suited to identify effects that are far away from the policy shock.

[3] The probability that the difference in CO daily concentration between any pair of adjacent weekdays be more than 20% is 26.6% in the HNC time series and 59.2% in the TS series; and be more than 50% is 7.4% and 22.7%, respectively.

[4] This net increase comes close to the 24% net increase during weekdays (from −13% to +11%). These net increases are all statistically significant at 1%.

[5] Note that the short-run results for both HNC and TS are similar to those obtained with the RDD estimator of Imbens and Kalyanaraman (2012), when used with monthly averages, but are different from the short-run results of Davis (2008) for HNC. Part of the reason is that Davis (2008) does not distinguish for the hour of the day and location of the monitoring stations as we do. More on this is in Section 5.

already sufficient car capacity to cope with the driving restriction while only a few low-income households own a car, and those that do, cannot afford a second one. Despite TS increased the cost of using public transport uniformly over the city, its impact is also felt uneven across municipalities and consistent with the ex-ante (heterogeneous) use of public transport. While the short-term impact is negligible everywhere, the long-term impact, measured as the increase in CO levels, varies inversely with income from about 40% in the poorest areas to 17% in the richest municipality. Note that these results are theoretically sound which is reassuring for our methodology in that it captures well how responses vary with income in two very different settings.

There are valuable policy lessons from these experiences. One is that policy impacts are likely to vary widely among different income groups. Another one is that driving restrictions can be quite effective—partly because they are relatively easy to enforce, as HNC shows—in reducing car use, but only in the short run and for some income groups.[6] So rather than working on a permanent basis, driving restrictions could be used sporadically; for example, to attack short episodes of very bad pollution. And a third lesson is how fast households adjust, when they do, to both policies. The speed at which the stock of vehicles has increased in both cases appears much faster than that suggested by the transportation literature (e.g., Litman, 2011; Paulley et al., 2006).[7] It is also faster than that suggested by the earlier literature on consumption of durable goods (e.g., Caballero, 1990) but closer to the more recent literature (e.g., Chah et al., 1995; Gallego et al., 2001) that finds that over 90% of the adjustment to a demand/supply shock is reached within the first year of the shock. In any case, both policy experiences confirm that the adjustment process is quite fast which leaves little room for ex-post corrections. This calls for nothing but more careful ex-ante policy design, including the combination of instruments and a serious consideration of market-based instruments such as road pricing and pollution taxes (e.g., Fullerton and Gan, 2005) that so far has received none in the region.

The rest of the paper is organized as follows. Section 2 describes the two transport policies in more detail. Section 3 provides a theoretical framework, based on the model that is in our companion paper (Gallego et al., forthcoming), to guide the empirical analysis. Our empirical strategy is described in Section 4 while the empirical results are in Sections 5 and 6 for HNC and TS, respectively. We conclude in Section 7. In the online Appendix we discuss how additional evidence—coming from gasoline sales, car registrations, car sales and traffic flows—is consistent with the CO results above.

2. Transport policies in Mexico City and Santiago

HNC was established on November 20 of 1989, as a response to record levels of air pollution and congestion in Mexico City (Molina and Molina, 2002). The program banned every vehicle—except taxis, buses, ambulances, fire trucks and police cars—from driving one weekday per week, from 5 am to 8 pm, based on the last digit of its license plate (GDF, 2004). The program did not experience any relevant changes for the next two years, but in 1991 authorities begun introducing additional environmental policies (GDF, 2004; Molina and Molina, 2002; Onursal and Gautam, 1997).[8] If anything, these additional policies would bias our long-run results downward making the case that HNC did lead to more CO even stronger.[9]

There is also the issue of whether some households could have moved forward the purchase (and use) of an additional car to right after the announcement of HNC (November 6) or even before that in anticipation of a possible increase in car prices because of HNC. There are several reasons to believe this should not be a concern, namely, that the initial announcement of HNC had the program lasting until the end of February (and only then, the program was officially made permanent), that the effect of HNC on the stock of vehicles seems to have been rather modest,[10] and that there was not much time between the announcement and implementation so at best only very few households could have adjusted so quickly. Even if there was some anticipation (which we did not find when we test for it in some of our regressions), and hence, some increase in emissions a couple weeks before implementation, the effect of this in the construction of our counterfactual (and thus in our estimations) would be negligible in that our estimation strategy is *not* based on a narrow window of a few days or weeks around the policy.

Fig. 1 plots average daily CO concentration levels for peak hours for the period 1987–1991, which is the 4-year (symmetric) window we use in our empirical estimation of HNC. The daily levels were constructed by averaging over morning peak hours (8–10 am) and all monitoring stations. The vertical line indicates the exact moment HNC was implemented. Had HNC being effective in making people substitute away from the car, one would expect to see some of it reflected in a reduction in CO concentrations. A quick look at the plot shows no clear indication of a decrease in CO in the short-run neither a subsequent increase over the long-run. As we elaborate further in the online Appendix, CO records are highly volatile and therefore relying on visual inspection to determine policy impacts is a bit risky.

Nearly 18 years later, on February 10 of 2007, Chile's government implemented TS, with a motivation similar to that of HNC, that of persuading drivers to give up their cars, but by improving—supposedly—the quality of public transport. The old public transportation system was regarded as highly polluting, unsafe, and inefficient both in terms of travel time and cost (e.g., Briones, 2009; Muñoz et al., 2009). The TS reform involved a significant reduction in the number of buses, from roughly 7500 to 5500,[11] together with a radical (centrally-planned) change in the number, design and scheduling of bus routes more in line with a hub-and-spoke network where the existing subway system would play the role of the hub and where passengers were expected to make many more transfers than in the past to complete their trips. All these changes were introduced at once, i.e., on a single day.

While originally conceived to reduce the relative cost of using public transport, TS has done the exact opposite. Table 1 provides numbers illustrating the extent of the intervention. Commuting time increased,

[6] Lin and Zhang (2012) also find for cities not included in our analysis that driving restrictions may lead to pollution reductions in the short run.

[7] Litman's (2011) survey paper suggests that cross-elasticities between public transport and automobile travel are virtually zero within the first or second year of the shock (0.05) but can increase over time (after 5 years) to as high as 0.40.

[8] For example, in 1991 authorities introduced a taxi modernization program that sought to replace all pre-1985 taxis with newer vehicles meeting stricter emission standards over a two-year period. The same year drivers saw vehicle inspections to increase from 1 to 2 per year and the introduction of stricter emission standards for new vehicles (i.e., use of a catalytic converter in all new models). Gasoline specifications have also been tightened over the years; in particular, unleaded gasoline (called Magna Sin and equivalent to the 87-unleaded gasoline sold in the US) became first available in September of 1990 but in a few gas stations. In addition, a major oil refinery located in the district of Azcapotalco (not too far from the monitoring stations Tlalnepantla and I.M. del Petróleo) was shut down on March of 1991 (Carmona, 1992).

[9] We say "if anything" because we expect the effects of these additional policies on CO to be minor within our window of estimation. For example, the refinery's contribution to total CO between August 1989 and January 1991 was less than 2%, or 50 thousands tonnes per year (Carmona, 1992). Likewise with the use of unleaded gasoline: it had no effect on CO emissions of older models (1986–90) and a 60% reduction in the new models (1991–96) (www.ref.pemex.com/octanaje/25magna.htm). But as explained by GDF (2004), it took a few years for unleaded gasoline to become widely available so it was usual in those early years to see cars with catalytic converters running on leaded gasoline. In any case, assuming that all cars sold in 1991 (around 6% of the fleet) were 60% cleaner from running on unleaded gasoline, the impact on CO was less than 0.5% (recall that the CO share of the newest 18% of the fleet was only 2%; see Beaton et al., 1992).

[10] According to GDF (2004), of the total number of new cars sold in Mexico in 1990, 44.1% went to Mexico City as opposed to 45.6% in 1989 and 46.5% in 1988. In addition, the numerical simulations in Gallego et al., forthcoming suggest that HNC prompted an increase of 5% in the stock of vehicles.

[11] See Briones (2009) for more details. More importantly for our analysis, the share of public transportation on CO emissions is only 3% (CONAMA, 2004), so such a reduction in the number of buses has virtually no effect on CO concentrations. Likewise, any changes in CO emissions from industrial boilers and power-plants would go unnoticed since their CO share is only 0.5%. We should, on the other hand, expect TS to have a greater and negative impact on particulates (PM10) due to the presence of fewer and cleaner buses that traditionally have been a main contributor of that pollutant — 33% according to CONAMA (2004). Using also high-frequency data, Figueroa et al. (2013) find this to be the case.

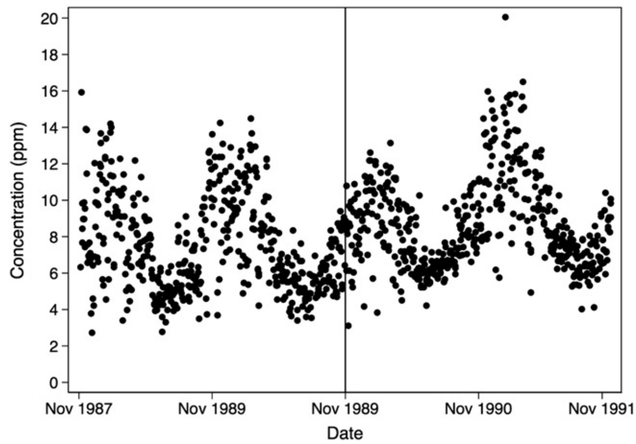


Fig. 1. Daily CO records during peak hours for HNC.

Table 1
Travel time before and after TS.

Indicator	Before TS	After TS		
		0–6 months	12–18 months	2010
Total number of buses[a]	7472	5444	6396	6649
% of people waiting at least 10 min at bus stop[b]		21.0	7.1	
Waiting time per connection[b]		6.08	3.65	3.49
Travel time to work (both ways; min)[c]	76.8	89.7		
Travel time by transportation mode (both ways; min)[c]				
Public transportation	102.4	133.3		
Private car	65.4	63.4		
Taxi	35.1	33.9		

Sources.
[a] Subsecretaria de Transporte, Ministerio de Transporte y Telecomunicaciones.
[b] DICTUC, several reports.
[c] Bravo and Martínez (2007).

on average, from 77 to about 90 min (both ways), mainly because of the increase in the average travel time of public transport that went up by about 30% (from 102 to 133 min). In contrast, travel time of cars and taxis does not seem to have been affected nearly as much. Unlike HNC, TS has suffered from some adjustments but with minor effects on the quality of service which has remained way below pre-TS levels, even after four years of implementation.[12]

The issue of policy anticipation is even more unlikely in the case of TS despite its launch was announced several months in advance; this is simply because nobody anticipated the actual outcome. As shown in Fig. 2, good evidence on the latter is provided by the stable prices of taxi licenses in the city of Santiago the year before implementation. Furthermore, the significant ex-post increase in prices, although not immediate as one would expect in quota markets like this where price formation takes time (Joskow et al., 1998), provides additional evidence of the impact of TS on the quality of public transport. In fact, in Appendix A we show, econometrically and using Lagos' (2003) model, that the demand for taxicab rides in Santiago has at least doubled because of TS.

In any case, this deterioration in quality should have resulted in a switch towards alternative modes of transportation, e.g., cars, and hence, in an increase in CO emissions. Fig. 3 plots average daily CO concentration levels for peak hours (7–9 am) for the period 2005–2009 which is the period we use in our empirical estimation and the vertical line indicates the exact moment TS went into operation. Again, there is no strong indication in the plot that TS have resulted in more CO; perhaps some if we look at the winter months.

3. Motivating theory

It is useful to go to the empirical analysis with a theoretical background illustrating the potential responses households might have to these policies. In Gallego et al. (forthcoming) we develop a novel model of transportation decision-making that captures in a simple way the essential elements of a household's problem which are the allocation of existing vehicle capacity, if any, to competing uses (peak vs off-peak hours) and how that capacity is adjusted in response to a policy shock. Unlike other transport models, our model is particularly well equipped to study the effects of policy interventions that affect car utilization such as driving restrictions. Existing models that aggregate preferences at the level of a representative agent or by income, would miss much or all of the action that our model captures, for example, that two households of similar income may have quite different responses.

Borrowing from the "bundling" literature, households are both horizontally and vertically differentiated: they differ in their preferences for transportation modes—cars vs public transport—and in the amount of travel. Some households will find it optimal to purchase the car-bundle (i.e., use the car for both peak and off-peak hours), others to rely exclusively on public transportation (bus-bundle), yet others to "two-stop shop" (e.g., car for peak travel and buses for off-peak travel). One of the advantages of the model is that it can be calibrated and utilized for policy simulations and estimations of policy costs using few observables at the city level, namely, the fraction of households owning either none, one, or more cars, the share of car trips at peak hours, the share at off-peak hours, and the ratio of peak trips over off-peak trips. One still needs to make an assumption about the distribution of horizontal and vertical preferences in the population.

One message of the model is that the shot-run impact of a policy can say little about its overall performance; hence, the importance to also empirically estimate its long-run effect. Take a HNC-like policy, for instance, which the model captures with a reduction in car capacity. The short-run impact is unambiguous, at least qualitatively: a reduction in the number of car trips (and in CO levels). Depending on the cost of buying or selling a car, however, the long-run impact of the policy can go either way. The reason for this ambiguity is that there are two forces at play. By lowering the value of owning a car, a driving restriction like HNC produces opposite reactions among households. For some it may be optimal to give up their cars or postpone the purchase of a first car while for others it may be optimal to buy a second car to restore the value of owning a no-restriction car. The model shows that if purchasing

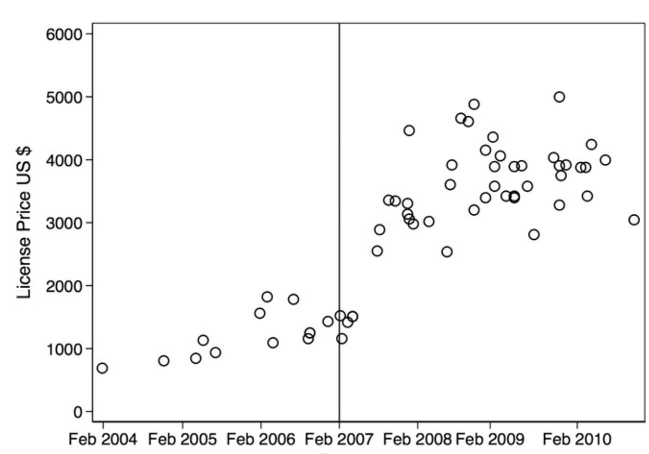


Fig. 2. Prices of taxi licenses in Santiago (sub-sample of license-only ads).

[12] The main adjustment was an increase in the number of buses within a period of 12 months (see the last two columns of Table 1). More importantly for our estimation, the fact TS suffered some modifications after implementation should not bias our results in any meaningful way. If anything, the speed of adjustment may be under-estimated as some commuters probably decided to wait longer to see how much of an improvement in service was likely before purchasing a car.

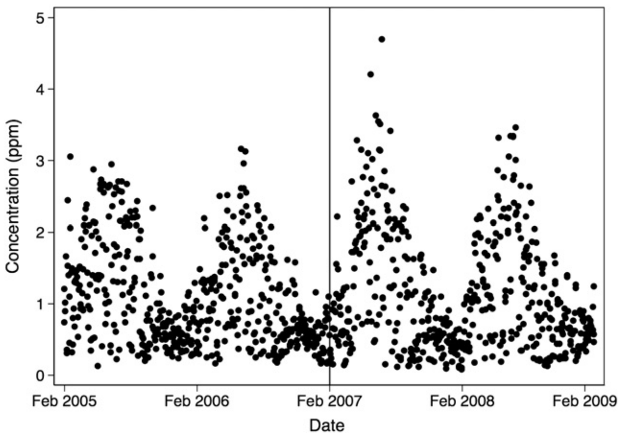


Fig. 3. Daily CO records during peak hours for TS.

an additional car to bypass the driving restriction costs as much as a new car, the reduction in car trips is likely to remain in the long-run or even extend if there are enough households that find it optimal to return their existing cars (this latter is unlikely because of lemon and transaction costs associated to trading in a used car, which the model also considers). Conversely, if no household returns its car and those households that decide to purchase an additional vehicle to bypass the restriction opt for an older and cheaper car, the policy results in an increase in the number of vehicles in the long-run but not necessarily in more trips during weekdays, which is when the restriction is active. For periods in which the restriction is not active—weekends—any increase in cars will necessarily lead to an increase in car trips. A similar "short- vs long-run" argument applies to a public transportation reform like TS, which the model captures with a uniform increase in the relative cost of using public transport for the entire city. Regardless of the direction of the price change, its short-run effect on car use (and CO) is likely to be small and hard to detect empirically.[13] The long-run effect, however, can be shown to be substantial.

A second message that comes out of the model is that policy impacts are likely to vary widely across income groups for either type of policy and in a distinctive way. This can tell us a great deal of whether the empirical estimates we obtain for the different monitoring stations are theoretically sound. A driving restriction like HNC has its greatest impact in middle-income groups. Many of these households already own one car, probably none return their cars and a good fraction of them are likely to buy an additional car to bypass the restriction. Under these conditions, we should expect to see a significant reduction in the short run (because they only own one car) followed by a noticeable increase in the long run. High- and low-income households are not hit as much but for different reasons. High-income households have already sufficient car capacity (from owning two or more cars) to cope with the driving restriction while only a few low-income households own a car, and those that do, cannot afford a second one. On the other hand, a city-wide transport reform like TS is also likely to have very heterogenous effects in CO in the long-run (while minimum in the short-run regardless of location): lowest among the rich—that rely less on public transport—and highest among the poor. Numerical exercises in Gallego et al. (forthcoming) show that it is not unusual to find threefold differences in impact between the two groups.

In addition, the model in Gallego et al. (forthcoming) can be used to compute the transportation costs inflicted by these policies. One of the most interesting findings in that regard is that despite some households adjust over the long run, policy costs remain largely unchanged in the long run, so any cost-benefit analysis may well abstract from long-run adjustment considerations. One reason is that households that decide to buy an additional (or first) car because of the policy were households that before the policy were not that far from buying that additional (or first) car anyway (they did not do it before because buying a car is a lumpy investment). And even if a policy prompts a much larger response in terms of additional cars on the street, the long-run losses are still likely to be slightly smaller than the short-run losses. As we increase the policy shock, not only we increase the number of households adjusting to the shock but also the costs borne by those that do not adjust.

4. Empirical strategy

We now describe our empirical approach in terms of data and econometric specifications. We start with a discussion of why CO records act as a good proxy for car use.

4.1. Why CO?

A natural candidate to study changes in car use is a time series with hourly readings of vehicle traffic from traffic-control stations. There exist a number of problems with this "proxy". To start, we do not have this information for Mexico City (at the time of HNC, at least). Second, we only have data for a partial count of the total vehicle traffic in Santiago as stations are highly concentrated in the northeastern part of the city. Third, traffic counts, like CO records, do not distinguish between private and public transportation flows. Fourth, and more importantly, the use of this local information presents a number of problems from theoretical and empirical points of view. There may be general equilibrium and displacement effects in which, for instance, temporary local interventions or increases in congestion at a particular location (street) force drivers to look for alternative streets (e.g., a station in a clogged street would report virtually no traffic flow). As the counting stations cover only a small fraction of the streets, it is impossible to record all these "detour" flows. Therefore, these traffic records can greatly underestimate car use. It is not yet obvious to us and to the literature how to aggregate this partial traffic data in a way that can correct for these problems.[14]

Despite it does not measure car usage directly but a by-product of it, our preferred proxy for car use is CO at peak hours. Given the complexity of transport dynamics in large cities like Mexico City and Santiago, the use of hourly CO concentration records appears encouraging for several reasons. First, according to emission inventories, mobile sources, and light-duty vehicles in particular, are by far the main emitters of CO—97% and 94%, respectively, at the time HNC and TS were implemented.[15] Hence, we should expect any change in city traffic be picked up by changes in CO concentrations. Second, CO measures, unlike hourly records of vehicle traffic, are better at capturing effects at the scale of the city or municipality rather than at a particular location (e.g., street). Third, and closely related to the latter, since CO emissions are inversely related to vehicle speed for a wide range (Robertson et al., 1998), CO levels grow monotonically with traffic (i.e., number of car trips) regardless of the initial level of congestion.

But the main advantage of using CO is that concentration records at the morning peak, say at 8–9 am, are directly related to traffic activity at that time of the day.[16] One reason for this is that CO is the only

[13] Besides the negligible cross elasticities between public and private transportations documented by Litman (2011) for the short-run, the 2006 Origin–destination survey for the city of Santiago (EOD-2006 for its initial in Spanish) shows that most of the (passenger) cars in the city (799,811) must be already in use to cover an equivalent number of morning trips (706,518).

[14] See Daganzo (2007) for a discussion on the limitations of using this "microscopic" approach (i.e., using data at the station level) to learn about transportation patterns *at the city level*.

[15] The CO figures for Mexico City are from the 1998 emissions inventory (CAM, 2001) and for Santiago from the 2004 inventory (CONAMA, 2004). Light vehicles, which include passenger cars and commercial vehicles other than buses and trucks, are responsible for 72 and 88% of CO emissions in Mexico City and Santiago, respectively. The same inventories report that mobile sources are responsible for, respectively, 36 and 56% of PM10 emissions.

[16] We thank Rainer Schmitz (Geophysics Department, University of Chile) for the explanations that follow and for convincing us to concentrate our efforts on CO estimations at morning peak hours.

pollutant that can be regarded as non-reactive (i.e., that does not react with other pollutants or to sunlight) on a time scale of one day, which is more than enough time for a pollutant to get dispersed (Schmitz, 2005). A second reason is that under stable meteorological conditions—before and around the morning peak—rapid increases in vehicle use (and in CO emissions) are immediately reflected in changes in CO pollution levels that only get dispersed as wind picks up later in the morning (Jorquera, 2002). As Fig. 4 illustrates for a week day in the month of January 2002 in Santiago, these chemical–atmospherical conditions produce a low-correlation mapping between CO emissions and concentration for most of the day except at the morning peak and after taking into account the background pollution that exists before the peak forms. This brief period of high correlation is what we exploit in our CO estimations. For this same reason we abandon the use of other pollutant records associated to car use, like nitrogen oxide (NO_x), for which we failed to find such a systematic pattern.[17]

Another reason to focus on CO records at peak hours is that readings from individual monitoring stations are likely to capture the commuting of nearby households (later in the day and as winds develop, concentration records at one particular station become "contaminated" by emissions from distant locations). In addition to the two reasons above—rapid build-up and low dispersion of CO at peak—the distance between monitoring stations (10/20 km), the relatively low vehicle speed in the morning commute (20 km/h) and the fact that most trips (particularly from high- and low-income zones) end up in downtown, should prevent that much of the CO recorded by one monitoring station be also recorded by a second station. And if it does, it is probably by stations located in places with households of similar income (in the online Appendix there are maps of Mexico City and Santiago with the location of the monitoring stations we use in the estimations).

4.2. The pollution data

Our datasets are high-frequency readings of pollution concentration and weather variables of a network of monitoring stations in each city. In Mexico City, the network is operated by the Department of Environment and Natural Resources (www.semarnat.gob.mx). At the time of HNC, it reported hourly measures of CO—and other several pollutants, namely, (ground-level) ozone, nitrogen dioxide (NO_2), NO_x, and sulfur dioxide (SO_2)—and for some of the stations, it also reported hourly measures of temperature, real humidity, wind speed and wind direction. The average failure rate of the network—fraction of time stations do not report CO information—is about 31% and roughly constant over time (before and after HNC) and across different days of the week and hours of the day. Summary statistics of variables used in the HNC estimations are in Table B.1 of Appendix B.

In Santiago, the network is operated by the National Environmental Commission (www.conama.cl). Each station also collects hourly measures of CO, ozone, NO_2, NO_x, SO_2, and particulates smaller than 10 and 2.5 μm (PM10 and PM2.5, respectively) as well as hourly measures of temperature, real humidity, precipitation, atmospheric pressure, wind speed, and wind direction. Failure rates are much smaller than in Mexico City (9.4% on average) but there are different patterns *before and after* TS. While the overall failure rate decreased from 6.6% to 4.9 at morning peak hours, it increased from 4.3 to 6.9% at off peak hours. In addition, the unit of measurement in which CO was recorded in each station changed over time: while before TS the concentration level

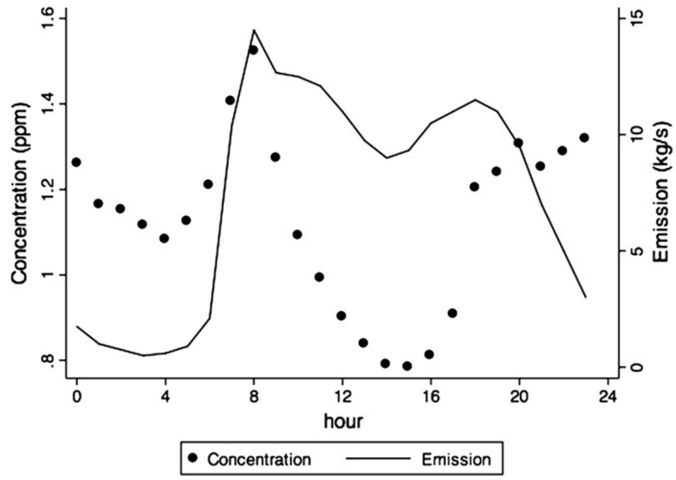


Fig. 4. CO emissions and concentrations in Santiago (January 2002).

was measured in multiples of 0.1145 ppm (with a minimum of 0.1145 ppm), after TS it was a continuous variable with a minimum of 0.0001. We will see below that this measurement change hardly affect our estimations. Summary statistics of variables used in the TS estimations are in Table B.2 of Appendix B.

4.3. Econometric specifications

Our dependent variable is CO records during peak hours of weekdays (Saturdays, Sundays, and holidays are excluded). Peak hours are defined as the two consecutive hours that on average mark the highest readings in the four years of either sample. This occurs from 8:00 to 10:00 am for HNC and from 7:00 to 9:00 am for TS. We employ two estimation approaches: (1) a flexible fit that includes a treatment dummy for the whole ex-post policy period and a series of monthly dummies that capture the adjustment phase following implementation and (2) a more structural fit that includes a (linear) trend for the adjustment phase—with its length endogenously determined as part of the estimation process—and a dummy for the period that follows the adjustment phase. Approach (1) is more flexible but it is likely to introduce noise in the estimation as it may capture idiosyncratic shocks (which are very relevant in our pollution dataset as illustrated in the online Appendix). That is why we prefer approach (2) that imposes a smooth and monotonic adjustment process. The estimating equations under the two approaches are given by

$$y_t = \alpha_0 + \alpha_1 y_t^b + \alpha_2 t + \alpha_3 x_t + \beta T_t + \sum \delta_t M_t + \varepsilon_t \quad (1)$$

$$y_t = \alpha_0 + \alpha_1 y_t^b + \alpha_2 t + \alpha_3 x_t + [a + b(t - t_T)]A_t + cT_t(1 - A_t) + \varepsilon_t \quad (2)$$

where y_t is CO concentration in period/h t in logs.

There are explanatory variables common to both approaches: y_t^b is the background pollution before the peak forms, which corresponds to pollution at night (we consider the average of 4 consecutive CO records that, on average, showed the least dispersion over the course of the sample: 2–6 am for HNC and 1–5 am for TS); t is a linear trend that captures any pre-existing trend in pollution (we also experimented with higher-order trends—quadratic and cubics—with similar results)[18]; and x_t is a vector that includes fixed effects (hour of the day, day of the week and month of the year), hourly measures of weather variables (that are entered in fourth order polynomials) including temperature, real humidity,

[17] While it is true that vehicles are also largely responsible for NO_x emissions—CAM (2001) and CONAMA (2004) document, respectively, that mobile sources are responsible for 81 and 87% of NO_x emissions—the mapping between car use and NO_x records *at peak hours* is not as good. It was not unusual to find days, in either city, with NO_x records peaking 3 or 4 h after the traffic peak while others with records peaking much closer to it. This is hardly surprising since NO_x is a highly reactive pollutant (Jorquera, 2002). We clearly do not know how to control for these reactions and the biases that a lack of control can produce in the estimations. Furthermore, we are not longer sure if NO_x records at traffic-peak hours (or later) are really picking up local traffic.

[18] In the online Appendix we present plots of residuals for the period before the policy supporting the linear trend. Furthermore, the problem of using higher order trends is that of over-fitting in that we may fit the complete evolution of the dependent variable with a sufficiently high-order polynominal. A discussion of this problem in a RDD context can be found in Dell (2010).

precipitation, atmospheric pressure, wind speed and wind direction, and monthly economic covariates that may affect the decision to own and use a car such as real exchange rates and gasoline prices.[19] We also include SO_2 records as explanatory variable, which are readily available from the same monitoring stations. Since SO_2 is mainly related to industrial activity and energy generation,[20] it serves as a control for weather phenomenon common to all pollutants but not entirely captured by our weather variables such as thermal inversions (Molina and Molina, 2002).

The effect of the policy is captured with different variables depending on the approach: T_t is a dummy that takes the value of 1 after the policy; M_t is a month indicator that covers several months after implementation; t_T is the time at which the policy gets implemented; and A_t is an indicator function that takes the value of 1 during the adjustment phase. The effect of the policy under approach (Angrist and Pischke, 2009) would be $\beta + \delta_1$ on impact (i.e., first month) and β in the long-run while under approach (Beaton et al., 1992) it would be a on impact (i.e., first day) and c in the long-run, where the (constant) speed of transit from a to c is b. Finally, ε_t is the error term.[21]

Before moving on to the empirical results it is worth mentioning that the length of the adjustment phase in Eq. (2) is endogenously determined. Let t_A mark the end of the adjustment, so $t_A - t_T$ is the length of the adjustment phase. We first estimate a, b, and c for different lengths of the adjustment period that go from 0.5 to 18 months in increments of 0.5 months. We then apply the structural-break *sup F* method of Andrews (1993) and Hansen (2000): Among those adjustment lengths for which we cannot reject the null hypothesis that $a + b(t_A - t_T) = c$, we choose the one with the highest value of the F test (notice that $\widehat{(t_A - t_T)} = (\widehat{c} - \widehat{a})/\widehat{b}$).

5. Empirical results for HNC

We report our empirical results in four steps. First, we present results at the level of the city, that is, we run Eqs. (1) and (2) on average concentration levels at morning peak hours, which correspond to the unweighted average of the hourly records from the different stations in the network during those hours. This may not only facilitate comparison with existing studies but it also works as benchmark for the single-station results that come later (note that city-level results may be the only meaningful ones when individual stations fail to capture local traffic activity). We then carry out a few exercises to check the robustness of the city-level results. Third, we run Eq. (2) separately on observations coming from a set of monitoring stations for which we have a complete series of data. And finally, we ask whether these results make any theoretical sense according to the framework of Section 3. Notice that for going over these steps it is easier to focus on one policy first and only then illustrate how it extends to a second one. We start with HNC to maintain chronological order.

5.1. City-level results

For the city-level analysis of HNC we use average records from 15 monitoring stations that were in operation in 1987, the first year of the sample. Column (1) in Table 2 presents the results of running (Angrist and Pischke, 2009) on peak hours (8–10 am). The coefficient of HNC, which is significant at the 14% level, indicates that over the long run HNC has increased CO by about 13%. According to the evolution of the monthly dummies this long-run effect is reached by month 10, which is when the monthly dummies are not longer significantly different from zero. Furthermore, since the dummies for the first months after implementation are statistically different from 0, we can immediately reject that the effect of HNC during those first months is the same as that over the following months. In fact, the effect of HNC within the first month of implementation, which is obtained from adding the coefficients of HNC and Month 1, is a reduction in CO of about 7%. As indicated by the "HNC + Month1 = 0" test at the bottom of the table, this short run effect is only significant at the 15% level. Columns (2) and (3) present results when excluding SO_2 and background pollution, y^b, from the regressions. While the exclusion of SO_2 produces few changes, the exclusion of y^b leads to a much larger effect in the long-run, of about 29%. This confirms that HNC has also impacted CO emissions beyond peak hours, resulting in higher levels of background pollution. But since our interest is to identify changes in car use at peak hours, background pollution must be controlled for.

While the pattern followed by the monthly dummies in either column (1) or (2) shows convergence towards zero, it is clear that it is not perfectly monotonic (the monthly dummies in column (3) are even more volatile and at times with the wrong sign, which adds to

Table 2
Effect of HNC on CO concentration (flexible approach).

	Actual policy			Placebo
	(1)	(2)	(3)	(4)
HNC	0.129	0.122	0.289***	0.002
	(0.087)	(0.091)	(0.093)	(0.157)
Month 1	−0.199***	−0.173***	−0.350***	−0.036
	(0.067)	(0.063)	(0.067)	(0.080)
Month 2	−0.214***	−0.180***	−0.278***	−0.006
	(0.052)	(0.052)	(0.050)	(0.021)
Month 3	−0.151***	−0.114**	−0.289***	−0.022
	(0.046)	(0.048)	(0.050)	(0.038)
Month 4	−0.092*	−0.064	−0.159**	0.122
	(0.047)	(0.051)	(0.060)	(0.085)
Month 5	−0.197***	−0.184***	−0.244***	−0.139***
	(0.049)	(0.051)	(0.065)	(0.040)
Month 6	−0.151***	−0.077*	−0.071	0.080**
	(0.039)	(0.040)	(0.052)	(0.035)
Month 7	−0.245***	−0.171***	−0.120**	−0.069**
	(0.037)	(0.035)	(0.054)	(0.026)
Month 8	−0.166***	−0.104***	−0.027	−0.089*
	(0.039)	(0.036)	(0.040)	(0.051)
Month 9	−0.114***	−0.077**	0.023	0.089*
	(0.040)	(0.037)	(0.038)	(0.052)
Month 10	−0.064	−0.039	0.062	−0.020
	(0.044)	(0.042)	(0.044)	(0.030)
Month 11	0.021	0.010	0.118**	0.061
	(0.050)	(0.048)	(0.056)	(0.038)
Month 12	0.061	−0.046	0.036	0.002
	(0.075)	(0.053)	(0.082)	(0.047)
y^b	0.339***	0.374***		0.276***
	(0.052)	(0.050)		(0.040)
SO2	0.258***			0.310***
	(0.046)			(0.064)
p-value HNC + month 1 = 0	0.151	0.278	0.214	0.686
R^2	0.704	0.678	0.619	0.703
Observations	1872	1872	1874	1556

Notes: The dependent variable is CO concentration during morning peaks (8–10 am, weekdays) in logs. Column (4) contains the results of the placebo policy. HNC is a variable equal to 1 after the implementation of the program on November 20, 1989 (or the placebo policy two years before). Month 1–Month 12 are month indicators for after program implementation. SO_2 is sulfur dioxide and y is the average concentration of CO (in log) from 2 to 6 am of the same day. All regressions control for weather covariates (fourth order polynomials of hourly measures of temperature, real humidity, wind speed and wind direction) and month of the year, day of the week, and hour of the day fixed effects. Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within 5-week groups. Levels of significance are reported as ***p < 0.01, **p < 0.05 and *p < 0.1.

[19] We also experimented with other variables related to unemployment and industrial activity but they were typically not significant or with the unexpected sign. Our sense is these additional economic variables become redundant once we include linear trends and the other monthly variables.

[20] In the case of Mexico City, 79% of the SO_2 emissions came from industrial activity and energy generation and 16% from transportation (mainly trucks), with 2% from light vehicles and taxis (CAM, 2001). In the case of Santiago, 74% of the SO_2 emissions came from industrial activity and power generation and 19% from transportation, with 2% from light vehicles (CONAMA, 2004). We entered the SO_2 records in the regressions in different forms (i.e., daily, weekly and monthly averages) with similar results.

[21] An additional estimation issue relates to the standard errors of the estimates. We follow Davis (2008) and Chen and Whalley (2012) in allowing errors to follow an arbitrary correlation within 5-week clusters.

Table 3
Policy effects on CO concentration (more structural approach).

	HNC			TS	
	Weekdays	Saturdays	Sundays	Adj. data	Raw data
	(1)	(2)	(3)	(4)	(5)
Immediate impact (a)	−0.130**	−0.048	0.022	−0.059	−0.052
	(0.051)	(0.071)	(0.037)	(0.098)	(0.091)
Adaptation trend (b)	3.29e−05***	2.68e−05***	2.75e−05***	8.38e−05***	9.36e−05***
	(0.935e−05)	(9.85e−06)	(8.59e−06)	(2.50e−05)	(2.59e−05)
Impact after adaptation (c)	0.113	0.160*	0.189***	0.268***	0.283***
	(0.081)	(0.082)	(0.043)	(0.071)	(0.072)
Impact difference (c–a)	0.243***	0.207***	0.167***	0.327***	0.335***
	(0.0578)	(0.066)	(0.0938)	(0.0978)	(0.0986)
Trend (θ)	−9.13e−06*	−5.72e−06	−7.60e−07	1.12e−05***	1.02e−5***
	(4.65e−06)	(5.24e−06)	(3.39e−06)	(2.80e−06)	(3.07e−06)
Real exchange rate	−0.646**	−0.180	−0.065	−0.493	−0.428
	(0.279)	(0.307)	(0.318)	(0.309)	(0.334)
Real gasoline price				−0.593***	−0.645**
				(0.264)	(0.242)
y^b	0.313***	0.586***	0.532***	0.431***	0.408***
	(0.050)	(0.040)	(0.031)	(0.026)	(0.023)
SO_2	0.236***	0.090*	0.092***	0.541***	0.537***
	(0.046)	(0.050)	(0.036)	(0.085)	(0.087)
Months of adaptation	12.5***	11.5***	9.5***	7.0***	6.0***
	(1.74)	(2.28)	(1.68)	(0.86)	(0.73)
R^2	0.698	0.863	0.882	0.749	0.749
Observations	1872	376	568	2004	2004

Notes: Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within 5-week groups. Levels of significance are reported as ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

the importance of including background pollution to act as control for unobserved atmospheric phenomena). But this lack of monotonicity is hardly surprising for high-frequency observations subject to the noise our pollution records show (see fn. 3). In the online Appendix we perform a series of exercises with simulated data from a data-generating process with the noise and auto-correlation we observe in both the HNC and TS time series. Results of regressing (Angrist and Pischke, 2009) on this simulated data also failed to show a strictly monotonic pattern for the monthly dummies; it was not unusual to find dummies statistically significant and with the wrong sign.

Our second and preferred approach solves this monotonicity problem by imposing up-front a policy impact that follows a smooth and monotonic path. Results of regressing Eq. (2) on the same observations are in column (1) of Table 3. The effect of HNC on impact is now bigger and statistically different from 0: a reduction of 13% the day after implementation. It is not surprising that effect is less than a 20% reduction because there are always substitution possibilities, even in the very short run, especially from families owning more than one car. In turn, the estimated coefficients imply an adjustment period of 12.5 months (with a 95% confidence interval that goes from 9 to 15 months). The long-lasting effect, i.e., the effect after the adjustment phase is over, is an increase of 11% that is significant at the 12% level. The 24% net difference between long and short-run effects, however, is statistically significant at the 1% level. More importantly, and as we elaborate further at the end of the section, this net increase is a clear indication that agents responded overtime by buying more cars. The remaining coefficients in column (1) have all the expected signs, namely, the inertia of background pollution, the positive correlation between CO and SO_2, and the negative impact of the real exchange rate. We also find a negative but small trend affecting CO concentrations.[22]

5.2. Robustness checks

There are several exercises one can perform to check the robustness of the above results. One that is specific to HNC is to look at potential policy impacts on days in which the policy is not active, say, Saturdays and Sundays. In principle, however, part of the weekday driving that is affected by the restriction could be moved to periods in which the restriction is not active including Saturdays and Sundays. Although we cannot rule out the latter, we believe that most of this intertemporal substitution takes place within weekdays not only because they are more alike but also because the weekly schedule with the plate-number restrictions is known in advance and fixed for a period of two months. Thus, looking at impacts on Saturdays and Sundays should provide both a falsification exercise for short run effects, when we expect to see little change, and a good check for long-run effects, since additional cars on the street should necessarily result in more car trips during hours and days in which the driving restriction is not active. Results of running (Beaton et al., 1992) on CO readings for Saturday and Sunday morning hours (8–11 am)—period that also exhibits a rapid build-up of CO concentration—are, respectively, in columns (2) and (3) of Table 3. Together with the negligible short-run effects, these results are entirely consistent with those of weekdays in terms of both the magnitude of the net increase over the long run and the length of the adjustment phase.

Our second check focuses on the short-run results, for which we rely on Imbens and Kalyanaraman's (2012) optimal-bandwidth RDD. This RD method allows for arbitrary pre- and post-trends in CO and identifies in a semi-parametric way (using local polynomial regressions and triangular Kernel weights) the effect at the moment of implementation without relying on explicit parametric assumptions. To handle the volatility of the data discussed above, columns (1) and (2) of Table 4 report the Imbens and Kalyanaraman RD estimates but for monthly average observations of CO at peak hours of weekdays. Regardless of whether we control for background pollution or not, we find that the RD estimation of −10% comes close to the −13% in Table 3. Fig. 5 plots the monthly CO observations (in logs) along with the RD fit using the estimates in column (1). The discontinuity in the figure captures the HNC impact on the month right after implementation. While estimates in column (2) would produce virtually the same picture, it is worth indicating that the Imbens and Kalyanaraman (2012) estimator does not provide a simple way of plotting the RD fit when the RD estimation includes control variables such as background pollution.

These short-run results differ from the RD results in Davis (2008) that report no reductions in any of the pollutants, including CO, the day after implementation. We can advance two explanations. One is

[22] For brevity, we do not report here the estimates of all weather variables and the day and month fixed effects but we can say that imposing structure to the estimates is supported by the data, as the standard errors of the different coefficients tend to decrease in comparison to specification (Angrist and Pischke, 2009).

Table 4
RDD estimations using Imbens and Kalyanaraman (2012).

	HNC		TS	
	(1)	(2)	(3)	(4)
Coefficient	−0.0976**	−0.1047*	−0.2654	−0.0232
	(0.0453)	(0.0513)	(0.1820)	(0.2485)
Control for background pollution	No	Yes	No	Yes

Notes: Standard errors in parenthesis. Levels of significance are reported as ***p < 0.01, **p < 0.05 and *p < 0.1.

that Davis' (2008) methodology estimates the impact on one particular day, the day right after implementation. We find this problematic given the daily volatility we see in the pollution series even after controlling for weather variables. Our estimation strategy handles this daily volatility by estimating the policy impact not on a particular day or week but over a period of several months. But perhaps more importantly, Davis (2008) imposes a uniform policy impact across the day. Given the imperfect mapping from emissions to concentrations (see Fig. 4), our focus is instead on impacts at peak hours, and more so, if we want to capture local effects.

In the absence of a city that could act as control group, our third robustness check consists of a simple falsification exercise: we subject our dataset to a "placebo HNC" implemented two years before the actual implementation (i.e., November of 1987). The last column in Table 2 present the results of running (Angrist and Pischke, 2009) on CO records for peak hours over the same vector of control variables and also for a four-year window around the implementation of the placebo. The placebo policy has no effects and even though some monthly dummies are statistically significant, they do not follow a monotonic pattern as they do in the actual policy. These results imply that our main estimates cannot be explained by (i) an underlying trend in the dataset, possibly connected to income growth, and/or (ii) seasonal patterns not captured by our fixed effects. Yet, an additional robustness check, interesting in its own, is to look at the pattern of impacts across income groups. We turn to that next.[23]

5.3. Results from individual monitoring stations

It is natural to expect transport policies to affect households with different private/public transportation demands in different ways. Here we exploit income variation within Mexico City and CO records from individual monitoring stations distantly located (see map with monitoring stations in the online Appendix) to study how the response to HNC varies with income (or ex-ante car use[24]). Table 5 provides a summary with the results of running (Beaton et al., 1992) on CO records for 10 individual monitoring stations for which we have a complete series of data during the four-year window around policy implementation. We have ordered the stations according to both location (i.e., sector) and the (relative) income level reported in INEGI (1989b) for the representative household living in the municipality (*delegación*) where the station is located (average income for the entire population has been normalized to 1). We believe that accounting for both income and location gives a better idea of the household wealth. Households living in Plateros and Pedregal, in the Southwest area, exhibit the largest income levels, four times higher than those in the Northeast.

The next four columns in the table present estimates of short and long run effects, the difference between the two, and the length of the

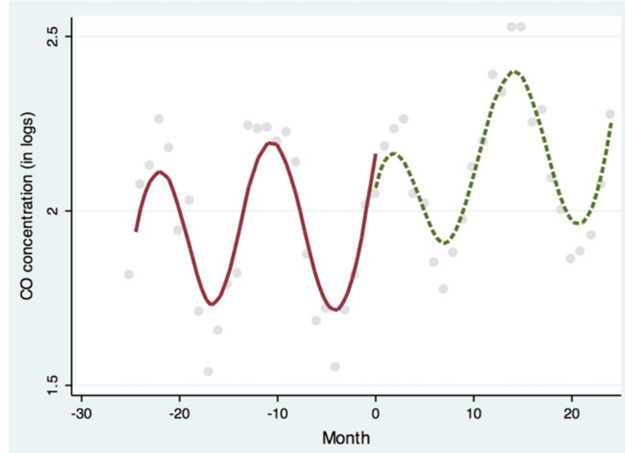


Fig. 5. RD results for HNC without controlling for background pollution.

adjustment phase. The first reaction to these results is that city-level figures can mask a wide heterogeneity in responses. HNC has its largest impact in middle-income groups—with net increases in CO above 30%—and not nearly as large in lower-income (e.g., Xalostoc) and high-income groups (e.g., Plateros and Pedregal). We can safely say that the short-run reduction of 13% we find at the city-level is mostly driven by reductions in monitoring stations located in middle-income neighborhoods. Unfortunately there is not much else we can say about the speed of adjustment and how it varies with income because most of the adjustment action occurs in just one income group (TS is different in that regard).

5.4. Discussion of results

How does the theory presented in Section 3 make sense of these empirical results? Can it explain the responses we observe? In particular, can the long-run increase in CO be simply explained by more cars on the street or also by dirtier ones and/or more congestion? Can differences in income lead to such heterogeneous effects? What are the transport costs inflicted by these policies? Gallego et al. (forthcoming) run a calibration and different simulation exercises to answer these questions for both HNC and TS.

A first simulation exercise for HNC was to replicate the city-level empirical estimates by decreasing car capacity. As depicted in the benchmark simulation of Panel A of Table 6, hitting the 13% short-run reduction (ΔCO_{SR}) is less of a problem than hitting the 11% long-run increase (ΔCO_{LR}). The latter requires to drop two assumptions in the benchmark simulation that are unlikely to hold in practice (note that in the benchmark simulations for both HNC and TS there is a one-to-one mapping between the number of car trips and the amount of CO). First, that households have the option to return their cars—since the policy made them more expensive per unit of capacity—at the ex-ante original price (according to the change in the stock of vehicles, Δ stock, shown in the last columns of rows 1 and 2, households would like to return $13.3\% = 9.2\% + 4.1\%$ of the current stock). If instead we assume that transaction/lemon costs, as in Eberly (1994), are such that no household returns its car(s), the numbers in exercise A2 show that in the long-run the policy leads to a net increase in the stock of vehicles of 4.1%, although still accompanied by a minor decline in car use or CO (−2.7%). The fact that a large fraction of households do not return their less valuable cars makes the adjustment process that follow the policy shock in part irreversible.[25]

[23] A referee suggested an additional falsification test based on SO_2 records since SO_2 emissions are little related to transportation (see fn. 20). We find HNC to have no effect on SO_2 when running Eq. (2) on city-average SO_2 records for all hours of the day (note that SO_2 emissions do not peak like CO emissions). The impacts on weekdays are (standard errors in parenthesis): $a = -0.0453$ (0.0671) and $c = 0.0306$ (0.1103); and on Sundays: $a = -0.0911$ (0.0767) and $c = -0.0544$ (0.1342).

[24] The simple correlation between (the log of) household income and (the log of) the number of cars per household at the county level is 0.85 (significant at the 5% level) for Mexico City in 1989 and 0.94 (significant at the 5% level) for Santiago in 2006.

[25] As explained by Eberly (1994), the irreversibility comes from the gap that transaction costs introduce between selling and buying prices. If so, households will not return to their ex-ante level of vehicle stock if the policy shock is fully removed ex-post, that is, after the stock has been adjusted. Note that the model of Eskeland and Feyzioglu (1997) also obtains that a driving restriction should prompt some households to "return" their cars.

Table 5
Policy effects by monitoring station: HNC.

Station	Sector	Income per HH (relative to average income)	Short-run effect	Long-run effect	Difference LR–SR	Months of adaptation	R^2	Observations
Xalostoc	NE	0.55	0.1196	0.1760	0.0564	12.5**	0.635	1222
			(0.1044)	(0.1118)	(0.0903)	(6.06)		
Tlalnepantla	NW	0.50[a]	−0.2132*	0.0760	0.2208*	9***	0.646	
			(0.1172)	(0.1730)	(0.1240)	(3.1)		
I.M. del Petróleo	NW	0.53	−0.1781***	0.1598	0.3379***	14***	0.666	1209
			(0.0627)	(0.1244)	(0.0910)	(1.91)		
M. Insurgentes	CE	0.70	−0.2458***	0.1427	0.3885***	15***	0.599	1473
			(0.0727)	(0.1026)	(0.1023)	(2.33)		
Lagunilla	CE	0.71	−0.2821***	−0.0652	0.2169*	11***	0.620	1403
			(0.0906)	(0.1030)	(0.1145)	(1.78)		
Merced	CE	0.84	−0.1527*	0.0807	0.2334**	12***	0.543	1588
			(0.0802)	(0.1310)	(0.1057)	(1.52)		
Cerro Estrella	SE	0.54	−0.1781**	0.2037*	0.3818***	11.5***	0.333	1499
			(0.0857)	(0.1196)	(0.1001)	(1.51)		
Taqueña	SE	1.14	−0.0948	0.2255**	0.3203***	15***	0.326	1381
			(0.0618)	(0.1277)	(0.1011)	(2.41)		
Plateros	SW	1.99	−0.0331	−0.0331	0.0000	0	0.579	1355
			(0.0973)	(0.0973)				
Pedregal	SW	1.99	−0.0338	0.1378	0.1716**	10.5***	0.590	1708
			(0.0789)	(0.0789)	(0.0852)	(3.06)		

Notes: Levels of significance are reported as ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.
[a] Authors' own estimate. Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within 5-week groups.

The second assumption in the benchmark simulation in A1 is that the additional stock is equally polluting (and fuel-efficient) as the existing fleet, which we know from Eskeland and Feyzioglu (1997) is unlikely for HNC because of the import of older cars from adjacent regions. Thus, if we also let the additional cars be 2.4 times as polluting (and less fuel-efficient) as the existing ones,[26] the results in A3 match our long-run estimates, which illustrates that they are consistent with the theory once we incorporate these more realistic assumptions. Even though the short-run gains are for most part undone, these exercises show for the case of HNC that this is much less due to increases in car use and congestion (actually they hardly changed with respect to pre-HNC levels) than to the entry of older and more polluting cars.

Gallego et al. (forthcoming) also report simulations for different income groups by calibrating the (relative) cost of purchasing a car to the level that is consistent with the ex-ante statistics on car ownership and use that one can obtain for different income groups from information available in INEGI (1989a) and Molina and Molina (2002), p. 227. For example, the exercise A4 of Table 6 extends A3 to a higher-income group that exhibits an ex-ante car use of 70% during peak hours (ratio of car trips over the total number of trips). Consistent with the single-station results, the effect of the policy is unsurprisingly small compared to the city average in A3 because, as the theory indicates, these households have already sufficient car capacity to cope with the driving restriction (the long-run figure goes down to 0.6% if we let the additional cars in this high-income neighborhood be equally dirty than the fleet average).

Exercise A5, on the other hand, looks at the other extreme by extending A3 to a lower-income neighborhood that exhibits an ex-ante car use of only 4%. The effects of the policy are again intuitive since these are households that at most have one car, so the driving restriction hits them hard in the short-run and only a few of them can afford a second car in the long-run. The reason the 20.4% short-run drop differ so widely from the empirical estimates in Table 5 for low-income Xalostoc is because in the simulation we consider cars to be the only source of CO. While this is a good approximation in areas with high car use, in poor areas such as Xalostoc cars contribution to CO is no more than 20% relative to the contribution of other sources not affected by HNC (i.e., taxis, buses).[27] While is reassuring that the empirical results exhibit a pattern of effects that is consistent with the numerical exercises, the fact that we find small effects in both the very high- and low-income stations also dissipates the concern for the presence of time-varying omitted variables that could be introducing a bias in the long-run estimates of the remaining stations.

The final exercise in Gallego et al. (forthcoming) is to use the model to compute the transport costs inflicted by these policies. In the short (long) run, HNC made households in Mexico City bear losses equivalent, on aggregate, to 5.5% (5.3%) of the value of the existing stock of vehicles at the time of implementation.

6. Empirical results for TS

The application of our empirical strategy to TS is interesting in its own but it also serves as "robustness check" of the methodology because while TS shares many similarities with HNC—a big, sudden and permanent shock over an entire city—it has some important differences beyond the nature, location and timing (i.e., initiated in summer as opposed to autumn and 18 years later) of the policy. The TS pollution series exhibits a more pronounced seasonality with many more hours of very low pollution (see Figs. 1 and 3 and summary statistics in Tables B.1 and B.2 in the Appendix) and it is far noisier. More importantly, in the case of TS we had a good idea a priori about the direction of the CO impact in the long run (not about its magnitude, the length of the adjustment phase, and the heterogeneity in responses). Hence, it would be a major setback for our methodology if we fail to find that TS led to more CO in the long run.

6.1. City-level results

For the city-level analysis of TS we use average records from 7 monitoring stations that were in operation in 2005, the first year of the sample. Table 7 reports results from regressing (Angrist and Pischke, 2009) on two slightly different data sets. Those in column (1) are from a data

[26] Based on Beaton et al. (1992), who find that each additional year increases CO emissions by approximately 16%, a factor of 2.4 would suggest that the additional vehicles are on average 6 years older than the fleet average, which is perfectly reasonable since 8% of the gasoline fleet in 1989 is at least 20 years old (Molina and Molina, 2002).

[27] From Onursal and Gautam (1997) and GDF (2004) one obtains that 70% of the CO emitted in the city was subject to HNC. Given that CO records do not vary much across monitoring stations (particularly at peak hours) and that car use in poor areas is about one-fourth of city-average, cars contribution to CO in these areas should be about 18%.

Table 6
Simulations.

Exercise	ΔCO_{SR}	ΔCO_{LR}	Δ stock
Panel A: HNC			
A1. Benchmark	−12.5%	−8.3%	−9.2%
A2. Lemon costs	−12.5%	−2.7%	4.1%
A3. More-polluting vehicles	−12.5%	11.0%	4.1%
A4. High-income	−1.7%	3.9%	3.0%
A5. Low-income	−20.4%	2.3%	2.6%
Panel B: TS			
B1. Benchmark	0.0%	27.8%	18.4%
B2. Unequal quality impact	0.2%	11.2%	6.0%
B3. Additional congestion	0.2%	27.5%	6.0%
B4. High-income	1.7%	17.2%	1.5%
B5. Low-income	0.3%	40.7%	10.6%

set in which CO records below 0.1145 have been corrected by imputing a value of 0.1145. Results in column (2) are based on the original records without any correction. In either case, results indicate that TS had little impact in the first month but a positive and large effect of more than 30% in the long run (the correction for low values does not have much of an impact as low values of CO concentration are less relevant in peak estimations except for constructing the background pollution level). As in HNC, the last two columns of the table show that excluding background pollution and SO_2 has some quantitative impact on the estimated coefficients but not enough to change signs.

The monthly dummies present a weak pattern of increasing effects and clearly more volatile than in HNC. This is not surprising, as we explain in the online Appendix, because the TS data is way noisier than the HNC data. This is the main reason to impose the structure of Eq. (2). The result of doing that for the same two data sets—the adjusted data set and the raw data set—is in columns (4) and (5) of Table 3. Regardless of the sample, the estimated coefficients imply a slightly negative short-run effect, although not statistically significant, and a large and significant effect over the long run of about 27%. Interestingly, the adaptation period, 6 to 7 months, is faster than in HNC. Given that households in Santiago in 2007 were on average richer than those in Mexico City in 1989, they should have reacted faster, among other things, for better access to financing opportunities (Chah et al., 1995; Gallego et al., 2001 present evidence of the impact of financial development on liquidity constraints that affect the demand for durable goods).[28] Still, the differences in the speed of adjustment are not that big when we consider the standard errors of our point estimates; so these results tend to be suggestive more than a final proof of this point. Finally, all the other determinants of CO that are in the table present the expected signs, including the negative impact of gasoline prices and real exchange rates.

6.2. Robustness checks

Our first robustness check, which is specific to TS, uses pollution data from a monitoring station in a city that did not receive the "TS treatment". Panel A of Table 8 presents the correlation matrix of CO records from the monitoring stations located in Santiago and in several cities in the rest of the country *before* TS was implemented. The correlation between any pair of monitoring stations located in Santiago is high and only comparable to the correlation with the station in the city of Quillota (130 km northwest of Santiago), which makes it a good control group as opposed to stations in larger cities like Viña del Mar, Temuco or Rancagua that not only show significantly smaller correlations but at times with a negative sign. That the station in Quillota looks much like another station in Santiago in terms of CO is not surprising because both cities, unlike the other larger cities in the table, share similar weather and geographic characteristics that result in similar conditions for the dispersion of pollutants.

Table 7
Effect of TS on CO concentration (flexible approach).

	(1)	(2)	(3)	(4)
TS	0.321***	0.312***	0.278**	0.357***
	(0.075)	(0.078)	(0.103)	(0.121)
Month 1	−0.322***	−0.284**	−0.087	−0.237**
	(0.100)	(0.105)	(0.100)	(0.114)
Month 2	−0.311***	−0.309***	−0.275***	−0.450***
	(0.073)	(0.078)	(0.087)	(0.096)
Month 3	0.020	0.021	0.051	0.141*
	(0.052)	(0.055)	(0.066)	(0.082)
Month 4	−0.220***	−0.202***	−0.143*	0.081
	(0.053)	(0.055)	(0.081)	(0.093)
Month 5	0.012	0.029	0.035	0.101
	(0.064)	(0.064)	(0.079)	(0.116)
Month 6	−0.137	−0.148	−0.174*	−0.040
	(0.087)	(0.095)	(0.100)	(0.108)
Month 7	−0.032	−0.043	−0.104	−0.097
	(0.094)	(0.127)	(0.114)	(0.197)
Month 8	−0.466***	−0.459***	−0.647***	−0.782***
	(0.067)	(0.062)	(0.077)	(0.121)
Month 9	0.087	0.149*	0.010	−0.239*
	(0.095)	(0.082)	(0.095)	(0.141)
Month 10	−0.022	0.119*	−0.036	−0.264***
	(0.060)	(0.063)	(0.072)	(0.083)
y^b	0.414***	0.395***	0.494***	
	(0.026)	(0.024)	(0.027)	
SO_2	0.503***	0.497***		
	(0.085)	(0.086)		
R^2	0.758	0.758	0.712	0.682
Observations	2004	2004	2006	2006

Notes: The dependent variable is CO concentration during morning peaks (7–9 am, weekdays) in logs. SO_2 is sulfur dioxide. TS is a variable equal to 1 after the implementation of the program on February 10, 2007. Month 1–Month 10 are month indicators for after program implementation and y^b is the average concentration of CO (in log) from 1 to 5 am of the same day. All regressions control for weather covariates (fourth order polynomials of hourly measures of temperature, real humidity, precipitation, atmospheric pressure, wind speed, and wind direction) and month of the year, day of the week, and hour of the day fixed effects. Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within 5-week groups. Levels of significance are reported as ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

This additional data is used in two ways. Columns (1) and (2) of Panel B in Table 8 report the results of a falsification exercise in which we subject the CO records of the Quillota station to a "placebo TS" implemented together with TS in Santiago. There is no indication of a change in CO levels whether the "TS effect" is captured with a single dummy (column 1) or with Eq. (2) (column 2). On the other hand, columns (3) and (4) report results of a difference-in-difference regression using Quillota as control city (and the adjusted data set). The long-run impact in column (4), 26.4%, is almost identical to the impact of 26.8% in column (4) of Table 3.

Columns (3) and (4) in Table 4 presents the results of our second check, the RDD estimates using the Imbens and Kalyanaraman (2012) approach. Consistent with the estimates using Eq. (2), we find zero effects on impact; although controlling for background pollution proves relevant here for the point estimates.[29] Fig. 6a plots the monthly CO observations (in logs) along with the RD fit using the estimates in column (3). Recall from Table 4 that the discontinuity in the figure, while noticeable, it is not statistically significant. Since we cannot provide the exact figure associated to the results in column (4), we carried out an alternative RD exercise that does control for background pollution. We run a regression of CO (in logs) over background pollution (in logs) and applied the Imbens and Kalyanaraman (2012) estimator to the residuals of that regression. Results are presented in Fig. 6b. The TS coefficient of this alternative estimation comes very close to that in column (4): a point estimate of −0.0563 with a standard error of 0.5067.

[28] There is an important gap between Mexico-1989 and Chile-2007 in terms of both GDP per capita—\$9, 697 vs \$13, 047 (both measured in 2005 PPP \$) and financial development (e.g., domestic credit to the private sector, as % of GDP, of only 16% vs 88%).

[29] Like in HNC, we find TS to have no impact on transport-unrelated SO_2. The results of running Eq. (2) on city-average SO_2 records for all hours of the day are: $a = -0.0019$ (0.1040) and $c = -0.0845$ (0.0634).

Table 8
Falsification exercise for TS: Placebo policy implemented in a city not affected by TS.

Panel A: CO correlation matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Santiago														
1. La Paz	1													
2. La Florida	0.819^{***}	1												
3. Las Condes	0.420^{**}	0.584^{*}	1											
4. Pudahuel	0.836^{***}	0.842^{***}	0.394^{**}	1										
5. Cerrillos	0.693^{***}	0.771^{***}	0.323	0.888^{***}	1									
6. El Bosque	0.854^{***}	0.883^{***}	0.358^{*}	0.922^{***}	0.812^{***}	1								
7. Cerro Navia	0.735^{***}	0.701^{***}	0.170	0.942^{***}	0.860^{***}	0.846^{***}	1							
Regions														
8. Temuco	0.111	0.308	0.151	0.145	0.120	0.108	0.121	1						
9. Con Con	−0.128	0.011	−0.222	−0.161	−0.130	−0.065	−0.193	0.222	1					
10. Iquique 1	0.118	0.273	0.121	0.046	0.110	0.152	−0.013	0.028	0.220	1				
11. Iquique 2	0.246	0.110	0.082	0.123	0.256	0.106	0.147	−0.255	−0.509	0.013	1			
12. Quillota	0.533^{***}	0.724^{***}	0.405^{**}	0.619^{***}	0.584^{***}	0.656^{***}	0.521^{**}	0.313	0.310	0.336^{*}	−0.094	1		
13. Rancagua	0.205	0.146	0.020	0.267	0.129	0.189	0.329	0.360^{*}	−0.101	-0.459^{***}	−0.244	−0.133	1	
14. Viña del Mar	−0.262	−0.118	0.180	-0.414^{***}	-0.421^{***}	−0.291	-0.572^{***}	−0.087	0.251	0.009	−0.160	−0.136	−0.095	1

Panel B: Falsification exercise and differences-in-difference estimates using Quillota

	Falsification		Diff-in-diff	
	(1)	(2)	(3)	(4)
TS	0.048		0.256^{*}	
	(0.173)		(0.133)	
Immediate impact (a)		−0.013		0.092
		(0.140)		(0.164)
Adaptation trend (b)		−2.64e−05		3.30e−05
		(3.50e−05)		(3.51e−05)
Impact after adaptation (c)		0.078		0.264^{**}
		(0.148)		(0.119)
Trend (θ)	−3.94e−06		−7.11e−06	
	(6.83e−06)		(6.20e−06)	
y^b	0.885^{***}			
	(0.076)			
Δy^b			0.576^{***}	0.585^{***}
			(0.047)	(0.048)
SO_2	−0.033	−0.019		
	(0.021)	(0.022)		
ΔSO_2			0.022	0.031
			(0.025)	(0.023)
R^2	0.475	0.479	0.454	0.471
Observations	1678	1678	1674	1674

Notes: Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within 5-week groups. Levels of significance are reported as $^{***}p < 0.01$, $^{**}p < 0.05$ and $^{*}p < 0.1$.

6.3. Results from individual monitoring stations

Table 9 reports a summary of results of the effects of TS on 7 monitoring stations in Santiago (a map with the location of the stations is in the online Appendix). We have ordered the stations according to the location and the income level reported in CASEN (2006) for the representative household living in the municipality where the station is located (average income for the entire population has again been normalized to 1). Given that TS affected the supply of public transport throughout the city, we also include in the table the ratio of bus traffic flows to total flows at peak hours which was computed from a sample of traffic stations located close to the corresponding pollution monitoring station. We think of this ratio as a good proxy of the relative importance of buses over other forms of transportation ex ante (i.e., before TS). Data suggest, as expected, a strong negative correlation between this proxy and household income (the simple correlation is −0.90, which is significant at the 5% level), which immediately suggests that a household's dependence on public transport varies greatly across the city: from as low as 2% in rich Las Condes to 13% in poor Cerro Navia. Closely related, the next column in the table presents a proxy of the change introduced by TS in bus service (i.e., frequency) in the vicinity of each pollution station. It is noticeable that despite the ex-ante differences in bus coverage, frequencies in all neighborhoods fell more or less in the same proportion. Then, variations in the intensity of the TS treatment mostly come from ex-ante differences on how much households depend on public transport.

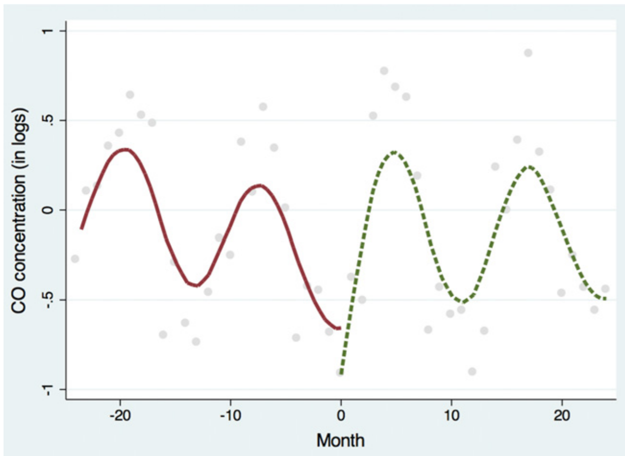
The last three columns of Table 9 present estimates of the TS effects in the short and long run, and the length of the adjustment process. The immediate impact of TS is not different from 0 in all the stations (with mostly negative but statistically insignificant effects in the short-run). As for the long-run estimates, there is a strong positive correlation between the size of the coefficient and the ex ante degree of dependence on public transport (and also a negative correlation with household income).[30] Effects are big and precisely estimated in all stations. It is also interesting to notice that the speed of adjustment appears indeed inversely related to income.

6.4. Discussion of results

The first exercise in Panel B of Table 6 considers a TS-like policy that inflicts a uniform deterioration in the quality of public transport sufficient to produce a long run impact (ΔCO_{LR}) comparable to our city-level CO estimates of 27%. The 18.4% increase in the stock of vehicles

[30] The correlation between the estimated long-run effect and the use of public transport is 0.91 (significant at the 1% level) and between the same effect and relative income is −0.80 (significant at the 5% level).

(a)



(b)

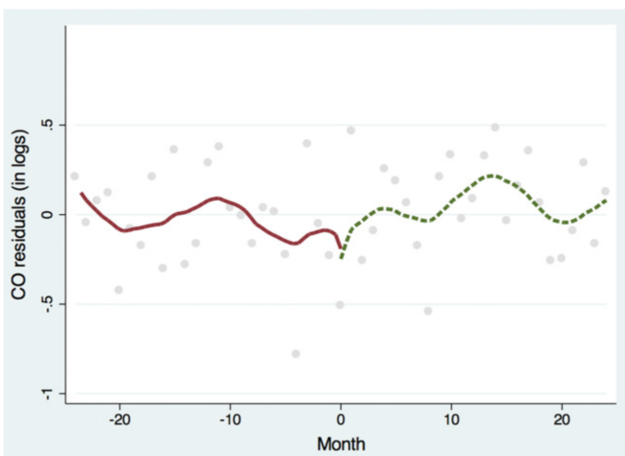


Fig. 6. (a) RD results for TS without controlling for background pollution. (b) RD results for TS controlling for background pollution.

in this benchmark simulation, however, is way above our empirical finding that we report in the online Appendix that is in the range of 5 to 8%. While TS has resulted in a deterioration of service quality at all times, survey reports of the time indicate that service at peak hours has suffered the most (note that the numbers in Table 1 are for peak hours). Accordingly, the next exercise in Table 6 considers an uneven change in the quality of public transport in which service at peak deteriorates three times as much. The stock variation now is 6%, which is more in line with our empirical findings, but car use (or CO) appears below the empirical estimate of 27%. There are two factors, however, that B2 does not account for: additional congestion and entry of used cars. Exercise B3 extends B2 to incorporate both factors. First, we let the additional vehicles be 24% more polluting than existing ones (consistent with the increase in the trade of used cars reported in the online Appendix; this captures that a third of the additional stock corresponds to used cars, some of which quite old),[31] and second (and consistent with the changes in traffic flows also reported in the online Appendix), we let the extra congestion reduce the average speed at peak hours by 8%, which, according to Robertson et al. (1998), should increase CO emissions by a factor of 1.12. With these corrections, the long-run change in CO concentrations at peak hours returns to 27.5%.

The last two exercises in the table present the predictions for the effects of TS on households with different income levels. Exercise B4 extends B3 to a high-income neighborhood that displays an ex-ante car use of 72% during peak hours (ex-ante statistics on car ownership and use for different income groups were obtained from CASEN (2006) and Santiago's 2006 Origin–destination survey). The short run effect is still very small—somehow positive during peak hours because of the excess capacity—but the long-run effect is considerably smaller than the city average, i.e., the one in B3, and close to our empirical estimate of 17% for Las Condes. This is simply because households in this neighborhood rarely use public transportation. Exercise B5, on the other hand, extends B3 to a lower-income neighborhood that has an ex-ante car use of only 8%. Again, the short-run effect is negligible but the long-run effect is substantial (40.7%), which again, is consistent with our empirical findings for low-income counties such as Cerro Navia and Pudahuel. And the transport costs associated to these responses are also substantial. Based on the numbers in Gallego et al. (forthcoming), the short (long) run costs inflicted by TS amount to 9.7% (9.5%) of the value of the existing stock of vehicles at the time of implementation. Like in HNC, transport costs have less to do with the cost of purchasing additional cars but with the cost associated to price changes in the short-run.

7. Final remarks

We have looked at two major transport policy reforms in Latin America—HNC in Mexico City and TS in Santiago—to understand how households respond, in terms of car use and ownership, to a sudden and permanent change in relative transport prices. The focus of the paper has been on understanding how that response evolved over time and whether it varied with income. The econometric results presented in the paper are based on series of hourly CO pollution records, but they are consistent with and complementary to additional evidence related to car use (gasoline sales, car registrations, car sales, traffic flows and prices of taxi licenses) and the numerical exercises in Gallego et al. (forthcoming). Although it is evident that the long-run increases in CO detected by our empirical analysis must come from additional cars on the street, the numerical exercises help clarify that in the case of HNC they happen to emit significantly more than the fleet average and in the case of TS they add to the existing congestion.

We learned from HNC that driving restriction policies can be effective in reducing congestion and pollution but only in the short-run. As these policies appear politically feasible—they have been applied in quite a few cities—and are relatively easy to enforce, there is more to be understood on how to design them in a way that the short-run gains are not wiped out by the long-run losses associated to the purchase of additional (higher-emitting) vehicles; perhaps some combination of a permanent ban on older vehicles and a sporadic one on newer vehicles to attack a limited number of episodes/days of bad pollution. Paradoxically, we were able to use HNC to empirically evaluate the short-run benefits of a driving restriction only because HNC became a permanent one. The high volatility of pollution data makes it hard to estimate the daily impact of sporadic policies, using, for example, a regression discontinuity design. Nevertheless, more methodological work needs to be done to figure out the best way to estimate the impact of such policies since they are quite common.

The TS experience, on the other hand, showed how rapidly commuters can abandon public transport in favor of cars after a (permanent) deterioration in the quality of service. But even more successful public transport reforms (e.g., Transmilenio in Bogotá) indicate that it is challenging to persuade drivers to give up their cars. We believe there is a lot more to learn on how commuters decide between public and private

[31] This correction increases the change in CO at peak from 11.2% to 13.8%. More precisely, we are assuming that a third of the additional stock corresponds to used cars that are 8 years older than the fleet average and two thirds to new cars that are 10 years newer than this average. According to ANAC (Chile's National Automobile Association), the stock in 2007 was on average 10.4 years old and a 22% of it was at least 20 years old.

Table 9
Policy effects by station: TS.

Station	Sector	Income per HH (relative to a verage income)	Ratio of buses to total flows at peak hours (before-TS)	Percentage change in bus availability (after-TS)	Short-run effect	Long-run effect	Difference LR–SR	Months of adaptation	R^2	Observations
El Bosque	S	0.53	10.8%	−34.60%	−0.1246	0.202**	0.3266**	5.5**	0.550	1935
					(0.1134)	(0.0898)	(0.1307)	(1.52)		
Cerro Navia	W	0.54	13.0%	−28.10%	−0.0699	0.3979***	0.4678***	7***	0.725	1726
					(0.1834)	(0.1336)	(0.1218)	(1.25)		
Pudahuel	W	0.65	11.2%	−26.70%	−0.0568	0.3465***	0.4033***	7***	0.720	1813
					(0.1657)	(0.0845)	(0.1326)	(1.71)		
Cerrillos	SW	0.81	10.5%	−29.30%	−0.0715	0.3580**	0.4295**	9***	0.627	1492
					(0.1678)	(0.1401)	(0.1843)	(2.33)		
Independencia	N	0.93	6.2%	−30.20%	−0.0288	0.2915***	0.3203**	7***	0.622	1693
					(0.1179)	(0.1051)	(0.1267)	(1.53)		
La Florida	SE	1.06	7.6%	−29.50%	0.0013	0.3228***	0.3215**	5***	0.591	1887
					(0.1152)	(0.0860)	(0.1341)	(0.73)		
Las Condes	NE	2.45	2.2%	−31.90%	−0.0431	0.1663**	0.2094*	4.5***	0.484	1900
					(0.0879)	(0.0729)	(0.1151)	(1.11)		

Notes: Authors' own estimate. Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within 5-week groups. Levels of significance are reported as ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

transportation and the role different instruments—including road pricing and pollution/gasoline taxes—play in that decision. This is particularly important in cities that exhibit a fast increasing motorization rate, for dealing not only with local problems such as urban air pollution and congestion, but also with the global problem of climate change.

Appendix A. Taxi medallions

Since HNC and TS are transport policies that affect the relative prices of all transportation options, one would hope to see changes in the price of taxi licenses (or taxi medallions, as known in New York City) in response to the policies. As in most cities around the world, the taxicab markets in Mexico City and Santiago are regulated in terms of both fares and the total number of licenses (i.e., number of taxicabs that can operate).[32] License prices must then reflect the scarcity rents of operating in markets where there is no free entry. While significantly lower than those in New York City (NYC), license prices in Mexico City and Santiago were nevertheless positive and comparable at the time HNC and TS were introduced, around US$1000. Moreover, despite taxi rides constitute a small share of all trips in these cities—2 and 1%, respectively—there are good reasons for license prices to be reliable indicators of the changes in relative prices. One reason is that since these prices represent the present value of a stream of economic rents over an infinite horizon, they should capture from the start the expected long-run effect of the policy. And second, the introduction of both HNC and TS came with no modification in fares nor in the number of licenses,[33] so any change in prices after policy implementation can be largely attributed to it.

An analysis of the taxicab market in Mexico City at the time of HNC can be found in Davis (2008). He finds no evidence of an increase in the price of a taxi license—the HNC coefficients were all negative but not statistically different from zero. Given the positive price of licenses, this lack of evidence can only be explained by a modest (long run) increase in the demand for taxi rides, or alternatively and according to the (search) model in Lagos (2003), by an increase in demand accompanied by an equivalent increase in the number of licenses, which in this case must come from unauthorized vehicles.

We carried out a similar analysis of the taxicab market in Santiago. We compiled a novel database of 430 observations of license prices based on each of the weekend's classified advertisements appeared in El Mercurio—Santiago's main newspaper but not the only place where ads are posted—for either taxi licenses or taxicabs for the period January 2004 through November 2010. Since 370 of the ads we collected consisted of taxicabs with a single posted price for the vehicle and the license, we proceeded to subtract from the posted price the average price of an equivalent passenger car (i.e., same model and year) advertised the same day. The remaining 60 observations correspond to ads of just taxi licenses. We are aware that the "constructed" observations are probably biased because, among other things, the vehicles we are comparing are not necessarily of the same market value (e.g., taxis are more heavily used). However, since we do not expect the bias to change with TS, this methodology should provide us with an unbiased estimator of the effect of TS on license prices.

The evolution of license prices (from the 60 only-license ads) is depicted in Fig. 2 in the text. As discussed earlier, prices are quite stable right up to the implementation of TS which is a good indication that nobody really anticipated the large impact TS later had. The figure also shows a big and relatively quick increase in prices soon after TS.[34] Table A.1 provides more precise estimates of the effect of TS on a license price. We start in column (1) with an OLS regression of (the log of) license prices on a dummy that takes the value of 1 for observations after TS. The coefficient of TS indicates a large and statistically significant impact of 71%. If we control for the total number of licenses (per capita), the coefficient of TS, as shown in column (2), drops to 56%. Interestingly, the value of −0.91 for the price elasticity of licenses is entirely consistent with the −1.57 value found by Lagos (2003) for NYC medallions, which are traded at much higher prices. As the other columns in the table show, these results are robust to the inclusion of linear trends and/or fixed-effects intended to correct for the potential biases generated during the construction of our sample as well as to the sub-sample of 60 license ads. The coefficients are never below 50% and always statistically significant at conventional levels.[35]

[32] There were 69,000 taxis in Mexico City (Molina and Molina, 2002), or 1 for every 120 residents, and 27,000 in Santiago (INE, 2010) , or 1 for every 220 residents.

[33] Except, obviously, for any rise in illegal activity. We have some anecdotal evidence, from talking to several taxi drivers, that at least in Santiago the fraction of unauthorized taxis does not reach 5%. There seems to be a good deal of enforcement in place with fines of US$1000 (or, alternatively, the confiscation of the car).

[34] There are reasons to believe that prices do not adjust instantaneously in quota markets such as this where price formation takes time (Joskow et al., 1998) either because agents learn gradually about the new market conditions or because they may form (temporary) expectations that the policy may be improved or ultimately removed. The number of transactions in the taxicab market in Santiago is much smaller than in the US sulfur market were took several months after the first auctions for spot prices to reflect the lower than expected demand for sulfur permits (Joskow et al., 1998).

[35] The inclusion of a large number of fixed-effects in some of the regressions leads, not surprisingly, to less efficient estimates.

Table A.1
The effects of TS on taxi license prices.

	Dependent variable: taxi's license price							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TS	0.709***	0.561***	0.620***	0.483***	0.572***	0.547***	0.509*	0.730***
	(0.041)	(0.064)	(0.190)	(0.070)	(0.072)	(0.096)	(0.279)	(0.102)
Log (licenses/population)		−0.910***	0.197	−0.632**	−0.859**	−1.118**	−0.130	−2.941***
		(0.288)	(0.649)	(0.309)	(0.356)	(0.464)	(0.915)	(0.458)
Trends	Yes	No	Yes	No	No	No	Yes	No
Year fixed effects	No	No	No	Yes	No	Yes	Yes	No
Model fixed effects	No	No	No	No	Yes	Yes	Yes	No
Sample	All	All	All	All	All	All	All	License-ads only
R^2	0.422	0.437	0.466	0.493	0.546	0.719	0.741	0.738
Observations	430	430	430	430	430	430	430	60

Notes: The dependent variable is the log of the price of taxi licenses in Santiago for the period January 2004 to November 2010. TS is an indicator variable that equals 1 after the implementation of Transantiago. Log (licenses/population) is the log of the total number of licenses established by the authority to operate a taxi in Santiago divided by the total population of the city. Trends are two linear time-trends different for before and after the implementation of TS. Year is the year-of-fabrication of the car. Model is the car model. Standard errors, in parentheses, are robust to heteroskedasticity. Levels of significance are reported as ***$p < 0.01$, **$p < 0.05$ and *$p < 0.1$.

The model in Lagos (2003) can also be used to get a better idea of how much of a demand increase in taxi rides can explain the 50–70% surge in license prices in Santiago. Given that prices in NYC prices are substantially higher than those in Santiago, there is more reason for the taxicab market in Santiago to clear above the "no-frictions frontier" (i.e., a taxi driver's search for a passenger in Santiago must necessarily take longer than in NYC). And if so, the Lagos' (2003) analytical expression for the equilibrium price of licenses is readily applicable, at least conceptually, to Santiago (recall that regulated fares remained unchanged). A lower bound for the demand increase can be obtained directly from the increase in the license price, i.e., 50–70%. A second estimate can be taken from the same NYC market: an increase in the medallion price of 50–70% corresponds to a ceteris paribus increase in demand of almost 3 times (note that the equilibrium is still above the "no-frictions frontier"). Yet, a third estimate can be obtained if we use Santiago's 2006 Origin–destination Survey and the numbers in Table 1 to get an idea of the aggregate number of taxi meetings (270 per min) and the average duration of a taxi ride (17 min): the increase in demand (i.e., meetings) now is a bit less than 6 times. Based on this range of estimates, one can safely argue that TS has at least doubled the demand for taxicab rides.

Appendix B. Summary statistics

Table B.1
Summary statistics for CO estimations in HNC.

Series	Obs	Period	Frequency	Mean	Std. dev.	Min	Max
Carbon monoxide	33,704	Nov 1987 to Nov 1991	Hourly	5.102	2.110	0.644	20.78
Sulfur dioxide	33,794	Nov 1987 to Nov 1991	Hourly	0.052	0.019	0.012	0.254
Temperature	33,378	Nov 1987 to Nov 1991	Hourly	15.94	4.786	0.467	30.77
Real humidity	32,773	Nov 1987 to Nov 1991	Hourly	47.92	20.20	2.300	99.60
Wind speed	33,671	Nov 1987 to Nov 1991	Hourly	4.597	2.032	0.400	17.60
Wind direction	33,677	Nov 1987 to Nov 1991	Hourly	173.3	56.03	1.000	420
Precipitation	35,088	Nov 1987 to Nov 1991	Hourly	2.232	4.381	0.000	53.52
Real exchange rate	48	Nov 1987 to Nov 1991	Monthly	7.30	0.65	6.28	9.41

Notes: pollutant levels are reported in parts per million, temperature in degree Celsius, humidity in percentage, wind speed in kilometers per hour, wind direction in azimuth degrees, and Real exchange rate in Mexican pesos.

Table B.2
Summary statistics for CO estimations in TS.

Series	Obs	Period	Frequency	Mean	Std. dev.	Min	Max
Carbon monoxide	34,994	Feb 2005 to Feb 2009	Hourly	0.919	1.151	0.000	9.649
Sulfur dioxide	34,944	Feb 2005 to Feb 2009	Hourly	9.258	5.873	0.852	102.7
Temperature	35,064	Feb 2005 to Feb 2009	Hourly	14.30	5.18	0.18	31.60
Real humidity	35,064	Feb 2005 to Feb 2009	Hourly	66.44	16.01	13.99	98.01
Wind speed	35,064	Feb 2005 to Feb 2009	Hourly	2.68	1.40	0.20	9.02
Wind direction	35,064	Feb 2005 to Feb 2009	Hourly	187.08	49.98	38.62	302.14
Precipitation	34,752	Feb 2005 to Feb 2009	Hourly	0.01	0.09	0.00	4.87
Atmospheric pressure	34,719	Feb 2005 to Feb 2009	Hourly	970.63	14.14	718.53	1021
Real exchange rate	120	Jan 2000 to Dec 2009	Monthly	95.5	6.3	81.4	108.8
Gasoline Price	96	Jan 2001 to Dec 2008	Monthly	517.9	517.9	368.4	721.7

Notes: pollutants concentration is measured in micrograms per cubic meter with the exception of carbon monoxide which is measured in parts per million (ppm); temperature in degrees Celsius, humidity in percentage, wind speed in kilometers per hour, wind direction in azimuth degrees, precipitation in millimeters, atmospheric pressure in millibars, real exchange rate and gasoline price in Chilean pesos.

Appendix C. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2013.08.007.

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