INSTITUTO DE ECONOMÍA

DOCUMENTO de TRABAJO

407

2011

The Effect of Transport Policies on Car Use: Theory and Evidence from Latin American Cities

Francisco Gallego; Juan-Pablo Montero; Christian Salas.

www.economia.puc.cl • ISSN (edición impresa) 0716-7334 • ISSN (edición electrónica) 0717-7593

Versión impresa ISSN: 0716-7334
Versión electrónica ISSN: 0717-7593

PONTIFICIA UNIVERSIDAD CATOLICA DE CHILE
INSTITUTO DE ECONOMIA

Oficina de Publicaciones
Casilla 76, Correo 17, Santiago
www.economia.puc.cl

THE EFFECT OF TRANSPORT POLICIES ON CAR USE: THEORY AND EVIDENCE FROM LATIN AMERICAN CITIES

Francisco Gallego*
Juan-Pablo Montero**
Christian Salas

Documento de Trabajo Nº 407

Santiago, Diciembre 2011

* fgallego@uc.cl
** jmontero@uc.cl

INDEX

The Effect of Transport Policies on Car Use: Theory and Evidence from Latin American Cities

Francisco Gallego, Juan-Pablo Montero and Christian Salas*

October 14, 2011

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. Two notable examples are 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). We develop a simple model of car use and ownership, and show that policies that may appear effective in the short run can be highly detrimental in the long run, i.e., after households have adjusted their stock of vehicles. Based on hourly concentration records of carbon monoxide, which comes primarily from vehicles exhaust, we find that household's responses to both HNC and TS have been remarkably similar and consistent with the above: an expected short-run response followed by a rapid (before 11 months) increase in the stock of vehicles.

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 policies in an effort to contain such trend.

*Gallego (fgallego@uc.cl) and Montero (jmontero@uc.cl) are with the Department of Economics of the Pontificia Universidad Católica de Chile (PUC-Chile) and Salas (csalaspaulic@worldbank.org) is with the Development Economics Research Group of the World Bank. We thank Vincent Breteau, Lucas Davis, Denny Ellerman, Matti Liski, Tomas Rau, Luis Rizzi, Stephen Ryan, Rainer Schmitz and seminar participants at the Aalto University School of Economics, Ecole Polytechnique, Harvard University, University of Maryland, Paris School of Economics, PUC-Chile, PUC-Rio and UAI for comments; Felipe González, Enrique Ide, and Andrés Osorio for excellent research assistance; Camila Correa and Francisco Muñoz for data collection; and El Mercurio for access to data. Gallego also thanks financial support from Fondecyt (Grant No. 1100623) and Montero from Instituto Milenio SCI (P05-004F) and Fondecyt.

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 city-wide transportation reform, Transantiago (TS), to improve and increase the use of public transport. As shown in Table 1.1, other major efforts in Latin America are also of the above type, either driving restrictions or reforms in public transportation.

There is quite a bit of controversy on the effectiveness of these type of policies in persuading drivers to give up their cars in favor of public transport, and hence, in reducing congestion and pollution (e.g., EIU, 2010). The problem with evaluating these policies is that it is hard to construct a counterfactual against which the performance of the policy can be contrasted upon. Transportation systems are remarkably complex and dynamic (Small and Verhoef, 2007), which makes any evaluation even more difficult because we are often not much interested in the immediate or short-run effect of the policy but in its long-run effect, i.e., whether and how fast households adjust their stock of vehicles.[1] This paper is a theoretical and empirical attempt at evaluating both of these effects.

We develop a theoretical model that distinguishes short from long-run impacts of transportation policies that can take different forms. In constructing the model we partially borrow from the bundling literature (e.g., McAffee et al, 1989; Armstrong and Vickers, 2010), so as to capture in a simple way the essential elements of a household's decision 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. Households are both horizontally and vertically differentiated: they differ in their preferences for transportation modes —cars vs buses— and in the amount of travel.[2] 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).

The model illustrates how uninformative the short-run impact of a policy can be. For example, a driving restriction policy, which the model captures with a reduction in vehicle capacity, has an unambiguous short-run impact (i.e., before any household has

[1] In this paper, short-run and immediate 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 can be longer-run effects (e.g., inter and intra city migration of people and commercial activity) but we do not have the data to quantify them; neither our empirical methodology is well suited to identify effects that are too far away from the policy shock. Besides, these effects are likely to be minor here given the unique characteristics of Mexico-City and Santiago and the fact that both policies, HNC and TS, were applied city wide. Duranton and Turner (2011), however, find that these longer-run effects are important in explaining the increase in vehicle travel after an expansion of interstate highways in the US.

[2] Note that cars refer to private transportation more generally (e.g., passenger cars, motorcycles, etc) and buses to any kind of public transportation including the subway.

adjusted its stock of vehicles), at least qualitatively: a reduction in car trips during both peak and off-peak hours. Depending on parameter values (e.g., price of cars), the long-run impact of the policy can go either way, however. If cars are relatively expensive, the reduction in car trips can remain in the long-run or even extend if enough households find it optimal to "return" their cars. Conversely, if cars are less expensive, the policy can result in an increase in the number of vehicles in the long-run.

Similar arguments apply to a public transportation reform, which the model captures with a change in the variable cost of using public vis-à-vis private transportation. Regardless of the direction of the relative price change, its short-run effect on car use is likely to be small and hard to detect empirically.[3] The long-run effect, however, can be shown to be substantial in either direction. The model also shows that the effect that a policy intervention may have on car use can vary widely depending on the hours of the day and days of the week; thereby, the importance of estimating these effects separately. We do so not only for theoretical reasons but also for empirical ones. As we explain below, some of our estimates are obtained from just looking at changes in vehicle use during peak hours.

With these theoretical insights, we study the impact on car use of the two policies mentioned above: the driving restriction in Mexico-City (HNC) and the public transportation reform in Santiago (TS). We look at these policies for two reasons. These are policies of different nature and implemented in different cities, almost 18 years apart, which makes it interesting to contrast the way households responded to them. Secondly, they amount to one-time drastic interventions like no other.

HNC, as implemented in 1989, affected almost all drivers in a permanent way — and according to several sources (e.g., Eskeland and Feyzioglu, 1997), compliance with the program was near universal. In contrast, most other driving restrictions presented in Table 1.1 have affected only a fraction of drivers (e.g., those using older cars) and under special circumstances (e.g., days of unusually high pollution).[4] TS, on other hand, consisted of a complete transformation of the public transportation system of an entire city at once (Muñoz et al., 2009). Other public transportation reforms like Transmilenio in Bogotá have been more limited in scope and introduced gradually. TS involved, among other things, a significant and sudden reduction in the number of buses and a radical change in the design and number of routes. Unfortunately, and for reasons well

[3] Litman (2004) argues that cross elasticities between public and private transportation are quite low in the short-run (0.05). Furthermore, it is highly likely that most of the car capacity is already in use. According to the *Encuesta Origen-Destino* (Origin-Destination Survey) of 2006 for the city of Santiago, EOD-2006, most passenger cars in the city (799,811) were already in use to cover an equivalent number of morning trips (706,518).

[4] The driving restrictions in Medellín and Quito also appear quite comprehensive (e.g., Cantillo and Ortuzar, 2011), but there is limited data to study them as we do here.

discussed in Briones (2009) and Muñoz et al. (2009), TS was plagued with design and operation problems that brought the city's public transport near collapse right after implementation and that for the most part have remained even until today. According to different statistics, the result has been a significant and permanent increase in the cost of using public transport.[5]

Our empirical evaluations are mainly based on hourly observations of concentration of carbon monoxide (CO), which are recorded by a network of several monitoring stations —15 in Mexico-City and 7 in Santiago— distributed over the two cities (stations also keep records of other pollutants and weather variables). CO is found to be a good proxy for vehicle use, particularly at peak hours, compared to alternative candidates like hourly records of traffic flows and of other pollutants. Mobile sources, and light vehicles in particular, are by far the main emitters of CO —97% and 94%, respectively, at the time HNC and TS were implemented.[6] We compare CO levels before and after policy implementation for different hours of the day and days of the week. In order to control for seasonal variation and any phenomena unrelated to the policy, we employ several control variables including (hour, day, and month) fixed effects, linear trends, and proxies for economic activity and meteorological and atmospheric conditions. These pollution observations are available for several years in some of the cities where pollution is more acute, which makes its use attractive for policy evaluation. Davis (2008) and Chen and Whalley (2011) are two good recent examples of the use of this high-frequency data.[7]

Empirical results for HNC show statistically significant reductions of CO in the short run of 11% and 6% for peak and off-peak hours, respectively. This short-run result is in line with the perception of high compliance with the program. For the long run we find an increase of 13% during peak hours and of 11% during of off-peak hours. Estimates for weekends show no reduction in the short-run, as expected, and a significant increase in the long run of 20%.[8] In all three estimations, the long-run impact of the policy is reached about the same time: 10 to 11 months after implementation. As for TS, we can only report results for peak hours.[9] We find no impact on CO in the short run

[5] The Economist (Feb 7th, 2008) referred to TS as "...a model of how not to reform public transport." In the next section we provide more details on both policies.

[6] Further justification for the use of CO is discussed in section 4.1.

[7] Davis (2008) explores the effect of HNC on various pollutants and so do Chen and Whalley (2011) in the case of an investment in public transportation in Taipei, Taiwan. They specifically focus, with methods different than ours, on the impact of the policy right after implementation.

[8] Note that this 20% increase comes close to the 24% net increase at peak hours (from -11% to +13%) and the 17% increase at off-peak hours. These net increases are all statistically significant at 1%.

[9] Off-peak and weekend results were highly sensitive to small changes in specification and inconsistent with theoretical predictions. This was partly because weekend and off-peak CO levels are quite low —much lower than the 1989 levels observed in Mexico-City.

and a 33% increase in the long run. This long-run level is reached 9 to 10 months after implementation.

These CO results show that households' response to both HNC and TS have been remarkably similar: an expected immediate or short-run impact —sizeable reduction in car use in HNC and almost no change in the case of TS— followed by a rapid increase in the stock of vehicles.[10] These disappointing CO results[11] are consistent with additional evidence coming from other data sources including gasoline consumption, car registrations and sales and prices of taxi licenses (medallions).[12] Furthermore, the ample magnitude of the CO effects may suggest that a large fraction of households were nevertheless able to accommodate, at a reasonable cost, to policy shocks that did not work as intended. With the help of the model we show otherwise, that only a few did.

We also exploit income variation within cities and CO records from individual monitoring stations distantly located to test whether the response to these transport policies depends on income (ex-ante car use) in a way that is consistent with the model. Looking at these more disaggregate responses not only constitutes an additional robustness check of our empirical strategy but it can also reveal important heterogeneities (in costs and benefits) that may prove relevant for policy evaluation.[13] We find HNC to have its largest impact in middle-income neighborhoods, where households were more likely to buy a second car, and lowest in high-income neighborhoods where households had already sufficient car capacity to cope with the driving restriction. Results for TS follow the predicted pattern as well. We find the short-term impact to be negligible in all parts of the city and the long-term impact to be decreasing with income.[14]

A main implication from these theoretical and empirical results is that policies that may appear effective in the short-run can be highly detrimental in the long run; thereby, the importance of understanding when and the extent to which households adjust their stock of vehicles and how fast. Both policy experiences confirm that the adjustment

[10] Interestingly, the speed at which the stock of vehicles has adjusted in both of these experiences is 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.

[11] We are certainly not the first ones in documenting that these programs have proven ineffective in getting people off their cars —Eskeland and Feyzioglu (1997), Molina and Molina (2002) and Davis (2008) have done so for HNC and so have Muñoz et al (2009) and Yáñez et al (2010) for TS— but we are the first ones in characterizing and quantifying their long-run effects and the underlying adjustment process.

[12] On this latter, our econometric results and Lagos' (2003) model suggest that the demand for taxicab rides in Santiago has at least doubled because of TS. Still, taxi rides consituted less than 1% of all trips before TS (EOD-2006).

[13] Although we do not do it here, accounting for differential effects may be particularly relevant for quantifying health costs associated to non-uniformly mixed pollutants, like CO, in large cities. And these costs that can be substantial as reported elsewhere (e.g., Currie and Neidell, 2005).

[14] These income effects are also seen, at least qualitatively, in the traffic flow data.

process is quite fast —adjustment that for most part is irreversible— 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., Feng et al., 2005; Fullerton and Gan, 2005) that so far has received none in the region.[15]

The rest of the paper is organized as follows. Section 2 describes the two transport policies in detail. The theoretical model is presented in Section 3. Empirical results based on CO records are in Section 4. Additional empirical exercises using alternative data sources are in Section 5. Discussion of results including estimations of the costs inflicted by these policies are in Section 6. Concluding remarks are in Section 7.

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 5am to 10pm, based on the last digit of its license plate. The program was implemented all at once and the low cost of detecting non-compliers, the heavy fines, and high police control resulted in near universal compliance (Eskeland and Feyzioglu, 1997; and Davis, 2008). The program did not experience any relevant changes for the next two years.[16]

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. Figure 2.1 plots average hourly CO concentration levels for the period 1987-1991, which is the 4-year (symmetric) window we use in our empirical estimation. The vertical line indicates the exact moment HNC was implemented. Dots are average hourly concentration levels of CO for all hours in our sample and the continuous line is the weekly average. A quick look at the plot shows no clear indication of a decrease or stabilization in CO concentrations; if anything, it shows an increase in both the weekly average and the lower bound of the range of hourly concentrations.

[15] Salient market-based transport policies are the London's congestion charge (e.g., Leape, 2006) and the Singapore's market for tradeable car quotas (e.g., Koh, 2004). The political economy of why Latin America has stayed away from these or similar policies (e.g., fuel taxes aimed at correcting pollution externalities) is beyond the scope of the paper but it is nevertheless an interesting area for more research. Caffera (2011) touches on the issue but in the specific context of pollution control from industrial sources.

[16] The first relevant change following the implementation of HNC came almost two years later in October of 1991 when the existing ban to public transportation (introduced in January of 1991) was extended from Saturdays to weekdays from 10 am to 9 pm in an alternating manner similar to that of cars. Later, in 1992, cars using natural or liquefied gas were exempt from HNC. For more details on these policy changes see Ide and Lizana (2011).

Nearly 18 years later, on February 10 of 2007, Chile's government implemented TS, with a similar motivation than HNC, that of persuading drivers to give up their cars, but with a different instrument: improving 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).[17] TS was intended to remedy these problems at once and for the entire city. It involved a significant and sudden reduction in the number of buses, from roughly 7500 to 5500,[18] and a radical (and centrally-planned) change in the design and number of routes more in line with a hub-and-spoke network where the existing subway would play the role of the hub.

While the original design of TS was expected to deliver significant reductions in congestion and pollution from fewer cars on the street,[19] its actual implementation has been recognized by many as a major policy failure (e.g., Briones, 2009). Table 2.1 provides numbers illustrating the extent of the intervention. Commuting time increased, on average, from 77 to about 90 minutes (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 minutes). In contrast, travel time of cars and taxis does not seem to have been affected nearly as much.[20] Unlike HNC, TS suffered from modifications right from the start but that for most part took place within 12 months of implementation when the number of buses stabilized at its current level (see also the last two columns of Table 2.1).[21] Yet, public opinion and quality indicators suggest that the level of service never returned to pre-TS levels at least during the period of our estimation.[22]

This deterioration in the quality of service should have resulted in a switch towards

[17]Most bus routes passed through the central business district connecting terminal points on the pheriphery, with average length of more than 60 kms (counting both directions), so most passangers could travel almost anywhere in the city without transfers. Under TS, passangers are expected to transfer a few times before completing their journeys (Muñoz et al., 2009).

[18]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, Gómez-Lobo et al. (2011) find this to be the case.

[19]DICTUC (2009) estimates that TS, as conceived by its architects, would have reduced CO concentrations by 15% by 2010.

[20]Bravo and Martínez (2007) document that average commuting time to work of skilled workers has remained roughly constant (from 53.4 to 53.2 minutes) while the one of unskilled workers has increased by 17% (from 82.9 to 97 minutes).

[21]This stabilization is also found by Yañez et al (2010) that show that for a sample of 250-300 individuals that use of buses hardly changed between May 2007 and October 2008.

[22]According to survey data collected by Libertad y Desarrollo (www.lyd.com), the approval rate of Santiago's public transport dropped immediately with the implementation of TS (February 2007) to recover a bit a year later (March 2008) and to remain there until these days (May 2011).

alternative modes of transportation, mainly cars, and hence, in an increase in CO emissions. Figure 2.2 plots average hourly CO concentration levels for the period 2005-2009 which is the period we use in our empirical estimation.[23] Again, dots are hourly averages of CO concentrations for all hours in the sample, the continuous line represents the weekly average, and the vertical line indicates the exact moment TS went into operation. The plot shows no indication that TS has lead to a decline in CO concentrations; at best, it has lead to an increase in the upper bound of the range of hourly concentrations and, consequently, a slight increase in the weekly average. Note also the large number of records of nearly zero value, which is never the case in HNC. This not only suggests that pollution in Mexico-City in 1989 was significantly higher than in Santiago in 2007 but, more importantly, that we may face identification difficulties, as we discuss in Section 4, to study the effect of TS on off-peak CO concentration levels.

3 A model of car ownership and use

Can theory explain the empirical results presented in the introduction? To answer this question, we develop a "bundling" model that captures in a simple way two essential elements of a household's problem which are the allocation of existing vehicle capacity to competing uses (peak vs off-peak hours) and how that capacity is adjusted in response to a policy shock. The model is flexible and simple enough to accommodate to all sorts of policy interventions. Following the presentation of the model, we calibrate it for both cities using ex-ante (i.e., before the policy) information on car ownership and use. The calibrated model is then used to generate predictions of how households respond to different policies.

3.1 Notation

There is a continuum of agents (households) of mass 1 that decide between two modes of transportation —polluting cars and public transport (e.g., buses)— to satisfy its demand for travel during both peak and off-peak hours (we will often refer to peak demand as high (h) demand and off-peak demand as low (l) demand). Households differ in two ways: in their preferences for one mode of transportation over the other (horizontal differentiation) and in the quantity of transportation (e.g., kms traveled, number of trips) they wish to consume (vertical differentiation). Horizontal preferences are captured with a two-dimensional Hotelling linear city. A household's horizontal preferences are

[23] Another reason to concentrate on this four-year window is that by the end of 2008 the financial international crisis started to have an impact on the Chilean economy creating price and income effects that possibly affected the use of private transportation.

denoted by $(x^h, x^l) \in [0,1] \times [0,1]$, where x^h is the household's distance to the car option for peak hours and x^l is the distance to the car option for off-peak hours. This same household's distance to the bus option is $(1 - x^h, 1 - x^l)$. The density of (x^h, x^l) is $f(x^h, x^l)$. Furthermore, the product differentiation (or transport cost) parameter is t^h for the peak and t^l for the off-peak. A household's vertical preferences are captured with inelastic travel demands which are denoted by $(q^h, q^l) \in [0,1] \times [0,1]$, where q^h and q^l are the household's number of trips during peak and off-peak hours, respectively.[24] The density of (q^h, q^l) is denoted by $g(q^h, q^l)$.

A household is assumed to have a choice of owning zero, one, or two vehicles. Unlike public transportation (buses), private transportation comes with a capacity restriction that depends on the stock $s \in \{0, 1, 2\}$ of vehicles owned by the household. A household that owns a single vehicle ($s = 1$) has $k < 1$ trips available to be shared between peak and off-peak hours.[25] In turn, we assume that a household that owns two vehicles ($s = 2$) faces no capacity constraints. The unit cost of using a car during peak hours is p_c^h and during off-peak hours is p_c^l. The unit cost of taking a bus is p_b^i for $i = h, l$. In principle these costs should also depend on congestion (i.e., aggregate car travel), but we do not need to be explicit about them because we are only interested in the price difference, i.e., $\Delta p^i \equiv p_b^i - p_c^i$ for $i = h, l$,[26] which simplifies the analysis greatly.

A type-(q^h, q^l, x^h, x^l) household enjoys a gross utility of $\upsilon(q^h, q^l)$ from consuming q^h and q^l trips, which we assume large enough that all types complete all their trips either by bus or car. A household's utility depends on whether vehicle capacity is binding or not. It is not binding if either (i) $s = 2$ or (ii) $s = 1$ and $q^h + q^l \leq k$, in which case the household's (net) utility as a function of its car stock is given by

$$u(\cdot|s) = \begin{cases} \upsilon - p_c^h q^h - p_c^l q^l - t^h x^h - t^l x^l & \text{if car for } h \text{ and } l \\ \upsilon - p_c^h q^h - p_b^l q^l - t^h x^h - t^l (1 - x^l) & \text{if car for } h \text{ and bus for } l \\ \upsilon - p_b^h q^h - p_c^l q^l - t^h (1 - x^h) - t^l x^l & \text{if bus for } h \text{ and car for } l \\ \upsilon - p_b^h q^h - p_b^l q^l - t^h (1 - x^h) - t^l (1 - x^l) & \text{if bus for } h \text{ and } l \end{cases} \tag{1}$$

Note that the fourth row in (1) also corresponds to the utility of a household that owns no vehicles.

[24] The model can be easily extended, at the cost of additional notation, to elastic demands, e.g., $q^i(p^i) = \theta^i D(p^i)$ for $i = h, l$ and with $\theta^i \in [0, 1]$.

[25] Because of this capacity constraint, we think of q^h and q^l as weekly quantities. This would accomodate, for example, a household with a single car that on a daily basis alternates its use between peak (commuting to work) and off-peak (shoping).

[26] Take, for example, a transport policy that improves public transportation and, as a result, it also alleviates congestion. Our model captures these changes as reductions in both p_b^i and p_c^i. However, given the structure of the model, the household only cares about Δp^i. Note also, that this formulation can accomodate that car trips may be longer than bus trips.

On the other hand, the car capacity is (potentially) binding if $s = 1$ and $q^h + q^l > k$. Since now the household needs to rely on buses to complete one or both of its travel demands, there are two cases to consider. The first case —car specialization— is when the household allocates the entire car capacity k to satisfy $i = h, l$ and the bus to satisfy $j \neq i$. If so, its utility is

$$u(\cdot|s) = \upsilon - p_c^i \min\{q^i, k\} - p_b^i \max\{0, q^i - k\} - p_b^j q^j - t^i x^i - t^j(1 - x^j) \qquad (2)$$

If $q^i > k$, this household completes its demand for i trips with buses despite it was not its preferred option. Note that under this formulation two households, say 1 and 2, that only differ in their demand for i travel ($q_2^i > q_1^i \geq k$) are equally likely to use and buy a single vehicle. In other words, if household 1 is indifferent between using (and buying) a single car or taking the bus for i-travel, household 2 is equally indifferent (having a larger demand does not make the single-car option more attractive; it may eventually move the household to buy two vehicles).

The second case —car splitting— is when the household shares the car capacity between h and l. Letting $k^i \leq k$ denote the fraction of the capacity going to i and $k^j = k - k^i$ to j, the household's utility in this case is

$$u(\cdot|s) = \upsilon - p_c^i k^i - p_b^i(q^i - k^i) - p_c^j k^j - p_b^j(q^j - k^j) - t^i x^i - t^j x^j \qquad (3)$$

where $k^i \leq q^i$ and $k^j \leq q^j$. Note, however, that if $\Delta p^i > \Delta p^j$, the household would like to allocate as much capacity as possible towards i-travel. But an allocation such as $k^i = k$ and $k^j = 0$ would invalidate (3) almost by construction since none of j demand would be satisfied with car trips. We solve this in a simple way; if the car capacity is to be shared, it is done proportional to the demands, i.e., $k^i = q^i k/(q^i + q^j)$ for both $i = h, l$.[27]

In deciding whether to own zero, one or two vehicles the household solves

$$\max_s\{\max u(\cdot|s) - rs\} \qquad (4)$$

where $\max u(\cdot|s)$ is the utility from the best (short-run) transportation mix for a given stock $s \in \{0, 1, 2\}$ and r is the cost of buying a car.[28] Implicit in (4) is the assumption that households constantly adjust their stock of durables to their optimal level while in reality liquidity constraints and/or transaction costs may create a range of inaction where

[27] We are informally saying that there may be decreasing marginal benefits in car use that justify an interior (splitting) solution. This latter is more reasonable if Δp^i is not too far apart from Δp^j, which is what we find in the calibrations.

[28] Note that if $r < \min\{t^h, t^l\}$, households with strong preferences for cars, say $x^h = 0$ or $x^l = 0$, would buy a car even if $q^h = q^l \approx 0$.

agents do not adjust their stocks at all (e.g., Eberly, 1994).[29] We will come back to this issue below.

3.2 Short and long-run choices

We now compute a household's optimal use and ownership choices.[30] The structure of the model allows us to conveniently sequence the analysis from vertical preferences to horizontal preferences. We can first segment households on their likelihood of buying one or two vehicles from looking at their demands q^h and q^l; then we can tell which of these households will indeed buy and use the vehicle(s) from looking at their horizontal preferences x^h and x^l.

Consider first households with $q^h + q^l \leq k$. These households, those in group A in Figure 3.1, will at best consider buying and using a single vehicle; the ones that do are shown in Figure 3.2(a) (for now, ignore the dotted lines in both Figures 3.1 and 3.2 and the α's in Figure 3.2). As in any (multi-product) bundling problem, some consumers will choose to consume both products (h and l travel) from the same "supplier" (car or bus), i.e., "consume the bundle", while others will choose to consume from both suppliers. Figure 3.2(a) consolidates in one place both household's long- and short-run choices. All households with $x^i \leq \hat{x}^i(q^i) \equiv 1/2 + \Delta p^i q^i / 2t^i$ would rather use the car than the bus for i-travel (provided they have one available). And all households with $x^i \leq \hat{x}^i - r/2t^i \equiv \tilde{x}^i(q^i)$, buy a vehicle despite it will only be used for i-travel, i.e., despite $x^j > \hat{x}^j(q^j) \equiv 1/2 + \Delta p^j q^j / 2t^j$. There is fraction of households with weaker preferences for cars, i.e., $\tilde{x}^i < x^i < \hat{x}^i$ for $i = h, l$, which also buy the car because of the "bundle discount" associated to it. The car-bundle discount is exactly equal to r.[31]

We can now use Figure 3.2(a) to illustrate the short and long run effects of a public transport reform like TS. Suppose the policy means a slight deterioration of the quality of public transport during peak hours, which can be captured by an increase in $\Delta p^h / t^h$ of some small amount ε, as illustrated by the dotted line in the figure. Unlike households that buy (and use) the car-bundle, households that only use the car for l-travel (the "two-stop shoppers" of the bottom-right corner) have spare car-capacity that is ready to be used for h-travel. Hence, there is an immediate (i.e., short-run) increase in car trips

[29] Transaction costs may come from sales fees, sales taxes, search costs or the lemons problem afflicting used vehicles.

[30] Note that if $\Delta p^h = \Delta p^l = 0$, $r = 0$ and $f(x^h, x^l) \equiv 1$, only 50% of trips will be made on cars.

[31] The (long-run) purchasing cost of consuming car for i-travel only is $r - \Delta p^i q^i$ while for both h and l travel is $r - \Delta p^h q^h - \Delta p^l q^l$. The "bus-bundle" does not come with any discount.

(and pollution) during peak hours from households in group A equal to

$$\Delta C^h_{SR}(A) \equiv \iint_A \varepsilon q^h \alpha^h_1(q^h, q^l) g(q^h, q^l) dq^l dq^h = \int_0^k \int_0^{k-q^h} \varepsilon q^h \alpha^h_1(\cdot) g(\cdot) dq^l dq^h > 0$$

where α^h_1 (see the figure) is given by

$$\alpha^h_1(q^h, q^l) = \int_0^{\tilde{x}^l(q^l)} f(\hat{x}^h(q^h), x^l) dx^l \tag{5}$$

If the policy shock ε is permanent, there is an extra increase in car trips from additional car purchases, so the long-run effect of the policy upon group A during peak hours is equal to

$$\Delta C^h_{LR}(A) \equiv \iint_A \varepsilon q^h (\alpha^h_1 + \alpha^h_2 + \alpha^h_3) g(\cdot) dq^l dq^h$$

where $\alpha^h_2(q^h, q^l)$ is given by an expression similar to (5) and α^h_3 by

$$\alpha^h_3(q^h, q^l) = \int_{\tilde{x}^l(q^l)}^{\hat{x}^l(q^l)} f\left(\tilde{x}^h(q^h) + [\hat{x}^l(q^l) - x^l] t^l / t^h, x^l\right) dx^l$$

But because the policy also moves some households from the bus-bundle to the car-bundle, there is a long run effect during off-peak hours as well (despite the price of public transport has not changed there), which is equal to

$$\Delta C^l_{LR}(A) \equiv \iint_A \varepsilon q^l \alpha^h_3 g(\cdot) dq^l dq^h$$

Consider now households with $q^h + q^l > k$. There are four cases to study: groups B, C, D and E in Figure 3.1. Like those in group A, households in group B buy at most one vehicle, $s \in \{0, 1\}$, because q^h and q^l are, either individually or together, not large enough to justify the purchase (and use) of two vehicles. It does not pay to buy two vehicles for multiple use if $u(\cdot|s = 2) \leq u(\cdot|s = 1)$, or more precisely, if

$$2r - \Delta p^h q^h - \Delta p^l q^l \geq r - \Delta p^h k^h - \Delta p^l k^l \tag{6}$$

where $k^i = q^i k/(q^i + q^j)$ for $i = h, l$. Note that if $\Delta p^h \approx \Delta p^l = \Delta p$, then (6) reduces to $q^h + q^l \leq k + r/\Delta p$: It only pays to buy a second (multi-purpose) car if the saving $\Delta p(q^h + q^l - k)$ more than offset the cost r. The equivalent of (6) for a (single-purpose) vehicle is $q^i \leq k + r/\Delta p^i$ (see Figure 3.1). The fraction of households in group B that effectively end up buying and using the car is shown in Figure 3.2(b). Note that the car-bundle discount continues to be r despite the capacity constraint.

More interestingly, we can now use Figure 3.2(b) to illustrate the short- and long-run effect of a second type of policy intervention: a driving restriction like HNC. Suppose the policy reduces car capacity k by a small amount ε. There are three short-run effects. The first is the ε drop in car trips from households that use (and continue using) the car at full capacity, i.e., those that consume the car-bundle. The second short-run effect, which is captured by the horizontal dotted line in the upper-left corner in the figure, is the reduction of car trips during off-peak hours from households that no longer consume the car-bundle. This drop amounts to $\iint_B \varepsilon(\Delta p^l k^l/2t^l k)k^l\alpha_1^l g(\cdot)dq^l dq^h$. Similarly, the third short-run effect, which is captured by the vertical dotted line in the lower-right corner, is the reduction of car trips during peak from households that no longer consume the car-bundle and is equal to $\iint_B \varepsilon(\Delta p^h k^h/2t^h k)k^h\alpha_1^h g(\cdot)dq^l dq^h$.

The driving restriction can also have an additional and "positive" effect on car travel in the long-run upon this group. For some households owning a car is no longer that attractive (although using it is, provided the car is available). In fact, if the resale price of a car is still r, a fraction of households in B would sell their cars, and hence, reduce their car trips, in both peak and off-peak, by $\iint_B \varepsilon(\Delta p^h k^h/2t^h k)k^h(\alpha_2^h + \alpha_3^h)g(\cdot)dq^l dq^h$ and $\iint_B \varepsilon(\Delta p^l k^l/2t^l k)k^l(\alpha_2^l + \alpha_3^l)g(\cdot)dq^l dq^h$, respectively.[32] However, if these households face a transaction cost equal to

$$\lambda \geq \varepsilon\frac{\Delta p^l}{r}, \tag{7}$$

none of these additional long-run benefits will accrue since no household will return a car at a resale price of $(1-\lambda)r$.

That the driving restriction reduces car travel (in the short-run and potentially in the long-run) extends to all other households in group B except to those close to the border $q^h + q^h = k - r/\Delta p$. As captured by the (downward) sloping dotted line in Figure 3.1, these households now belong to group C, so some of them will find it attractive to increase the size of their car-bundle and buy a second car; not only by-passing the driving restriction altogether but what is worse, increasing car travel during both peak and off-peak hours.[33] Figure 3.2(c) distinguishes precisely those households in group C that buy two vehicles from those that buy one and from those that buy none (to simplify the exposition the figure focus on the case in which $q^h, q^l \geq k$, say, subgroup C1).[34] In this case the bundle discount is not longer r but $\Delta p^l(q^l - k) + \Delta p^h(q^h - k)$. This is because households that want the car only for i-travel do not buy two vehicles but just

[32] Note that α_3^h and α_3^l are related by $\Delta p^h k^h \alpha_3^h/t^h = \Delta p^l k^l \alpha_3^l/t^l$.

[33] Note that the same inward shift of the border $q^h + q^l = k + r/\Delta p$ would happen with a policy intervention that increases both Δp^l and Δp^h by ε.

[34] There are three more subgroups: C2, where $q^h, q^l < k$; C3, where $q^h < k$ and $q^l \geq k$; and C4, where $q^h \geq k$ and $q^l < k$.

one.

The dotted line in Figure 3.2(c) depicts the effect of the driving restriction on group C1. The short-run effect is simply the drop by the amount ε of car trips from the two-stop shoppers. The long-run effect can be divided in two parts. The first corresponds to the two-stop shoppers that would like to sell their cars if the resale price were to remain at r; if so, this would reduce car trips by $\iint_{C1} \varepsilon(\Delta p^h/2t^h)k\alpha_2^h g(\cdot)dq^l dq^h$ during peak and by $\iint_{C1} \varepsilon(\Delta p^l/2t^l)k\alpha_2^l g(\cdot)dq^l dq^h$ during off-peak. And the second part corresponds to two-stop shoppers that buy a second car; not only by-passing the driving restriction for their i trips but now also using the car for all of their j trips. This increase in car trips amounts to $\iint_{C1} \varepsilon(\Delta p^h/2t^h)q^h\alpha_1^h g(\cdot)dq^l dq^h$ during peak and $\iint_{C1} \varepsilon(\Delta p^l/2t^l)q^l\alpha_1^l g(\cdot)dq^l dq^h$ during off-peak. This is by far the most adverse effect of a driving restriction.

As shown by the horizontal and vertical dotted lines in Figure 3.1, this adverse effect extends to households in group C that now belong to group D; a group in which households own either two vehicles, one or none. As shown in Figure 3.2(d), the difference with group C is that some households in group D may buy two cars just for i-travel (again, the figure focus on the case in which $q^h \geq k + r/\Delta p^h$ and $k \leq q^l < k + r/\Delta p^l$, say, subgroup D1).[35] The effect of the driving restriction policy on the two-stop shoppers that have one car is the same as on the equivalent two-stop shoppers in C1. Finally, there is the group of households, group E, that because of their large demands own either two vehicles or none. As shown in Figure 3.2(e), these households never face capacity restrictions (shortly we will come back to the dotted lines in the figure).[36]

3.3 Numerical exercises: Calibration and simulations

We first calibrate the model to parameter values that reflect the ex-ante (i.e., before the policy) situation of each city in terms of car ownership and use. The car-ownership information includes the fraction of households that either own no cars ($s = 0$), one car ($s = 1$), or two (or more) cars ($s = 2$). The car-use information, on the other hand, includes the share of car trips at peak hours (q^h_{car}/q^h), the share at off-peak (q^l_{car}/q^l), and the ratio of car trips at peak over car trips at off-peak (q^h_{car}/q^l_{car}). The ex-ante information is summarized in the first half of Table 3.1.[37] In all numerical exercises, we assume that

[35] There are three more subgroups: D2, where $q^h \geq k + r/\Delta p^h$ and $q^l < k$; D3, where $q^l \geq k + r/\Delta p^h$ and $k \leq q^h < k + r/\Delta p^l$; and D4, where $q^l \geq k + r/\Delta p^h$ and $q^h < k$.

[36] Note that the bundle discount for these households is $2r$ since they would buy two cars even if they are to be used only for i-travel.

[37] The ex-ante information for HNC was obtained as follows: car-ownership from INEGI (1989a), q^i_{car}/q^i from Molina and Molina (2002, p. 227), and q^h_{car}/q^l_{car} from the EOD-2007 for Mexico-City. In the absence of more information, and based on what we know from EOD-2007 for Mexico-City and EOD-2006 for Santiago, we also assumed for HNC that $q^h_{car}/q^h = q^l_{car}/q^l$. All the ex-ante information for TS was obtained from the EOD-2006 for Santiago.

households' preferences are drawn from uniform distributions, i.e., $f(x^h, x^l) = g(q^h, q^l) \equiv 1$. The bottom half of Table 3.1 presents the calibration parameters obtained for each city.[38] The differences we observe are for the most part expected; for example, the higher use of cars in Santiago is consistent with a higher k and lower r.

As a first simulation exercise with the calibrated model, we try to replicate the empirical results found for HNC by decreasing car capacity k by 20%. As shown in the first row of Panel A of Table 3.2, this HNC-like policy leads to declines in car use not only in the short run ($\Delta C_{SR}^{i=h,l}$) but also in the long run ($\Delta C_{LR}^{i=h,l}$). These numerical figures are quite consistent with our short-run CO estimates (reductions of 11 and 6% for peak and off-peak hours, respectively) but are far from our long-run estimates (increases of 13 and 11%, respectively). The long-run inconsistency can be partly explained by two assumptions in exercise A1 that are unlikely to hold in practice. First, in A1 all households have the option to return their cars at the original price r (according to the change in the stock of vehicles shown in the last column there is indeed a large number of households that would like to do so). If instead we assume that transaction/lemon costs are such that no household returns its car(s), i.e., eq. (7) holds, exercise A2 shows that in the long run the policy leads to a net increase in the stock of vehicles (2.8%) although still accompanied by a minor decline in car use (-1.2%).

The second assumption in A1 is that the additional stock is equally polluting (and fuel-efficient) as the existing one, which we know from Eskeland and Feyzioglu (1997) is not true for HNC because of the import of older cars from adjacent regions. Thus, if we also let the additional stock be three times as polluting (and less fuel-efficient) as the existing one,[39] the results in ex. A3 illustrate that our empirical estimates are consistent with the theory once we incorporate these more realistic assumptions. More generally, even tough we find that the long-run effects of the policy on car use and on the stock of cars imply that the short-run effect is for the most part undone, these exercises suggest that the long-run increase in CO during weekdays that we observe for the case of HNC is much less due to increases in car use and congestion (actually they hardly changed with respect to the pre-HNC levels) than to the entry of older and more polluting cars.

We move now onto the policy experience in Santiago, TS. Recall that the model captures a TS-like policy with changes in Δp^h and/or Δp^l. The first exercise in Panel B of Table 3.2 (exercise B1) considers a TS-like policy that inflicts a uniform deterioration of 24% in the relative quality of the public transport, i.e., Δp^h and Δp^l go up by that

[38] We used the same initial values in both calibrations: $\Delta p^h = \Delta p^l = t^h = t^l = r = 2k = 1$.

[39] Based on Betaon et al (1992), who find that each additional year increases CO emissions by approximately 16%, a factor of 3 would suggest that the additional stock is 7.3 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).

amount. Both short and long-run effects ($\Delta C_{SR}^{i=h,l}$ and $\Delta C_{LR}^{i=h,l}$, respectively) are entirely consistent with our CO estimates for peak hours (no impact and 33% increase, respectively). Since our empirical analysis of CO records failed to identify effects at off-peak hours, for reasons we explain below, the reader may wonder what kind of TS-like policy could simultaneously generate sizeable effects at peak and virtually none at off peak. Exercise B2 considers such possibility; the relative quality of public transport must deteriorate by 75% at peak and improve by 61% at off-peak.[40] But such a pronounced asymmetric change in quality is unlikely since the main elements of TS (i.e., fewer buses and new routes) are common to peak and off-peak service. One could argue nevertheless that off-peak service was less affected or at best not affected (i.e., $\Delta p^l \approx 0$), partly because of the more frequent subway service at off-peak prompted by TS. In any case, these results confirm that failing to identify effects at off peak is nothing but an empirical problem.

Exercise B1 also shows a big increase in the stock vehicles of 22%, which is way above our empirical finding of around 5% (see section 5). The next two exercises consider changes in Δp^h and Δp^l that can produce stock variations more in line with this empirical finding. In B3 we let both Δp^h and Δp^l raise by 6% while in B4 we let Δp^h raise by 15% and Δp^l remain unchanged. But now, car use (or CO) during peak hours is way below our empirical estimate of 33% in either case. There are two factors, however, that neither B3 nor B4 account for. Unlike in HNC, the increase in car use could have very well generated additional congestion, more so if at peak hours streets already presented some degree of saturation at the time the policy was implemented.[41] While the effect of additional congestion on car use is already captured by our model with smaller than otherwise increases in Δp^h and Δp^l, the effect on CO is not. The second factor, also present in HNC, is the possible arrival of older and more polluting cars. Exercise B5 extends B4 to incorporate both of these corrections. First, we let the stock of additional vehicles be 1.24 times as polluting as the existing stock (this captures that a third of the additional stock corresponds to used cars, some of which quite old),[42] and second (and consistent with the changes in traffic flows we report in section 5), we let the extra congestion reduce the average speed at peak hours by 10%, which, according to Robertson

[40] Note that exercise B2 assumes the presence of transaction costs; otherwise, it is impossible to generate zero impact at off-peak if we let households return their cars at the original price as a response to the improvement of public transport at off-peak.

[41] This seems to be the case according to the relatively low average speeds (20 km/h) reported in SDG (2005). The latter also predicts that the average speed, including peak and off-peak hours, should fall by approximately 10% between 2005 and 2010.

[42] 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.

et al. (1999), should increase CO emissions by a factor of 1.15. With these corrections, the long-run change in CO concentrations at peak hours is again above 30%.

One of the main insights from these numerical exercises is how little informative the short-run or immediate impact of a policy, whether is HNC or TS, can be. Exercise B6 illustrates this further for TS. A policy that improves the quality of the public transport by 22% has virtually no impact in the short run, just like in B1, but leads to a 15% reduction in car use (and CO) in the long run —consisted with what DICTUC (2009) projected for the "original design" of TS. The limited short-run response can be further illustrated with the aid of Figure 3.2(e), where the dotted line captures a policy shock that reduces Δp^h. The short-run response include only those households in the upper left corner that no longer use the car at peak hours. Instead, the long-run response include the latter households plus the ones that abandon the two-car bundle.

We finish the section with some empirically testable predictions for HNC and TS. Exercise A4 in Table 3.2 considers the effect of the same HNC-like policy of A1-A3 but on a higher-income neighborhood that exhibits higher ex-ante use of the car. We model this assuming that $r = 0.25$ (where r can be interpreted more generally as the price of cars relative to household income), which leads to an ex-ante car use of 70% during peak (q^h_{car}/q^h) and 74% during off peak (q^l_{car}/q^l). The effect of the policy is unsurprisingly small (and negligible compared the city average we find in exercise A3) because these households have already sufficient car capacity to cope with the driving restriction. In turn, exercise A5 looks at the other extreme, that of the effect of the same policy on a lower-income neighborhood with $r = 1.3$ and 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 last two rows of Panel B present the predictions for the effects of TS on households with different income levels. Exercise B7 extends B5 to a high-income neighborhood with $r = 0.1$ and that displays an ex-ante car use of 72% during peak and 81% during off peak. The short run effect is still quite 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 B5. This is simply because households in this neighborhood rarely use public transportation. Exercise B8, on the other hand, extends B5 to a lower-income neighborhood with $r = 1.5$ and that has an ex-ante car use of 8%. Again, the short-run effect is negligible but the long-run effect is substantial, almost 50% above the city average. With these predictions in mind we now turn into the empirical analysis.

4 Policy effects on carbon monoxide (CO)

This section contains our central empirical results. We proceed in three steps. First, we justify the use of CO concentrations as a proxy for car use. Then, we present our empirical strategy and use the average of CO records from all monitoring stations to obtain policy effects at the city level. And third, we use CO records from individual monitoring stations to obtain policy effects at various neighborhoods that differ in terms of household income and car-use intensity.

4.1 Why CO?

It may help asking first what would be the "ideal" data set to study car use in real time. It would have to include information on private and public transportation use by day of the week and hour of the day, on car ownership including quality and associated use, and on household characteristics (e.g., income, size, distance to subway station, etc.). Unfortunately, such information does not exist, so we are forced to look for proxies.

A first potential candidate is hourly records of vehicle traffic from traffic-control stations scattered around the cities. 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).[43] 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 do not distinguish between private and public transportation flows. Fourth, and more importantly, the use of these local information present a number of problems from a theoretical and empirical point 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) produce incentives for car drivers to look for alternative streets (e.g., a station in a clogged street would report virtually no traffic flow) and, 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.[44] We still use this information as it provides some complementary (although qualitative) evidence on the effects of TS on households of varying income levels.

[43] In the case of Santiago, this traffic data is collected and processed by the *Unidad Operativa de Control de Transito* (UOCT) for a total of 46 stations. The only attempt we found in the literature using this kind of data for policy evaluation is de Grange and Troncoso (2011) who look at the effect of (partial and sporadic) driving restrictions in Santiago.

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

The second (and our preferred) proxy for car use is CO records. Given the complexity of transport dynamics in large cities like Mexico-City and Santiago,[45] the use of hourly CO concentration records appears encouraging for several reasons. First, as previously discussed, according to emissions inventories, mobile sources, and light vehicles in particular, are by far the main emitters of CO —97% and 94%, respectively, at the time HNC and TS were implemented.[46] Hence, we should expect any change in city traffic be picked up by changes in CO concentrations. Second, CO is the only pollutant that can be regarded as non-reactive on a time scale of one day (Schmitz, 2005), which is what we use in our empirical estimations. Thus, under stable meteorological conditions, rapid increases in vehicle use (i.e., and in CO emissions) should be immediately reflected in changes in CO concentrations both at the city and at the station level.[47] Third, CO measures, unlike hourly records of vehicle traffic, are better at capturing effects at the scale of the city (or a neighborhood) rather than at a particular location (i.e, street). Fourth, the use of CO emissions also allows us to identify potential increases in pollution due to either more congestion or the use of more-polluting cars. As already seen in the numerical simulations, it may well be the case that modest increases in the stock of vehicles and/or traffic can lead to a much larger increase in CO if the additional stock is dirtier than the existing one.

The use of CO data for policy evaluation is not free of hurdles, however. As explained by Jorquera (2002) for the case of Santiago, there is never a perfect mapping between CO emissions and CO concentrations even after controlling for all the available meteorological variables collected by the monitoring stations such as temperature, humidity, wind speed, and wind direction. This imperfect correlation can be readily seen in Figure 4.1 that plots concentration and emission patterns reported by Schmitz (2005) for a weekly day in the month of January 2002 in Santiago. This imperfect correlation would not be much of a problem if we believe the policy to have a uniform effect on emissions across the day. But that is rarely the case, as both the theory and empirical estimations show. One way to get around this problem is to concentrate on observations at peak-hours (8:00–10:00

[45] At the time of implementation of HNC and TS, the population of Mexico-City and Santiago were about eight and six million, respectively.

[46] 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 passanger 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, 81 and 87% of NO_x emissions, and 36 and 56% of PM10 emissions.

[47] It is worth explaining here that we also disregard nitrogen oxide (NO_x) as a proxy for car use —despite vehicles also contribute largely to it— because, unlike with CO, we failed to see in the data a clear mapping between car use and NO_x concentration *at peak hours*. It was not unusual to find in the data of either city NO_x peaks forming 3 to 4 hours later than traffic peaks. This does not come as a surprise since NO_x is a highly reactive pollutant (Jorquera, 2002).

am for HNC and 7:00-9:00 am for TS) and control for the background pollution that exists before the peak forms. This is because the concentration build-up at peak is quite rapid and during a relatively short period of time of very stable atmospheric conditions (which translates into low dispersion).[48] The increase in concentration at peak should then closely reflect traffic activity at that time both at the city and individual station levels.[49] We adopt these considerations in the empirical estimations that follow.

4.2 Empirical strategy

Our main datasets are time series collection of pollution and weather variables recorded by monitoring stations in each city. In the case of Mexico-City, the network of monitoring stations is operated by the Department of Environment and Natural Resources (www.semarnat.gob.mx). At the time of HNC, this network reported hourly measures of several pollutants, namely, (ground-level) ozone, nitrogen dioxide (NO_2), nitrogen oxide (NO_x), sulfur dioxide (SO_2), and CO, 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 pollution 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.[50]

In the case of Santiago, the network of stations is operated by the National Environmental Commission (www.conama.cl). Each station collects hourly measures of (ground-level) ozone, NO_2, NO_x, SO_2, CO and particulates smaller than 10 and 2.5 micrometers (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 (average failure rate is 9.4% at all times and days) but there are different patterns *before and after* TS. While the overall failure rate decreased from 6.6% to 4.9% at 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 was measured in multiples of

[48] We thank Rainer Schmitz (Geophisycs Department, University of Chile) for long conversations on these issues and for convincing us to concentrate our efforts on TS estimations at peak hours.

[49] In contrast, these same arguments imply that using CO records at off-peak hours from individual stations, as opposed to an average measure, is problematic because, as time passes and winds develop, concentration records at one particular station become "contaminated" by emissions from distant locations.

[50] In the case of Mexico-City there is not much variation across stations in their average pollution levels. Therefore, when we compute the average across stations we do not find significant differences if –instead of computing the values just for all the available stations– we restrict the average to a balanced sample of stations with data available in most periods. As we discuss below this is not the case for Santiago.

0.1145 mg/m3 (with a minimum of 0.1145 mg/m3), after TS it was a continuous variable with a minimum of 0.0001. This measurement change can affect estimations especially at off-peak (weekday) hours and weekends when concentration levels are particularly low. We discuss the implications of this data problem for our empirical estimations below.[51]

Our dependent variable are hourly CO records that depending on the estimation can be either concentration records of an individual station or city-averages which are obtained as the unweighted average of CO records from 15 of the network stations in the case of Mexico-City and 7 in the case of Santiago. We limit the number of stations, as Davis (2008) does, to the ones that were operating during the entire period of our analysis, which is a four-year window symmetrically spaced around the time of policy implementation. Summary statistics of the variables used in the CO estimations are in Tables A.1 and A.2.

One of the challenges for our estimation strategy is related to the inclusion of control variables that can partial out the effect of other phenomena that may affect CO concentrations; in particular, pre-existing trends in pollution and car use. We include both linear trends[52] and different variables that might capture economic determinants on the decision of owning and using a car such as real exchange rates and gasoline prices.[53] Another proxy for economic activity we consider, and that are readily available from the same monitoring stations, are the hourly records of sulfur dioxide (SO_2), a pollutant that is highly tight to industrial activity and energy generation.[54]

A second challenge for our estimation procedure is that even, as we discussed above, after controlling for all the available weather variables, CO concentrations do not perfectly match CO emissions. They do it in a manner that is particular to the geography and climate of each city. It is true that SO_2 records may also work as a control for meteorological phenomena common to all pollutants and that are not entirely captured by the weather records (the variation in weekly-averages we observe in Figures 2.1 and 2.2 is

[51] The percentage of the variance explained by variation of pollution across stations is higher in Santiago than in Mexico-City and this implies that compositional changes at the station level are more important in Santiago, especially at off-peak hours.

[52] We experimented with the inclusion of higher-order trends such as quadratic and cubic polynomials that in general yielded similar results. 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 (2011).

[53] We also experimented with other variables related to unemployment and industial 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.

[54] 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 energy generation and 19% from transportation, with 2% from light vehicles (CONAMA, 2004). We entered the SO_2 recods in the regressions in different forms (i.e, daily, weekly and monthly averages) with similar results.

probably a good indication of these more global meteorological phenomena). Hence, to correct for this imperfect matching we restrict our CO dependent variable to peak hours (8–10 am for HNC and 7–9 am for TS) and simultaneously control for the background level of CO before the peaks form. The latter is computed as the average concentration from CO records at night which is when CO has stabilized (from 2–6 am for HNC and from 1–5 am for TS). In the case of Mexico-City, we also extend this approach to estimate policy effects at off-peak hours (12 am – 3 pm) and Sundays (8–11 am). Since pollution levels at peak can "contaminate" records at of off-peak, we also include pollution levels at peak as background control in off-peak estimations.[55]

We employ two estimation approaches: (i) a flexible polynomial 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 (ii) a more structural fit that includes a linear trend for the adjustment phase (which length is endogenously determined as part of the estimation process) and a dummy for the period that follows the adjustment phase. The estimating equations under the two approaches are given by

$$y_t = \alpha + \phi y_t^b + \beta T_t + \sum \delta_t d_t + \theta t + \gamma x_t + \varepsilon_t \qquad (8)$$

$$y_t = \alpha + \phi y_t^b + [a + b(t - t_T)]A_t + cT_t(1 - A_t) + \theta t + \gamma x_t + \varepsilon_t \qquad (9)$$

where y is (the log of) CO at period (i.e., hour) t, y_t^b is background pollution (pollution at night for peak and weekend estimates and pollution at night and at peak for off-peak estimates), x_t includes fixed effects (hour of the day, day of the week, month of the year), weather variables and economic covariates, T_t is the dummy that takes the value of 1 after the policy, d_t are the monthly dummies for several months after implementation, t_T is the time at which the policy gets implemented, A_t is an indicator function that takes the value of 1 during the adjustment phase (its length is determined through a manual search that stops when the estimated coefficients come sufficiently close to satisfying $a + b(t_A - t_T) = c$, where t_A marks the end of the adjustment phase), and ε_t is the error term.

The effect of the policy under the first approach, i.e., eq. (8), is $\beta + \delta_1$ on impact (i.e., first month) and β in the long-run. On the other hand, the effect of the policy under the more structural approach, i.e., eq. (9), is a on impact (i.e., first day) and c in the long-run; and the (constant) speed of transit from a to c is b. Approach (i) is more flexible but it is likely to introduce too much noise in the estimation as it may capture idiosyncratic shocks (which are very relevant in our pollution dataset). That is

[55] An additional estimation issue relates to the standard errors of the estimates. We follow Davis (2008) and use clustered standard errors to capture serial correlation in CO. In particular, we allow for arbitrary correlation within 5-week clusters.

why we prefer approach (ii) that, following the theory, explicitly imposes a smooth and monotonic adjustment process.[56]

4.3 Results at the city level

We proceed first with estimations at the city level using average concentrations of CO records from the network stations operating in each city. As suggested by our theoretical model, the impact of the policy may differ depending on the hour of the day and day of the week. In Santiago, however, we restrict attention to estimations at peak hours.

4.3.1 Mexico City

Column (1) in Panel A of Table 4.1 presents the results of estimating equation (8) for peak hours (8–10 am) for HNC. We find that HNC decreased CO concentration at peak hours by about 7% within the first month of implementation. While the dummy for a differential effect for month 1 is statistically significant, the total effect is just marginally significant with a p-value of 0.15. As for the long-run, when monthly dummies are zero valued and beyond, we find that HNC has increased CO by about 13%, which is again just marginally significant with a p-value of 0.14. Since the dummies for the first months after implementation are statistically different from 0, we can reject that the effect of HNC during those first months is the same as that over the following months (i.e., long-run). Interestingly, the monthly dummies tend to present a clear pattern of convergence towards 0 which is reached around nine months of implementation. Note that the existence of such an adaptation process is consistent with the result in Eskeland and Feyzioglu (1997) that gasoline consumption increased with respect to the counterfactual in all the periods after HNC except for the first quarter.

As we discussed above, these pollution records can be subject to quite some noise when used in a high frequency format. This would explain the relatively volatile behavior of the monthly dummies which may appear inefficient from an econometric point of view. Thus, in column (1) of Panel B we present the results of adding some structure to the estimation according to equation (9). The effect of the program on impact is now bigger

[56] A regression discontinuity design (RDD) approach appears problematic in the context of our high-frecuency and volatile data. Any idiosyncratic shock that happens at the time of the discountinuity would be confounded with the effect of the policy on impact, whether time is kept at the hour, day or weekly level. Nevertheless, we provide RDD estimates using the optimal bandwidth estimator of Imbens and Kalyanaraman (2009) applied to monthly, weekly and daily data and after controlling for all the above economic and weather variables. Results from daily and weekly data vary widely indeed and without a clear pattern. A more detailed econometric discussion on these issues deserves further research.

and statistically different from 0: a reduction of 11% in the day after implementation.[57] It is not surprising that effect is less than 20% as it captures substitution possibilities, especially from families owning more than one car. In turn, the estimated effects imply an adaptation period of 11.5 months. The estimated effect after this adjustment phase imply an increase of 13% in CO levels, even though the effect is only marginally significant (p-value of 0.119). As we discussed in the numerical simulations, the 24% net difference between long and short-run effects (i.e., after minus immediate impact), which is statistically significant at 1%, can only be explained if agents responded buying not only more cars but also high-emitting ones.

The remaining coefficients in column (1) have all the expected signs, namely, the significant inertia of CO with respect to background pollution, the positive correlation between CO and SO_2, and the negative impact of the real exchange rate. We also find a small negative trend affecting CO concentrations. For brevity, we do not report here the estimates of all weather variables and the hour, day and month fixed effects. We can add however that imposing structure to the estimates seem to be supported by the data, as the standard errors of the different coefficients tend to decrease in comparison to specification (8).

We also present, in column (2), a falsification exercise in which we run SO_2 on the same right hand side variables of column (1) —and we now use the night level of SO_2 as a background control. We want to check whether there are phenomena other than HNC that may be affecting overall pollution in Mexico-City. If this is the case we should find that HNC has similar –at least qualitatively– impacts on SO_2 during the different periods following the introduction of HNC. However, results in column (2) in Panel B —in which for comparability we impose the same structure we use for CO— indicate that HNC has a *positive* effect on impact (which we cannot explain other than it helps illustrate the volatility of this pollution data), a non-significant adjustment process, and no effect in the long-run. These results, while not bullet-proof, are at least reassuring that our estimates for the effect of HNC on CO at peak hours are not capturing omitted variables.

We move now to our results for off-peak hours during weekdays in HNC. Following our "peak estimation" logic that a rapid build-up of concentration is likely due to traffic activity, the window we choose for off-peak estimation (12 am – 3 pm) takes advantage

[57] As a comparison, the estimated effect of HNC on impact at peak hours using the Imbens and Kalyanaraman's (2009) RDD approach for montly observations is -5.7% (with a standard error of 0.012). As we discussed above, however, RDD estimates in the context of a forcing variable that presents a lot of idiosyncratic volatility must be taken with extreme care. For instance, the Imbens-Kalyanaraman RDD estimator of the HNC effect on impact at peak hours raises to -25% (with a standard error of 0.017) when using weekly observations and to -64% (with a standard error of 0.17) when using daily observations.

of an afternoon hump we identify in the CO concentration profile for Mexico-City (unlike for Santiago, see Figure 4.1). Column (3) in Panel A of Table 4.1 contains the results of estimating equation (8). Coefficient values are much in line with those in column (1) and with the model that predicts analogous patterns for both peak and off-peak hours (including the speed of the adjustment process).

Off-peak estimation involves an additional concern, however, that of a potential inertia in CO levels from peak to off-peak hours (recall that a CO emission can remain several hours in the atmosphere or even days under low dispersion conditions). If this is so, our off-peak findings may be mimicking those of peak hours without HNC having a causal impact on off-peak concentrations. We handle this in column (4) by controlling for the pollution level at peak hours of the same day. As expected, the effects of HNC decrease in absolute value and the control for peak hours is statistically and economically significant. These results extend to the bottom half of column (4), Panel B, where we allow for the same background control but under the structure of equation (9). Note that the 17% net difference between long and short-run effects, which is again statistically significant at 1%, is smaller than that for peak hours which helps explain the somewhat faster adjustment process we obtain at off-peak. Finally, column (5) contains results from the same falsification exercise we did for peak hours. As before, HNC does not seem to have an effect on SO_2 concentrations at off-peak that is comparable to what we found for CO.

The last two columns of Table 4.1 presents results for Sundays. Again, the window we choose for the estimation (8 am – 11 am) takes advantage of a morning hump we identify in the CO concentration profile for Mexico-City (and again we failed to identify a comparable hump in Santiago). Looking at Sunday effects is interesting for two reasons: (i) HNC should have no immediate impact since the driving restriction did not operate on weekends and (ii) the increase in the stock of cars to by-pass the weekday restriction should be necessarily reflected in an increase in car use during Sundays. In other words, Sunday results provide both an additional falsification exercise for short run effects, since we should not observe any, and a robustness check for long-run effects, since we have more cars on the street. The results in column (6) are entirely consistent with these observations whether those in Panel A or in Panel B. Among the results in Panel B, it is worth noting how precisely estimated the long-run effect is and how comparable the length of the adaptation process is to those in columns (1) and (4).

We also run a SO_2 falsification exercise for Sundays. The results in column (7) suggest, if anything, the presence of some phenomenon, contemporaneous to HNC, contributing to reduce SO_2 pollution over Sundays. We do not have a good explanation for it; only that it is unrelated to HNC since the adaptation process in column (7) goes the opposite way (and nothing like it is observed during weekdays). In any case, these results illustrates the importance of controlling for SO_2 in our CO estimations and the advantages of a more structural estimation approach that can better handle the inherent volatility of pollution data.

In all, our results show, after a period of adaptation of 10 to 11 months, that HNC has long-lasting impacts on CO and, therefore, on car use. It is interesting that the difference between long- and short-run impacts for peak hours and Sundays is very similar suggesting that the increase in the use of more-polluting cars induced by HNC on periods in which the policy was most binding (peak hours) translates into a similar impact in periods in which it was not (Sundays). Our results also demonstrate that for evaluating policies such as HNC it is important to allow for time varying estimates and to consider heterogeneous effects at different times of the day and of the week.[58]

4.3.2 Santiago

Unlike in Mexico-City, data limitations in Santiago allow us to present credible estimates for peak hours only.[59] Table 4.3 presents CO estimates for two slightly different data sets. Those in column (1) are from a data set in which some of the CO records have been corrected by imputing a value of 0.1145 anytime the observed record at an individual station was below this level. Results in column (2) are based on the original records without any correction for low values. Results in Panel A for these two columns indicate that TS has had virtually no effect on impact (with point of estimates of -0.002 and 0.03, respectively) and a positive and large effect of 0.32 and 0.31, respectively, in the long run. As expected, the correction for low values does not seem to have much of an impact (low values of CO concentration are less relevant in peak estimations except for constructing the background pollution level).

The monthly dummies do present a pattern of increasing effects as time passes, but

[58] If we just include a dummy for the post-HNC —equivalent to dropping all the monthly dummies when estimating specification (8)—, we find the following: zero effects at peak and off-peak hours (with insignificant point estimates equal to -0.075 and 0.046, respectively) and a positive effect on Sundays (equal to 0.088). If instead, we do not divide the sample in peak, off-peak, and Sundays and just run a regression with all the observations from 7 am to 10 pm for the seven days of the week, we find again a zero effect of HNC (with a point estimate of -0.019 and standard error of 0.05).

[59] As discussed before, this is mostly related to (i) the differential pattern of data measurement we observe in Santiago before and after TS and (ii) the high between-station variation in pollution levels across stations (especially at off peak hours when concentrations are very low). We have tried different ways of correcting for missing data and measurement differences and using a panel of stations instead of averages. Our estimates indicate that the effect of TS for a 4 hours window of off-peak hours (12 am – 4 pm) was close to 0 (the point estimate for the long-run effect is 0.02 with standard error of 0.09 and with volatile estimates for the adaptation period). However, these estimates are not robust to changes in the window of estimation (e.g., we get positive impacts for an hour, say 1–2 pm, and negative estimates for the following hour). They are also not robust to some suggestive evidence coming from traffic flow data showing a smaller but still positive effect of TS at off-peak hours (we come back to these traffic results in section 5.3). More generally speaking, this lack of robustness remarks the caution researchers must have when using pollution data.

the pattern is more volatile than the one we find for HNC. This suggests that imposing structure to the estimation procedure should give us more meaningful estimates. Panel B contains the results of estimating equation (9). The estimated coefficients imply that the effect on impact of TS was slightly positive (with insignificant point estimates of 0.05 and 0.06) and that the long-run estimate is positive, with point estimates of 0.31 and 0.34 (and p-values below 0.01 in both cases). Interestingly, the adaptation period, 9 to 10 months, takes basically the same time that in HNC (10 to 11 months). This suggests that our CO estimates are capturing reasonably well the speed of adjustment of households in middle-income big cities as they face unexpected shocks.[60]

All the other determinants of CO in Panel B present the expected signs: Background pollution presents a big, positive, and significant effect (with a coefficient bigger but of the same order of magnitude as in HNC, which is not surprising because peaks are less pronounced in Santiago; see Figure 4.1), gasoline prices and the real exchange rate have a negative impact, and SO_2 and CO levels are positively correlated. Finally, column (3) in Table 4.2. presents the results of the same SO_2 falsification exercise we introduced in HNC.[61] The apparent "effect" of TS on this pollutant follows a pattern that is completely unrelated to the pattern we found for CO (if anything, it confirms the need for imposing some structure to the estimation). This evidence is reassuring that our CO estimates are not capturing the effect of some omitted variable related to overall pollution in Santiago.[62]

4.4 Variation in policy effects within cities

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 cities and CO records from individual monitoring stations distantly located to test whether the response to HNC and TS depends on income (or ex-ante car use[63]) in a way that is consistent with the predictions of the model presented in Section 3.3. Looking at these more disaggregate responses not only constitutes an additional robustness check

[60] That the adaptation pattern happens to be similar with both policy experiences cannot be attributed to some monthly patterns in weather conditions or pollution as TS was implemented in the summer and HNC in the fall.

[61] As a reference, DICTUC (2009) presents simulations in which the expected effect of TS on SO_2 is a decrease of just 0.4% with respect to 2005 levels.

[62] We report here the results of some additional exercises for TS. If we just include a dummy for the post-TS period we find a positive effect of 0.17 (with a standard error of 0.06). On the other hand, the estimated effect of Transantiago on impact at peak hours using the Imbens-Kalyanaraman RDD estimator is -0.02% (with a standard error of 0.03) when using montly observations. As in HNC, the same estimator raises sharply to -55% (with a standard error of 0.10) when using weekly observations and to -27% (with a standard error of 0.45) when using daily observations.

[63] 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 for Mexico City in 1989 and and 0.94 for Santiago in 2006.

of our empirical strategy but it can also reveal important heterogeneities (in costs and benefits) that may prove relevant for policy evaluation. We restrict our estimations to peak hours, as concentration levels at off-peak hours at any individual station are most likely picking up traffic activity from far distant places. For brevity, we only present estimates of equation (9).

Table 4.4 provides a summary with the results of the effects of HNC on CO for 10 monitoring stations in Mexico City.[64] 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 neighborhood (*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 a 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 of the table present estimates of the HNC effects in the short and long run, the difference between the two effects, and the length of the adaptation process. These results are entirely consistent with the predictions of the model in that they indicate that HNC has its largest impact (measured by the LR-SR difference) in middle-income neighborhoods, where households were more likely to buy a second car to by-pass the driving restriction, and lowest in high- and low-income neighborhoods.[65]

Similarly, Table 4.5 provides a summary with results of the effects of TS on CO for 7 stations in Santiago. We have also ordered the stations according to the location and the income level reported in CASEN (2006) for the representative household living in the neighborhood (*municipalidad*) 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 immediately suggests that a household's dependence on public transport varies greatly across the city: from as low

[64] In some of our estimations, the SO_2 control was borrowed from SO_2 records of the closest monitoring station.

[65] It is worth mentioning that in the case of Mexico-City a non-trivial part of the CO emissions are not produced by passenger and commercial vehicles (the ones affected by the policy) but by other vehicles that are part of the public transportation system (e.g., combis). This is evident from the data: while CO levels at peak hours do not vary much from station to station (or county to county), car ownership and income levels do and in a significant way. This may explain why there is a zero effect on impact in the Xalostoc station.

as 2% in the rich Las Condes to 13% in the 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 4.5 present estimates of the TS effects in the short and long run, and the length of the adaptation process. Again, these results are entirely consistent with the predictions of the model. The immediate impact of TS is not different from 0 in all the stations. 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).[66] Effects are big and precisely estimated in all stations (in three of them above 40%), including rich Las Condes.[67] It is also interesting to notice that the length of the adaptation period is for most part decreasing in income, which is consistent with poorer households having a relatively larger expense and less access to credit.

Estimates at the station level for both HNC and TS not only proved to be remarkably consistent with the theoretical predictions but also served to validate the empirical results we obtained for the complete city.

5 Policy effects on other variables

The policy effects on CO we report in the previous section should also be reflected in effects on other variables related to car use and to the substitution between private and public transportation. In this section in particular, we look at the effects on gasoline sales, number of registered cars (stock of vehicles), sales of new cars, and traffic flows. Such an analysis will serve to validate and complement some our CO results —especially because we can take advantage of control groups (regional trends) we did not have in the CO estimations— and to provide support to the numerical exercises of Section 3. Unfortunately, we restrict the empirical analysis of this entire section to TS (for lack of comparable data for HNC where we could apply the same empirical approach) but we still discuss and contrast similar empirical results that are available for HNC. Summary

[66] The only station that somehow deviates from the gradient is El Bosque. One potential explanation is the big expansion of the subway network to neighborhoods nearby that was concurrent with the implementation of TS.

[67] The fact that we find a positive and statistically significant effect even in Las Condes is probably because it is the workplace of many agents living in distant neighborhoods that saw their transportation costs increase after TS.

statistics of variables used in empirical exercises that follow are in Table A.3.

5.1 Gasoline sales

Using publicly available information from Chile's *Superintendencia de Electricidad y Combustibles* (SEC), we construct a panel of monthly gasoline sales at the *region* level and run a differences-in-differences regression of the form

$$y_{it} = \beta T_t \times S_i + \gamma x_{it} + \theta_i + \theta_t + \varepsilon_{it} \tag{10}$$

where y_{it} is the log of the volume of gasoline sales per capita (seasonally adjusted at the regional level using $X-12\ ARIMA$) in region $i = 1, ..., 13$ during month t, T is a dummy that takes the value of 1 for months after TS, S is a dummy that takes the value of 1 for city/region of Santiago, x is a vector of controls that vary by region and time, and θ_i and θ_t are vectors of region and time fixed effects, respectively. The parameter β in (10) captures the differential effect on gasoline sales that we observe in Santiago because of TS, conditional on the other variables included in the regression. The time fixed effects are supposed to capture movements in all the variables that affect symmetrically all the regions and the effects of all the variables that do not vary by region (e.g., general financial conditions or even car prices). We also include as control variables the growth rate of per-capita regional GDP and interactions of the average difference between gasoline and diesel prices interacting with the regional dummies. Thus, the time evolution of all the other regions in Chile serves as a control group for the evolution of Santiago.[68]

We estimate the model for two samples: for the complete period for which we have data (Jan 2002 - Dec 2008) and for the same period we use in the CO estimations, that is, Feb 2005 - Dec 2008. We cluster the standard errors at the region level. Table 5.1 presents the results. Relative to other regions, there is a differential positive increase in gasoline sales per capita in Santiago after TS went into operation: 5.8% for the complete sample and 4.8% for the restricted sample. Both (β) coefficients are statistically significant at the 1% level.

To get a sense of whether these gasoline estimates are consistent with our CO estimates, we run regressions of monthly average CO concentrations at peak hours on gasoline sales and find that a 1% increase in monthly gasoline sales lead to a 4% increase in CO concentrations at peak hours.[69] Hence, a 5% increase in gasoline sales is consistent

[68] The evolution of gasoline sales per capita in Santiago and other Chilean regions is similar before TS was implemented. In most regions there was a secular decrease in gasoline sales before 2007.

[69] Regressions are only for Santiago as we do not have data on pollution for other cities. Results available upon request.

with a 20% increase of CO at peak hours, which is somewhat lower but nevertheless close to our CO estimates.

With respect to estimates of changes in gasoline sales because of HNC, we are not aware of a comparable regional data set of gasoline sales in Mexico that one could use to implement a similar estimation strategy. Eskeland and Feyzioglu (1997) and Davis (2008), however, look at the effect of HNC on gasoline sales using data just for Mexico-City.[70] They find no evidence that HNC reduced gasoline sales; on the contrary, Eskeland and Feyzioglu (1997) find a year-average increase of about 7%. These findings are consistent with our CO results and numerical exercises (A2 and A3) that require a moderate increase in gasoline sales in the long-run to support more circulation during weekends and a similar but less fuel-efficient circulation during weekdays.

5.2 Car registrations and sales

Common sense (and the model) indicates that the only way to support the long-run increases in CO that we find for both HNC and TS is with more cars on the street (beyond any changes in use of the existing fleet). We study here evidence on this for TS by looking at the evolution of three variables: number of registered cars, sales of new cars, and trades of used cars. We are interested not only in estimating the effect of TS on the total number of registered cars (stock) but also in having some idea about the composition of the change. Was it mostly related to sales of new cars or trades of used cars coming either from regions outside Santiago or from (the stock of) car dealers in Santiago?

We work with two datasets. First, data on registered light vehicles obtained from *Instituto Nacional de Estadísticas* is at the annual and regional level and, following the window of the CO estimations, goes from 2005 to 2009. Data on sales and trades, obtained from the *Servicio de Registro Civil de Chile*, is at the monthly and regional level and cover about the same period: a 49 month window centered at February 2007. For our estimation we employ again a differences in differences model along the lines of (10). As done for gasoline sales, monthly observations of sales and trades were seasonally adjusted at the regional level using $X - 12$ $ARIMA$. Unfortunately we do not have control variables —in particular, car prices— that vary at the regional level; though time dummies probably capture the evolution of these terms.[71] Again the time evolution of regions other than Santiago serves as control for the TS treatment. We focus on a

[70] If we estimate our model only using data for Santiago (and controlling for a the relative price of gasoline to diesel and a linear trend), we find a 7.7% increase of gasoline after TS was implemented (with a standard error of 0.015).

[71] Anecdotal evidence suggests there are no big differences in car prices across regions.

four-year window centered at the time TS went into operation.

Results for changes in the number of registered cars are in Table 5.3. Column (1) contains results of a regression that includes time and region fixed effects and column (2) contains results when we add regional trends. Results imply that TS lead to a significant increase in the stock of cars in Santiago between 11.9% (column 1) and 3.8% (column 2). One could think of results in column (2) as a lower-bound of the true effect; with four years of data it is likely that the TS effect may be at least partially captured by the Santiago-specific trend. On the other hand, results in column (1) are probably an upper-bound of the true effect as they do not control for trends that may increase the stock of cars faster in Santiago than in other regions. In all, our conclusion is that the estimated increase in the stock of cars is in the range of estimates consistent with the results of our numerical exercises (see exercises B4 and B5 in Table 3.2) —in fact, we cannot reject in the model of column (1) an increase in the stock equal to a 5.4% with a p-value of 0.18.

Results for changes in trades of used cars and sales of new cars are in Table 5.4. Columns (1) and (4) present average effects when time effects are common to all regions. We find positive effects on both the trade and sale margins. In the trade margin, our estimate implies an increase of about 10% with respect to the (monthly) average trading volume of used vehicles in Santiago in the two years before TS. In turn, the estimate in column (4) implies a sizeable 30% increase in the (monthly) average sales volume of new cars with respect to the previous two years in Santiago.

These results remain, at least qualitatively, under other specifications. In columns (2) and (5) we allow for differentiated time trends by region. Estimates now decrease in magnitude (to 4.4% and 21.2%) and in the case of trades, the TS coefficient is not longer statistically significant. In addition, in columns (3) and (6) we allow for a gradual adaptation to the policy. In the case of used cars (column 3), we find big and statistically significant effects for the first months of implementation, suggesting a quite rapid reallocation of the existing used-car capacity. Unfortunately, we do not have information that could help us disentangle how much of this increase in used-car trading is coming from outside Santiago and how much within Santiago (anecdotal evidence suggest that many car dealers in Santiago run out of their stocks of used cars, including some very old ones). The case of new cars (column 6), on the other hand, shows an interesting pattern in that the month coefficients suggest that agents moved forward their purchase decisions to the first month after TS. Unfortunately, neither we have data on the types of cars agents bought to comment further on this pattern.

In all, our results confirm that TS had significant effects on the markets for used and new cars. Our coefficients imply that about 2/3 of the increase in the stock of cars corresponds to new cars and the remaining 1/3 to used cars (but concentrated within the first year of implementation). Furthermore, the fact that these effects realized rather quickly is entirely consistent with our CO results that show an adaptation period of 10 months or so.

Similar analysis have been carried out for HNC. Using only data for Mexico-City but controlling for flexible trends, Davis (2008) finds a much bigger effect in the number of registered cars (about 20%) but somewhat smaller in the sales of new cars (of about 16%). Eskeland and Feyzioglu (1997) also present evidence supporting the increase of registered cars in Mexico-City. They explain that the increase is mainly driven by imports of used cars from regions outside Mexico-City and much less by the sale of new vehicles. The latter finding, which is also in Davis (2008), is entirely consistent with our numerical exercise A3 that shows that the long-run increase in CO can only be explained by the arrival of dirtier vehicles; although of fewer (3%) than the 20% figure in Davis (2008).[72]

5.3 Traffic flows

Despite the problems identified above, in this section we look at the evolution of traffic flows for a restricted sample of 26 of the 46 traffic stations operated by Santiago's *Unidad Operativa de Control de Transito* (UOCT; www.uoct.cl).[73] Our aim here with this information is more qualitative than anything. We would like first to confirm whether TS hit harder in relatively poor areas, and second, to identify whether effects at peak differ from those at off-peak. We proceed first by aggregating the information coming from individual stations into two groups: high-income stations (i.e., with flows registered in stations located in high-income areas) and low/middle-income stations (ie., with flows registered in stations located in low- and middle-income areas).[74]

The effect of TS on traffic flows is estimated with the following equation

$$y_t = \alpha + \beta T_t + \theta t + \gamma x_t + \varepsilon_t$$

where y is (the log of) total flows during period (i.e., hour) t, T is the TS indicator, x_t is a vector that includes fixed effects (hour of the day, day of the week, month), weather variables, economic covariates, dummies for holidays, dummies for days in which

[72]Note, however, that 3% falls in Davis' (2008) ninety-fifth percentile confidence interval (which is quite wide as the effects are not that precisely estimated).

[73]We limit our analysis to data from the 26 stations that neither suffered from (i) significant shocks that were collinear to the implementation of TS (e.g., one month before TS, a new entry to a main highway near La Dehesa station was open to traffic) nor (ii) unusual traffic flows likely due to the construction or repairing of streets nearby.

[74]We called this group low/middle income areas because, as we discussed in Section 4.1, low-income areas are under-represented in the traffic flow stations. Note also that the aggregation avoid the problems of using specific stations as discussed in section 4.1 and by Daganzo (2007).

(transitory) driving restrictions were in place, and a set of dummies that control for the opening of several urban highways and extensions of the subway network.[75]

We run regressions for a four-year window centered around the time TS was introduced and differentiating for peak (7 – 9 am) and off-peak hours (10 am – 4pm). Unfortunately, station records cannot possibly distinguish between private and public transportation flows.[76] Then, in order to compute changes in private vehicles net of changes in public transportation we do the following. We estimate the percentage decrease in the actual number of buses passing through each traffic station by hour using (i) the number of buses passing through each station before TS, (ii) the actual change in the number of routes passing through each station, and (iii) the estimated decrease in the total number of buses for the whole city. Data for (i) and (ii) comes from Transantiago (www.transantiago.cl). Using data in Briones (2009) and in Muñoz et al. (2009), we compute that the number of buses actually circulating in the city in the first year of TS was, on average, about 27% lower than the pre-TS level. Briones (2009) also argues that due to incentive problems, the effective use of each bus dropped significantly relative to pre-TS levels. Thus, we assume a (probably conservative) reduction in the number of buses actually circulating on the street of 30%. As a robustness check, we also compute changes in bus flows assuming an even more conservative scenario with a reduction of 20%.

Next, to estimate the hourly changes in the number of buses in each station (and therefore in each of the two group of stations) we distribute the change in the total number of buses in proportion to the change in the number of routes that cross over each station. This calculation implies that the drop in total flows due to the reduction in bus flows caused by TS is about 3% in peak hours and 2.1% in off-peak hours.

Table 5.2 presents our estimates for the two group of stations and the two reduction scenarios. While the impact of TS on private traffic in stations located in high-income areas is very close to 0, the impact in low/middle-income areas is clearly bigger. For peak hours the increase in private traffic is 14% (and statistically significant) and for off-peak hours is 10% (but only marginally significant with a p-value of 0.19). This evidence is consistent with our CO results that the effect of TS is bigger in lower income areas. Regarding differentiated effects between peak and off-peak hours, the imprecision of our estimates —coming from the limitations of traffic data— do not allow us to be too conclusive. That the effects are bigger at peak hours in low/middle-income areas may suggest that the relative price of public transportation did increase in both but more in peak than in off-peak. The smaller impact found at off-peak may also explain why we fail to identify CO effects at off-peak hours.

[75] As with the CO regressions, we cluster standard errors in five-week periods.

[76] For this same reason we do not attempt an estimation of the adaptation process since the exact progression in the number of buses after TS is unknown to us.

6 Discussion of results and welfare costs

It is evident from our empirical results that neither of the two policies passes a cost-benefit test. They not only failed to accomplished their main purpose —persuade drivers to give up their cars in favor of public transport— but worse, they induced drivers to buy additional cars (and in many cases more polluting ones). Given that a full-fledge cost-benefit analysis is beyond the scope of the paper, the welfare discussion that follows concentrates on the transport costs that these policies have imposed on households by making them face new sets of transportation options.

6.1 Estimation of transport costs

One of the main objectives of the paper has been to understand the way agents adjust to transport policies. This includes computing how the costs (or benefits) inflicted by these policies on agents evolve over time. Costs are expected to be higher in the short-run when agents have little margin of adjustment and lower in the long-run as the margin of adjustment widens. Based on the large difference between the short- and long-run CO impacts we find for both HNC and TS (24 and 33% at peak hours, respectively), one may argue that despite the fact that these policies did not work as intended, a large fraction of households were nevertheless able to accommodate to them. And if so, the long-run costs associated to these ineffective policies are perhaps not that large.

An estimate of these transport costs can be obtained with the help of the model in Section 3. Given the functional forms adopted in eqs. (1)–(4), welfare costs are obtained directly as the difference between ex-ante and ex-post household's utilities (i.e., agents' willingness to pay to avoid the policies). But before we can compute these costs we must agree on the most likely effects attributable to these policies as described, for example, by some of the exercises in Table 3.1. Based on our CO estimates, the additional evidence discussed in Section 5, as well as results (not shown) from additional runs of the model, we believe that the numbers in exercise A3/B5 capture reasonably well the impacts of HNC/TS.

Consequently, Table 6.1 presents transport costs imposed by HNC and TS based, respectively, on exercises A3 and B5. Cost figures have been normalized by the annual value of the ex-ante existing stock of cars in the corresponding economy, that is, $r\Sigma_i s_i^0$, where s_i^0 is household i's ex-ante vehicle stock. The first row of the table indicates that in the short run HNC made households in Mexico-City bear losses equivalent, on aggregate, to a 3.6% of the (annual) value of the current stock. The short-run figure in the case of TS is even higher, 9.0%. We cannot immediately read from these numbers that TS was 2.5 times costlier for households than HNC because stock values, relative to total surplus, are not the same. One possible correction is to normalize losses in TS by the stock value in Mexico-City. The second row of the table shows that with this correction TS becomes 4 times costlier in the short run than HNC. An alternative correction, which leads to an identical conclusion, is to normalize the ex-ante total surplus in each economy to the same number (by simply adjusting the gross utility v). Either way, it seems that TS has imposed much larger losses than HNC (in the next section we provide additional empirical support for this).

The next two rows in Table 6.1 show that this cost difference extends to the long run. More importantly, it shows that the long-run losses are surprisingly close to the short-run ones. One possible explanation is that only a few households accommodated to the shocks after all. This seems to be the case in both policies. In fact, the model indicates that only 4.3% of the households that own a car before HNC decided to buy a second one and that only 2.8% of all households in Santiago decided to buy a car (or an extra one) because of TS. But this is not the full story. 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.[77] Overall, these numbers indicate that the long-run flexibility does not provide much of a cost alleviation. Consequently, any cost-benefit analysis may well abstract from long-run adjustment considerations.

Given the heterogeneous CO responses we report in Section 4.4, it is unlikely that the transport costs in Table 6.1 are distributed evenly among households of varying incomes. We again use the model to shed some light on this. Table 6.2 reports welfare costs for three groups of households: high-income (as portrayed by exercises A4 and B7 in Table 3.2), middle-income or city-average (numbers are in Table 6.1), and low-income (as portrayed by exercises A5 and B8). Not surprisingly, middle-income households suffer the most in HNC; many of them own a single car but only a few can afford a second one to by-pass the driving restriction. TS, on the other hand, appears fairly regressive with low-income households being hit, on average, 3.4 times as bad as high-income ones.

6.2 Cost difference: Evidence from taxi medallions

One of the striking observations that arise from Table 6.1 is that TS appears much costlier than HNC. This is not obvious given some of our empirical findings, e.g., comparable

[77] Take for instance exercise B1 in Table 3.2. The response is quite large, a 22% increase in the stock; yet the difference between short- and long-run losses is again small: 33.2 vs 31.0%. Note that this small difference also extends to "good" policies. For example, the short-run (transport) gains in B6 amount to 13.2% while the long-run gains to 13.6%.

impacts in CO at peak hours (i.e., similar short vs long run differences) and in vehicle stocks (increases of 3 and 5%, respectively). But this cost difference is not interesting in itself; it would be interesting, in our context at least, only if it provides an opportunity for additional hypothesis testing that may add (or not) to the robustness of our previous empirical and numerical findings.

Since these transport policies 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).[78] 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, unlike other variables like CO records, gasoline sales and car sales, rather instantaneously the long-run effect of the policy.[79] And second, the introduction of both HNC and TS came with no modification in fares nor in the number of licenses,[80] so any change in prices around the time of 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

[78] There were 69000 taxis in Mexico-City (Molina and Molina, 2002), or 1 for every 120 residents, and 27000 in Santiago (INE, 2010), or 1 for every 220 residents.

[79] There are reasons to believe that prices do not adjust instantaneously because agents either learn gradually about the new market conditions or form (temporary) expectations that the policy may be improved or ultimately removed.

[80] Except, obviously, for any rise in ilegal 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).

a novel database of 430 observations of license prices based on weekend's classified advertisements appeared in El Mercurio —Santiago's main newspaper— of taxi licenses, taxicabs and passenger cars for the period January 2004 through November 2010. Since most 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 price of an equivalent passenger car advertised the same day. 370 of our observations were obtained this way (the remaining 60 observations correspond to ads of taxi licenses). We are aware that these 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. Summary statistics are in Table A.3.

The evolution of license prices (from the 60 license ads only) along with the monthly averages from all observations is depicted in Figure 6.1. Prices are quite stable right up to the implementation of TS, which suggests that nobody really anticipated the large impact TS later had; otherwise, prices would have gone up together with the announcement of implementation. This observation is important for all our empirical estimations that are built on the assumption that agents' adjustments only begun once the policies were in place. The figure also show a big and quick increase in prices soon after TS, which provides further evidence of the large impact TS had on forcing people substitute away from public transport towards more expensive means of transportation.

Table 6.3 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.[81]

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.

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

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 taxidriver'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 licence 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 the EOD-2006 for Santiago and the numbers in Table 2.1 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, which portraits quite neatly the much higher cost of using public transport after TS. Furthermore, because taxis are a relatively expensive mode of transportation whatever the city, these findings are also consistent with the idea that TS was much costlier than HNC.

7 Concluding remarks

We have developed in this paper a theoretical and empirical framework to evaluate whether and how different transport policies —driving restrictions and public transport reforms, in particular— can persuade drivers in highly congested and polluted cities to give up their cars in favor of public transport. Because unique in several respects, our empirical analysis has focused on the driving restriction program introduced in Mexico-City in 1989 (HNC) and the public transport reform carried out in Santiago in 2007 (TS). Using hourly concentration records of carbon monoxide (CO), a pollutant directly associated to car use, we found that households' response to both HNC and TS have been remarkably similar but unfortunate: an expected immediate impact —11 and 0% reductions in CO concentrations, respectively— followed by a rapid and significant increase in CO in the long run —13% and 33%, respectively. These latter numbers are the result of more cars on the street that in the case of HNC happen to emit more than the fleet average and in the case of TS add to the existing congestion.

Despite the bad news, there are some valuable policy lessons (not to mention how good a proxy for car use CO proved to be, particularly at peak hours). As illustrated by the theoretical model, the immediate or short-run impact of a policy may say little about its overall effectiveness. Both experiences confirm that policies that may appear effective in the short run can be highly detrimental in the long run; thereby, the importance of understanding whether and the extent to which households adjust their stock of vehicles and how fast in response to these policies. Again, both experiences show that the adjustment process is quite fast, 9 to 11 months.

The magnitude of the adjustment, as measured by the large CO effects, may suggest that a good fraction of households were nevertheless able to accommodate, at a reasonable cost, to policy shocks that did not work as intended. With the help of the model we showed otherwise, not only that a few did but also that the short-run (transport) costs these policies inflicted on households —equivalent to 4 and 9%, respectively, of the value of the existing stock of cars— remain largely unchanged in the long run regardless of income. In this regard, the short run can be quite informative in a cost-benefit analysis. It would be wrong, however, to interpret this limited welfare improvement as an ex-post opportunity to remove ineffective policies and restore welfare; quite the contrary: it will take a long time for the stock of cars to return to its ex-ante level.

Because the speed of adjustment leaves little room for ex-post corrections, the paper also draws attention on the importance of complementing (or replacing) this type of policies with other measures. It is clear that HNC was ineffective in moving people away from their cars, but it less clear how much of that result can be exported to other driving restriction programs that include elements that HNC did not, at least in its early years, such as incentives towards a faster and cleaner fleet turnover. In fact, a few years after implementation, both HNC and the driving restriction program in Santiago (and in other Latin American cities) exempted cleaner vehicles (i.e., with catalytic converters) from the ban. Our theoretical results also confirm how difficult is to persuade drivers, particularly in the short run, to give up their cars with just improvements in public transport, however large they be. More reason then for a serious consideration of market-based instruments such as road pricing that so far has received none in the region.

References

[1] Armstrong, M. and J. Vickers (2010), Competitive nonlinear pricing and bundling, *Review of Economic Studies* 77, 30-60.

[2] Beaton, S., G. Bishop, D. Stedman (1992), Emission characteristics of Mexico City vehicles, *Journal of Air and Waste Management* 42, 1424-1429.

[3] Bravo, D., and C. Martínez (2007), Transantiago y el mercado del trabajo, mimeo, University of Chile.

[4] Briones, I. (2009), Transantiago: Un problema de informacion, *Estudios Publicos* 16, 37-91.

[5] Caballero, R. (1990), Expenditure on durable goods: A case for slow adjustment, *Quarterly Journal of Economics* 105, 727-743.

[6] Caffera, M. (2011), The use of economic instruments for pollution control in Latin America: Lessons for future policy design, *Environment and Development Economics*, forthcoming.

[7] CAM (2001), *Inventario de Emisiones 1998 de la Zona Metropolitana del Valle de México*, Comisión Ambiental Metropolitana (CAM), Mexico.

[8] Cantillo, V., and J.D. Ortuzar (2011), Restricting cars by license plate numbers: An erroneous policy for dealing with transport externalities, mimeo, Universidad del Norte, Colombia.

[9] CASEN (2006), *Encuesta de Caracterización Socioeconómica* 2006, Chile.

[10] Chah, E.Y., V.A. Ramey, and R.M. Starr (1995), Liquidity constraints and intertemporal consumer optimization: Theory and evidence from durable goods, *Journal of Money, Credit and Banking* 27, 272-287.

[11] Chen, Y., and A. Whalley (2011), Green infraestructure: The effects of urban rail transit on air quality, *American Economic Journal: Economic Policy*, forthcoming.

[12] CONAMA (2004), *Plan de Prevención y Descontaminación Atmosférica de la Región Metropolitana*, CONAMA Metropolitana de Santiago, Chile.

[13] Currie, J., and M. Neidell (2005), Air pollution and infant health: What can we learn from California's recent experience?, *Quarterly Journal of Economics* 120, 1003-1030.

[14] Davis, L. (2008), The effect of driving restrictons on air quality in Mexico City, *Journal of Political Economy* 116, 38-81.

[15] de Grange, L., and R. Troncoso (2011), Impacts of vehicle restrictions on urban transport flows: The case of Santiago, Chile, *Transport Policy*, forthcoming.

[16] Dell, M. (2011) "The Persistent Effects of Peru's Mining Mita", *Econometrica*, forthcoming.

[17] DICTUC (2009), *Evaluación Ambiental del Transantiago*, Report prepared for the United Nations Environment Programmme, DICTUC, Santiago.

[18] Duranton, G., and M. Turner (2011), The fundamental law of road congestion: Evidence from US cities, *American Economic Review*, forthcoming.

[19] Eberly, J. (1994), Adjustment of consumers' durables stocks: Evidence from automobile purchases, *Journal of Political Economy* 102, 403-436.

[20] EIU (2010), *Latin America Green City Index: Assesing the Environmental Performance of Latin America's Major Cities*, Economist Intelligence Unit (EIU), Munich.

[21] Eskeland, G., and T. Feyzioglu (1997), Rationing can backfire: The "day without a car" in Mexico City, *World Bank Economic Review* 11, 383-408.

[22] Feng, Y., D. Fullerton, and L. Gan (2005), Vehicle choices, miles driven, and pollution policies, working paper, NBER.

[23] Fullerton, D., and L. Gan (2005), Cost-effective policies to reduce vehicle emissions, *AER Papers and Proceedings* 95, 300-304.

[24] Gallego, F., F Morandé, and R. Soto (2001), El ahorro y el consumo de bienes durables frente al ciclo económico ¿Consumismo, frugalidad, racionalidad? In: F. Morandé and R. Vergara (Eds.), *Análisis Empírico del Ahorro en Chile,* Banco Central de Chile, Santiago, pp. 105-139.

[25] Gómez-Lobo, A., E. Figueroa, P. Jorquera and F. Labrín (2011), Valoración de los efectos medioambientales del Transantiago, mimeo, University of Chile.

[26] Ide, E. and P. Lizana (2011), Transportation policies in Latin America, mimeo, PUC-Chile.

[27] Imbens, G. and K. Kalyanaraman (2009), Optimal bandwidth choice for the Regression Discontinuity Estimator, NBER Working Paper No. 14726, February.

[28] INEGI (1989a), *La Industria Automotriz en México*, Instituto Nacional de Estadística Geográfica e Informática (INEGI), México.

[29] INEGI (1989b), *Encuesta Nacional de Ingreso-Gasto de los Hogares*, Instituto Nacional de Estadística Geográfica e Informática (INEGI), México.

[30] Jorquera, H. (2002), Air quality at Santiago, Chile: A box-modeling approach—I. Carbon monoxide, nitrogen oxides and sulfur dioxide, *Atmospheric Environment* 36, 315-330.

[31] Koh, W. (2003), Control of vehicle ownership and market competition: Theory and Singapore's experience with the vehicle quota system, *Transportation Research A* 37, 749–770.

[32] Lagos, R. (2003), An analysis of the market for taxicab rides in New York City, *International Economic Review* 44, 423-434.

[33] Leape, J. (2006), The London Congestion Charge, *Journal of Economic Perspectives* 20, 157-176.

[34] Litman, T. (2004), Transit price elasticities and cross-elasticities, *Journal of Public Transportation* 7, 35-58 (a 2011 version is available at www.vtpi.org/tranelas.pdf).

[35] McAfee, R.P., J. McMillan, and M. Whinston (1989), Multiproduct monopoly, commodity bundling an correlation of values, *Quarterly Journal of Economics* 104, 371-383.

[36] Molina, L., and M. Molina (2002), Eds., *Air Quality in the Mexico Megacity: An Integrated Assessment*, Kluwer Academic Publishers, Dordrecht.

[37] Muñoz, J.C., J.D. Ortuzar, and A. Gschwender (2009), Transantiago: The fall and rise of a radical public transport intervention, In: W. Saleh and G. Sammer (Eds.), *Travel Demand Management and Road User Pricing: Success, Failure and Feasibility*, Ashgate, Farnham, pp. 151-172.

[38] Robertson, S., H. Ward, G. Marsden, U. Sandberg, and U. Hammerstrom (1998), The effects of speed on noise, vibration and emissions from vehicles, working paper, UCL.

[39] Schmitz, R. (2005), Modelling of air pollution dispersion in Santiago de Chile, *Atmospheric Environment* 39, 2035-2047.

[40] SDG (2005), *Estudio de Tarificación Vial para la Ciudad de Santiago*, Report prepared for the United Nations Development Programmme, Steer-Davies-Gleave (SDG), Santiago.

[41] Small, K. and E. Verhoef (2007), *The Economics of Urban Transportation*, Routledge, New York.

[42] Yañez, M.F, P. Mansilla, and J.D. Ortuzar (2010), The Santiago Panel: Measuring the effects of implementing Transantiago, *Transportation* 37, 125-149.

Table 1.1: Transport policies in Latin America

Program	City	Start year	Type[a]	Scope	In force?
Restricción Vehicular	Santiago	1986	DR	gradual	yes
Hoy No Circula	Mexico D.F.	1989	DR	drastic	yes
Metrobus-Q	Quito	1995	PT	gradual	yes
Operação Rodizio	Sao Paulo	1996	DR	gradual	yes
Pico y Placa	Bogotá	1998	DR	gradual	yes
Transmilenio	Bogotá	2000	PT	gradual	yes
Pico y Placa	Medellín	2005	DR	drastic	yes
Metrobus	México D.F.	2005	PT	gradual	yes
Restricción Vehicular	San José	2005	DR	gradual	yes[b]
Transantiago	Santiago	2007	PT	drastic	yes
Pico y Placa Quito	Quito	2010	DR	drastic	yes

Notes: [a] DR: driving restriction; PT: public transportation reform. [b] The program suffered a temporary interruption in June-July of 2009. Source: Ide and Lizana (2011)

Table 2.1: Travel time before and after TS

Indicator	Before TS	After TS		
		0-6 months	12-18 months	2010
Total number of buses[a]	7,472	5,444	6,396	6,649
% of people waiting at least 10 minutes in 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] *Subsecretaría de Transporte, Ministerio de Transporte y Telecomunicaciones*; [b] DICTUC, several reports; [c] Bravo and Martínez (2007).

Table 3.1: Calibration

Targets	HNC	TS	Parameters	HNC	TS
$s = 0$	0.71	0.62	Δp^h	0.91	0.91
$s = 1$	0.23	0.30	Δp^l	1.01	1.23
$s = 2$	0.06	0.08	t^h	0.95	1.22
q^h_{car}/q^h	0.16	0.31	t^l	0.90	1.20
q^ℓ_{car}/q^ℓ	0.16	0.32	k	0.29	0.40
q^h_{car}/q^ℓ_{car}	0.98	0.85	r	0.98	0.95

Table 3.2: Simulations

Exercise	ΔC^h_{SR}	ΔC^ℓ_{SR}	ΔC^h_{LR}	ΔC^ℓ_{LR}	Δ stock
			Panel A: HNC		
A1	-8.2%	-8.1%	-5.5%	-5.6%	-5.7%
A2	-8.2%	-8.1%	-1.2%	-1.2%	2.8%
A3	-8.2%	-8.1%	12.7%	12.4%	2.8%
A4	-1.1%	-1.1%	3.3%	3.7%	2.0%
A5	-13.5%	-13.7%	3.4%	4.2%	1.6%
			Panel B: TS		
B1	0.0%	0.3%	33.2%	32.2%	21.8%
B2	5.1%	-5.2%	32.9%	0.0%	13.4%
B3	0.0%	0.0%	8.1%	8.1%	5.3%
B4	0.4%	-0.3%	11.2%	5.7%	5.4%
B5	0.4%	-0.3%	31.0%	7.2%	5.4%
B6	-0.6%	0.4%	-15.2%	-8.5%	-7.9%
B7	2.0%	0.0%	13.5%	0.3%	1.6%
B8	0.4%	-0.3%	45.3%	10.2%	9.4%

Table 4.1: HNC effect on CO concentration

	Peak		Off-Peak			Sunday	
y	CO	SO_2	CO	CO	SO_2	CO	SO_2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Fexible approach							
HNC	0.129	0.058	0.126**	0.084*	-0.053	0.143**	-0.203**
	(0.087)	(0.067)	(0.050)	(0.044)	(0.080)	(0.068)	(0.093)
Month 1	-0.199***	0.008	-0.221***	-0.161***	0.046	-0.109	0.077
	(0.067)	(0.049)	(0.046)	(0.040)	(0.069)	(0.074)	(0.079)
Month 2	-0.214***	0.063*	-0.182***	-0.131***	0.036	-0.106**	0.090
	(0.052)	(0.036)	(0.036)	(0.034)	(0.055)	(0.046)	(0.058)
Month 3	-0.151***	0.095***	-0.154***	-0.122***	0.055	-0.074	0.117**
	(0.046)	(0.031)	(0.037)	(0.033)	(0.046)	(0.046)	(0.057)
Month 4	-0.092*	0.075	-0.150***	-0.126***	0.074	-0.059	0.143**
	(0.047)	(0.059)	(0.036)	(0.035)	(0.045)	(0.049)	(0.058)
Month 5	-0.197***	0.066*	-0.153***	-0.086***	0.080	-0.101	0.123**
	(0.049)	(0.035)	(0.031)	(0.029)	(0.062)	(0.079)	(0.058)
Month 6	-0.151***	0.109***	-0.074*	-0.031	0.200***	-0.034	0.116**
	(0.039)	(0.036)	(0.041)	(0.038)	(0.057)	(0.054)	(0.050)
Month 7	-0.245***	0.153***	-0.084***	-0.027	0.176***	-0.098**	0.137*
	(0.037)	(0.035)	(0.028)	(0.027)	(0.051)	(0.041)	(0.068)
Month 8	-0.166***	0.096***	-0.029	0.013	0.095*	0.048	0.116
	(0.039)	(0.035)	(0.030)	(0.030)	(0.049)	(0.044)	(0.079)
Month 9	-0.114***	0.036	-0.016	0.013	0.233***	0.054	0.200***
	(0.040)	(0.033)	(0.034)	(0.031)	(0.045)	(0.042)	(0.050)
Month 10	-0.064	0.074**	-0.090**	-0.070*	0.232***	0.029	0.182***
	(0.044)	(0.037)	(0.044)	(0.038)	(0.041)	(0.051)	(0.057)
Month 11	0.021	0.021	0.017	0.018	0.219***	0.084*	0.152***
	(0.050)	(0.043)	(0.051)	(0.044)	(0.052)	(0.044)	(0.055)
Month 12	0.061	-0.134***	0.075	0.077	-0.072*	0.061	-0.087
	(0.075)	(0.040)	(0.072)	(0.058)	(0.038)	(0.044)	(0.071)
Panel B: Estimation with structure							
Immediate impact	-0.114**	0.092**		-0.064**	-0.003	0.034	-0.111**
	(0.053)	(0.039)		(0.030)	(0.046)	(0.038)	(0.053)
Adaptation trend	3.03e-05***	-6.94e-06		2.29e-05***	-1.86e-05**	2.43e-05	-1.48e-05
	(1.04e-05)	(6.93e-06)		(7.07e-06)	(7.77e-06)	(7.45e-06)	(9.03e-06)
Impact after adaptation	0.132	0.028		0.106**	-0.063	0.196***	-0.166
	(0.083)	(0.063)		(0.041)	(0.074)	(0.048)	(0.106)
Trend	-9.94e-06**	-1.84e-05***		2.48e-06	-1.49e-06	-9.77e-07	3.89e-06
	(4.75e-06)	(4.45e-06)		(2.90e-06)	(4.30e-06)	(3.48e-06)	(5.30e-06)
Real exchange rate	-0.627**	-0.693**		-0.468	-0.049	-0.060	0.695*
	(0.274)	(0.341)		(0.302)	(0.360)	(0.317)	(0.387)
y_{night}	0.322***	0.644**		0.069*	0.413***	0.532***	0.780***
	(0.049)	(0.341)		(0.038)	(0.033)	(0.031)	(0.041)
y_{peak}				0.290***	0.213		
				(0.036)	(0.040)		
SO_2	0.231***			0.287***		0.094**	
	(0.045)			(0.024)		(0.036)	
Months of adaptation	11.5	11.5		10	10	10	10
After - Immediate impact (p-value)	0.000	0.175		0.000	0.269	0.000	0.440

Notes: The dependent variable is the pollution level in logs; for Peak it corresponds to 8 and 9 AM of all week days, for Off -Peak 12-2PM of all week days, and for Sunday 8 to 10 AM. CO is carbon monoxide and SO2 is sulfur dioxide. HNC is a variable equal to 1 after the implementation of the program on November 20, 1989. Months 1 to 12 are indicator variables equal to 1 if the observation belongs to the respective month after the implementation of the program. ynight is the mean concentration of the pollutant y from 2 to 5 AM of the corresponding day; ypeak is the mean concentration of the pollutant y during 8 and 9 AM of the corresponding 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, *p<0.1.

Table 4.2: TS effect on CO concentration

Dependent variable is:	CO		SO_2
	Panel A: Flexible approach		
	(1)	(2)	(3)
TS	0.321***	0.312***	0.009
	(0.075)	(0.078)	(0.074)
Month 1	-0.322***	-0.284**	0.201***
	(0.100)	(0.105)	(0.063)
Month 2	-0.311***	-0.309***	-0.011
	(0.073)	(0.078)	(0.069)
Month 3	0.020	0.021	-0.032
	(0.052)	(0.055)	(0.050)
Month 4	-0.220***	-0.202***	0.049
	(0.053)	(0.055)	(0.054)
Month 5	0.012	0.029	0.009
	(0.064)	(0.064)	(0.044)
Month 6	-0.137	-0.148	-0.094*
	(0.087)	(0.095)	(0.052)
Month 7	-0.032	-0.043	-0.126***
	(0.094)	(0.127)	(0.044)
Month 8	-0.466***	-0.459***	-0.303***
	(0.067)	(0.062)	(0.061)
Month 9	0.087	0.149*	-0.075
	(0.095)	(0.082)	(0.053)
Month 10	-0.022	0.119*	-0.000
	(0.060)	(0.063)	(0.052)
	Panel B: Estimation with structure		
Immediate impact	0.045	0.059	0.131*
	(0.084)	(0.076)	(0.065)
Adaptation trend	3.87e-05*	4.14e-05**	-4.95e-5**
	(2.08e-05)	(1.7e-05)	(1.90e-5)
Impact after adaptation	0.310***	0.339***	0.001
	(0.067)	(0.080)	(0.067)
Trend	1.12e-05***	1.02e-5***	-6.26e-06
	(2.80e-06)	(3.07e-06)	(2.40e-06)
Real exchange rate	-0.290	-0.210	-0.270
	(0.293)	(0.327)	(0.243)
y_{night}	0.419***	0.396***	0.486***
	(0.027)	(0.025)	(0.039)
SO_2	0.517***	0.514***	
	(0.086)	(0.087)	
Months of adaptation	9	9	9
After - Immediate impact (p-value)	0.008	0.003	0.136

Notes: The dependent variable is the pollution level in logs corresponding to 7 and 9 AM of all week days. CO is carbon monoxide and SO2 is sulfur dioxide. TS is a variable equal to 1 after the implementation of the program on February 10, 2007. Months 1 to 10 are indicator variables equal to 1 if the observation belongs to the respective month after the implementation of the program. ynight is the mean concentration of the pollutant y from 1 to 4 AM of the corresponding 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, *p<0.1.

Table 4.3: Policy effects by station: HNC

Station	Sector	Income per HH (relative to average income)	Short-run effect	Long-run effect	Difference LR-SR effects	Months of adaptation
Xalostoc	NE	0.55	0.0736	0.1502*	0.0766	10
			(0.0916)	(0.0884)		
Tlalnepantla	NW	0.50[a]	-0.1646	0.0386	0.2032	9
			(0.1137)	(0.1649)		
I.M. del Petróleo	NW	0.53	-0.1741**	0.1735	0.3476***	12.5
			(0.0647)	(0.1223)		
M. Insurgentes	E	0.70	-0.2257***	0.1596*	0.3853***	14
			(0.0713)	(0.987)		
Lagunilla	E	0.71	-0.2202***	-0.0002	0.2200**	10.5
			(0.0997)	(0.1227)		
Merced	E	0.84	-0.1037	0.1389	0.2426**	11
			(0.0756)	(0.1248)		
Cerro Estrella	SE	0.54	-0.1571*	0.2344**	0.3915***	10.5
			(0.0840)	(0.1607)		
Taqueña	SE	1.14	-0.0999	0.2579**	0.3578***	12.5
			(0.0726)	(0.1243)		
Plateros	SW	1.99	-0.0331	-0.0331	0.0000	0
			(0.0973)	(0.0973)		
Pedregal	SW	1.99	-0.0323	0.1375	0.1706*	11
			(0.0807)	(0.1163)		

Notes: [a] Authors' estimate. Levels of significance are reported as ***p<0.01, **p<0.05,*p<0.1.

Table 4.4: Policy effects by station: TS

Station	Sector	Income per HH (relative to average 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	Months of adaptation
El Bosque	S	0.53	10.8%	-34.6%	-0.1091	0.2678*	11
					(0.1038)	(0.1466)	
Cerro Navia	W	0.54	13.0%	-28.1%	0.0000	0.5131***	11
					(0.000)	(0.1576)	
Pudahuel	W	0.65	11.2%	-26.7%	0.0028	0.4398***	9
					(0.1815)	(0.0910)	
Cerrillos	SW	0.81	10.5%	-29.3%	-0.1068	0.4313***	8
					(0.1707)	(0.1150)	
Independencia	N	0.93	6.2%	-30.2%	0.0233	0.3084***	8
					(0.0997)	(0.0966)	
La Florida	SE	1.06	7.6%	-29.5%	0.0033	0.3079***	9
					(0.0927)	(0.0905)	
Las Condes	NE	2.45	2.2%	-31.9%	-0.0156	0.1759***	8
					(0.0768)	(0.0709)	

Notes: Levels of significance are reported as ***p<0.01, **p<0.05,*p<0.1.

Table 5.1: TS effect on gasoline sales

	(1)	(2)
TS	0.058***	0.048**
	(0.018)	(0.016)
GDP growth	0.054	-0.013
	(0.246)	(0.200)
F-test joint significance Log($P_{\text{Gasoline}}/P_{\text{Diesel}}$)		
× Region Dummies (p-value)	0.00	0.00
Observations	936	611
R^2	0.945	0.957

Notes: The dependent variable is seasonally adjusted per capita monthly gasoline sales. TS is the interaction of a dummy that takes the value of 1 after January 2007 and a dummy for Santiago. The omitted region for heterogeneous interaction effects with the relative price of gasoline is Region 1. Regressions include regional and time fixed effects. Region 1 is the ommited category. Standard errors, in parentheses, are robust to heteroskedasticity. Levels of significance are reported as ***p<0.01, **p<0.05,*p<0.1.

Table 5.2: Registered vehicles

	(1)	(2)
TS	120,068***	38,622***
	(4,376)	(10,507)
Region fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Region-year fixed effects	No	Yes
Observations	52	52

Table 5.3: TS effect on car trades and sales

	Trades			Sales		
	(1)	(2)	(3)	(4)	(5)	(6)
TS	2,406.6***	1,028.5	-272.6	3,078.6***	2,201.2**	2,421.2**
	(503.1)	(1,035.5)	(1,230.7)	(474.0)	(969.5)	(1,037.2)
Month 1			1,889.8***			2,989.1***
			(674.6)			(568.4)
Month 2			2,594.7***			-316.3
			(647.6)			(540.9)
Month 3			1,032.5			-1,644.3***
			(622.3)			(515.8)
Month 4			2,778.7***			-560.5
			(598.8)			(493.3)
Month 5			1,438.1**			-1,212.0**
			(577.4)			(474.0)
Month 6			-702.1			-1,876.9***
			(558.3)			(458.1)
Regional linear trends	No	Yes	Yes	No	Yes	Yes

Notes: Dependent variable is seasonally adjusted monthly data on car transfers (used cars) and car registrations (new cars) in all the Chilean regions. TS is the interaction of ts a dummy that takes the value of 1 after January 2007and a dummy for Santiago. Months 1 to 6 are indicator variables equal to 1 if the observation belongs to Santiago in the respective month after the implementation of the program. All regressions control for region and time fixed effects. Standard errors, in parentheses, are robust to heteroskedasticity and arbitrary correlation within months in sample. Levels of significance are reported as ***p<0.01, **p<0.05, *p<0.1.

Table 5.4: TS Effects on traffic flows

Final	Peak hours	Off-peak hours
	Scenario 1: 30% reduction in bus flows	
High-income stations	0.00	-0.05
	(0.08)	(0.05)
Low/middle-income stations	0.14**	0.05
	(0.07)	(0.04)
	Scenario 2: 20% reduction in bus flows	
High-income stations	0.00	-0.05
	(0.22)	(0.04)
Low/middle-income stations	0.13**	0.05
	(0.06)	(0.03)

Notes: Levels of significance are reported as ***p<0.01, **p<0.05,*p<0.1.

Table 6.1: Transport costs inflicted by HNC and TS

Costs	HNC	TS	ratio TS/HNC
Short-run	3.62%	9.02%	2.5
Short-run (corrected)	3.62%	14.30%	4.0
Long-run	3.54%	8.84%	2.5
Long-run (corrected)	3.54%	14.03%	4.0
Long-run (w/car return)	3.28%	14.03%	4.3

Table 6.2: Transport costs as a function of income

Neighborhood	HNC (SR)	HNC (LR)	TS (SR)	TS (LR)
Low-income	1.52%	1.51%	11.39%	11.30%
Middle-income	3.62%	3.54%	9.02%	8.84%
High-income	2.08%	1.84%	3.38%	3.25%

Table 6.3: TS effect on taxi license prices

	Dependent variable: taxi 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	lic. ads
Observations	430	430	430	430	430	430	430	60
R^2	0.422	0.437	0.466	0.493	0.546	0.719	0.741	0.738

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 equal to 1 after the implementation of TS on February 10, 2007. Total number of licenses is the amount of licenses available in Santiago which are set by the authority. Trends are two linear time-trends diff erent 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, *p<0.1.

Table A.1: Summary statistics for CO estimations in HNC

Series	Obs	Period	Frequency	Mean	Std. Dev.	Min	Max
Carbon Monoxide	33704	Nov 1987 to Nov 1991	Hourly	5.102	2.110	0.644	20.78
Sulfur Dioxide	33794	Nov 1987 to Nov 1991	Hourly	0.052	0.019	0.012	0.254
Temperature	33378	Nov 1987 to Nov 1991	Hourly	15.94	4.786	0.467	30.77
Real Humidity	32773	Nov 1987 to Nov 1991	Hourly	47.92	20.20	2.300	99.60
Wind Speed	33671	Nov 1987 to Nov 1991	Hourly	4.597	2.032	0.400	17.60
Wind Direction	33677	Nov 1987 to Nov 1991	Hourly	173.3	56.03	1.000	420
Precipitation	35088	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 celsius degrees, Humidity in percentage, Wind Speed in kilometers per hour, Wind Direction in azimut degrees, and Real Exchange Rate in Mexican Pesos.

Table A.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 Exchage 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 celsius degrees, Humidity in percentage, Wind Speed in kilometers per hour, Wind Direction in azimut degrees, Precipitation in milimeters, Atmospheric Pressure in milibars, Real Exchange Rate and Gasoline Price in Chilean Pesos.

Table A.3: Variables used for additional analyses in TS

Series	Obs	Period	Frequency	Level of Analysis	Mean	Std. Dev.	Min	Max
Gasoline sales	1,088	Jan2002 to Dec 2008	Monthly	Region	19,166	28,832	1,027	146,875
Car sales	624	Feb 2005 to Feb 2009	Monthly	Region	1,804.6	3,563.0	67.6	18,390.3
Car trades	624	Feb 2005 to Feb 2009	Monthly	Region	3,829.3	6,611.2	189.9	30,263.2
Taxi license price	430	Jan 2004 to Nov 2010	Weekly	Santiago	2,629.0	1,202.5	679.0	5,215.1

Notes: Gasoline sales is measured in litres, taxi license price in US Dollars (US$); Car sales and trades as well as Car Traffic measure the number of cars.

Figure 2.1: CO data for HNC

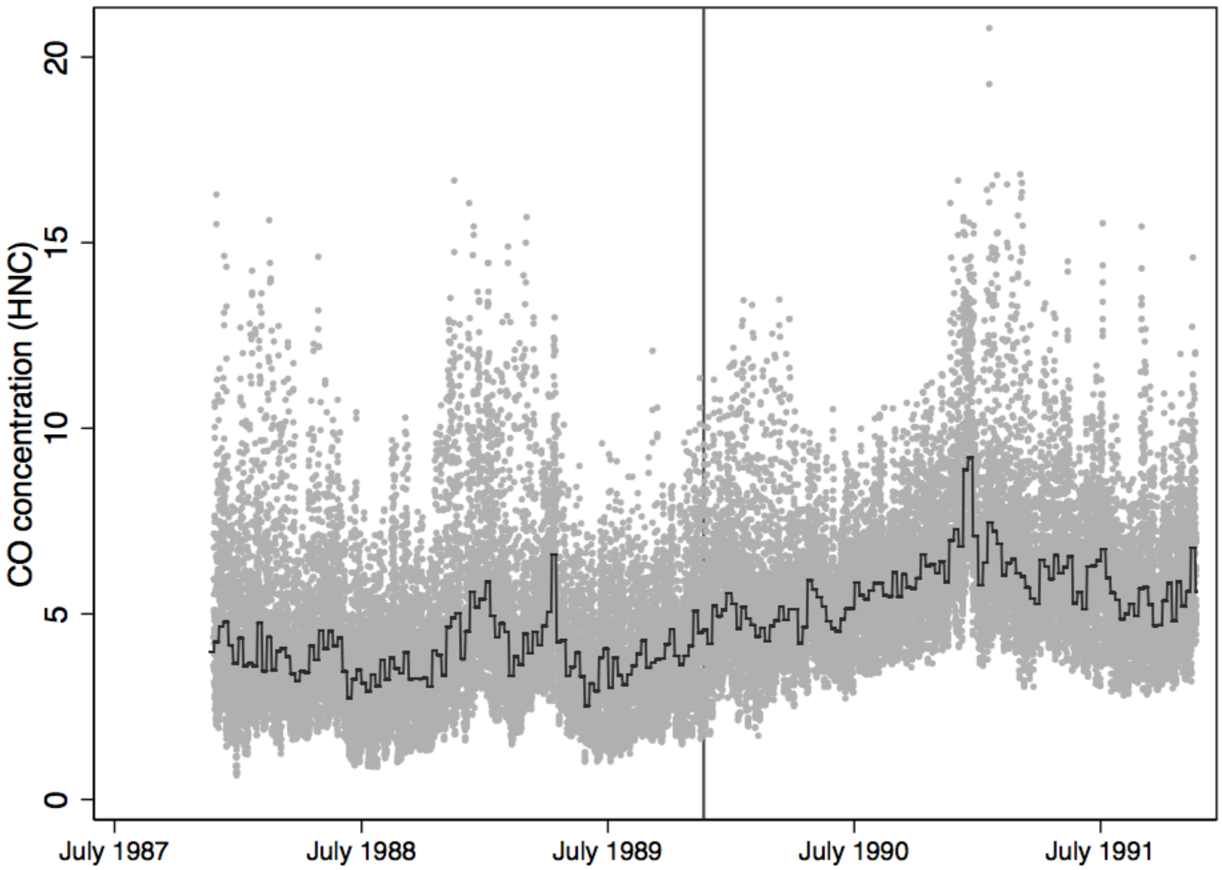

Figure 2.2: CO data for TS

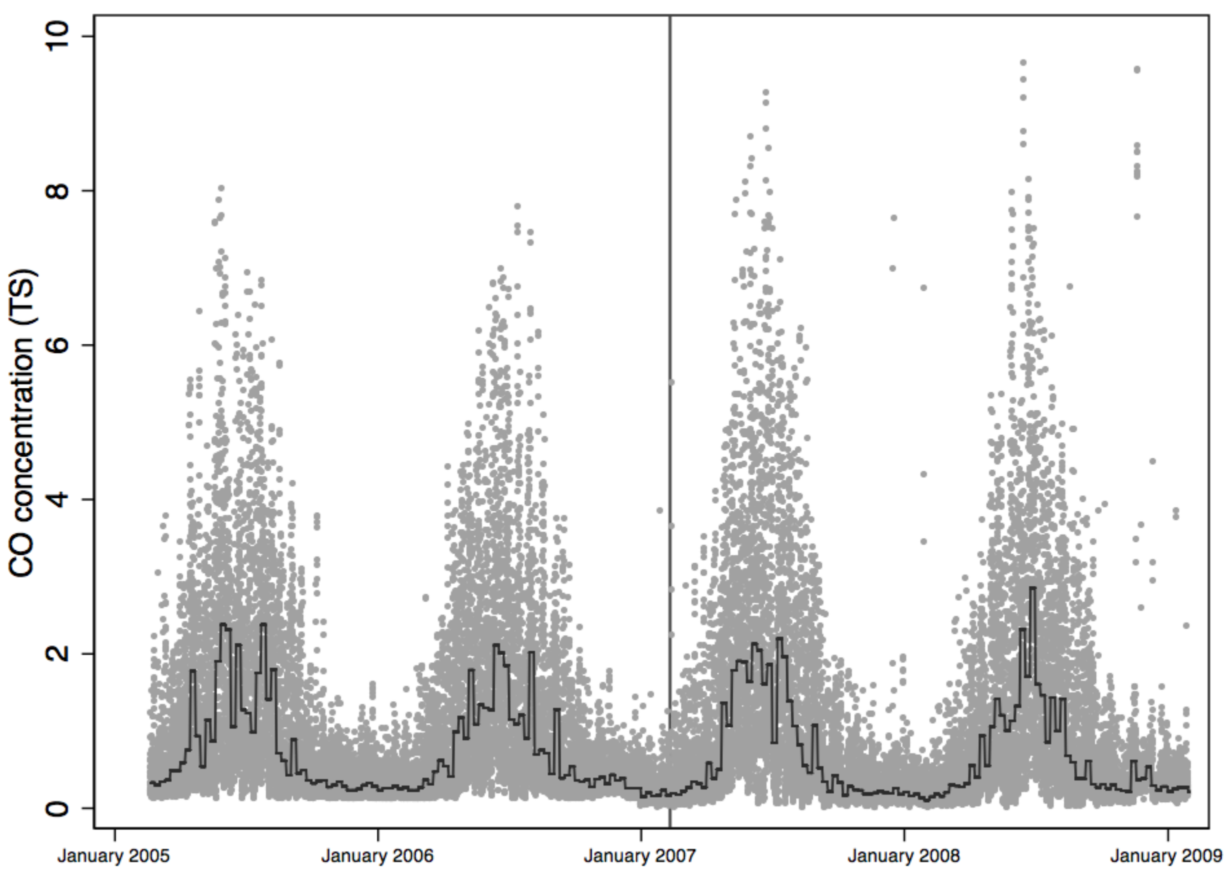

Figure 3.1: Decision to own a vehicle based on vertical preferences

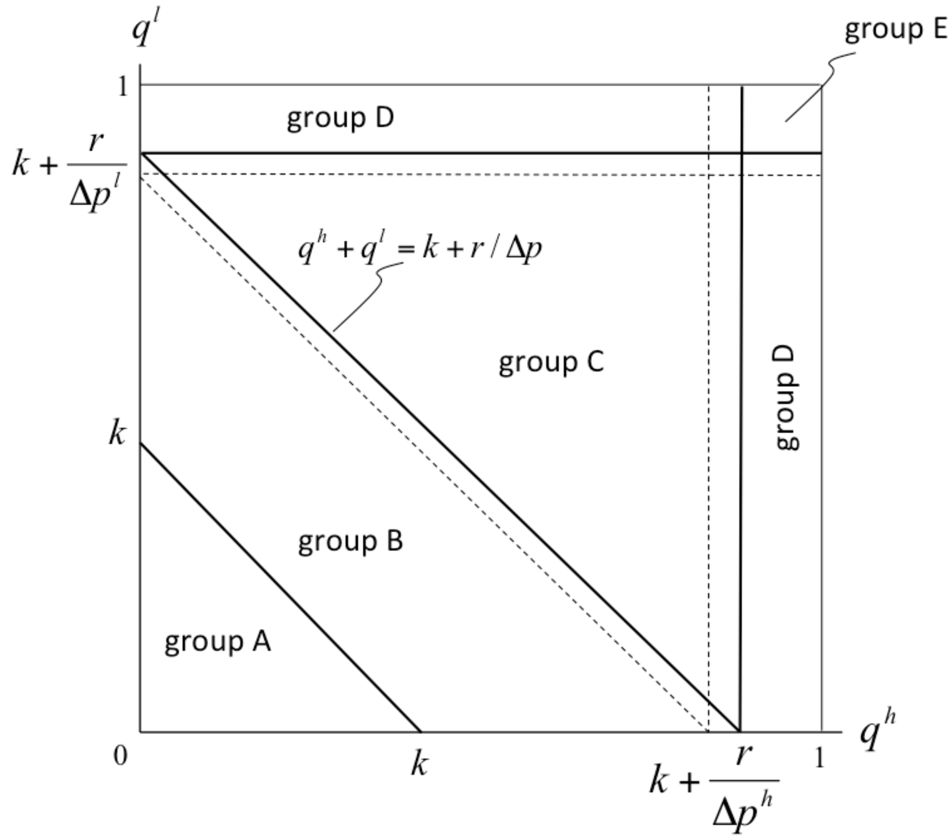

Figure 3.2: (a) Households in group A

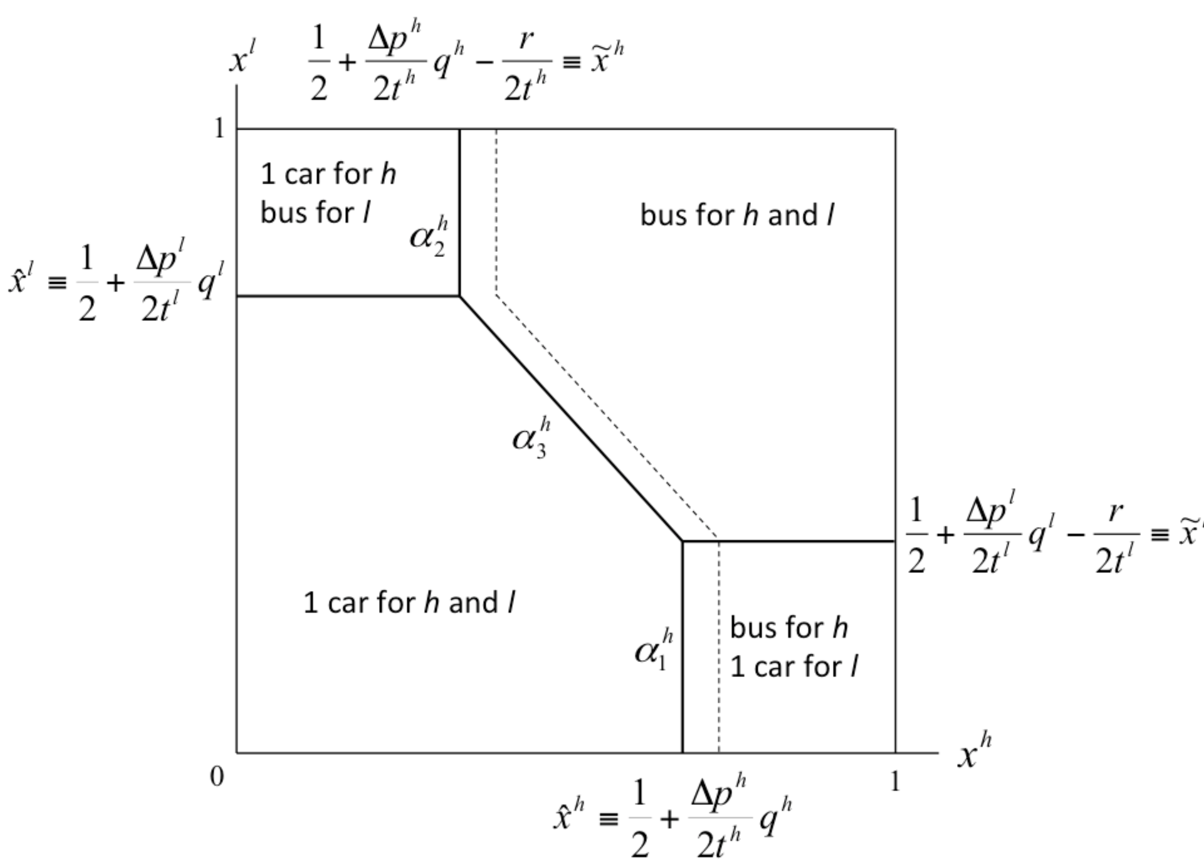

Figure 3.2: (b) Households in group B

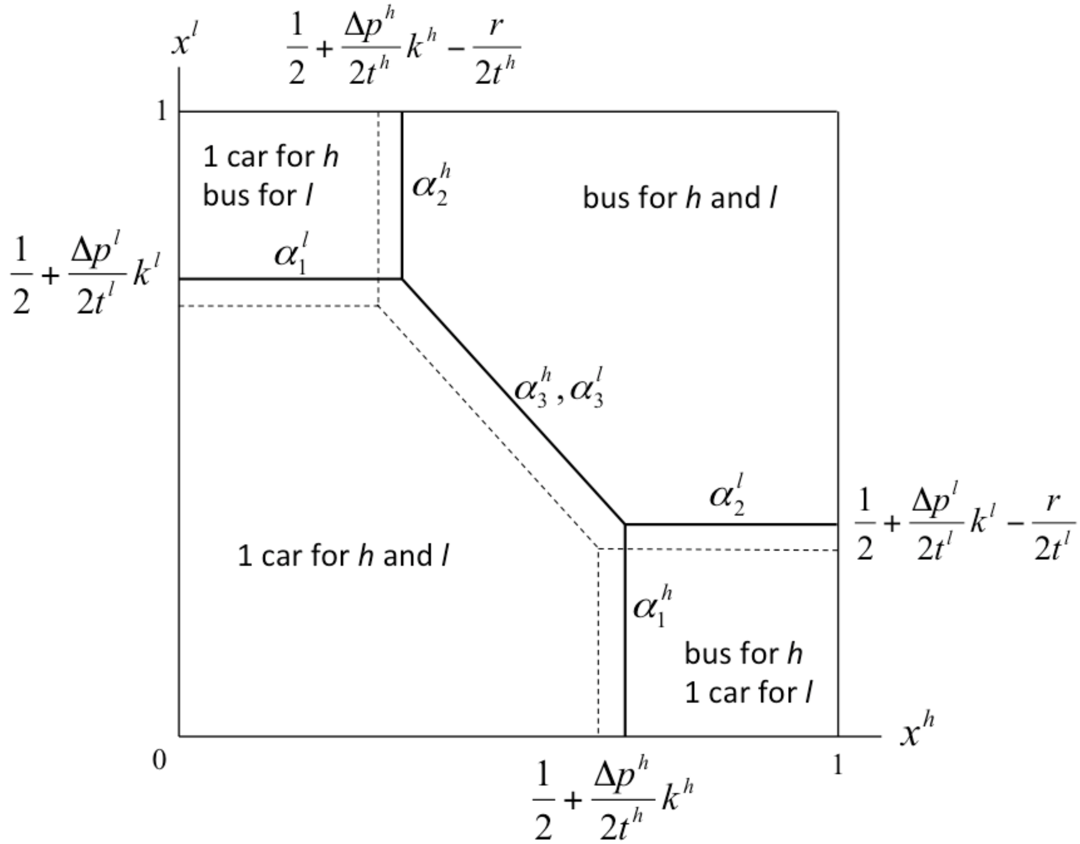

Figure 3.2: (c) Households in group C

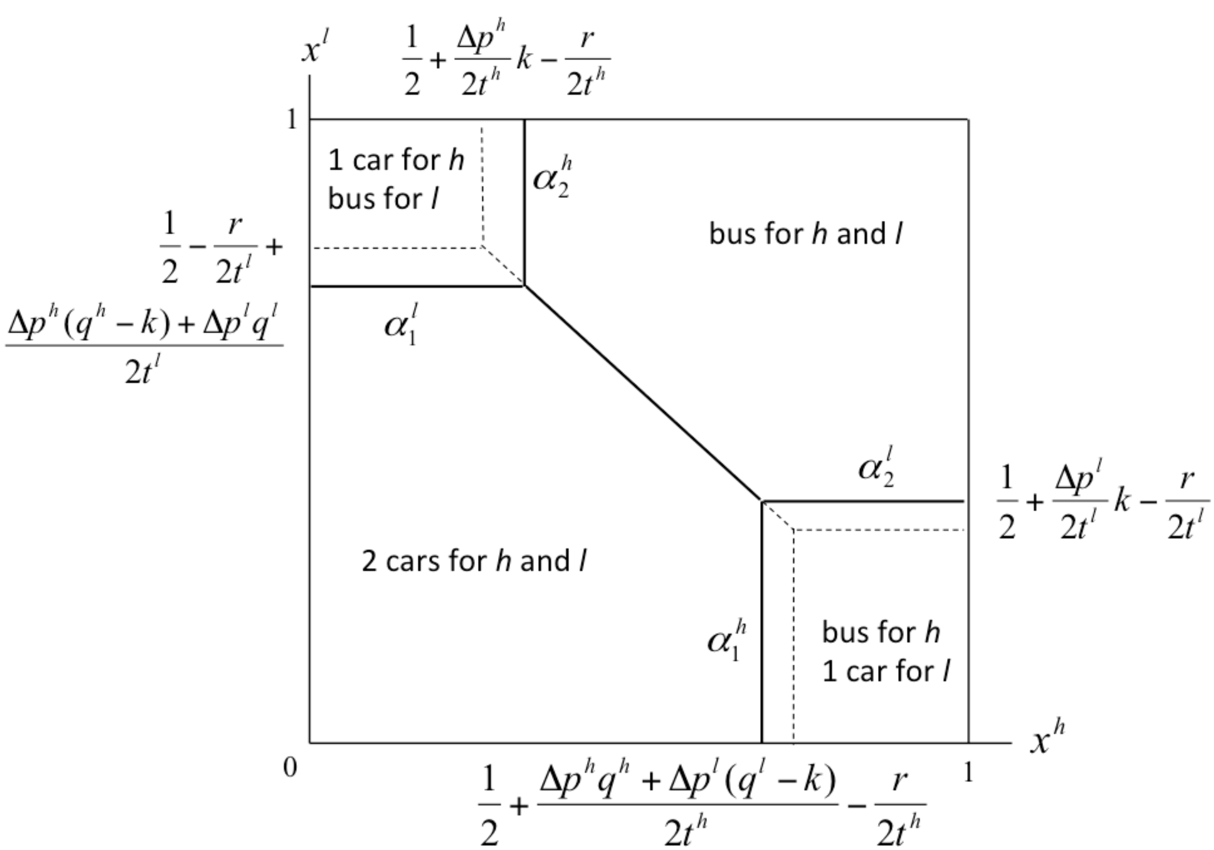

Figure 3.2: (d) Households in group D

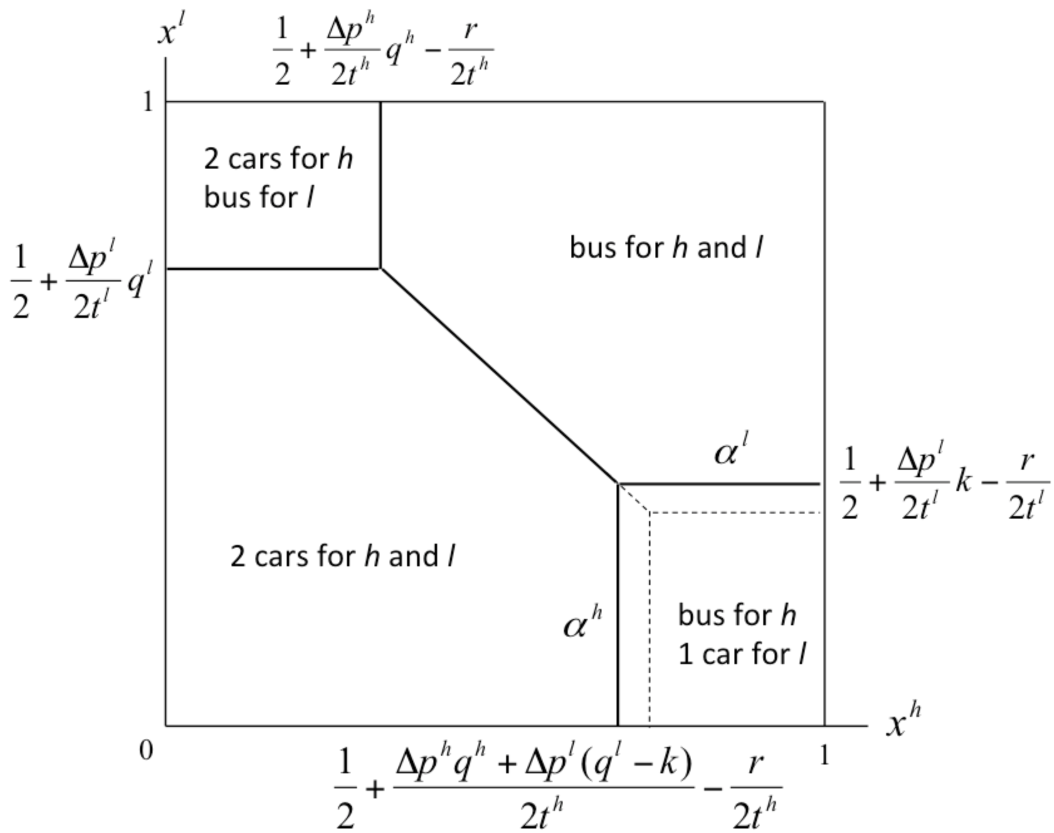

Figure 3.2: (e) Households in group E

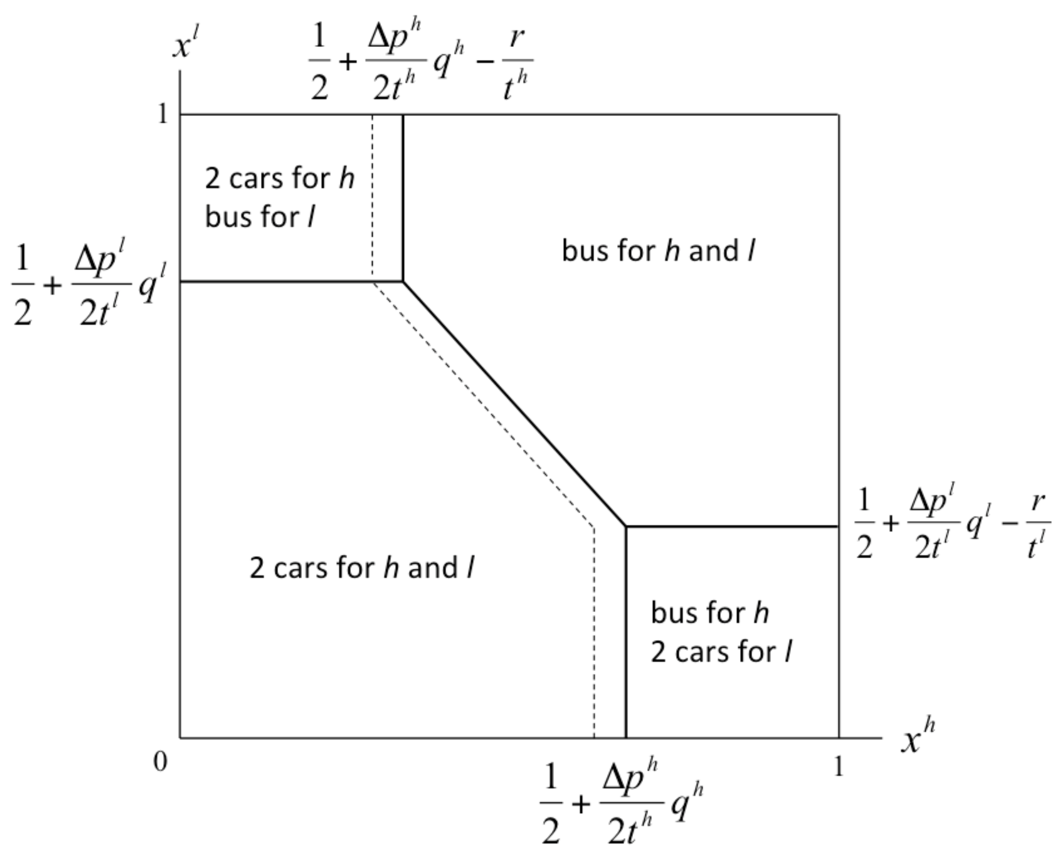

Figure 4.1: CO Emissions and Concentrations in Santiago (January 2002)

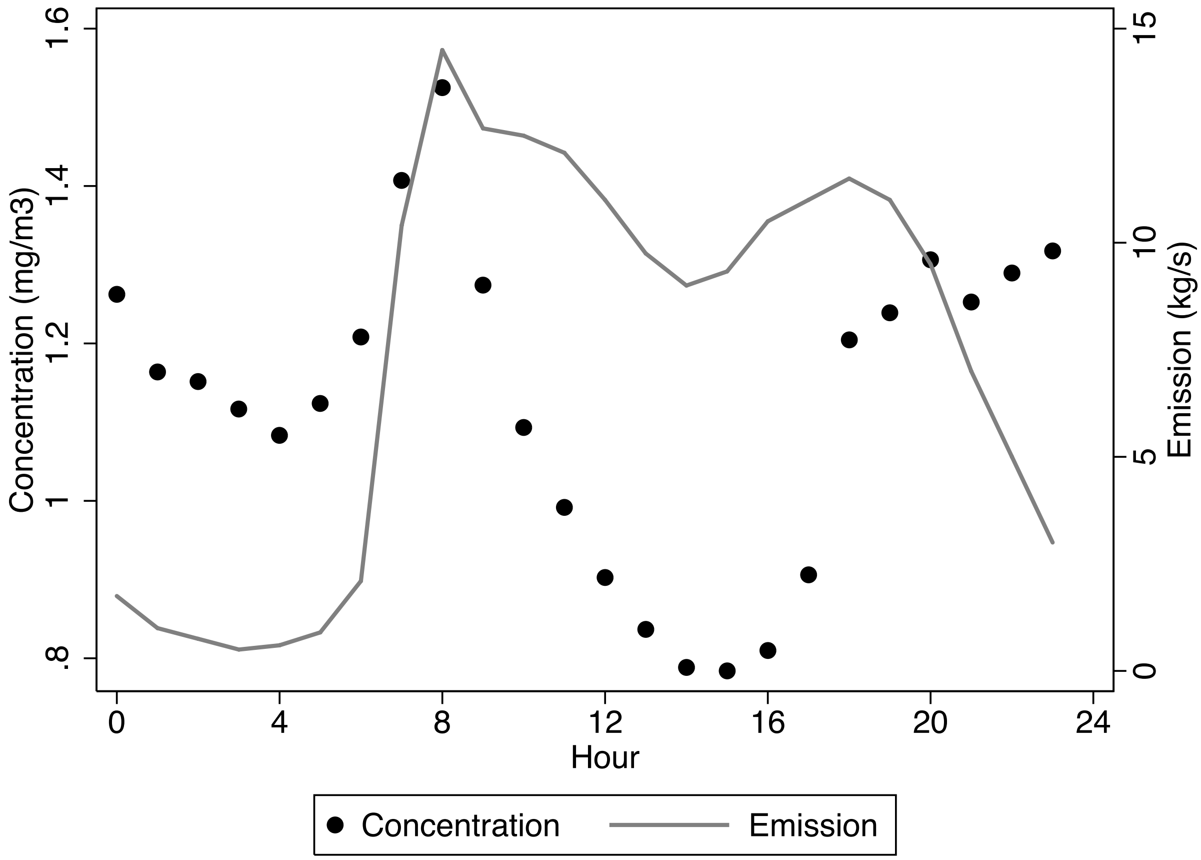

Figure 6.1: Prices of taxi licenses in Santiago (sub-sample of license ads)