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

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Abstract

In an effort to reduce air pollution and congestion, Latin American cities have experimented with different policies to persuade drivers to give up their cars in favor of public transport. 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 ownership and use 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 households' response to both HNC and TS have been remarkably similar and consistent with the above: an expected short-run response followed by a significant and rapid (before 11 months) increase in concentration levels associated to increases 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.

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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).[3]

The model illustrates how uninformative the short-run impact of a policy can be.

[1] Throughout the paper, we understand for short run one month after a policy shock and long-run the time it takes (most) agents to adjust their stock of vehicles as a response to the 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 wel suited to identify effects that are far in time from the policy change. 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] For simplicity, cars refer to private transportation more generally (e.g., passenger cars, motorcycles, etc) and buses to any kind of public transportation including the subway.

[3] Note that only the car-bundle comes with a discount because the same car can be used for both peak and off-peak travel.

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 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.[4] 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).[5] 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

[4] In fact, 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. In fact, the Encuesta Origen-Destino (Origin-Destination Survey) of 2006 for the city of Santiago, EOD-2006, shows that most of the (passenger) cars in the city (799,811) were already in use to cover an equivalent number of morning trips (706,518).

[5] The driving restrictions in Medellín and Quito also appear quite comprehensive (see, for instance, Cantillo and Ortuzar, 2011), but there is no enough data to study them either because the policy is too recent (Quito) or the lack of ex-ante observations (Medellín).

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 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.[6]

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 (such as nitrogen oxides, NO_x). 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.[7] 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 add a vector of variables including fixed effects (ie. dummies for hour of the day, day of the week, and month of the year) , a linear trend, 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.[8]

Empirical results for HNC show statistically significant reductions of CO in the short-run, i.e., during the first month after implementation, of 11% and 8% 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 8% 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%.[9] In all three estimations, the long-run impact of the policy is reached about the same time: between

[6] 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.

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

[8] Davis (2008) explores the effect of HNC on various pollutants and so do Chen and Whalley (2011) 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.

[9] Note that this 20% increase comes close to the 24% net increase of peak hours (from -11% to +13%) and the 16% increase of off-peak hours. These net increases are all statistically significant at 1%.

9 and 11 months after implementation. As for TS, we can only report results for peak hours.[10] We find no impact on CO in the short run 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 short-run impact —sizeable reduction in car use in HNC and almost no change in the case of TS— followed by a significant and rapid increase in the stock of vehicles.[11] Interestingly, the speed at which the stock of vehicles has adjusted in both of these experiences is much 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).[12] These disappointing CO results are consistent with additional evidence coming from other data sources including fuel consumption, car stocks and registrations and prices of taxi licenses (medallions).[13] 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.[14] 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

[10] 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.

[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] Caballero (1990) finds that the (long-run equilibrium) adjustment to a demand/supply shock takes three years while Chah et al. (1995) find that 93% of the adjustment is reached within the first year of the shock. Following Chah et al. (1995), Gallego et al. (2001) find a slightly higher number for Chile, 97%. The speed of adjustment specific to cars that one can infer from these studies should be about the same if, as de Gregorio et al. (1998) explain for the case of Chile, the consumption of durables is mostly composed by cars and, therefore, is closely related to the sales of cars (0.97 correlation).

[13] On this latter, our econometric results and Lagos' (2003) model suggest that the demand for taxicab rides in Santiago have almost doubled because of TS. Note, however, that according to EOD (2006) taxi rides consituted less than 1% of all trips before TS.

[14] 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).

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.

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 process is quite fast —adjustment that for most part is irreversible[15]— 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.[16]

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 and concluding remarks are in Sections 6 and 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.[17]

[15] The presence of transaction and lemon costs (e.g., Eberly, 1994) make it unlikely that household that buy a car to bypass a (bad) policy would return their cars if the policy is removed soon thereafter.

[16] 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.

[17] 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 10am to 9pm 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).

According to government authorities the program was able to revert the increasing trend in CO concentrations despite the raise in the number of vehicles.[18] 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. Interestingly, the volatile behavior of the weekly averages does nothing but confirm the high volatility of these CO records.

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).[19] 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,[20] 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.[21]

While the original design of TS was expected to deliver significant reductions in congestion and pollution from fewer cars on the street,[22] 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).[23] In contrast, travel time of cars and taxis does not seem to have been affected

[18] See Ide and Lizana (2011) and the government references therein.

[19] 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 (Muñoz et al, 2009).

[20] 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 effect on particulates (i.e., 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.

[21] Under the new system passangers are expected to transfer a few times before completing their journeys (Muñoz et al., 2009).

[22] DICTUC (2009) estimates that TS, as conceived by its architects, would have reduced CO concentrations by 15% in the long-run.

[23] The travel-time increase in public transportation is due to both: (i) an increase in the time riding the

nearly as much.[24] 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).[25] 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.[26]

This deterioration in the quality of service should have resulted in a switch towards 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.[27] As in HNC (Figure 2.1), 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 located very nearly zero, 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.

bus(es) and (ii) the time waiting for a bus/subway connection. We do not have waiting-time information for pre-TS, but anecdotal evidence suggests it was much smaller than the average waiting time post-TS.

[24] 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).

[25] 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.

[26] The ADIMARK-GFK monthly quality survey reported an approval rate of the new transport system of 15% in March of 2007, 9% in August of 2007 and 30% in June of 2009. For more visit www.adimark.cl.

[27] 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.

The calibrated model is then used to generate predictions of how households respond to different policies.

3.1 Notation

There is continuum of agents (households) of mass 1 that decide between two modes of transportation —polluting cars and public transport (e.g., buses)[28]— 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 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.[29] 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.[30] 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 $i = h, l$,[31] which simplifies the analysis greatly.

[28] We assume CO emissions from public transport to be invariant to the policy, so we do not need to model them explicitely.

[29] 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 $\theta^i \in [0,1]$.

[30] 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).

[31] 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

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.

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

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

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

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

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

the structure of the model, the household only cares about the change in Δp^i.

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 $i = h, l$.[32]

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

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

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.[33] 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 agents do not adjust their stocks at all (e.g., Eberly, 1994).[34] 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.[35] 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 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

[32] 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.

[33] 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 $q^h = q^l \approx 0$.

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

[35] 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.

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.[36]

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.[37] 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 (and pollution) during peak hours from households in group A equal to

$$\Delta C^h_{SR}(A) \equiv \iint\limits_A \varepsilon q^h \alpha^h_1(q^h, q^l) g(q^h, q^l) dq^l dq^h = \int\limits_0^k \int\limits_0^{k-q^h} \varepsilon q^h \alpha^h_1(\cdot) g(\cdot) dq^l dq^h > 0$$

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

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

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

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

[36] 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.

[37] Since $\Delta p > 0$, reducing product differentiation (i.e., making a bus ride as (un)comfortable as a car ride) will move many people from the bus to the car. Then the two reasons that keep people riding buses are strong preferences towards the bus ($t > 0$ and large x) and the cost of buying a car ($r > 0$).

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.[38] 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.[39] However, if these households face a transaction or lemon cost (see Eberly, 1994) equal to

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

[38] We do not insist any further with illustrations on the effect of a TS-type policy since it follows directly from the analysis in Figure 3.2(a).

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

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.[40] 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).[41] 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 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).[42] 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

[40] 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.

[41] 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 $q^l < k$.

[42] 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 $q^h < k$.

(shortly we will come back to the dotted lines in the figure).[43]

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.[44] In all numerical exercises, we assume that households' preferences are drawn from uniform distributions, i.e., $f(x^h, x^l) = g(q^h, q^l) \equiv 1$.[45] The bottom half of Table 3.1 presents the calibration parameters obtained for each city.[46] 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 ($\Delta C^{i=h,l}_{SR}$) but also in the long run ($\Delta C^{i=h,l}_{LR}$). These numerical figures are quite consistent with our short-run CO estimates (reductions of 13 and 8% 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 of 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

[43] 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.

[44] The ex-ante information for HNC was obtained as follows: car-ownership from INEGI (1989), 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.

[45] We also examine triangular distributions in the calibration exercises with little change in the results.

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

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,[47] the results in ex. A3 illustrate that our empirical estimates are consistent with the theory once we incorporate 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 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.[48] 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 on average 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

[47] 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).

[48] 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.

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.[49] 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;[50] see section 5),[51] and second, we let the extra congestion reduce the average speed at peak hours by 10%,[52] which, according to Robertson 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.[53] 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 (high-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

[49] This seems to be the case according to the relatively low average speeds (20 km/h) reported in Gleave (2005).

[50] 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 20 or more years old.

[51] Following footnote 48, a factor of 1,5 implies that the additional stock is 2,7 years older than the existing one.

[52] Gleave (2005) predicts that the average speed, including peak and off-peak hours, should fall by approximately 10% between 2005 and 2010.

[53] Note that given the fleet turnover and the steady increase in vehicle demand we do not need to resort to the "no transaction-cost" argument to have the 10% drop in the stock of vehicles after 3 years of implementation, which is the time horizon in DICTUC (2009).

(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.[54] 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.[55]

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.[56] 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

[54] The long-run effect is even lower —$\Delta C^h_{LR} = 0.2\%$ and $\Delta C^l_{LR} = 0.3\%$— if we abstract from transaction costs and let the additional stock of cars to be equally polluting than the existing cars.

[55] Many households would like instead to return their cars at the original price. If we allow for that and let the additional stock be equally polluting we obtain for the long run: $\Delta C^h_{LR} = -13.8\%$, $\Delta C^l_{LR} = -13.8\%$, and $\Delta S = -8.1\%$.

[56] Since the additional stock in this richer neighborhood is considerably smaller (1,6%) and possibly cleaner than the city average, we use half the value of the correction factors in B5.

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.[57] There exist a number of problems with this "proxy". To start, we do not have this information for Mexico-City (at the time of HNC, at least). Second, we only have data for a partial count of the total vehicle traffic in Santiago as stations are highly concentrated in the Northeastern part of the city. Third, traffic counts 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.[58] We still use this information as it provides some complementary (although qualitative) evidence on the effects of TS on households of varying income levels.

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,[59] 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.[60] Hence, we should expect any change in city traffic be picked up by changes in CO concentrations. Second, CO is the only pollutant that

[57] In 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.

[58] For instance, 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 the behavior of private and public transportation *at the city level*.

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

[60] 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.

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.[61] 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 (8:00–9:59 am for HNC and 7:00-8:59 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).[62] The increase in concentration at peak should then closely reflect traffic activity at that time both at the city and individual station levels.[63] We adopt these considerations in the empirical estimations that follow.

[61] It is worth explaining here that we also disregard NO_x as a proxy for car use —despite vehicles 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*. For example, the peak in NO_x pollution in Mexico City during the month of December before HNC was implemented was 3 to 4 hours later than the traffic peak; likewise in Santiago for the month of June before TS was implemented. This does not come as a surprise since NO_x is a highly reactive pollutant (Jorquera, 2002).

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

[63] 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.

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), NO_x, sulfur dioxide (SO_2), and CO.[64] For our estimations at the city level we work with an average measure of concentration constructed upon information from 15 of these stations.[65] In addition, some of the network stations also report 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.[66]

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. Here the average measure of concentration is constructed upon information from 7 of the stations.[67] Failure rates are much smaller than in Mexico (average failure rate is 9.4% at all times and days) but there are different patterns *after and before* TS was implemented. 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 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.[68]

[64] Some time after the implementation of the HNC the network also started reporting particulates smaller than 10 and 2.5 micrometers, PM10 and PM2.5 respectively.

[65] We follow Davis (2008) in only including in the sample of stations that were operating in the complete window of analysis.

[66] In the case of Mexico 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.

[67] As in the case of Mexico, we just include stations that were operating in the complete window of analysis.

[68] The percentage of the variance explained by variation of pollution across stations is higher in Santiago

With these data, we first study the hourly average of all stations in both cities for a narrow window of two-years around the policy change. Our aim is to identify both the immediate impact of the policy (i.e., first month under the new policy) and the future process of adaptation to the policy. To do so, we employ two approaches: (i) a flexible polynomial fit with a treatment dummy for the whole policy period and monthly dummies to capture adaptation over the first months of implementation and (ii) a model in which we fit a linear trend for the initial adaptation period (the length of the process was endogenously determined in the estimation process) and a dummy for the second year of the policy. Alternative (i) is more flexible but may introduce too much noise in the estimation as it may also capture idiosyncratic shocks (which are very relevant in our pollution dataset) and, therefore, we prefer approach (ii) that assumes a smooth process of adaptation to the policy with the length of the period of adaptation being endogenously determined in the estimation.[69]

One of the most difficult problems with our strategy is related to the inclusion of control variables to partial out the effect of other phenomena that may affect carbon monoxide concentrations, in particular, pre-existing trends in pollution and car use. To do so we include linear trends[70] and controls for variables that capture economic determinants that may affect the decision of owning and using a car. The only proxy for economic activity we have in an hourly frequency is the average of sulfur dioxide (SO_2) in the same stations for which we compute carbon monoxide. As the pollutants inventory for both cities shows most of the sources of emissions of SO_2 are related to industry.[71] In all the empirical exercises related to carbon monoxide, we include additional economic

than in Mexico and this implies that a compositional changes at the station level are more important in the case of Santiago than in the case of Mexico, especially at off-peak hours.

[69] Applying a regression discontinuity design (RDD) approach appears problematic in the context of our dataset. The forcing variable is time and the discontinuity happens at the moment of policy implementation. The problem with applying RDD to high-frequency and very volatile data (as we have here) is that any idiosyncratic shock that happens at the same time (hour, day, week) would be confounded with the effect of the policy on impact. Nevertheless, we also mention RDD estimates using the optimal bandwidth estimator suggested by Imbens and Kalyanaraman (2009) applied to monthly data after controlling for all the other economic and climate variables we include in the regressions. In fact, RDD estimates vary significantly (and without a clear pattern) if instead of using monthly data, we use daily and weekly data. We leave a more detailed econometric discussion on this for future research.

[70] We experimented with the inclusion of higher-order trends such as quadratic and cubic polynomials that in general yielded similar results. When using higher order trends the problem is that there may be over-fitting in the sense that with a sufficiently high-order polynominal of time we may fit the complete evolution of the dependent variable (see a discussion of this applied to the RDD context in Dell, 2011).

[71] In the case of Mexico about 79% of the SO_2 emissions were produced by sectors different then transportation before HNC (mostly industry and energy generation), and among transportation, private cars and taxis just explain 1.7% of emissions with the most important contributor being trucks (Lezama, 1997). In the case of Santiago, about 92% of emissions were related to industry before TS and about 4.3% to transportation, with cars just explaining about 1.8% of SO_2 emissions.

variables such as the real exchange rate and gasoline prices.[72] We collect these variables from several official sources in each country.

A second relevant challenge for our estimation procedure is that, as discussed above, even after controlling for climate variables, the stock of carbon monoxide in the atmosphere interact in a very complicated manner with climate variables given the geography and climate of each city. Thus, we introduce the level of background carbon monoxide as control. This variable is computed in each city during the small hours of each day, specifically, at the time in which the stock of carbon monoxide stabilizes (in particular, from 1 to 4 am in the case of Santiago and from 2 to 5 am in the case of Mexico-City).[73] We face a similar problem when we run regressions for CO concentrations levels at *off-peak hours*. In this case, the pollution levels at peak hours can contaminate the estimates of off-peak hours and, consequently, we also include pollution levels at peak hours as a control for background pollution.[74]

In all, our main estimating equations for carbon monoxide are

$$y_t = \alpha + \phi y_t^b + \beta T_t + \sum \delta_t d_t + \theta t + \gamma x_t + \varepsilon_t \tag{8}$$

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

where y is (the log of) CO at period (i.e., hour) t, y_t^b is background pollution (simply pollution at night for estimates at peak hours and pollution at night and at peak hours for estimates at off-peak hours), x_t includes fixed effects (hour of the day, day of the week, month), climate characteristics, and economic covariates. Regarding the estimates of the policies, in our first approach (equation 8), we simply use T_t as a dummy that take a value of one after the policy was implemented and d_t are dummies for the first months of implementation.

The effect of the policy on impact (i.e., first month) is $\beta + \delta_1$ and the effect of the policy in the long run, i.e., after the adjustment period, is β. In our second approach (equation 9) the effect of the policy is estimated in a more structural way. In this case A_t is an indicator function that takes the value of one during the adjustment period (which is endogenous), and t_T is the time of policy implementation. Thus, the effect of the policy on impact is a, on the long-run is c, and the (constant) speed of the transit from a to c

[72] We experimented with also adding other variables related to unemployment and production but they were not typically significant or did not present the expected signs. Our sense is that after including linear trends and the other monthly variables these economic variables are redundant.

[73] Specifically, we define the time interval at night as the period in which CO concentration levels stabilize in the time period before the programs were implemented.

[74] 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 carbon monoxide. In particular, we allow for arbitrary correlation within 5-week clusters.

is b.

4.3 Results at the city level

4.3.1 Results for Mexico-City

Table 4.1 presents the results of estimating equation (8) for the case of HNC for peak hours, weekday off-peak hours, and Sundays. As suggested by our theoretical model the impact of a driving ban may differently affect these three margins and therefore we estimate different regressions. We start with the effects of HNC on pollution concentrations at peak hours in column (1) of Panel A. We present the results of our baseline estimation equation (8). We find that HNC decreased carbon monoxide pollution by about 7% during the first month of its 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. Regarding the estimates of the effect of the policy over the second year of the implementation, we find that there is an increase of carbon monoxide of about 13%, which is again just marginally significant with a p-value of 0.14. However, note that the fact that the dummies for the first months of implementation of the program are statistically different from 0 implies that we can reject that the effect of HNC is the same during the first months of implementation and during the second year of implementation. Interestingly, the monthly dummies for implementation tend to present a pattern of convergence towards 0 specially after eight months that the program was implemented.

As we discussed above, pollution data is subject to a lot of noise when using high frequency data, as we do, and then the relatively volatile behavior of the monthly dummies may be a consequence of this noise and then the flexible use of monthly dummies may be inefficient from an econometric point of view. Thus, in column 1 of Panel B of Table 4.1 and Figure 4.2 we present the results of adding structure to the estimation produce accordingly to equation (9). The effect of the program on impact is now bigger and statistically different from 0 and implies a reduction of pollution by about 11% in the first month in which the program was implemented.[75] It is interesting to notice that the size of the effect is reasonable as it probably captures behavioral responses taking place slowly–except for the case of families owning more than one car and having spare capacity.

[75] As a comparison the estimated effect of HNC on impact using the Imbens and Kalyanaraman (2009) RDD approach is -5.7% (with a standard error of 0.012). However, note that, as we discussed above, RDD estimates in the context of a forcing variable that presents a lot of idiosyncratic volatility have to be looked carefully. For instance, if we instead estimate using (i) weekly data the Imbens-Kalyanaraman RDD estimator of the HNC effect on impact is -25% (with a standard error of 0.017) and (ii) dayly data the Imbens-Kalyanaraman RDD estimator of the HNC effect on impact is -64% (with a standard error of 0.17).

In turn, the estimated effects imply the adaptation period ends around 11 months after the program was implemented. The estimated effect after the adaptation period 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 simulations of the model, this positive estimate can only be explained if the agents bought new cars that are much more polluting than the stock of existing cars.

Regarding the effects of other determinants of CO, results in general are as expected. There is a lot of inertia of CO with the background level of pollution presenting a point estimate of 0.33. In addition, CO and SO_2 are positively correlated, the real exchange rate has a negative impact on pollution, and there is a small negative trend affecting this pollutant. For brevity, we do not report the estimates of all climate variables and the hour, day of the week, and month of the year fixed effects.

In column (2) we present a falsification exercise in which we run SO_2 on the same right hand side variables as in Column (1)–and we now use the night level of SO_2 as a control. We want to check whether there are other relevant phenomena different than HNC that may affect overall pollution levels in Mexico. If this is the case we should find that HNC has similar –at least qualitatively–impacts during the first months of implementation, over the adaptation period, and during the second year of operation of HNC on SO_2. However, results in column (2) in Panel B –in which for comparability we impose the same structure we use for CO–imply that HNC has a *positive* effect on impact (which is hard to explain and probably just captures a temporary shock to SO_2), no effect over the second year after the program was implemented, and a non-significant adaptation process. Thus, these results, while not bullet-proof, are at least reassuring that our estimates for the effect of HNC on carbon monoxide over peak hours are not capturing omitted variables.

Now we move to our estimates for off peak hours during weekdays. In column (3) of Panel A of Table 4.1, we start with the same specification as in column (1). In this case, we find that HNC decreased carbon monoxide pollution by about 9.5% during the first month of its implementation–with the effect being statistically significant even at a 1% level. The estimate for the effect of the policy over the second year of the implementation is positive and equal to 13% (with a p-value of 0.015). There is an adaptation process with the vector of dummies converging towards 0 as in the case of peak hours.

In this case, however, we face the challenge of potential existence of inertia in CO levels from peak to off-peak hours. If this is the case our findings for off-peak hours may be mimicking those for peak hours without HNC having a causal impact on off-peak carbon monoxide concentration levels. To control for this, in column (4) we introduce a control for 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. Now, there is a decrease of about 8% during the first month of the implementation of HNC and the effect over the second year of implementation of the program decreases to about 8%, and it is not as precisely estimated as before (the p-value is 0.066).

Column (4) in Panel B presents estimates for the adaptation period imposing the structure of equation (9)). In this case our results imply that the effect of HNC on impact is equivalent to about -6.4% (statistically significant with a p-value of 0.038). In turn, the effect after the period of adaptation –which takes 10 months–is 10.6% (statistically significant with a p-value of 0.015). In Columns (5) we present estimates in which we run a falsification exercise with SO_2 as the dependent variable. Results imply that HNC does not seem to have an effect on SO_2 that is comparable to what we found for carbon monoxide.

Finally, we study the effect of HNC on carbon monoxide during Sundays. The motivation to implement this exercise is that it captures a set-up in which (i) the policy should have had no effect on impact as the driving restrictions did not operate during weekends and (ii) as time passes the increase in the stock of cars to by-pass the restriction on weekdays could be allocated (at least partially) to car use on Sundays. Thus, the impact effects of HNC on Sundays gives us a sort of falsification exercises for the short run effects of HNC, as we should not find any significant effect on impact because HNC did not decrease car capacity during weekends (and the stock of cars was fixed in the short-run). In turn, the long-run impact of HNC on Sundays gives us a check for our results for weekdays, as agents should have used at least partially the additional capacity that comes with the increase of car use over weekdays.

In column (6) we present our basic estimates of estimation (8) for the effect of HNC on Sundays. In this case, as expected, HNC has a zero effect on impact (the point estimate is 3.4% with a p-value of 0.65) and presents a positive effect of about 14% over the second year of implementation of HNC (with a p-value of 0.04). When we add structure to the estimation using specification (9) in column (8) of Panel B of Table 4.2, the estimated effect of HNC on impact is still insignificant and the effect after when the adaptation period ends–10 months– is equal to about 19.6%. As before, imposing structure to the estimates seem to be supported by the data, as the standard errors tend to decrease in comparison to specification (8).

Lastly, in column (7) we run a similar regression using SO_2 as the dependent variable and find that this pollutant decreases both on impact and during the second year of implementation of HNC. This suggests that, if anything, after the implementation of HNC there were other phenomena decreasing overall pollution over Sundays and thus it

is important to control for SO_2 in our baseline specifications.[76] Finally, also notice that, as in the previous cases, the adaptation trend follows a different pattern than in the case of CO.

In all, our estimates imply that HNC has a negative effect on impact for carbon monoxide both at peak and off-peak hours. In contrast, the effect of HNC on impact during Sundays was close to 0, as expected. We show that after a period of adaptation of between 10 and 11 months, HNC has a positive impact on carbon monoxide pollution and, therefore, on car use. Interestingly, in addition, the difference between the long-run and short-run impacts for peak hours and Sundays is very similar suggesting that the increase in the use of dirtier cars induced by HNC on the periods in which the policy was more binding (peak hours over weekdays) translates into a similar impact in periods in which the policy was not binding (Sundays).

We find that when estimating the impact of policies such as HNC it is important to allow for time varying estimates and also to consider heterogeneous effects at different times of the day and of the week, as suggested by our theoretical model.[77] The result of the existence of 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 was implemented except for the first quarter after of implementation.

These results are consistent with the fact that HNC effectively constrained only week days and hence had a strong immediate effect should be seen in peak hours and no effect during weekends. Additionally, the results are consistent with the hypothesis that households in the margin will tend to buy cars to try to return to their pre-policy use in peak hours, behavior that will leave households with additional cars available during weekends, and eventually off-peak hours, to use and hence increasing pollution during such hours. Off-peak results mix both peak and weekend behaviors since the policy did constrain the use of cars during these hours yet the needs of mobilization are more similar to the weekend hours, hence the decrease in the short-run and the increase in the long-run.

[76] It is unclear why we observe this decrease in SO_2 after HNC was implemented only for Sundays and not for weekdays. One potential explanation is that this is capturing the restriction to public transportation operating since 1991 over weekends.

[77] 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 for peak and off-peak hours (with insignificant point estimates equal to -0.075 and 0.046, respectively) and a positive effect for Sundays (equal to 0.088). If we instead do not divide the sample in peak, off-peak, and Sundays and just run a regressions for all the observations from 7am to 10pm in all the days of the week, we find again a zero effect of HNC (with a point estimate of -0.019 with a standard error of 0.05).

4.3.2 Results for Santiago

Now we move to the estimates of TS. In this case, data limitations do not allow us to present credible estimates for off peak hours and Sundays.[78] Thus, we focus on estimates for peak hours. Table 4.3 presents the estimates. We start presenting in Column (1) of Panel A estimates for (8) in which we correct carbon monoxide levels by station by imputing 0.1145 when the observed pollution level was below this value. In column (2) we present the estimates of the raw data without correcting for these low values. Results in columns (1) and (2) imply that TS has an insignificant effect on impact (with point of estimates of -0.002 and 0.03 in the first and second columns, respectively) and a positive and significant effects of 0.31 and 0.32 over the second year of the program, respectively. As expected, the correction for low values does not seem to have any relevant impact on the estimates (as very low values of CO concentration were very unlikely at peak hours).

The monthly dummies tend to present a pattern of increasing effects as time passes, in which there is initially a zero effect and after some months there is a positive effect of TS on concentration levels. However, in contrast to Mexico, in this case the pattern of adaptation is non-monotonic and more volatile. This suggests imposing structure to the estimation procedure may give us more meaningful estimates. Thus, we present results of estimating equation (9) in columns (1) and (2) of Panel B of Table 4.3. 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). The adaptation period lasts between nine and 10 months.[79]

All the other determinants of CO present the expected signs. Background pollution presents a big, positive, and significant effect on carbon monoxide concentration at peak hours (with a coefficient bigger but of the same order of magnitude as in the case of HNC), both gasoline prices and the real exchange rate have a negative impact, and SO_2

[78] As we have 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 decrease a lot). We have tried different ways of correcting for missing data and measurement differences and using a panel of stations instead of averages. Our estimates are that the effect of TS during off-peak hours was close to 0 (the point estimate for the long-run effect is 0.02 with standard error of 0.09 with volatile estimates for the adaptation period). However, we find that the estimated effects of the policy vary a lot across hours (eg., we get positive for an hour and negative estimates for the following hour). More generally speaking this lack of robustness remarks the caution researchers have to exercise when using pollution data.

[79] "In turn, 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). As a comparison the estimated effect of Transantiago on impact using the Imbens-Kalyanaraman RDD estimator is -0.02% (with a standard error of 0.03). If we instead estimate the TS effect using the Imbens-Kalyanaraman RDD estimator with weekly data the effect on impact is -55% (with a standard error of 0.10) and with daily data the effect on impact is -27% (with a standard error of 0.45).

concentration levels are positively related to CO concentration levels.

Finally, we present a regression in which we put SO_2 as the left-hand side variable.[80] Results in column (3) imply that the pattern of the effects of TS on this pollutant are basically unrelated to the pattern we found for carbon monoxide: in this case there is a positive effect on impact, a zero effect after the adaptation period and the adaptation trend presents actually a negative sign. Thus, this evidence is reassuring that our estimates for CO are not capturing the effect of some omitted variable related to overall pollution in Santiago.

In all, these results are consistent with the fact that households waited before moving away from the public transportation system, perhaps evaluating how permanent the problems of the new public transportation system were going to be or perhaps because of credit constraints. This is consistent with the survey analysis conducted by Yáñez et al. (2010). This also supports the hypothesis that additional vehicles were used as a substitute for public transportation during the peak hours in order to bypass the highly crowded and awfully unpredictable public transportation. Interestingly, the adaptation period in the case of TS lasts basically the same number of months that in the case of HNC (10 months). This suggests that our estimates are capturing the speed of adjustment of households in middle-income big cities as they face unexpected shocks.[81] Unfortunately, we cannot test the effects of TS on CO over off-peak hours given the data constraints we face.[82]

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[83]) 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 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

[80] As a reference, DICTUC (2009) presents simulations in which the expected effect of TS on SO_2 is a decrease of just about 0.04% with respect to the 2005 level.

[81] Note that the fact that the adaptation pattern is similar in both cities cannot be related to some monthly patterns in weather conditions or pollution as TS was implemented in the Summer and HNC was implemented in the Fall.

[82] We present some suggestive evidence using results for traffic flows that the impact of TS seem to have been a bit smaller over off-peak hours. See section 5.3 for these results.

[83] 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.

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).

4.4.1 Results for Mexico City

Table 4.4 present a summary of the results for the effects of HNC on CO concentration levels at different stations located across Mexico City. In this case we estimate an equation analogous to (9) but also including a control for SO_2 concentration level at the closest monitoring station. In the table, we first present the name and the location of the station in the city. Next we present the (relative) household income level of the neighborhood in which the station is located (average household income equals to 1).[84] As it is evident in the Table there is a high degree of income heterogeneity across the city: household income in the richest area is about four times the income of the poorest station. Next, we present estimates of the HNC effects on impact and in the long-run and also compute the difference between the long-run and the short-run effects as a measure of adjustment in the use of car as a consequence of HNC. Results imply that both the short-term impacts and (especially) the change in CO concentration levels associated with HNC are smaller in stations located in both poor and richer areas (located in the SO of the city) then in middle income stations (located in the CE and SE of the city), as predicted by our model.

In the case of poor areas, the rationale is that households do not use cars in a very intensive way and, therefore, HNC should not affected them strongly as they do not rely a lot on cars. Richer areas are not affected much because of different reasons: these households tend to have a big enough initial stock of cars that allows them to bypass the driving restriction without new cars (and also to bypass more easily the driving restriction even in the short-run). In contrast, middle-income counties present the bigger reactions as are those that suffer the most given that HNC is more binding for them and also have enough income to increase the stock of cars in response to HNC.[85]

In all, results in this section complement our previous discussion for the effects of HNC at a city-wide level with estimates that support the idea that there are heterogenous effects depending on household income levels. Interestingly also, as we discussed before,

[84] We compute average income of the *delegación* (county) in which the station is located using data from the 1989 version of the *Encuesta Nacional de Ingreso-Gasto de los Hogares*.

[85] It is worth noting that in the case of Mexico a non-trivial part of the CO emissions are not produced by private cars (i.e., the vehicles affected by the policy) but by other light vehicles. This is easily noticeable as while CO levels at peak hours do not vary a lot at the stations level, car ownership and income levels do vary in a significant way at the same level. This may explain why there is a zero effect on impact in the Xalostoc station.

we can construct simulations of our theoretical model in which the only households that react strongly to HNC are those with middle levels of income.

4.4.2 Results for Santiago

Table 4.5 present a the results for the effects of TS on CO concentration levels at different stations located across Santiago. As in the case of stations in Mexico, we estimate an equation analogous to (9) but also including a control for SO concentration levels in the same monitoring station. We first present the name and the location of the station in the city and the (relative) household income level of the neighborhood in which the station is located.[86] Given that TS is a policy that mainly affected the supply of public transportation in the city, we next present the ratio of bus traffic flows to total flows at peak hours in a sample of streets located close to the pollution monitoring stations. We think of this variable as a proxy to capture the importance of buses (relative to other forms of transportation) before TS was implemented. Data suggest, as expected, a strong negative correlation between household income and the importance of public transportation on total traffic flows (the simple correlation is -0.90) and also imply that the dependence of household on public transportation varies a lot in the city: from a number close to 0 in the rich area of Las Condes to values above 10% in the stations located in the four poorest areas of our sample. Next we present a proxy for the change in bus frequencies around each station after TS: it is noticeable that even tough there is some heterogeneity in the changes in all the neighborhoods frequencies fell more or less in the same proportion. Then, the heterogeneity of the intensity of the TS treatment mostly depends upon the initial dependence of households on public transportation.

Next we present the estimates of the short- and long-run effects. In the first case, as expected, the impact of TS is not different from 0 for all the stations. In turn, in the case of the long-run effects, there is a strong positive correlation between the size of the effect and the level of dependence on public transportation before TS (and also a negative correlation with household income). The effect for three of the stations is above 0.40 and the effect for the station located in the richest area is close to 0.18.[87] The only station that somehow deviates from the gradient is El Bosque –with low income and a high degree of dependence on public transportation– with a long-run effect of just about 0.26.[88] It is also interesting to notice that the length of the adaptation period

[86] We compute average income of the *municipalidad* (county) in which the station is located using data from the 2006 version of the *Encuesta de Caracterización Socioeconómica* (CASEN).

[87] The fact that we find a positive and statistically significant effect even for this rich neighborhood is probably related to the fact that for this area circulate not only cars owned by rich agents but also card owned by people from other poorest areas of Santiago that have to travel to this neighborhood.

[88] One potential explanation is that there was a big expansion of the subway network to neighborhoods

is decreasing in the income level of the area in which the station is located, which is consistent with poorer household having bigger adverse effects of the policy and more imperfect access to financial markets.

Thus, as in the case of HNC, estimates at the station level are consistent with the theoretical predictions and mechanisms and, also, 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 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.

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

located close to the area in a manner which is collinear to the period in which TS was implemented.

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.[89]

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.[90] Hence, a 5% increase in gasoline sales is consistent 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.[91] 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

[89] 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.

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

[91] 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).

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 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 a 49 month window centered around 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.[92] Again the time evolution of regions other than Santiago serves as control for the TS treatment. We focus on a 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 (2) 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

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

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).[93]

5.3 Traffic flows

Despite the problems identified above, in this section we study the evolution of traffic flows for a restricted sample of the 46 traffic stations operated by Santiago's *Unidad*

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

Operativa de Control de Transito (UOCT; www.uoct.cl).[94] Our aim for the analysis of traffic flows in this section is two-fold. First, we use this information to confirm whether the adverse effects of TS were bigger in relatively poorer areas. Second, we use this information to identify whether there were heterogenous effects in peak and off-peak hours.

In order to study heterogeneous effects by income level, we aggregate the information from individual stations and create two groups: (i) high-income stations (ie. with flows originated in high-income areas), and (ii) low/middle-income stations (ie., with flows originated in low- and middle-income areas).[95] This way we investigate whether the impact of TS varies in areas with higher and lower income levels.[96]

To identify changes in total flows after TS was implemented we estimate the following equation:

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

where y is (the log of) total flows at period (i.e., hour) t, T is the TS dummy, x_t includes fixed effects (hour of the day, day of the week, month), climate characteristics, economic covariates, dummies for holidays, days in which driving restrictions were implemented, and a vector of dummies to consider the start of the operation of several urban highways and extensions of the subway network.[97]

We run regressions for a four-year window centered around the time in which TS was implemented differentiating peak hours and weekday off-peak hours (which include information from 10am to 4pm). This way we are also able to estimate whether TS had different effects at different times of the day.

Unfortunately the stations do not split flows between private and public transportation.[98] Then, in order to compute changes in private vehicles net of changes in public transportation we implement the following procedure. We estimate the percentage decrease in the actual number of buses passing through each counting station by hour using (i) the number of buses passing on each counting station before TS was implemented,

[94] We limit our analysis to data from 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.

[95] 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

[96] We aggregate flows to avoid the problems of using specific stations as discussed in section 4.1 and by Daganzo (2007).

[97] As in the case of regressions for CO concentration levels, we cluster standard errors in five-week periods.

[98] Becuase of this same reason we do not compute estimates of the adaptation process as the availability of buses after TS was implemted follow a dynamic process (with an increase in the number of buses) unknown to us.

(ii) the actual change in the number of routes passing through each counting 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 presented in Briones (2009) and Muñoz et al. 82009), we compute that the number of buses actually circulating in the city in the first year after TS was implemented was in average about 27% lower than the pre-TS level. Briones (2009) also discusses that due to incentive problems, the effective use of each bus also decreased significantly with respect to the pre-TS levels. Thus, we assume a (probably conservative) decrease in the number of buses actually circulating in the streets of 30%. As a robustness check we also computed changes in bus flows assuming a much more conservative 20% decrease of the total number of buses in the city.

Thus, we estimate changes in the number of buses by hour in each station (and therefore in each of the two disaggregated categories) by distributing 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 decrease in total flows due to a decrease in bus flows after TS is about 3% in peak hours and 2.1% in off-peak hours.

Panel A of Table 5.2 presents our estimates of the effects of TS on traffic flows at peak and off-peak hours for the two types of stations.[99] Results imply an interesting pattern: while the impacts in stations located in high-income areas present estimates that are very close to 0, estimates for the impact of TS on traffic flows are bigger: over peak hours private vehicle flows increased by about 14% (which is a statistically significant effect) and the increase over off-peak hours is 5% (but only marginally significant with a p-value of 0.19). Therefore, this evidence confirms our previous results for CO concentration levels that the effects of TS are bigger in areas with low income households.

Regarding differentiated effects of TS on peak and off-peak hours, the imprecision of our estimates –and the limitations of traffic flows discussed in Section 4.1–do not allow us to infer clear conclusions but the fact that the effects are slightly bigger in peak hours (in low-middle income areas) may suggest that the relative price of public transportation increased in both periods, but the increase was bigger at peak hours. The result of a smaller impact on peak-hours may also explain why we fail to identify any effect in CO concentration levels during off-peak hours.

In all, these results imply that there is an increase in car use, especially during peak hours and in middle/low income areas. This is consistent with our estimates for CO concentration levels (and also gasoline sales) and also imply that it is highly likely that the average speed of private transportation decreased due to the additional cars that

[99] Results assuming a more conservative 20% decrease of the total number of buses in the city are presented in Panel B of Table 5.2.

were circulating in the streets–thus, given support to our discussion in Section 3.3.

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 drives 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 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.

The model developed in Section 3 is help us to formalize this analysis. Given the functional forms adopted in eqs. (1)–(4), welfare costs (i.e., agents' willingness to pay to avoid the policies) are obtained directly as the difference between ex-ante and ex-post household's utilities. 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 4, as well as results (not shown) from additional runs of the model,[100] we believe that the numbers in exercise A3 capture reasonably well the impacts of HNC. Similarly, we believe the impacts of TS are well captured in exercise B5.

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, $r\Sigma_i s_i^0$,

[100] For example, the model can generate an increase in the stock of vehicles twice as large as the one in A3 if we let the driving restriction halve the car capacity k. But that would generate a short-run drop in car use (and CO) of 20%, which is largely inconsistent with our CO estimates.

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 buy an additional 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.[101] 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 (short-run) 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 much as high-income ones.

[101] 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%.

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 impacts in CO at peak hours (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). 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.[102] 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 among transportation options. 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.[103] And second, the introduction of both HNC and TS came with no modification in fares nor in the number of licenses,[104] 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 coefficient was negative in all specifications 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

[102] 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.

[103] There are reasons to beleive 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.

[104] 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 $1000 (or, alternatively, the confiscation of the car).

(search) model of 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 taxis (note that it is impossible for a passenger to tell whether a taxi is official or not).

We carried out a similar analysis of the taxicab market in Santiago. We compiled a novel database of 430 observations of license prices based on 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 between before and after TS, this methodology should allow us to obtain an unbiased estimator of the effect of TS on license prices.

The evolution of license prices (from the ads of taxi licenses) 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 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 after TS, which provides further evidence of the large impact TS had on the relative prices of transportation.

Table 6.3 provides a more accurate idea of the effect of TS on license prices. 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 somewhat to 56%. Interestingly, the value of -0.91 for the price elasticity in relation to the number 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. The coefficients are never below 50% and always statistically significant at conventional levels.[105] Finally,

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

results in column (8) show that the impact of TS can go up to 73% if we restrict the estimation sample to prices from license ads. These results are clear evidence not only that TS affected the demand for taxis in a very significant way, therefore, the shadow price of using public transportation. It is also confirm that TS was much costlier than HNC.

7 Concluding remarks

This paper shows that economic agents in developing countries react within one year to public policies that try to affect the use of private transportation. In particular, it provides evidence that after between 9 and 11 months agents in Mexico City and Santiago adjusted their stocks of car in reaction to a strictly enforced driving restriction and to a policy (failure) that increased substantially the cost of using public transportation, respectively. We develop a model that allows us to rationalize this pattern and to understand the margins in which the agent react. The model emphasizes heterogeneous effects in different margins: (i) the effects are different in different hours of the day, as cars are goods that may provide services both for peak and off-peak transportation and (ii) the effects are heterogenous depending on the income level of the households, which in turn affects the demand for private and public transportation.

The estimated effects are economically relevant. Our estimates imply that CO concentration levels (a pollutant directly associated to car use) increases after the stock of cars adjusted by about 10% in the case of Mexico and by about 33% in the case of Santiago. These are sizeable impacts that are driven by both an increase in the stock of cars of between 3 and 5% in both cases), an increase in car use, and also multiplier effects on pollution related to the quality of cars (the additional cars bough by households were much dirtier than the previous stock) and to congestion in the case of TS (implying a decrease in the average speed, which implies more CO emissions per unit of car use). The fact the results for both HNC and TS are consistent in showing how rapid households adjust their vehicle stocks suggest that there is little room left for policy makers to make ex-post adjustments to their policies. We also find heterogeneous effects by income: HNC has 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 imply the short-term impact to be negligible in all parts of the city and the long-term impact to be decreasing with income.

The paper also provides lessons 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 and reducing pollution, 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 their early years of implementation, 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 move people away from their cars with just improvements in public transportation, however large they be. Our theoretical results imply that in the short-run the public system has to improve a lot to move people away from using cars as, in the short-run, household already own cars and these goods suffer a discount when sold back. Along these lines, there is a large literature now looking at policy instruments that affect vehicle use and pollution (e.g., Feng et al., 2005; Fullerton and Gan, 2005) that should help in the design of more comprehensive transport policies.

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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]
TS	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		

Notes: Sources: [a] TS; [b] DICTUC, several reports; [c] Bravo and Martínez (2007)

Table 3.1: Calibration

Targets	HNC	TS
$s = 0$	0.71	0.62
$s = 1$	0.23	0.30
$s = 2$	0.06	0.08
qhcar/qhtot	0.16	0.31
qlcar/qltot	0.16	0.32
qhcar/qlcar	0.98	0.85
Parameters		
Δp^h	0.91	0.91
Δp^l	1.01	1.23
t^h	0.95	1.22
t^l	0.90	1.20
k	0.29	0.40
r	0.98	0.95

Table 3.2: Simulations

Exercise	Δ^h_{SR}	Δ^ℓ_{SR}	Δ^h_{LR}	Δ^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	NO	0.50[a]	-0.1646	0.0386	0.2032	9
			(0.1137)	(0.1649)		
I.M. del Petróleo	NO	0.53	-0.1741**	0.1735	0.3476***	12.5
			(0.0647)	(0.1223)		
Lagunilla	CE	0.71	-0.2202***	-0.0002	0.2200**	10.5
			(0.0997)	(0.1227)		
Merced	CE	0.84	-0.1037	0.1389	0.2426**	11
			(0.0756)	(0.1248)		
M. Insurgentes	CE	0.70	-0.2257***	0.1596*	0.3853***	14
			(0.0713)	(0.987)		
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	SO	1.99	-0.0331	-0.0331	0.0000	0
			(0.0973)	(0.0973)		
Pedregal	SO	1.99	-0.0323	0.1375	0.1706*	11
			(0.0807)	(0.1163)		

Notes: 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	O	0.54	13.0%	-28.1%	0.0000	0.5131***	11
					(0.000)	(0.1576)	
Pudahuel	O	0.65	11.2%	-26.7%	0.0028	0.4398***	9
					(0.1815)	(0.0910)	
Cerrillos	SO	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 $\text{Log}(P_{\text{Gasoline}}/P_{\text{Diesel}})$		
$\times$ 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: TS Effects on traffic flows

Final	Peak hours	Off-peak hours
	Scenario 1: decrease of bus flows by 30%	
High-income stations flows	0.00	-0.05
	(0.08)	(0.05)
Low-income stations flows	0.14**	0.05
	(0.07)	(0.04)
	Scenario 2: decrease of bus flows by 20%	
High-income stations flows	0.00	-0.05
	(0.22)	(0.04)
Low-income stations flows	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 5.3: Stock of private 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 5.4: TS effect on car sales and trades

	Sales			Trades		
	(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 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.

Figure 2.1: CO data for TS

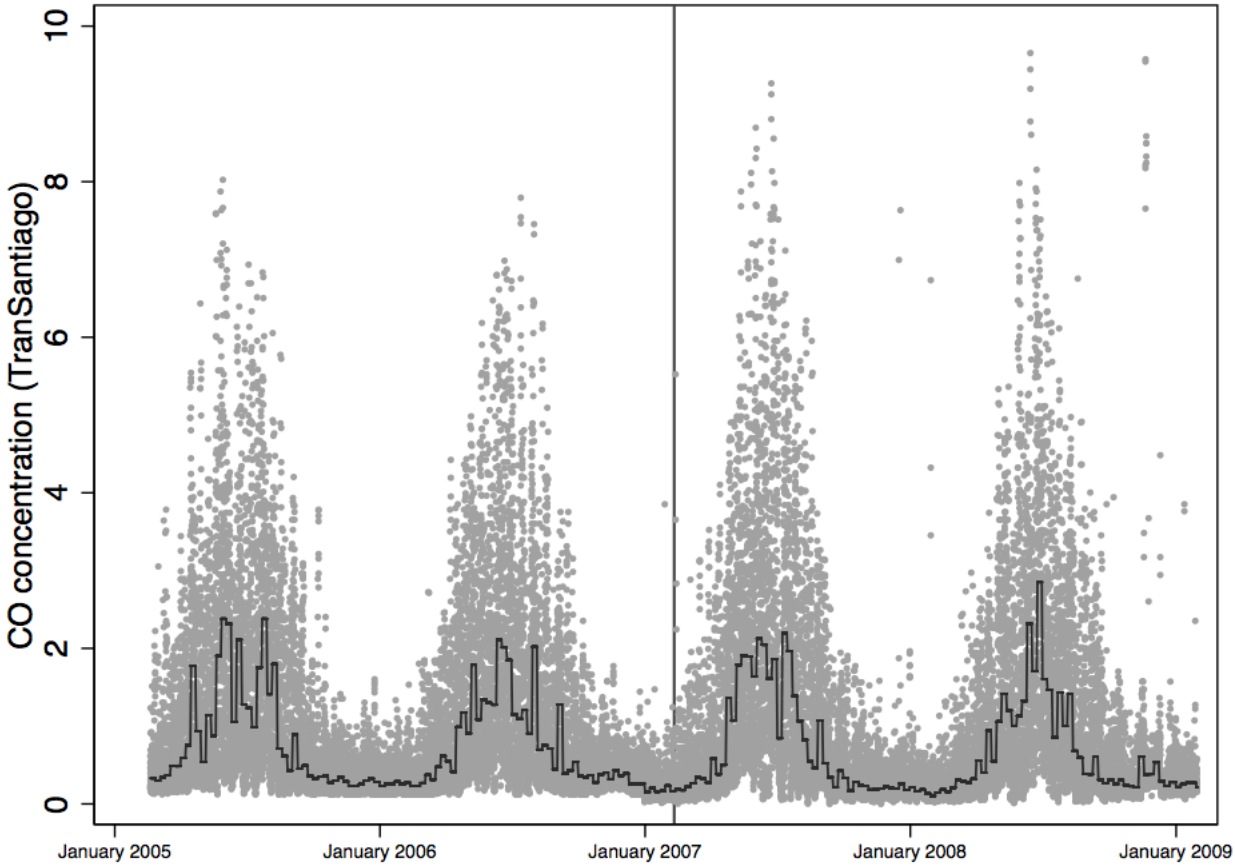

Figure 2.2: CO data for HNC

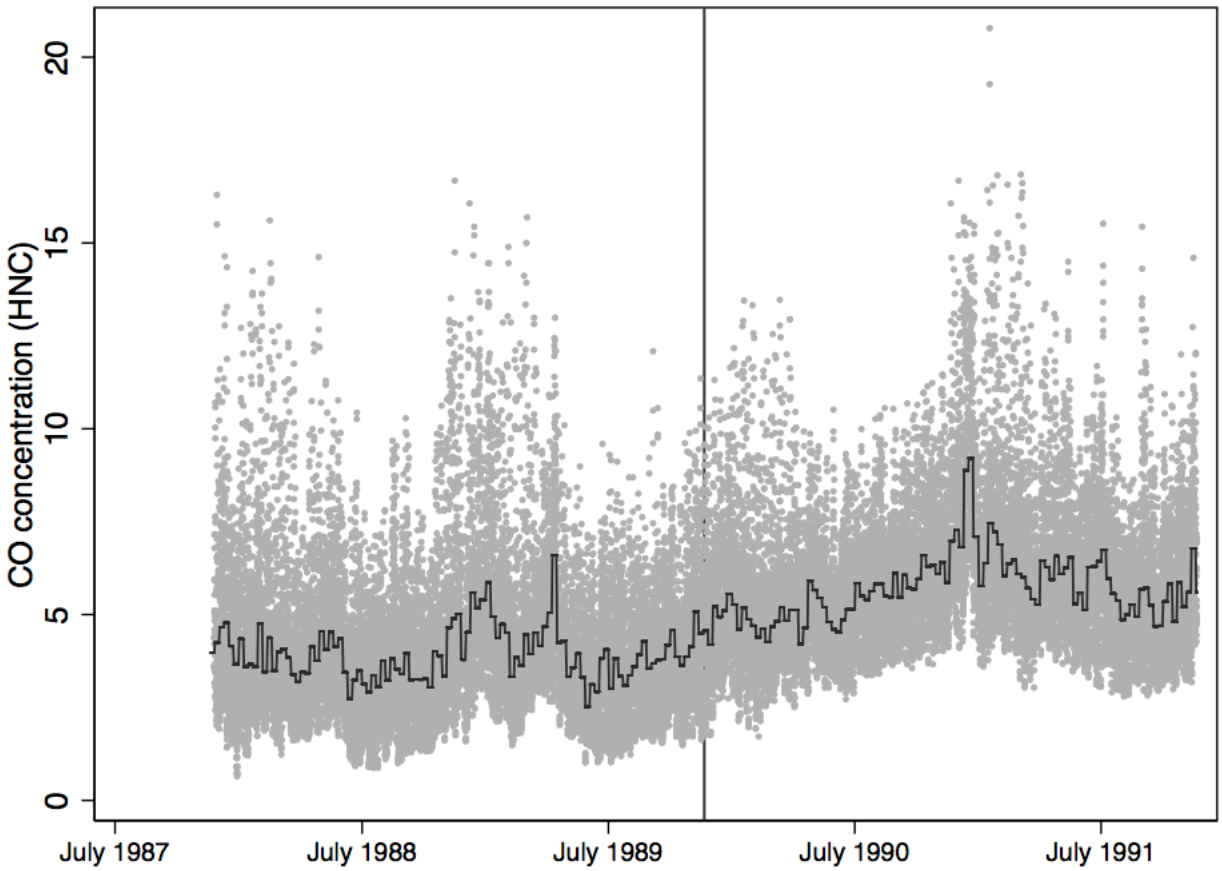

Figure 3.1: Decision to own a vehicle based on vertical preferences

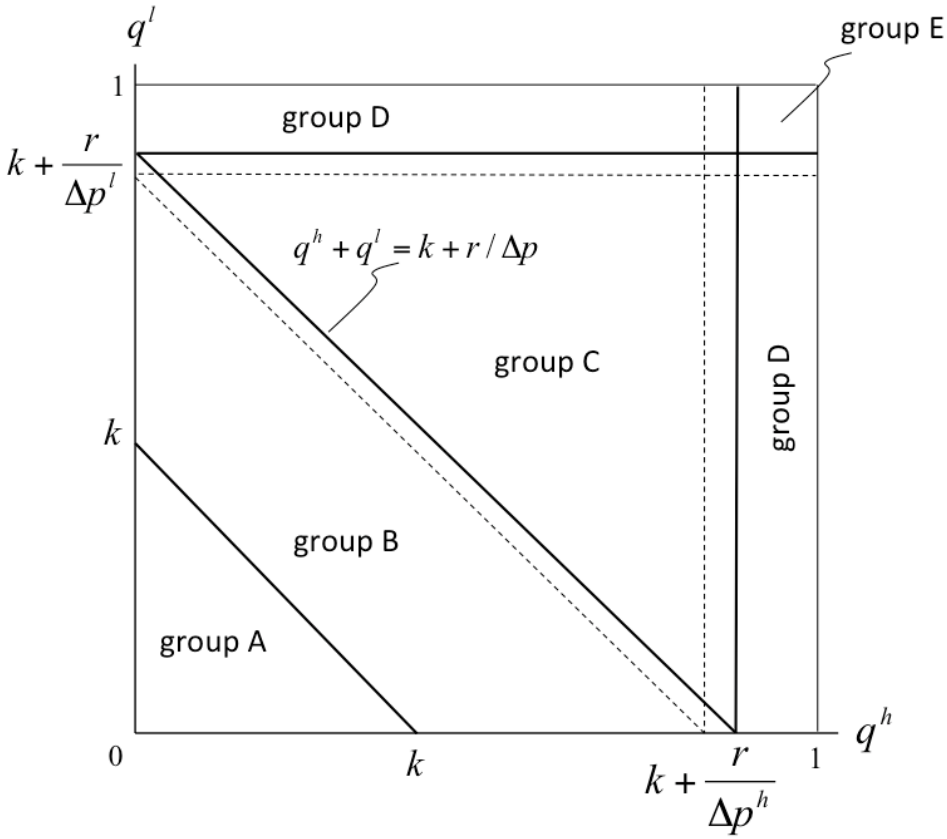

Figure 3.2: (a) Households in group A

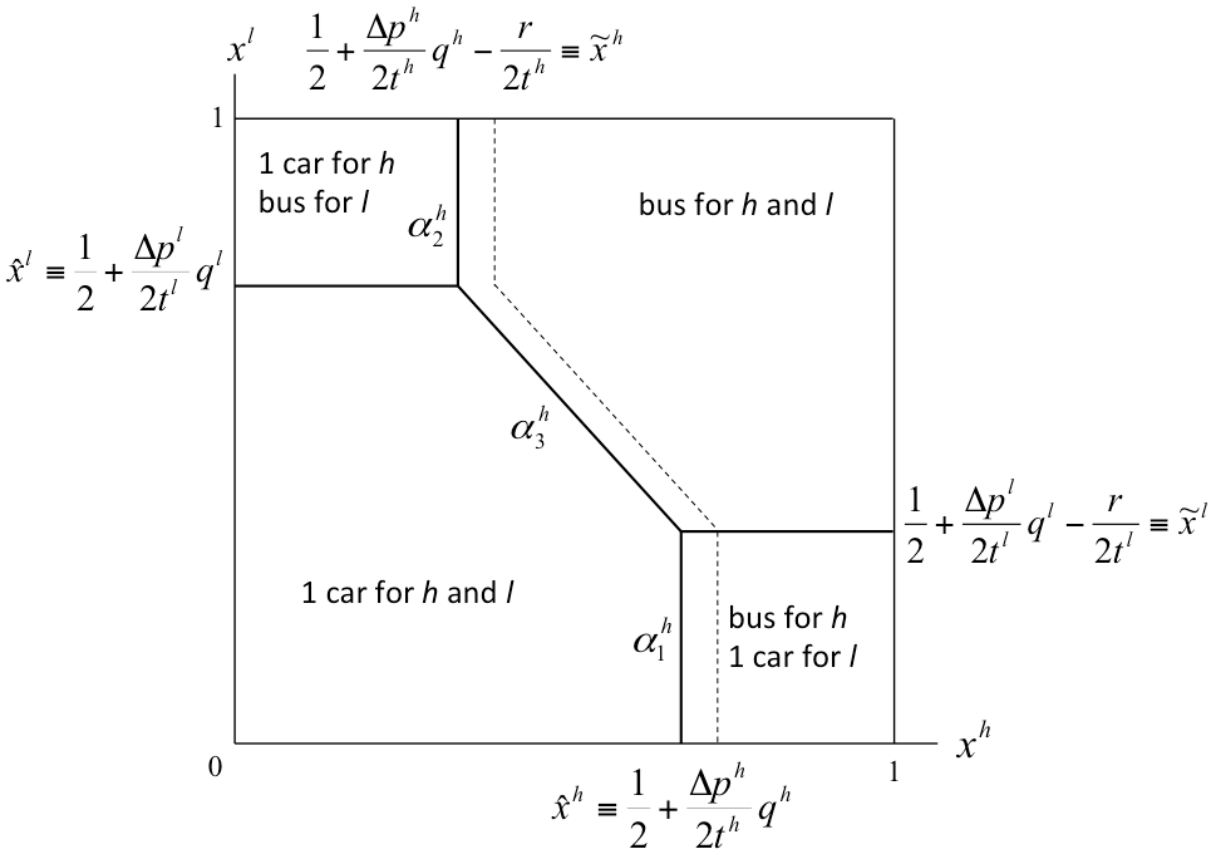

Figure 3.2: (b) Households in group B

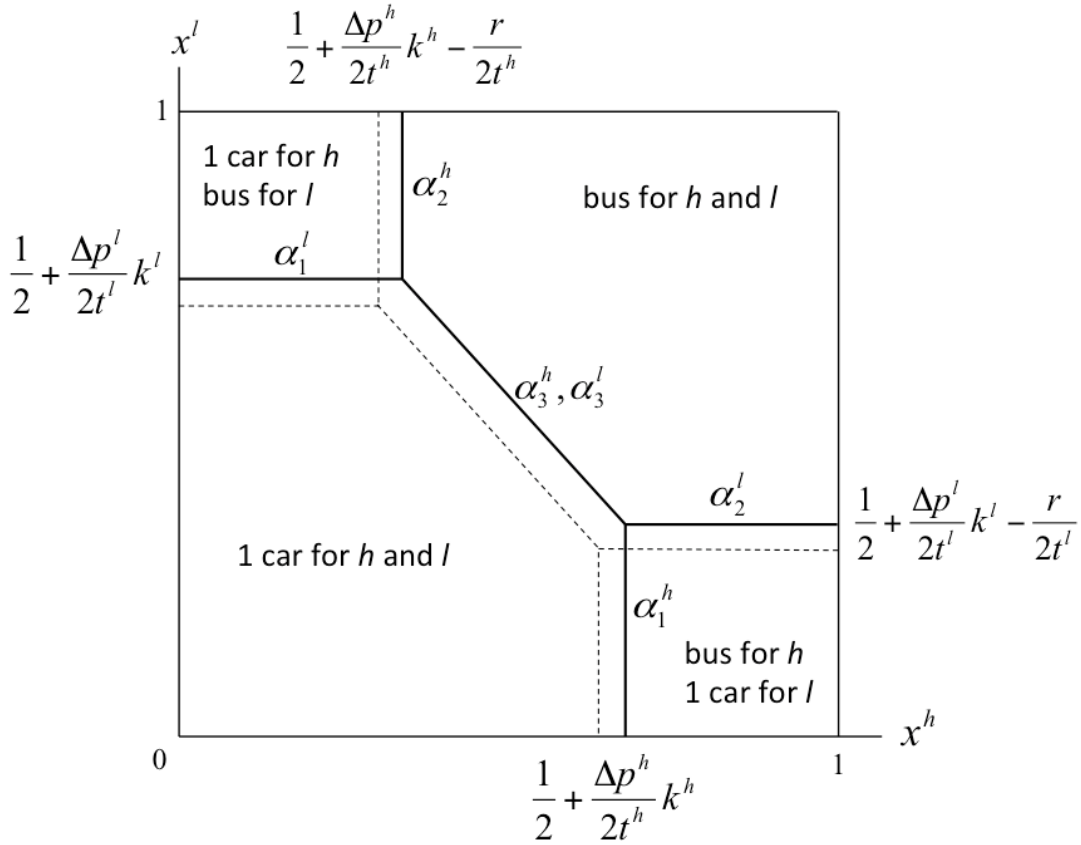

Figure 3.2: (c) Households in group C

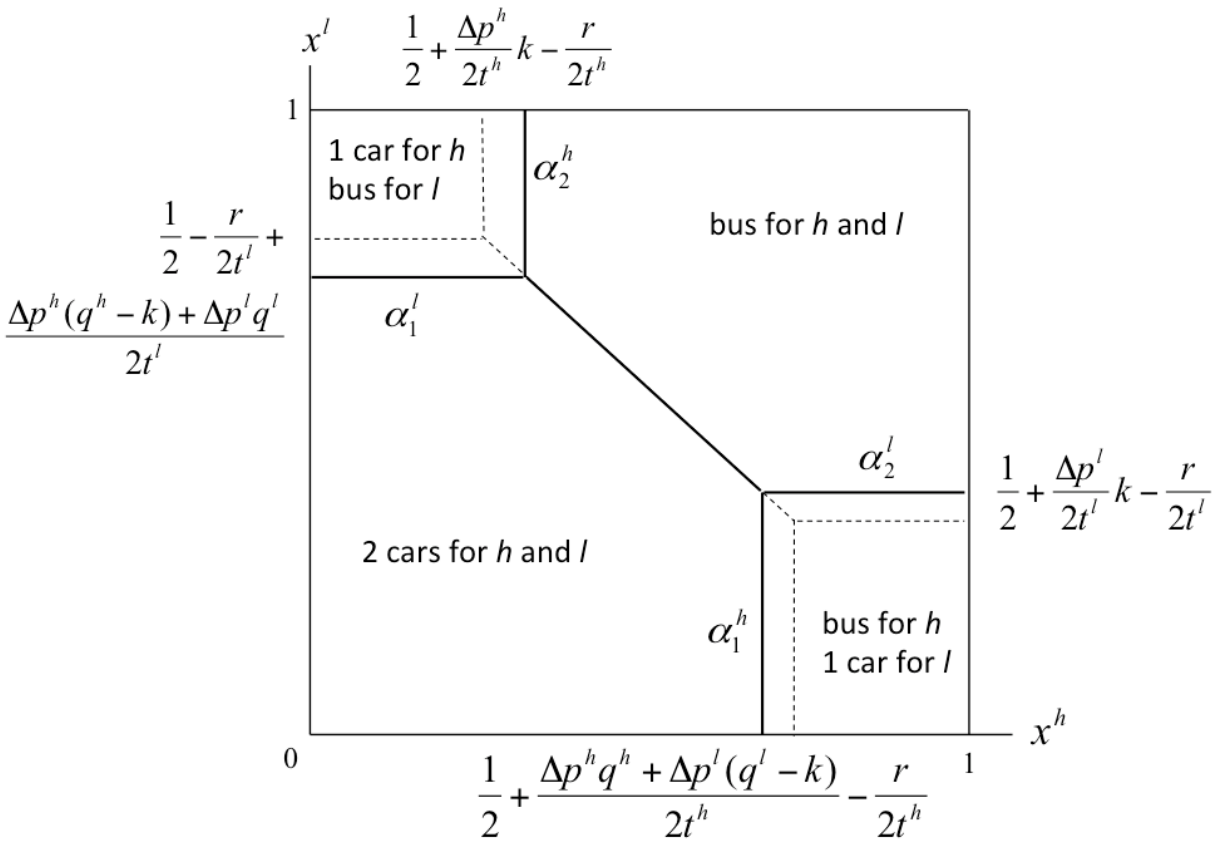

Figure 3.2: (d) Households in group D

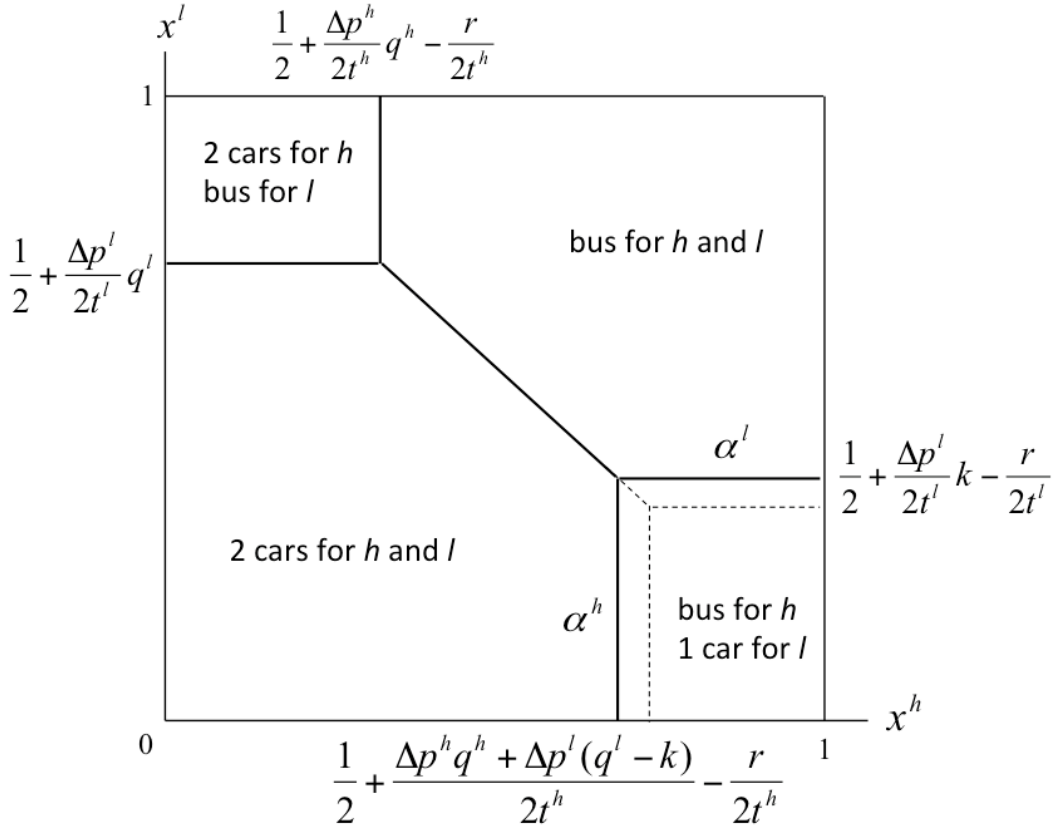

Figure 3.2: (e) Households in group E

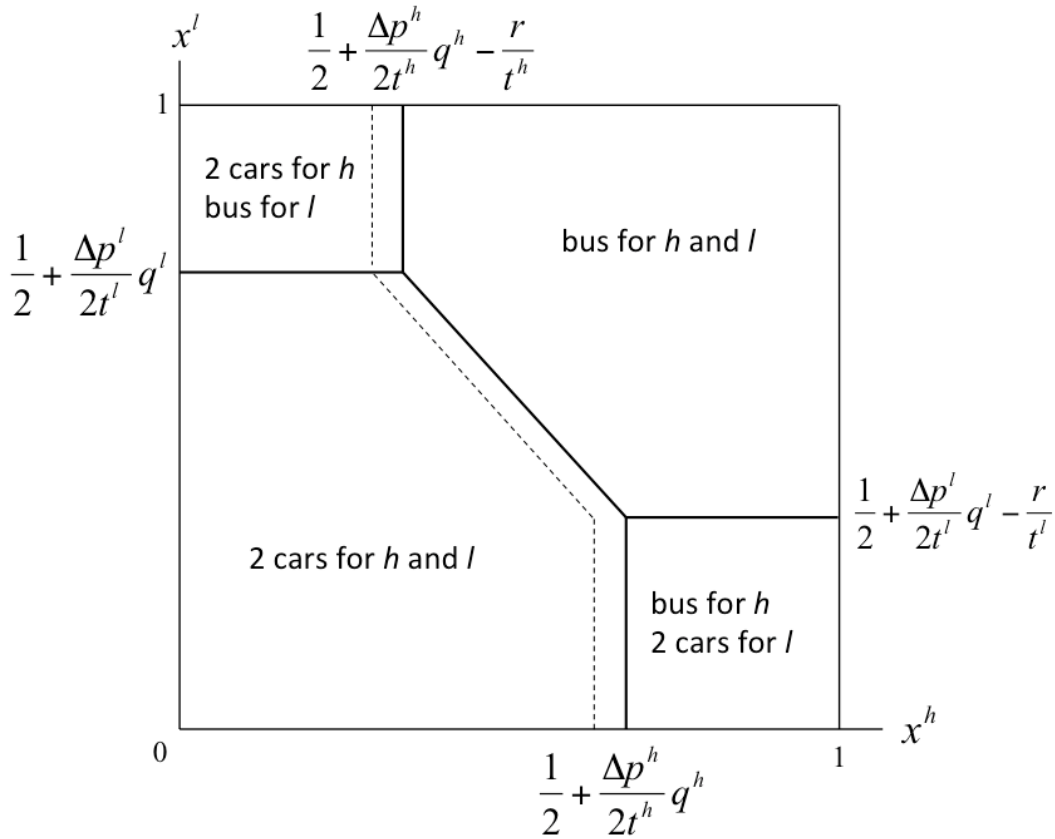

Figure 4.1: CO Emissions and Concentrations in Santiago (January 2002)

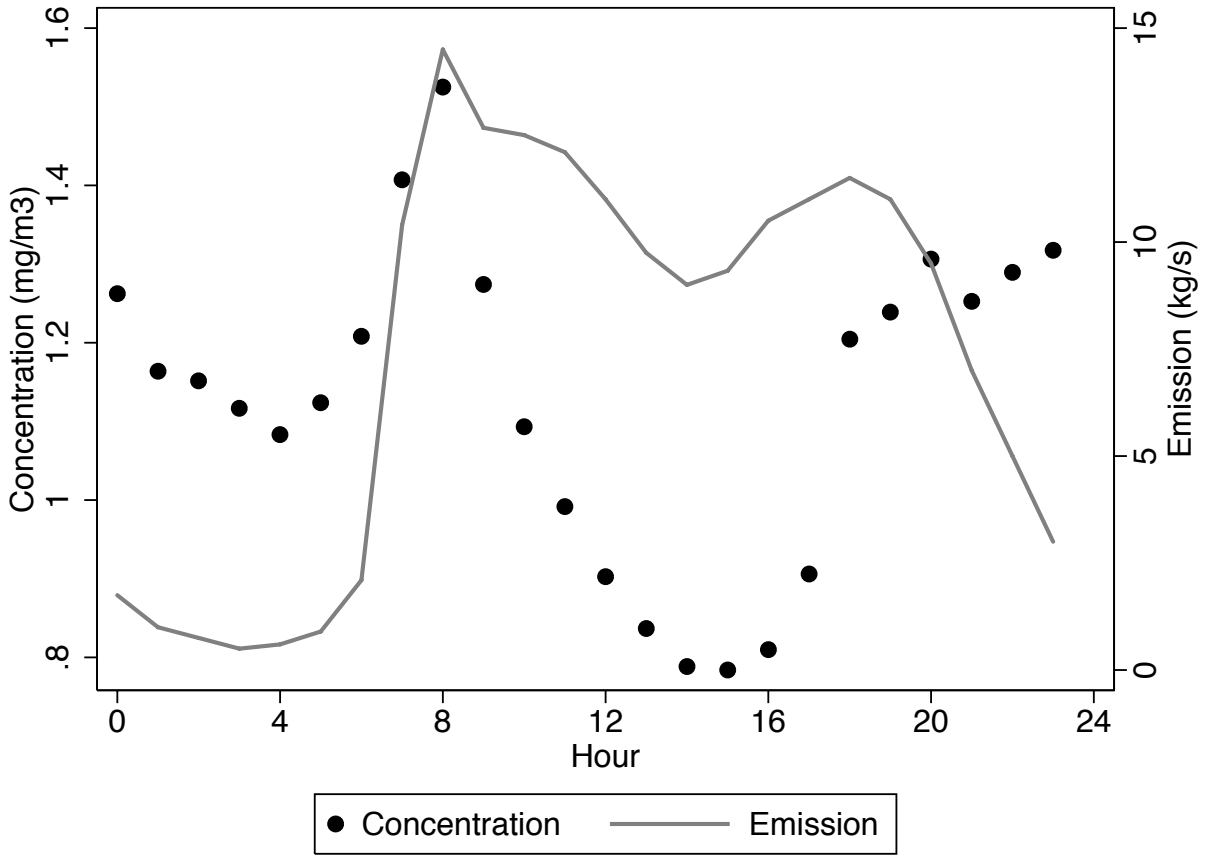
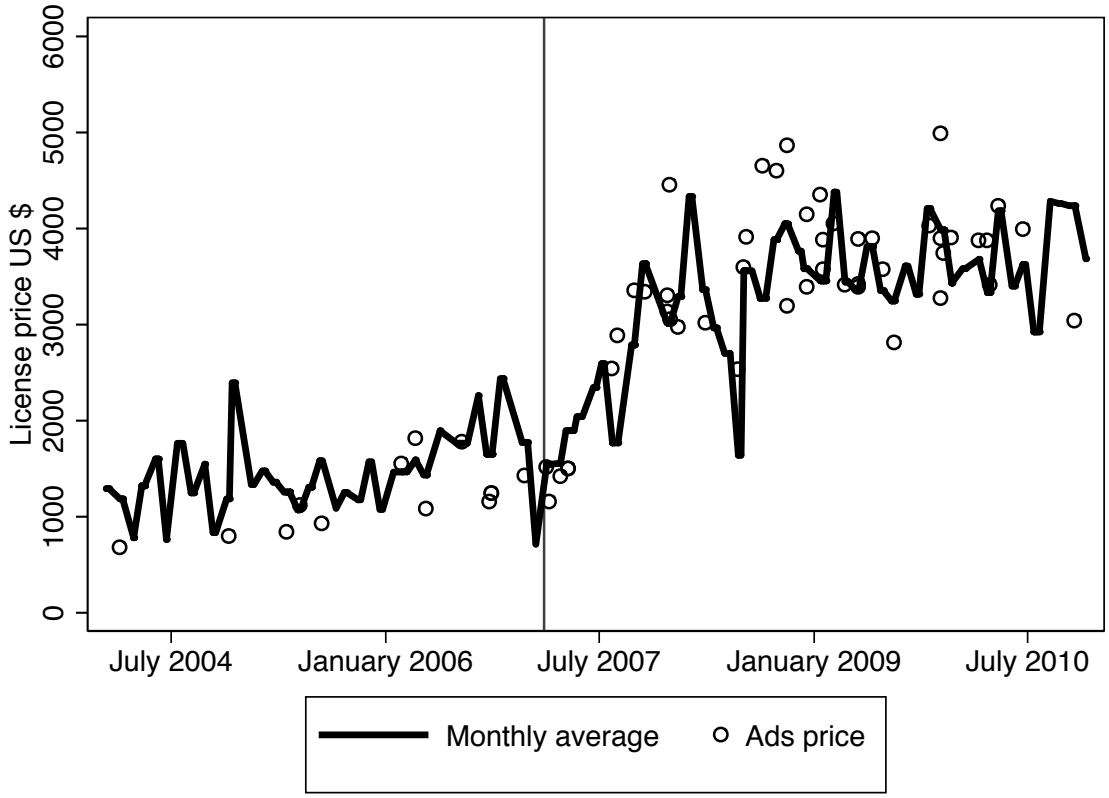
Figure 6.1: Prices of taxi licenses in Santiago (sub-sample of license ads)