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. This paper looks at two of such policies: the driving restriction program introduced in Mexico-City in November of 1989 —Hoy-No-Circula (HNC)— and the public transport reform carried out in Santiago in February of 2007 —Transantiago (TS). Based on hourly concentration records of carbon monoxide, which comes primarily from vehicles exhaust, we find that household responses to both HNC and TS have been ultimately unfortunate —more cars on the road and higher pollution levels— but also remarkably similar in how fast households have adjusted their stock of vehicles, within a year. Another empirical finding is how different short- and long-run effects of the policies can be. In fact, we find that a (permanent) driving restriction like HNC can still be effective in the short-run, say, for a month or two. We also document significant heterogeneity of the effects of the policy across the city. For the case of TS we complement these results with additional evidence coming from gasoline sales, sales of used and new cars, traffic flows, and the price of taxi medallions. A novel theoretical model is also developed to explain the empirical results and to compute policy costs based on few observables.

Key words: public transport, driving restrictions, pollution, congestion

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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 (EIU, 2010). In November of 1989, for example, authorities in Mexico City introduced a program, Hoy-No-Circula (HNC), that restricted drivers from using their vehicles one weekday per week. More recently, in February of 2007, authorities in the city of Santiago-Chile embarked in a city-wide transportation reform, Transantiago (TS), with the idea of improving and increasing the use of public transport. Given the complexity of transport dynamics in large cities like Mexico-City and Santiago,[1] drastic interventions like these represent unique opportunities to improve our understanding of how households respond to these policies. This paper is a theoretical and empirical attempt at evaluating these responses.

There is not much controversy in that HNC and TS did not succeed in persuading drivers to give up their cars in favor of public transport, and hence, in reducing congestion and pollution (there is some about the timing and the magnitude of the impacts; something this paper contributes to as well). Yet, both policies are most valuable for the purposes of this paper. 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. More importantly, they amount to one-time drastic interventions like no other in the region.

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, other driving restrictions in Latin America have affected only a fraction of drivers (e.g., those using older cars) and under special circumstances (e.g., days of unusually high pollution).[2] Some believe that HNC had a good start (e.g., Onursal and Gautam, 1997; GDF, 2004), but most agree that over the longer term it lead instead to an increase in the number of vehicles on the road (e.g., Eskeland and Feyzioglu, 1997; Ornasul and Gautam, 1997; Molina and Molina, 2002; and Davis, 2008).

TS, on other hand, consisted of a complete transformation of the public transportation system of an entire city at once (Muñoz et al., 2009). Other public transportation reforms like Transmilenio in Bogotá have been more limited in scope and introduced gradually. TS involved, among other things, an overnight and significant reduction in the number of buses and radical change in the design and number of routes more in line with a hub-

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

[2] The driving restrictions in Medellín and Quito also appear quite comprehensive (e.g., Cantillo and Ortuzar, 2011), but there is limited data to study them as we do here. For a survey on transport policies implemented in the region see Ide and Lizana (2011).

and-spoke network where passengers were expected to make many more transfers than in the past to complete their trips. 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 from the first day; which is what we exploit in our analysis.[3]

The paper adds to this literature by characterizing and quantifying how households adjust to these policies over time and how that adjustment varies across different income groups. We pay particular attention to the long run effects, that is, whether and how fast households adjust their stock of vehicles.[4] Understanding these effects is important for policy evaluation and design (or redesign). For example, accounting for differential location effects within a large city can be particularly relevant for quantifying health costs associated to non-uniformly mixed pollutants (e.g., carbon monoxide, ozone, particulates). And these costs can be substantial as reported elsewhere (Currie and Neidell, 2005). Likewise it can help better target policies both across income groups and time (e.g., we find HNC to have the expected effect of decreasing car use on middle-income groups but only in the short-run).

Along with the empirical analysis (and to help interpret its results and estimate policy costs based on few observables), we develop a theoretical model that characterizes the short and long-run impacts of different transportation policies and for different income groups. In constructing the model we partially borrow from the bundling literature (e.g., 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 public transport— and in the amount of travel.

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 (morning) peak hours, compared to alternative candidates like hourly records of traffic flows and of other pollutants. Mobile sources, and light-duty vehicles in particular, are by far the main emitters of CO —97% and 94%, respectively, at

[3] 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.

[4] In this paper, short-run and immediate impact are used interchangeably and long-run is the time it takes (most) agents to adjust their stock of vehicles as a response to a policy shock. There can be longer-run effects (e.g., inter and intra city migration of people and commercial activity) but we do not have the data to quantify them; neither our empirical methodology is well suited to identify effects that are too far away from the policy shock.

the time HNC and TS were implemented. Another reason to focus on CO is that it allows us to study the response of different income groups by looking at the evolution of CO records from individual monitoring stations which happen to be located in neighborhoods of disparate incomes.[5]

Our empirical strategy is to compare CO levels two years before and after policy implementation for different hours of the day and different income groups. We employ several control variables including (hour, day, and month) fixed effects and proxies for economic activity and records of meteorological and atmospheric conditions. A potential problem with this approach is that there may be still time-varying (economic and/or weather) factors we cannot control for, so past CO records may not build a good counterfactual of how CO would have evolved in the absence of the policy. In other words, the effects of these time-varing omitted factors on CO would be entangled with those of the policy. In addition, there may be an endogeneity problem if the introduction of the policy is correlated with an increasing trend of congestion and pollution. One way to get around both of these concerns is to focus exclusively on the short-run impact of the policy using a regression discontinuity design (RDD), as done by Davis (2008) for HNC. We also run this exercise in the paper but instead using the RDD estimator of Imbens and Kalyanaraman (2012) which is based on local linear regressions on either side of the threshold (i.e., time of policy implementation). Interestingly, our estimates of the short-run effects of the policies (see below) are remarkably similar to the ones obtained with the Imbens and Kalyanaraman's RD method. This gives additional support to our empirical strategy.

However, we are often not much interested in the immediate or short-run effect of a policy but in its long-run effect. Indeed, according to Litman (2011) it may take 1 to 3 years for some policies (e.g., new transit service) to reach its full potential. We then employ different approaches as part of our empirical strategy to deal with the above "long-run" concerns. First, we run a difference-in-difference regression with a comparable pollution data from another city that did not experience a similar transportation policy but did follow a similar trend in air pollution before the policy was enacted. There is no such city for Mexico-City —because lack of data— but fortunately there is one for Santiago that is neither too close, so that transportation was not disturbed by TS, nor too far, so that there likely be economic ties and similar weather conditions. The town of Quillota, which is 130 kms north-west of Santiago, exhibits an ex-ante CO trend that makes it indistinguishable, in relative terms, from a county in Santiago. As for HNC, we run a different falsification exercise: we subject our dataset to a placebo policy implemented two years before the actual policy was put in place. Second, we exploit the existence of a network of monitoring stations in each city to identify counties or municipalities in which, according to the theoretical model, the transport policy (either

[5] Davis (2008) uses the same hourly records but from all pollutants to evaluate HNC. We restrict our attention to CO because this pollutant is closely related to car use. We discuss this point in more detail in Section 3.1.

TS or HNC) should have no or limited effects. Estimates from these counties would then provide a good idea of potential time-varying omitted factors that could be affecting the rest of the counties in the city. More generally, the empirical estimates from all individual stations help check whether the policy effects we find across income groups are consistent with the predictions of the model.

Empirical results at the city level for HNC show statistically significant reductions of CO in the short run (i.e., first month) of 13% and 9% for morning-peak and off-peak hours, respectively. This short-run result is in line with the perception of high compliance with the program and with the initial announcement that the program would only last for three months (GDF, 2004). For the long run we find an increase of 11% during morning-peak hours and of 9% 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 about 20%.[6] The length of the adjustment phase, i.e., the time it takes for the policy to reach its long-term effect, is estimated using the structural-break (*sup F*) method of Quandt (1960), Andrews (1993) and Hansen (2000). We find the long-run impact of the policy is reached between 8 to 12.5 months after its implementation. As for TS, we can only report results for morning-peak hours.[7] We find no impact on CO in the short run and a 27% increase in the long run (this number drops marginally to 26% in the diff-in-diff exercise mentioned above). This long-run impact is reached 6 to 7 months after implementation.[8]

The above city-level figures mask a wide heterogeneity in responses. Consistent with the predictions of the model, we find HNC to have the largest impact in middle-income groups, where households were more likely to buy a second car, and lowest in high-income neighborhoods where households had already sufficient car capacity to cope with the driving restriction. Results for TS follow the model predictions 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 from a high of about 40% in the poorest areas to 17% in the richest county. These disappointing CO results are consistent with additional evidence coming from other data sources including gasoline consumption, car registrations and

[6] Note that this 20% increase comes close to the 24% net increase at peak hours (from -13% to +11%) and the 18% increase at off-peak hours. These net increases are all statistically significant at 1%. Note also that the long term figures may be seen as lower bounds because of the later introduction of other measures (e.g., cleaner gasoline, a refinery closure) that could have had, if at all, some negative impact on CO within our window of estimation.

[7] 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.

[8] As mentioned above, the short-run results for both HNC and TS are similar to those obtained with the RDD estimator of Imbens and Kalyanaraman (2012) but when used with monthly averages. Note also that our short-run numbers are different than those in Davis (2008) for HNC, perhaps because he uses a parametric seventh-order polynomial of time to control for trends before and after the policy and/or he does not distinguish for the hour of the day and the location of the monitoring stations as we do.

sales, traffic flows and prices of taxi licenses (medallions).[9]

There are valuable lessons to learn from these experiences. One is that policy impacts can vary widely among different income groups. Another one is that driving restrictions can be quite effective —partly because they are relatively easy to enforce, as HNC shows— in reducing car use among households in middle-income groups that are capacity constrained, but only in the short run.[10] So rather than working on a permanent basis, driving restrictions should be used sporadically, to attack short episodes of very bad pollution (for example, during the last decade Santiago had faced about 10 of these daily episodes per year).[11] A third and more subtle implication is that from the magnitude of the CO results it may appear 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; hence, the costs inflicted by these policies remain largely unchanged in the long run. In the case of TS, these costs amount to approximately \$120 million annually (in 2007 U.S. dollars) or 9% of the value of the stock of vehicles in 2007 (in the case of HNC these costs reach 5% of the stock value).

Yet, the most remarkable lesson is perhaps how fast households adjust, when they do, to these policies (more so if it took a month or so before it became clear the policy shock was permanent). The speed at which the stock of vehicles has increased in both cases appears much faster than that suggested by the transportation literature (e.g., Litman, 2011; Paulley et al., 2006).[12] It is also faster than that suggested by the earlier literature on consumption of durable goods (e.g., Caballero, 1990) but closer to the more recent literature (e.g., Chah et al., 1995; Gallego et al., 2001) that finds that over 90% of the adjustment to a demand/supply shock is reached within the first year of the shock. In any case, both policy experiences confirm that the adjustment process is quite fast —adjustment that for most part is irreversible— which leaves little room for ex-post corrections. This calls for nothing but more careful ex-ante policy design, including the combination of instruments and a serious consideration of market-based instruments such as road pricing and pollution taxes (e.g., Fullerton and Gan, 2005) that so far has received

[9] On this latter, our econometric results and Lagos' (2003) model suggest that the demand for taxicab rides in Santiago has at least doubled because of TS. Still, taxi rides constituted less than 1% of all trips before TS, according to the *Encuesta Orgien-Destino* (Origin-Destination Survey) of 2006 (EOD-2006) for the city of Santiago.

[10] Lin et al (2012) also find that driving restrictions in other regions may lead to pollution reductions in the short run.

[11] The driving restriction policy in Santiago works somehow like that. Unfortunately, it is not possible to evaluate the effectiveness of these one-day interventions using a RDD approach because of the volatility of this hourly pollution data that ultimately comes from idiosyncratic shocks we cannot control for. In fact, these RDD results change dramatically when we try to estimate the impact at the hour, day or weekly level.

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

none in the region.[13]

The rest of the paper is organized as follows. Section 2 describes the two transport policies in more detail. Empirical results based on CO records are in Section 3. The theoretical model and its use in interpreting some of the empirical results are in Section 4. Additional empirical analysis using alternative data sources are in Section 5. A discussion of results and estimations of the costs inflicted by the policies are in Section 6. We conclude in Section 7 with a brief annotation of topics for further research.

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 8pm, based on the last digit of its license plate (GDF, 2004). 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 (Onursal and Gautam, 1997; Eskeland and Feyzioglu, 1997; Davis, 2008). The program did not experience any relevant changes for the next two years.[14] Soon after HNC, in 1991, authorities begun introducing additional environmental policies (GDF, 2004; Molina and Molina, 2002; Onursal and Gautam, 1997);[15] but if anything, these additional policies would biased our long-run results downward making the case that HNC did lead to more CO even stronger.[16]

[13] Work by Parry and Govinda (2011), for example, shows that the socially optimal gasoline tax for Mexico-City should be 16 times the current tax. The political economy of why Latin America has stayed away from this and other market-based policies 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.

[14] The first relevant change following the implementation of HNC came almost two years later in October of 1991 when the "Saturday" ban introduced in January 1991 to public transportation (taxis and microbuses) was extended from Saturdays to weekdays from 10 am to 9 pm in an alternating manner similar to that of cars. Later, in 1992, cars using natural or liquefied gas were exempt from HNC. For more details on these policy changes see Onursal and Gautam (1997) and Molina and Molina (2002).

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

[16] We say "if anything" because we expect the effects of these additional policies on CO to be minor within our window of estimation. For example, the refinery's contribution to total CO between August 1989 and January 1991 was less than 2%, or 50 thousands tonnes per year (Carmona, 1992). Likewise with the use of unleaded gasoline: it had no effect on CO emissions of older models (1986-90) and a 60%

There is also the issue of whether some households could have moved forward the purchase (and use) of an additional car to right after the announcement of HNC (November 6) or even before that in anticipation of a possible increase in car prices because of HNC. There are several reasons to believe this should not be a concern, namely, that the initial announcement of HNC had the program lasting until the end of February (and only then, the program was officially made permanent), that the effect of HNC on the stock of vehicles seems to have been rather modest,[17] and that there was not much time between the announcement and implementation so at best only very few households could have adjusted so quickly. Even if there was some anticipation (which we did not find when we test for it in some of our regressions), and hence, some increase in emissions two or three weeks before implementation, the effect of this in the construction of our counterfactual (and thus in our estimations) would be negligible.

Figure 2.1 plots average hourly CO concentration levels for all hours for the period 1987-1991, which is the 4-year (symmetric) window we use in our empirical estimation of HNC. The vertical line indicates the exact moment HNC was implemented. Had HNC being effective in making people substitute away from the car, one would expect to see some of it reflected in a reduction in CO concentrations. A quick look at the plot shows no clear indication of a decrease in CO concentrations in the short-run neither a subsequent increase over the long-run, which we attribute to the volatility of this pollution data. However helpful a visual analysis may be, its use to identify the effect of a sharp discountinuity is quite limited when working with this volatile data.[18]

Although they do not constitute a proof, Figures 2.2 and 2.3 fit quite well with our empirical findings. Figure 2.2 compares the 24 hrs CO profile for the average weekday of December 1989 (the month after HNC) with the same profile for the average weekday a year earlier. We observe important reductions in CO at both (morning) peak hours (8-10am) and evening rush hours (6-8pm). The increase we observe during the hours in which the restriction was not active is also expected as some day trips were likely moved to those hours. Figure 2.3 displays the same kind of comparison but for the long-run (11 months after implementation) and with significant increases in all hours of the day.

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

reduction in the new models (1991-96) (www.ref.pemex.com/octanaje/25magna.htm). But as explained by GDF (2004), it took a few years for unleaded gasoline to become widely available so it was usual in those early years to see cars with catalytic converters running on leaded gasoline. In any case, assuming that all cars sold in 1991 (around 6% of the fleet) were 60% cleaner from running on unleaded gasoline, the impact on CO was less than 0.5% (recall that the CO share of the newest 18% of the fleet was only 2%; see Beaton et al. 1992).

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

[18] We are not alone here. For example, figures 4 and 5 of Rockoff and Turner (2010), who also worked with highly volatile data (test scores), show no indication of a discontinuity around the different cutoff points; only the RDD econometric analysis picked up the effects, which were significant.

with a different instrument: improving, supposedly, the quality of public transport. The old public transportation system was regarded as highly polluting, unsafe, and inefficient both in terms of travel time and cost (e.g., Briones, 2009; Muñoz et al, 2009).[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 (centrally-planned) change in the design and number of routes more in line with a hub-and-spoke network where the existing subway system would play the role of a hub.

While the original design of TS was expected to deliver significant reductions in congestion and pollution from fewer cars on the street,[21] its actual implementation has been recognized by many as a major policy failure (e.g., Briones, 2009). Table 2.1 provides numbers illustrating the extent of the intervention. Commuting time increased, on average, from 77 to about 90 minutes (both ways), mainly because of the increase in the average travel time of public transport that went up by about 30% (from 102 to 133 minutes). In contrast, travel time of cars and taxis does not seem to have been affected nearly as much. 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).[22] Yet, for the purposes of our evaluation, public opinion and quality indicators indicate that the level of service never came close to pre-TS levels even after four years of implementation.[23] Finally, the issue of anticipation to TS does not arise here despite its launch was announced several months in advance; this is simply because nobody anticipated the final outcome. As shown in Figure 2.4, good evidence on the latter is

[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 passengers could travel almost anywhere in the city without transfers. Under TS, passengers are expected to transfer a few times before completing their journeys (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 impact on particulates (PM10) due to the presence of fewer and cleaner buses that traditionally have been a main contributor of that pollutant —33% according to CONAMA (2004). Using also high-frequency data, Gómez-Lobo et al. (2011) find this to be the case.

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

[22]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. More importantly for our estimation, the fact TS suffered some modifications after implementation should not biase our results in any meaningful way. For example, if anything, the speed of adjustment may be under estimated as some commuters probably decided to wait longer to see how much of an improvement in service was possible before purchasing a car.

[23]According to survey data collected by Libertad y Desarrollo, a think tank (www.lyd.com), the approval rate of Santiago's public transport dropped immediately with the implementation of TS (February 2007) and recovered a bit a year later (March 2008) and has remained there ever since (May 2011).

provided by the constant prices of taxi licenses in the city of Santiago the year before implementation. And the significant ex-post increase in prices, although not immediate, as expected in quota markets such as this where price formation takes time (Joskow et al., 1998), provides further evidence of the impact of TS on the quality of public transport.

In any case, this deterioration in quality should have resulted in a switch towards alternative modes of transportation, e.g., cars, and hence, in an increase in CO emissions. Figure 2.5 plots average hourly CO concentration levels for the period 2005-2009 which is the period we use in our empirical estimation and the vertical line indicates the exact moment TS went into operation.[24] The plot shows no clear indication that TS has lead to an increase in CO concentrations; at best, it has lead to an increase in the upper bound of the range of hourly concentrations. Because of data volatility, a more informative way to present this data is again with 24 hrs concentration profiles. Consistent with our empirical findings, Figure 2.6 shows that TS did not have much of an effect the month after implementation (some reduction at peak hours perhaps because less congestion from the fewer buses on the street) whereas Figure 2.7 shows a big effect already 6 months later.

Note also in Figure 2.5 the large number of records of nearly zero value, which is never the case in HNC. This not only suggests that pollution in Mexico-City in 1989 was significantly higher than in Santiago in 2007 but, more importantly, that we may face identification difficulties, as we discuss in the next section, to study the effect of TS on off-peak CO concentration levels.

3 Policy effects on carbon monoxide (CO)

This section contains our central empirical results and is organized as follows. We first justify the use of CO concentrations as a proxy for car use. We then present our empirical strategy followed by the empirical results, first for HNC and then for TS.

3.1 Why CO?

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

A first potential candidate is hourly records of vehicle traffic from traffic-control stations scattered around the cities. There exist a number of problems with this "proxy". To

[24] 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.

start, we do not have this information for Mexico-City (at the time of HNC, at least).[25] 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.[26] 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, the use of hourly CO concentration records appears encouraging for several reasons. First, as previously discussed, according to emissions inventories, mobile sources, and light-duty vehicles in particular, are by far the main emitters of CO —97% and 94%, respectively, at the time HNC and TS were implemented.[27] Hence, we should expect any change in city traffic be picked up by changes in CO concentrations. Second, CO is the only pollutant that can be regarded as non-reactive (i.e., that does not react with other pollutants or to sunlight) on a time scale of one day (Schmitz, 2005), which is what we use in our empirical estimations (note also from the 24hrs profiles of Section 2 and also from Figure 3.1 that CO is rapidly dispersed as wind picks up during the day). Thus, under stable meteorological conditions (e.g., before and around dawn), rapid increases in vehicle use (and in CO emissions) should be immediately reflected in changes in CO concentrations both at the city and at the station level.[28] Third, CO measures, unlike hourly records

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

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

[27]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.

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

of vehicle traffic, are better at capturing effects at the scale of the city or municipality rather than at a particular location (e.g., street). Fourth, the use of CO emissions also allows us to identify potential increases in pollution due to more congestion and/or the use of more-polluting cars.

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, precipitation, wind speed, and wind direction. This imperfect correlation can be readily seen in Figure 3.1 that plots concentration and emission patterns reported by Schmitz (2005) for a week 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 morning-peak hours 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).[29] The increase in concentration at peak should then closely reflect traffic activity at that time both at the city and individual station levels.[30] We adopt these considerations in the empirical estimations that follow.

3.2 Empirical strategy

Our main datasets are time series collections 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 CO —and other several pollutants, namely, (ground-level) ozone, nitrogen dioxide (NO_2), nitrogen oxide (NO_x), and sulfur dioxide (SO_2)— and for some of the stations, it also reported hourly measures of temperature, real humidity, wind speed and wind direction. The average failure rate of the network —fraction of time stations do not report CO information— is about 31% and roughly constant over time (before and after HNC) and across different days of the week and hours of the day.[31]

surprise since NO_x is a highly reactive pollutant (Jorquera, 2002).

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

[30] 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.

[31] In the case of Mexico-City there is not much variation across stations in their average CO levels. Therefore, when we compute the average of CO across stations we do not find significant differences if

In the case of Santiago, the network of stations is operated by the National Environmental Commission (www.conama.cl). Each station also collects hourly measures of CO —and several other pollutants such as ozone, NO_2, NO_x, SO_2, and particulates smaller than 10 and 2.5 micrometers (PM10 and PM2.5, respectively)– as well as hourly measures of temperature, real humidity, precipitation, atmospheric pressure, wind speed, and wind direction. Failure rates are much smaller than in Mexico-City (average failure rate is 9.4%) but there are different patterns *before and after* TS. While the overall failure rate decreased from 6.6% to 4.9% at morning peak hours, it increased from 4.3 to 6.9% at off peak hours. In addition, the unit of measurement in which CO was recorded in each station changed over time: while before TS the concentration level 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.[32]

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

We employ two estimation approaches: (1) a flexible fit that includes a treatment dummy for the whole ex-post policy period and a series of monthly dummies that capture the adjustment phase following implementation and (2) a more structural fit that includes a (linear) trend for the adjustment phase —with its length endogenously determined as part of the estimation process using the structural-break *sup F* method of Quandt (1960), Andrews (1993) and Hansen (2000)— and a dummy for the period that follows the adjustment phase. Approach (1) is more flexible but it is likely to introduce too much noise in the estimation as it may capture idiosyncratic shocks (which are very relevant in our pollution dataset). That is why we prefer approach (2) that imposes a smooth and monotonic adjustment process.[33]

–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.

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

[33] We performed a series of regressions with simulated data in which we assume (i) a DGP for CO hourly concentration levels with a Normal distribution (with mean and variance levels matched to the data), and (ii) an effect of the policy of -15 log points on impact that fades out gradually and linearly

The estimating equations under the two approaches are given by

$$y_t = \alpha + \phi y_t^b + \theta t + \gamma x_t + \beta T_t + \sum \delta_t d_t + \varepsilon_t \quad (1)$$

$$y_t = \alpha + \phi y_t^b + \theta t + \gamma x_t + [a + b(t - t_T)]A_t + cT_t (1 - A_t) + \varepsilon_t \quad (2)$$

where y_t is our dependent variable that for most part corresponds to (the log of) the concentration of CO at morning-peak hours of weekday t (8:00-10:00 am for HNC and 7:00-9:00 am for TS). In the case of HNC, we also estimate policy effects at the city level at off-peak hours (12am–3pm) and Sundays (8–11am).

As explained in section 3.1, in some of our regressions we control for background pollution, y_t^b, which for peak and weekend estimates it corresponds to pollution at night (which is when CO has stabilized: from 2–6 am for HNC and from 1–5 am for TS) and for off-peak estimates it corresponds to pollution at night and at peak. Thus, our estimates attempt at capturing the increase in CO from the plateau we observe at night.[34] To capture pre-existing trends in pollution and car use, we include a linear trend, θt, but also experimented with higher-order ones (quadratic and cubics) with similar results.[35] The vector x_t includes fixed effects (hour of the day, day of the week, month of the year), weather variables and economic covariates that may affect the decision to own and use a car such as real exchange rates and gasoline prices.[36] It also includes SO_2 records, which are readily available from the same monitoring stations. Since SO_2 is mainly related to industrial activity and energy generation,[37] it serves as a control for weather phenomenon common to all pollutants but not well captured by our weather variables such as thermal inversions (Molina and Molina, 2002).

The effect of the policy is captured with different variables: T_t is a dummy that takes the value of 1 after the policy, d_t are monthly dummies for several months after implementation, t_T is the time at which the policy gets implemented, A_t is an indicator function that takes the value of 1 during the adjustment phase. The length of the adjustment phase, $t_A - t_T$ (where t_A marks the end of the adjustment phase) is endogenously

over a period of one year. Results from regressions using (1) show high volatility in the policy dummies (i.e., δ_t's), some of which being statistically significant with the "wrong" sign. This analysis is available from the authors upon request.

[34] We also present regression results in which we do not control for y_t^b and results are similar.

[35] The problem of using higher order trends is that of over-fitting in that we may fit the complete evolution of the dependent variable with a sufficiently high-order polynominal. A discussion of this problem in a RDD context can be found in Dell (2011).

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

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

determined in the following way:

1. We first estimate a, b, and c for different lengths of the adjustment period (we discretize the support of the length of the adjustment phase in 1/2 months from 0.5 to 18 months).

2. We keep those estimates in which we cannot reject the null hypothesis that $a + b(t_A - t_T) = c$.

3. We then follow the structural-break *sup F* method of Quandt (1960), Andrews (1993) and Hansen (2000): Among those lengths of the adjustment process that satisfy the previous criterion, we choose the one with the highest value of the F test for the estimated length of the adjustment phase (notice that $(\widehat{t_A - t_T}) = (\hat{c} - \hat{a})/\hat{b}$).

Thus, the effect of the policy under approach (1) would be $\beta + \delta_1$ on impact (i.e., first month) and β in the long-run while under approach (2) would be a on impact (i.e., first day) and c in the long-run, where the (constant) speed of transit from a to c is b. Finally, ε_t is the error term.[38]

3.3 Results for HNC

We proceed first with estimations at the city level using average concentrations of CO records from the network stations in operation. Column (1) in Table 3.1 presents the results of estimating equation (1) for peak hours (8–10 am) for HNC. We find that HNC decreased CO concentration at peak hours by about 7% within the first month of implementation. While the dummy for a differential effect for month 1 is statistically significant, the total effect is just marginally significant with a p-value of 0.15. As for the long-run, when monthly dummies are zero valued and beyond, we find that HNC has increased CO by about 13%, which is again just marginally significant with a p-value of 0.14. Since the dummies for the first months after implementation are statistically different from 0, we can reject that the effect of HNC during those first months is the same as that over the following months (i.e., long-run). Interestingly, the monthly dummies tend to present a clear pattern of convergence towards 0 which is reached around nine months of implementation.

Columns (2) and (3) of the table present robustness checks to the exclusion, respectively, of y^b and SO_2 from the regressions. While the exclusion of SO_2 produces few changes in the estimated coefficients, the exclusion of background pollution (y^b) leads to a much larger effect in the long-run of about 29%. This captures the fact that HNC

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

also has an impact on CO emissions at off-peak which translates in higher levels of background pollution. But since our interest in these regressions is to identify car use at peak hours, it makes sense to control for background pollution.[39]

As we discussed above, these pollution records are subject to some noise when used in a high frequency format. This would explain the relatively volatile behavior of the monthly dummies which may appear inefficient from an econometric point of view. Thus, in column (1) of Table 3.2 we present the results of regressing equation (2). The effect of the program on impact is now bigger and statistically different from 0: a reduction of 13% the day after implementation. It is not surprising that effect is less than 20% as it captures substitution possibilities, especially from families owning more than one car. In turn, the estimated effects imply an adaptation period of 12.5 months (with a 95% confidence interval including values between 9 and 15 months). The estimated effect after this adjustment phase imply an increase of 13% in CO levels, even though the effect is only marginally significant (p-value of 0.119). As we discussed in the numerical simulations, the 24% net difference between long and short-run effects, which is statistically significant at 1%, can only be explained if agents responded buying not only more cars but also higher-emitting ones.

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

In addition, in columns (1) and (2) of Table 3.3 we present estimates of the short-run effects at peak hours using the RDD approach of Imbens and Kalyanaraman (2012) for monthly (average) observations of CO.[40] Regardless of whether we control for background pollution or not, we find that the RDD estimation of -10% comes very close to the -13% under approach (2). It is worth emphasizing that this RD method allows for arbitrary pre- and post-trends in CO and identifies in a semi-parametric way (using local polynomial regressions and triangular Kernel weights) the effect at the moment of implementation

[39]Notice also that when we exclude y^b the monthly dummies become a bit more volatile (e.g., the dummy for month 11 changes sign and becomes statistically significant) confirming our motivation for including background pollution as a control for unobserved athmospheric phenomena.

[40]The application of RDD in contexts in which the forcing variable is time creates challenges that are not present in the typical micro-econometric application of this technique. As Lee and Lemieux (2008) state "..At this time we are unable to provide any more specific guidelines for analyzing these age/time discontinuities since it seems that how one models expectations, information, and behavior in anticipation of sharp changes in regimes will be highly context-dependent..." One additional aspect not mentioned in Lee and Lemieux (2008) is related to the effect of time idiosyncratic shocks to the dependent variable that may be concurrent with the policy shock. We are working on this in a separate paper.

without relying on explicit parametric assumptions. Thus, the fact that the estimated effect on impact comes very close to our parametric specification (2) is reassuring that parametric assumptions needed to estimate the impact of the policy over the adjustment phase are reasonable. In all, these results come to confirm that HNC had a negative and significant effect on impact at peak hours, with the estimates ranging from -7 to -13%.

We move now to our results for off-peak hours during weekdays in HNC. Following our "peak estimation" logic that a rapid build-up of concentration is likely due to traffic activity, the window we choose for off-peak estimation (12am–3pm) takes advantage of an afternoon hump we identify in the CO concentration profile for Mexico-City (unlike for Santiago, see Figure 3.1). Off-peak estimation involves the additional concern, however, that a potential inertia in CO levels from peak to off-peak hours (recall that a CO emission can remain several hours in the atmosphere or even a few days under low dispersion conditions). If this is so, our off-peak findings may be mimicking those of peak hours without HNC having a causal impact on off-peak concentrations. We handle this in our main estimates by controlling for the pollution level at peak hours of the same day.

Column (4) of Table 3.1 presents the results of estimating equation (1). The pattern of coefficient values are much in line with those in column (1) and with simulations in the next section of the model that predicts analogous patterns for both peak and off-peak hours. Our estimates imply that the effect of HNC on impact is -7.7% (very similar to the -7% we find when we estimate the same equation for peak hours). Interestingly, the control for peak hours is statistically and economically significant (and the control for night pollution is not statistically different from 0). In turn, the effect of HNC after the adaptation period is 8.4%, a bit smaller than the estimated effect for peak hours. In columns (5) to (7) we drop several controls and find that the size and general pattern of the estimated effects is quite robust: a decrease of CO concentration on impact and a gradual increase over the adjustment phase.[41] These results extend to column (2) in Table 3.2, where we allow for the same background control but under the structure of equation (2). Note that the 18.6% net difference between long and short-run effects, which is again statistically significant at 1%, is smaller than that for peak hours which helps explain the somewhat faster adjustment process we obtain at off-peak hours, 8 months with a standard error of about 1 month.

Finally, the last three columns of Table 3.1 presents results for Sundays (8-11am). Looking at Sunday effects is interesting for two reasons: (i) HNC should have little immediate impact since the driving restriction did not operate on weekends (and because some of the weekday trips affected by the restriction are more likely to be moved to other weekdays than to Sundays) and (ii) the increase in the stock of cars to by-pass the weekday restriction should be necessarily reflected in an increase in car use during

[41] For the same reasons of peak hours, the estimated long-run effect at off peak also increases in magnitude as we drop the background control.

Sundays over and after the adjustment phase. In other words, Sunday results provide both a falsification exercise for short run effects, since we should not observe much of a change, and a robustness check for long-run effects, since we have more cars on the street. The results in column (8) are entirely consistent with these observations whether those in previous columns of Table 3.1 or in Table 3.2. Results in columns (9) and (10) confirm the pattern with some minor changes in the estimated values.[42] Finally, we present estimates using the structure of equation (2) in column (3) in Table 3.2. It is worth noting how precisely estimated the long-run effect is and that the length of the adaptation process is between peak and off-peak hours.

In all, our results show, after a period of adaptation of between 8 to 12 months, that HNC has long-lasting impacts on CO and, therefore, on car use. It is interesting that the difference between long- and short-run impacts for peak hours and Sundays is very similar suggesting that the increase in the use of more-polluting cars induced by HNC on periods in which the policy was most binding (peak hours) translates into a similar impact in periods in which it was not (Sundays). Our results also demonstrate that for evaluating policies such as HNC it is important to allow for time varying estimates and to consider heterogeneous effects at different times of the day and of the week.[43]

Our results so far rely on the identification assumption that controlling for linear trends and a vector of observable variables we are able to identify the short and long-run impacts of a policy like HNC. This identification assumption for capturing short-run effects is in the literature (e.g., Davis, 2008) but less so for capturing long-run effects. To validate our identification strategy for HNC we run a simple falsification exercise: we subject our dataset to a "placebo HNC" implemented two years before the actual implementation. So we run regressions using specification (1) with a HNC implemented in November 1987 (including the same vector of control variables and including a two-year window around the date of the policy implementation for peak and off-peak weekday hours and for Sundays). Table 3.4 presents the results. In none of the specifications we find a significant "long-run" effect for the placebo and even though we find some statistically significant monthly dummies, they do not follow a monotonic pattern (as they do in the actual policy). These results suggest that any omitted variables in our regressions should be related to (i) an underlying trend in the dataset, possibly connected

[42] The one change in the estimated coefficients that is worth discussing is the short-term impact of HNC presented in column (10) that jumps to -8.7% when we do not control for background pollution. Besides this effect is actually not different from 0 (with a p-value of 0.31), the same discussion we had before in terms of not controlling for background pollution applies here. Since pollution accumulated at Sunday night/dawn is affected by the effects of HNC on pollution over the previous weekdays, it is not strange to find a negative effect in the short-run when we drop the control for background pollution.

[43] For instance, if we just include a dummy for the post-HNC —equivalent to dropping all the monthly dummies when estimating specification (1)—, we find the following: zero effects at peak and off-peak hours (with insignificant point estimates equal to -0.075 and -0.046, respectively) and a positive effect on Sundays (equal to 0.088). If instead, we do not divide the sample in peak, off-peak, and Sundays and just run a regression with all the observations from 7am to 10pm for the seven days of the week, we find again a zero effect of HNC (with a point estimate of -0.019 and standard error of 0.05).

to income growth, and/or (ii) seasonal patterns that cannot explain our results.[44,45]

The final exercise using CO records explores heterogeneous effects across the city. It is natural to expect transport policies to affect households with different private/public transportation demands in different ways. Here we exploit income variation within Mexico City and CO records from individual monitoring stations distantly located to test whether the response to HNC depends on income (or ex-ante car use[46]) in a way that is consistent with the predictions of the model presented in Section 4.3. 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 (morning) 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 (2).

Table 3.5 provides a summary with the results of the effects of HNC on CO for 10 monitoring stations in Mexico City.[47] We have ordered the stations according to both location (i.e., sector) and the (relative) income level reported in INEGI (1989b) for the representative household living in the county (*delegación*) where the station is located (average income for the entire population has been normalized to 1). We believe that accounting for both income and location gives a better idea of the household wealth. Households living in Plateros and Pedregal, in the Southwest area, exhibit the largest income levels, four times higher than those in the Northeast. The next four columns of the table present estimates of the HNC effects in the short and long run, the difference between the two effects, and the length of the adaptation process. These results are entirely consistent with the predictions of the model in Section 4.3 in that they indicate that HNC has its largest impact (measured by the LR-SR difference) in middle-income neighborhoods, where households were more likely to buy a second car to by-pass the driving restriction, and lowest in high- and low-income neighborhoods.[48]

[44] As we discussed in the Introduction, in the case of TS we implement a more transparent falsification exercise: we run regressions for a city that was not affected by the policy and that has similar characteristics than the area affected by the policy. In the case of HNC, to the best of our knowledge, there is no information available for such a comparable city or group of cities.

[45] Another way of discarding that some unobserved phenomenon explains our results is to notice that our vector of control variables explain 71.8%, 74.9%, and 87.5% of the variation of CO concentration levels before HNC was implemented at peak, off-peak, and Sundays, respectively. These numbers are very high, especially considering that we work with hourly data.

[46] 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.

[47] In some of our estimations, the SO_2 control corresponds to SO_2 records of a close monitoring station.

[48] It is worth mentioning that in the case of Mexico City a non-trivial part of the CO emissions (about 30%) are not produced by passenger and commercial vehicles (the ones affected by the policy) but by other vehicles that are part of the public transportation system (e.g., combis, taxis). This is also evident from the data: while CO levels at peak hours do not vary much from station to station (or county to county), car ownership and income levels do and in a significant way. This probably explains why there is a zero effect on impact in the Xalostoc station.

3.4 Results for TS

Unlike in Mexico-City, data limitations in Santiago allow us to present credible estimates for peak hours only.[49] Table 3.6 presents CO estimates for two slightly different data sets. We proceed first with estimations at the city level using average concentrations of CO records from the network stations operating in each city. Those in column (1) are from a data set in which some of the CO records have been corrected by imputing a value of 0.1145 anytime the observed record at an individual station was below this level. Results in column (2) are based on the original records without any correction for low values. Results for these two columns indicate that TS has had virtually no effect in the first month (with point of estimates of -0.002 and 0.03, respectively) and a positive and large effect of 32 and 31%, respectively, in the long run. As expected, the correction for low values does not seem to have much of an impact (low values of CO concentration are less relevant in peak estimations except for constructing the background pollution level).[50]

The monthly dummies do present a pattern of increasing effects as time passes, but the pattern is more volatile than the one we find for HNC. This suggests that imposing structure to the estimation procedure should give us more meaningful estimates. Table 3.7 contains the results of estimating equation (2) using the same two dependent variables. The estimated coefficients imply a slightly negative effect on impact of TS (with insignificant point estimates of -0.06 and -0.05) and a large and significant effect over the long run, with point estimates of 0.27 and 0.28 (and p-values below 0.01 in both cases). Interestingly, the adaptation period, 6 to 7 months, is somehow faster than in HNC (8 to 12 months). This is not surprising as households in a more developed economy, Chile in 2007 versus Mexico in 1989, are expected to react faster due to better access to financing opportunities (Chah et al., 1995 and Gallego et al. 2001 present evidence of the impact of financial development on liquidity constraints that affect the demand for durable goods –such as cars).[51] Still, the differences in the speed of adjustment are not

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

[50] As in HNC, excluding background pollution and SO_2 has little impact on the estimated coefficients. See results in Columns (3) and (4) of Table 3.6.

[51] While GDP per capita in Mexico in 1989 was \$9,697, in Chile in 2007 was \$13,047 (both figures are in 2005 PPP \$). Similarly, the typical proxies for financial devlopment used in the literature (eg., Levine, 2006) are much higher in Chile in 2007 than in Mexico in 1989, e.g., domestic credit to the private sector (as % of GDP) was 16% in Mexico in 1989 and 88% in Chile in 2007.

that big when we consider the standard errors of our point estimates; so these results tend to be suggestive more than a final proof of this point.[52]

All the other determinants of CO in Table 3.7 present the expected signs: Background pollution presents a big, positive, and significant effect (with a coefficient bigger but of the same order of magnitude as in HNC, which is not surprising because peaks are less pronounced in Santiago; see Figure 3.1); gasoline prices and the real exchange rate have a negative impact; and SO_2 and CO levels are positively correlated. [53]

Columns (3) and (4) in Table 3.3 presents the results of the RDD estimates using the Imbens and Kalyanaraman's (2012) optimal bandwidth method. Consistently with the estimates using our more structural approach we basically find zero effects on impact. However, in this case the control for background pollution proves to be relevant for the point estimates we find and, in column (4) we find a small and negative effect (of about -2%) very similar to the estimates we find in Tables 3.6 and 3.7. Like in HNC, this result gives additional support to our parametric strategy of simultaneously identifying short- and long-run effects.

As a final exercise to validate our city-wide estimates, we now present a falsification exercise in which we subject a monitoring station in the city of Quillota to a placebo policy implemented at the same time as TS (we work with the corrected sample for this and the remaining exercises in the section). Quillota, which is located 130 kilometers from Santiago with a population of about 85,000 people (with 87% of the people living in urban areas), exhibits similar weather and geographic conditions than Santiago: same average temperature (14.4 °C vs. Santiago's 13.9 °C) and somewhat higher average precipitation (457 mm vs. Santiago's 338 mm). Quillota is also to some extent surrounded by mountains so it shares the same ventilation conditions that affect pollution dispersion. More interestingly, Panel A of Table 3.8 presents the correlation matrix of CO records for a number of stations located in Santiago and in regions other than Santiago's Metropolitan Region *before* TS was implemented. Correlation coefficients suggest that Quillota looked much like another station in Santiago (notice that, in contrast, the correlation coefficients for stations located in other regional cities are significantly smaller and in some cases with a negative sign). All this makes Quillota a very good control city to TS.

Next, we present in columns (1) and (2) of Panel B in Table 3.8 the results of a falsification exercise in which we subject Quillota CO records to a placebo TS implemented at the same time TS was implemented in Santiago. Results show no indication of changes in CO levels with TS whether its effect is captured with a single dummy (column 1) or with equation (2) (column 2). In columns (3) and (4), on the other hand, we estimate a difference-in-difference model with Quillota as control city. As expected, we find a sta-

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

[53]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).

tistically significant positive effect of TS that is associated with a 26.4% increase in the long-run. Again, these results provide additional support to the identification strategy we use for TS and suggests it is highly unlikely that some unobserved process (related to either some atmospheric phenomena or increases in car use related to higher income) could explain both the short- and long-run effects of TS.[54]

Finally, we move to estimates of the effects of TS within Santiago. Table 3.9 provides a summary with results of the effects of TS on CO for 7 stations in Santiago. We have also ordered the stations according to the location and the income level reported in CASEN (2006) for the representative household living in the neighborhood (*municipalidad*) where the station is located (average income for the entire population has again been normalized to 1). Given that TS affected the supply of public transport throughout the city, we also include in the table the ratio of bus traffic flows to total flows at peak hours which was computed from a sample of traffic stations located close to the corresponding pollution monitoring station. We think of this ratio as a good proxy of the relative importance of buses over other forms of transportation ex ante (i.e., before TS). Data suggest, as expected, a strong negative correlation between this proxy and household income (the simple correlation is -0.90), which immediately suggests that a household's dependence on public transport varies greatly across the city: from as low as 2% in the rich Las Condes to 13% in the poor Cerro Navia. Closely related, the next column in the table presents a proxy of the change introduced by TS in bus service (i.e., frequency) in the vicinity of each pollution station. It is noticeable that despite the ex-ante differences in bus coverage, frequencies in all neighborhoods fell more or less in the same proportion. Then, variations in the intensity of the TS treatment mostly come from ex-ante differences on how much households depend on public transport.

The last three columns of Table 3.9 present estimates of the TS effects in the short and long run, and the length of the adaptation process. Again, these results are entirely consistent with the predictions of the model in the next Section. The immediate impact of TS is not different from 0 in all the stations (with mostly negative but statistically insignificant effects in the short-run). As for the long-run estimates, there is a strong positive correlation between the size of the coefficient and the ex ante degree of dependence on public transport (and also a negative correlation with household income).[55] Effects are big and precisely estimated in all stations (in four of them above 30%), including rich Las Condes.[56] These estimates at the station level for both HNC and TS not only proved

[54] As in HNC, it is worth mentioning that our vector of control variables explain a significant share of the variation of CO concentration levels before the policy was implemented: the R^2 of a regression of our vector of control variables on CO concentration levels at peak hours is 72.9%.

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

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

to be remarkably consistent with the theoretical predictions we will discuss in the next Section but also served to validate the empirical results we obtained for the complete city.

4 A model of car ownership and use

Can theory explain the empirical results? In particular, can the long-run increase in CO be simply explained by more cars on the street or also by dirtier ones and/or more congestion? Can impacts at peak in TS be consistent with no impacts at off-peak (given that we could not estimate them)? Can differences in income lead to such different effects? Do most households adjusts to these policies in the long-run, or only a few? Can we obtain an estimate of the short- and long-run transport costs inflicted by the policies using few observables like changes in the stock of cars? To answer these and other questions, we develop a novel "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 and income groups.[57] Following the presentation of the model, we calibrate it for both cities using ex-ante (i.e., before the policy) information on car ownership and use. The calibrated model is then used to answer the questions above (we leave answers to the latter two questions to the concluding section).

4.1 Notation

There is a continuum of agents (households) of mass 1 that decide between two modes of transportation —polluting cars and public transport (e.g., buses)— to satisfy its demand for travel during both peak and off-peak hours (we will often refer to peak demand as high (h) demand and off-peak demand as low (l) demand). Households differ in two ways: in their preferences for one mode of transportation over the other (horizontal differentiation) and in the quantity of transportation (e.g., kms traveled, number of trips) they wish to consume (vertical differentiation). Horizontal preferences are captured with a two-dimensional Hotelling linear city. A household's horizontal preferences are 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 $f(x^h, x^l)$.[58]

[57] The model abstracts from longer-run considerations such as migration from and to the city, which in any case we can not estimate empirically.

[58] Obviously this function may depend on income, but this is immaterial for what follows unless we believe policies can directly affect $f(\cdot)$, which we do not. They could, for example, if a policy introduces public transport in a place where it was absent before. But here we interested in changes in the quality, i.e., price, of an existing system.

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 (weekly) trips at peak and off-peak hours, respectively.[59] 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.[60] In turn, we assume that a household that owns two vehicles ($s = 2$) faces no capacity constraints.[61] 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 $i = h, l$. These costs depend on congestion (i.e., aggregate car travel), and agents correctly anticipate that, but we do not need to be explicit about them in this model because we are only interested in the price difference, i.e., $\Delta p^i \equiv p_b^i - p_c^i$ for $i = h, l$, which simplifies the analysis greatly.[62]

A type-(q^h, q^l, x^h, x^l) household enjoys a gross utility of $v(q^h, q^l)$ 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

$$u(\cdot|s) = \begin{cases} v - p_c^h q^h - p_c^l q^l - t^h x^h - t^l x^l & \text{if car for } h \text{ and } l \\ v - p_c^h q^h - p_b^l q^l - t^h x^h - t^l(1 - x^l) & \text{if car for } h \text{ and bus for } l \\ v - p_b^h q^h - p_c^l q^l - t^h(1 - x^h) - t^l x^l & \text{if bus for } h \text{ and car for } l \\ v - p_b^h q^h - p_b^l q^l - t^h(1 - x^h) - t^l(1 - x^l) & \text{if bus for } h \text{ and } l \end{cases} \tag{3}$$

Note that the fourth row in (3) 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 $q^h + q^l > k$.

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

[60] Because of this capacity constraint, 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).

[61] Note that to study the impact of a driving restriction we cannot simply let $s \in \{0, 1\}$ since houselholds that already own a car may either want to buy an additional one or return the one they have.

[62] Take, for example, a transport policy that improves public transportation and, as a result, it also alleviates congestion. Our model captures these changes as reductions in both p_b^i and p_c^i. However, given the structure of the model, the household only cares about Δp^i. Note also that this formulation easily accommodates the fact that car trips are generally longer (or more numerous) than bus trips. In other words, by simply scaling Δp^i we can work with $q_{bus}^i = q^i$ and $q_{car}^i = \vartheta^i q^i$, where $\vartheta^i > 1$ (and equal to all agents). The model also accomodates, by simply adjusting Δp^i, increasing returns in public transport, e.g., that increasing ridership leads to a more frequent service.

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

$$u(\cdot|s) = \upsilon - p_c^i \min\{q^i, k\} - p_b^i \max\{0, q^i - k\} - p_b^j q^j - t^i x^i - t^j (1 - x^j) \tag{4}$$

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 because of the capacity constraint; 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

$$u(\cdot|s) = \upsilon - p_c^i k^i - p_b^i (q^i - k^i) - p_c^j k^j - p_b^j (q^j - k^j) - t^i x^i - t^j x^j \tag{5}$$

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 (5) almost by construction since none of j demand would be satisfied with car trips. We solve this in a simple way; if the car capacity is to be shared, it is done proportional to the demands, i.e., $k^i = q^i k/(q^i + q^j)$ for both $i = h, l$.[63]

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

$$\max_s \{\max u(\cdot|s) - rs\} \tag{6}$$

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, which can be made endogenous to the policy.[64] Implicit in (6) 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).[65] We will come back to this issue below.

[63] 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.

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

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

4.2 Short and long-run choices

We now compute a household's optimal use and ownership choices.[66] 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 4.1, will at best consider buying and using a single vehicle; the ones that do are shown in Figure 4.2(a) (for now, ignore the dotted lines in both Figures 4.1 and 4.2 and the α's in Figure 4.2). 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 4.2(a) consolidates in one place both household's long- and short-run choices. All households with $x^i \leq \hat{x}^i(q^i) \equiv 1/2 + \Delta p^i q^i / 2t^i$ would rather use the car than the bus for i-travel (provided they have one available). And all households with $x^i \leq \hat{x}^i - r/2t^i \equiv \tilde{x}^i(q^i)$, buy a vehicle despite it will only be used for i-travel, i.e., despite $x^j > \hat{x}^j(q^j) \equiv 1/2 + \Delta p^j q^j / 2t^j$. There is fraction of households with weaker preferences for cars, i.e., $\tilde{x}^i < x^i < \hat{x}^i$ for $i = h, l$, which also buy the car because of the "bundle discount" associated to it. The car-bundle discount is exactly equal to r.[67]

We can now use Figure 4.2(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 (note that consideration of a small amount ε is only to facilitate the exposition). 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

$$\alpha^h_1(q^h, q^l) = \int_0^{\tilde{x}^l(q^l)} f(\hat{x}^h(q^h), x^l) dx^l \tag{7}$$

[66] 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.

[67] 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.

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

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

where $\alpha^h_2(q^h, q^l)$ is given by an expression similar to (7) and α^h_3 by

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

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

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

Consider now households with $q^h + q^l > k$. There are four cases to study: groups B, C, D and E in Figure 4.1. 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

$$2r - \Delta p^h q^h - \Delta p^l q^l \geq r - \Delta p^h k^h - \Delta p^l k^l \tag{8}$$

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 (8) 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 (8) for a (single-purpose) vehicle is $q^i \leq k + r/\Delta p^i$ (see Figure 4.1). The fraction of households in group B that effectively end up buying and using the car is shown in Figure 4.2(b). Note that the car-bundle discount continues to be r despite the capacity constraint.

More interestingly, we can now use Figure 4.2(b) to illustrate the short- and long-run effect of a second type of policy intervention: a driving restriction like HNC. Suppose the policy reduces car capacity k by a small amount ε (again, we restrict attention to small changes just to facilitate the exposition).[68] 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 $\iint_B \varepsilon(\Delta p^l k^l/2t^l k)k^l \alpha^l_1 g(\cdot) dq^l dq^h$. Similarly, the third short-run effect, which is captured by the vertical dotted line in the lower-right corner,

[68] Note that a large reduction in k can also affect Δp from changes in congestion (but we omit that here).

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.[69] However, if these households face a transaction cost equal to

$$\lambda \geq \varepsilon \frac{\Delta p^l}{r}, \tag{9}$$

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 4.1, 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.[70] Figure 4.2(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 focuses on the case in which $q^h, q^l \geq k$, say, subgroup C1).[71] 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 4.2(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 4.1, this adverse effect

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

[70] 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.

[71] 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 $q^l < k$.

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 4.2(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).[72] 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 4.2(e), these households never face capacity restrictions (shortly we will come back to the dotted lines in the figure).[73]

4.3 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 4.1.[74] In all numerical exercises, we assume that households' preferences are drawn from uniform distributions, i.e., $f(x^h, x^l) = g(q^h, q^l) \equiv 1$. The bottom half of Table 4.1 presents the calibration parameters obtained for each city.[75] 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, let us replicate the empirical results found for HNC by decreasing car capacity k to 0.20 (all the other parameters remain unchanged). As shown in the first row of Panel A of Table 4.2, this HNC-like policy leads to a short-run decline in car use during peak hours (ΔC^h_{SR}) equal to the 13% reduction in CO concentrations found in the empirical analysis. The short-run decline at off-peak (ΔC^l_{SR}) is a bit higher than the empirical estimates, but the most striking results are the long-run numbers ($\Delta C^{i=h,l}_{LR}$) which are far from the empirical estimates (increases of CO of 11 and 9%, respectively). The long-run inconsistency can be 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

[72]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 $q^h < k$.

[73]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.

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

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

change in the stock of vehicles shown in the last columns of A1 and A2, households would like to return 13.3% of the current stock). If instead we assume that transaction/lemon costs are such that no household returns its car(s), i.e., eq. (9) holds, exercise A2 shows that in the long run the policy leads to a net increase in the stock of vehicles of 4.1%, although still accompanied by a minor decline in car use (e.g., −2.7% in peak hours).

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 unlikely for HNC because of the import of older cars from adjacent regions. Thus, if we also let the additional stock be 2.4 times as polluting (and less fuel-efficient) as the existing ones,[76] the results in ex. A3 match our long-run empirical estimates, which illustrates that they are consistent with the theory once we incorporate these more realistic assumptions. Even though the short-run gains are for most part undone, these exercises show for the case of HNC that this is much less due to increases in car use and congestion (actually they hardly changed with respect to the pre-HNC levels) than to the entry of older and more polluting cars.

Let us now use the model to study the response of different income groups and see how they match the empirical pattern. We can do this by simply varying r —which can be interpreted more generally as the price of cars relative to household income— so as to match ex-ante car use in different municipalities. Exercise A4 extends A3 to a higher-income neighborhood ($r = 0.25$) that exhibits an ex-ante car use of 70% during peak (q^h_{car}/q^h) and 74% during off peak (q^l_{car}/q^l). The effect of the policy is unsurprisingly small compared the city average in A3 because these households have already sufficient car capacity to cope with the driving restriction (the long-run numbers go down to 0.6 and 0.7%, respectively, if we let the additional cars in this higher income neighborhood be equally dirty than the fleet average). In turn, exercise A5 looks at the other extreme by extending A3 to a lower-income neighborhood ($r = 1.3$) 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. One reason the empirical estimates in Xalostoc do not reflect the large reduction of car use in the short run is partly because cars contribution to CO in such poor areas is likely minor relative to the contribution of other sources not affected by HNC (i.e., taxis, buses).[77]

We move now onto 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 4.2 (exercise B1) considers a

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

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

TS-like policy that inflicts a uniform deterioration of 20% in the relative quality of the public transport, i.e., Δp^h and Δp^l go up by that amount in the long-run, so that short and long-run effects ($\Delta C_{SR}^{i=h,l}$ and $\Delta C_{LR}^{i=h,l}$, respectively) are equal to our CO estimates for peak hours (no impact and 28% increase, respectively).[78] Since our empirical analysis of CO records failed to identify effects at off-peak hours, for reasons we explained above, 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 68% at peak and improve by 56% at off-peak.[79] But such a pronounced asymmetric change in quality is unlikely since peak and off-peak services are supplied by the same system. One could argue nevertheless that off-peak service was less affected or at best not at all (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 18.4%, 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 below our empirical estimate of 28% in either case. There are two factors, however, that neither B3 nor B4 account for. Unlike in HNC, the increase in car use could have very well generated additional congestion, more so if at peak hours streets already presented some degree of saturation at the time the policy was implemented.[80] 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, based on an increase in trade of used car (see section 5), is the possible arrival of older and more polluting cars. Exercise B5 extends B4 to incorporate both of these corrections. First, we let the additional vehicles be 24% more polluting than existing ones (consistent with the increase in trade of used cars reported in section 5, this captures that a third of the additional stock corresponds to used cars, some of which quite old),[81] and second

[78]These short-run numbers confirm that families that own a car use it to a maximum extent and only complement it with buses when they are capacity constraint. In fact, according to EOD-2006 for Santiago, most passenger cars in the city (799,811) were already in use to cover an equivalent number of trips (706,518) during the morning hours (6-12am).

[79]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.

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

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

(and consistent with the changes in traffic flows reported in section 5), we let the extra congestion reduce the average speed at peak hours by 8%, which, according to Robertson et al. (1999), should increase CO emissions by a factor of 1.12. With these corrections, the long-run change in CO concentrations at peak hours returns to 28%.

The last two rows of Panel B present the predictions for the effects of TS on households with different income levels (recall from Table 3.9 that service quality went down similarly across the city, so all parameters are as in B5 except r). Thus, exercise B6 extends B5 to a high-income neighborhood ($r = 0.1$) that displays an ex-ante car use of 72% during peak and 81% during off peak. The short run effect is still very small —somehow positive during peak hours because of the excess capacity— but the long-run effect is considerably smaller than the city average, i.e., the one in B5, and close to our empirical estimate of 17% for Las Condes. This is simply because households in this neighborhood rarely use public transportation. Exercise B7, on the other hand, extends B5 to a lower-income neighborhood ($r = 1.5$) that has an ex-ante car use of 8%. Again, the short-run effect is negligible but the long-run effect is substantial (41.5%), which again, is consistent with our empirical findings for low-income counties such as Cerro Navia and Pudahuel.

Although it seems evident that the long run increases in CO must come from additional cars, the model helps clarify that in the case of HNC they happen to emit significantly more than the fleet average and in the case of TS they add to the existing congestion. They model also confirms, in direction and magnitude, the empirical finding that policy impacts vary widely among different income groups. But the model also shows, as the empirical evidence does, how little informative the short-run or immediate impact of a policy, whether is HNC or TS, can be. Exercise B8 illustrates this further for a TS-like policy. A policy that improves the quality of the public transport by 22% during peak hours 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. This can be seen in Figure 4.2(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.

5 Policy effects on other variables

We have presented so far estimates of the impact of both policies on pollution levels and a theoretical model to rationalize and explain these finding. We now expand the analysis to check whether the policy effects we found on CO are also found on other variables related to car use and to the substitution between private and public transportation. In particular, we look at the effects on gasoline sales, number of registered cars (stock of vehicles), sales of new cars, and traffic flows. Such an analysis will serve to validate and complement some our CO results —especially because we can take advantage of control groups (regional trends) we did not have in the CO estimations, except for the case of the city of Quillota for TS— and to provide additional support to the numerical exercises of Section 4. Unfortunately, we restrict the empirical analysis of this entire section to TS (for lack of comparable data for HNC where we could apply the same empirical approach) but we still discuss and contrast similar empirical results that are available for HNC. Summary statistics of variables used in empirical exercises that follow are in Table A.3.

5.1 Gasoline sales

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

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

where y_{it} is the log of the volume of gasoline sales per capita (seasonally adjusted at the regional level using $X-12\ ARIMA$) in region $i = 1, ..., 13$ during month t, T is a dummy that takes the value of 1 for months after TS, S is a dummy that takes the value of 1 for city/region of Santiago, x is a vector of controls that vary by region and time, and θ_i and θ_t are vectors of region and time fixed effects, respectively. The parameter β in (10) captures the differential effect on gasoline sales that we observe in Santiago because of TS, conditional on the other variables included in the regression. The time fixed effects are supposed to capture movements in all the variables that affect symmetrically all the regions and the effects of all the variables that do not vary by region (e.g., general financial conditions or even car prices). We also include as control variables the growth rate of per-capita regional GDP and interactions of the average difference between gasoline and diesel prices interacting with the regional dummies. The evolution of gasoline sales per capita in Santiago and other regions in the country are very similar before TS was implemented: the correlation coefficient of both series is about 0.93. Thus, the time evolution of all the other regions in Chile serves as a control group for the evolution of Santiago.

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. 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.[82]

[82] If we allow the standard errors to also account for arbitrary correlation of errors within regions using a GLS estimator, the standard errors increase to 0.0173 and 0.0198 in columns (1) and (2), respectively. Thus, the estimates are still significant at the 1% level. Notice we do not implement the typical intra-

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.[83] Hence, a 5% increase in gasoline sales is consistent with a 20% increase of CO at peak hours, which is somewhat lower but nevertheless very 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.[84] They find no evidence that HNC reduced gasoline sales; on the contrary, Eskeland and Feyzioglu (1997) find a year-average increase of about 7%. These findings are consistent with our CO results and numerical exercises (A2 and A3) that require a moderate increase in gasoline sales in the long-run to support more circulation during weekends and a similar but less fuel-efficient circulation during weekdays.

5.2 Car registrations and sales

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

We work with two datasets. First, data on registered light vehicles obtained from *Instituto Nacional de Estadísticas* is at the annual and regional level and, following the window of the CO estimations, goes from 2005 to 2009. Data on sales and trades, obtained from the *Servicio de Registro Civil de Chile*, is at the monthly and regional level and cover about the same period: a 49 month window centered at February 2007. For our estimation we employ again a differences in differences model along the lines of (10). As done for gasoline sales, monthly observations of sales and trades were seasonally adjusted at the regional level using $X - 12$ $ARIMA$. Unfortunately we do not have

class cluster because of the small number of clusters we have (just 13 regions). See Angrist and Pischke (2009) for more details.

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

[84] 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).

control variables —in particular, car prices— that vary at the regional level; though time dummies probably capture the evolution of these terms.[85] Again the time evolution of regions other than Santiago serves as control for the TS treatment. The correlation coefficient of the series of the number of registered cars is 0.83 before TS; and for the series of transactions this number is 0.92. Again, regions other than Santiago appear to be a good control group.

Results for changes in the number of registered cars are in Table 5.2. 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 4.2) —in fact, we cannot reject in the model of column (1) an increase in the stock equal to a 5.4% with a p-value of 0.18.

Results for changes in trades of used cars and sales of new cars are in Table 5.2. Columns (1) and (4) present average effects when time effects are common to all regions. We find positive effects on both the trade and sale margins. In the trade margin, our estimate implies an increase of about 10% with respect to the (monthly) average trading volume of used vehicles in Santiago in the two years before TS. In turn, the estimate in column (4) implies a sizeable 30% increase in the (monthly) average sales volume of new cars with respect to the previous two years in Santiago.

These results remain, at least qualitatively, under other specifications. In columns (2) and (5) we allow for differentiated time trends by region. Estimates now decrease in magnitude (to 4.4% and 21.2%) and in the case of trades, the TS coefficient is no 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, which is consistent with the speed of adjustment we find using CO records. 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 ran 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

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

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 7 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 (4%) than the 20% figure in Davis (2008).[86]

5.3 Traffic flows

Despite the problems identified above, in this section we look at the evolution of traffic flows for a restricted sample of 26 of the 46 traffic stations operated by Santiago's *Unidad Operativa de Control de Transito* (UOCT; www.uoct.cl).[87] Our aim here with this information is more qualitative than anything. We would like first to confirm whether TS hit harder in relatively poor areas, and second, to identify whether effects at peak differ from those at off-peak. We proceed first by aggregating the information coming from individual stations into two groups: high-income stations (i.e., with flows registered in stations located in high-income areas) and low/middle-income stations (ie., with flows registered in stations located in low- and middle-income areas).[88]

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

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

where y_t is (the log of) total flows during period (i.e., hour) t, T_t is the TS indicator,

[86] Note, however, that 4% falls in Davis' (2008) ninety-fifth percentile confidence interval (which is quite wide as the effects are not that precisely estimated).

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

[88] We called this group low/middle income areas because, as we discussed in Section 3.1, low-income areas are under-represented in the traffic flow stations. Note also that the aggregation avoid the problems of using specific stations as discussed in section 3.1 and by Daganzo (2007).

x_t is a vector that includes fixed effects (hour of the day, day of the week, month), weather variables, economic covariates, dummies for holidays, dummies for days in which (transitory) driving restrictions were in place, and a set of dummies that control for the opening of several urban highways and extensions of the subway network.[89]

We run regressions for a four-year window centered around the time TS was introduced and differentiating for peak (7–9 am) and off-peak hours (10am–4pm). Unfortunately, station records cannot possibly distinguish between private and public transportation flows.[90] Then, in order to compute changes in private vehicles net of changes in public transportation we do the following. We estimate the percentage decrease in the actual number of buses passing through each traffic station by hour using (i) the number of buses passing through each station before TS, (ii) the actual change in the number of routes passing through each station, and (iii) the estimated decrease in the total number of buses for the whole city. Data for (i) and (ii) comes from Transantiago (www.transantiago.cl). Using data in Briones (2009) and in Muñoz et al. (2009), we compute that the number of buses actually circulating in the city in the first year of TS was, on average, about 27% lower than the pre-TS level. Briones (2009) also argues that due to incentive problems, the effective use of each bus dropped significantly relative to pre-TS levels. Thus, we assume a (probably conservative) reduction in the number of buses actually circulating on the street of 30%. As a robustness check, we also compute changes in bus flows assuming an even more conservative scenario with a reduction of 20%.

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

Table 5.4 presents our estimates for all traffic flows and the two group of stations and for the two reduction scenarios. The estimated effect of TS on total flows at peak hours is about 10% but this estimate is not statistically significant (with a p-value of 0.23). However, we do find heterogeneous effects by type of station: while the impact of TS on private traffic in stations located in high-income areas is very close to 0, the impact in low/middle-income areas is clearly bigger: the increase in private traffic is 14% (and statistically significant). Results for off-peak hours are similar: effects close to 0 in high-income stations and about 5% (but not statistically significant with a p-value of 0.19) for lower income stations. This evidence is consistent with our CO results that the effect of TS is bigger in lower income areas. Regarding differentiated effects between peak and off-peak hours, the imprecision of our estimates —coming from the limitations of traffic data— do not allow us to be too conclusive. That the effects are bigger at peak hours in low/middle-income areas may suggest that the relative price of public transportation

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

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

did increase in both but more in peak than in off-peak. The smaller impact found at off-peak may also explain why we fail to identify CO effects at off-peak hours.

6 Discussion of results

It is clear from our results that both policies not only failed to accomplished their main (long-run) purpose —persuade drivers to give up their cars in favor of public transport— but worse, they induced drivers to buy additional cars (and in many cases more polluting ones). Given that a full-fledge cost-benefit analysis is beyond the scope of the paper, we conclude the paper with a welfare discussion that focuses on the transport costs that these policies have imposed on households by changing the relative prices of the transportation options.

6.1 Estimation of transport costs

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 28% at peak hours, respectively), one may argue that despite the fact that these policies did not work as intended, a large fraction of households were nevertheless able to accommodate to them. And if so, the long-run costs associated to these ineffective policies are perhaps not that large.

An estimate of these transport costs can be obtained with the model in Section 4. Given the functional forms adopted in eqs. (3)–(6), welfare costs are obtained directly as the difference between ex-ante and ex-post household's utilities (i.e., agents' willingness to pay to avoid the policies). But before we can compute these costs we must agree on the most likely effects attributable to these policies as described, for example, by some of the exercises in Table 4.1. Based on our CO estimates, the additional evidence discussed in Section 5, as well as results (not shown) from additional runs of the model, we believe that the numbers in exercise A3/B5 capture reasonably well the impacts of HNC/TS at the city level.

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 city, that is, $r\Sigma_i s_i^0$, where s_i^0 is household i's ex-ante vehicle stock. The first row of the table indicates that in the short run HNC made households in Mexico-City bear losses equivalent, on aggregate, to 5.5% of the value of the current stock. The short-run figure in the case of TS is even higher, 9%, which would amount, in annual terms, to \$120 million (in 2007 U.S. dollars).[91]

[91] This number, which may be even slightly higher since p_c^i was kept constant in the analysis despite

We cannot immediately read from the first row in Table 6.1 that TS was 1.6 times costlier than HNC in the short-run because stock values, relative to total surplus, are not the same. One possible correction is to normalize the ex-ante total surplus in each economy to the same number (by simply adjusting the gross utility υ), which is the same as 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 2.6 times costlier. The next two rows in the table show that this cost difference extends to the long run; but more importantly, 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 6.2% of the households that owned a single car before HNC decided to buy a second one and that only 2.8% of all households in Santiago decided to buy a car (or an extra one) because of TS.

But this is not the full story. Even if a policy prompts a much larger response in terms of additional cars on the street, the long-run losses are still likely to be slightly smaller than the short-run losses. As we increase the policy shock, not only we increase the number of households adjusting to the shock but also the costs borne by those that do not adjust.[92] 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 3, it is unlikely that the transport costs in Table 6.1 are distributed evenly among households of varying incomes. We again use the model to shed some light on this. Table 6.2 reports welfare costs for three groups of households: high-income (as portrayed by exercises A4 and B6 in Table 4.2), middle-income or city-average (numbers are in Table 6.1), and low-income (as portrayed by exercises A5 and B7). 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, 3.5 times as bad as high-income ones.

some increase in congestion, is the product of three variables: $r = 1404$ (see below), $\Sigma_i s_i^0 = 0.945$ million (from INE) and 9.02% (from the model). r was constructed as follows: the average vehicle in the city of Santiago in 2006/2007 was US$6556 in value (from www.sii.cl) and 10.4 years old (from ANAC, see fn. 79). And following conversations with ANAC, we then assumed an annual depreciation for such car of $983 (15%) that divided by 0.7, to account for a 30% additional spending in insurance, taxes, registration and some maintenance, leads to the number above (a similar number is obtained using a 2006 household survey). Unfortunately, we cannot carry out a similar exercise for HNC because of lack of comparable information for 1989. If we use the numbers reported in Davis (2008, p. 78), which are based on a 2005 household survey, we obtain a short run cost for HNC of $132 million annually (in 2006 U.S. dollars); figure that may be seen as an upper bound since the average value of a car running in 1989 was clearly lower than one in 2005.

[92] Take for instance exercise B1 in Table 4.2. The long-run response is quite large, a 18,4% increase in the stock; yet the difference between short- and long-run losses is again small: 27.6 vs 26.1%. Note that this small difference also extends to "good" policies. For example, the short-run (transport) gains in B8 amount to 13.2% while the long-run gains to 13.6%.

6.2 Additional evidence on costs: Taxi medallions

That TS appears much costlier than HNC is not obvious given some of our empirical findings, e.g., comparable impacts in CO at peak hours (i.e., similar short vs long run differences) and in vehicle stocks (increases of 4 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).[93] License prices must then reflect the scarcity rents of operating in markets where there is no free entry. While significantly lower than those in New York City (NYC), license prices in Mexico-City and Santiago were nevertheless positive and comparable at the time HNC and TS were introduced, around US$1000. Moreover, despite taxi rides constitute a small share of all trips in these cities —2 and 1%, respectively—, there are good reasons for license prices to be reliable indicators of the changes in relative prices. One reason is that since these prices represent the present value of a stream of economic rents over an infinite horizon, they should capture, unlike other variables like CO records, gasoline sales and car sales, the long-run effect of the policy. And second, the introduction of both HNC and TS came with no modification in fares nor in the number of licenses,[94] so any change in prices after policy implementation can be largely attributed to it.

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

We carried out a similar analysis of the taxicab market in Santiago. We compiled a novel database of 430 observations of license prices based on each of the weekend's classified advertisements appeared in El Mercurio —Santiago's main newspaper but not the only place where ads are posted[95]— for either taxi licenses or taxicabs for the period

[93] 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.

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

[95] The web site www.rastro.com posts an even larger number of ads but unfortunately they do not

January 2004 through November 2010 . Since 370 of the ads we collected consisted of taxicabs with a single posted price for the vehicle and the license, we proceeded to subtract from the posted price the average price of an equivalent passenger car (i.e., same model and year) advertised the same day. The remaining 60 observations correspond to ads of just taxi licenses. We are aware that the "constructed" observations are probably biased because, among other things, the vehicles we are comparing are not necessarily of the same market value (e.g., taxis are more heavily used). However, since we do not expect the bias to change with TS, this methodology should provide us with an unbiased estimator of the effect of TS on license prices. Summary statistics are in Table A.3.

The evolution of license prices (from the 60 only-license ads) is depicted in Figure 2.4. Prices are quite stable right up to the implementation of TS, which suggests that nobody really anticipated the large impact TS later had. This observation is important for all our empirical estimations that were built on the assumption that agents' adjustments, if any, only begun once the policies were in place. The figure also show a big and relatively quick increase in prices soon after TS.[96] Table 6.3 provides more precise estimates of the effect of TS on a license price. We start in column (1) with an OLS regression of (the log of) license prices on a dummy that takes the value of 1 for observations after TS. The coefficient of TS indicates a large and statistically significant impact of 71%. If we control for the total number of licenses (per capita), the coefficient of TS, as shown in column (2), drops to 56%. Interestingly, the value of -0.91 for the price elasticity of licenses is entirely consistent with the -1.57 value found by Lagos (2003) for NYC medallions, which are traded at much higher prices. As the other columns in the table show, these results are robust to the inclusion of linear trends and/or fixed-effects intended to correct for the potential biases generated during the construction of our sample as well as to the sub-sample of 60 license ads. The coefficients are never below 50% and always statistically significant at conventional levels.[97]

The model in Lagos (2003) can also be used to get a better idea of how much of a demand increase in taxi rides can explain the 50-70% surge in license prices in Santiago. Given that prices in NYC prices are substantially higher than those in Santiago, there is more reason for the taxicab market in Santiago to clear above the "no-frictions frontier" (i.e., a taxidriver's search for a passenger in Santiago must necessarily take longer than in NYC). And if so, the Lagos' (2003) analytical expression for the equilibrium price of

keep track of old records. More importantly, we found no difference between comparable data of the two sources.

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

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

licenses is readily applicable, at least conceptually, to Santiago (recall that regulated fares remained unchanged). A lower bound for the demand increase can be obtained directly from the increase in the licence price, i.e., 50-70%. A second estimate can be taken from the same NYC market: an increase in the medallion price of 50-70% corresponds to a *ceteris paribus* increase in demand of almost 3 times (note that the equilibrium is still above the "no-frictions frontier"). Yet, a third estimate can be obtained if we use the EOD-2006 for Santiago and the numbers in Table 2.1 to get an idea of the aggregate number of taxi meetings (270 per min) and the average duration of a taxi ride (17 min): the increase in demand (i.e., meetings) now is a bit less than 6 times. Based on this range of estimates, one can safely argue that TS has at least doubled the demand for taxicab rides. And because taxis are a relatively expensive mode of transportation whatever the city, these findings are also consistent with the idea that TS was quite costly, and apparently costlier than HNC.

7 Final remarks: Avenues of future research

We learned from HNC that driving restriction policies can be effective in reducing congestion and pollution but only in the short-run. As these policies appear politically feasible —they have been applied in quite a few cities— and are relatively easy to enforce, there is more to be understood on how to design them as to reap the short-run gains without prompting the long-run losses associated to the purchase of additional (higher-emitting) vehicles; perhaps some combination of a permanent ban on older vehicles and a sporadic one on newer vehicles to attack a limited number of episodes/days of bad pollution. Paradoxically, we were able to use HNC to empirically evaluate the short-run benefits of a driving restriction only because HNC became a permanent one. The high volatility of pollution data makes it hard to estimate the daily impact of sporadic policies, using, for example, a regression discontinuity design. Nevertheless, more methodological work needs to be done to figure out the best way to estimate the impact of such policies since they are quite common.

The TS experience, on the other hand, showed how rapidly commuters can abandon public transport in favor of cars after a (permanent) deterioration in the quality of service. But even more successful public transport reforms (e.g., Transmilenio in Bogotá) indicate that it is challenging to persuade drivers to give up their cars. We believe there is a lot more to learn on how commuters decide between public and private transportation and the role different instruments —including road pricing and pollution/gasoline taxes— play in that decision. This is particularly important in cities that exhibit a fast increasing motorization rate, not only for dealing with local problems such as urban air pollution and congestion, but also, with the global problem of climate change.

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Table 2.1: Travel Time after and before 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 at bus stop[b]		21.0	7.1	
Waiting time per connection[b]		6.08	3.65	3.49
Travel time to work (both ways; min.)[c]	76.8	89.7		
Travel time by transportation mode (both ways; min.)[c]				
Public transportation	102.4	133.3		
Private car	65.4	63.4		
Taxi	35.1	33.9		

*S*ources: [a] Subsecretaría de Transporte, Ministerio de Transporte y Telecomunicaciones; [b] DICTUC, several reports; [c] Bravo and Martínez (2007)

Table 3.1: The effect of HNC on CO concentration (flexible approach)

	Peak			**Off-Peak**				**Sunday**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HNC	0.129	0.122	0.289***	0.084*	0.046	0.092*	0.161**	0.143**	0.125*	0.173
	(0.087)	(0.091)	(0.093)	(0.044)	(0.055)	(0.052)	(0.067)	(0.068)	(0.070)	(0.139)
Month 1	-0.199***	-0.173***	-0.350***	-0.161***	-0.118***	-0.187***	-0.269***	-0.109	-0.091	-0.260**
	(0.067)	(0.063)	(0.067)	(0.040)	(0.043)	(0.041)	(0.048)	(0.074)	(0.079)	(0.109)
Month 2	-0.214***	-0.180***	-0.278***	-0.131***	-0.095**	-0.152***	-0.207***	-0.106**	-0.109**	-0.225***
	(0.052)	(0.052)	(0.050)	(0.034)	(0.036)	(0.031)	(0.036)	(0.046)	(0.045)	(0.077)
Month 3	-0.151***	-0.114**	-0.289***	-0.122***	-0.096***	-0.129***	-0.218***	-0.074	-0.065	-0.220***
	(0.046)	(0.048)	(0.050)	(0.033)	(0.031)	(0.032)	(0.032)	(0.046)	(0.044)	(0.073)
Month 4	-0.092*	-0.064	-0.159**	-0.126***	-0.096***	-0.120***	-0.161***	-0.059	-0.024	-0.115
	(0.047)	(0.051)	(0.060)	(0.035)	(0.033)	(0.030)	(0.033)	(0.049)	(0.046)	(0.080)
Month 5	-0.197***	-0.184***	-0.244***	-0.086***	-0.049	-0.126***	-0.160***	-0.101	-0.104	-0.181**
	(0.049)	(0.051)	(0.065)	(0.029)	(0.036)	(0.039)	(0.044)	(0.079)	(0.074)	(0.085)
Month 6	-0.151***	-0.077*	-0.071	-0.031	0.069	0.035	0.033	-0.034	-0.012	-0.045
	(0.039)	(0.040)	(0.052)	(0.038)	(0.046)	(0.051)	(0.059)	(0.054)	(0.052)	(0.109)
Month 7	-0.245***	-0.171***	-0.120**	-0.027	0.062*	0.008	0.036	-0.098**	-0.064	-0.051
	(0.037)	(0.035)	(0.054)	(0.027)	(0.034)	(0.033)	(0.041)	(0.041)	(0.040)	(0.077)
Month 8	-0.166***	-0.104***	-0.027	0.013	0.077**	0.037	0.070*	0.048	0.059	0.110
	(0.039)	(0.036)	(0.040)	(0.030)	(0.032)	(0.032)	(0.036)	(0.044)	(0.042)	(0.075)
Month 9	-0.114***	-0.077**	0.023	0.013	0.108***	0.090**	0.139***	0.054	0.087**	0.204***
	(0.040)	(0.037)	(0.038)	(0.031)	(0.034)	(0.036)	(0.036)	(0.042)	(0.040)	(0.064)
Month 10	-0.064	-0.039	0.062	-0.070*	0.006	-0.005	0.052	0.029	0.046	0.210**
	(0.044)	(0.042)	(0.044)	(0.038)	(0.038)	(0.044)	(0.044)	(0.051)	(0.049)	(0.080)
Month 11	0.021	0.010	0.118**	0.018	0.072*	0.081	0.139***	0.084*	0.082*	0.259***
	(0.050)	(0.048)	(0.056)	(0.044)	(0.042)	(0.049)	(0.048)	(0.044)	(0.044)	(0.070)
Month 12	0.061	-0.046	0.036	0.077	0.010	-0.006	0.034	0.061	0.016	0.128
	(0.075)	(0.053)	(0.082)	(0.058)	(0.041)	(0.054)	(0.068)	(0.044)	(0.040)	(0.076)
y^{night}	0.339***	0.374***		0.058	0.054	0.173***		0.526***	0.535***	
	(0.052)	(0.050)		(0.038)	(0.042)	(0.049)		(0.033)	(0.034)	
SO_2	0.258***			0.289***				0.100**		
	(0.046)			(0.025)				(0.038)		
y^{peak}				0.294***	0.356***					
				(0.035)	(0.040)					
R^2	0.7041	0.6782	0.6187	0.7589	0.7117	0.6612	0.6430	0.8840	0.8799	0.7410
Observations	1872	1872	1874	2811	2811	2835	2884	568	568	568

Notes: The dependent variable is carbon monoxide (CO) level in logs; for Peak it corresponds to 8 to 10 am of all weekdays, for Off-Peak 12 to 3 pm of all weekdays, and for Sunday 8 to 11 am. 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, and SO_2 is sulfur dioxide, y^{night} is the mean concentration of CO (in log) from 2 to 5 am of the corresponding day; y^{peak} is the mean concentration of CO (in log) from 8 to 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 3.2: The effect of HNC on CO concentration (more structural approach)

	Peak	**Off-Peak**	**Sunday**
	(1)	(2)	(3)
Immediate impact (a)	-0.130**	-0.094***	0.022
	(0.051)	(0.029)	(0.037)
Adaptation trend (b)	3.29e-05***	3.40e-05***	2.75e-05***
	(0.935e-05)	(5.83e-06)	(8.59e-06)
Impact after adaptation (c)	0.113	0.092**	0.189***
	(0.081)	(0.041)	(0.043)
Impact difference ($c - a$)	0.243***	0.184***	0.167***
	(0.0578)	(0.0319)	(0.0938)
Trend (θ)	-9.13e-06*	2.83e-06	-7.60e-07
	(4.65e-06)	(2.85e-06)	(3.39e-06)
Real exchange rate	-0.646**	-0.512*	-0.065
	(0.279)	(0.291)	(0.318)
y^{night}	0.313***	0.061	0.532***
	(0.050)	(0.038)	(0.031)
y^{peak}		0.294***	
		(0.036)	
SO_2	0.236***	0.281***	0.092**
	(0.046)	(0.025)	(0.036)
Months of adaptation	12.5***	8.0***	9.5***
	(1.74)	(0.92)	(1.68)
R^2	0.6984	0.7562	0.8816
Observations	1872	2811	568

Notes: See table 3.1.

Table 3.3: RDD estimations using Imbens and Kalyanaraman (2012) with optimal bandwith

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

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

Table 3.4: Falsification exercise for HNC:
Placebo policy implemented two years before HNC

	Peak (1)	**Off-Peak** (2)	**Sunday** (3)
Placebo HNC	0.0271	0.0890	0.1739
	(0.1563)	(0.1301)	(0.1605)
Month 1	-0.0450	-0.0208	-0.0782
	(0.0801)	(0.0804)	(0.0862)
Month 2	-0.0050	-0.1082***	-0.0313
	(0.0225)	(0.0271)	(0.0411)
Month 3	-0.0191	0.0879***	0.0061
	(0.0386)	(0.0249)	(0.0623)
Month 4	0.1240	0.0319	-0.0272
	(0.0844)	(0.0448)	(0.0592)
Month 5	-0.1376***	-0.1021**	-0.0594*
	(0.0395)	(0.0399)	(0.0323)
Month 6	0.0810**	0.0825***	0.1769***
	(0.0352)	(0.0200)	(0.0240)
Month 7	-0.0696***	0.0114	0.0086
	(0.0262)	(0.0146)	(0.0456)
Month 8	-0.0879*	-0.0953***	-0.1066
	(0.0514)	(0.0218)	(0.0653)
Month 9	0.0896*	0.0624***	-0.0576
	(0.0521)	(0.0190)	(0.1132)
Month 10	-0.0191	-0.0106	0.1704*
	(0.0298)	(0.0303)	(0.0946)
Month 11	0.0618	-0.0104	-0.1011*
	(0.0385)	(0.0397)	(0.0558)
Month 12	0.0083	0.0344	0.0189
	(0.0469)	(0.0517)	(0.0598)
R^2	0.7027	0.6139	0.7892
Observations	1558	2384	471

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

Table 3.5: Policy effects by monitoring station: HNC

Station	Sector	Income per HH (relative to average income)	Short-run effect	Long-run effect	Difference LR-SR effects	Months of adaptation	R^2	Observations
Xalostoc	NE	0.55	0.1196	0.1760	0.0564	12.5**	0.6351	1222
			(0.1044)	(0.1118)	(0.0903)	(6.06)		
Tlalnepantla	NW	0.50[a]	-0.2132*	0.0760	0.2208*	9***	0.6455	1474
			(0.1172)	(0.1730)	(0.1240)	(3.1)		
I.M. del Petróleo	NW	0.53	-0.1781***	0.1598	0.3379***	14***	0.666	1209
			(0.0627)	(0.1244)	(0.0910)	(1.91)		
M. Insurgentes	CE	0.70	-0.2458***	0.1427	0.3885***	15***	0.5985	1473
			(0.0727)	(0.1026)	(0.1023)	(2.33)		
Lagunilla	CE	0.71	-0.2821***	-0.0652	0.2169*	11***	0.6204	1403
			(0.0906)	(0.1030)	(0.1145)	(1.78)		
Merced	CE	0.84	-0.1527*	0.0807	0.2334**	12***	0.5425	1588
			(0.0802)	(0.1310)	(0.1057)	(1.52)		
Cerro Estrella	SE	0.54	-0.1781**	0.2037*	0.3818***	11.5***	0.3331	1499
			(0.0857)	(0.1196)	(0.1001)	(1.51)		
Taqueña	SE	1.14	-0.0948	0.2255**	0.3203	15***	0.3258	1381
			(0.0618)	(0.1277)	(0.1011)	(2.41)		
Plateros	SW	1.99	-0.0331	-0.0331	0.0000	0	0.5786	1355
			(0.0973)	(0.0973)				
Pedregal	SW	1.99	-0.0338	0.1378	0.1716**	10.5***	0.5904	1708
			(0.0789)	(0.0789)	(0.0852)	(3.06)		

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

Table 3.6: The effect of TS on CO concentration (flexible approach)

	(1)	(2)	(3)	(4)
TS	0.321***	0.312***	0.278**	0.357***
	(0.075)	(0.078)	(0.103)	(0.121)
Month 1	-0.322***	-0.284**	-0.087	-0.237**
	(0.100)	(0.105)	(0.100)	(0.114)
Month 2	-0.311***	-0.309***	-0.275***	-0.450***
	(0.073)	(0.078)	(0.087)	(0.096)
Month 3	0.020	0.021	0.051	0.141*
	(0.052)	(0.055)	(0.066)	(0.082)
Month 4	-0.220***	-0.202***	-0.143*	0.081
	(0.053)	(0.055)	(0.081)	(0.093)
Month 5	0.012	0.029	0.035	0.101
	(0.064)	(0.064)	(0.079)	(0.116)
Month 6	-0.137	-0.148	-0.174*	-0.040
	(0.087)	(0.095)	(0.100)	(0.108)
Month 7	-0.032	-0.043	-0.104	-0.097
	(0.094)	(0.127)	(0.114)	(0.197)
Month 8	-0.466***	-0.459***	-0.647***	-0.782***
	(0.067)	(0.062)	(0.077)	(0.121)
Month 9	0.087	0.149*	0.010	-0.239*
	(0.095)	(0.082)	(0.095)	(0.141)
Month 10	-0.022	0.119*	-0.036	-0.264***
	(0.060)	(0.063)	(0.072)	(0.083)
y^{night}	0.414***	0.395***	0.494***	
	(0.026)	(0.024)	(0.027)	
SO_2	0.503***	0.497***		
	(0.085)	(0.086)		
R^2	0.7575	0.7580	0.7121	0.6821
Observations	2004	2004	2006	2006

Notes: The dependent variable is carbon monoxide (CO) level in logs corresponding to 7 to 9 am of all weekdays. SO_2 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. y^{night} is the mean concentration of CO (in log) 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 3.7: The effect of TS on CO concentration (more structural approach)

	(1)	(2)
Immediate impact (a)	-0.059	-0.052
	(0.098)	(0.091)
Adaptation trend (b)	8.38e-05***	9.36e-05***
	(2.50e-05)	(2.59e-05)
Impact after adaptation (c)	0.268***	0.283***
	(0.071)	(0.072)
Impact difference ($c-a$)	0.327***	0.335***
	(0.0978)	(0.0986)
Trend (θ)	1.12e-05***	1.02e-5***
	(2.80e-06)	(3.07e-06)
Real exchange rate	-0.493	-0.428
	(0.309)	(0.334)
y^{night}	0.431***	0.408***
	(0.026)	(0.023)
SO_2	0.541***	0.537***
	(0.085)	(0.087)
Months of adaptation	7.0***	6.0***
	(0.86)	(0.73)
R^2	0.7489	0.7486
Observations	2004	2004

Notes: See table 3.6.

Table 3.8: Falsification exercise for TS: Placebo policy implemented in a city not affected by TS

Panel A: CO correlation matrix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Santiago:															
1. La Paz	1														
2. La Florida	0.819	1													
3. Las Condes	0.420	0.584	1												
4. Parque O'Higgins	0.788	0.757	0.169	1											
5. Pudahuel	0.836	0.842	0.394	0.905	1										
6. Cerrillos	0.693	0.771	0.323	0.826	0.888	1									
7. El Bosque	0.854	0.883	0.358	0.890	0.922	0.812	1								
8. Cerro Navia	0.735	0.701	0.170	0.887	0.942	0.860	0.846	1							
Regions:															
9. Temuco	0.111	0.308	0.151	0.003	0.145	0.120	0.108	0.121	1						
10. Con Con	-0.128	0.011	-0.222	-0.144	-0.161	-0.130	-0.065	-0.193	0.222	1					
11. Iquique 1	0.118	0.273	0.121	0.060	0.046	0.110	0.152	-0.013	0.028	0.220	1				
12. Iquique 2	0.246	0.110	0.082	0.041	0.123	0.256	0.106	0.147	-0.255	-0.509	0.013	1			
13. Quillota	0.533	0.724	0.405	0.481	0.619	0.584	0.656	0.521	0.313	0.310	0.336	-0.094	1		
14. Rancagua	0.205	0.146	0.020	0.269	0.267	0.129	0.189	0.329	0.360	-0.101	-0.459	-0.244	-0.133	1	
15. Viña del Mar	-0.262	-0.118	0.180	-0.410	-0.415	-0.421	-0.291	-0.572	-0.087	0.251	0.009	-0.160	-0.136	-0.095	1

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

	Falsification		Diff-in-diff	
	(1)	(2)	(3)	(4)
TS	0.048		0.256*	
	(0.173)		(0.133)	
Immediate impact (a)		-0.013		0.092
		(0.140)		(0.164)
Adaptation trend (b)		-2.64e-05		3.30e-05
		(3.50e-05)		(3.51e-05)
Impact after adaptation (c)		0.078		0.264**
		(0.148)		(0.119)
Trend (θ)	-3.94e-06	-4.82e-06	-7.11e-06	-6.26e-06
	(6.83e-06)	(6.08e-06)	(6.20e-06)	(6.21e-06)
y^{night}	0.885***	0.919***		
	(0.076)	(0.078)		
Δy^{night}			0.576***	0.585***
			(0.047)	(0.048)
SO_2	-0.033	-0.019		
	(0.021)	(0.022)		
ΔSO_2			0.022	0.031
			(0.025)	(0.023)
R^2	0.4746	0.4794	0.4542	0.4714
Observations	1678	1678	1674	1674

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

Table 3.9: 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	Difference LR-SR	Months of adaptation	R^2	Observations
El Bosque	S	0.53	10.8%	-34,60%	-0.1246	0.202**	0.3266**	5.5**	0.5502	1935
					(0.1134)	(0.0898)	(0.1307)	(1.52)		
Cerro Navia	W	0.54	13.0%	-28,10%	-0.0699	0.3979***	0.4678***	7***	0.7252	1726
					(0.1834)	(0.1336)	(0.1218)	(1.25)		
Pudahuel	W	0.65	11.2%	-26,70%	-0.0568	0.3465***	0.4033***	7***	0.7203	1813
					(0.1657)	(0.0845)	(0.1326)	(1.71)		
Cerrillos	SW	0.81	10.5%	-29,30%	-0.0715	0.3580**	0.4295**	9***	0.6269	1492
					(0.1678)	(0.1401)	(0.1843)	(2.33)		
Independencia	N	0.93	6.2%	-30,20%	-0.0288	0.2915***	0.3203**	7***	0.622	1693
					(0.1179)	(0.1051)	(0.1267)	(1.53)		
La Florida	SE	1.06	7.6%	-29,50%	0.0013	0.3228***	0.3215**	5***	0.5907	1887
					(0.1152)	(0.0860)	(0.1341)	(0.73)		
Las Condes	NE	2.45	2.2%	-31,90%	-0.0431	0.1663**	0.2094*	4.5***	0.4841	1900
					(0.0879)	(0.0729)	(0.1151)	(1.11)		

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

Table 4.1: Calibration

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

Table 4.2: Simulations

Exercise	ΔC^h_{SR}	ΔC^ℓ_{SR}	ΔC^h_{LR}	ΔC^ℓ_{LR}	Δ stock
Panel A: HNC					
A1	-12.5%	-12.1%	-8.3%	-8.1%	-9.2%
A2	-12.5%	-12.1%	-2.7%	-2.0%	4.1%
A3	-12.5%	-12.1%	11.0%	12.1%	4.1%
A4	-1.7%	-1.7%	3.9%	4.2%	3.0%
A5	-20.4%	-20.6%	2.3%	3.0%	2.6%
Panel B: TS					
B1	0.0%	0.3%	27.8%	28.1%	18.4%
B2	4.5%	-4.4%	28.2%	-0.1%	11.5%
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%	27.5%	7.1%	5.4%
B6	2.0%	0.0%	18.6%	0.3%	1.6%
B7	0.4%	-0.3%	41.5%	10.1%	9.4%
B8	-0.6%	0.4%	-15.2%	-8.5%	-7.9%

Table 5.1: TS effect on gasoline sales

	(1)	(2)
TS*Santiago	0.058***	0.048***
	(0.0106)	(0.0122)
GDP growth	0.054	-0.013
	(0.0885)	(0.0893)
F-test joint significance Log(PGasoline/PDiesel)		
X Region Dummies (p-value)	0.00	0.00
R^2	0.945	0.957
Observations	936	611

Notes: The dependent variable is seasonally adjusted per capita monthly gasoline sales. TS is a variable equal to 1 after the implementation of the program on February, 2007 and Santiago is a dummy variable. 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 omitted category. Standard errors, in parentheses, are robust to heteroskedasticity. Levels of significance are reported as ***p<0.01, **p<0.05,*p<0.1.

Table 5.2: Registered vehicles

	(1)	(2)
TS*Santiago	120,068***	38,622***
	(4,376)	(10,507)
Region fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Region-year fixed effects	No	Yes
R^2	0.9982	0.9993
Observations	52	52

Notes: TS is a variable equal to 1 after the implementation of the program on February, 2007 and Santiago is a dummy variable. Standard errors in parenthesis. Levels of significance are reported as ***p<0.01, **p<0.05,*p<0.1.

Table 5.3: The effect of TS on car trades and sales

	Trades of used cars			Sales of new cars		
	(1)	(2)	(3)	(4)	(5)	(6)
TS*Santiago	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
R^2	0.9951	0.9955	0.9960	0.9856	0.9864	0.9884

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

Table 5.4: The effects of TS on traffic flows

	Peak hours	Off-peak hours
	Scenario 1: 30% reduction in bus flows	
All stations	0.11	0.03
	(0.09)	(0.05)
High-income Stations	0.00	-0.05
	(0.08)	(0.05)
Low-income Stations	0.14**	0.05
	(0.07)	(0.04)
	Scenario 2: 20% reduction in bus flow	
All stations	0.10	0.03
	(0.08)	(0.06)
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: Standard errors in parenthesis. 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	5.47%	9.02%	1.6
Short-run (corrected)	5.47%	14.30%	2.6
Long-run	5.29%	8.84%	1.6
Long-run (corrected)	5.29%	14.03%	2.7
Long-run (λ=0)	4.71%	14.03%	3

Notes: λ=0 indicates the absence of transaction/lemon costs afflicting used cars.

Table 6.2: Transport costs as a function of income

Income group	HNC (SR)	HNC (LR)	TS (SR)	TS (LR)
Low	2.29%	2.25%	11.39%	11.30%
Middle	5.47%	5.29%	9.02%	8.84%
High	3.27%	2.73%	3.38%	3.25%

Table 6.3: The effects of TS on taxi license prices

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

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

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

Series	Obs	Period	Frequency	Mean	Std. Dev.	Min	Max
Carbon Monoxide	33704	Nov 1987 to Nov 1991	Hourly	5.102	2.110	0.644	20.78
Sulfur Dioxide	33794	Nov 1987 to Nov 1991	Hourly	0.052	0.019	0.012	0.254
Temperature	33378	Nov 1987 to Nov 1991	Hourly	15.94	4.786	0.467	30.77
Real Humidity	32773	Nov 1987 to Nov 1991	Hourly	47.92	20.20	2.300	99.60
Wind Speed	33671	Nov 1987 to Nov 1991	Hourly	4.597	2.032	0.400	17.60
Wind Direction	33677	Nov 1987 to Nov 1991	Hourly	173.3	56.03	1.000	420
Precipitation	35088	Nov 1987 to Nov 1991	Hourly	2.232	4.381	0.000	53.52
Real Exchange Rate	48	Nov 1987 to Nov 1991	Monthly	7.30	0.65	6.28	9.41

Notes: Pollutant levels are reported in parts per million, Temperature in celsius degrees, Humidity in percentage, Wind Speed in kilometers per hour, Wind Direction in azimut degrees, and Real Exchange Rate in Mexican Pesos.

Table A.2: Summary statistics for CO estimations in TS

Series	Obs	Period	Frequency	Mean	Std. Dev.	Min	Max
Carbon Monoxide	34994	Feb 2005 to Feb 2009	Hourly	0.919	1.151	0.000	9.649
Sulfur Dioxide	34944	Feb 2005 to Feb 2009	Hourly	9.258	5.873	0.852	102.7
Temperature	35064	Feb 2005 to Feb 2009	Hourly	14.30	5.18	0.18	31.60
Real Humidity	35064	Feb 2005 to Feb 2009	Hourly	66.44	16.01	13.99	98.01
Wind Speed	35064	Feb 2005 to Feb 2009	Hourly	2.68	1.40	0.20	9.02
Wind Direction	35064	Feb 2005 to Feb 2009	Hourly	187.08	49.98	38.62	302.14
Precipitation	34752	Feb 2005 to Feb 2009	Hourly	0.01	0.09	0.00	4.87
Atmospheric Pressure	34719	Feb 2005 to Feb 2009	Hourly	970.63	14.14	718.53	1021
Real Exchage Rate	120	Jan 2000 to Dec 2009	Monthly	95.5	6.3	81.4	108.8
Gasoline Price	96	Jan 2001 to Dec 2008	Monthly	517.9	517.9	368.4	721.7

Notes: Pollutants concentration is measured in micrograms per cubic meter with the exception of Carbon Monoxide which is measured in parts per million (ppm); Temperature in celsius degrees, Humidity in percentage, Wind Speed in kilometers per hour, Wind Direction in azimut degrees, Precipitation in milimeters, Atmospheric Pressure in milibars, Real Exchange Rate and Gasoline Price in Chilean Pesos.

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

Series	Obs	Period	Frequency	Level of Analysis	Mean	Std. Dev.	Min	Max
Gasoline Sales	1088	Jan2002 to Dec 2008	Monthly	Region	19,166	28,832	1,027	146,875
Taxi license Price	430	Jan 2004 to Nov 2010	Weekly	Santiago	2,629.0	1,202.5	679.0	5,215.1
Car Registration	624	Feb 2005 to Feb 2009	Monthly	Region	1,804.6	3,563.0	67.6	18,390.3
Car Transfers	624	Feb 2005 to Feb 2009	Monthly	Region	3,829.3	6,611.2	189.9	30,263.2

Notes: Gasoline Sales is measured in liters, Taxi license Price in US Dollars; Car Registration and Transfers as well as Car Traffic measure the number of cars. 370 of the 430 observations of the Taxi license Prices are extracted by substracting the price of the equivalent car (same model, year) from the sale price of a taxi including the permit. This is done for every taxi announced during the weekends of each month in the sample. The remaining 60 observations correspond to license-only ads.

Figure 2.1: CO data for HNC

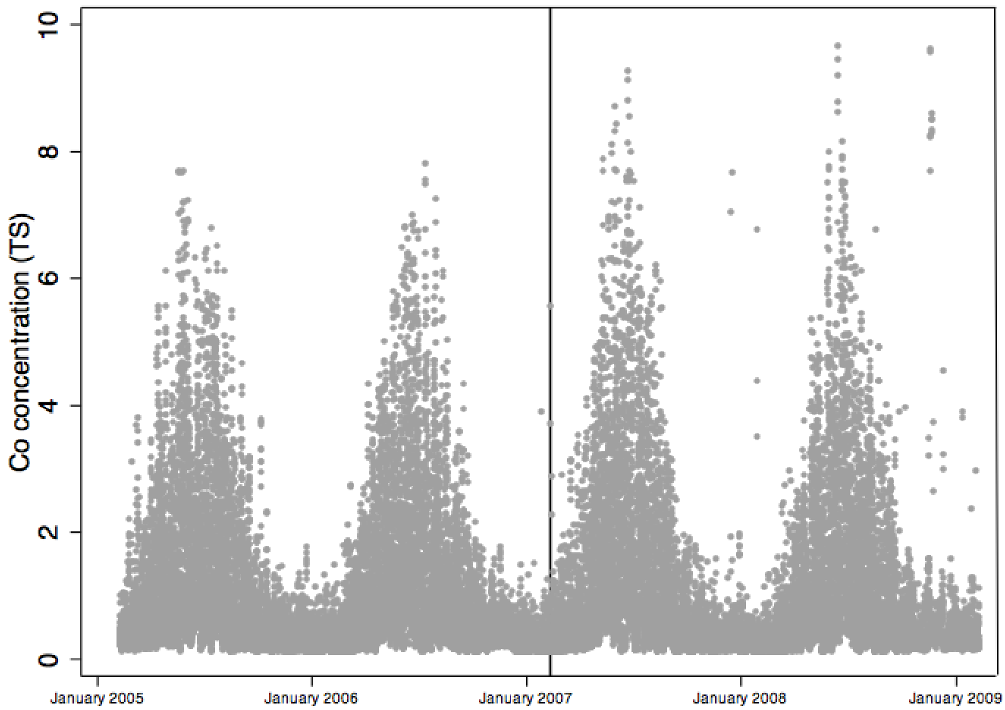

Figure 2.2: HNC short run

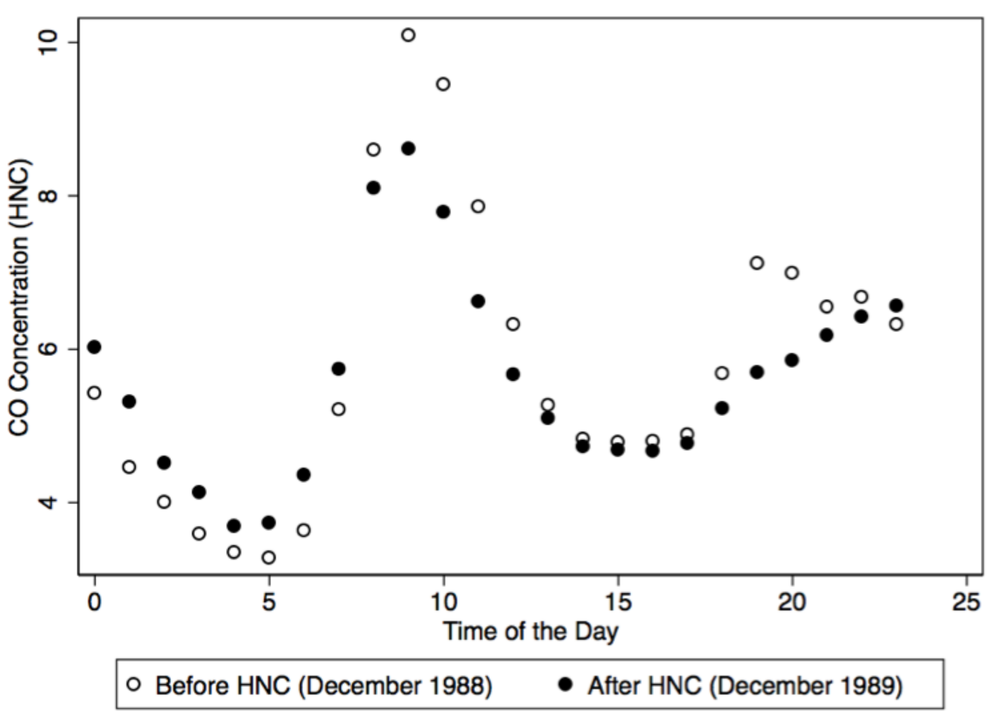

Figure 2.3: HNC long run

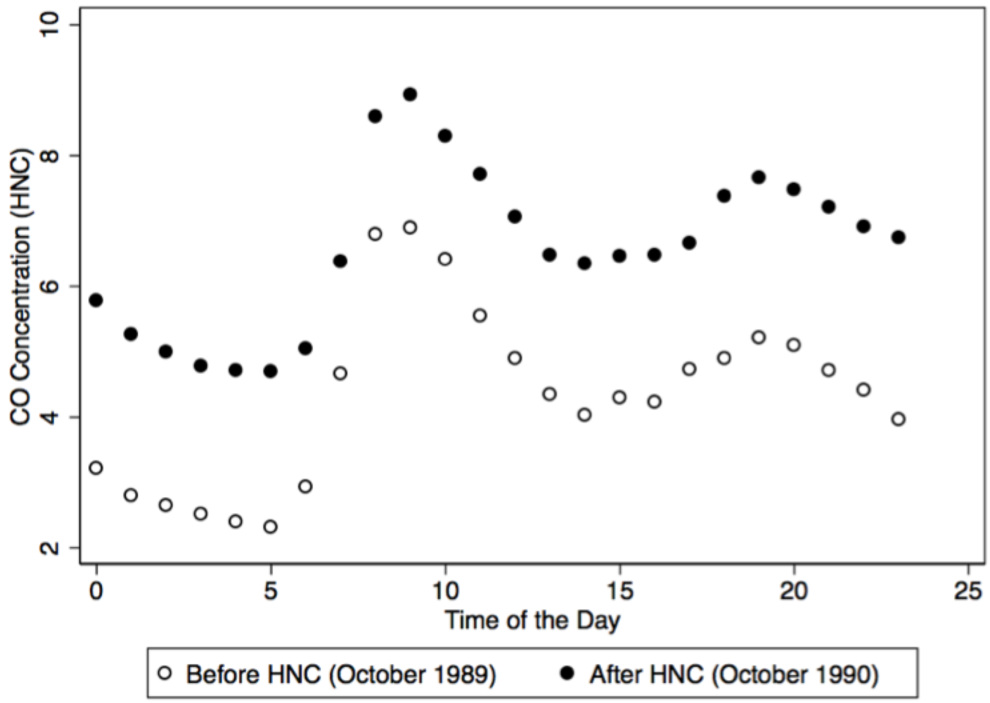

Figure 2.4: Prices of taxi licenses in Santiago (sub-sample of license ads)

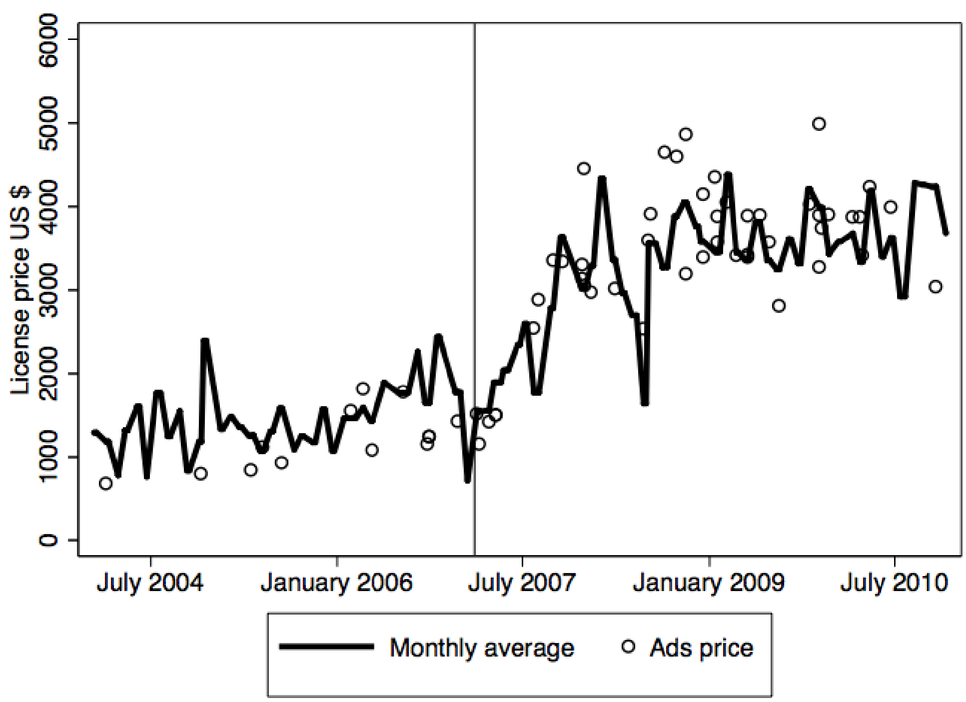

Figure 2.5: CO data for TS

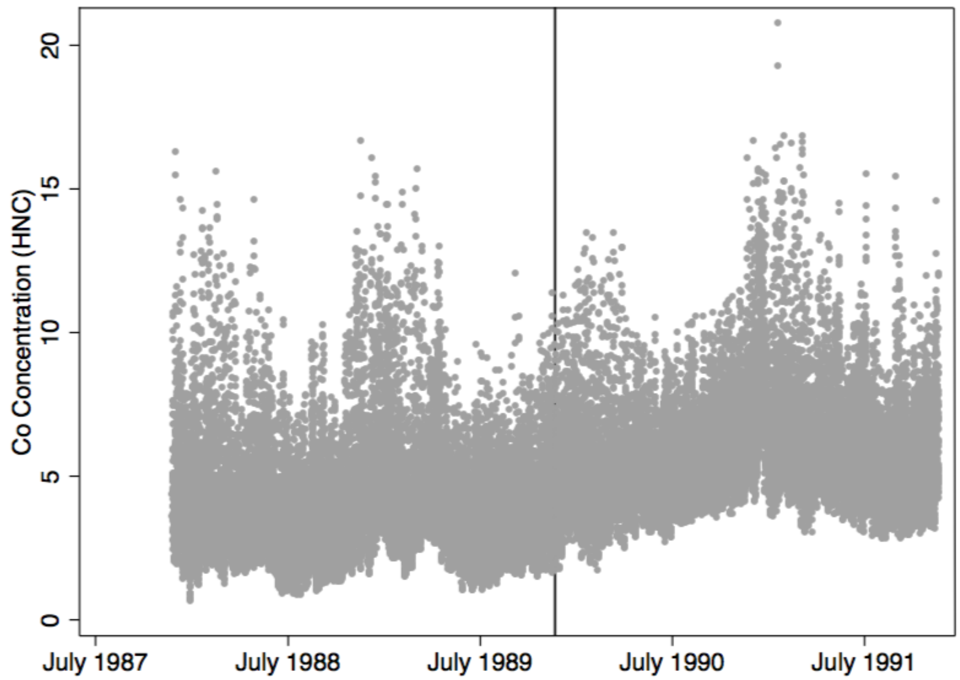

Figure 2.6: TS short run

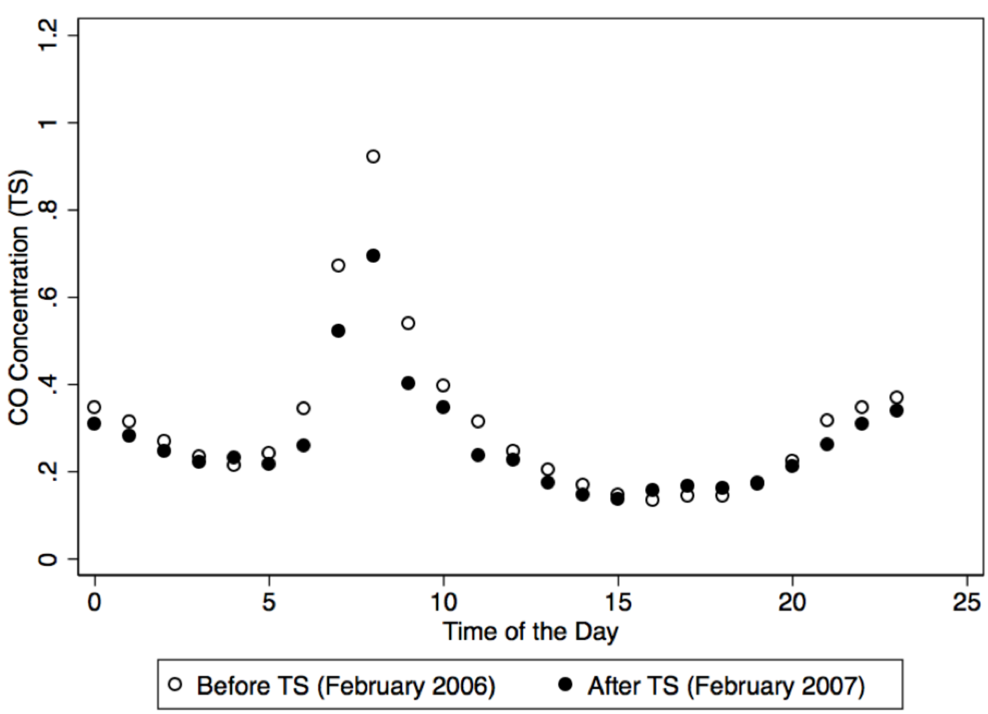

Figure 2.7: TS long run

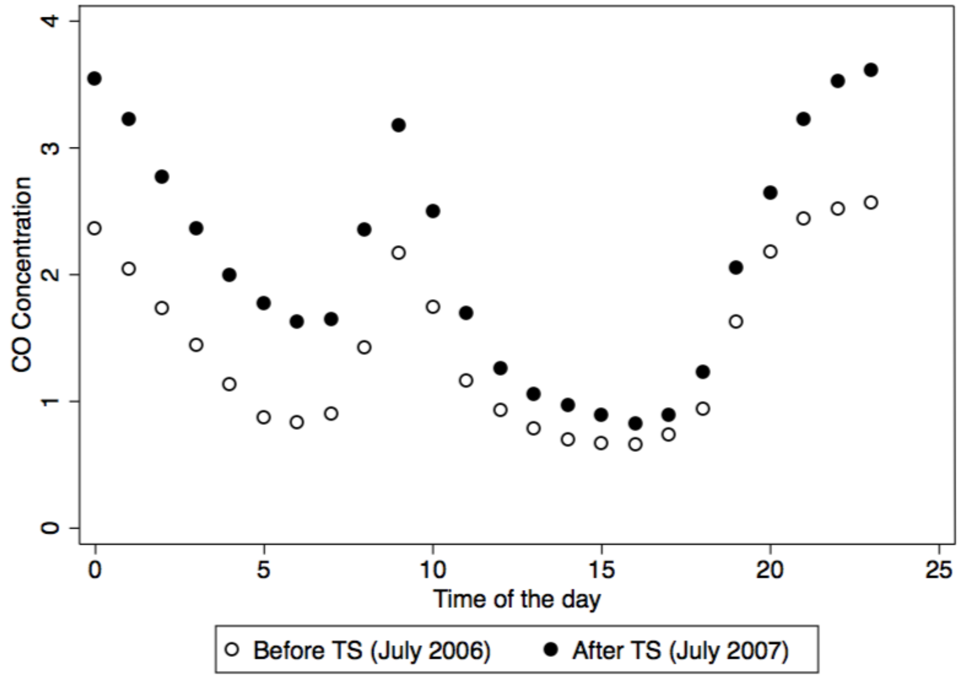

Figure 3.1: CO emissions and concentrations in Santiago (January 2002)

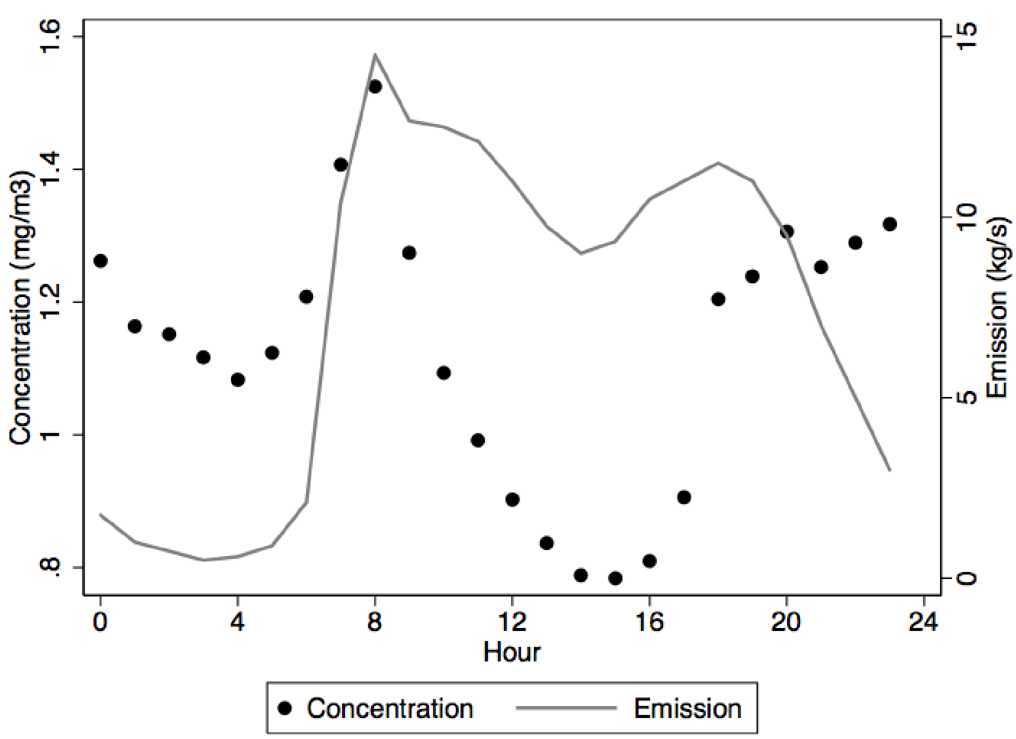

Figure 4.1: Decision to own a vehicle based on vertical preferences

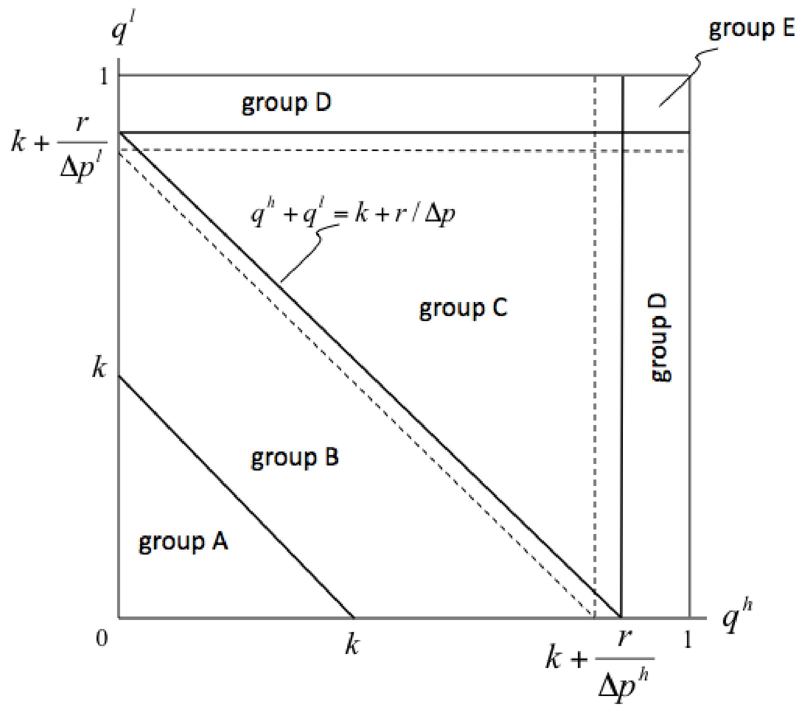

Figure 4.2(a): Households in group A

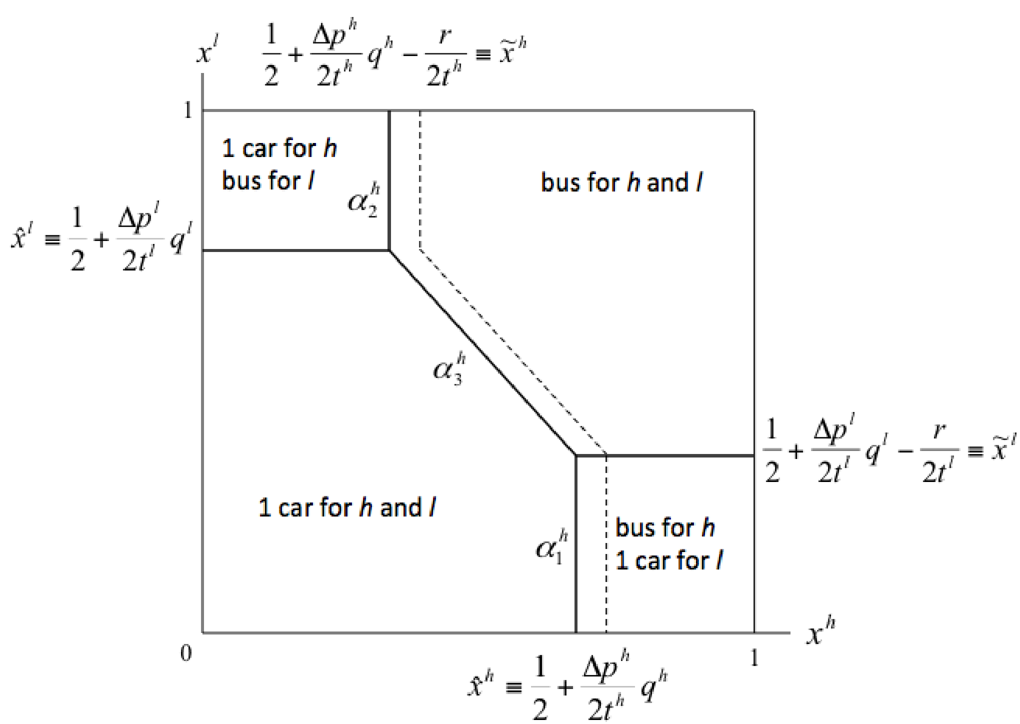

Figure 4.2(b): Households in group B

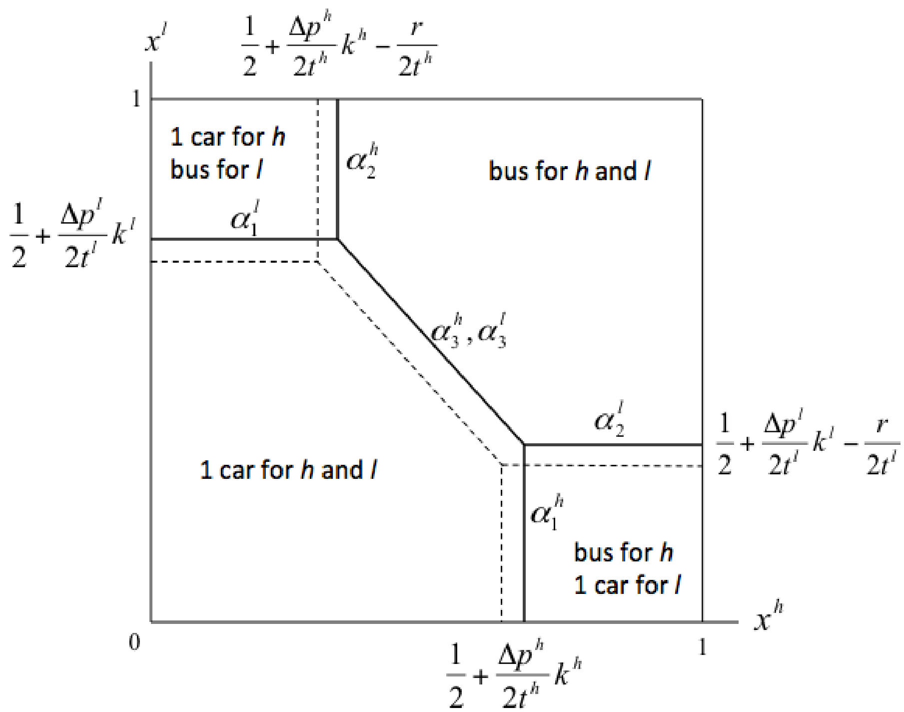

Figure 4.2(c): Households in group C1

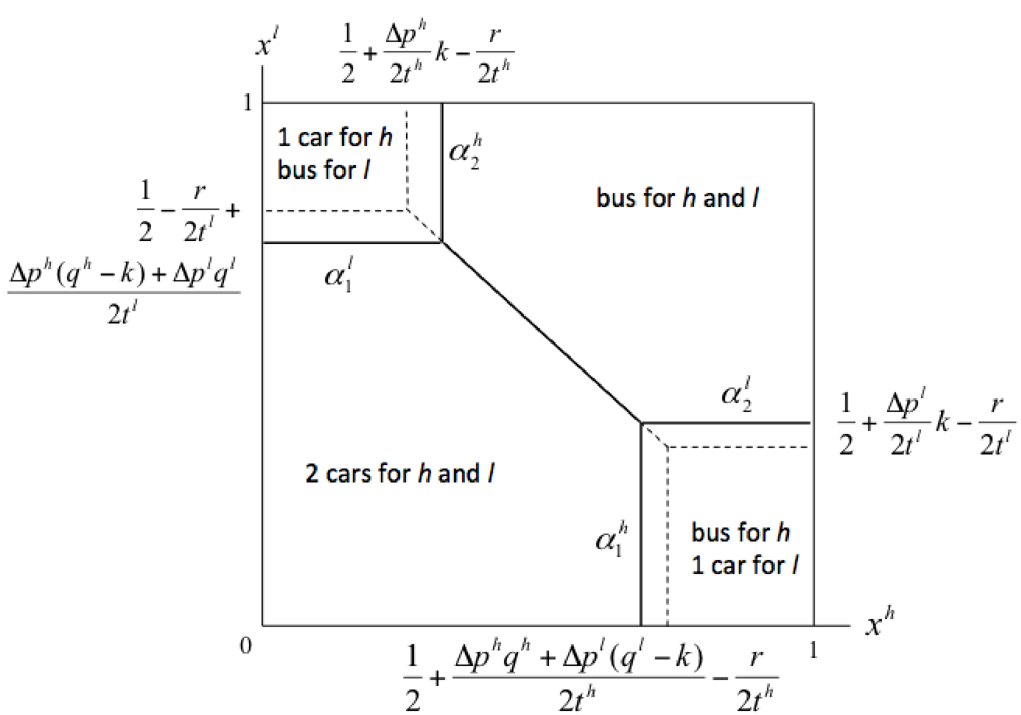

Figure 4.2(d): Households in group D1

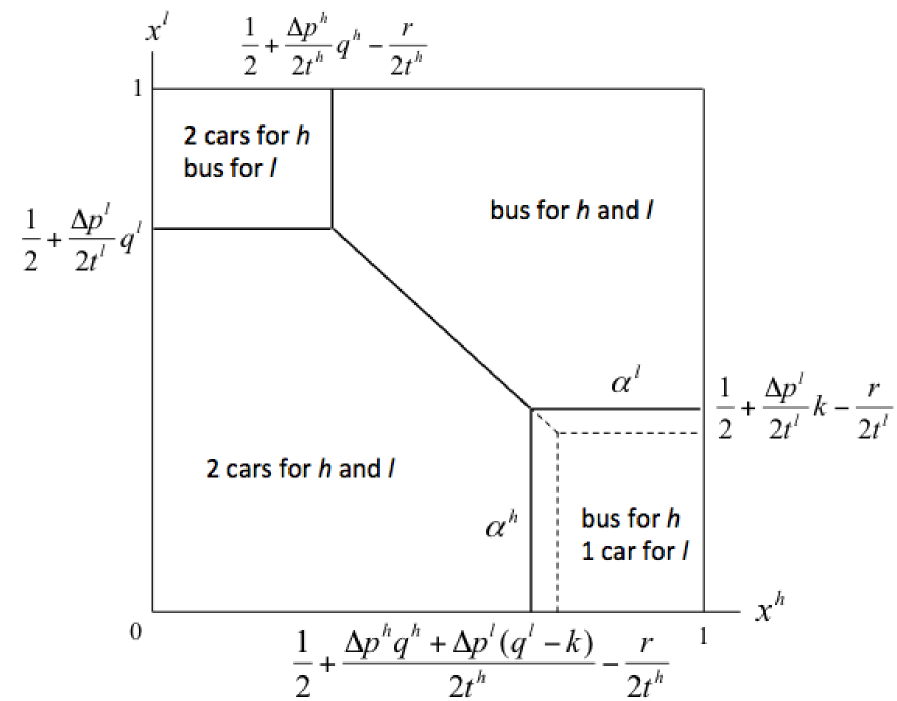

Figure 4.2(e): Households in group E