Online Appendix for: Vintage-specific driving restrictions (Not for publication)

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A.1. Additional figures

Figure A.1: Vintage effects of population

Notes: This figure presents the estimated vintage effects after estimating equation (2) using data at the municipality level for 2006. It presents the coefficients for municipality population. Other coefficients are presented in Figure 2 of the main text. Dark dots represent the point estimates for each coefficient and light gray dots correspond to the 95% confidence intervals using robust standard errors. The vertical line marks the division between 1992 and 1993 vintages.



Figure A.2: Price of a second-hand Toyota Corolla

Notes: The unit of observation in this figure corresponds to a Toyota Corolla newspaper offer published in October, November and December of 1991, 1995 and 1997 respectively. The vertical line separates offers of cars vintage pre and post-1992. The lines are best linear predictor functions of price (in logs) and vintage at each side of the discontinuity.



Figure A.3: Transition phase: Sales of new cars

Notes: This figure shows the number of new cars (q_0) that are sold in every period in Santiago and the rest of the country after the implementation of a regime of Pigouvian taxes. t = 0 corresponds to the time when the tax policy is implemented. Values for t < 0 correspond to the steady state under no intervention.



Figure A.4: Distributional implications of the optimal driving restriction

Notes: This figure shows consumer welfare under two different regimes: no interventions and the optimal driving restriction.



Figure A.5: Vintage effects of driving restrictions, income, and population (no correction)

Notes: This figure presents the estimated vintage effects after estimating equation (1) using data at the municipality level for 2006. Panel (a) presents the coefficients for municipality income, and Panel (b) for the effect of the driving restriction. Dark dots represent the point estimates for each coefficient and light gray dots correspond to the 95% confidence intervals using robust standard errors. The vertical line marks the division between 1992 and 1993 vintages. This figure differs from Figure 2 in the paper in that here we use the original permit-circulation data without correcting for a small number of vintage 1993, 1994 and 1995 not equipped with converters. Obviously, none of these models were in Santiago.

A.2. Additional tables

	88-89	89-90	90-91	91-92	92-93	93-94	94-95	95-96
DR_i	0.0702	0.125^{**}	0.0637	-0.0211	-1.184***	0.0882	0.131^{***}	0.0659
	(0.050)	(0.056)	(0.085)	(0.070)	(0.106)	(0.058)	(0.045)	(0.061)
Controls	Yes^+	Yes^+	Yes^+	Yes^+	Yes^+	Yes^+	$\mathrm{Yes^{+}}$	$\mathrm{Yes^{+}}$
Obs	326	331	331	332	332	332	329	326
R^2	0.107	0.134	0.166	0.079	0.492	0.201	0.047	0.088

Table A.1: The effects of the driving restriction on the share of cars for different contiguous vintages

Notes: OLS regressions with one observation per municipality. The dependent variable corresponds to $\log(q_{\tau})/\log(q_{\tau+1})$, where q_{τ}^i is the total number of cars of vintage τ found in municipality *i* in 2006. Standard errors are calculated via block bootstrap at the province level (53 provinces in total). Municipality controls include, population, income per-capita, quadratic income per-capita, coefficient of variation of income per capita, urbanization ratio, a quadratic function of distance to Santiago, and dummies for municipalities in northern and far away regions. * p < 0.10, ** p < 0.05, *** p < 0.01

	(1)	(2)	(3)	(4)
FIAT	0.031***	0.027^{***}	0.034***	0.027***
UNO	(0.006) [5220]	(0.006) [4705]	(0.007) [4705]	(0.006) [4705]
HONDA	0.127***	0.105***	0.121***	0.122***
ACCORD	(0.008) [10583]	(0.011) [3978]	(0.011) [3978]	(0.011) [3978]
HONDA	0.031***	0.069***	0.054^{***}	0.05***
CIVIC	(0.007) [7281]	(0.007) [5655]	(0.007) [5655]	(0.007) [5655]
MAZDA	0.031***	0.054^{***}	0.055***	0.052***
323	(0.006) [8377]	(0.005) [5576]	(0.005) [5576]	(0.005) [5576]
PEUGEOT	0.033***	0.025***	0.024^{***}	0.021***
205	(0.007) [4285]	(0.008) [3716]	(0.008) [3716]	(0.008) [3716]
PEUGEOT	0.103***	0.138^{***}	0.116^{***}	0.114^{***}
505	(0.008) [11665]	(0.009) [5115]	(0.01) [5115]	(0.01) [5115]
TOYOTA	0.094^{***}	0.17^{***}	0.174^{***}	0.175^{***}
COROLLA	(0.011) [9385]	(0.01) [6564]	(0.012) [6564]	(0.012) [6564]
Age, Model and	Ves	Ves	Ves	Ves
Date f.e.	100	169	105	100
$g(\tau)$	No	Quality	Flexible line	Flexible age f.e.

Table A.2: The effects of having a catalytic converter on the price of used cars

Notes: The unit of observation corresponds to a car offer published in "*El Mercurio*", Chile's main newspaper, the first Sunday of every month between 1988 and 2000. Each row corresponds to estimates of the effect of having a catalytic converter in the context of equation (3) using different specifications for different models. Standard errors clustered by ad date are presented in parentheses. The number of observations in each specification are presented in squared brackets. * p < 0.10, ** p < 0.05, *** p < 0.01

	(1)	(2)
Santiago	-0.0263**	-0.0198*
	(0.009)	(0.009)
Santiago \times pre-1993		-0.0277***
		(0.005)
Vintage f.e.	yes	yes
Date f.e.	yes	yes
Model f.e.	yes	yes
Observations	53915	53915
R^2	0.717	0.717

Table A.3: Prices in Santiago and the rest of the country (2013)

Notes: The unit of observation is a car sell offer posted in "*Chileautos*", an online platform, during October of 2013 of vintages 1990 to 1995. The dependent variable is the posted price of the car (in logs). *Santiago_i* is a dummy that takes the value of 1 is the offer is the car is being sold in Santiago. $pre-1993_i$ takes the value of 1 if the car was built before 1993. We control by vintage, date of the offer and model fixed effects. Standard errors, which are calculated via block bootstrap clustering at the week-region level, are presented in parenthesis. * p < 0.10, ** p < 0.05, *** p < 0.01

	Table A.4:	Survival	rates	and	external	costs	per mile	
(\cdot)	0	1	0		ก	4	۲	

Age (a)	0	1	2	3	4	5	6
γ_a	0.9966	0.9966	0.9966	0.9434	0.8267	0.7226	0.5828
ϵ^r_a	0.0244	0.0397	0.1011	0.2878	0.8225	1.4796	2.1367
ϵ_a^{nr}	0.0028	0.0046	0.0118	0.0336	0.0960	0.1726	0.2493

Notes: Survival rates are calculated using constrained OLS. The unit of observation is the total number of cars of a given vintage τ found in the country on a given year $t(y_a^t)$, which we use in the regression $y_a^t = \gamma_a y_{a+1}^{t+1} + \varepsilon_a^t$, along with imposing $\gamma_a \leq 1$ and $\gamma_{a+1} \leq \gamma_a$. External costs are estimated following the procedure described in section 4.2 of the paper.

Table A.5: The effects of the driving restriction on the share of cars for contiguous vintages (no correction)

	92-93	92-93	92-93	91-92	93-94
DR_i	-0.900***	-0.874***	-0.801***	-0.0211	-0.0560
	(0.065)	(0.075)	(0.098)	(0.067)	(0.059)
Controls	No	Yes	Yes^+	Yes^+	Yes^+
Observations	332	332	332	332	332
R^2	0.300	0.310	0.335	0.079	0.222

Notes: OLS regressions with one observation per municipality. The dependent variable corresponds to $\log(q_{\tau})/\log(q_{\tau+1})$, where q_{τ}^i is the total number of cars of vintage τ found in municipality *i* in 2006. The first three columns correspond to the case of $\tau = 1992$, while columns 4 and 5 correspond to $\tau = 1991$ and $\tau = 1993$, respectively. Standard errors are calculated via block bootstrap at the province level (53 provinces in total). Municipality controls in column 2 include income per capita and population. Municipality controls in column 2 plus quadratic income per-capita, coefficient of variation of income per capita, urbanization ratio, a quadratic function of distance to Santiago, and dummies for municipalities in northern and far away regions. This table differs from Table 1 in the paper in that here we do not correct for a small number of models vintage 1993, 1994 and 1995 not equipped with converters. * p < 0.10, ** p < 0.05, *** p < 0.01

Appendix B: Additional material for Section 2

B.1. Car fleet data

The main database to study changes in fleet composition comes from vehicle circulation permits at the municipality level collected by the National Statistics Bureau. In March every year, each car owner is required to obtain a circulation permit upon payment of an annual fee to her home municipality. We use data for 332 municipalities for the 2006-2012 period. The data includes the number of cars of each vintage by municipality for each year, thus capturing the age profile of the fleet of cars in municipalities around March of each year. The data is available only since 2006, 13 years after the implementation of the 1992 reform.

We also use information for a vector of controls at the municipality level, which include, among others, total population, urbanization rate, mean and coefficient of variation of income per capita at the municipality level, and some additional geographic controls related to location.

Notice that some of the post-1992 cars that circulated outside Santiago were not equipped with a converter. As we do not have information on their exact locations, we correct our estimates using the information from Onursal and Gautam (1997, p. 177), which reports that only 79, 87.6, and 94.8% of all new models registered in 1993, 1994, and 1995, respectively, came with a catalytic converter. If we run the regressions with the raw data (i.e. without applying this correction), our results are qualitatively similar (see Table A.5 and Figure A.5).

B.2. Regression discontinuity design for effects on fleet composition

We implement a regression discontinuity design for cars in Santiago (the treated region) with vintage (τ) as the running variable, where we consider $\tau \geq 1993$ as the treated vintages. It is worth noting that implementing an RDD is challenging in our context. As shown in Figure 1, there is a jump in the stock of cars in Santiago between vintages 1992 and 1993, but similar jumps can be found in other pair of vintages (e.g., for the 1998 and 1999 vintages). These jumps are driven by national level shocks affecting the total number of cars in the country in specific years. To account for this, we first run a regression of the number of cars in each municipality on vintage fixed effects using information for all the municipalities in 2006. By construction, the average across municipalities of the residuals of this regression is 0 for each vintage and therefore we will have a normalized version of the size of each vintage (in terms of the number of cars). Then, we keep the municipalities located in Santiago and run a regression discontinuity design on the residuals of the former regression. Following the usual assumptions of RDDs, this estimator allows us to identify the *local* effect of the treatment on the discontinuity (i.e., the difference in the stock of cars between the 1993 and 1992 vintages) for municipalities in Santiago. We run a local linear regression using a uniform Kernel and a bandwidth of three vintages at each side of the discontinuity to get an estimate of 1.222 and a standard error of 0.201. The graphic implementation can be found in Figure B.1¹



Figure B.1: Regression discontinuity design estimates for car fleet in treated municipalities (Santiago)

Notes: The units of observation are municipalities affected by the driving restriction program. We follow the procedure explained in section 2.1 of the paper. The point estimate of the RDD estimate is 1.222 and the standard error is 0.201.

¹Note that since our running variable is discrete in nature (i.e. vintage, measured in years), we do not follow the existing literature in calculating the optimal bandwidth, as those methods are developed for assignment variables with density, say, $\sigma(x)$, where x is the running variable and $\sigma(\cdot)$ is continuous and bounded away from zero (Calonico et al., 2014). Our results are robust to different bandwidths and Kernel choices.

B.3. Second-car effect

In order to study the potential second-car effect of Santiago's 1992 reform, we use the Socioeconomic Characterization Surveys (CASEN) for years 1998 and 2006. Despite these are national surveys taken every two or three years to thousands of households, these are the only two years when the surveys included detailed questions on car ownership that can be used for our purpose here.

Figure B.2 presents histograms of households owning zero, one or more than one car for each year. Whether in Santiago or in the rest of the country, the majority of households own no car and less than 5 percent own more than one. This latter number already indicates that this margin may not be of first order importance. It does suggest, though, that the fraction of households owning more than one car is twice as large in Santiago as in the rest of the country.



Figure B.2: Histogram with numbers of cars per household

Notes: The figure shows the percentage of households owning 0, 1 or more than 1 car in Santiago and the rest of the country. Panel (a) shows results for 1998, while panel (b) for 2006.

Since living in Santiago (and be affected by the driving restriction) is not the only variable affecting purchasing decisions, we run different regressions to capture how the number of cars owned by a household is affected by living in Santiago along with other household characteristics such as income, assets, age, gender and employment status of the head of the household, the composition of the household (in terms of number of members and also number of employed members), and the size of the municipality where household is located. We employ two types of models to estimate the probability of owning a second car, conditional on owning at least one (i.e., $\Pr[c > 1 | c \ge 1]$, where c is the number of cars in the household). Results for the marginal effect of living in Santiago are presented in the first two rows of Table B.1 for both OLS and probit models, respectively. Results indicate that living in Santiago does not change the probability of having more than one car, after controlling for relevant

variables. This means that the differences seen in Figure B.2 between Santiago and the rest of the country are mostly driven by households characteristics other than location, mainly income.

	1998 survey	2006 survey	
OLS	0.0159	0.00999	
	(0.015)	(0.014)	
Probit	0.0103	0.00310	
	(0.014)	(0.011)	
Hurdle poisson-logit	0.062	0.0136	
	(0.081)	(0.101)	

Table B.1: Effect of living in Santiago on having more than one car

Notes: The unit of observation is the household and the dependent variable is the number of cars in a given household. We present only the marginal effects of living in Santiago but the models also include the following variables: household characteristics related to income, assets, age, gender and employment status of the head of the household, the composition of the household (in terms of number of members and also number of employed members), and the size of the county in which the household is located. OLS and probit estimations are on households with at least one car. The Hurdle poisson-logit model uses all the observations. Observations are weighted using expansion factors. Standard errors, which are clustered at the municipality level, are in parenthesis. * p < 0.10, ** p < 0.05, *** p < 0.01

The second model, a Poisson-logit hurdle model, takes a more structural approach. It is a count model that combines a logit process for the generation of the extensive margin (owning at least one car or not) and a Poisson process for the intensive margin (the actual number of cars). The third row of Table B.1 presents the results of this hurdle model and again we find a statistically insignificant effect of living in Santiago on the probability of owning more than one car, conditional on having at least one. In all, these results show no indication of a second-car effect; and if any, it fully evaporated shortly after policy implementation.

B.4. Effect on car prices - Additional exercises

An alternative empirical strategy is to use a regression discontinuity design with τ as the running variable and, as before, we consider $\tau \geq 1993$ for all models as the treated vintages. Following the same approach used for the RDD for quantities in Section B.2, we start calculating residuals from a regression of log prices on date of the offer fixed effects, model fixed effects, age of the car fixed effects, and our proxy for car quality. We do this for the pooled sample and for each model. Thus, under the usual assumptions, this estimator identifies the *local* effect of the treatment on the 92-93 discontinuity. Let β_{RDD}^m be this estimator. As in the case of our estimates for the effects on quantities, we run a local linear regression using a uniform kernel and a bandwidth of 3 vintages on each side of the discontinuity. We obtain a point estimate of 0.061 with a standard deviation of 0.004 (see Figure B.3). This number is remarkably similar to the estimate we find using our parametric approach in equation (3), i.e., 0.065.



Figure B.3: Regression discontinuity design estimates for the effect of a catalytic converter on prices of used cars

Notes: The units of observation are car ads published in "*El Mercurio*", Chile's main newspaper, the first Sunday of every month between 1988 and 2000. We follow the procedure explained in section 2.2 of the paper. The estimates (standard errors) for each individual model are as follows: FIAT UNO: 0.005 (0.007), HONDA ACCORD: 0.089 (0.012), HONDA CIVIC: 0.062 (0.008), MAZDA 323: 0.057 (0.005), PEUGEOT 205: 0.035 (0.009), PEUGEOT 505: 0.073 (0.011), TOYOTA COROLLA: 0.211 (0.024).

Finally, we run price regressions using newspaper ads for Honda Accords, exploiting the fact that some pre-1993 models were already equipped with catalytic converters, and therefore, exempted from the restriction. This exercise is important as one may argue that 1993 models could be more expensive than 1992 models not because of the driving restriction, but because of a discrete jump in quality or costs between these two vintages (note that this concern is not relevant as long as our proxy for the quality of cars of different vintages is related to the true variable). This is unlikely to be the case for models of the same make and of the

same year. We exploit the fact that in many instances this feature of the car (i.e. having a catalytic converter) was explicitly reported in the ads along with the price quote. So we run the following cross-section regression

$$\log(P_i^{HA,\tau}) = \beta_{\tau}^{HA} CONVERTER_i + \varepsilon_i \tag{B.1}$$

where $P_i^{HA,\tau}$ refers to the price of a Honda Accord of vintage τ , and $CONVERTER_i$ is a dummy that takes a value of one if the ad reports that the car has a catalytic converter. We test for the effect of reporting a catalytic converter on the price offer by running four separate OLS regressions for vintages 1991 through 1994 using ads published in October, November, and December of 1995. This provides an additional test that exploits the fact that since converters were required by law in all post-1992 models, reporting its existence in ads for these models should make no difference.

This is precisely what we see in the last two columns of Table B.2, where having a catalytic converter is not statistically different from zero. This contrasts with the catalytic premiums observed in the first two columns of the table. Moreover, we can compare the estimate of β_{τ}^{HA} with the estimate provided for the same model using our previous empirical strategies, as a robustness check. Albeit somewhat larger, the 1991 and 1992 premiums are not that different from the 12 log point premium reported in Table A.2 for the same model.

	(1991)	(1992)	(1993)	(1994)
CONVERTER	0.223^{***}	0.189^{***}	0.0206	-0.00487
	(0.054)	(0.035)	(0.035)	(0.010)
Constant	15.60***	15.68***	15.96***	16.40***
	(0.032)	(0.034)	(0.025)	(0.010)
Observations	47	53	58	49
R^2	0.245	0.309	0.006	0.001

Table B.2: The effects of reporting a catalytic converter on the price of used Honda Accords

Notes: Each observation corresponds to a Honda Accord newspaper-ad published in October, November and December of 1995. The dependent variable is posted price of the car (in logs). We present the estimates of a dummy that takes a value of 1 if the ad reports a catalytic converter is installed in the car in the context of equation (4). Each column presents the results for the different car vintages. Robust standard errors are presented in parenthesis. * p < 0.10, ** p < 0.05, *** p < 0.01

B.5. Proxy for car quality

To construct our proxy of car quality, we assume that prices of new cars are a good approximation of the cars' intrinsic value. We see this assumption as particularly relevant for Chile, where there are no car manufacturers and the import of used cars has been forbidden since 1985. Therefore, relative differences in prices of new cars depend on international prices and do not depend on local demand shocks (in contrast to what happens in the second-hand market, which operates as a close economy). Under that assumption, we construct our proxy for the quality of the model of a specific vintage as follows. First, we run a regression of log prices of all the ads in our sample against date of the offer fixed effects, model fixed effects and age of the car fixed effects. Then, we average the residuals of that regression by vintage and model using only new car offers. We use the averaged residuals as a proxy for the vintage-model specific quality of a car.

APPENDIX C: ADDITIONAL MATERIAL FOR SECTION 3

C.1. Proof of Proposition 1

The solution that maximizes (21) is defined by four variables: (i) the last driver to rent a new car, θ_0^* , (ii) the last driver to rent an old car, θ_1^* , (iii) the travel schedule for new cars, $x_0^*(\theta)$, and (iv) the travel schedule for old cars $x_1^*(\theta)$. From (21), the four first-order conditions that determine these four variables are, respectively

$$0 = (c - 2v)f_p(\theta_0) - \left(\frac{\alpha}{\alpha - 1}\theta_0^* s_0 \left[x_0^*(\theta_0^*)\right]^{(\alpha - 1)/\alpha} - (\psi_0 + h_p e_0)x_0^*(\theta_0^*)\right)f_p(\theta_0) + \left(\frac{\alpha}{\alpha - 1}\theta_0^* s_1 \left[x_1^*(\theta_0^*)\right]^{(\alpha - 1)/\alpha} - (\psi_1 + h_p e_1)x_1^*(\theta_0^*)\right)f_p(\theta_0)$$
(C.1)

$$0 = v f_p(\theta_1) - \left(\frac{\alpha}{\alpha - 1} \theta_1^* s_1 \left[x_1^*(\theta_1^*)\right]^{(\alpha - 1)/\alpha} - (\psi_1 + h_p e_1) x_1^*(\theta_1^*)\right) f_p(\theta_1)$$
(C.2)

$$0 = \theta s_0 [x_0^*(\theta)]^{-1/\alpha} - \psi_0 - h_p e_0$$
(C.3)

$$0 = \theta s_0 [x_1^*(\theta)]^{-1/\alpha} - \psi_1 - h_p e_1$$
(C.4)

Now consider a driver θ of vehicle $a \in \{0, 1\}$ facing a Pigouvian tax $h_p e_a$ per mile driven. It is easy to see that this driver will drive $x_a^*(\theta)$ miles, where $x_a^*(\theta)$ is given by either (C.3) or (C.4). Anticipating this, the driver that is indifferent between renting an old car and taking public transport is θ_1^* , as given by (C.2), and the one that is indifferent between renting an old car and a new car is θ_0^* , as given by (C.1).

C.2. Proof of Proposition 3

The planner's problem is to choose the restriction upon new vehicles, $R_0 \leq 1$, the number of old vehicles in circulation, q_1 (which can be interpreted as a restriction of $R_{1-} = 0$ upon the old vehicles that do not make the quota q_1), and the restriction upon the old vehicles that remain in circulation, $R_{1+} \leq 1$, so as to maximize

$$W = -c[F_p(\bar{\theta}) - F_p(\theta_0)] + v[F_p(\bar{\theta}) - 2F_p(\theta_0) + F_p(\theta_1)] + \int_{\theta_0}^{\bar{\theta}} R_0 \left[\kappa_0(\theta s_0)^{\alpha} - h_p e_0 \left(\frac{\theta s_0}{\psi_0}\right)^{\alpha} \right] f_p(\theta) d\theta + \int_{\theta_1}^{\theta_0} R_{1+} \left[\kappa_1(\theta s_1)^{\alpha} - h_p e_1 \left(\frac{\theta s_1}{\psi_1}\right)^{\alpha} \right] f_p(\theta) d\theta$$
(C.5)

where θ_0 is obtained from the indifference condition (between renting a new and an old vehicle)

$$R_0 \kappa_0 (\theta_0 s_0)^{\alpha} - p_0 = R_{1+} \kappa_1 (\theta_0 s_1)^{\alpha} - p_1$$
(C.6)

and θ_1 is a function of R_{1+} and q_1 to be obtained from either the indifference condition (between renting an old vehicle and taking public transport)

$$R_{1+}\kappa_1(\theta_1s_1)^\alpha - p_1 = 0$$

or the "rationing" condition

$$q_1 = F_p(\theta_0) - F_p(\theta_1)$$

whichever is greater. Using $p_0 = v - c$ and $p_1 = v$, these cutoff functions can be written as

$$\theta_0 = \theta_0^{vr}(R_0, R_{1+}) = \left(\frac{c - 2v}{\kappa_0 R_0 s_0^{\alpha} - \kappa_1 R_{1+} s_1^{\alpha}}\right)^{1/c}$$

and

$$\theta_1 = \theta_1^{vr}(R_{1+}, q_1) = \max\left\{F_p^{-1}(F_p(\theta_0) - q_1), \left(\frac{v}{\kappa_1 R_{1+} s_1^{\alpha}}\right)^{1/\alpha}\right\}$$
(C.7)

where superscript "vr" denotes vintage restriction.

The determination of θ_1 deserves some explanation. If it is optimal to set R_{1+} relatively close to 1, the last driver to rent a car in the market will be θ'_1 , which solves $R_{1+}\kappa_1(\theta'_1s_1)^{\alpha} = v$. But when R_{1+} is close to 1 the social value created by this last driver is less than the scrappage value of the car, that is

$$R_{1+}\kappa_1(\theta_1's_1)^{\alpha} - h_p R_{1+}e_1\left(\frac{\theta_1's_1}{\psi_1}\right)^{\alpha} < v$$

There is an excess of old cars in the market, which the restriction design fixes by setting the quota q_1 so that the social value of the last car in the market is exactly equal to its outside option. In other words, q_1 must be such that $\theta_1 = F_p^{-1}(F_p(\theta_0) - q_1)$ solves

$$R_{1+}\kappa_1(\theta_1 s_1)^{\alpha} - h_p R_{1+} e_1 \left(\frac{\theta_1 s_1}{\psi_1}\right)^{\alpha} = v \tag{C.8}$$

and $\theta_1 > \theta'_1$ (note we are relying on efficient rationing to sort out drivers because old cars are all equal; in the general model we do not need this because old cars are differentiated by vintage). As R_{1+} happens to be lower in the optimal design, the excess of old cars in the market reduces and the quota q_1 may not longer be needed. The exact point when this latter occurs is when the private value of using a restricted old car is exactly equal to its social value without restriction, that is, when

$$R_{1+}\kappa_1(\theta_1s_1)^{\alpha} = \kappa_1(\theta_1s_1)^{\alpha} - h_p e_1 \left(\frac{\theta_1s_1}{\psi}\right)^{\alpha} = v$$

We consider both cases in the proof.

Consider first the case in which q_1 is active, i.e., θ_1 is dictated by the first term in (C.7). Differentiating (C.5) with respect to R_0 yields the first-order condition

$$0 \leq (c-2v)f_{p}(\theta_{0}^{vr}(\cdot))\frac{\partial\theta_{0}^{vr}(\cdot)}{\partial R_{0}} + \int_{\theta_{0}^{vr}}^{\bar{\theta}} \left[\kappa_{0}(\theta s_{0})^{\alpha} - h_{p}e_{0}\left(\frac{\theta s_{0}}{\psi_{0}}\right)^{\alpha}\right]f_{p}(\theta)d\theta$$
$$-R_{0}\left(\kappa_{0}(\theta_{0}^{vr}s_{0})^{\alpha} - h_{p}e_{0}\left(\frac{\theta_{0}^{vr}s_{0}}{\psi_{0}}\right)^{\alpha}\right)f_{p}(\theta_{0}^{vr}(\cdot))\frac{\partial\theta_{0}^{vr}(\cdot)}{\partial R_{0}}$$
$$+R_{1+}\left(\kappa_{1}(\theta_{0}^{vr}s_{1})^{\alpha} - h_{p}e_{1}\left(\frac{\theta_{0}^{vr}s_{1}}{\psi_{1}}\right)^{\alpha}\right)f_{p}(\theta_{0}^{vr}(\cdot))\frac{\partial\theta_{0}^{vr}(\cdot)}{\partial R_{0}} \tag{C.9}$$

And using the indifference condition (C.6), (C.9) reduces to

$$0 \leq \int_{\theta_0^{vr}}^{\bar{\theta}} \left[\kappa_0(\theta s_0)^{\alpha} - h_p e_0 \left(\frac{\theta s_0}{\psi_0}\right)^{\alpha} \right] f_p(\theta) d\theta + h_p \left(R_0 e_0 \left(\frac{\theta_0^{vr} s_0}{\psi_0}\right)^{\alpha} - R_{1+} e_1 \left(\frac{\theta_0^{vr} s_1}{\psi_1}\right)^{\alpha} \right) f_p(\theta_0^{vr}(\cdot)) \frac{\partial \theta_0^{vr}(\cdot)}{\partial R_0}$$
(C.10)

On the other hand, differentiating (C.5) with respect to R_{1+} and using (C.6) and (C.8) yields

$$0 \leq \int_{\theta_1^{vr}}^{\theta_0^{vr}} \left[\kappa_1(\theta s_1)^{\alpha} - h_p e_1 \left(\frac{\theta s_1}{\psi_1}\right)^{\alpha} \right] f_p(\theta) d\theta + h_p \left(R_0 e_0 \left(\frac{\theta_0^{vr} s_0}{\psi_0}\right)^{\alpha} - R_{1+} e_1 \left(\frac{\theta_0^{vr} s_1}{\psi_1}\right)^{\alpha} \right) f_p(\theta_0^{vr}(\cdot)) \frac{\partial \theta_0^{vr}(\cdot)}{\partial R_{1+}}$$
(C.11)

Finally, differentiating (C.5) with respect to q_1 yields precisely (C.8), which we already used to reach (C.11).

The values of R_0 and R_{1+} correspond to one of these three possibilities: (i) $R_0 < 1$ and $R_{1+} < 1$, (ii) $R_0 < 1$ and $R_{1+} = 1$, and (iii) $R_0 = 1$ and $R_{1+} \leq 1$. If possibility (i) is true, (C.10) and (C.11) must hold with equality. Since the first term in (C.10) is positive and $\partial \theta_0^{vr}(\cdot)/\partial R_0 < 0$, from (C.10) we have that $R_0 e_0 (\theta_0^{vr} s_0/\psi_0)^\alpha - R_{1+}e_1 (\theta_0^{vr} s_1/\psi_1)^\alpha > 0$. But since the first term in (C.11) is also positive and $\partial \theta_0^{vr}(\cdot)/\partial R_{1+} > 0$, from (C.11) we have that $R_0 e_0 (\theta_0^{vr} s_0/\psi_0)^\alpha - R_{1+}e_1 (\theta_0^{vr} s_1/\psi_1)^\alpha < 0$; a contradiction.

If, on the other hand, (ii) is true, (C.10) must hold with equality and $R_0 e_0 \left(\theta_0^{vr} s_0/\psi_0\right)^{\alpha} - R_{1+}e_1 \left(\theta_0^{vr} s_1/\psi_1\right)^{\alpha} > 0$. Rearranging, this latter requires that

$$\frac{e_1/\psi_1}{e_0/\psi_0} < \left(\frac{s_0}{s_1}\right)^{\alpha} \left(\frac{\psi_1}{\psi_0}\right)^{\alpha-1} \frac{R_0}{R_{1+}} \tag{C.12}$$

But from (25) in the paper we have that

$$g(\alpha) \equiv \frac{1 - (1 + h_p e_1/\psi_1)^{1-\alpha}}{1 - (1 + h_p e_0/\psi_0)^{1-\alpha}} > \left(\frac{s_0}{s_1}\right)^{\alpha} \left(\frac{\psi_1}{\psi_0}\right)^{\alpha-1}$$
(C.13)

and from (C.13) and $s_0/s_1 > 1$ and $\psi_1/\psi_0 \ge 1$ that $(e_1/\psi_1)/(e_0/\psi_0) > 1$. It is then easy to see that $g(\alpha)$ is decreasing in α with $\lim_{\alpha \downarrow 1} g(\alpha) = \ln(1 + h_p e_1/\psi_1)/\ln(1 + h_p e_0/\psi_0) > 1$ and $\lim_{\alpha \uparrow \infty} g(\alpha) = 1$. But because

$$\frac{\ln(1+h_p e_1/\psi_1)}{\ln(1+h_p e_0/\psi_0)} < \frac{e_1/\psi_1}{e_0/\psi_0}$$

when $(e_1/\psi_1)/(e_0/\psi_0) > 1$, (C.12) and (C.13) enter in evident contradiction when $R_0 < 1$ and $R_{1+} = 1$. We are left with possibility (iii), i.e., $R_0 = 1$ and $R_{1+} \leq 1$, as the only possible outcome. Whether $R_{1+} < 1$ or R = 1 depends on whether $R_0 e_0 (\theta_0^{vr} s_0/\psi_0)^{\alpha} - R_{1+} e_1 (\theta_0^{vr} s_1/\psi_0)^{\alpha}$ is sufficiently negative.²

Consider now the second case in which q_1 is not longer active, that is, $R_{1+}[\kappa_1(\theta_1^{vr}s_1)^{\alpha} - he_1(\theta_1^{vr}s_1/\psi_1)^{\alpha})] \ge v$, where θ_1^{vr} is given by the second term in (C.7). This implies that there

²Note that the reason a lower e_0 makes $R_{1+} < 1$ more likely is because $R_{1+} < 1$ encourages a wider adoption of new cars, i.e., it leads to a lower $\theta_0^{vr}(R_0, R_{1+})$. To be clear, making $R_{1+} < 1$ is by no means to reduce rides in old cars, which are already socially beneficial given the definition of q_1 .

is an additional term in the first-order condition associated to R_{1+} , so (C.11) becomes

$$0 \leq \int_{\theta_1^{vr}}^{\theta_0^{vr}} \left[\kappa_1(\theta s_1)^{\alpha} - h_p e_1 \left(\frac{\theta s_1}{\psi_1} \right)^{\alpha} \right] f_p(\theta) d\theta + h_p \left(R_0 e_0 \left(\frac{\theta_0^{vr} s_0}{\psi_0} \right)^{\alpha} - R_{1+} e_1 \left(\frac{\theta_0^{vr} s_1}{\psi_1} \right)^{\alpha} \right) f_p(\theta_0^{vr}(\cdot)) \frac{\partial \theta_0^{vr}(\cdot)}{\partial R_{1+}} + \left(v - R_{1+} \kappa_1 \left(\theta_1^{vr} s_1 \right)^{\alpha} + h_p R_{1+} e_1 \left(\frac{\theta_1^{vr} s_1}{\psi_1} \right)^{\alpha} \right) f_p(\theta_1^{vr}(\cdot)) \frac{\partial \theta_1^{vr}(\cdot)}{\partial R_{1+}}$$
(C.14)

Since $\partial \theta_1^{vr}(\cdot)/\partial R_{1+} < 0$, this new term is positive, which again leaves possibility (iii) as the only possible outcome. But in this case it is easy to see that the optimal design must have $R_{1+} < 1$ necessarily, otherwise $[\kappa_1(\theta_1^{vr}s_1)^{\alpha} - h_p e_1(\theta_1^{vr}s_1/\psi_1)^{\alpha})] < v$; a contradiction with the assumption that q_1 is not binding.

C.3. Uniform driving restriction

Replacing

$$\theta_0 = \theta_0^u(R) = \left(\frac{c - 2v}{\kappa_0 R s_0^\alpha - \kappa_1 R s_1^\alpha}\right)^{1/\alpha}$$

and

$$\theta_1 = \theta_1^u(R) = \left(\frac{v}{\kappa_1 R s_1^\alpha}\right)^{1/\alpha}$$

in (C.5) and differentiating with respect to R yields

$$0 \leq \int_{\theta_{0}^{ur}}^{\bar{\theta}} \left[\kappa_{0}(\theta s_{0})^{\alpha} - h_{p}e_{0} \left(\frac{\theta s_{0}}{\psi_{0}} \right)^{\alpha} \right] f_{p}(\theta) d\theta + \int_{\theta_{1}^{ur}}^{\theta_{0}^{ur}} \left[\kappa_{1}(\theta s_{1})^{\alpha} - h_{p}e_{1} \left(\frac{\theta s_{1}}{\psi_{1}} \right)^{\alpha} \right] f_{p}(\theta) d\theta + h_{p}R \left(e_{0} \left(\frac{\theta_{0}^{ur} s_{0}}{\psi_{0}} \right)^{\alpha} - e_{1} \left(\frac{\theta_{0}^{ur} s_{1}}{\psi_{1}} \right)^{\alpha} \right) f_{p}(\theta_{0}^{ur}(\cdot)) \frac{\partial \theta_{0}^{ur}(\cdot)}{\partial R} + h_{p}Re_{1} \left(\frac{\theta_{1}^{vr} s_{1}}{\psi_{1}} \right)^{\alpha} f_{p}(\theta_{1}^{ur}(\cdot)) \frac{\partial \theta_{1}^{ur}(\cdot)}{\partial R}$$
(C.15)

where superscript "ur" denotes uniform restriction.

The sum of the first two terms is positive since the uniform restriction can at least deliver $W^n > 0$ from setting R = 1. And since $\partial \theta_0^{ur}(\cdot)/\partial R < 0$, the lower e_0 the more likely (C.15) holds with strict inequality, which calls for R = 1. In fact, if $e_0 = 0$ the last two terms reduce to

$$-h_p Re_1 \left(\frac{\theta_0^{ur} s_1}{\psi_1}\right)^{\alpha} f_p(\theta_0^{ur}(\cdot)) \frac{\partial \theta_0^{ur}(\cdot)}{\partial R} + h_p Re_1 \left(\frac{\theta_1^{vr} s_1}{\psi_1}\right)^{\alpha} f_p(\theta_1^{ur}(\cdot)) \frac{\partial \theta_1^{ur}(\cdot)}{\partial R}$$
(C.16)

which, from $\partial \theta_0^{ur}(\cdot)/\partial R < \partial \theta_1^{ur}(\cdot)/\partial R < 0$ and $\theta_1^{ur} < \theta_0^{ur}$, is positive whenever $f_p(\theta_0^{ur}(\cdot))$ is not much smaller than $f_p(\theta_1^{ur}(\cdot))$.

APPENDIX D: ADDITIONAL MATERIAL FOR SECTION 4

D.1. Validation checks

We check the validity of our model assumptions and the estimated parameters of section 4 by contrasting some of its predictions and assumptions to the data. We start with an out-of-sample validation that contrasts the predictions of the model for 2012 with the empirical estimation for the 2012 sample. As shown in Figure D.1, the model captures reasonably well the policy effects on fleet composition both around the 92-93 discontinuity and before that. It fails, however, to capture the larger fraction of newer cars in Santiago relative to the rest of the country.³



(a) Model prediction for DR coefficients after 20 (b) Empirity years of the 1992 reform. (c) 20 years of the 1992 reform.

(b) Empirical estimation of DR coefficients after 20 years of the 1992 reform.

Figure D.1: Out of sample validation

Notes: Panel (a) contains model predictions for 20 years after policy implementation. The prediction is for a policy equivalent to the Santiago-1992 reform using the parameters estimated with the 2006 data and controlling for income and population. Panel (b) shows the estimated coefficients using data from 2012, i.e., 20 years after policy implementation.

According to the model predictions contained in Figure D.1(a), a driving restriction like Santiago-1992 should produce only relative changes in car holdings for models just on either side of the 92-93 discontinuity. This is not only fairly consistent with what we see in Figure D.1(b) but more so with what we see in Figure 2(b), which shows that the DR coefficients in equation (1) for vintages away from the discontinuity are either not statistically different from zero or barely so. The fact that the DR coefficients for the 94 and 95 vintages, and not just 93, are positive is not surprising because there is always noise in car quality due to

³As documented in Gallego et al. (2013), one reason for this difference that is not captured in our model is the substantive shift to private transport caused by the poorly implemented public transport reform in Santiago in February 2007. Unfortunately, we cannot control for this in our empirical estimation in order to separate it from the driving restriction.

different features such as individual preferences, heterogeneity in cars' aging, etc. The same is true on the other side of the discontinuity: DR coefficients for 90 and 91 are comparable to that for 92. For this reason we adopted in the estimation clusters of 4-vintage groups around the 92-93 discontinuity.

Figures 2(b) and D.1(b) also serve to discuss the validity of our single-ownership assumption. Some readers may question this assumption on the basis that the DR coefficients for older models return to zero as we move away from the 92-93 discontinuity. This interpretation is incorrect accordingly to our model. Using (31), our model predicts that $\theta_{\tau}^{nr} = (1/R)^{1/\alpha} \theta_{\tau}^{r}$ for both τ and $\tau + 1$. So, if F is linear in the relevant range, which is a good approximation given the large number of vintages considered, we have $q_{\tau}^{nr} = F_{nr}(\theta_{\tau}^{nr}) - F_{nr}(\theta_{\tau+1}^{nr}) \approx$ $q_{\tau}^{r} = F_{r}(\theta_{\tau}^{r}) - F_{r}(\theta_{\tau+1}^{r}).^{4}$ Therefore, any evidence of a second-car effect should have been reflected in strictly positive DR coefficients for the older models. Failing to find this in a less than optimal vintage-specific design, together with the fact that total-ban vintage design eliminates the second-car effect by construction, validates the use of the single-ownership assumption in our model given our focus on vintage-specific designs. Moreover, in Section B.3) we provide econometric evidence suggesting the driving restriction did not affect the second-car margin.

⁴The linearity in $F(\cdot)$ is actually not necessary given our estimation results below: $(1/R)^{1/\alpha} = 1.015$.

D.2. Emissions and within vintage heterogeneity

Throughout our analysis, we have been using vintage as the main observable that determines whether and the extent to which a car is subject to a driving restriction (or payment of circulation fees). In this appendix, we implement several exercises that document that vintage explains a relevant share of the variation of emission rates at the vehicle level. We use the 2008 and 2016 datasets of smog checks (the first and the last available datasets) and a series of fixed-effect models to study the variation of emission rates within and between vintages. We drop from our sample any car older than 40 years old and any model with fewer than 50 observations. We focus on CO readings at 2500 rpm.⁵ The first thing to notice is that CO readings may be miss-measured because of varying measurement conditions such as changes in ambient temperature, humidity, etc. We take advantage of cars with multiple measurements in the same year to identify the extent of this miss-measurement. If we assume that there is classical measurement error, the R^2 of a regression of emissions on plate number fixed effects gives the signal-to-noise ratio and, therefore, a proxy for the share of variation that reflects real differences in emission rates across cars. Column (1) in Table D.1 presents the value for the 2008 and 2016 samples. In both cases, results imply that about one-third of the variation in emission rates seem to be related to noise (31% in 2008 and 37% in 2016).

	(1)	(2)	(3)	(4)	(5)
Panel (a):	Sample 2008				
R^2	0.685	0.397	0.401	0.422	0.450
Obs.	703304	790736	790736	790683	784633
Panel (b)	: Sample 2016				
R^2	0.624	0.271	0.280	0.300	0.330
Obs.	704793	1057427	1057425	1057239	1047960
			Vintage	Vintage	Vintage
Controls	Car plate f.e.	Vintage f.e.	+	+	×
			Maker f.e.	Model f.e.	Model f.e.

Table D.1: R^2 of different fixed-effect models

Notes: The table presents the R^2 of different model specifications. Panel (a) uses smogcheck data of 2008, while Panel (b) of 2016. Column (1) corresponds to a regression of emission measures of cars with multiple smog-checks measurements in the same year against car plate fixed effects. Columns (2)-(5) corresponds to a regression where each car appears only once, and we regress emissions against vintage, maker, and model fixed effects as noted in the table.

These results give an upper bound of how much of the variation in emission rates can be explained by observable characteristics of a car. Now, we move to see how much of the variation can be explained by vintage effects. Column (2) presents results of regressions of emission rates

 $^{^{5}}$ This give us a sample of 50 makers, 3826 models, and about 800,000 vehicles in 2008 and 95 makers, 5609 models, and about 1,050,000 vehicles in 2016.

on a vector of vintage fixed effects. Results imply that 39% of the total variation in emission rates in 2008 is explained by variation *between* vintages. Given the results in Column 1, this implies that around 57% (=39%/68%) of the "real" variation of emission rates is explained just by vintage effects.⁶ Next, in columns (3) to (5) we add other fixed effects related to observable characteristics: the maker (column 3), the model (column 4), and a vector of *model* × *vintage* fixed effects (Column 5). While all these observable characteristics increase the R^2 of the models, the marginal contribution of each of them in explaining variation seems to be much smaller than the contribution of vintage. This is interesting because among all observable characteristics of a car that can be observed by regulators (and not subject to manipulation by users), vintage is by far the most relevant to explain emission rates.

Results for the 2016 dataset confirm that vintage is the most important observable characteristic to explain variation in emission rates; although the share of the variation explained by it decreases to about 27%, or about 44% (= 27%/62%) when considering variation net of measurement error. This decrease is not entirely surprising since vintage captures not only the fact that control technologies deteriorate overtime but also the fact that at time passes new cars enter the market with better control technologies. The 2008 sample has a much higher fraction of pre-1993 models, so much of the variation in emission rates is explained by the big jump at the 92-93 discontinuity, as shown in Figure 3 in Section 2.3 of the paper. Nevertheless, the share of emission rates explained by variation between vintages in the 2016 sample is still high, especially considering the big sample we use, and the fact that the contribution of other observable characteristics remains much smaller (which is remarkable as the number of models increases significantly in the 2016 sample).⁷

Despite an important share of the variation in emission rates is explained by vintage, there is still the concern that a focus on vintage may reduce the effectiveness of a vintage-specific restriction by not correcting for the presence of older cars with lower-than-average emission rates and of newer cars with higher-than-average emission rates. In Figure D.2 we present some measure of emission rate dispersion for different vintages. We use the interquartile range of CO readings for the four-year intervals used in the text (i.e., cars between 0 and 3 years old, 4 and 7,..., and 24 and more years old) for the 2008 and 2016 samples.⁸ Results for the 2008 sample mimic Figure 3 in Section 2.3 by showing a big discontinuous change for the 1989-1992 group. The two figures suggest that the overlap of the interquartile variation for different

⁶More formally, let us assume that $e = x + m + \epsilon$, where e is the emission rate measured during the smog check, x is a function of observable variables, m is miss-measurement, and ϵ is unobserved heterogeneity. We assume the three components are orthogonal among them (i.e., we have classical measurement error). Then, the R^2 of the regressions in column (1) is $(\sigma_x + \sigma_\epsilon)/(\sigma_x + \sigma_m + \sigma_\epsilon)$ and the R^2 of the regressions in columns (2) to (5) is $(\sigma_x)/(\sigma_x + \sigma_m + \sigma_\epsilon)$. Therefore, the R^2 of a regression of (e - m) (i.e. emission rate net of miss-measurement) is the R^2 of the regression of e on x divided by the R^2 of the regression in column (1).

⁷This probably reflects the increase in the demand for cars following the aforementioned (failed) reform in public transport that took place in 2007 (Gallego et al., 2013).

⁸For the 2016 sample, and following our procedure from Section 4.2, we use only post-1992 cars equipped with a catalytic converter.



Figure D.2: Interquartile distribution of CO emissions at 2500 RPM by different vintage groups

Notes: This figure shows interquartile ranges of car emissions for different age groups. Panel (a) has cars corresponding to the sample of 2008. Panel (b) shows emission measures in 2016 for cars built after 1992 equipped with a catalytic converter.

vintages is limited, especially for cars in the middle-range of their lifespans. For instance, Figure D.2(a) shows that there is almost no overlap across vintages in the first five groups (i.e. for cars 20 years old and younger) and just then the last three groups present more or less the same level and dispersion of emission rates (which may include selection effects, i.e., the fact that cars in better condition, including lower emission rates, are likely to survive longer in the market). A similar picture appears in Figure D.2(b) using the 2016 sample, where there is little overlap in the interquartile range across vintages. These results are important for our paper as they suggest that the cost of targeting policies just on vintage cannot be large. Yet, as discussed in the main text, regulators can always add other observables, provided they can be enforced, to reduce the residual variation across cars.

D.3. Emissions: age and vintage effects

As discussed in Section 2.3 of the paper, our estimates of emissions are a combination of newer cars entering with cleaner technologies (vintage effects) and pollution-control technologies wearing out over time (age effects). In this section, we exploit smog checks data from a balanced panel of cars that we can follow in the 2008-2016 samples, and estimate a regression of emissions on age and vintage fixed effects.⁹ We estimate vintage effects for the 1988-2005 cohorts and age effects from age 3 to 20 using CO emissions as the dependent variable.¹⁰ Figure D.3(a) presents the estimated age and vintage fixed effects. For a more straightforward comparison, we align the age effects with the vintage effects such that a 3-year-old car is equivalent to vintage-2005 car. In addition, we present as reference the cross-sectional estimates (which include both age and vintage effects) using the 2008 smog checks dataset. Interestingly, the average difference between the sum of age and vintage effects from our panel data estimates and the cross-sectional estimates using the 2008 cross-section is equal to 0.0013, which allows us to interpret the cross-sectional estimates as the sum of vintage and age fixed effects.

These results reveal the significant discontinuous drop in emissions in post-1992 cars. We also observe smooth increasing age effects. Figure D.3(b) show that for most post-1992 vintages about half of the cross-sectional effect corresponds to age effects. In contrast, for the pre-1993 cars, vintage effects explain around 90% of the cross-sectional estimates.

These estimates provide an idea of how much emission heterogeneity over time is explained by newer cars entering with cleaner technologies and how much by pollution-control technologies wearing out over time. We show that both dimensions are important and that, while age effects matter and seem to evolve smoothly, there are some technological changes that produce discrete changes in emissions (such as the catalytic converter) alongside smoother technological advances that contribute to changes in emissions too. The latter is of the same order of magnitude than age effects. Given that all cars in the market are imported, these patterns respond to a world-wide tendency in the car industry and, therefore, similar patters should be found in other contexts.

 $^{^{9}}$ We drop from the sample cars that are more than 20 years old to avoid selection problems in the right tail of the distribution.

 $^{^{10}}$ We cannot estimate the effects for ages 1 and 2 because most cars are exempted from smog checks during the first two years.



(a) Age and vintage fixed effects (b) Relative contribution of each f.e. to emissions

Figure D.3: Vintage v/s age effects

Notes: This figures shows the estimated fixed effects coefficients of two different regressions. In Panel (a), "Cross-section estimates" are the coefficients of age/vintage fixed effects in a cross-sectional data corresponding to the 2008 smog-checks sample. "Vintage f.e." and "Age f.e." are the estimates of vintage and age fixed effects respectively of a balanced panel of cars that merges data from 2008 to 2016 smog-checks sample. In Panel (b), we show the contribution of each of the "Vintage f.e." and "Age f.e." to total emissions.

D.4. Emissions and mileage

In this section, we study whether there is any correlation between miles traveled and emission rates, in particular the extent to which car that is run more intensively tends to emit more local pollutants per mile. Our model is built upon the assumption that there does not exist such correlation. So, as explained at the end of section 5.2 in the text, finding a positive correlation would necessarily reduce the convexity in the emissions-age/quality relationship we have used in our simulations, which could eventually invalidate our results.

We run regressions of CO readings at 2500 rpm on miles traveled in 2016 (as explained in section 2.3 of the text, miles traveled are obtained from odometer readings in 2015 and 2016). Regression results are in Table D.2 below. We start presenting the bi-variate regression in column (1). Results imply a negative and statistically significant correlation between both variables. However, there are obvious omitted variables relevant to identify the relationship between both variables. Thus, in column (2) we add vintage fixed effects. Then, the estimate of the correlation becomes positive but is statistically insignificant. The same results appears in the next column when we add *vintage* × *model* fixed effects. Thus, once we control for the age of the car, there appears to be no correlation between miles traveled and emission rates, which is what we have been assuming in the paper.

	(1)	(2)	(3)
Miles traveled	-0.405***	0.0166	0.0216
(in hundreds of thousands)	(0.018)	(0.016)	(0.016)
		· ·	
			Vintage
Controls	No	Vintage f.e.	×
			Model f.e.
Observations	263478	263478	263478
\mathbb{R}^2	0.002	0.239	0.334

Table D.2: CO emissions and mileage

Notes: The independent variable is measured in hundreds of thousands of miles. The average value of the dependent variable (CO emissions) is 0.28%. The average value of the independent variable (miles traveled) is 9.3 thousands miles. The coefficient in column (3) implies that increasing miles traveled by 10% increases CO emissions by 0.07%.

D.5. Emissions and new cars

Another important assumption in our model is that individuals driving the most (i.e., those with the highest θ) buy less polluting cars. The fact that these individuals buy newer, more expensive cars seems reasonable, since θ is highly correlated with income, but one may nevertheless argue that these individuals also tend to buy larger cars which may end up emitting more local pollution per mile driven. We explore this possibility by looking at correlations at the district level between CO emission rates, fraction of newer cars (i.e., cars no more than 4 years old), and income levels. For the analysis, we merge the 2008 sample of smog checks with information on income and the share of new cars that we use in Section 2.1 of the paper (which uses data for 2006). Figure D.4 presents the results. We can see, from Panels (c) and (d), that districts with higher income per capita have indeed a bigger share of new cars per capita and, from Panel (a), that districts with a larger fraction of newer cars have cars that emit less per mile driven. Consistent with our assumption, all this leads to a negative correlation between income and emissions, as shown in Panel (b).



Figure D.4: Correlation between CO emissions, new cars, and income

Notes: This figure shows several correlations between district characteristics. Panel (a) shows that districts with higher shares of new cars tend to have a cleaner fleet. Panel (b) shows that richer districts tend to have a cleaner fleet. Panel (c) shows that richer districts tend to have larger shares of new cars. Panel (d) shows the relationship from (c), but using a log specification on the dependent variable.

APPENDIX E: ADDITIONAL MATERIAL FOR SECTION 5

E.1. Policy simulations for Santiago 1992

The main difference between our policy simulations in Section 5 of the paper and the driving restriction implemented in Santiago in 1992 is the role played by a drastic change in the pollution-control technology, namely, the introduction of the catalytic converter. In the simulations in Section 5 we consider an invariant relationship between age and emission rates. This is clearly a bad approach to evaluate the impact of the 1992 policy since as time goes by the number of cars without the converter drops eventually to zero. In this section, we extend our model to consider a time-varying relationship between age and emission rates. In particular, we now let the external cost per mile driven in region k to depend on vintage as follow

$$\epsilon_{a,t}^{k} = \begin{cases} h_{k}^{nc} \exp(\omega a) & \text{if } \tau \leq 1992 \\ h_{k}^{c} \exp(\omega a) & \text{if } \tau > 1992 \end{cases}$$
(E.1)

so as to reflect the fact that cars equipped with a converter (c) can generate quite different external costs than cars not equipped with it (nc). To estimate the values of h_k^c , h_k^{nc} and ω we follow the procedure from Section 4.2 of the paper but adding the parametric structure of (E.1) and minimizing the difference between actual harm and predicted harm rather than matching it as we do in equation (33) in the paper. The parametric structure on the external cost per mile allows us to extrapolate the curve to other periods, when non-catalytic converter cars are only prevalent in older cars (to eventually disappear). Instead of using smog-check readings from the 2016 sample, we now use readings from the 2008 sample —the oldest available sample—, which includes both post- and pre-1993 models.¹¹

Results from the estimation are presented in Table E.1. Panel (a) shows the external cost per mile following the non-parametric procedure of section 4.2. In Panel (b) we impose the structure of equation (E.1) and estimate the curve as described above. Both the parametric and the non-parametric methods give similar results, validating our ad-hoc functional form assumption. We use the estimated values of h_k^c , h_k^{nc} , and ω to compute the external cost curve for other years. in Panel (c), we present as an example the estimated external cost in year 2000, where relatively newer cars where still not equipped with a catalytic converter.

With those estimates at hand, we proceed to do counterfactual simulations. We present welfare results in Table E.2. The dynamic component of the policy makes it computationally hard to find the equilibrium path in many cases, so we focus only on four scenarios: (i) no intervention, (ii) first best pigouvian taxation, (iii) a vintage-specific restriction as implemented in Santiago in 1992, and (iii) a vintage-specific restriction that places a heavier restriction on pre-1993 models than the 1992 policy does. Note that driving surplus in the absence of any

¹¹We do not use NO_x readings, which account only for 3%, as it is not available in the 2008 dataset.

Age (a)	0	1	2	3	4	5			
Panel (a): non-parametric estimation $(t = 2008)$									
$\epsilon^r_{a,t}$	0.0193	0.0315	0.0886	0.1884	1.8853	3.9252			
$\epsilon^r_{a,t}$	0.0023	0.0036	0.0103	0.0219	0.2199	0.4579			
Panel (b): parametric estimation $(t = 2008)$									
$\epsilon^r_{a,t}$	0.0189	0.0399	0.0844	0.1783	1.8717	3.9551			
$\epsilon^{nr}_{a,t}$	0.0022	0.0047	0.0098	0.0208	0.2184	0.4614			
Panel (c): parametric estimation $(t = 2000)$									
$\epsilon^r_{a,t}$	0.0189	0.0399	0.4192	0.8857	1.8717	3.9551			
$\epsilon^{nr}_{a,t}$	0.0022	0.0047	0.0489	0.1033	0.2184	0.4614			

Table E.1: External costs per mile

Notes: External costs are estimated following the procedure described in section 4.2 and imposing the parametric form given by equation (E.1).

intervention is unchanged relative to that in Section 5. Pollution costs, however, change due to the new damage function.

Table E.2:	Welfare	calculations
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#	Counterfactual	Transport surplus	Pollution cost	Welfare	Welfare gain/loss
		(in 2006 dollars)	(in 2006 dollars)	(in 2006 dollars)	(relative to first-best)
1.	No intervention	5579.6	-1935.1	3644.5	0%
2.	First best	5339.3	-483.6	4855.7	100%
3.	Santiago's 1992 restriction	5564.6	-1794.2	3770.4	10%
	$(R = 0.9684, \tau > 1992)$				
4.	Optimal driving restriction	5197.8	-624.7	4573.1	77%
	$(R = 0, \tau > 1992)$				

Notes: The table shows present-value welfare calculations under different policy counterfactuals for the case of Santiago's 1992 reform. All calculations are in per capita terms in 2006 dollars. The first column presents household surplus from driving, ignoring pollution costs. The second column presents pollution costs. The third column corresponds to welfare calculations as the sum of driving surplus and pollution costs. The fourth column presents welfare gains/losses as a fraction of the welfare gain under the first best (i.e., Pigouvian taxation). Differences between rows 3. and 1. correspond to the welfare gains associated with the policy implemented in 1992.

When looking at the welfare consequences of the policy implemented in Santiago in 1992 we find a positive and relatively large effect. Pollution costs decrease by 7% relative to the case of no intervention, resulting in a 3% increase in overall welfare. When we look at 3 periods after policy implementation (equivalent to the 2004-2008 period) we find that emissions in Santiago dropped by 10% relative the no-intervention counterfactual level, slightly lower that our "reduced-form" results in Section 2.3. The difference is partly explained by a general equilibrium effect on the overall fleet, more precisely, an increase of around 3% of new cars at

the country level during the first years following the implementation of the policy. According to our model, Santiago's policy achieved 10% of the potential welfare gains from implementing pigouvian taxes.

In the last row of Table E.2 we present the result of a more aggressive vintage-specific restriction that places a complete ban on pre-1993 models starting in 1996. As in section 5.2, this exercise helps to illustrate that welfare gains can vary substantially depending on how these vintage-specific restrictions are designed. In this particular case, the gains under this more aggressive restriction are six times larger than the gains under the 1992 design.

E.2. Temporal variation in pollution harm

Following the discussion in section 5.4 of the paper, here we extend the model to the case in which local pollutants have different external costs depending on when they are emitted (e.g., peak vs. off-peak hours, weekdays vs. weekends, winter vs. summer months, etc). For simplicity let us assume that the harm caused by a unit of pollution is h_r during a fraction λ of the time and 0 otherwise. In such a setting, driving restrictions appear particularly flexible.¹² The owner of an *a*-year-old car subject to a restriction of intensity R_a applied over a fraction λ of the time will drive $x^{r,\lambda}(\theta, a) = \lambda R_a \left(\frac{\theta s_a}{\psi}\right)^{\alpha}$ miles during that time and $x^{r,1-\lambda}(\theta, a) = (1-\lambda) \left(\frac{\theta s_a}{\psi}\right)^{\alpha}$ during the remaining time, resulting in an overall utility of

$$u^{r}(\theta, a; \lambda) = (1 - \lambda + \lambda R_{a}) \kappa (\theta s_{a})^{\alpha} - p_{a}$$
(E.2)

per period. Since the social value of driving during times in which pollution is a problem is exactly as before, i.e., $R_a \kappa (\theta s_a)^{\alpha} - h_r R_a (\theta s_a/\psi)^{\alpha}$, obtaining the optimal value of R_a follows the same reasoning of section 3.5, so it ends up being independent of the value of λ .¹³

Of the alternative instruments considered in the simulations in section 5, it is evident that by construction scrappage subsidies cannot cope with this temporal variation as they require the scrapping car to be removed permanently from the market. Gasoline taxes face a similar problem. In contrast, circulation/registration fees have the potential to cope with temporal variation. The authority must offer each year a menu of circulation fees that vary by vintage: drivers have the option to pay either a positive fee for unlimited use of the car or no fee for its use only during the $1 - \lambda$ hours in which pollution is not a problem. In equilibrium, there is a cutoff age below which all car owners opt for the fee and above which none does. Not surprisingly, this cutoff is very similar, and sometimes equal, to the threshold in the optimal vintage-specific restriction design. As illustrated in Figure E.1, however, if the option of offering these circulation menus is not available, the advantage of circulation fees over (optimal) vintage-specific restrictions rapidly vanishes as λ drops and completely disappears when $\lambda = 0.25$. Note that in all simulations we assume pollution harm in Santiago to be $h_r = h/\lambda$ a fraction λ of the time and 0 otherwise, so that welfare under no intervention would remain constant across simulations.

 $^{^{12}}$ The model easily extends to an even higher number of partitions, as in today's Mexico City HNC, which differentiates by weekdays, Saturdays, and Sundays. Damage h will vary across these partitions, and so will the optimal vintage threshold in each of them. The procedure to obtain these thresholds is the same as in the case of two partitions.

¹³This "independence" result seems reasonable for divisions that concern months and weekdays from weekends, it may appear less reasonable when it concerns hours of the day, as drivers may substitute peak for off-peak driving. This substitution can still be handled by the model at the cost of additional notation without any fundamental change of our results.



Figure E.1: Welfare under temporal variation in pollution harm

Notes: This figure shows welfare estimations when extending the model to allow for temporal variation in pollution harm (i.e., λ) under various scenarios: optimal dirving restrictions, (no-menu) optimal circulation fees, and no intervention.





Figure E.2: Steady-state fleet composition under a uniform driving restriction



Figure E.3: Steady-state fleet composition under a driving restriction that exempts cars 12 years old and younger



Figure E.4: Steady-state fleet composition under optimal circulation fees



Figure E.5: Steady-state fleet composition under the optimal scrappage subsidy with full arbitrage between restricted and non-restricted areas (\$2420)



Figure E.6: Steady-state fleet composition under the optimal scrappage subsidy with no arbitrage between restricted and non-restricted areas (\$3240)



Figure E.7: Steady-state fleet composition under an optimal gasoline tax in Santiago (\$1.06 per gallon)



Figure E.8: Steady-state fleet composition under a vintage driving restriction & gasoline tax $(R = 0, a \ge 4, c80 \text{ per gallon})$

References

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