Getting around a license-plate ban: Behavioral responses to Mexico City's driving restriction

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Erick Guerra[a] & Adam Millard-Ball[b]

Authors' names presented in alphabetical order

[a]Assistant Professor of City and Regional Planning, Corresponding author
University of Pennsylvania
Mailing address: 127 Meyerson Hall 210 S. 34th St. Philadelphia, PA 19104
Voice: (215) 746-8234
Fax: (215) 898-5731
E-mail: erickg@design.upenn.edu

[b]Assistant Professor of Environmental Studies
University of California, Santa Cruz
Mailing address: Environmental Studies Department, 1156 High St, Santa Cruz, CA 95064, United States
Voice: (831) 459-1838
Fax: (831) 459-4015
E-mail: adammb@ucsc.edu

Abstract

License-plate-based driving restrictions are among the highest profile policies for local governments to address congestion and air pollution. Cities as varied as Sao Paulo, Paris, Tianjin, and New Delhi have enacted temporary or permanent restrictions to improve local air quality. Using household travel survey data and a research design based on the abrupt shift in how the policy applies to 10-year-old vs. 9-year-old vehicles, we evaluate the impact of *Hoy No Circula*, one of the earliest and most studied driving restrictions, in Mexico City. In line with previous studies, we find that *Hoy No Circula* has done little to reduce overall vehicle travel, but we reject the prevailing theory that its lack of success is due to perverse incentives for households to buy second cars. Instead, we highlight the range of other, less costly ways that people adjust behavior to avoid the restrictions. Although no single behavior dominates, most households –– particularly those that own older, higher-polluting vehicles — do not use their car every weekday regardless of the restriction. As a result, it is relatively easy to shuffle travel from restricted days to unrestricted days and thus avoid the ban. Shuffling travel days is less costly, more immediately available, and far simpler for most households than buying a second car.

Highlights

- Examines behavioral responses to a one-day-per-week driving ban in Mexico City
- Employs novel research design based on a discontinuity in the restriction policy
- Confirms findings that *Hoy No Circula* had a limited influence on driving
- Rejects finding that households adapt to the policy through second car purchases
- Finds that households avoid ban by shuffling weekly travel routines

Keywords

Car restrictions; driving ban; *Hoy No Circula*; travel behavior; policy avoidance; Mexico City

1. Introduction

Faced with some of the worst pollution in any city in the 1980s, the Mexican government instituted a policy to restrict car use in the Mexico City Metropolitan Area. The policy *Hoy No Circula*, roughly translated as "Don't Drive Today," began in 1989, and restricted private cars from driving one weekday per week based on the last digit of the license plate. Mexico City recently doubled down on its policy. *Doble Hoy No Circula* — enacted temporarily due to a recent surge in local pollution — applied to more of the vehicle fleet and banned cars as many as three days out of the week.

Other Latin American cities like Buenos Aires, Bogota, Cartagena, Lima, Sao Paolo, and Santiago de Chile have enacted similar policies (Onursal and Gautam 1997; de Grange and Troncoso 2011; Gallego, Montero, and Salas 2013). More recently, the policy has expanded in other parts of the world. After successfully reducing local pollution ahead of the 2008 Summer Olympic, Beijing became the first Chinese city to enact a license-plate-based car restriction (Viard and Fu 2015; Sun, Zheng, and Wang 2014; Wang, Xu, and Qin 2014; Gu, Deakin, and Long 2017). Cities as varied as Paris, Tianjin, and New Delhi have also enacted temporary or permanent restrictions to improve local air quality. Throughout the paper, we refer to these policies interchangeably as car bans, driving restrictions, or car restrictions.

Driving restrictions tend to be politically more palatable than congestion charging and other pricing-based policies to reduce road congestion and pollution (e.g. Mahendra 2008). Wirth (1997), Mahendra (2008), and Wang et al. (2014) cite surveys that indicate high public approval for the policy in Mexico City, Sao Paulo, and Beijing. Yet *Hoy No Circula* in Mexico City and its counterparts elsewhere in the world are economically costly (Blackman et al. 2015; Davis 2008; Cantillo and Ortúzar 2014; Nie 2017) and may have done little-to-nothing to improve congestion or local air quality. Three empirical studies of *Hoy No Circula* suggest that because the policy encourages households to purchase second cars to avoid the restrictions, there may be no effect or even an increase in driving (Eskeland and Feyzioglu 1997; Davis 2008; Gallego, Montero, and Salas 2013).

That so many households respond to a car ban by purchasing a second vehicle is surprising. There are many other ways — such as shuffling trips to a different day — for households to avoid a ban, most of them more immediate and less expensive than purchasing an additional vehicle. However, many of these behavioral responses cannot be examined using aggregate emissions, travel, or vehicle purchase data. Two studies to date have examined household-level responses to a car ban using disaggregate data from Beijing. Wang, Xu, and Qin (2014) focus entirely on just one avoidance mechanism: non-compliance with the restriction. Gu, Deakin, and Long (2017) examine non-compliance and whether households shift car travel to unrestricted hours, weekdays, or second vehicles.

This study is the first to present a systematic analysis of the various ways that individuals and households –– the correct unit of analysis to study behavioral adjustments to a policy — might have responded to Mexico City's travel ban. Our contribution is thus both theoretical and empirical. Relying on household-level data from the Mexico City Metropolitan Area's (MCMA) 2007 household travel survey, we present a half-dozen hypotheses about how households might adapt to a license-plate-based car restriction program like *Hoy No Circula*. We then explore each hypothesis using a research design based on the abrupt shift in how the policy applies to 10-year-old and 9-year-old vehicles. By examining a single day of travel after the policy has been in effect for several years, our research design also has the advantage of providing insight into the long-run behavioral equilibrium, rather than the short-run aggregate responses as in Davis (2008).

The *Hoy No Circula* policy has undergone numerous changes since the survey was conducted. Thus, our primary contribution is to analyze the underlying household responses to a driving restriction, rather than to assess the Mexico City policy as it currently stands. Moreover, the diversity in how ostensibly similar car bans are implemented across the world – geographic and temporal scope, exempted vehicles and the number days that a car is banned – mean that parsing the underlying mechanisms may prove more fruitful than seeking a universal answer to the effectiveness of such policies. In Beijing, for example, strict limits on vehicle licenses make it difficult to avoid the restriction through second car purchases.

Understanding how households respond to *Hoy No Circula* and similar policies can help policymakers improve the design of car bans or at least temper expectations about the impacts on pollution. For example, if second car purchases indeed limited the effectiveness of license-plate car restrictions, then varying which days are associated with which plates would reduce the incentive to purchase a second car since it would sometimes be banned on the same day as the first car. More broadly, better understanding behavioral responses to license-plate-based car bans is increasingly important as these policies proliferate and increase in intensity.

The remainder of this paper is organized as follows. Section 2 provides an overview of the literature on behavioral responses to driving restrictions with an emphasis on policy avoidance. Section 3 describes the policy context, research design, and data. Section 4 discusses the findings in relationship to our hypotheses about how households might avoid a travel ban. Section 5 concludes with an overview of the paper's implications for public policy and describes areas for future study.

2. Behavioral responses to a driving restriction

Throughout Asia and Latin America, researchers have generally found that car bans have no effect (de Grange and Troncoso 2011; Davis 2008; Sun, Zheng, and Wang 2014; Lin Lawell, Zhang, and Umanskaya 2015), a small positive effect (de Grange and Troncoso 2011), or even a negative effect (Eskeland and Feyzioglu 1997; Lin Lawell, Zhang, and Umanskaya 2015; Gallego, Montero, and Salas 2013) on local pollution or car use.[1] Sun, Zheng, and Wang (2014) estimate that the Beijing car ban reduced congestion but did not affect pollution, perhaps because reduced congestion led to greater traffic flow and its associated pollution. Few independent academic assessments come close to finding the 20% reduction in emissions that policy makers hoped would come from banning one-fifth of cars from the road each day. Two notable exceptions suggest that the policy may be substantially more effective in Beijing. Viard and Fu's (2015) regression discontinuity analysis attributes a 21% reduction in air pollution to Beijing's one-day-per-week ban, though the graphical evidence in the paper suggests that the results are highly dependent on the regression specification. Supporting this finding, however, Gu, Deakin, and Long (2017) find that car-owning households reduce weekday car trips by 15.8% to 18.6% on restricted days. Carrillo et al. (2014) observe a 10% reduction in Quito's local pollution relative to areas and times just outside the ban, but Quito's policy only affects peak hours and the central part of the metropolis.

In the remainder of this section, we explore hypotheses about how households might respond to reduce the effectiveness of a driving ban, discuss the related literature, and theorize about whether and how each hypothesis could be tested using available data from a household travel survey. Travel behavior is complex

[1] Some papers are listed more than once as they have different findings depending on the type of effect (one or more pollutants, vehicle travel, etc.) or the time period analyzed.

and there are a variety of ways that an individual or household could avoid a car ban — some legal, others not. Each might help explain why the impacts of a one-day-per-week car ban might diverge from a naïve estimate of a one-fifth reduction in local road-based emissions. Some of these strategies, such as driving just before or after a ban, are immediately deployable by households in response to unpredictable bans like those on high pollution days. Others, such as buying a second car, are longer-term responses. Few behavioral responses have been empirically tested — and almost none using the correct ecological unit, the household.

2.1 Changes in car ownership

The primary explanation in the literature for why car bans have failed to produce substantial reductions in pollution is that households purchase a second car, often older and higher polluting than their current car, with a different final digit of the license plate, and drive it on restricted days (Onursal and Gautam 1997; de Grange and Troncoso 2011; Eskeland and Feyzioglu 1997; Davis 2008; Wang, Xu, and Qin 2014; Mahendra 2008; Sun, Zheng, and Wang 2014; Lin Lawell, Zhang, and Umanskaya 2015; Gallego, Montero, and Salas 2013). Once they have a second vehicle, household members use it, even on days when the first car is not restricted. Thus, through the channel of car ownership, a driving restriction could in theory even increase congestion and air pollution.

However, buying a second car is a fairly expensive and inconvenient response to a one-day-per-week car ban and the empirical evidence for its occurrence is weak, even anecdotal. Eskeland and Feyzioglu's (1997) emphasis on second car purchases notwithstanding, the authors' model of car ownership actually predicts that more one-car households choose to sell a car than buy a second. The households who sell a vehicle are almost certainly at the margin of choosing not to own a car and the added restriction makes car ownership less valuable. In the authors' own words:

> The model indicates that, for restriction factors in the range of 0.8 to 0.9, the number of "sellers" exceeds the number of "buyers" by 2 to 3 percentage points. Thus, the model predicts a slight increase in exports of used cars to the rest of the country as a result of the restriction. However, most observers believe that the opposite occurred. Many Mexico City households have bought an additional car in response to the regulation. Increased purchases of used cars is consistent with our estimation that total gasoline consumption has increased, but this result is not indicated by our car ownership model.

Thus, an unsupported anecdote trumps the actual empirical findings.

Another study (Davis 2008) applies a regression discontinuity design to vehicle registrations and vehicle sales using higher-order polynomial time trends — ranging from fourth to eleventh. While the author reports that *Hoy No Circula* appears to have led to a point increase in registrations and sales, not all of the results are statistically significant and they rely on just 30 data points — not nearly enough given that higher-order polynomials are included. The times series also includes at least two substantial shocks that invalidate the regression discontinuity design: the economic crisis at the end of 1994 and a substantial change in the policy to exempt newer cars with catalytic converters at the end of 1996. More recently, Gu, Deakin, and Long (2017) find that households with multiple cars are unaffected by Beijing's car restriction, indicating the potential important of second-hand car purchases. However, only 2.2% of the sample report having a second car.

A second mechanism through which changes in car ownership may influence the effectiveness of a car ban is through incentives to drive a newer vehicle. Some car bans — such as those in Santiago and in Mexico City since the end of 1996 — are designed to encourage a cleaner fleet. Santiago exempts cars with catalytic converters on all but the most polluted days. This permanent restriction had no measurable impact on driving, but may have encouraged households to install catalytic converters (de Grange and Troncoso 2011). On several occasions, *Hoy No Circula*'s restrictions were modified to apply less stringently to newer, low-polluting cars. Since newer cars tend to be more comfortable and more fuel efficient, people tend to drive them more than older cars. If this increase in driving — as well as any associated increase in congestion — outweighs the reduction in pollution from a newer vehicle, the shift from one old car to one new car could potentially increase aggregate pollution.

Finally, if car restrictions make car ownership less attractive, many households would respond by shedding or never purchasing a car in the first place. This response would tend to increase rather than decrease the effectiveness of the policy, however.

2.2 Reshuffling travel

There are two primary ways that households might reschedule their travel routines to avoid a car ban on a restricted day: shifting travel to different times of day and shifting travel to different days (Davis 2008; de Grange and Troncoso 2011; Wang, Xu, and Qin 2014; Lin Lawell, Zhang, and Umanskaya 2015; Viard and Fu 2015; Gu, Deakin, and Long 2017). *Hoy No Circula*'s restrictions only apply between 5AM and 10PM and until 2008 only applied on weekdays. Other programs have similar features. For example, Santiago's restriction applies from 7:30AM to 9PM and Beijing's from 7AM to 8PM. This form of avoidance is detectable using both aggregate measures of travel as well as household travel diaries. In Santiago, de Grange and Troncoso (2011) observe a 5.5% decrease in overall vehicle flow during emergency bans, but a statistically significant 3.5% increase in traffic during the hours prior to the ban. Examining bans in Sao Paolo, Bogota, Beijing, and Tianjin, Lin, Zhang, and Umanskaya (2011) find some evidence of pollution increasing before and after the ban and recommend that bans have extended hours to be more effective.

In addition to shifting travel to different times of day, households might lump travel, particularly discretionary non-work travel, from banned travel days to other days in the week. Davis (2008) estimates that weekend and weeknight pollution increased relative to weekday pollution in Mexico City after the implementation of *Hoy No Circula*. Gu, Deakin, and Long (2017) find that households are particularly likely to lump travel to the day before or after the ban. If households lump travel from one weekday to another, the policy would decrease the number of cars on the road on a given day, but keep the amount of weekday driving fairly constant. If drivers do not use their cars every day and simply shuffle their schedule instead of lumping travel, neither the number of cars on the road nor the amount driven would change. De Grange and Troncoso (2011) mention that many households, particularly poorer ones, do not use their car every day and thus may be unaffected by a ban even on days that a ban applies. This is particularly true for older, lower quality cars that are likeliest to be the highest polluters.

2.3 Network effects

Drivers unaffected by the ban may adjust their behavior in response to the behavior of those banned from driving. For example, residents who are not restricted may choose to drive more, if a driving ban reduces congestion by getting restricted cars off the road (Eskeland and Feyzioglu 1997; Wang, Xu, and Qin 2014;

Sun, Zheng, and Wang 2014; Gu, Deakin, and Long 2017). Empirical studies of responses to increases in road supply suggest that any reduction in congestion from banned drivers would quickly be filled by others (Cervero and Hansen 2002; Duranton and Turner 2011). Such a rebound effect is not unique to a car ban. Indeed, such a response by other drivers to reduced congestion would be expected from any policy to reduce driving, in contexts where congestion is a constraint. Despite its potential importance, however, this effect is difficult to isolate and test using available aggregate data or cross-sectional disaggregate data.

Shifting travel to other modes may also compromise car restrictions' air pollution impacts, if the new mode of travel produces substantial amounts of pollution, like Mexico City's informal minibuses and minivans which are often old and highly polluting (see Wirth 1997 for a description of the fleet quality). In 1994 and 2007, 50% and 60% of trips involved at least one leg on a minivan or minibus (Guerra 2014). While mode shift to public transit may be generally desirable from a congestion and pollution perspective, the effect of the shift on air pollution would depend on the amount of pollution produced by those modes. In 2006, road-based transit produced 16% of CO_2 emissions in Mexico City (SETRAVI 2010, Table 2). Taxis, meanwhile, produced 12% of CO_2 emissions, but only moved about 6% of people (INEGI 2007).

2.4 Driving despite the ban (a.k.a., cheating)

Although studies such as Davis (2008) and Eskeland and Feyzioglu (1997) assume high compliance with the policy, cheating may be fairly commonplace. Indeed, there are any number of ways that a driver could cheat to avoid a car ban, such as using a fake license plate, bribing police, or simply ignoring the ban and relying on patchy enforcement or low penalties for violations. Following the implementation of *Doble Hoy No Circula*, a local newspaper reported observing 100 trucks without license plates over a five-hour period on a major arterial, and the government reported just over 5,000 sanctions over the course of a month (*Mexico News Daily* 2016).

Only three peer-reviewed studies examine noncompliance in any detail. Their authors come to different conclusions despite examining the same policy in the same city (Beijing). Matching license plates to survey days from a household travel survey, Wang, Xu, and Qin (2014) report that Beijing residents complied only half of the time; 48% of the 730 restricted vehicles were used illegally on the survey day. Travelers were less likely to break the rules when traveling to the city center or during peak hours, suggesting differences in enforcement, transit quality, or travel flexibility by geography play a role. Gu, Deakin, and Long also find that households are more likely to drive on banned days for shorter trips with a presumably lower chance of getting caught. By contrast, Viard and Fu (2015) find near universal compliance when examining the entry day and time of vehicles by license plate number in an off-street parking garage serving a mall and office complex in Beijing. The authors suggest that high compliance may be why they find the car ban to have a stronger impact on pollution than earlier studies from Mexico City.

3. Context, Data and Approach

3.1 Hoy No Circula

Hoy No Circula's restrictions have evolved over time. At the time of our analysis in 2007, *Hoy No Circula* applied to significantly fewer vehicles than the original 20% ban implemented in 1996. Starting in 2004, vehicles under 2 years old were exempted to encourage fleet turnover, and gasoline-powered vehicles under

10 years old could get exempted from the policy by passing an emissions test and obtaining a special exemption sticker. Based on the regulation and reported vehicle characteristics, we estimate that less than 5% of vehicles (accounting for 4.4% of private vehicle travel) would be banned during a given weekday in 2007 — comparable to an unpublished estimate by the Centro Mario Molina that, at the beginning of 2009, only 6.6% of vehicles would have been restricted on a given weekday by the restrictions in place 2007. For a full description of the 2004-2007 ban, vehicles exempted, and other features see Secretaria de Medio Ambiental (2006, 171–74).

Since 2007, the ban has continued to change to provide new exemptions, encourage specific types of vehicles, and reduce impacts on different types of residents. Since 2008, the ban also applies to certain vehicles on certain weekend days. Starting in 2017, exemptions will be based on the level of emissions, rather than the age of the vehicle for any vehicle older than two years.

3.2 Research Approach

Our empirical approach takes advantage of the sharp cut-point that determines whether a vehicle is subject to the *Hoy No Circula* restrictions. At ten years old, almost all vehicles are prohibited from driving one day per week. At nine years old, residents can get an exemption from the policy after passing a smog test. While vehicle utilization and household income are likely to vary with vehicle age, not least because households that plan to drive more are likely to purchase a newer vehicle, no other factor sharply changes between nine and ten years of age.[2] Thus, our empirical design examines the response to car bans at the correct ecological unit, the household, and captures how the behavior of households subject to the policy differs from similar households that are not subject to the policy.

Relying on this sharp change in policy, we use a regression discontinuity analysis to identify the effects of a one-day-per-week car ban. We seek to identify a statistically significant difference between households where the oldest vehicle is just over or just under the age threshold that subjects it to the driving restrictions. Figure 1 shows a schematic version of how a hypothetical impact of *Hoy No Circula* would be revealed in the data. The expectation is that, if the policy works as intended, households on the left-hand side of the policy should drive approximately 80% as much as households just on the right side of the policy line. In addition to our main results regarding the impact on vehicle travel, we use a similar discontinuity analysis to graphically explore potential explanations for the (lack of) effectiveness of the policy, such as second-car purchases.

[2] A formal test of a regression discontinuity indicates that there is no sharp change in any of the covariates. Substituting each covariate as the dependent variable in place of the log VKT dependent variable in our preferred specification (presented below) yields coefficients for ρ that are insignificant at $p=0.10$. Moreover, graphical analysis shows no evidence of any discontinuity. These results are available from the authors on request.

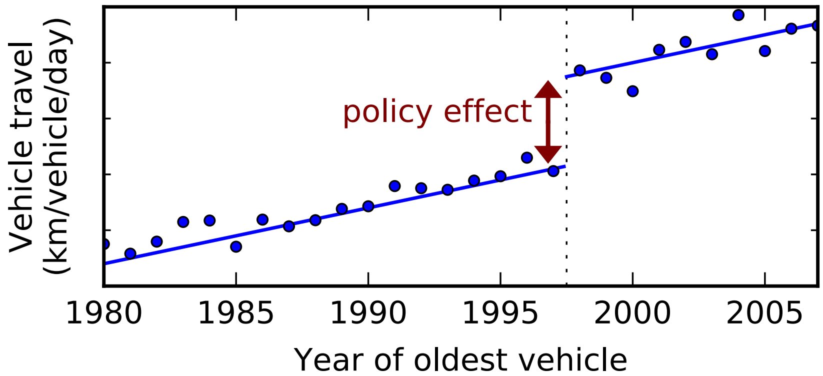

Figure 1 *Schematic impact of regression discontinuity analysis*

The assumption is that households on either side of the discontinuity are comparable in all respects, except that one set of households is subject to the policy and the other can seek an exemption. Regression discontinuity has been used in studies of driving restrictions (Davis 2008; Salas 2010; Gallego, Montero, and Salas 2013; Viard and Fu 2015; Lin Lawell, Zhang, and Umanskaya 2015), as well as in transportation and urban planning research more broadly (Deng and Freeman 2011; Millard-Ball, Weinberger, and Hampshire 2013).

The nine-to-ten-year threshold is not a strict discontinuity, given that some nine-year-old vehicles are subject to the car ban if they do not provide an emissions certification. However, the emissions test provides only a minimal hurdle to driving, given the relative ease of bringing a vehicle into compliance, or fraudulently obtaining an emissions certification. Moreover, the more a household drives, the more likely it is to obtain the certification for their nine-year-old vehicle. Unfortunately, we do not have data on the share of nine-year-old vehicles that obtain the certification.

Finally, we note that this research design does not provide an estimate of the changing effect of the policy over time, but rather of the long-run effect of the policy. The government began implementing the restrictions in place during the 2007 travel survey in 2004. At the time of the analysis presented below, households had had several years to adjust behavior and purchase and sell vehicles in response, and our results should be seen as reflective of the impacts of a driving restriction in equilibrium. Unfortunately, the cost of undertaking household-level travel surveys means that they are rarely conducted more frequently than each decade and almost invariably sample from different households; thus, a longitudinal analysis is not possible.[3]

Also, we estimate the impact of the policy on a specific subset of households: those with a nine- or ten-year-old vehicle. Formally, we estimate the local average treatment effect. If even these households do not drive less in response to the ban, others likely do not drive less either.

[3] Prior to 2007, the most recent travel survey was conducted in 1994. Another was recently completed, but is based on intercept instead of household surveys and is not yet available to policy-makers or researchers.

3.3 Model specification

The local effect of *Hoy No Circula* on the vehicle travel of households subject to the policy can be assessed by comparing one-car households with a vehicle just under the age threshold, with households with a vehicle just over the age threshold. Controlling for vehicle age (as a cubic trend) and household demographics and location, we estimate the impacts of the policy through Eq. (1):

$$y_i = \alpha + \rho D_i + \gamma_1 v_i + \gamma_2 v_i^2 + \gamma_3 v_i^3 + \gamma_4 D_i v_i + \gamma_5 D_i v_i^2 + \gamma_6 D_i v_i^3 + \beta X_i + \epsilon_i \quad (1)$$

Where: y_i indicates the log of vehicle kilometers traveled by household i on the survey day; ρ is the estimated effect of the *Hoy No Circula* policy; D_i is a dummy variable that indicates whether the household's vehicle is older than nine years and thus subject to the restrictions; v_i is the age of the vehicle, centered on the nine-year threshold (so that $v_i > 0$ indicates that a vehicle is older than nine years); $\gamma_1 ... \gamma_6$ are estimated coefficients that allow for a flexible parametric relationship between vehicle age and vehicle travel, and for that relationship to vary either side of the age threshold; X_i is a vector of covariates for household i; β is a vector of coefficients; $\propto$ is the intercept; and ϵ_i is a mean-zero error term. Our Python/R code is available on request.

Our set of covariates consists of the natural log of household income, household size, number of workers, presence of children (binary), the average age of working-age adults in the household, whether the household rents (binary), distance to the city center, and whether the household is within 1km of a metro station (binary), along with a binary variable for each of the 56 municipalities and boroughs in the dataset. In order to avoid an undue influence of distant years affecting the shape of the polynomial relationship, we drop households from the sample where the oldest vehicle is from earlier than 1980. Unless noted, no substantive difference is found if we instead drop households where the oldest vehicle is from earlier than 1988, which gives an equal number of years on either side of the age threshold (1988-1997, and 1998-2007).

3.4 Data sources

Data for our analysis come from the MCMA's 2007 household travel survey (INEGI, 2007). Members of just over 50,000 households report information on income, household size and composition, and 200,000 weekday trips, including the geographic location of origins and destinations, trip purpose, trip duration, trip time, out-of-pocket expenses, and mode of travel. As in Guerra (2014), we combine survey data with transportation infrastructure and Census shapefiles from the National Statistics and Geography Agency (INEGI 2013) and the Secretary of Transportation and Highways (SETRAVI 2013) to estimate local population densities and distance from metro stations and the center of the city. We use the unweighted survey data, but using the provided household survey weights produces almost identical results. Descriptive statistics are shown in Table 1.

The survey includes a question on the number of vehicles that residents did not use on account of the *Hoy No Circula* regulation. However, the question is awkwardly worded and the results are not consistent with expectations of the extent of the ban.[4] For this reason, our main results do not rely on the responses to this specific question.

The original sample includes 51,475 household-level responses. We sequentially drop (i) 1,794 households that report owning a public transit vehicle (e.g. a minibus or *colectivo*), since these vehicles are subject to different restrictions; (ii) 28,338 households that do not own a vehicle; and (iii) 2,570 households where the age of the oldest vehicle is missing or predates 1980. This yields an effective sample size of 18,773 households. For many of the analyses, we further restrict the sample to the 14,202 one-car households for whom vehicle travel data is not missing. In some cases, the sample is further reduced due to missing data for covariates such as income.

	One-vehicle households				**Households with 2+ vehicles**	**Households with 1+ vehicle**
Year of oldest vehicle	*Pre-1997*	*1997*	*1998*	*Post-1998*		
Log VKT	1.10	1.50	1.67	1.98	2.89	1.85
Year of oldest vehicle	1989.7	1997.0	1998.0	2002.7	1997.4	1996.9
Log household income	8.83	8.92	9.02	9.21	9.73	9.19
Household size	4.25	4.21	4.04	3.67	4.26	4.03
Number of workers in household	1.73	1.70	1.65	1.63	2.10	1.78
Average age of adults in household	36.5	36.1	37.0	37.2	37.3	37.0
Children in household? (1=yes, 0=no)	0.481	0.507	0.436	0.398	0.363	0.421
Household rents? (1=yes, 0=no)	0.122	0.161	0.146	0.148	0.097	0.128
Distance to Zocalo (m)	18775.1	17665.6	17041.9	15580.8	15721.2	16754.5
Distance to Metro (m)	7869.4	6844.3	6580.3	5623.1	5796.2	6457.3
Within 1km of Metro? (1=yes, 0=no)	0.15	0.17	0.19	0.22	0.18	0.19
N	6060	485	706	7009	4513	18773

Table 1 Descriptive Statistics

Values indicate the mean for each sub-group.

Data source: INEGI, Encuesta Origen-Destino 2007

4. Findings

4.1 Overall policy effect

Table 2 shows the estimated coefficients from the model specification presented in Section 3.3, as well as several alternative specifications that make different assumptions about the impact of covariates, and about

[4] The survey first asks: "How many of the vehicles available to your household were used on the travel day," and then: "How many of the vehicles available to your household were not utilized on the day of travel because of the 'Hoy No Circula' program." A household that was not planning to use a vehicle regardless of the *Hoy No Circula* restrictions may have had trouble responding to the question, particularly if there were multiple reasons for not driving. For example, only 9.8% (as opposed to 20%) of one-car households whose car is older than 1998 report being banned. See Table 4 for a summary of responses to this survey question.

the shape of the relationship between vehicle age and vehicle travel. Model 1 represents the estimates from Eq. 1, and assumes a cubic relationship between vehicle age and vehicle travel. The alternate specifications assume a linear (Model 3), quadratic (Model 4) or quartic relationship (Model 5); constraining the relationship to be the same either side of the age threshold (Model 6; i.e., $\gamma_4 = \gamma_5 = \gamma_6 = 0$); and dropping the demographic and locational covariates (Model 2).

Model 1, our preferred specification, shows a small but statistically insignificant (p=0.31) effect of the driving restrictions. Models 3 and 6 do find a significant effect, but this is likely because the relationship between vehicle age and vehicle travel is not well captured by the linear model (Model 3), or the single cubic polynomial (Model 6).[5] Figure 2 provides evidence that our preferred specification best captures the relationship between vehicle age and vehicle travel.

The absence of an effect of *Hoy No Circula* is also evident from a plot of vehicle travel against vehicle age (Figure 3). Households with a nine-year-old car drive a similar amount to households with a ten-year-old vehicle. Almost identical results are obtained when only considering vehicle travel during the 5AM-10PM period, when the restrictions are in force. Similar results are also evident from the plots in Figure 3 of the coefficients of binary variables for each vehicle age, after controlling for household income and other demographic and locational variables.

This null finding is particularly notable since our method risks overstating any effect of *Hoy No Circula*. We estimate the effect on households that own a ten-year-old vs a nine-year-old car. If the households most directly affected by the policy do not drive less due to *Hoy No Circula*, those less affected by the policy almost certainly do not either. As seen in Figure 3, older cars tend to be driven less, so the impact on owners of (say) 15-year-old cars is likely to be less than those for 10-year-old vehicles. Moreover, any effect we find may be inflated by selection bias, since the more a household drives, the more it is likely to replace a ten-year old vehicle with a newer one to avoid the license-plate restriction.[6] Thus, our null finding is even more notable.

In summary, we find no robust evidence that the driving restrictions have reduced vehicle travel. This is in line with previous studies on the air pollution impacts of the policy (Eskeland and Feyzioglu 1997; Davis 2008; Gallego, Montero, and Salas 2013). We now turn to some of the potential explanations for the lack of any measurable impact, drawing on the theoretical framework developed above. The following sub-sections consider in turn each group of the behavioral responses that would dampen the effects of the policy.

[5] That the poor fit rather than *Hoy No Circula* is responsible for the statistically significant coefficients can be seen in three ways. First, Figure 2 plots the raw data against the fitted model. In both Model 3 and Model 6, a spurious policy effect arises because the model specification underestimates driving to the left of the discontinuity (the data point is well above the line estimate), and overestimates driving to the right (the point is well below the line). Second, estimating Model 3 with different (i.e. incorrect and synthetic) discontinuity years yields statistically significant coefficients of a similar size (results available from authors on request). Third, for Model 6, the effect disappears at conventional levels of statistical significance ($p<0.05$) when considering only households with a 1988 or newer model vehicle, so that the polynomial function is less sensitive to years that are far removed from the discontinuity.

[6] In the terminology of Lee and Lemieux (2010), buying a newer car in response to the driving restrictions represents a manipulation of the assignment variable in a regression discontinuity design.

	Model 1 *Cubic*	Model 2 *Cubic, no covariates*	Model 3 *Linear*	Model 4 *Quadratic*	Model 5 *Quartic*	Model 6 *Cubic, same either side of discontinuity*
Dependent variable	log VKT	log VKT	log VKT	log VKT	log VKT	log VKT
Policy ρ	-0.117 (0.116)	-0.156 (0.109)	-0.347*** (0.0547)	-0.133 (0.0852)	-0.0436 (0.151)	-0.229*** (0.0681)
Vehicle age γ_1	-0.0738 (0.0739)	-0.0652 (0.0688)	-0.0351*** (0.00739)	-0.0957*** (0.0300)	-0.129 (0.157)	-0.0484*** (0.00741)
Age^2 γ_2	-0.000715 (0.0173)	0.00329 (0.0161)	--	-0.00627** (0.00299)	-0.0256 (0.0643)	-0.000511 (0.000486)
Age^3 γ_3	0.000382 (0.00116)	0.000527 (0.00109)	--	--	-0.00353 (0.00980)	0.000115*** (4.33e-05)
Age^4	--	--	--	--	-0.000199 (0.000495)	--
Age-policy interaction γ_4	-0.0159 (0.0827)	-0.0250 (0.0770)	0.00920 (0.00843)	0.0302 (0.0337)	-0.00943 (0.175)	--
Age^2-policy γ_5	0.00661 (0.0180)	0.00155 (0.0168)	--	0.00859*** (0.00311)	0.0442 (0.0667)	--
Age^3-policy γ_6	-0.000520 (0.00118)	-0.000617 (0.00110)	--	--	0.00225 (0.00992)	--
Age^4-policy	--	--	--	--	0.000232 (0.000497)	--
Intercept $\propto$	-1.220*** (0.232)	1.657*** (0.0811)	-1.134*** (0.220)	-1.239*** (0.225)	-1.252*** (0.245)	-1.164*** (0.220)
Other covariates	Yes	No	Yes	Yes	Yes	Yes
Fixed effects for municipality	Yes	No	Yes	Yes	Yes	Yes
N	12,090	14,202	12,090	12,090	12,090	12,090
R^2	0.118	0.075	0.118	0.118	0.118	0.118
AIC	10,232	12,416	10,236	10,229	10,235	10,230

Table 2 Regression discontinuity estimates

*** Significant at p=0.01. ** Significant at p=0.05. * Significant at p=0.10.*
Heteroskedasticity-consistent standard errors in parentheses.
Sample consists of one-vehicle households where the age of the oldest vehicle >=1980.

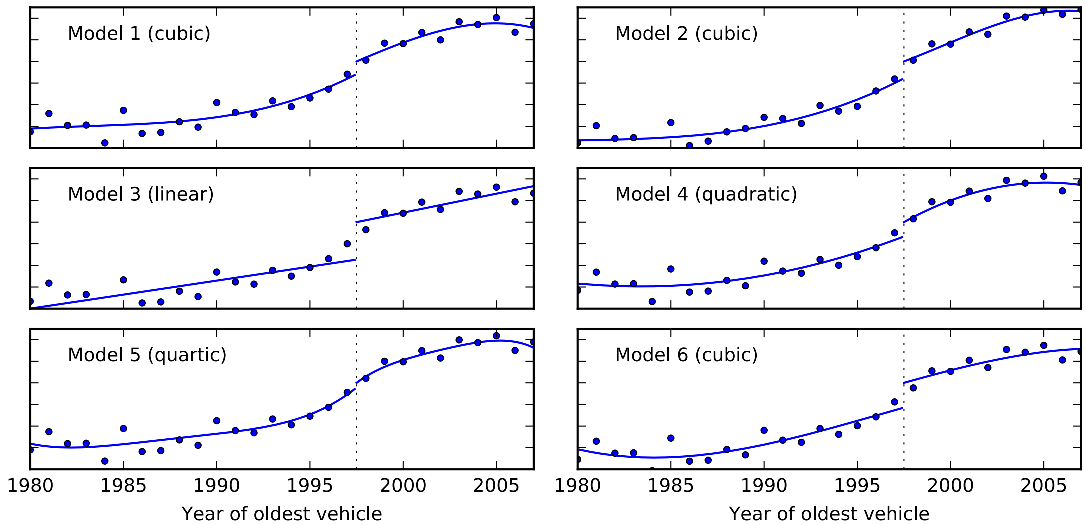

Figure 2 Fit of regression discontinuity models

Points indicate the mean residual ϵ_i for each age category, plus the estimated policy effect ρ and polynomial terms $\boldsymbol{\gamma}$. Lines indicate the estimated policy effect ρ and polynomial terms $\boldsymbol{\gamma}$, without including the residuals. The aim is to show the fit of the polynomial function, while accounting for the impact of the other covariates in each model specification.

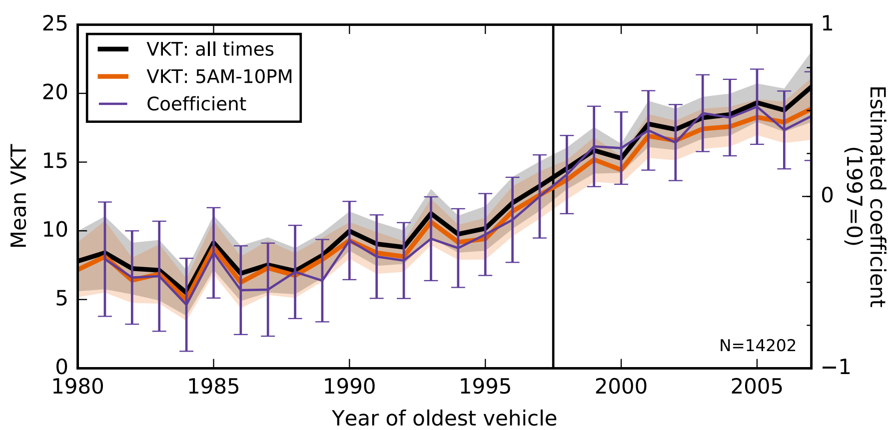

Figure 3 Vehicle travel vs vehicle age

Shaded areas and error bars indicate the 95% confidence interval. The vertical line indicates the threshold at which most vehicles are subject to the Hoy No Circula restrictions. The coefficients[7] have a similar interpretation to the solid line trends, except that they incorporate the effects of household demographics and location.

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[7] The coefficients are estimated via the following equation: $y_i = \alpha + \gamma_1 V_{1i} + \gamma_2 V_{2i} + \ldots + \gamma_N V_{Ni} + \beta X_i + \epsilon_i$. Where: y_i indicates vehicle kilometers travelled by household i on the survey day; $V_{1i} \ldots V_{Ni}$ are binary variables that indicate whether the vehicle owned by household i is 1…N years old; $\gamma_1 \ldots \gamma_N$ are the coefficients plotted in Figure 3; X_i and β are the full set of covariates and coefficients listed above; and $\epsilon_i$is the error term.

4.2 Changes in car ownership

4.2.1 Purchase of an additional vehicle

We find no evidence that households are responding to driving restrictions by purchasing an additional vehicle. First, there is no discernible uptick in vehicle registrations in the MCMA after 1989, either compared to the years prior to 1989 or to the rest of the country (Figure 4). If households are purchasing an additional, presumably older, vehicle in order to avoid the driving restrictions, registrations in the MCMA should rise more rapidly following the 1989 policy introduction, compared to other states in Mexico.

Second, there is no discontinuity in vehicle ownership against the age of the oldest vehicle (Figure 5, panel A). If the policy encourages households to purchase a second car, a household where the oldest vehicle is ten years old should be more likely to own a second car than a similar household where the oldest vehicle is nine years old. But to judge from Figure 5, if anything the reverse is true — households with an older vehicle are less likely to purchase a second car.

In any case, the small prevalence of two-car households with two older vehicles would make it surprising if many households had responded to the restrictions by purchasing a second car. Only 3.0% of car-owning households, and 1.2% of households overall, have two pre-1998 vehicles (Table 3). This is the group that may have responded to the policy through the mechanism that bears the closest resemblance to the second car purchases that Davis (2008) and Eskeland and Feyzioglu (1997) conclude were responsible for reducing the effectiveness of *Hoy No Circula* prior to the change to allow exemptions for newer vehicles in 1997. From 1997 onward, many of these households could have purchased newer cars instead of a second car. Any such response, however, ought to be evident in a notable discontinuity in the driving behavior of those with 1997 vs 1998 model-year cars. As presented in the previous section, no such discontinuity is evident.

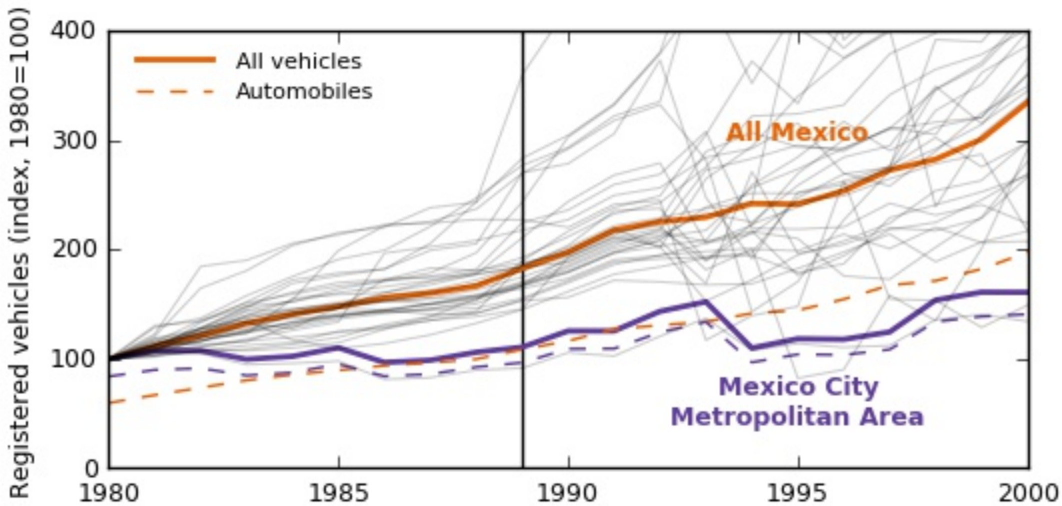

Figure 4 Vehicle registrations, 1980-2000

Gray lines indicate trends for individual states. Registrations include all motor vehicles, including motorcycles, trucks, commercial, and government vehicles.

Data source: Sistema Estatal y Municipal de Bases de Datos (INEGI 2016)

Category	**Number of households**	**Percent**
No car	28,331	60.1
One vehicle, pre-1998	6,545	13.9
…of which pre-1997	*6,060*	*12.9*
…of which year=1997	*485*	*1.0*
One vehicle, post-1997	7,715	16.4
…of which year=1998	*706*	*1.5*
…of which post-1998	*7,009*	*14.9*
2+ vehicles, all pre-1998	571	1.2
2+ vehicles, all post-1997	2,745	5.8
2+ vehicle, mixed ages	1,197	2.5

Table 3 Vehicle ownership by age of vehicle

Just 1.2% of households have two pre-1998 vehicles, suggesting that very few households buy a second car in order to circumvent the Hoy No Circula *driving restrictions.*

Data source: INEGI, Encuesta Origen-Destino 2007

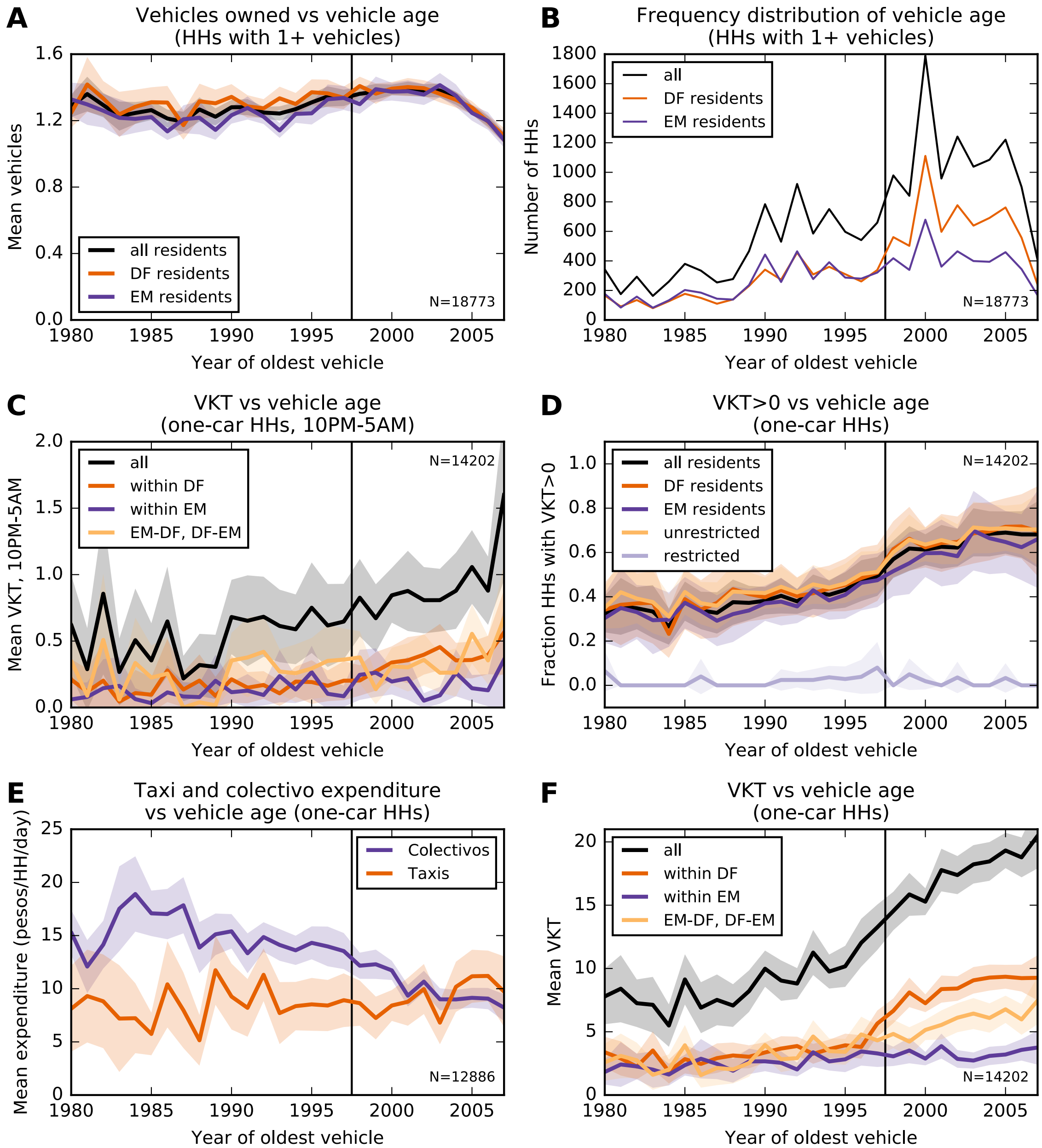

Figure 5 Vehicle ownership and travel behavior by age of oldest vehicle

Shaded areas indicate 95% confidence interval. Vertical line indicates the threshold at which most vehicles are subject to the Hoy No Circula restrictions. DF = Distrito Federal (Federal District). EM=Estado de México (State of Mexico). Unrestricted and restricted refer to a household's response to the question on whether they did not drive on the survey day because of the Hoy No Circula restrictions (see main text). Data source: INEGI, Encuesta Origen-Destino 2007. The sample size differs between the plots because of the restriction to one-car households, and because of missing responses on taxi and colectivo *fares.*

4.2.2 Vehicle replacement

If households respond to driving restrictions by replacing an older vehicle with a newer model that is exempt from the policy, there should be a marked decline in the number of households whose oldest vehicle is just within the threshold that subjects them to the restrictions. One would expect many of these cars to be resold elsewhere in Mexico, for example in surrounding cities such as Puebla and Cuernavaca. Figure 5, panel B, provides some suggestive evidence that this is the case; there is a dip in the frequency of households that own a model year 1993-1997 vehicle.[8]

4.3 Other behavioral responses

4.3.1 Shift trips to nighttime hours

One way that households with an older vehicle may respond to the driving restrictions is to shift trips to times outside the enforcement period of 5AM to 10PM on weekdays. If that were the case, one would expect one-car households with a nine-year-old vehicle to drive less at night than those with a ten-year-old vehicle. Figure 5, panel C, however, provides no evidence that this shift is occurring. While the confidence interval is large (reflecting the fact that relatively few trips are made at night), the plot gives no indication of a discontinuity. While studies in other cities have shown a larger temporal reshuffling effect, the relatively long hours of the Mexico City restrictions are likely to produce a smaller effect. It is likely to be hard, for example, to reschedule many trips to 4:30AM.

4.3.2 Shift trips to other days

A similar way that households may reshuffle trips is to shift them to another day. This could happen in one of two ways. Households could lump trips from the restricted day onto other trips on the unrestricted days. If households do not drive every day, they could also shuffle their weekly schedule to avoid driving on a restricted day. These are particularly plausible behavioral responses for discretionary trips where the timing can be flexible, such as shopping and recreational trips.

If households are lumping trips on other weekdays there should be a discontinuity in the proportion of cars not driven on a given weekday, but no discontinuity in average VKT. A household with a nine-year-old vehicle should drive about 25% more days per week (or, equivalent, have a 25% greater probability of driving on a given weekday) compared to a household with a ten-year-old vehicle. If households are shuffling their driving days, and making trips on a day when they previously would not have driven at all, there should be no discontinuity in either VKT or the proportion of cars not driven on a given weekday. Thus, in this case, the *absence* of a discontinuity indicates either that households are responding by shuffling their weekly schedule, or that the policy is not being enforced.

Figure 5, panel D, provides little evidence of such a discontinuity. While the probability that a household drives on a given day is increasing as its vehicle gets newer, there is no sharp jump between nine and ten years — and certainly, the increase is less than 25%. The implication is that a household that is restricted

[8] Interpretation is complicated by the large spike in 2000, which is not consistent with official registration figures or manufacturing data. This issue is discussed in the Limitations section.

from driving on, say, Monday, is instead reorganizing weekly travel to make those trips on another day when no car travel would have otherwise occurred.

Given that only 38% of households with a single older (pre-1998) vehicle drove on the day of the survey, drivers have considerable scope to shuffle weekly travel schedules and shift trips around in this way. Even among households with a single newer vehicle, only 64% drove on the survey day. While some of these households may use their car for weekend travel only, the lower level of weekend congestion suggest that this group will be a minority. As the household travel survey only provides data on weekday travel, we are unable to assess the extent to which trips are shifted to weekend days — a limitation of relying on a weekday travel survey.

The survey question on the *Hoy No Circula* program, while difficult to interpret as discussed above, provides further evidence for the potential of trip shifting. Of one-car households that did not drive on the survey day, just 14% (1072 out of 7658) indicated that this was because of the driving restrictions (Table 4).

Number of vehicles used on the survey day	Number of vehicles not used because of *Hoy No Circula*			
	0	1	2+	Total
0	6,582	1,072	*4*	7,658
1	8,269	*69*	*6*	8,344
2+	56	*4*	*2*	62

Table 4 Households not using a vehicle because of Hoy No Circula (one-car households)

Cell entries refer to unweighted number of households. Entries in italics likely represent a respondent error, or households that borrowed a vehicle on the survey day.
Data source: INEGI, Encuesta Origen-Destino 2007

4.3.3 Shift trips to other modes

One of the goals of the *Hoy No Circula* policy is to encourage mode shift away from the private car. Indeed, mode shift to the subway, public buses, walking or cycling would reduce vehicle travel and emissions. However, mode shift to taxis and, to a lesser extent, minibuses and minivans, may partially or completely offset these reductions. Figure 5, panel E, provides no evidence of such a mode shift. Expenditure on taxis and *colectivos* declines steadily as the household's vehicle gets newer, as one would expect given that both vehicle age and taxi use correlate with income. There is no sharp decline between nine and ten years that would indicate that private car trips are shifting to taxis or *colectivos*.

4.4 Network effects

If the *Hoy No Circula* policy were successful in reducing vehicle travel by households subject to the restrictions, one would also have to consider the responses by other road users. Reduced vehicle travel would be likely to reduce travel times, particularly given the congested conditions that characterize Mexico City. In response, other households that were not affected by the restrictions would be likely to travel more — the induced travel or "triple convergence" phenomenon (Downs 2004). The net effect might still be substantial for air pollution, if the induced travel takes place in newer, lower-emission vehicles, but the travel demand impacts would be lessened or, at the extreme, eliminated entirely.

The absence of a main effect (Table 2 and Figure 3) means that such network effects will be small in the MCMA. However, in places where driving restrictions are more extensive, for example affecting more vehicles or each vehicle on more days, network effects may offset the direct impact of the policy.

4.5 Lack of enforcement

Unlike Wang et al. (2014) and Gu, Long, and Deakin (2017), we do not have license plate data to examine the extent of policy avoidance by cheating. However, we can exploit likely differences in how the policy is enforced to see whether residents ignore the ban when possible. Enforcement of car restrictions is almost certainly uneven within a metropolitan area. In some neighborhoods, the police presence may be less extensive or officers may turn a blind eye or engage in corrupt practices. Within the MCMA, the centrally located Federal District has an unusually high police presence, while the State of Mexico does not. When arrested in the State of Mexico, for example, drug kingpin Edgar Valdez Villarreal (*La Barbie*) indicated that operating within the Federal District was too difficult due to the police presence (The Economist 2011).

While we do not directly observe the level of enforcement or corruption, we use whether trips take place wholly or partially within the State of Mexico as a proxy for the level of enforcement. We would therefore expect any effect of the driving restrictions to be less pronounced in the State of Mexico.

Figure 5, Panel F provides some suggestive evidence for this hypothesis. The relationship between vehicle age and vehicle travel is more pronounced for trips that are wholly within the Federal District (red line). In the State of Mexico, older vehicles are driven more in relative terms. However, even within the Federal District, any discontinuity is not sharp, and there are also many other factors, such as income and access to transit, that distinguish trips in the two geographic areas.

4.6 Limitations

While using a cross-section of household-level data and a research design based on the varying restrictions of vehicle ages allows us to test more hypotheses about whether and how households adjust behavior to avoid a car ban, the data and analysis also present several limitations:

- The survey does not include weekend observations, preventing us from testing whether households shift travel to the weekend.
- The reporting of vehicle year includes some likely inaccuracy due to rounding with a noted spike in 2000, 1990, and 1980 vehicles. However, rerunning the regression discontinuity estimates with year 2000 vehicles excluded produces almost identical results to those in Table 2.
- We examine travel behavior rather than the emissions that result from travel behavior. Sun, Zheng, and Wang (2014) find a quadratic relationship between congestion and pollution with decreased congestion only reducing pollution on days where congestion is low. They conclude that since

Beijing's roads are generally congested, effectively implemented car bans may actually increase pollution by reducing congestion. Lin Lawell, Zhang, and Umanskaya (2015) suggest that driving restrictions may reduce some pollutants but increase others, depending on shifts in mode and changes to the vehicle fleet.

- The cross-sectional nature of the research design prevents an analysis of how households respond to travel bans over time or in the short-run.
- We do not assess the impacts of *Hoy No Circula* on either welfare or air pollution, which may differ from the effects on driving. To the extent that it has encouraged drivers to purchase newer vehicles, the effects here will be larger than those on vehicle travel. However, *Hoy No Circula* and other driving restrictions are often presented as a way to reduce driving, not just air pollution, particularly in cities where congestion is endemic.

5. Conclusions

Driving restrictions are some of the most high-profile ways in which a city can tackle congestion and air pollution. At least in Mexico City, however, the impact of *Hoy No Circula* may be much more limited than the popular attention implies. Using disaggregate household travel survey data, we find no effect of the car ban on vehicle travel, most likely for two main reasons. First, the potential of the policy to reduce vehicle travel is constrained by the relatively small fraction of the vehicle fleet that is affected. Second, households have numerous behavioral responses available to them, which allow them to continue driving as much as before.

In line with most previous studies of car bans, we conclude that *Hoy No Circula* has done little-to-nothing to reduce overall vehicle travel. However, we find no evidence to support the prevailing theory that its lack of success is due to second car purchases as argued in landmark studies of *Hoy No Circula* (Eskeland and Feyzioglu 1997; Davis 2008). As presented in Section 2.1, the actual empirical evidence supporting this second car hypothesis is weak, or even contradictory. That large numbers of households would purchase a second car with a license plate that ends in a different digit is unlikely given the costs of a second vehicle relative to other avoidance strategies and empirically unsupported in our analysis.

Instead, we highlight a series of other, low-cost behavioral responses by households that compromise the effectiveness of a car ban. No previous research has used disaggregate household-level data to analyze the impacts of a full suite of behavioral responses listed in Table 5, but instead has focused on one or two. The range of potential responses mean that there is likely no single explanation for the limited effects of *Hoy No Circula*. However, shifting trips to other days stands out as particularly plausible given its low costs to households and the results of our empirical analysis. Faced with a car ban on (say) Wednesday, a household may be able to shift shopping, recreation and other discretionary trips to (say) Thursday. Most cars in Mexico City are not driven every weekday, and this is particularly true of the older vehicles targeted by *Hoy No Circula*. Households with a single vehicle that is ten years or older drive it an average of just 1.9 weekdays per week. In general, the households that own the oldest and highest polluting vehicles are also the least likely to use that car every weekday, regardless of a ban.

Where shifting the day of travel is more costly or infeasible, as in the case of some commuting trips, other behavioral responses are available. Rather than buy a second vehicle, a household might simply replace its car with a newer model (which is an intended effect of the policy), or rely on corrupt officials or patchy enforcement. Few households appear to be shifting to taxis or *colectivos* as a response.

The driving restrictions in Mexico City, then, appear to primarily represent a modest inconvenience to a small number of drivers, rather than an effective way of bringing about major reductions in vehicle travel or pollution. Moreover, given the constraints of congestion on vehicle travel, any reductions that did come about would likely be mostly or completely offset through induced travel by other drivers — the "triple convergence" phenomenon.

Behavioral Response	**Evidence from Mexico City**
Buy a second car	No support
Replace an older car with a newer model	Unclear
Shift trips to nighttime	No support
Shift trips to other days	Supported by theory and travel survey data
Shift trips to other destinations	Unable to assess
Shift trips to taxis and *colectivos*	No support
Network effects	Possible, but assumes a first-order impact of the car ban on vehicle travel
Lack of enforcement	Unclear

Table 5 Summary of behavioral responses by households

5.1 Policy Implications

In this study, we investigated behavioral responses to a specific car-restriction policy at a specific point in time, nearly ten years ago. The Mexico City program is more limited in scope than the driving restrictions implemented in recent years in other cities. *Hoy No Circula* applies to older vehicles on one day per week. In contrast, the temporary restrictions in Delhi applied on an odd-even basis to half the vehicles each day, albeit with exemptions for female drivers, government ministers and other special categories. Cities like Santiago and Mexico City, furthermore, increase the severity of restrictions when pollution is particularly bad. The more stringent the restriction (and its enforcement), the less feasible will be these behavioral responses that offset the direct impact of the policy. Nevertheless, a number of the findings are highly relevant to other cities that are considering adopting or amending license-plate-based restriction schemes.

First, using a substantially different data set and methodology from existing studies, our study supports the overall assertion that car bans do not reduce driving or local pollution. This should give policymakers pause when considering whether or not to implement a license-plate based restriction.

Second, the heart of this failure is not second-car purchases but the myriad of small behavioral adjustments that allow single-car households to avoid the ban altogether. This suggests that adjustments to make a policy harder to avoid through the purchase of a second vehicle are unlikely to make the policy more effective. However, it also suggests that the economic costs of car restrictions to consumers may be lower than previously estimated. If the ban is relatively easy to avoid, it is also relatively non-burdensome to consumers. The perverse truth is that the more effective a license-plate ban is at keeping drivers off the road, the higher the welfare costs will be. A policy that restricts drivers for two or even more days per week may have a more of an impact on traffic volumes, but will also increase the costs for drivers who reshuffle their trips and find other ways to get around it.

Third, policymakers need a better understanding of drivers' behavior in order to get better estimates of how drivers will respond to new policies. Contrary to the popular view of drivers driving all the time, many drivers in developing cities leave their cars at home, particularly for work trips, which are often longer, more expensive, and require paid parking at the end. This is even more likely for drivers with the older kinds of vehicles that produce the most pollution. We found that only 38% of vehicles older than 10 years in single-car households were used on the day of the survey. This type of vehicle is the primary target of policies like *Hoy No Circula*. Similar driving rates occur in other cities where car restrictions are in place or under consideration. For example, more than a third of Beijing's households with unrestricted cars do not drive on a typical weekday (Gu, Deakin, and Long 2017, 7). In Buenos Aires, households with cars (and that also made trips on the survey day) only used the car on 50% of weekdays. For the poorest households, it was only 41% of the time (Proyecto de Transporte Urbano para Áreas Metropolitanas 2010).

Fourth, the ability of a car ban program to reduce emissions and its economic welfare costs will depend on the alternatives available to households. Where transit or other modes provide close substitutes for driving, car bans may produce substantial emission reductions with little welfare loss. Indeed, this is the desired response by policy makers. Where households prefer to respond by shuffling the days on which a trip is made, or even by purchasing a second car, the effectiveness of the ban is undermined, even if households are only lightly inconvenienced by the restriction. If a ban is hard to avoid by any means, on the other hand, it is likely to be socially expensive. Paradoxically, this suggests that car bans may be more successful in reducing congestion and emissions in areas with few discretionary trips and high rates of driving.

Finally, there remains a need for a more detailed understanding of precisely how people avoid license-plate based car bans. While we present evidence that it is not through secondary car purchases, the precise nature and extent of the avoidance mechanisms remains somewhat elusive. In-depth surveys or weekly, rather than daily, travel surveys will likely be required to advance knowledge in this area. Nevertheless, our findings suggest households have a variety of avoidance mechanisms at their disposal. If reducing car travel and pollution is the primary goal, driving restrictions need to be designed in a way that anticipates how households will seek to get around them. This will be particularly challenging in places where only a minority of car owners drive every day.

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