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Abstract

Driving restriction programs have been implemented in many cities around the world to alleviate pollution and congestion problems. Enforcement of such programs is costly and can potentially displace policing resources used for crime prevention and crime detection. Hence, driving restrictions may increase crime. To test this hypothesis, we exploit both temporal and spatial variation in the implementation of Quito, Ecuador's *Pico y Placa* program, and evaluate its effect on crime. Both difference-in-difference and spatial regression discontinuity estimates provide credible evidence that driving restrictions can increase crime rates.

Keywords: Crime, Difference-in-Differences, Regression Discontinuity, Crime displacement
JEL Codes: C20, Q52, R28, R48

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1 Introduction

Many cities in Latin America, Asia and Europe have imposed restrictions on the use of motor vehicles in an effort to reduce traffic congestion or improve air quality. The restrictions limit use of vehicles in either all or part of a city for part of the day.[1] The various programs differ in terms of the types of vehicles that are targeted, the size of the restricted zone, and the times of day during which restrictions are in effect, but they share common goals of either reducing traffic congestion or improving air quality, or both.

A handful of studies have examined the effectiveness of these programs, focusing primarily on their ability to improve air quality. Mexico City's program has received the most attention,[2] but recent papers also examine the programs in Sao Paolo, Bogota, Beijing, Tianjin, Santiago and Quito.[3] Most studies in the literature conclude that permanent driving restrictions have not reduced traffic congestion or air pollution. Where reductions have been detected, they have been short-lived, lasting less than a year. The exception are the studies of Beijing and Quito's programs, where noticeable improvements in air quality are attributed to the implementation of driving restrictions. Viard and Fu (2015) find that every-other-day driving restrictions in Beijing can decrease pollution levels by as much as 19%; Carrillo et al. (2015) show that Quito's driving restriction program, which restricts vehicles one day a week during peak hours, reduces carbon monoxide levels by almost 10%. The "success" of Quito's program is attributed, to a large extent, to its strict enforcement.

In this paper we identify a side-effect of driving restrictions that has yet to be studied:

[1]The best known is Mexico City's *Hoy No Circula (HNC)* program introduced in 1989 to improve air quality. Sao Paulo (Brazil) and Bogota (Colombia) introduced similar programs in 1996 and 1998, respectively. Beijing and Tianjin (China) introduced temporary driving restrictions during the 2008 Olympic Games. Athens (Greece) introduced permanent driving restrictions in 1982. Santiago (Chile) has used a combination of permanent and temporary driving restrictions since 1998 to reduce air pollution. Most recently, in 2015, Paris and several cities in Italy introduced temporary driving restrictions in response to poor air quality.

[2]See for example, Eskeland and Feyzioglu (1997), Davis (2008), and Gallego et al. (2013).

[3]Lin et al. (2011), Chen et al. (2011), de Grange and Troncoso (2011), Troncoso et al. (2012) and Bonilla (2011).

Driving restrictions may *increase* crime rates.[4] It is clear that driving restrictions can have a direct impact on congestion and pollution, but, why would they affect criminal activity? The crime-and-punishment literature suggests at least two reasons. First, enforcement of driving restrictions is a resource-intensive endeavor that is typically the responsibility of the police. The marginal cost of committing a crime depends on the frequency with which criminal activities are detected. When driving restrictions are imposed, the burden of enforcement could result in fewer policing resources being allocated to crime prevention. As crime prevention decreases, so does the marginal cost of crime.[5] Second, the cost of committing a crime also depends on the availability of opportunities to engage in criminal activities. If a driving restriction policy is successfully enforced, it can increase pedestrian flows and public transportation use, raising the number of potential victims. In equilibrium, a decrease in the marginal cost of committing a crime would result in higher crime rates.

To test these hypotheses we evaluate the effects of Quito's *Pico y Placa* program on crime. *Pico y Placa* (PyP) went into effect in Quito on May 3, 2010. It restricts access to the central part of the city. The last digit of a vehicle's license plate number determines the one day of the week on which the vehicle is barred from the road. The PyP program is well suited to being studied because of the availability of data on criminal offenses for the parts of the city that are subject to PyP restrictions as well as those that are not. Moreover, the program restricts vehicles during workday rush (or peak) hours but not weekends or holidays. These features of the program are exploited to identify treatment effects.

Crime data were gathered from two sources. The first is the Ecuadorian National Police. We obtain records of every crime reported to the police between January 2010 and May 2012 in Quito and Guayaquil, the two largest urban areas of the country. Our second source of data is Quito's Municipal Government ("Observatorio Municipal Ciudadano OMS). OMS

[4] Davis (2008) is the only study we are aware of that briefly mentions this possibility.

[5] DiTella and Schargrodsky (2004) and Draca et al. (2011) offer evidence that police presence reduces crime.

collects crime data from the police and creates monthly reports on citizen security. They shared all of their property crime records for the period 2008-2012. Each of these two data source has advantages and weaknesses. The police data is for all crimes reported to the police. This data has information on the time of each crime, but it does not have geocoded information on location of the crimes. The police data are used to calculate the number of crimes of all types that took place every *hour* in Quito and in Guayaquil. The OMS data from Quito's municipality is a subset of the police records with geocoded information on the location of crimes. Unfortunately, OMS data are not comparable across time. Their sample selection criteria changed in April 2009 and reverted back a year later.

Police crime data and a difference-in-difference (DD) strategy are used to assess whether crime rates during the hours when PyP is in effect changed after the introduction of the program. In all specifications, the treatment group is working-day peak hours in Quito. Finding an appropriate control group is not straightforward. Ideally, one would want a control group that, in the absence of treatment, has the same trend as the treatment group. Rather than making an *ad hoc* choice, we use three alternative control groups, each of which can be a reasonable representation of the counterfactual trend, under appropriate conditions. The control groups are: a) non-working-day peak hours in Quito, b) working-day off-peak hours in Quito, and c) working-day peak hours in Guayaquil. Our regression models include a comprehensive set of controls, including month-year fixed effects, day-hour fixed effects obtained by interacting each hour of the day with each working day, and a long list of weather variables. Results show that crime rates in Quito increased during peak hours after the introduction of PyP relative to changes in each of the control groups. The magnitude of the effects is large, between 5% and 10%, and statistically significant at conventional levels. Estimates from our preferred specification suggest that PyP led to an increase of about 0.4 crimes per hour (about 10%) during the restricted hours. Models are also estimated with several "placebo" samples and no statistically significant results are found.

OMS data are used to analyze changes in the spatial distribution of crimes before and after PyP was introduced. We focus on the spatial distribution of crimes near the boundary of the restricted zone. The portion of the boundary that passes through populated areas is demarcated by major roads. Policing resources on these roads, and adjacent areas, are likely to have been diverted to staff a small number of PyP checkpoints located on the boundary. Thus, intensity of PyP enforcement and the potential for displacement of policing resources is likely higher along the boundary. We show that the post-treatment frequency of crimes as a function of the distance to the boundary has a large spike, or "excess bunching," at the boundary. The estimate of excess bunching is large: about 1.6 crimes per meter, over 60% higher than the baseline predicted by a counterfactual without excess bunching. More importantly, we show that in the pre-treatment period, there is very little excess bunching at the boundary.[6]

Excess bunching is much higher in areas just inside the boundary compared to areas just outside. Even though the displacement of policing resources likely affects both sides of the boundary, driving restrictions can dispropoportionately affect economic activity and pedestrian flows inside the restricted zone. For these reason, we also employ a spatial regression discontinuity design to assess if crime rates change discontinuously at the boundary. Intuitively, crime rates just inside the boundary are compared to their counterparts just outside. While in a comparable pre-treatment period the spatial distribution of crime is smooth around the driving restriction boundary, there is a sharp spike just inside the restricted zone in the post-treatment sample. Our preferred model's estimates suggest that PyP has increased the number of crimes along the inside edge of the boundary by as much as 100% compared to crime rates on the outside edge.

The combined empirical findings provide credible evidence that PyP has increased crime rates in Quito during peak hours and near the driving restriction boundary. Do these in-

[6]The difference between post and pre-treatment excess bunching is 1.39 crimes per meter.

creased crime rates reflect a shift in police enforcement allocation or an increase in the use of public transportation and pedestrian flows, or both? We report evidence of a substantial commitment of police resources to PyP. As a result, driving restrictions enforcement has been vigorous: tens of thousands of violations were punished during the first year of the program alone. On the other hand, we find little evidence that pedestrian flows or public transportation use has increased. PyP may induce drivers to use alternative forms of transportation, such as walking or public transportation, during the days when they are subject to the driving restriction, and make them more vulnerable to criminal activities. But we find no evidence that a crime is more likely to occur on a day when the use of one of the victim's vehicles is restricted.

This paper is closely related to studies that analyze the relationship between shifts in monitoring resources and crime displacement. For example, trade-offs in the allocation of police resources have been discussed in Benson et al. (1998), Benson et al. (1992) and Benson and Rasmussen (1991); Yang (2008) also shows evidence of crime displacement in the contexts of a customs reform; Carrillo et al. (2014) find that an increase of monitoring efforts on one specific margin of a firm's tax return can shift misreporting to other margins. Ours is the first study to document that driving restrictions can increase crime rates.

The rest of this paper is organized as follows. The next section provides a simple conceptual framework. Section 3 describes Quito's PyP program. Section 4 describes the data and identification strategy. In Section 5, we discuss our empirical results. The last section concludes.

2 Conceptual Framework

In this section we present a very simple theoretical model that allows us to illustrate the channels through which driving restrictions can affect criminal activity. The seminal work of

Becker (1968) provides a natural framework to model criminal behavior and to understand the potential links between driving restrictions and crime.

In our stylized model, a representative criminal devotes a fixed amount of time T to criminal activity each day, and allocates this time between two mutually exclusive periods $j = \{0, 1\}$. The periods correspond to peak hours ($j = 1$) and non-peak hours ($j = 0$), so that $t_1 + t_0 = T$. The total time devoted to daily criminal activities T is exogenous, but the criminal chooses the optimal intra-day allocation of his time. Alternatively, we can view the criminal as choosing to allocate his time over space, with $j = 1$ denoting an area inside the restricted zone and $j = 0$ denoting an area outside it. This simplified setup allows us to focus on the within-day (or within-region) distribution of crime and is consistent with the setup of the empirical models in subsequent sections. For the sake of brevity and clarity, the presentation below assumes an intra-day allocation of time. We note, however, that the model will also serve as a guide to explaining the optimal time allocated to criminal activity in the areas just inside and outside the restricted zone.

The production of crime, measured by the number of (successful) crimes in a period, depends on the time allocated to that period and on the number of potential victims ν_j. Formally, we let $N(t_j, \nu_j)$ be the number of crimes produced during period j and assume that this function is concave and increasing in each argument ($N_1 > 0, N_2 > 0, N_{11} < 0, N_{22} < 0$). Furthermore, we assume that a larger number of victims increases the marginal product of time devoted to crime ($N_{12} > 0$). To keep our exposition simple, we let $N(t_j, \nu_j) \equiv n(t_j)\nu_j$, with $n' > 0$ and $n'' < 0$.

Crime costs are a function of the expected value of punishment, and depend on the probability of detection, arrest and conviction, as well as on penalties (fines and imprisonment). Without loss of generality, we normalize the penalty to one, and we define θ_j as the joint probability of detection, arrest and conviction. Let g denote the average gain from a crime and assume that $g > \theta_j$.

A risk-neutral criminal's problem is

$$\max_{t_0,t_1} E[\pi] = (g-\theta_0) * n(t_0)\nu_0 + (g-\theta_1) * n(t_1)\nu_1$$

subject to the constraint $t_0 + t_1 = T$. First-order conditions imply that

$$(g-\theta_0) * n'(t_0^*)\nu_0 = (g-\theta_1) * n'(t_1^*)\nu_1. \tag{1}$$

Given the concavity of $n'(.)$ it is easy to see that, ceteris paribus, the time allocated for criminal activities during peak hours increases with ν_1; similarly, t_1^* increases as θ_1 diminishes.[7] As t_1^* increases, so does the number of crimes during peak hours.

How do driving restrictions affect the intra-day or the intra-regional distribution of crime? Let us assume that a driving restriction during peak-times is imposed. As is the case in Quito, Mexico D.F. and many other cities, the enforcement of driving restrictions is typically the responsibility of the police. Enforcement is a resource-intensive endeavor. Police have to invest resources both to monitor compliance and to impose penalties. The burden of enforcement could displace resources and result in fewer policing resources being allocated to crime prevention, at least in the short run. As we pointed out in the introduction, the literature has documented trade-offs in the allocation of police resources (Benson et al., 1998, 1992; Benson and Rasmussen, 1991), and provided evidence of crime displacement due to changes in enforcement (Yang, 2008). In the context of our paper, the shift in enforcement allocation due to the driving restriction decreases θ_1, causing crime rates to rise. If a driving

[7] Rearranging the optimality condition we obtain that t_1^* solves:

$$h(t_1^*) \equiv \frac{n'(T-t_1^*)}{n'(t_1^*)} = \frac{(g-\theta_1) * \nu_1}{(g-\theta_0) * \nu_0}.$$

The concavity assumption ensures that $n'(.)$ is decreasing; hence, $h(.)$ is an increasing function. Assume that an interior solution exists. An increase (decrease) in ν_1 (θ_1) shifts the right hand side of the equation up, leading to a higher level of t_1^*.

restriction policy is successfully enforced, it can also increase pedestrian flows and public transportation use, raising the number of potential victims. In our simple model, ν_1 would increase, leading to a higher production of crime during peak hours (or in areas inside the restricted zone).

Thus, driving restrictions could affect crime prevention, pedestrian flows or both, increasing crime rates during restricted hours and/or in areas inside the restricted zone. To test these implications we evaluate the effects of Quito's *Pico y Placa* (PyP) program on crime.

3 *Pico y Placa* Program

The city of Quito is located in north-central Ecuador. The city is part of the much larger Metropolitan District of Quito (MDQ), which is shown in Figure 1. The city has an area of 372 square km and a population of 1.6 million, whereas the MDQ has an area of 4,218 square km and a population of 2.2 million.

PyP restricts the circulation of vehicles in a restricted zone identified by the solid line in Figure 1. The boundary was carefully chosen and coincides with highly trafficked roads that skirt the city. Particularly in the north and south west portions of the city, the boundary crosses areas with high population density and economic activity. The program regulates access to the city center during non-holiday weekday rush hours, 7:00-9:30am and 4:00-7:30pm. On weekends and holidays, there are no restrictions. While both private and government-owned light vehicles (motorcycles, cars, SUVs, and pickup trucks) face PyP restrictions, heavy vehicles, taxis, and other forms of public transportation are exempt. As is the case in other cities, vehicle use on particular days is restricted based on the last digit of each vehicle's license plate number. For example, as of December 2012, circulation of vehicles with license plates ending in 1 or 2 (3 or 4) face restrictions on Monday (Tuesday), etc.

The enforcement of PyP began on May 3, 2010, and it has been extended every six months since its implementation. Unlike other programs, the days associated with particular license numbers has not changed since PyP's inception. According to the municipality (ADMQ, 2010), PyP was designed with several aims in mind: (i) to reduce emissions of conventional mobile-source pollutants as well as emissions of greenhouse gases, (ii) to reduce traffic congestion during rush hours, and (iii) to reduce gasoline and diesel fuel consumption in order to lower government expenditures on subsidies to these fuels. Theoretically, PyP should reduce the number of vehicles in the restricted categories by up to 20% during rush hour. Quito's municipal government predicted a 2.36% reduction in the number of vehicles on the road each day, and an equal reduction in vehicular emissions (ADMQ, 2010). Carrillo et al. (2015) find evidence that PyP led to a reduction of ambient carbon monoxide concentration (CO) during peak hours of about 9%. Because levels of CO have been shown to be correlated with traffic flows, Carrillo et al. (2015) conclude that PyP may have also decreased traffic congestion.

Enforcement of PyP has been the responsibility of both national and municipal police. Police officers gather in 12 strategic locations near the main access points to the restricted zone (shown in Figure 1), and 15 additional teams locate in random points inside the city (Municipio del Distrito Metropolitano de Quito, Secretaria de Movilidad 2012). While the exact allocation of policing resources is confidential to the police, internal reports confirm that police presence has been historically strong along many of the streets that form the boundary, and that after the introduction of PyP, the police allocated its manpower to both crime prevention and enforcement of the driving restrictions.

Penalties for violating the PyP rules are stiff. For the first violation, cars are impounded for one day, and for up to five days for three or more violations. Violators must also pay fines, the amounts of which are linked to Ecuador's monthly minimum wage. The first violation requires a payment of one-third of the monthly minimum wage (USD 97 as of 2012), half the monthly wage for the second violation, and the full monthly wage for the third and subsequent violations (USD 292 as of 2012). According to the municipal government, PyP has been stringently enforced, with over 55,000 violations punished over the first 13 months (EMMOP, 2011, 2012). Police resources are also needed to impose the stiff penalties. With over 200 daily vehicle violations per working day, the manpower needed to handle this number of impoundments is likely very large.

4 Data and Methods

4.1 Crime Data

Crime data come from two sources. Our first source is the Ecuadorian National Police (Policía Nacional del Ecuador.) The National Police allowed us to collect information from their administrative records. We obtained descriptions of every crime (including property and other types of crimes) reported between January 1, 2010 and May 31, 2012 in Quito and Guayaquil, the two largest urban areas in the country. More than 140,000 crimes were reported over this period. Crime data from previous years were unavailable. Using crime data from the original source (the police) has several advantages. The data have not been subject to any type of manipulation, nor have they been aggregated in any way. They include all information available to police including the date and time of each reported crime.[8] Finally, police administrative records could be used to find certain characteristics of victims, such as vehicle ownership.[9] We use the individual records to calculate the total number of crimes that took place every *hour* in Quito and in Guayaquil. We will exploit time variation of

[8] The identity of the victim was not included. The description of the crime is, unfortunately, not standardized.

[9] We exploit these features of the data in the last section to assess if individuals are more likely to be victimized on a day when they face a driving restriction. Even though we obtained the last digit of the license plate number of the cars owned by crime victims, the identity of the victim remained anonymous to us.

these data to identify the causal effects of PyP on crime.

The solid and dashed lines in Figure 2 plot the average number of crimes per-hour reported to the police in the two largest urban areas in Ecuador: Quito and Guayaquil, respectively. Data have been aggregated by week to visualize the series' volatility as well as the general trends. In both cities, weekly crime rates are highly volatile. Despite this volatility, it is clear that in both areas crime occurances have increased during our sample period. For instance, in Quito (Guayaquil) the average number of crimes per hour in 2012 was 23% (25%) larger than its counterpart in 2010.

It is important to discuss the limitations of these data. As is the case elsewhere, we observe reported crime rather than true crime. If victims have a tendency to underreport crime, our data may underestimate aggregate crime levels. This should not be a problem for our empirical strategy unless underreporting patterns changed as a consequence of PyP.[10] The police crime data do not always include the specific geographic location where the offense occurred. For example, we can always determine if the offense occurred in Quito or in Guayaquil, but we cannot always identify the neighborhood where the crime took place. Hence, we are not able to accurately compute the number of crimes that occur inside (outside) Quito's driving restriction zone. Nonetheless, we can compare changes in crime rates in Quito before and after PyP, with similar changes in Guayaquil, a city that did not introduce any driving restrictions. Finally, the time when a crime occurs is likely measured with error. The exact timing of certain offenses, such as financial crimes and larceny, may be often unknown to the victim. Moreover, according to police staff, victims do not always report the precise time at which the crime took place. This is evidenced by the large number of crimes that are reported to occur exactly at the top of the hour (for example, at 9:00, 10:00, etc.). Classical measurement error in the dependent variable should not affect our

[10]It is important to note that in Ecuador crimes are not reported to police officers; crimes are reported in judiciary offices that are located throughout the city. Hence, it is unlikely that crime underreporting changed as a consequence of the driving restriction implementation.

identification strategy.

Our second source of data is Quito's Citizen Security Department, the "Observatorio Metropolitano de Seguridad Ciudadana" (OMS). OMS is part of Quito's local government, and it is in charge of monitoring criminal activity in the city. OMS periodically receives information from the National Police about most types of reported crimes in this city, including property crimes, sexual assaults, and homicides. OMS adds geographic references to each reported crime and produces monthly crime activity reports.[11] OMS crime data contain information about the type of offense, the date on which it occurred and an indicator if it included an assault. The data provided to us do not contain the exact time (hour) when the reported crime took place. We collected *property* crime OMS data for the years 2008, 2009, 2010, 2011 and 2012.

Unfortunately, OMS data are not entirely comparable across time. After March 2009, certain types of robberies (some non-violent larceny and thefts) were excluded from OMS's sample. Hence, the number of crimes recorded by OMS sharply decreased after this date. This sharp decrease, of course, does not reflect a drop in criminal activity per se, but rather reflects a change in the way OMS collected its crime statistics. In March 2010, OMS reverted back to its original criteria and, as a result, reported crime rates climbed significantly. A discussion of these issues is found in the 2010 OMS Annual Report (OMS, 2010). As a result, we cannot exploit time variation of these data to identify the effects of PyP on crime. Instead, we will take advantage of the geographic coordinates to assess if crime rates change discontinuously at the driving restriction boundary.

It is useful to compare our two data sources. Police data are used to calculate the total number of crimes that took place every *hour* in Quito and in Guayaquil. This allows us to estimate changes in the distribution of crime within the day after the introduction of

[11]Police crime reports do not have geographic identifiers. They contain long narratives that include the address where the crime occurred. OMS looks at these addresses and finds the corresponding geographic coordinates. Monthly reports are available at http://omsc.quito.gob.ec.

PyP. Police data, however, do not contain exact geographic references. The OMS dataset is basically a subset of the police administrative records that contains geographic references,[12] but it does not contain the exact hour when the offense took place and is not comparable before and after PyP. Figure 3 plots the evolution of total crimes aggregate by month for the two datasets. As expected, the total number of crimes in the police records is somewhat higher than in OMS's, but the trends are fairly similar. Police crime data are used to assess if the distribution of crime within a day has changed after the introduction of the program. We use OMS data to assess if crime rates change discontinuously at the driving restriction boundary after the introduction of PyP.

4.2 Methods

To identify the effect of PyP on crime we use a two-pronged approach. First, we exploit time variation and employ a difference-in-difference strategy, where we assess if crimes rates during working day peak hours –the period affected by PyP– has changed after the introduction of the program relative to three potential control groups. Our second strategy exploits spatial variation of crime rates. Here we assess if, after PyP was implemented, crime rates change discontinuously at the driving restriction boundary.

4.2.1 Exploiting Time Variation

To explore the relationship between PyP and crime we use a difference-in-difference (DD) identification strategy and a series of "placebo" tests. In all empirical models below the treatment group consists of the period affected by PyP: peak hours of working days in Quito. The choice of a valid control group, however, is not obvious. The DD strategy requires that, in the absence of treatment, there should be a common trend for treatment and control groups. Put differently, the pre-treatment trends of treatment and control should be parallel. Rather

[12]Some crimes such as financial crimes, or fraud appear in the police data but not in OMS's.

than making an ad-hoc choice, we estimate effects using three alternative control groups that, under certain conditions, can be reasonable representations of the counterfactual trend.

The first control group comprises peak hours during non-working days in Quito. In this specification, a DD model measures changes in the difference between peak hour crime rates on working days and non-working days before and after PyP was implemented. Formally, we estimate several variants of the following model:

$$q_{ymdh} = \alpha * After_{ymd} * Work_{ymd} + \rho Work_{ymd} + X_{ymdh}\beta + \gamma_{ym} + \delta_d + \mu_h + \varepsilon_{ymdh}, \quad (2)$$

where q_{ymdh} is a measure of crime (number of reported crime incidents) in each year y, month m, day d, and hour h, combination. $After$ is a post-treatment indicator that takes the value of one for all observations after May 2, 2010. The variable $Work$ takes the value of one on working days and zero otherwise. The vector X includes a series of variables that could influence the dependent variable, such as temperature, precipitation and other weather variables. The parameters γ, δ and μ are year-month, day-of-the-week and hour-of-the-day fixed effects. They capture any common factors that affect crime levels in a particular month-year, day-of-the-week, or hour-of-the day. ε is a mean zero random component (the error term). In this specification, a positive parameter α would suggest that the mean difference between working day and non-working day crime levels during peak hours has increased after the imposition of PyP.

One may worry that other factors unrelated to PyP are responsible for the increase in crime rates during working days. To alleviate this concern, we choose a second control group that comprises off-peak hours during working days in Quito. Essentially, here we assess if the difference between peak and non-peak mean crime per hour on working days has changed after the introduction of the policy. Formally, variants of the following model are estimated

$$q_{ymdh} = \alpha * After_{ymd} * Peak_h + X_{ymdh}\beta + \gamma_{ym} + \delta_d + \mu_h + \varepsilon_{ymdh}, \tag{3}$$

where the variable $Peak$ takes the value of one for peak hours and zero otherwise. The parameter α measures the treatment effect: the change in the mean difference between peak and non-peak crime levels after the imposition of PyP. This DD specification implicitly assumes that crime trends during non-peak hours are a valid counterfactual trend in the absence of the program. This is a somewhat strong assumption that may not hold if there is crime displacement between peak and non-peak hours. Thus, the parameter α should be interpreted with caution. It certainly measures changes in the distribution of crime within the day before and after PyP, but it may not be an unbiased estimate of the program's treatment effect.

Our third control group consists of working day peak hours in Guayaquil. Guayaquil is an appealing control for several reasons: a) it is comparable in size to Quito, b) it has not been subject to driving restrictions, and c) crime displacement between these two cities is unlikely.[13] When Guayaquil is chosen as a control, the DD model tests if the difference between working day peak hours crime rates in Quito and Guayaquil has increased after the introduction of PyP. Formally, we estimate several variants of the model

$$q_{cymdh} = \alpha * After_{ymd} * Quito_c + \beta Quito_c + \gamma_{ym} + \delta_d + \mu_h + \varepsilon_{cymdh}, \tag{4}$$

where the subscript c denotes city and the treatment effect is captured by the parameter α.

The general specification of equations (2),(3), and (4) deserves some discussion. To analyze the determinants of crime, DiTella and Schargrodsky (2004) use a linear model. The

[13] Travelling costs between these cities are not small: the shortest road trip is over 250 miles and takes about 7 hours.

data used by DiTella and Schargrodsky (2004) is similar in spirit to ours. In their study, total counts of individuals' car theft reports are computed in each block-month combination, and this variable is used as the dependent variable. In our study, we also aggregate crime reports by location but at a much higher frequency (hourly). Given these similarities, we choose to follow their approach and employ a linear specification.

As noted, PyP restricts vehicle use in the city of Quito and not elsewhere in Ecuador, and is in effect only on working day peak hours. We exploit these additional sources of variation to conduct several "placebo" tests. Specifically, we estimate equations (2), (3), and (4) using these"placebo" samples and compare estimates with the treatment effects. For instance, equations (2) and (3) could be estimated with the sample of Guayaquil, and equation (4) could be estimated using the sample of non-working days. Since all of these samples are not affected by PyP, finding statistically significant "placebo" effects would call into question our identification strategy.

4.2.2 Exploiting Spatial Variation

In Section 3 we highlight that: a) enforcement of PyP is limited to areas inside the restriction zone; b) the restriction boundary coincides with a network of highly trafficked roads; c) police regularly patrol these boundary roads to monitor crime activity; and d) to enforce driving restrictions, the police have 12 fixed check points at strategic road intersections near the boundary. While the actual distribution of policing resources in the city is unknown to us (and confidential to the police), it is clear that PyP enforcement efforts must have changed both the level and spatial distribution of crime monitoring activities. Moreover, economic activity (pedestrian flows) inside the restricted zone may also have been affected by the introduction of PyP. Hence, to identify the effect of driving restrictions on crime, we also analyze changes in the spatial distribution of crimes before and after PyP was introduced.

Specifically, a nonparametric estimate of the density of crimes near the boundary is estimated. Formally, we estimate the density of d: the nearest distance between the crime location and the driving restriction boundary.[14] The boundary is a natural area of interest: besides allowing a direct comparison of crime rates inside and outside the PyP zone, it is clear from our discussion above that the intensity of PyP enforcement is higher at the boundary compared to other parts of the city.

Bunching

We would first like to test if the density of d after PyP was introduced has additional mass or "excess bunching" at both sides of the boundary relative to the pre-treatment density. Crime rates could especially increase near (both sides of) the boundary zone if police patrolling activities along it decreased to provide manpower to enforce PyP restrictions (particularly to support enforcement activities at the fixed check points).[15] To compute the excess mass at the boundary, we follow the procedure developed by Kleven and Waseem (2013). We divide d into k equally spaced intervals d_k and then compute n_k, the number of crimes per unit of distance in interval k. This empirical density is compared with a "counterfactual density," which is estimated by fitting a flexible polynomial to the empirical density, excluding observations in a range $[d_L, d_H]$ around the boundary. The counterfactual distribution is obtained from the following regression

$$n_k = \sum_{i=0}^{p} \beta_i (D_k)^i + \sum_{i=n_L}^{n_U} \gamma_i 1\left[D_k = i\right] + \varepsilon_k, \tag{5}$$

where p is the order of the polynomial, D_k is the distance from the midpoint of bin d_k to the boundary and ε is the error term. The counterfactual density is estimated as $\hat{n}_k = \sum_{i=0}^{p} \hat{\beta}_i (D_k)^i$, the predicted values from (5) excluding the contribution of the dummy coefficients in the excluded range. Excess bunching at the boundary, B, is computed as

[14] The distance to the boundary of crimes outside (inside) the restricted zone are labeled with positive (negative) numbers.

[15] While we do not have official documentation that confirm this hypothesis, interviews with police officers and other anecdotic evidence support it.

the difference between the observed and counterfactual densities in the relevant range, $\hat{B} = \sum_{k=n_L}^{n_U} (d_k - \hat{d}_k)$.

A spike in the density of crime rates in the period after PyP was implemented could simply reflect a concentration of economic activity (and crime) along the busy boundary highways. For this reason, we also compute excess bunching in a pre-treatment period and estimate the treatment effect as the difference between excess bunching in the post and pre-treatment densities. As was previously discussed, the OMS data do not allow us to compare the period just before PyP was implemented with the period just after. We can, however, use data from an earlier period that is comparable to the dataset we use in the post-treatment sample. This period corresponds to the dates between March 1, 2008 and February, 28, 2009. As a final robustness test, we also compute the distance between each crime and the nearest police check point. All check points have strong police presence and are located at the driving restriction boundary. Excess bunching near check points would suggest that factors other than PyP are driving the increase in crime rates along the rest of the boundary.

Regression Discontinuity

To identify the effect of driving restrictions on crime we provide additional evidence from a spatial regression discontinuity strategy. Intuitively, crime rates just inside the driving restriction zone are compared with their counterparts just outside. While it is not likely that the displacement of police resources affects one side of the boundary exclusively, economic activity and pedestrian flows may be affected only in the restricted areas. Formally, we estimate the following model

$$n_k = \lambda T_k + g(D_k) + \varepsilon_k, \tag{6}$$

where D_k is the distance from the midpoint of bin d_k and the boundary; T_k is an indicator that equals to one if d_k is located inside the restricted zone; g is a well-defined function and

ε captures all other unobservables that explain crime rates. We focus our attention on the parameter λ, which captures the discontinuity at the driving restriction boundary.

For robustness and completeness, the function g is modeled in several ways. Besides conventional linear, quadratic and cubic specifications, we also estimate models that allow coefficients of these polynomials to vary on each side of the boundary. Moreover, models are estimated using different "windows" that restrict the sample to crimes within 1000, 800, 600, 500 and 400 meters of the boundary. The choice of k is, to a certain extent, arbitrary. For these reason, we estimate models for various values of k.

Again, to test the fundamental assumption that the discontinuity is related to PyP and not to other unobserved factors, we also analyze crime rates near the boundary in a period before PyP was implemented. These additional empirical tests should alleviate concerns about omitted factors that might result in crime rates being systematically higher or lower on one side of the boundary.

5 Results

5.1 Difference-in-Difference (DD)

In this section we estimate treatment effects using the DD strategy and our first source of crime data (the National Police). Recall that these data can be compared before and after treatment.

In all empirical models, the treatment group consists of working-day peak hours in Quito. As we mentioned in Section 4.2.1, we consider three alternative control groups: a) non-working-day peak hours in Quito, b) working-day off-peak hours in Quito, and c) working-day peak hours in Guayaquil. Before presenting our results it is worth discussing the main assumption underlying the DD strategy: the pre-treatment trends of treatment and control groups should be parallel. To verify this assumption, we plot in each panel of Figure 4 the evolution of the average number of crimes-per-hour for the treatment group and each of the control groups, as well as the difference between this two. Despite the relatively short pre-treatment period, it appears that the pre-treatment *difference* between treatment and control groups is stationary.[16]

We estimate several versions of equation (2) and present results in Table 1. In all specifications, the dependent variable is the number of crimes h during peak hours in Quito– between 7:00am and 9:59am, and between 4:00pm and 7:59pm. Our simplest specification in column (1) includes a covariate that equals 1 if the crime measurement was taken after the implementation of PyP ($After$); a variable that indicates a working day ($Work$); and the treatment variable, PyP, that equals one during any working day peak hour in the treatment period. This simple difference-in-difference pooled specification provides a useful benchmark to interpret our coefficients. First note that peak-hour crime rates on workdays is notably larger (about 1.04 crime per hour higher) than during weekends and holidays. Crimes are also more likely to occur during the post-treatment period. The estimated coefficient of the treatment variable, ($\alpha = 0.39$), suggests that the average difference between the number of weekday and weekend crimes per hour increased by 0.39 (about 9%) after imposition of PyP. The standard errors, which are clustered at the week level to account for any potential serial correlation between crime levels in consecutive hours, show that these estimates are statistically significant almost at the 1% significance level. As shown in columns (2)-(5), the estimate of α is notably robust when other covariates are added to the benchmark model. For instance, the coefficient barely changes even when year-month fixed effects, day-of-the-week fixed effects, hour-of-the-day fixed effects, day-hour interactions, and a comprehensive set of weather covariates are included.

The results from estimating equation (3) are shown in Table 2. In this specification

[16]Given the short pre-treatment period, we do not report any formal tests to validate this assumption. (Once we cluster standar errors at the week level, we are unable to detect statistically significant differences between the pre-treatment trends.)

we focus on the sample of working day hours in Quito. In the simplest specification in column (1) we include a covariate that equals 1 during the post-treatment period ($After$), a variable that indicates a peak hour ($Peak$), and the treatment variable, PyP. Notice that the average number of crimes per-hour during the non-peak period increased by 1.02 after PyP introduction; crimes are also substantially more likely to occur during peak hours. The estimated treatment effect ($\alpha = 0.23$), suggests that the average difference between the number of peak and non-peak crimes per hour increased by 0.23 (about 5%) after the program implementation. As was the case with equation (2), the estimate of the treatment effect does not change when other covariates are added to the basic model.

Finally, we estimate equation (4), which identifies treatment effects using crime rates during working-day peak hours in Guayaquil as the counterfactual trend. The dependent variable in all specifications is the total number of crimes during peak hours in Quito and Guayaquil. Results shown in Table 3 highlight positive and large effects. For instance, the difference in crime rates between Quito and Guayaquil during peak hours increased by as much as 0.4 crimes per hour after PyP was introduced. This estimate is robust across specifications and statistically significant at conventional levels.[17]

Crime data are only available from January 2010. Hence, the pre-treatment period (a little over 4 months) is much shorter than the post-treatment (25 months). Most empirical studies that analyze the effects of driving restrictions that exploit the policy discontinuity have used a symmetric window of observations. As a robustness check, we take a similar approach and estimate equations (2), (3) and (4) restricting the sample to observations between January 1, 2010, and August 31, 2010. This gives us a symmetric window of 4 months before and after policy implementation. Results are shown in Table 4. Each row-column combination in this table displays the coefficient of interest, i.e., the treatment effect

[17]We have not been able to collect hourly weather data in Guayaquil. For this reason, models do not include weather controls.

of a different model-specification, respectively. The estimated treatment effects using the restricted sample range between 0.2 and 0.8 and, overall, are consistent with our previous findings. Of course, given that the resticted sample includes only about 8 months of data, the estimated effects could be reflecting seasonal trends in crime rates. Hence, the restricted sample results should be interpreted with caution.

To assess whether the increase in crime rates during working-day peak hours is caused by factors other than PyP, we estimate equations (2), (3) and (4) using several "placebo" samples. The parameter α in equation (2) measures the change in the difference between peak hour crime rates for working and non-working days in Quito. This parameter is now estimated using the corresponding "placebo" sample of hours in Guayaquil. Results shown in the first row of Table 5 show no statistically significant effect in any of the specifications. The treatment effect estimated with equation (3) exploits differences between weekday peak and off-peak crime rates in Quito. Again, we estimate the same model using data for Guayaquil and show results of several specifications in the second row of Table 5. Results suggest that the placebo treatment effect is close to zero in all specifications (-0.034) and statistically insignificant. Finally, we estimate equation (4) using the sample of non-working days in both Quito and Guayaquil. Because PyP affected working days only, one should not expect to find any treatment effect in this placebo sample. As expected, results shown in the third row of Table 5 confirm our priors.

In sum, the combined empirical evidence supports our hypothesis: Driving restrictions can affect crime rates.

5.2 Spatial Distribution of Crime

In this section we exploit OMS property crime data and the identification strategy described in Section 4.2.1 to identify the causal effect of PyP on crime. Recall that OMS data include the geographic coordinates where a property crime occurred, allowing us to compute crime rates on each side of the boundary.

Given Quito's geography, we focus our attention on crimes that occurred within a 1 km "window" of the driving restriction boundary. As shown in Figure 1, areas far to the East and far to the West of the driving restriction are unpopulated. Moreover, in some areas, the distance between the East and West boundaries is less than 4 km. Hence a "top window" of 1 km seems appropriate (as a starting point) to measure differences in crime rates on each side of the driving restriction zone.

Figure 5 illustrates this approach: we would like to compute crime rates to the left and to the right of the driving restriction boundary. The right panel displays the location of crimes in the post-treatment calendar year. The left panel displays the location of crimes in a pre-treatment period.[18] From these figures alone, it is not possible to visualize any excess bunching or discontinuity in crime rates across space, and it is hard to spot any differences between the pre and post-treatment periods. The figures are useful, however, to identify the areas in the city that are more prone to crime. Note that the driving restriction boundary crosses unpopulated areas with very low or no crime occurrences, particularly in the east. Hence, most of the crimes in our sample are located near the western boundary of the city.

5.2.1 Results: Bunching

In Figure 6 we compute the density of crime rates near the boundary. Each point in this figure shows the average number of crimes per meter at that location, and the solid line displays the counterfactual density estimated using the methods described in section 4.2.2. The top panel computes the density for the pre-treatment period. It shows that the average number of crimes per meter declines with distance-from-the-city-center. This is expected, since population density is generally lower the further an area is from the central business

[18] Due to the way in which the OMS data were collected, we cannot compare the post-treatment period with the calendar year just before PyP was implemented. The closest calendar year to PyP implementation that is comparable to the post-treatment data is the period between March 1, 2008 and February 28, 2009.

district. Importantly, it is clear that crime rates are not substantially higher and do not change discontinuously at the PyP boundary during the *pre-treatment* period. The bottom panel in Figure 6 displays crime frequencies during the post-treatment period and shows a rather different pattern: a clear spike appears near the boundary. We measure excess bunching near the boundary by comparing the observed crime rates and the counterfactual density in areas located within 100 meters from it. Our estimate of excess bunching in the post-treatment period $\hat{B}_{Post} = 1.62$ crimes per meter. The difference between post- and pre-treatment excess bunching is slightly lower: $\hat{B}_{Post} - \hat{B}_{Pre} = 1.39$ crimes per meter.[19] This is a very large increase of almost 60 percent relative to the baseline crime rate predicted by the counterfactual (2.3 crimes per meter) at this location.

The fact that crime rates increase so much near the boundary is not entirely surprising. The boundary coincides with the main avenues of the city. It is plausible that police officers that used to routinely patrol these streets are now focusing their efforts to monitor traffic near the few entry zones at the boundary. As a robustness test, we estimate the density of crimes as a function of the distance to the nearest police check point. As we previously discussed, these check points have strong police presence and are located at the boundary. Unless there are other unobserved factors unrelated to PyP that systematically increased crime rates all along the boundary, one would not expect to see excess bunching of crimes near these check points. Results shown in Figure 7 confirm that in both pre and post-treatment periods, the differences between the observed crime rates and the counterfactual are negligible.[20]

[19] These estimates are statistically significant at all conventional levels. Standard errors are computed using the bootstrapping procedure suggested by Kleven and Waseem (2013).

[20] Our estimate of excess bunching in the post-treatment period $\hat{B}_{Post} = -0.14$ crimes per meter and $\hat{B}_{Post} - \hat{B}_{Pre} = 0.17$ crimes per meter. None of these numbers are statisically significant.

5.2.2 Results: Regression Discontinuity

In Figure 8 we compute again the average crime frequency as a function of the distance to the boundary, but also include a flexible non-parametric estimate of the density at each side of the boundary. The bottom panel of this figure shows a large discontinuous "jump" just inside the boundary. To compute the size of the jump, we estimate several versions of equation (6) and present results in Table 6. The top and bottom panels display results for the pre- and post-treatment periods, respectively. In both panels, each row and column combination presents the estimate of λ, the treatment effect. Each column displays results from a different "window" of observations, while each row corresponds to a different specification for the function g. In all models, we set the number of bins, k, equal to the total window width. Hence, the dependent variable is always the number of crimes in each one-meter-bin within our "window."

Results confirm the patterns observed in Figure 8. Before PyP, there is little evidence of a discontinuous jump at the boundary. In the post-treatment period, however, crime rates notably increase just inside the restriction zone. Both Bayesian Information Criteria (BIC) and Akaike Information Criteria (AIC) have been computed and favor model (6) over the others. Based on the estimates of model (6), PyP has increased the total number of crimes just inside the boundary by as much as 5.5 per meter, over a 100% increase compared to prevalent crime rates at the other side.

5.3 Discussion

The combined empirical evidence suggests that the introduction of PyP led to an increase in crime rates in Quito. However, the magnitude of the effect depends on the identification strategy we use. When spatial variation is exploited, results suggest that PyP led to a large increase of crime rates near the boundary (60 to 100 percent). Caution must be exercised when interpreting these results. While our findings show that crime along the boundary drastically increased, we cannot assess if PyP raised the overall *level* of crime in the city. It might well be that criminal activity was displaced from one part of the city to another. The spatial models identify *local* effects at the boundary but cannot identify *aggregate* effects.

The difference-in-difference models, on the other hand, might assess the effect of PyP on aggregate crime levels. And results point to much smaller (yet still large) treatment effects. DD results suggest that the implementation of PyP increased crime levels (during peak working day hours) by as much as 10%. Of course, results depend on the chosen control group (counterfactual trend), and every potential control group can be subject to criticism. For example, our second control group in the DD specification assumes that crime trends during non-peak hours are a valid counterfactual trend in the absence of the program. Because crimes could be displaced from off-peak to peak hours, however, this fundamental assumption may fail. To alleviate such concerns, we compute results using three alternative counterfactual trends. It is reassuring that results from all specifications are generally consistent and point to a clear conclusion: the introduction of PyP increased crime rates during peak hours in Quito.

5.4 Channels

As discussed in Section 2, driving restrictions have the potential to increase crime rates during restricted times through two channels: Crime rates go up due to higher pedestrian flows (ν_1 increases) and/or due to a shift in police enforcement allocation resulting in lower crime detection rates (θ_1 decreases). How much have pedestrian flows and crime enforcement changed after PyP? The answer to this question will help us understand the channels through which driving restrictions influence criminal activity.

As noted in Section 3, there has been a substantial commitment of police resources to PyP enforcement. Besides gathering in 12 strategic locations along the boundary, 15 additional teams locate at random points inside the city. Moreover, driving restrictions enforcement has been vigorous. As previously discussed, there have been tens of thousands of *reported* violations during the first year of the program; the number of detected violations might be much larger.[21] It seems clear that enforcement of PyP imposed a non-negligible burden on policing resources. At least in the short run, we would expect that policing resources were shifted from other crime detection activities, decreasing θ_1.

Ideally, to analyze the relationship between PyP and ν_1 we would like to assess the evolution of pedestrian flows in Quito since 2008. Unfortunately, these data are unavailable. As an alternative, we evaluate the trend in the use of Quito's main public transportation system, the Trolebus. The Trolebus is a trolleybus system that has over 15 miles of dedicated street lanes and is limited to North-South travel within the city. It serves more than 300 thousand passengers per day. Figure 9 plots the evolution of public transportation customers since 2008. No obvious shift in the level or slope appears after May 2010, when PyP was introduced.

PyP may induce drivers to use alternative forms of transportation, such as walking or public transportation, during the days when they are subject to the driving restriction, and make them more vulnerable to criminal activities. To test this hypothesis, we exploit the administrative records of the Ecuadorian Police and Tax Authority to obtain the license plate number of the vehicles owned by crime victims in Quito. Less than 10 percent of crime victims own a car (registered under their name in the police records). We focus on about 5,000 reported crimes whose victims owned fewer than three cars. Using a simple multinomial logit we test if an individual is more likely to be a victim of crime on a day when the use of one of the individual's vehicles is restricted. Formally, let $D_i \in \{1, 2, 3...7\}$ be the day of the week when individual i was victimized, and let X_{ij} be an indicator that

[21] Anecdotic evidence suggest that paying bribes to police officers to avoid fines for traffic violations is a common practice in Ecuador, but one can only speculate about the size of this black market.

takes the value of one if individual i faced a driving restriction on day j. The multinomial assumption implies that

$$\Pr\{D_i = k\} = \frac{e^{\tau_j + \gamma_j X_{ij}}}{1 + \sum_{k=2}^{7} e^{\tau_k + \gamma_k X_{ik}}}. \tag{7}$$

The model is estimated using maximum likelihood. Interestingly, we cannot reject the joint null hypothesis that all γ_j's are equal to zero (p-value is 0.65); hence, there is no evidence that individuals are more likely to be victimized on a day when they face driving restrictions.

Summarizing, it is likely that policing resources devoted to crime detection and crime prevention decreased after the introduction of PyP. On the other hand, we find no direct evidence that the increase in crime rates is associated with higher pedestrian flows.

6 Conclusions

Driving restrictions have been a popular instrument in many cities around the world to alleviate pollution and congestion problems. While there is mixed evidence in the literature about the effectiveness of these programs to improve air quality and alleviate congestion, our study is the first to document an important side-effect: increasing crime. Programs that restrict vehicle flows may primarily affect criminal activity because program enforcement is costly and can potentially displace resources used for crime prevention and crime detection. Driving restrictions may also raise pedestrian flows, increasing the number of potential victims.

To test these hypotheses we evaluate the effect of Quito's *Pico y Placa* program on crime. Our identification strategy exploits both temporal and spatial variation in the implementation of the program. Findings provide credible evidence that driving restrictions can increase crime. For instance, results from a DD identification strategy suggests that crime rates during the time period when PyP is in effect increased between 5% and 10% after the introduction of PyP. We see no changes when the DD models are estimated with “placebo” samples. Furthermore, the post-treatment density of crime shows substantial excess mass near the boundary of the driving restriction zone, particularly in the area just inside the boundary. These patterns are not present during the pre-treatment period. In our view, the combined empirical evidence presented in this paper supports our main hypothesis: Driving restrictions can affect crime rates.

The use of driving restrictions has been an attractive non-market policy to deal with congestion problems. Our study warns about non-negligible side-effects produced by these programs given high (opportunity) costs of enforcement. Given a fixed stock of resources to jointly monitor traffic regulations and criminal activity, policy makers in cities with driving restrictions may have to choose between congestion *or* crime, until other (perhaps market oriented) mechanisms are implemented to solve these urban problems.

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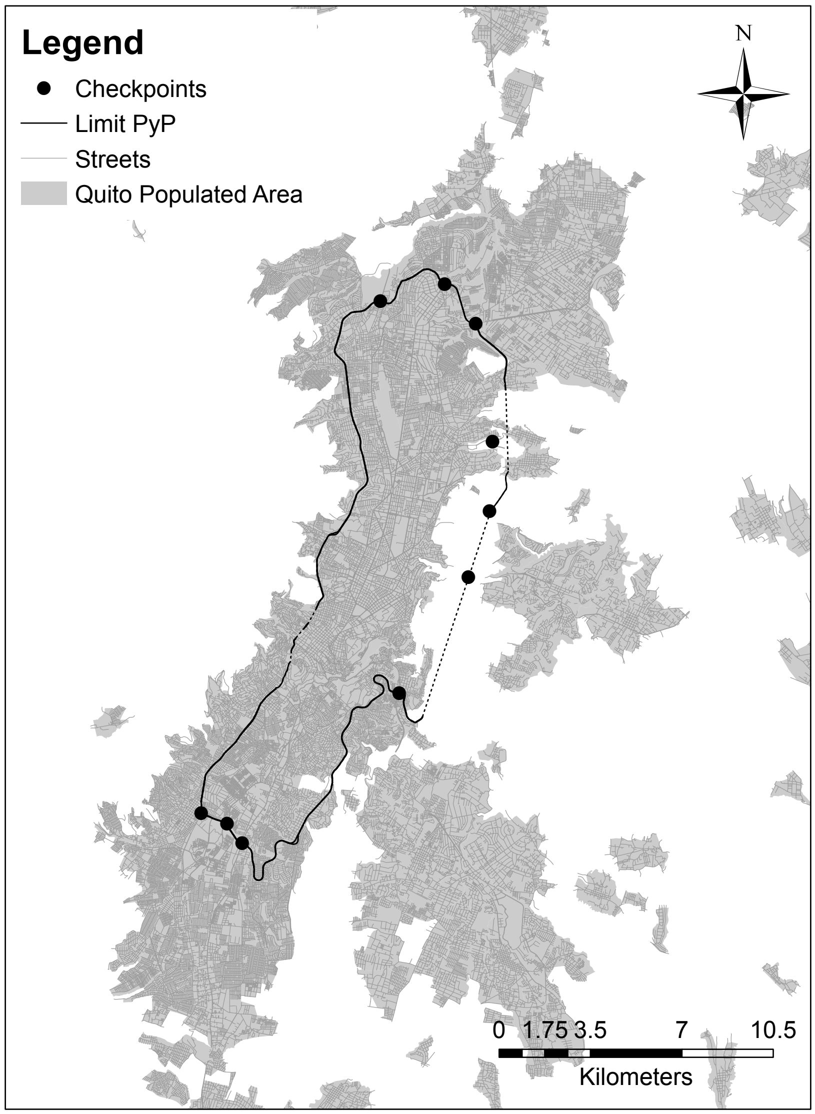
Figure 1:
Driving Restriction Boundary



Notes: This Figure shows the *Pico y Placa* driving restriction boundary in Quito, Ecuador. Dashed lines denote the boundary in areas with no population.

Figure 2:
Police Crime Data: Quito vs. Guayaquil

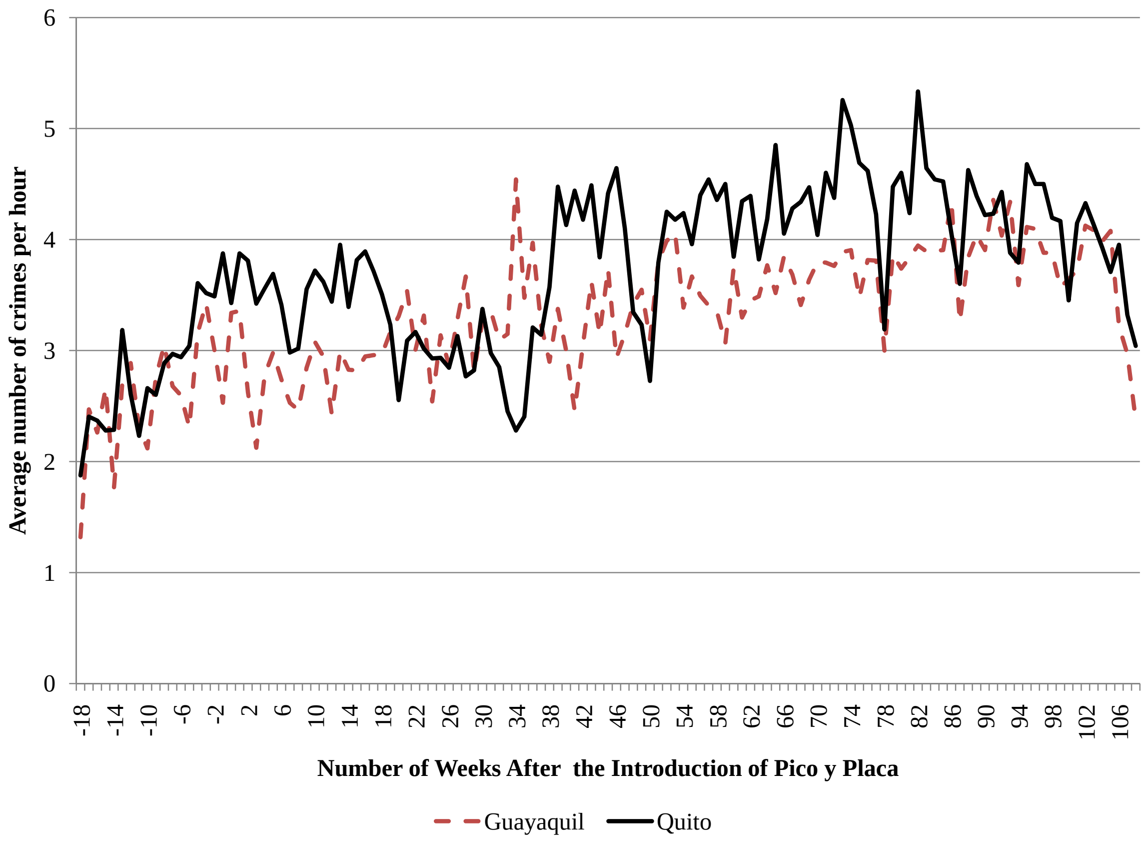
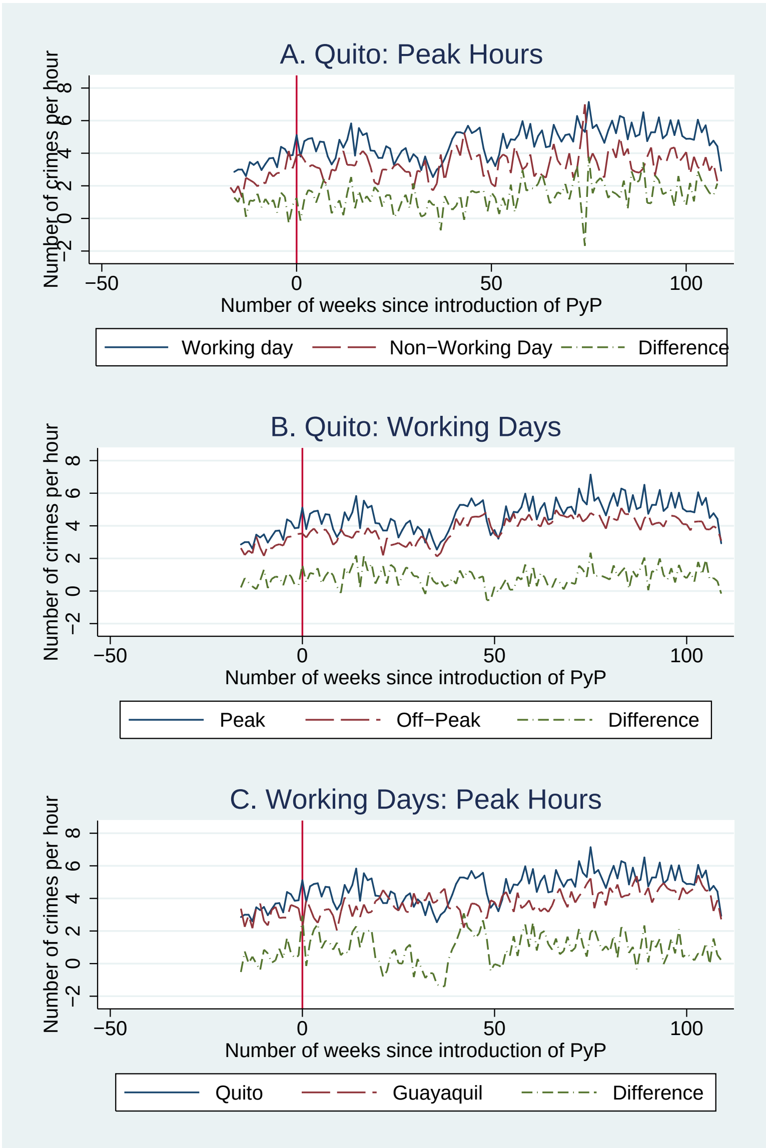


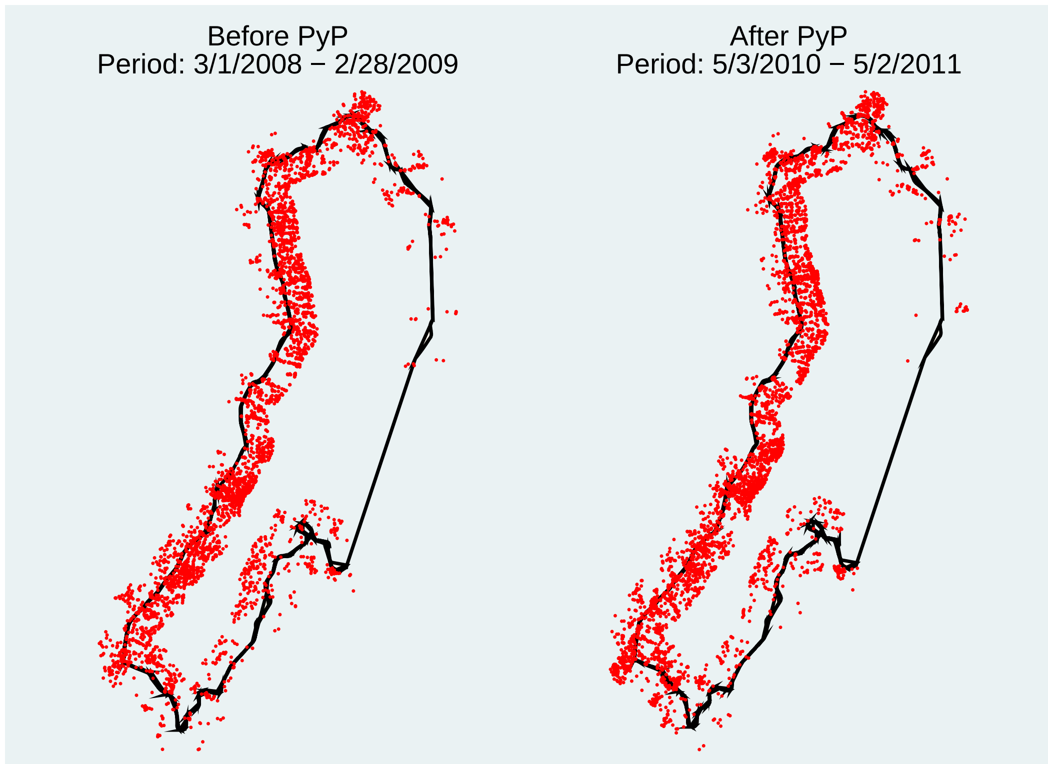
Figure 3:
OMS vs. Police Crime Data (Quito)

Figure 4:
Average Number of Crimes Per Hour



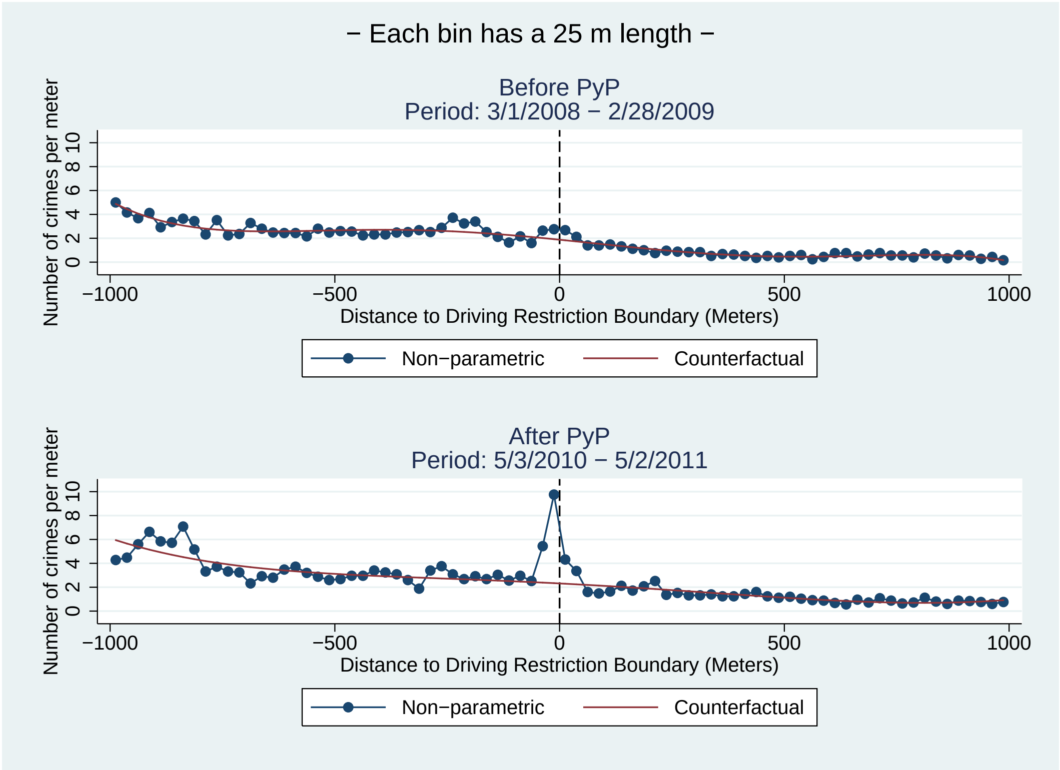
Notes: Figure shows the average number of crimes per hour for different samples in Quito and Guayaquil. Crime rate during (working day) peak hours in Quito, the treatment group, is compared with crime rates for three potential counterfactual control groups: a) non-working day peak hours in Quito, b) working day off-peak hours in Quito, and b) working day peak hours in Guayaquil.

Figure 5:
Location of Property Crimes Near PyP Boundary



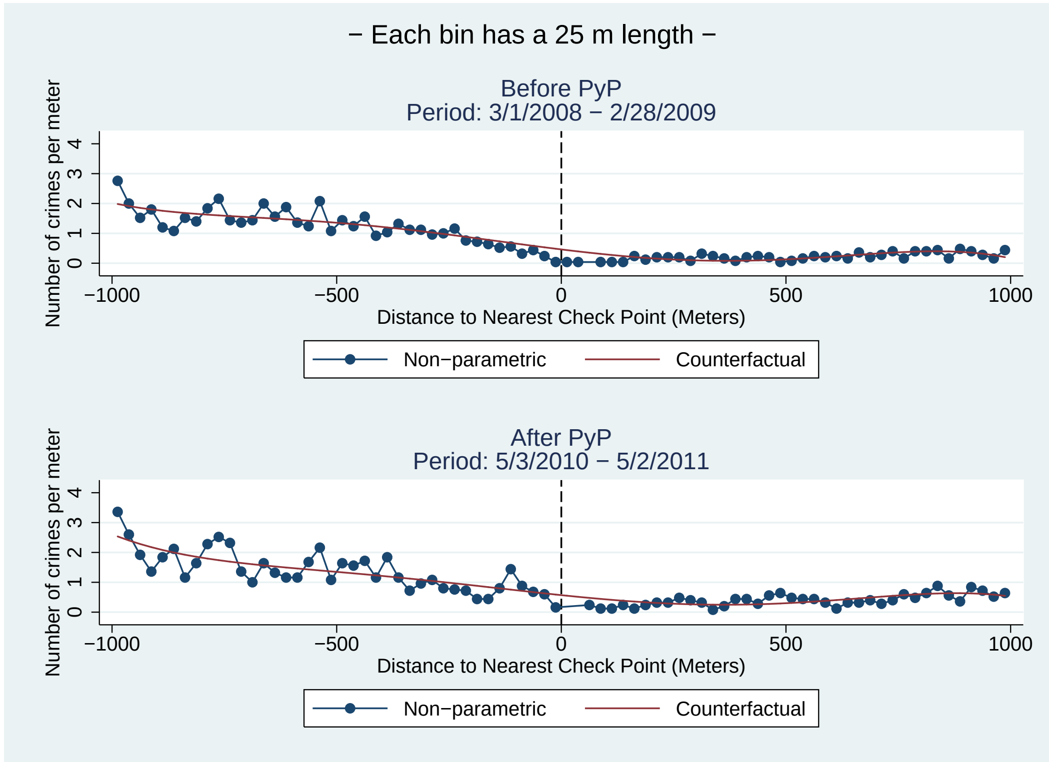
Notes: Each point shows the location of a property crime within 1 km of the driving restriction boundary (solid line). Sample includes all property crimes (excluding vehicle thefts)reported to the police. The pre-treatment period is between 3/1/2008 and 2/28/2009. The year after PyP corresponds to the period between 5/3/2010 and 5/2/2011.

Figure 6:
Average Crime Frequency as a Function of Distance To Boundary: Property Crimes
Excess Bunching at the Boundary



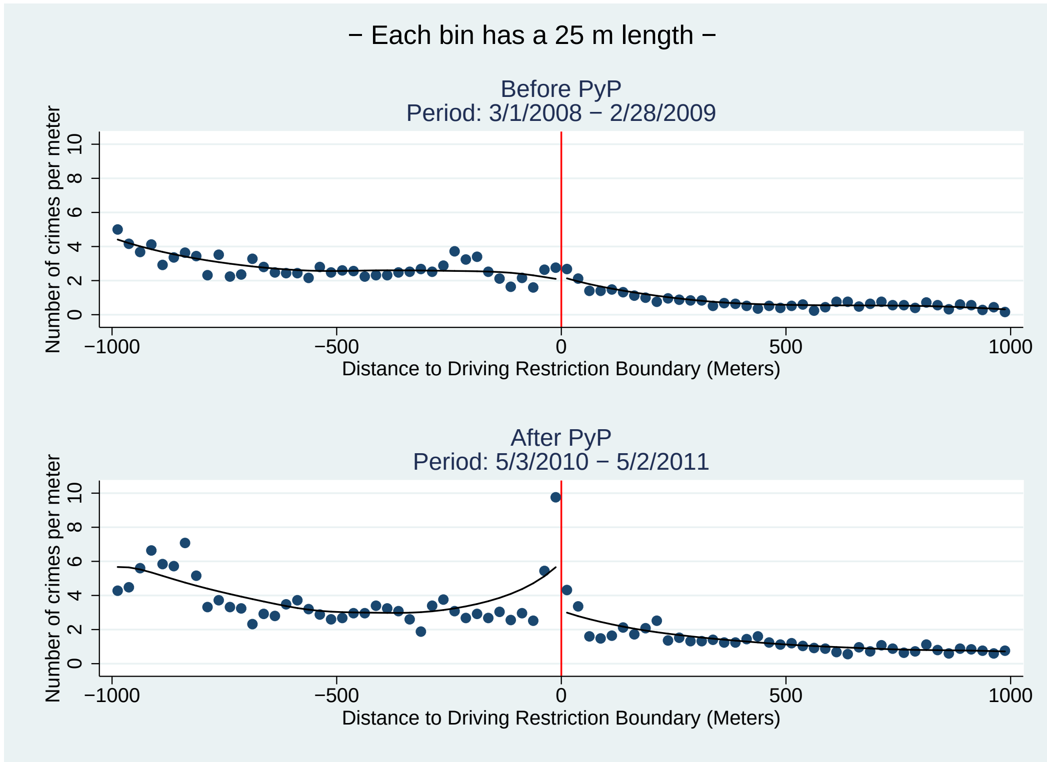
Notes: Each point shows the number of crimes per meter within the bin. Negative (positive) values denote areas inside (outside) the restricted zone. The vertical line shows the driving restriction boundary. Sample includes all property crimes (excluding vehicle thefts) reported to the police. Solid line is a parametric estimate of the crime density excluding crimes within 100 meters of the boundary (see text for details). The pre-treatment period is between 3/1/2008 and 2/28/2009. The year after PyP corresponds to the period between 5/3/2010 and 5/2/2011.

Figure 7:
Average Crime Frequency as a Function of Distance To Nearest Police Check Point: Property Crimes
Excess Bunching at the Boundary



Notes: Each point shows the number of crimes per meter within the bin. Each bin denotes the distance to the nearest PyP police check point (which are located at the boundary). Negative (positive) values denote areas inside (outside) the restricted zone. The vertical line shows the driving restriction boundary. Sample includes all property crimes (excluding vehicle thefts) reported to the police. Solid line is a parametric estimate of the crime density excluding crimes within 100 meters of the boundary (see text for details). The pre-treatment period is between 3/1/2008 and 2/28/2009. The year after PyP corresponds to the period between 5/3/2010 and 5/2/2011.

Figure 8:
Average Crime Frequency as a Function of Distance To Boundary: Property Crimes
Discontinuous Jump at the Boundary After PyP



Notes: Each point shows the number of crimes within the bin. Negative (positive) values denote areas inside (outside) the restricted zone. The vertical line shows the driving restriction boundary. Sample includes all property crimes (excluding vehicle thefts) reported to the police. Solid lines are non parametric estimates of relationship. The pre-treatment period is between 3/1/2008 and 2/28/2009. The year after PyP corresponds to the period between 5/3/2010 and 5/2/2011.

Figure 9:
Total Number of Passengers in Quito, Ecuador's Trolleybus System by Month (Millions)

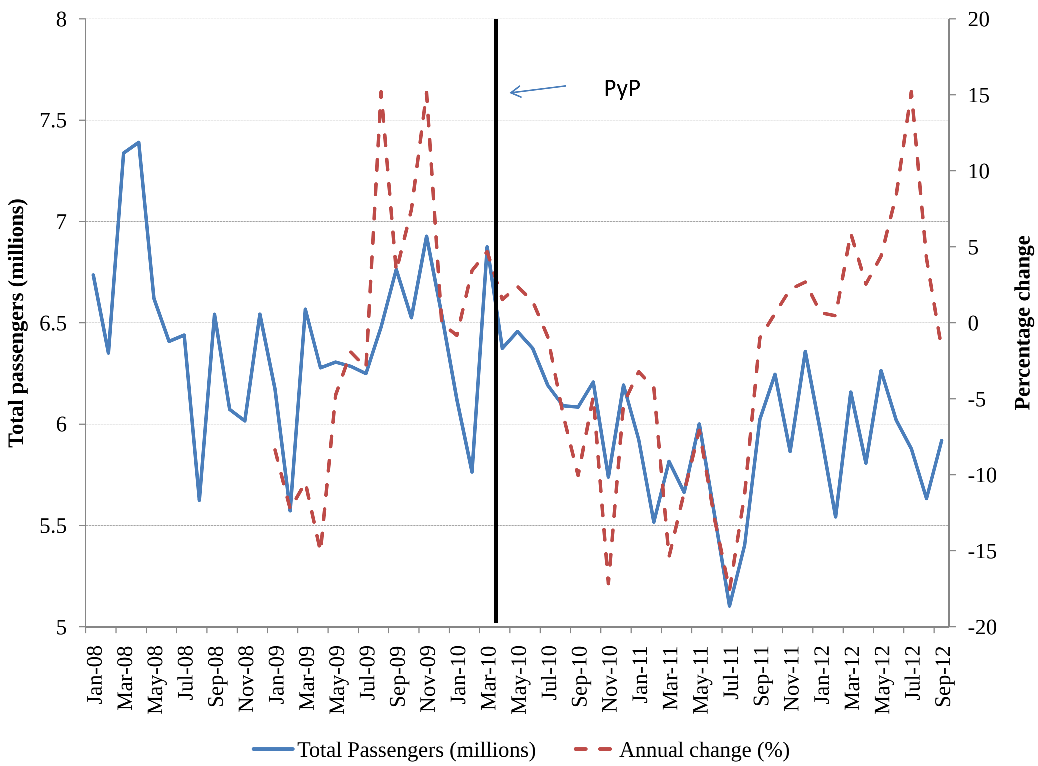


Table 1: Effect of Driving Restrictions (PyP) on Crime Using Non-Working Days as Control

	(1)	(2)	(3)	(4)	(5)
PyP: Workday * After	0.385 **	0.381 **	0.387 **	0.387 **	0.405 **
	(0.154)	(0.164)	(0.159)	(0.160)	(0.163)
After: 1 (Current Date > Policy Implementation)	0.868 ***				
	(0.178)				
Workday: 1 (Working Day)	1.044 ***	1.055 ***	1.543 ***	1.543 ***	1.591 ***
	(0.128)	(0.139)	(0.221)	(0.222)	(0.227)
Month-Year Interactions (29)	NO	YES	YES	YES	YES
Day-of-the-week fixed effects (6)	NO	NO	YES	YES	YES
Hour-of-the-day fixed effects (23)	NO	NO	YES	YES	YES
Day-Hour Interactions	NO	NO	NO	YES	YES
Weather variables	NO	NO	NO	NO	YES
Number of Observations	6,174	6,174	6,174	6,174	6,141
R2	0.079	0.134	0.283	0.304	0.307

This table analyzes the relationship between hourly crimes and driving restrictions. The dependent variable is the number of crimes reported to the police for each *peak* hour in Quito between January 1, 2010 and May 31, 2012. A new driving policy was implemented on 5/3/2010. This policy imposed driving restrictions during non-holiday weekday peak hours (working days). In our study, peak hours are those between 7am and 9:59am and between 4pm and 7:59pm. Weather variables include temperature, rain, wind speed, solar radiation, relative humidity, their square, cubic and quartic terms. Sample is restricted to peak hours. Sample size is smaller in last column due to missing observations (weather variables). Standard errors (in parenthesis) have been clustered at the week level. *, **, ***, denote significance at the 10, 5, and 1 percent level, respectively.

Table 2: Effect of Driving Restrictions (PyP) on Crime Using Off-Peak Hours as Control

	(1)	(2)	(3)	(4)	(5)
PyP: Peak * After	0.233 **	0.233 **	0.233 **	0.230 **	0.213 *
	(0.110)	(0.110)	(0.111)	(0.112)	(0.118)
After: 1 (Current Date > Policy Implementation)	1.021 ***				
	(0.126)				
Peak: 1 (Peak hour)	0.640 ***	0.640 ***			
	(0.095)	(0.095)			
Month-Year Interactions (29)	NO	YES	YES	YES	YES
Day-of-the-week fixed effects (4)	NO	NO	YES	YES	YES
Hour-of-the-day fixed effects (23)	NO	NO	YES	YES	YES
Day-Hour Interactions	NO	NO	NO	YES	YES
Weather variables	NO	NO	NO	NO	YES
Number of Observations	14,568	14,568	14,568	14,568	14,453
R2	0.026	0.061	0.508	0.519	0.521

This table analyzes the relationship between hourly crimes and driving restrictions. The dependent variable is the number of crimes reported to the police for each *non-holiday* hour in Quito between January 1, 2010 and May 31, 2012. A new driving policy was implemented on 5/3/2010. This policy imposed driving restrictions during non-holiday weekday peak hours (working days). In our study, peak hours are those between 7am and 9:59am and between 4pm and 7:59pm. Weather variables include temperature, rain, wind speed, solar radiation, relative humidity, their square, cubic and quartic terms. Sample is restricted to working-day hours. Sample size is smaller in last column due to missing observations (weather variables). Standard errors (in parenthesis) have been clustered at the week level. *, **, ***, denote significance at the 10, 5, and 1 percent level, respectively.

Table 3: Effect of Driving Restrictions (PyP) on Crime Using Working Day Peak Hours in Guayaquil as Control

	(1)	(2)	(3)	(4)
PyP: Quito * After	0.402 *	0.402 **	0.402 **	0.402 **
	(0.227)	(0.181)	(0.181)	(0.181)
After: 1 (Current Date > Policy Implementation)	0.852 ***			
	(0.144)			
Quito: 1 (Quito)	0.524 ***	0.524 ***	0.524 ***	0.524 ***
	(0.200)	(0.161)	(0.162)	(0.162)
Month-Year Interactions (29)	NO	YES	YES	YES
Day-of-the-week fixed effects (4)	NO	NO	YES	YES
Hour-of-the-day fixed effects (23)	NO	NO	YES	YES
Day-Hour Interactions	NO	NO	NO	YES
Number of Observations	8,498	8,498	8,498	8,498
R2	0.049	0.088	0.211	0.221

This table analyzes the relationship between hourly crimes and driving restrictions. The dependent variable is the number of crimes reported to the police for each peak hour in Quito and Guayaquil between January 1, 2010 and May 31, 2012. A new driving policy was implemented on 5/3/2010. This policy imposed driving restrictions during non-holiday weekday peak hours (working days) in Quito. In our study, peak hours are those between 7am and 9:59am and between 4pm and 7:59pm. Sample is restricted to peak hours in both cities. Standard errors (in parenthesis) have been clustered at the city-week level. *, **, ***, denote significance at the 10, 5, and 1 percent level, respectively.

Table 4: Estimating Treatment Effects Using Symmetric Samples

Model	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Model (1) : Using non-working day peak hours in Quito as control	0.205	0.205	0.235	0.235	0.203
	(0.215)	(0.224)	(0.219)	(0.222)	(0.212)
Model (2) : Using working day off-peak hours in Quito as control	0.406 **	0.406 **	0.406 **	0.402 **	0.400 **
	(0.176)	(0.176)	(0.177)	(0.180)	(0.187)
Model (3) : Using peak hours in Guayaquil as control	0.810 ***	0.810 ***	0.810 ***	0.810 ***	
	(0.283)	(0.246)	(0.246)	(0.247)	
Month-Year Interactions	NO	YES	YES	YES	YES
Day-of-the-week fixed effects (6)	NO	NO	YES	YES	YES
Hour-of-the-day fixed effects (23)	NO	NO	YES	YES	YES
Day-Hour Interactions	NO	NO	NO	NO	YES
Weather variables	NO	NO	NO	NO	YES

Each column-row combination in this table displays the coefficient of interest, i.e., the treatment effect of a different model specification. A new driving policy was implemented on 5/3/2010. The sample is restricted to 4 months before and after PyP implementation. The dependent variable in all models is the number of crimes reported to the police by hour-of-the-day. Standard errors (in parenthesis) have been clustered at the week-city level. *, **, ***, denote significance at the 10, 5, and 1 percent level, respectively.

Table 5: Effect of Driving Restrictions (PyP) on Crime Using "Placebo" Samples

Model / Sample	**(1)**	**(2)**	**(3)**	**(4)**
Model (1) using Guayaquil sample	0.178	0.162	0.177	0.177
	(0.235)	(0.212)	(0.209)	(0.210)
Model (2) using Guayaquil sample	-0.034	-0.034	-0.034	-0.034
	(0.099)	(0.099)	(0.099)	(0.099)
Model (3) using Non-Working day sample	0.195	0.125	0.133	0.133
	(0.300)	(0.199)	(0.192)	(0.193)
Month-Year Interactions (29)	NO	YES	YES	YES
Day-of-the-week fixed effects (6)	NO	NO	YES	YES
Hour-of-the-day fixed effects (23)	NO	NO	YES	YES
Day-Hour Interactions	NO	NO	NO	YES
Weather variables	NO	NO	NO	NO

Each column-row combination in this table displays the coefficient of interest, i.e., the treatment effect of a different model specification. A new driving policy in Quito was implemented on 5/3/2010. The dependent variable in all models is the total number of crimes reported to the police by hour-of-the-day. Models reported in Tables (1), (2) and (3) are estimated using samples not directly affected by PyP. Standard errors (in parenthesis) have been clustered at the week-city level. *, **, ***, denote significance at the 10, 5, and 1 percent level, respectively.

Table 6: Effect of PyP on Property Crime: RD Estimates

Dependent variable is the number of crimes

A] Pre-Treatment Period

	Maximum Distance to Driving Restriction Boundary (m)				
Model	1000	800	600	500	400
1) Linear	0.845 ***	1.042 ***	0.709 ***	0.413 *	0.049
	(0.156)	(0.178)	(0.209)	(0.231)	(0.260)
2) Quadratic	0.845 ***	1.042 ***	0.709 ***	0.413 *	0.049
	(0.156)	(0.175)	(0.205)	(0.227)	(0.257)
3) Cubic	0.930 ***	0.318	-0.252	-0.523	-0.929 **
	(0.221)	(0.243)	(0.284)	(0.320)	(0.372)
4) Linear x Treatment	0.845 ***	1.042 ***	0.709 ***	0.413 *	0.049
	(0.155)	(0.174)	(0.204)	(0.226)	(0.256)
5) Quadratic x Treatment	0.719 ***	-0.036	-0.618 *	-0.866 **	-1.200 ***
	(0.245)	(0.274)	(0.325)	(0.369)	(0.428)
6) Cubic x Treatment	-0.924 ***	-1.068 ***	-1.259 ***	-1.394 ***	-1.138 *
	(0.339)	(0.392)	(0.475)	(0.532)	(0.609)
Model with lowest AIC	6	6	6	6	6
Model with lowest BIC	6	6	5	3	3
Number of Observations	2,000	1,600	1,200	1,000	800

B] Post-Treatment Period

	Maximum Distance to Driving Restriction Boundary (m)				
Model	1000	900	750	500	400
Linear	0.794 ***	1.457 ***	1.676 ***	1.927 ***	2.144 ***
	(0.231)	(0.272)	(0.340)	(0.391)	(0.467)
Quadratic	0.794 ***	1.457 ***	1.676 ***	1.927 ***	2.144 ***
	(0.232)	(0.263)	(0.327)	(0.374)	(0.442)
Cubic	1.877 ***	2.038 ***	2.531 ***	2.721 ***	3.336 ***
	(0.360)	(0.418)	(0.519)	(0.594)	(0.695)
Linear x Treatment	0.794 ***	1.457 ***	1.676 ***	1.927 ***	2.144 ***
	(0.229)	(0.256)	(0.316)	(0.359)	(0.420)
Quadratic x Treatment	2.309 ***	2.363 ***	2.943 ***	3.198 ***	3.906 ***
	(0.380)	(0.448)	(0.537)	(0.601)	(0.676)
Cubic x Treatment	3.168 ***	3.470 ***	4.123 ***	4.947 ***	5.513 ***
	(0.555)	(0.637)	(0.737)	(0.786)	(0.869)
Model with lowest AIC	6	6	6	6	6
Model with lowest BIC	6	6	6	6	6
Number of Observations	2,000	1,600	1,200	1,000	800

Note: Each cell in this table displays the treatment effect using a particular model and sample combination. Panel A measures the discontinuity in the density of crime rates at the PyP boundary before the driving restriction was introduced. The second panel measures the discontinuity after the program started. Robust standard errors are shown in parenthesis. *, **, and *** denote significance at the 10, 5 and 1 percent level, respectively.