Motorcycle Restrictions and Crime in Colombia*

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Abstract

In many low and middle-income countries, motorcycles are the most commonly used means of transportation. Over and beyond their many legal uses, motorcycles are also widely used to commit crimes. While motorcycle restrictions to reduce crime have been adopted in at least 14 countries, we do not know if they are effective. We evaluate the impact of six of these restrictions in Colombia: two that ban male passengers, two that ban any passengers (regardless of sex) and two that restrict motorcycles outright. To identify causal effects on crime, we estimate difference-in-differences models that exploit spatial and temporal variation in the implementation of these measures using georeferenced crime data. In four cases we find no crime reduction effects of these policies. Even when we do identify such effects, they tend to be associated with spatial displacement of crime of equal magnitude, or the results are not robust to different model specifications. Given the high costs of ensuring compliance with these measures, and the costs they impose on ordinary citizens, local authorities should seek out other alternatives to improve citizen security.

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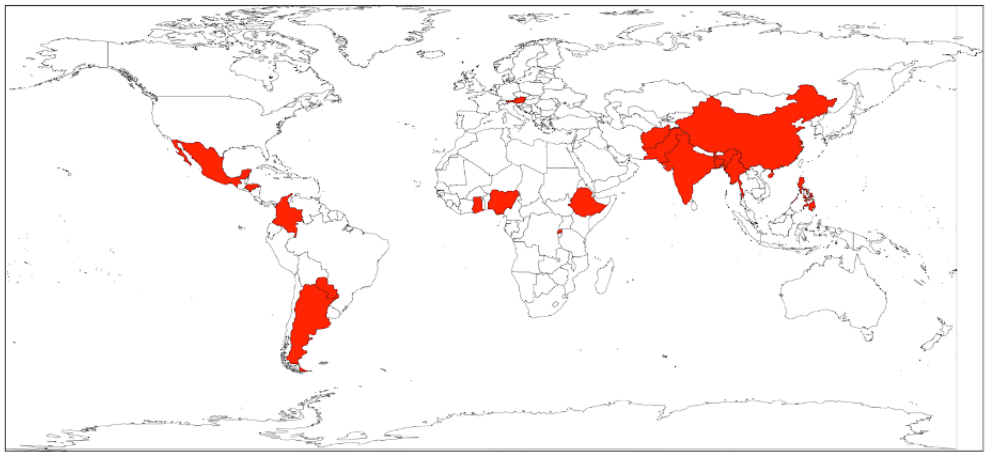
1 INTRODUCTION

In many low and middle-income countries, motorcycles are the most commonly used means of transportation, accounting for up to 85% of the total motorized vehicle fleet (Holgate et al. 2017). In Malaysia, Indonesia, Vietnam, and Thailand, for example, more than 80% of households own a motorcycle (Poushter 2015). In many developing countries motorcycles represent the fastest growing means of transport (Programme 2022): between 1993 and 2014, the number of motorcycles in these countries increased six-fold (PAHO and WHO 2018). Latin America has seen similar levels of growth in the motorcycle fleet, having increased by 81% between 2008 and 2012 (PAHO and WHO 2013).

The rapid increase in motorcycles can be explained in part by their affordability: in almost all large cities in Latin America, it is cheaper to travel by motorcycle than bus or car (Estupiñan et al. 2018). Other reasons include ease of access to credit for motorcycle purchases, the attractiveness of finding parking motorcycle spaces in dense urban environments, the relative simplicity of motorcycle maintenance, and motorcycles' ability to weave seamlessly through Latin America's notoriously impenetrable traffic (PAHO and WHO 2018). Motorcycles are used to carry out a wide variety of activities. In addition to being used for leisure, sports activities, and personal transportation, motorcycles are commonly used as taxis or cargo transport as well as courier services in developing countries.

Over and beyond their legal uses, motorcycles are also widely employed to carry out criminal activities. Since the early 1990s, motorcycles in Chinese cities such as Guangzhou to commit so-called "snatch thefts" (Xu 2012). In Nigeria, drivers of motorcycle taxis (*okadas*) have been accused of using their motorcycles to kidnap, rob, rape, and murder (Ukwayi, Agba and Michael 2013). In Afghanistan, most crimes committed in Kabul, including targeted killings, have reportedly been carried out by those on motorcycles (AFP 2020). In Mexico, a significant portion of homicides associated with organized crime in the state of Morelos have been committed using motorcycles (Monroy 2019). Even in more prosperous societies with lower levels of crime, such as the United States and Canada, gangs enmeshed in criminal activities – so-called "outlaw motorcycle gangs" (Matthews 2015) – are united by motorcycles.

Figure 1: Countries with motorcycle bans



To respond to these threats to public order, 14 countries throughout Asia, Africa, the Middle East, and the Americas since the 1990s have implemented some type of motorcycle ban (see Figure 1). These measures vary in terms of their specific characteristics: some prohibit male passengers; others prohibit motorcycles from traveling in specific zones or at particular times of the day; and finally others involve an outright and complete restriction on motorcycles. These measures are motivated by the classical economic model of crime, which argues that raising the cost of committing a crime should ultimately decrease the probability of it occurring in the first place (Becker 1968).

Despite the popularity of motorcycle bans and the high costs that they generate for motorcycle users, there exists no evidence regarding these policies' effectiveness in reducing crime. We evaluate the potential crime-reduction effects of seven separate motorcycle bans, implemented over a number of years in seven cities in Colombia, a country that has struggled to overcome a legacy of violence and crime. Three of these prohibitions banned the use of motorcycles with a male passenger: one did so during specific times (Medellín), while the other two did so in specific areas throughout the day (Bogotá and Barranquilla). Two other measures prohibited motorcycle passengers altogether, regardless of their sex: one was enforced all day in specific areas of the city (Cartagena), whereas the other was limited to one area of the city, and only during a certain time (Neiva). Two other measures outright prohibited motorcycles: one was limited to a particular area throughout the whole day (Barranquilla), while the other was applied throughout the city but only during a specific time (Soledad). Given the variety of measures evaluated here, we are in a strong position to understand whether motorcycle restrictions in Colombia reduce crime, and whether the particular characteristics of the restrictions played a role (or not) in achieving that goal.

We use a differences-in-differences identification strategy, leveraging temporal and/or spatial variation as to where and when motorcycle restrictions have been implemented. To ensure comparability across the different evaluations, we use a standard unit of analysis: a geographical

grid cell of 50 square meters. Our treatment group is made up of all grid cells within which a motorcycle prohibition was in place in a given month. We divide remaining grid cells into two groups: the spillover group, which are grid cells located between 0 and 450 meters from the treatment group, which we use to determine whether the measures caused spatial displacement of crime or diffusion of benefits; and the control group, which are grid cells located between 450 and 800 meters from treatment grid cells. For the policies that restricted motorcycle activity during particular moments of the day, we are also interested in evaluating potential temporal displacement of crime to moments when the policy was not being enforced.

We use three principal dependent variables for our analysis: first, the number of crimes committed in a grid cell in each month; second, the number of property crimes committed in a grid cell in each month; and, finally, the number of violent crimes committed in a grid cell in each month. Our data comes from the National Police of Colombia and, in the case of Medellín, from the Medellín Mayor's Office.

Overall, we find virtually no evidence that banning motorcycles – or restricting who rides them – reduces crime. Only two of the seven policies we evaluated had negative (crime reducing) effects. However, even in these two cases the estimated effects do not persist over time, or are not robust to different model specifications. More specifically, two of the three bans on the circulation of motorcycles with male passengers had no effect on crime. While the remaining policy did reduce total crime by 40% and property crime by 50%, these effects were offset by an increase of XX% in crimes occurring in areas adjacent to those in which the measure was implemented. Two of the three policies that banned the circulation of motorcycles with passengers (of either sex) had no impact whatsoever on crime. The third appears to have reduced total crime and property crime by 40%, although these results are highly sensitive to model specification and only hold for a narrow temporal window after implementation. Taken together, our results from these seven impact evaluations suggest that prohibiting motorcycle mobility had no effect on crime.

The paper contributes to the literature on the effectiveness of public policies to reduce crime. Existing studies have focused on prohibitions related to curfews (Carr and Doleac 2018), restricting the consumption and sale of alcohol (Carpenter 2007; Marcus and Siedler 2015; Sánchez et al. 2011) and firearms (Makarios and Pratt 2012; Villaveces et al. 2000; Marvell 2001). To our knowledge, only one study has evaluated the potential crime-reducing effects of restrictions on mobility: Carrillo, Lopez-Luzuriaga and Malik (2018) find that restrictions on automobiles in Quito, Peru in 2010 led to a 60%-100% increase in crime near the border of the area where the restriction was enforced, and increased by 10% the prevalence of crimes committed during peak hours. We also contribute to debates about how best to focus public policies related to citizen security, which have helpfully been categorized according to whether they are oriented towards high risk places, high risk individuals, or high risk behaviors.

Our results are particularly relevant for policy, and are quite straightforward: governments should stop using motorcycle bans as a crime prevention measure. These findings are in line with literature on crime deterrence that suggest the importance of targeting prevention programs to those most at risk of committing crimes, rather than implementing policies broadly: failing to appropriately target those with a meaningful chance of becoming criminals has significant negative externalities that should be avoided (Abt, Blattman and Magaloni 2018). At the same time, motorcycle prohibitions negatively affect millions: those who use motorcycles are forced to switch to other means of transportation that may induce longer travel times, incurring higher economic costs, as well. Those relying on motorcycles to earn a wage may likewise internalize increased costs, decreasing their ability to compete, or passing on those additional costs to consumers they serve.

The remainder of this paper is organized as follows. The following section describes the different forms of motorcycle restriction policies in Colombia that we evaluate. Section 3 presents our identification strategy, while Section 4 presents the results of the empirical exercise. The final section concludes with suggestions for future research and recommendations for public policy.

2 MOTORCYCLE RESTRICTIONS IN COLOMBIA

The costs associated with theft - including the value of stolen goods and public and private spending to prevent these crimes - are high throughout Latin America. Rates of theft are 40% higher in the region than the rest of the world,[1] while assault rates are 30% higher (Soares and Naritomi 2010). In Colombia, even given high levels of underreporting, robberies from individuals, homes, and businesses represent the most common crimes. This is a primarily urban phenomenon: 73% of property crimes reported in Colombia between 2010 and 2013 took place in the 32 largest cities. Within this time period, motorcycles were used by perpetrators to commit 16% of these crimes. Motorcycles also played an important role in crimes against persons. Almost 21% of homicides using a firearm were committed from a motorcycle. In 35.9% of these cases, a passenger on the motorcycle was the person wielding the gun. Moreover, in almost 94% of these cases, the culprits were male.[2]

The increase in use of motorcycles to commit crimes has prompted authorities around the world to implement restrictions on motorcycles. In Colombia, for example, as of July 2020 approximately 25 of Colombia's major cities had put in place some form of motorcycle restriction.[3]

[1] These statistics do not include countries in sub-Saharan Africa.
[2] Statistics from the National Police of Colombia.
[3] These municipalities include: Arauca, Armenia, Barranquilla, Bogotá, Bucaramanga, Cali, Cartagena, Cúcuta, Florencia, Ibagué, Inírida, Leticia, Manizales, Medellín, Mitú, Mocoa, Monteria, Neiva, Pasto, Pereira, Popayan, Puerto Carreño, Quibdó y Riohacha.

These measures vary in their characteristics but, as noted in the introduction, include three core types: prohibition of male passengers; prohibition of all passengers; and wholesale restriction of motorcycles (typically in a particular part of the city, or at a particular time of day). In the next section, we describe each of these measures in greater detail.

2.1 Ban on male passengers

We turn first to the most common type of restriction: a ban on male passengers, which 19 Colombian cities have implemented at some point in time. We examine two cities, Barranquilla and Bogotá. In Barranquilla the measure was put in place in February of 2017 in one part of the city,[4] and remained in force as of August 2020.[5] The measure was implemented in the city's central zone, which is characterized by high levels of crime. The case of Barranquilla is particularly interesting because it provides insight into Colombia's Caribbean Coast, where the use of motorcycles to commit crime is especially high when compared to the rest of the country. For example, in the Caribbean region, in more than 70% of robberies where the perpetrator used a firearm to steal a cell phone, the culprit used a motorcycle, while nearly 69% of assaults involved a motorcycle.[6]

In Bogotá, the ban on male passengers on motorcycles was put in place on February 2, 2018,[7] and was similarly applied in only a limited part of the city. It was implemented for six months, until August 2, 2018, and was not reinstated.

2.2 Ban on passengers of either sex

Some cities have banned motorcycles from carrying passengers entirely, regardless of sex/gender. We examine this measure in two cities: the Caribbean port and tourist hub of Cartagena, and in Neiva, the capital of the southwestern department of Huila.[8] In Cartagena, this restriction went into effect September 20, 2016, in seven neighborhoods in the city's north.[9] In this case, bodies of water separate these neighborhoods from the rest of city, posing a challenge for our empirical strategy: we have relatively few directly contiguous zones to compare. As such, we use a smaller bandwidth to estimate causal effects. In Neiva, the restriction went into effect on January 29, 2016 in a specific area of the city and was only enforced between the hours of 7:00am and 7:00pm.[10] As

[4] Decree 0176, January 27, 2017.
[5] The measure was extended by Decrees 0250 in February 2017, 0455 in June 2017, and 0819 in December 2017.
[6] Throughout the rest of Colombia, the latter corresponds to about 50%.
[7] Through Decree 068, February 1, 2017.
[8] In Colombia, the department is the provincial- or state-level equivalent unit of government. Colombia has 32 departments, with the capital, Bogotá, designated as a special administrative district.
[9] This measure was put in place September 20, 2016, by Decree 1424 of 2016.
[10] According to Decree 0094 of 2016.

we discuss below, our empirical strategy for Neiva leverages this temporal variation, in addition to the spatial variation between treatment and control areas.

2.3 TOTAL BAN ON MOTORCYCLES

A total ban motorcycles is the most restrictive of the policies that we analyze, and has been implemented in multiple Colombian cities. We evaluate this measure in Barranquilla, as well as the coastal city of Soledad.[11] In Barranquilla, this measure was in place for about 5 years in the northern area of the city from 2006 to 2011.[12] In Soledad, the measure was in place for the entire city in March 2013, but was only applied at night, between the hours of 11:00pm and 5:00am.[13]

In the Appendix, we show in map form the extent of the different restrictions across the four cities. The next section presents our data and empirical strategy.

3 DATA AND EMPIRICAL STRATEGY

We examine the effects of motorcycle restrictions on property and personal crimes. Property crimes include theft from individuals, car theft, and theft from homes and businesses; these crimes may employ violence or not. Crimes against bodily integrity include homicide, assault, and personal injury. In the case of Neiva, information about personal injuries is not available, as only homicide statistics are included in reporting on crimes against physical integrity. Our data source is the System on Statistics, Crime, Contraventions, and Operational Information (Sistema de Información Estadístico, Delincuencial, Contravencional y Operativo de la Policía Nacional – SIEDCO), maintained by the National Police of Colombia.[14] The data are georeferenced and, in addition to providing the precise coordinates of the crime (latitude and longitud), the police register the date and time, the weapon used (if any) and some basic information about the victim.

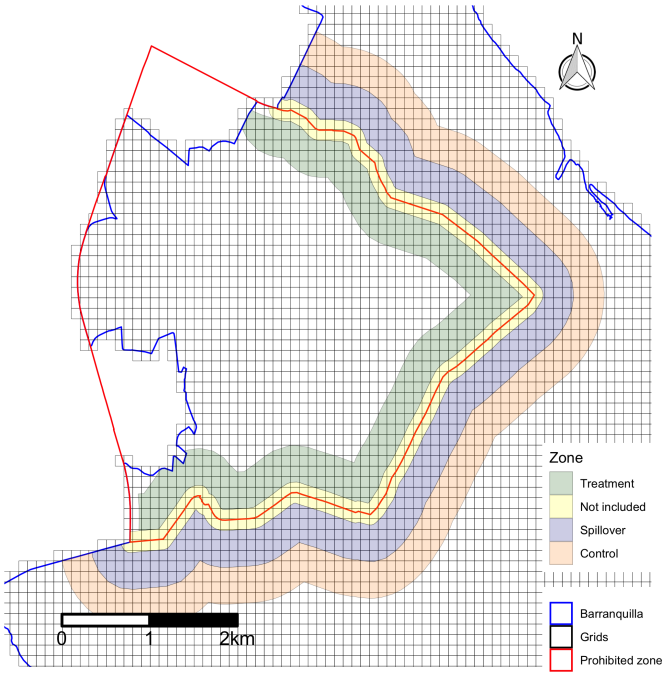
In order to standardize this information, we create a grid composed of cells of 50 meters by 50 meters and calculate the number of crimes that occur in each cell, by month, disaggregated by type of crime. This measure is more precise than other alternatives that use city blocks or street sections as the unit of analysis as it permits greater comparability among the cells of the grid and generates fewer classification problems when a crime takes place on a corner. The polygons representing the areas affected by the different motorcycle restrictions are created using the respective mayoral decrees in those cities. Using these polygons, we calculate the distance between the centroids of each cell and the limit of the areas in which the restrictions were in place. The treatment

[11] Soledad, one of Colombia's largest cities, lies within the metropolitan area of Barranquilla but is separate from it.
[12] According to Decree 0091 of the same date.
[13] According to Decree 0126 of the same date.
[14] In Colombia the National Police are housed within the Ministry of Defense and are a national force.

cells are those within that zone. To ensure that cells are as comparable as possible, we restrict treatment cells to those that at least 450 meters from the border of the restriction zone. Beyond this border we classify cells into two groups. Cells that are 0 and 450 meters from the border are considered "spillover," which allows us to identify any spatial displacement of crime generated by the restriction, or diffusion of benefits. We classify cells between 450 and 800 meters from the edge of the treatment zone as control units. To guarantee that our sample is balanced, we exclude treatment cells that do not have contiguous spillover zones, and vice-versa. Moreover, we exclude cells that are less than 100 meters from the edge (adopting a "donut design"), to reduce measurement problems around the boundary.

Figure 2 shows how this works. The area within the red line depicts the area in Barranquilla that restricted male passengers. Treatment cells are shaded in green, spillover cells in purple, and control cells in beige. The area that we exclude as part of our donut design is shaded in yellow, around the red line. In the appendix, Tables A1, A2, and A3 present descriptive statistics for crimes committed in each of our cities in the treatment, spillover, and control zones, for 6 months before and after the respective interventions.

Figure 2: Treatment, spillover, and control cells (Barranquilla)



Notes: Constructed by authors based on Decree 0176 of 2017, Barranquilla.

We identify causal effects for the different restrictions using difference-in-differences models, which compare treatment, spillover, and control cells, before and after the respective restriction

enters into force. We fit the following regression for our core model:

$$y_{it} = \psi_1 Treat_i \times Post_t + \psi_2 Spillover_i \times Post_t + \lambda_i + \tau_t + \varepsilon_{it}$$

where y_{it} is the number of crimes committed in cell i during month t. The variable $Treat_i$ takes a value of 1 if the cell is in the restriction zone and a value of 0 otherwise. The variable $Spillover_i$ takes a value of 1 if the cell is in the Spillover zone and 0 otherwise. The variable $Post_t$ is equal to 1 for months after the implementation of the restriction and 0 for months prior. Our principal specification uses a window of one year around the date on which the policy entered into force, with data disaggregated by month. All regressions include fixed effects by cell (λ_i) and month (τ_t), which control for observed and unobserved characteristics of each area and for shocks that might have affected the entire city. Standard errors are clustered at the cell level.

The principal assumption of the models is that treatment zones have parallel trends for the months prior to restrictions on motorcycles. In the next section we provide evidence to support this assumption.

4 Results

Our results are organized in two parts. First, we estimate the principal specification for the effect of the different measures in the respective cities. Only three of the measures that we examine demonstrate a significant, negative effect on any type of crime. We then evaluate the robustness of these results to changes in the temporal window and geographic bandwidths around the treatment area, and find that these effects are only significant for some specifications, demonstrating their fragility. We also exploit temporal variation in the two cases when restrictions were enforced during part of the day only and find that, once taking this temporal variation into account, the results are no longer significant. Taken together, these results indicate that Colombia's motorcycle restrictions did not have a consistently negative effect on crime.

4.1 Main specification

We begin by presenting differences-in-differences models in Table 1 for the six policies that restrict motorcycles, disaggregated by restriction type. Each column represents a city, and the three vertical panels refer to total crime, property crimes, and crimes against physical integrity. In most cases, these policies have no significant effect on crime. There are three exceptions, however, which are worth examining more closely.

The first is the restriction on male passengers in Barranquilla, which reduced property crimes considerably in the treatment zone. The coefficient estimated is -0.0076 crime per pixel per month

for total crime, equivalent to a reduction of 50% compared to the average observed in control group cells in the pre-treatment period. This effect, however, is almost entirely swamped by a 0.0062 increase in total crimes in the spillover zone, suggesting that the measure generated spatial displacement of crime, rather than overall suppression. The effect on total crime is also negative and statistically significant, with an estimated coefficient of -0.0079, which corresponds to a reduction of 40% compared to the average observed in the control zone in the pre-treatment period.

The second restriction that appears to show a deterrent effect on crime was the prohibition on all passengers (regardless of sex) in Neiva. Here we find a negative and significant effect on property crime and on total crime: -0.0348 per pixel per month.[15] As crime levels were higher in the treatment group before the measure's implementation, we use the average crime rate in treatment cells prior to implementation for comparison: for both, property crime and overall crime, the change is 44%.

The third measure that we examine is the ban on passengers in Cartagena. This measure had no effect on property crime, but did reduce physical integrity crimes by 0.0019 crimes per pixel per month. In this case, however, the effect is relatively small and we do not see significant changes in overall crime. In contrast to the prohibition on male passengers in Barranquilla, the passenger bans in Neiva and Cartagena do not show a change in crime in the spillover group, indicating that in these cases the policies appear not to have caused spatial displacement of crime.

[15] Note that there were no murders in Neiva in the treatment or control areas during the six month window, so our estimates for total crimes and property crimes are identical.

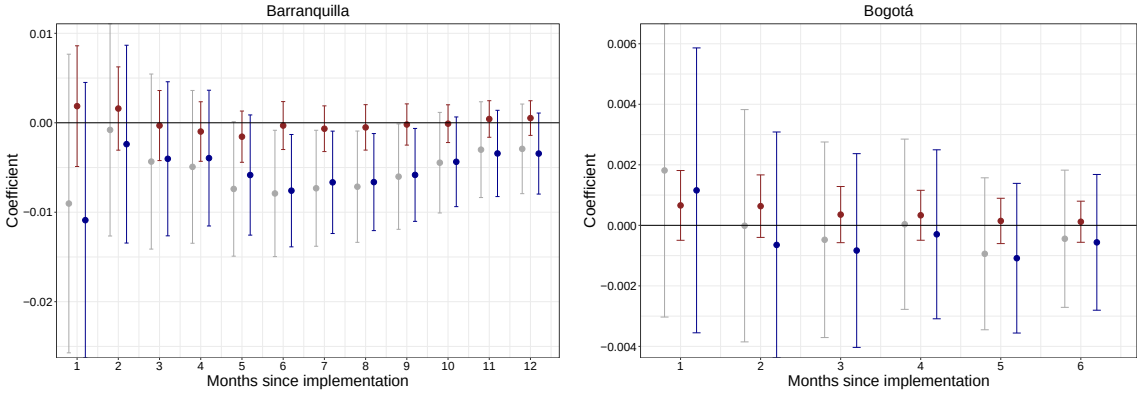
Table 1
Effect of motorcycle restrictions on crime

	Ban male passengers		**Ban passenger either sex**		**Total ban motorcycles**	
	Barranquilla	Bogotá	Cartagena	Neiva	Soledad	Barranquilla
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Total crime			
Treated*Post	-0.0079**	-0.0004	0.0017	-0.0348***	-0.0041	0.1392
	(0.0036)	(0.0012)	(0.0055)	(0.0117)	(0.0039)	(0.1298)
Spillover*Post	0.0053	-0.0005	-0.1217	0.0021	-0.0005	-0.0006
	(0.0033)	(0.0013)	(0.1183)	(0.0062)	(0.0038)	(0.0033)
			B. Property crime			
Treated*Post	-0.0076**	-0.0006	0.0036	-0.0348***	-0.0016	0.1067
	(0.0032)	(0.0011)	(0.0053)	(0.0117)	(0.0030)	(0.0993)
Spillover*Post	0.0062**	-0.0004	-0.1208	0.0019	0.0004	0.0007
	(0.0029)	(0.0012)	(0.1183)	(0.0062)	(0.0030)	(0.0030)
			A. Physical integrity crimes			
Treated*Post	-0.0003	0.0001	-0.0019*	0.0000	-0.0024*	0.0325
	(0.0014)	(0.0003)	(0.0010)	(.)	(0.0015)	(0.0305)
Spillover*Post	-0.0009	-0.0001	-0.0009	0.0002	-0.0009	-0.0013
	(0.0014)	(0.0004)	(0.0013)	(0.0002)	(0.0014)	(0.0013)
N	44,388	106,458	30,408	24,288	72,000	32,496
Month window	6	6	6	6	6	6
Fixed effects pixel	✓	✓	✓	✓	✓	✓
Fixed effects month	✓	✓	✓	✓	✓	✓

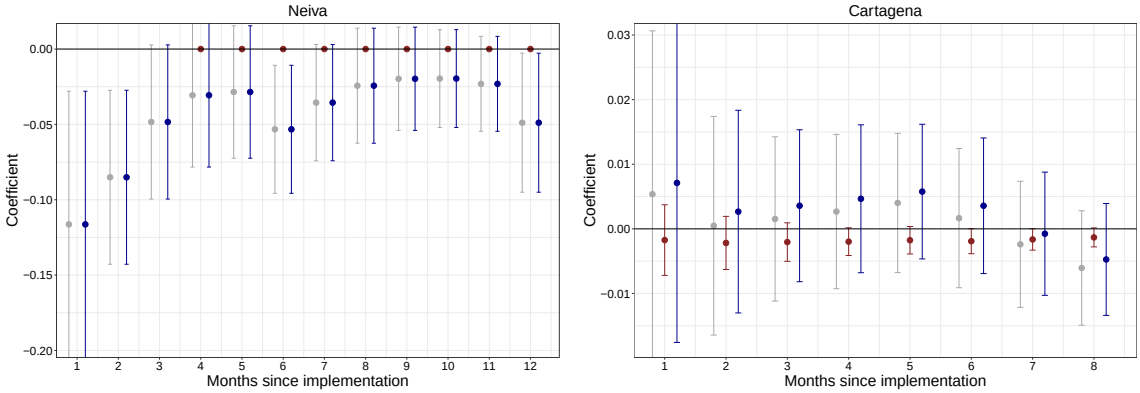
Notes: $***$ Significant at 1% , $**$ Significant at 5% , and $*$ significant at 10% . Each pixel is 50 meters by 50 meters. We exclude a bandwidth of 100 meters on each side of the border between the area in which the measure was in force and the area in which it was not. The treatment, spillover, and control bands are each 300 meters wide.

Figure 3: **Sensitivity to different temporal windows**

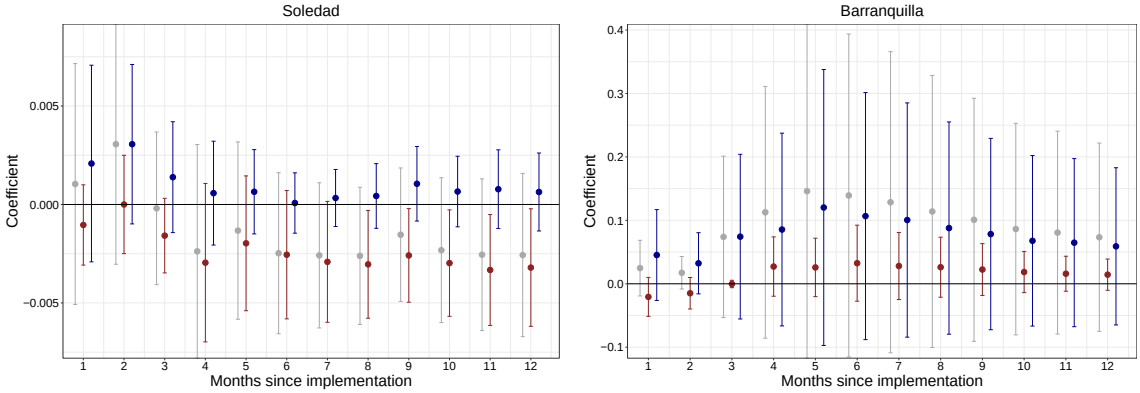
A. Male passenger



B. All passengers



C. Complete motorcycle restriction



Notes: Point estimates and 95% confidence intervals for total crimes are shown in grey. Estimations for property crimes and crimes against persons are shown in blue and red, respectively. Estimates for pixels of 50 meters by 50 meters, fixed effects by month and pixel. A band of 100 meters around the limit of the area where the policy was implemented is excluded.

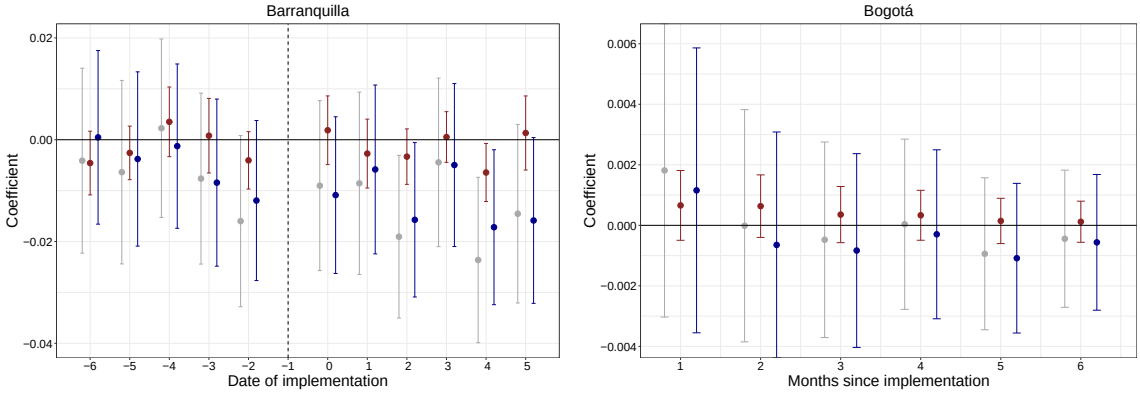
4.2 ROBUSTNESS CHECKS

We carry out a variety of robustness checks. First, we vary the temporal windows around the beginning of the restrictions, from 1 to 12 months. In Bogota and Cartagena, we restrict these windows to 6 and 8 months respectively, as there are not sufficient observations to study the full year. These results are presented in Figure 3. For the policies and cities in which we do not find significant effects within the 6 month window, we also do not find a significant effect for other temporal windows. Something similar occurs with the restriction on passengers in Cartagena, where both coefficients are similar in magnitude and significance for all windows. In contrast, for the ban on male passengers in Barranquilla, the estimated coefficients are similar in magnitude and significance for the 5 and 9 month windows, but are not significant for the rest. In Neiva, we find a similar effect: the coefficients are negative and significant between 1 and 3 months, and between 6 and 7 months. In both cases, the results of the principal specification are not robust to varying the temporal windows.

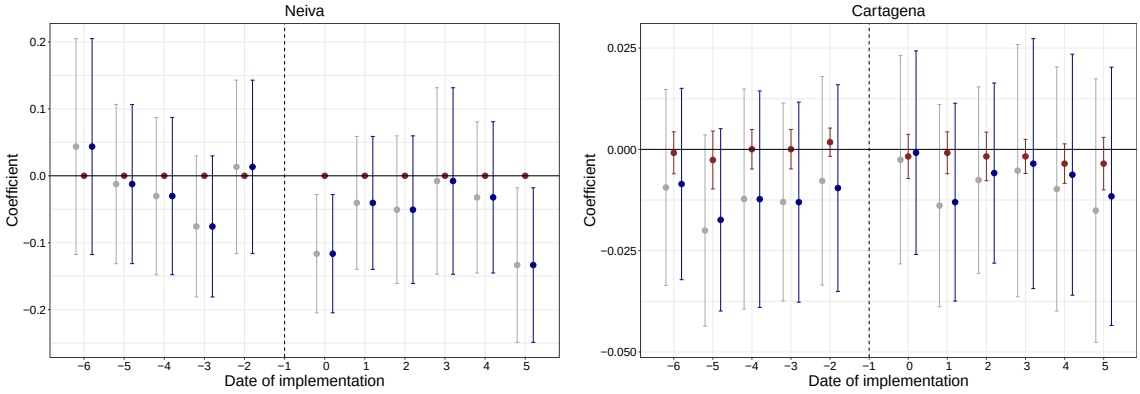
To complement this exercise and to understand better the time dynamic of different policies, we estimate event study models to demonstrate the effect of the measure from month to month. These results are shown in Figure 4. In general, we do not uncover any statistically significant relationship between assignment to treatment and crime prior to the implementation of the restrictions, providing support for the parallel trends assumption. In the case of Barranquilla, we see a particularly strong increase in crime in the spillover zone during the first and second month of the policy, while reductions in the treatment zone are distributed throughout various months. Results from the other cities are unremarkable.

Figure 4: **Event Study**

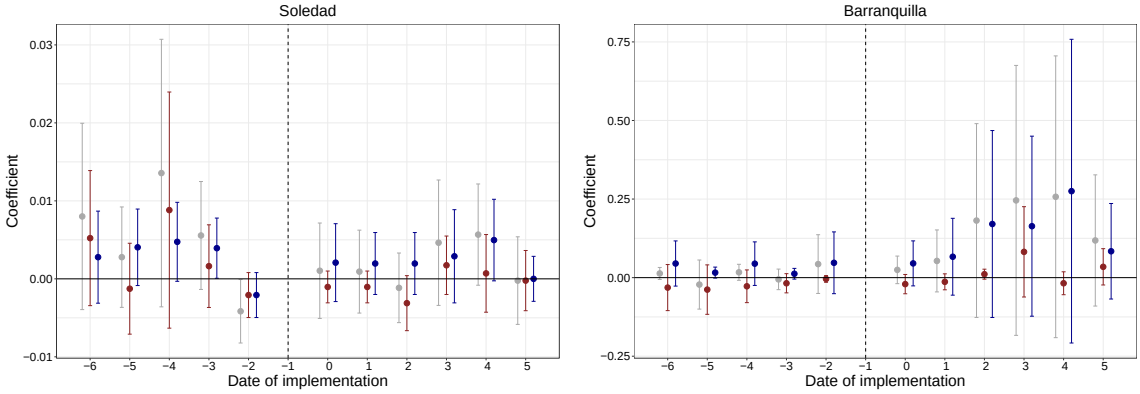
A. Male passenger



B. All passengers



C. Complete motorcycle restriction



Notes: Point estimates and 95% confidence intervals for total crimes are shown in grey. Estimations for property crimes and crimes against persons are shown in blue and red, respectively. Estimates for pixels of 50 meters by 50 meters, fixed effects by month and pixel. A band of 100 meters around the limit of the area where the policy was implemented is excluded.

In the next robustness exercise, we test the sensitivity of our results to changes in the bandwidth of the treatment area. Figure 6 shows our estimations for the principal specification, varying the bandwidths used to define the treatment zone between 100 and 800 meters.[16] The coefficients are consistently equal to zero. Similarly, the effect of the restriction on passengers in Cartagena remains robust to changes in bandwidths. The results are less robust for Barranquilla's ban on male passengers and Neiva's restriction on passengers: in the first case, the coefficients are only significant for bandwidths between 300 and 550 meters, while in the latter case they are only significant when the bandwidth is greater than 250 meters. This implies that the results of the principal specification for these policies are not particularly robust to changes in the bandwidth used.

In the final exercise we explore within-day variation for the restrictions applied during some hours of the day but not others. This includes Neiva, where the ban on passengers was only applied during daylight hours, and Soledad, where it was only applied late at night and in the wee hours (11pm to 5am). To take advantage of an additional source of variation, we measure crime both during hours when the measure was in effect and when it was not. This allows us to estimate a triple difference model, which interacts all the variables of our difference-in-differences model with a dummy variable that takes a value of 1 if the restriction was in force and 0 otherwise.[17]

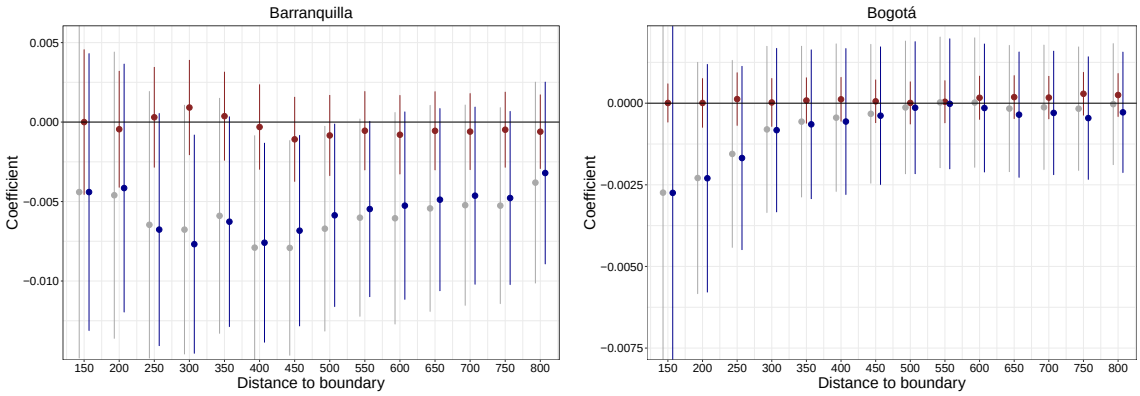
As we see in Table 2, in none of these cases are the coefficients of the term $\text{Treated}_i \times \text{Post}_t \times \text{Hour}_h$ statistically significant, implying that any change in crime was similar for hours when the restriction was applied and when it was not. Moreover, in Neiva we find a positive effect for the $\text{Spillover}_i \times \text{Post}_t \times \text{Hour}_h$ term, indicating that spatial displacement effects could have occurred during the hours in which the restriction was in effect.

[16] For Neiva, the treatment zone is relatively small, and therefore it is only possible to include 350 meters within the edges of the area where the measure was in place.

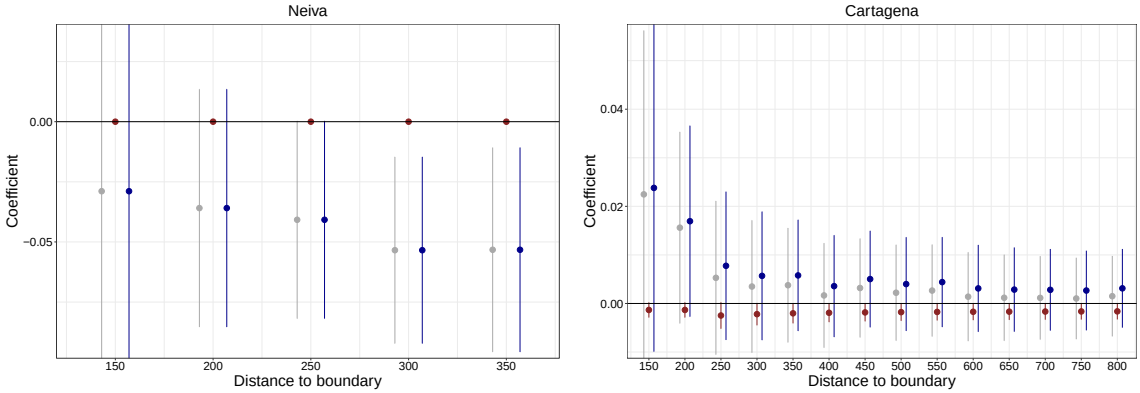
[17] In this case, estimations include fixed effects for month, cell, and time of day, and errors are clustered at the cell level.

Figure 5: **Sensitivity to Different Bandwidths**

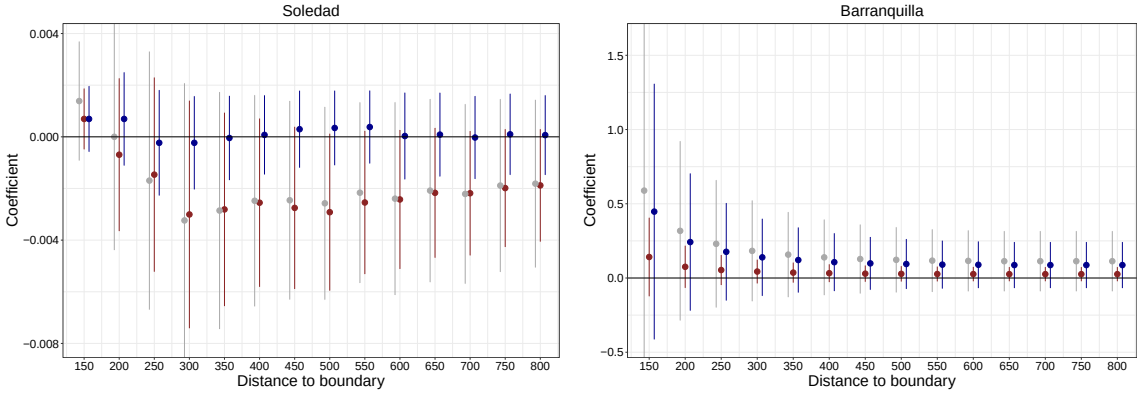
A. Male passenger



B. All passengers



C. Complete motorcycle restriction



Notes: Point estimates and 95% confidence intervals for total crimes are shown in grey. Estimations for property crimes and crimes against persons are shown in blue and red, respectively. Estimates for pixels of 50 meters by 50 meters, fixed effects by month and pixel. A band of 100 meters around the limit of the area where the policy was implemented is excluded.

Table 2
Time-based motorcycle restrictions

	Neiva			Soledad		
	Totales	Propiedad	C. la vida	Totales	Propiedad	C. la vida
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*Post	-0.0164	-0.0164	-0.0000	-0.0057	-0.0034	-0.0023
	(0.0101)	(0.0101)	(0.0000)	(0.0068)	(0.0055)	(0.0025)
Spillover*Post	-0.0093	-0.0093	-0.0000	-0.0026	-0.0002	-0.0024
	(0.0077)	(0.0077)	(0.0000)	(0.0068)	(0.0054)	(0.0025)
Treated*Post*Hour	-0.0368	-0.0368	0.0000	0.0032	0.0034	-0.0002
	(0.0243)	(0.0243)	(0.0000)	(0.0064)	(0.0051)	(0.0030)
Spillover*Post*Hour	0.0229*	0.0224*	0.0004	0.0041	0.0010	0.0031
	(0.0129)	(0.0129)	(0.0000)	(0.0061)	(0.0049)	(0.0024)
Observaciones	24,288	24,288	24,288	72,000	72,000	72,000
Meses	6	6	6	6	6	6
E.F. Pixel	✓	✓	✓	✓	✓	✓
E.F. Mes	✓	✓	✓	✓	✓	✓
Treated*Post + Hour	0.0140	0.0140	.	0.2353	0.9234	0.1249
Spillover*Post + Hour	0.1768	0.1912	.	0.2333	0.2755	0.2852

Notes: $***$ Significant at 1% , $**$ Significant at 5% , and $*$ significant at 10% . Each pixel is 50 meters by 50 meters. We exclude a bandwidth of 100 meters on each side of the border between the area in which the measure was in force and the area in which it was not. The treatment, spillover, and control bands are each 300 meters wide. The last two lines of the table show p-values for the sum of the coefficients of Treated*Post + Treated*Post*Hour, and Spillover*Post + Spillover*Post*Hour, respectively.

5 CONCLUSIONS

Faced with the growing prominence of motorcycles in criminal activities, a number of Latin American countries have implemented restrictions on motorcycles. We evaluate the effectiveness of three sets of policies implemented in Colombia: a ban on male passengers, a prohibition on all passengers regardless of sex, and a ban on motorcycles entirely at certain times of day and/or in specific parts of the city. We examine six such policies in which the administrative data is sufficiently detailed to allow us to geolocate each crime. Difference-in-differences models, which exploit spatial and temporal variation in the restrictions, provide us with causal estimates.

In general, motorcycle restrictions do not substantially reduce crime in Colombia. Of the 6 restrictions that we examine, only three have negative and significant effects. Even in these cases, the estimated effects do not persist over time or are not robust to different specifications. We also observe signs of spatial displacement in some cases where we find statistically significant crime reduction effects, suggesting that that these restrictions did not reduce aggregate crime but simply shifted crime to other areas without restrictions on motorcycles.

Even if we had found motorcycle restrictions to be effective, it would still be necessary to examine closely the costs of such policies. The potential costs associated with motorcycle restrictions include obliging citizens to seek alternate forms of transportation – which may include more lengthy commute times – as well as imposing on the police unnecessary responsibilities for monitoring compliance, time that could be better spent on other crime prevention activities. Future avenues for research could include a more precise calculation of the costs of motorcycle restrictions, including calculating the number of additional hours spent searching for commuting options, as well as extra time spent commuting to work. These results, combined with the high social costs that these measures impose on citizens – which essentially represents a regressive tax on poorer residents – when combined with the opportunity costs for police of enforcing these measures, suggest that the time is ripe to set aside motorcycle restrictions in favor of more effective policies for crime prevention.

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APPENDIX

Table A1
Descriptive statistics: male passenger ban

	Total		Property Crimes		Physical Integrity	
	Pre	Post	Pre	Post	Pre	Post
			A. Barranquilla			
Treated	0.0248	0.0152	0.0217	0.0124	0.0031	0.0028
	(0.1633)	(0.1308)	(0.1519)	(0.1174)	(0.0583)	(0.0583)
Spillover	0.0185	0.0221	0.0145	0.019	0.004	0.0031
	(0.1414)	(0.1607)	(0.1239)	(0.1504)	(0.0671)	(0.0576)
Control	0.0195	0.0178	0.0158	0.0141	0.0037	0.0037
	(0.1459)	(0.1384)	(0.1315)	(0.124)	(0.0603)	(0.0603)
			B. Bogotá			
Treated	0.0091	0.0083	0.0089	0.008	0.0002	0.0003
	(0.1054)	(0.0999)	(0.1043)	(0.0976)	(0.0151)	(0.0185)
Spillover	0.0101	0.0092	0.0092	0.0085	0.0009	0.0007
	(0.1105)	(0.1101)	(0.1053)	(0.1062)	(0.0303)	(0.0293)
Control	0.0096	0.0094	0.0087	0.0085	0.0009	0.0008
	(0.1055)	(0.106)	(0.1013)	(0.1016)	(0.0296)	(0.0305)

Notes: Standard errors in parentheses. Each pixel is 50 meters by 50 meters. We exclude a bandwidth of 100 meters on each side of the border between the area in which the measure was in force and the area in which it was not. The treatment, spillover, and control bands are each 300 meters wide.

Table A2
Descriptive statistics: all passengers

	Total		Property Crimes		Physical Integrity	
	Pre	Post	Pre	Post	Pre	Post
			A. Neiva			
Treated	0.0780	0.0487	0.078	0.0487	0	0
	(0.4033)	(0.2984)	(0.4033)	(0.2984)	(0)	(0)
Spillover	0.0262	0.0338	0.0262	0.0336	0	0.0002
	(0.223)	(0.2566)	(0.223)	(0.2562)	(0)	(0.015)
Control	0.0214	0.0269	0.0214	0.0269	0	0
	(0.2016)	(0.2194)	(0.2016)	(0.2194)	(0)	(0)
			B. Cartagena			
Treated	0.0096	0.0151	0.0090	0.0151	0.0006	0.0000
	(0.1304)	(0.1828)	(0.1236)	(0.1828)	(0.0241)	(0)
Spillover	0.3045	0.1867	0.3033	0.1851	0.0012	0.0016
	(8.315)	(5.6746)	(8.3149)	(5.6744)	(0.0447)	(0.0529)
Control	0.0157	0.0195	0.0148	0.0173	0.0009	0.0022
	(0.1731)	(0.1867)	(0.1671)	(0.1776)	(0.0455)	(0.0583)

Notes: Standard errors in parentheses. Each pixel is 50 meters by 50 meters. We exclude a bandwidth of 100 meters on each side of the border between the area in which the measure was in force and the area in which it was not. The treatment, spillover, and control bands are each 300 meters wide.

Table A3
Descriptive statistics: complete restrictions

	Total		Property		Physical Integrity	
	Pre	Post	Pre	Post	Pre	Post
			A. Soledad			
Treated	0.0127	0.0107	0.0067	0.0059	0.0059	0.0047
	(0.242)	(0.2372)	(0.1493)	(0.1435)	(0.1287)	(0.1114)
Spillover	0.0053	0.0069	0.0033	0.0045	0.0020	0.0023
	(0.1385)	(0.177)	(0.0894)	(0.125)	(0.0652)	(0.0652)
Control	0.0110	0.0131	0.0067	0.0075	0.0043	0.0055
	(0.249)	(0.2699)	(0.1717)	(0.1613)	(0.0993)	(0.1309)
			B. Barranquilla			
Treated	0.2522	0.3950	0.1632	0.2728	0.0890	0.1222
	(6.089)	(9.7799)	(3.8524)	(6.762)	(2.2763)	(3.1811)
Spillover	0.0179	0.0209	0.0155	0.0190	0.0025	0.0019
	(0.1549)	(0.1617)	(0.1337)	(0.156)	(0.0629)	(0.0434)
Control	0.0106	0.0143	0.0091	0.0120	0.0015	0.0023
	(0.1125)	(0.132)	(0.1027)	(0.1183)	(0.039)	(0.0537)

Notes: Standard errors in parentheses. Each pixel is 50 meters by 50 meters. We exclude a bandwidth of 100 meters on each side of the border between the area in which the measure was in force and the area in which it was not. The treatment, spillover, and control bands are each 300 meters wide.

Figure 6: **Maps of Motorcycle Restrictions by City**