UNIVERSIDAD EAFIT

Vigilada Mineducación

Documentos de trabajo

Economía y Finanzas

Nº 20-19

2020

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September 10, 2020

Abstract

During the 1990s, Latin America experienced a criminal procedural revolution (LACPR) when approximately 70% of its countries abandoned their inquisitorial system and adopted the U.S. adversarial model. Following the LACPR, the region experienced a dramatic increase in crime, consolidating it as one of the most violent areas in the world. Despite previous empirical evidence indicating that procedural law affects criminal behavior, the effects of the LACPR continue highly unexplored. In this paper, we use the Latin American case to evaluate the impact of an adversarial reform on crime rates. Exploiting the quasi-experimental implementation of the reform in Colombia, we use an event study approach combined with differences-in-differences to estimate the reform's effects on criminal activity. Despite the opposite incentives the reform created, we find an increase associated with the procedural transformation in overall crime rates (22%), violent crime (15%), and property crime (8%). We also observe a dramatic decrease in drug offenses associated with lower arrest rates. Our findings contribute to the literature on Latin American crime and the link between procedural law and criminal behavior.

Keywords: criminal procedural revolution, plea bargaining, certainty, severity, celerity.

JEL classification: K14, K40, K42

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We thank Greg Haugan and María Camila Gomez for excellent research assistance and Camila Uribe for her collaboration in the early stages of this document. We are grateful to Ian Ayres, Adriana Camacho, Patricio Domínguez, René Flores, Issa Kohler-Hausmann, Alexander Torgovitsky, Tom R. Tyler, Juan Fernando Vargas, and to participants from the Workshop in Law and Economics at ITAM, America Latina Crime and Policy Network Workshop 2018, Universidad EAFIT, and the Canadian Law and Economics 2019, for valuable discussions and feedback. We are also grateful to the Yale MacMillan Center and the Olin Center for Law and Economics at Yale Law School for financial support. We would like to thank the Colombian National Police Department and the Center for Economic Development at Universidad de Los Andes for providing the data used in this paper. An earlier and preliminary version of this paper circulated under the title "Certainty vs. Severity Revisited: Evidence for Colombia."

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I. INTRODUCTION

During the 1990s, Latin America experienced a criminal procedural revolution (LACPR) when approximately 70% of its countries abandoned their inquisitorial system and adopted the U.S. adversarial model (see Figure 1). The U.S. sponsored and promoted this conversion as part of its transnational war on crime initiated by President Clinton in 1995 (McLeod 2010; Veillette et al. 2007). Some scholars have identified the LACPR as the most profound legal transformation in the region in nearly two centuries (Langer 2007; McLeod 2010).

The campaign to transform Latin American procedural codes started in the 1960s through the U.S. military and economic assistantship programs, which lead to Mexico and Colombia's early reforms. However, the massive adoption of the U.S. adversarial model only occurred until the end of the Cold War when the National Bipartisan Commission recommended the Reagan administration to include a criminal justice reform in the region. A recommendation that Clinton's administration continued advancing the following years. The first countries to adopt the U.S. model were Guatemala, Argentina, Paraguay, and Venezuela. Latin American countries faced several difficulties completing the transition towards U.S. adversariality. Ultimately, only Colombia and Chile succeed in implementing the new procedural model as initially planned.

[Figure 1 Here]

Almost three decades after the LACPR, Latin America experienced a dramatic rise in crime (Bergman 2006, 2018; Brands 2011; Malone 2010; Müller 2018). Although the literature identifies Latin America as an exceptionally violent region prior to the reform, the nature of its violence changed after the implementation of the LACPR. Before the 1990s, scholars associated Latin American violence with military dictatorships and state repression; however, since the mid-1990s, researchers began to identify crime as the new form of violence (Bergman 2006, 2018; Müller

2018). Notwithstanding the existence of studies linking procedural law and criminal behavior, few investigations have explored the impact of procedural reforms on Latin American crime rates. Despite its importance, the relationship between the LACPR and the Latin American violence crisis remains highly unexplored.

Previous research on crime in Latin America has focused on exploring how the region's economic and social characteristics affect the incentives to engage in illegal activities (Di Tella, Edwards, and Schargrodsky 2010; Fajnzylber, Lederman, and Loayza 1998; Soares and Naritomi 2010). Other scholars use a governance and institutional approach to study crime in Latin America (Malone 2010; Müller 2018; Schultze-Kraft, Chinchilla, and Moriconi 2018). Yet, they ignore the effect of procedural law on criminal behavior.

In this paper, we seek to advance the literature on the causes of Latin American soaring crime rates and the causal relationship between procedural law and criminal behavior. To the best of our knowledge, this is one of the first econometrical investigations evaluating the effects of the LACPR in the region. For this purpose, we use Colombia as a case of study, since it offers unique conditions to develop a causal inference analysis. Out of the 19 Latin American countries that implemented the LACPR, we identify Colombia as the best setting to develop this evaluation. By exploiting the Colombian quasi-experimental implementation of the U.S. adversarial model, we suggest that adversariality increases crime rates (violent and property offenses) significantly, *ceteris paribus*. In particular, we find an increase associated with the procedural transformation in overall crime rates (22%), violent crime (15%) and property crime (8%).

Besides exploring the LACPR effect on crime, we classify the central reform changes using the literature on criminal procedural law and illegal activities into three mechanisms: i) celerity, ii) certainty and iii) severity. Then, we proceed to test whether those theoretical changes affected

the behavior of law enforcement and judicial agents. Finally, we compare our findings with the theoretical predictions to evaluate the applicability of previous results for the Latin American context.

We organize the paper as follows: Section II discusses previous theoretical and empirical work linking procedural law and criminal behavior. In sections III and IV, we discuss the nuances of the LACPR and explain in more detail the Colombian case. In the following two segments, we describe our data sources and define our methodological strategy. Finally, in the last two parts, we present our results followed by our conclusion.

II. PROCEDURAL LAW & CRIMINAL BEHAVIOR

The literature on crime control has largely documented the role of the penal system on crime rates. Some of the most notable work on this area has focused on the impact of sentencing (Beyleveld 1979; Walker 1979), incapacitation (Brody and Tarling 1980; Greenwood 1979), and rehabilitation (Brody 1986; Martinson, Adams, and Palmer 1976) on criminal behavior. These research projects have been extremely important to understand the role that criminal justice plays in altering the incentives to engage in illegal activities. However, these approaches usually neglect the effects criminal procedure might have on crime rates. Scholars focusing on the relationship between procedural law and crime identify three determining factors that influence criminal behavior: (i) certainty of adjudication, (ii) severity, and (iii) celerity of punishment. Some of the most important findings reveal that criminal procedure affects illegal activity by modifying the perception of the probability of punishment (Atkins and Rubin 2003; Dalla Pellegrina 2008; Dušek 2015; Soares and Sviatschi 2010), intertemporal preferences (Listokin 2007), and the effectiveness of the severity of punishment (Nagin and Pogarsky 2003; Pogarsky 2002).

Empirical findings have concluded that changes in the certainty of punishment have the strongest deterrent effect (Apel and Nagin 2011; Durlauf and Nagin 2010, 2011). Scholars associate this deterrent effect to four conditional probabilities: (i) apprehension, given a crime commission (police); (ii) being charged, given an apprehension (police & prosecution); (iii) conviction, given a criminal charge (prosecution & judge); and (iv) a formal sanction, given a sentence (judge & post-sentencing institutions). These probabilities affect the decision to engage in illegal activities differently. For instance, from an economic point of view, altering the likelihood of being apprehended affects the offender's perception of completing the event successfully, while modifying the conviction probability after detection, changes the perception of a legal sanction (Nagin, Solow, and Lum 2015).

Despite recognizing the different procedural stages that influence the likelihood of punishment, the literature associates it with the probability of apprehension almost exclusively, and the other mechanisms remain largely unexplored. According to Nagin (2013b), offenders respond more to changes in criminal opportunity than changes in the likelihood of a legal sanction. Under this view, detention prevents the crime's completion eliminating the gain, which is the initial incentive to commit it in the first place (Nagin 2013a; Wilson and McLaren 1972a).

Scholars studying the role of apprehension on illegal activities explain different responses to policing depending on the criminal opportunity. From an environmental criminological perspective, targets and locations explicate variation on crime and victimization risk (Bottoms and Wiles 1994; Brantingham and Brantingham 1981, 1982, 1984, 1993; Jeffery 1971; Newman 1972). For instance, paying wages by check or bank deposit makes individuals less vulnerable to muggings (changes in the environment) (Clarke 1983). Complementing this view, the opportunity perspective focuses on making targets less vulnerable (Clarke 1983, 1995, 1997; Felson and Clarke

1995). For example, Clarke (1983) suggests that using automobile steering column locks diminish criminal opportunity by making them less vulnerable to robbery (reduction in the opportunity of success) (Nagin 2013a; Wilson and McLaren 1972a).

In addition to the environmental and opportunity perspectives, the literature identifies routine activities as determinants of high-or-low-risk situations (Cohen and Felson 2003; Felson 1987; Felson and Clarke 1995, 1995, 2004; Groff 2008). Risky situations also depend on place-based factors (Jaitman and Anauati 2020; Weisburd 2015). For instance, criminologists have identified that houses or businesses next to those recently burglarized have a higher risk of burglary since the offenders perceived low risk of apprehension in the area (Bowers and Johnson 2005; Bowers, Johnson, and Pease 2004, 2005).

Empirical studies have also found that street crimes respond more to visible police presence than those offenses that occur in private spaces (Andens and Andenæs 1974). This evidence suggests that changes in policing activity might affect crime rates differently. For example, an increase or decrease in visible police presence might affect muggings and street robberies (Sampson and Cohen 1988), but not have a significant influence on domestic assaults (Felson, Ackerman, and Gallagher 2005). Additionally, the literature has identified policing activity to be proactive and reactive, depending on the offense. Proactive police work appears in situations where a violation without a victim occurs, and crime rates depend on arrest rates (i.e., narcotics violations) (Black 1970). In contrast, reactive police activity heavily relies on citizen complaints, particularly on victims' reports (i.e., assaults and robberies) (Black 1970; Stinchcombe 1963).

Even though the probability of apprehension plays a crucial role in the criminal justice deterrence effect, transformations in procedural law affect not only the certainty of punishment, but also the celerity and severity of legal sanctions. On the certainty side, social researchers have

linked increases in criminal convictions with lower crime rates (Pogarsky 2002; Soares and Sviatschi 2010). However, Pogarsky (2002) finds evidence that lenient penalties deteriorate the effect of the certainty of punishment. Thus, reforms that increase the productivity of the criminal justice system but decrease the severity of penalties might have unintended consequences on crime rates. For instance, as we will show in this paper, changes associated with plea bargaining can have unexpected results since it contributes to procedural efficiency in exchange for shorter prison sentences.

On the one hand, the literature identifies plea bargaining as an essential tool to increase conviction rates in short periods (Bushway, Redlich, and Norris 2014; Grossman and Katz 1983). On the other hand, empirical evidence reveals that defendants who pled guilty tend to receive substantially shorter sentences (Albonetti 1990, 1997; Bushway et al. 2014; Smith 1986; Ulmer and Bradley 2006; Ulmer, Eisenstein, and Johnson 2010). However, the literature has overlooked the relationship between plea bargaining and criminal behavior.

On celerity and severity, authors focusing on the process length claim that rational offenders discount future punishment, therefore increasing the temporal gap between the event and the penalty decreases the sanction value (Davis 1988; Lee and McCrary 2005; Listokin 2007; Polinsky and Shavell 1979). Research conducted in Italy (Dalla Pellegrina 2008), the Czech Republic (Dušek 2015; Dušek and Traxler 2016), and Costa Rica (Soares and Sviatschi 2010) shows that procedural length and crime rates are negatively correlated. However, Dusek (2015) warns that, while shorter and simpler procedures might reduce the lag between crime and punishment, these changes can alter the incentives of law enforcement agents to allocate resources in each phase of the process.

Empirical work has documented the responses of law enforcement agents to procedural law transformations in the U.S., Europe, and Latin America. In the U.S., legal scholars have explored the effects of changes in the "beyond-a-reasonable-doubt" standard of proof (Stuntz 1997)[5] and the evidence rules (Atkins and Rubin 2003; Nardulli 1983) in crime rates, concluding that police and prosecutors allocate fewer resources in activities with stricter criminal procedure regulations. Concretely, Atkins and Ruben (2003), studying the effects of the exclusionary rule in the 1960s (see Mapp v. Ohio 1961, Gideon v. Wainwright 1963, Miranda v. Arizona 1966) find that stricter criminal procedure rules that favored the defendant increased property crimes, contradicting Nardelli's (1983) results.

In the European case, Dusek (2015) studies the implementation of a fast-track proceeding for minor offenses in the Czech Republic, finding that prosecutors and police officers transferred resources from severe crimes to less serious cases. According to his study, reduction in procedural length for minor offenses decreased the relative price of pursuing them, causing the observed substitution. Finally, Hausman and Kronick (2019) explore the effects of imposing restrictions on arrests and evidence collection in Colombia and Venezuela. Their findings suggest that in Venezuela, police decided to allocate more resources to illegal forms of crime control instead of formal criminal justice mechanisms after the change. In contrast, the authors find that in Colombia, a severe decrease in arrest rates occurred after the introduction of those restrictions.[6]

[5] Stuntz (1997) suggests that, the "beyond-a-reasonable-doubt" requirement imposes a heavy burden of proof skewing errors in favor of the defendant. Although this constitutional requirement affects the probability of conviction, Stuntz argues that the underlying legitimacy of the criminal justice system relies on the high standards to obtain a criminal conviction. In addition to the allocation of the burden of proof, Stuntz explains that the criminal procedure extensively regulates the conduct of the agents that participate in the system (e.g., police officers, prosecutors, defense attorneys), intending to protect the defendants' rights to be free from certain kinds of government behavior. While the literature on criminal procedure recognizes that a decrease in the protection of the defendants' rights might increase the certainty of punishment, it is not a desirable solution considering that the criminal justice system tends to discriminate against the poorest and most vulnerable sectors of the population (Listokin 2007; Stuntz 1997).

[6] Hausman and Kronick (2019) explore the effects of the LACPR in crime rates in Venezuela and Colombia. We re-estimate the effect of the reform in arrest rates per 100,000 inhabitants confirming their findings.

Despite the theoretical and empirical evidence suggesting that changes in the certainty, celerity, and severity of the punishment cannot be separated one from the other, the literature exploring the relationship between procedural law and criminal behavior continues to analyze these factors separately. Moreover, different empirical studies often provide contradictory evidence on how procedural reforms modify the probability of conviction or detention, and how individuals respond to those changes. Most of these opposite conclusions come from studying specific transformations rather than looking at the entire criminal justice system. In this paper, we use the LACPR to evaluate the effects of an adversarial procedural model on crime rates.

Evaluating adversariality as a whole—instead of studying isolated phases—allows us to better understand the procedural law's impact on the offender's and law enforcement agent's decisions. Furthermore, our paper contributes to the literature by explaining other ways in which experiences with the penal system triggers upward revisions in probabilities of apprehension, conviction, and punishment imposition (Kohler-Hausmann 2018; Lochner 2007; Tyler 2006; Tyler and Huo 2002).

III. THE U.S. ADVERSARIAL MODEL EXPERIMENT IN LATIN AMERICA

Estimating the effect of the U.S. adversarial model in Latin American criminality is a challenging task due to the lack of good quality micro-data, the heterogeneity across countries, and the different levels of intervention in each one of them. However, the staggered implementation of the LACPR in Colombia (2005-2008), Perú (2006-2013),[7] México (2005-2016),[8] and Chile (2000-2005) offers

[7] The Peruvian case used a gradual implementation of the reform that started in 2006 and ended in 2013. The implementation took longer than expected originally due to budget constraints that led to the suspension of the implementation process in 2011. In contrast to the Colombian case that implemented the new adversarial model the next immediate year after the promulgation of the new code, it took the Peruvian government two years to start the implementation process.

[8] The Mexican reform started in 2008 with the constitutional reform and it was finally enacted in 2014. The implementation of the reform in each state depended on local government efforts and only until 2016 more than fifty percent of the states introduced the new adversarial model in their territories (Secretaría de Gobernación México 2016; Vázquez 2014)

unique potential settings to evaluate its effects. From these four potential cases of study, we consider Colombia and Chile to be the most successful cases executing their rolling-out process as they are the only countries that implemented it in their originally planned dates (CEJA 2015; de Luca 2008).

Despite their similarities, the gradual implementation in Colombia and Chile followed different temporal and geographic patterns. Chile introduced the reform in five waves, starting in December 2000 and finishing in June 2002. To facilitate the transition, Chile reserved all metropolitan regions for the final stage representing 40.1% of the population (Gendarmeria de Chile 2000). In contrast, Colombia implemented the U.S. adversarial model in four waves, starting in January 2005 and finishing in January 2008. In each stage of the reform in Colombia, the reform covered approximately 25% of the population without separating rural and urban areas in specific waves (see Figure 2).

[Figure 2 Here]

By comparing both implementation processes, we identify Colombia as a better quasi-experimental setting than Chile. In the first place, empirical evidence suggests that criminal activity and population type (urban and rural) are highly correlated (Berg and Lauritsen 2016; Bergman 2018; Jaitman and Anauati 2020; Sampson 1983, 1985; Vilalta 2020) and thus, the implementation order in Chile might not be exogenous to each stage's crime rates. On the contrary, at first sight, the homogeneous distribution of the reform in Colombia appears as an arguable exogenous process. To rule out a connection between the implementation order in Colombia and its crime rates, we reviewed the 22 versions of the procedural code drafted by the Colombian Congress and the four legislative debates in the Senate and House of Representatives and did not find a direct reference to the rolling-out order and crime.

In the second place, the implementation in Chile occurred in different months of the year introducing seasonality variation which makes it considerably harder to separately identify both the effect of the reform and the seasonality of some crime outcomes (Chamlin 1988; Corman and Mocan 2005; Draca, Machin, and Witt 2011; McDowall, Loftin, and Pate 2012; Ratcliffe 2010). Once again, the consistent implementation of the Colombian reform in January during the four-year rolling-out process helps us control for this source of variation, offering a unique quasi-experimental setting for our study.

Besides the advantages of this quasi-experimental implementation, the literature identifies Colombia as the closest country to the U.S. in the region (Langer 2007; McLeod 2010; Stokes 2003). During the 1990s, the U.S. created the Plan Colombia—a major counter-narcotics and criminal justice reform program—transforming Colombia in one of the largest recipients of U.S. military and financial aid in the world (Mejía 2012; Stokes 2003). As part of this foreign aid program, the U.S. directly influenced Colombia's justice reform and crime priorities while monitoring the progress of the country.

Following the creation of Plan Colombia, the U.S. Department of Justice (DOJ) urged Colombia to transform its penal procedural code following the U.S. adversarial model. Consequently, in 2004, Colombia enacted a new procedural code (Law 906/04) adopting this model. The change not only responded to foreign, but also to national pressures. The small number of solved cases and the high levels of due process violations affected the legitimacy of the old criminal procedural system (Law 600/00). Under the Law 600/00, prosecutors served a dual role as investigators and adjudicators in long written and private procedures. This model overloaded the Colombian system. By 2004, the system produced a low number of convictions every year (Martínez Cuéllar, Hernández Luna, and Parra González 2008; Zorro Medina 2020b) and

excessively relied on pre-trial detention as a form of crime control (Carranza 2001; Duce, Fuentes, and Riego 2009; Zorro Medina 2020a).

The national sentiment of impunity and arbitrariness motivated the Colombian elites to align their agenda with the U.S.'s desire to export its procedural model to the region, allowing for a higher participation level of the DOJ. During the reformation process, the DOJ contributed to the legislative debates and directly wrote parts of the new Colombian code (Law 906/04).[9] The Law 906/04 transformed the entire Colombian process from the pre-investigation stage to the trial, without changing the post-sentencing procedure. Figure 3 summarizes the four parts of the Colombian criminal system before and after the reform.

[Figure 3 Here]

As Figure 3 shows, the main change during the pre-investigation stage consisted of moving the statute of limitations to operate before the formulation of imputation. This change intended to reduce the process length to increase the number of convictions while reducing the time a person spends in pre-trial detention. This structural change imposed rigid deadlines to prosecutors during the post-imputation phase forcing them to only formulate an imputation once the investigation has been completed. Figure 4 confirms the reform success in reducing the process length. However, it also suggests that a substitution effect might have occurred since the number of days between opening an investigation and the formulation of imputation increased substantially.

[Figure 4 Here]

A second major transformation during the investigation phase consisted of forcing both parties to have a settlement hearing for some types of crime, including property crimes and assaults, among others. With this measure, the reform intended to decrease the number of cases

[9] While the DOJ worked with all Latin American governments that adopted the U.S. adversarial model, Colombia is the only country that permitted the DOJ direct intervention in the legislative debate (Langer 2007).

that ended in a formulation of imputation. This new procedural requirement might have contributed to the increase in the number of days between phases 1 and 2 (see Figure 3).

After the investigation phase, the reform included an impartial referee to decide whether pre-trial detention was necessary. The introduction of the Supervisory Judge (*Juez de Control de Garantías*-SJ) eliminated the adjudication functions that the prosecutor had between the formulation of imputation and accusation (see Figure 3 phases 2 and 3). Under this new model, prosecutors lost their ability to unilaterally impose preventive measures before trial; therefore, the government expected a decrease in the use of pre-trial detention. Zorro-Medina (2020a) shows that the reform decreased the number of persons in pre-trial detention, as intended.

Finally, the reform affected the trial stage by converting it into an oral public hearing. In addition to this structural change, the reform included new forms to terminate the criminal process: guilty pleas. Despite the pre-existence of agreements between the accused and prosecutor, those agreements never replaced the trial phase. Moreover, Law 600/00 listed the benefits an accused would obtain from accepting charges. In contrast, the new model (Law 906/04) gave prosecutors discretion to decide whether to offer a plea deal and the benefits from it. Zorro Medina (2020b) argues that the introduction of plea bargaining and the increase in prison sentences after the reform are highly correlated. In the same line, Figure 5 suggests that the decrease in the number of days between the investigation opening and a sentence seems to come from convictions and not from acquittals.

[Figure 5 Here]

Despite the radical transformation of the Colombian criminal procedure and the large-scale implementation of the U.S. adversarial model in the region, little research exploring the effects of the LACPR exists. Few studies have investigated the reform impact in prison conditions (Duce et

al. 2009; Hartmann Arboleda, Gómez, and Ortíz 2009; Zorro Medina 2020a, 2020b), but the impact it had on crime rates remains highly unexplored. In this paper, we attempt to provide one of the first major evaluations of the U.S. model in Latin America, while contributing to the literature on procedural law and crime rates. The introduction of the U.S. adversarial model transformed the certainty, celerity, and severity of punishment all at the same time, making unclear its effect on criminal behavior. In the next section, we explain in more detail the channels and mechanisms that we investigate in this paper.

IV. THE COLOMBIAN REFORM AND CRIME RATES

Besides exploring the effect of the U.S. adversarial model in Colombia, we intent on testing previous hypothesizes identified by the literature on other contexts on how procedural law affects criminal behavior. Figure 6 summarizes the nine channels through which the LACPR potentially affected criminal behavior and crime rates. We link each change with celerity (Mechanism 1), certainty (Mechanism 2), or severity (Mechanism 3). Figure 6 shows that, the literature suggests that the effect of the LACPR on crime rates is ambiguous. On the one side, the reform could have caused an increase in criminal behavior or a decrease in crime rates.[10]

[Figure 6 Here]

For Mechanism 1, we recognize that changes in the statute of limitations and strict deadlines to finish each phase after imputation affected procedural celerity differently. On one hand, and as shown in Figure 3, the gap between the event and imputation increased (Mechanism 1A). However, the time difference between imputation and sentencing decreased substantially (Mechanism 1B). Comparing both effects, Figure 5 suggests that the reduction in procedural times post-imputation is larger than the pre-imputation increase (Mechanism 1C). Focusing only on the

[10] We distinguish between criminal behavior and crime rates. We associated the former with actual illegal activity and the latter with how societies measure crime (Black 1970).

celerity effect, we predict that crime rates would decrease. Still, the delay in imputations has an additional impact since it postpones pre-trial detention requests, which affects the incapacitation effect of detention. Moreover, if the offender perceives pre-trial detention as a form of "punishment" or negative cost for criminal behavior, this delay would decrease the intertemporal value of the sanction.

Regarding certainty (Mechanism 2), the LACPR theoretically modified different conditional probabilities. First, it introduced stricter arrest rules to favor the rights of the defendant (Mechanism 2A). Following Atkins and Ruben (2003) and Hausman and Kronick's (2019) work, we anticipate a reduction in police activity related to arrests (probability of apprehension) and an increase in criminal behavior (Atkins and Ruben 2003). However, we expect that if this mechanism affected crime rates, we should only observe higher rates for offenses associated with reactive policing (i.e., robberies) and a decrease with those linked to proactive policing (i.e., drug offenses). To differentiate between street crime (i.e., muggings) from those occurring in private settings (i.e., sexual offenses), we include both types of crimes in our analysis.

The second certainty mechanism (2B) represents the introduction of stricter rules to request and impose pre-trial detention. Once again, this modification creates incentives for prosecutors and police officers to allocate fewer resources to pre-trial detention requests and focus on other procedural phases. Although pre-trial detention is not a sanction, for the offender, it represents a negative consequence of the illegal activity as it deprives his or her liberty. Hence, the change in the incentives of law enforcement agents reduces the probability of being detained and delays detention, affecting the perceived value of the sanction.

The last certainty mechanism (2C) is associated with the efforts to increase the criminal justice sentencing rate. The LACPR introduced plea bargaining to augment convictions at a lower

procedural cost. As discussed earlier, the literature relates plea bargaining with high conviction numbers, increasing the probability of conviction given being charged. From this effect, we would predict a decrease in criminal behavior. Nevertheless, it would depend on whether the likelihood of conviction given being charged has a higher impact than the reduction in the probability of apprehension.

Finally, Mechanism 3 is severity. The LACPR influenced the punishment severity without transforming any penalty. Since Colombia is a civil law country, the Penal Code lists all sanctions and guidelines to individualize them. The modifications came from the offender's intertemporal discounts. First, Mechanism 3A increased the value of the penalty by reducing the time between the event and the eventual punishment (connected to Mechanism 1C). Secondly, the settlement hearing requirement (Mechanism 3B) increases the likelihood that more cases do not end in formal criminal charges, reducing the value of the sanction. By making it a condition to formulate imputation and a fast way to close cases, prosecutors have incentives to promote quick settlements between the victim and the offender, at the cost of considerably reducing the severity of the punishment. Finally, Mechanism 3C includes the potential reduction in prison sentences from guilty-plea negotiations. Once again, prosecutors have incentives to allocate more resources into plea negotiations since they can achieve higher conviction rates in shorter periods at lower costs.

After summarizing the LACPR modifications that potentially altered criminal behavior and crime rates, we continue our empirical analysis by estimating the effect of the reform on crime rates. Later, we present the evidence suggesting that our nine theoretical mechanisms actually occurred. In the following sections, we describe the data and empirical method used.

V. DATA

To explore the impact of the U.S. adversarial model on Colombian crime rates, we use data from four sources. First, the Colombian National Police Department (NPD) provided the municipality monthly arrest and crime data from 2003 to 2008. We restrict our analysis to high impact social crimes, and from that subset, we selected those with lower measurement problems: homicides, assaults, muggings, business robberies, home burglaries, and vehicle thefts (De Mello, Mejía, and Suárez 2013; Di Tella and Schargrodsky 2004). According to the Colombian NPD, these six crime categories represented 97% of Colombian high impact social crimes in 2005 (Policía Nacional de Colombia-DIJIN 2005).[11] Additionally, we include sexual and drug offenses between 2004-2008 from the System of Statistical Information on Crime, Violations and Arrests (Sistema de Información Estadístico, Delincuencial, Crontravencional y Operativo de la Policia Nacional-SIEDCO). As mentioned in Section II, sexual (highly dependent on victims' report)[12] and drug (highly reliant on police activity) offenses capture a different relationship between crime rates and law enforcement agents than the selected high impact crimes. Therefore, we include these offenses to study the variations in crime and victimization risk (Stinchcombe 1963, Black 1970).

Using the information provided by the NPD, we constructed four aggregated measures: i) unweighted crime rate, ii) weighted crime index, iii) weighted violent crime index, and iv) weighted property crime index. We excluded sexual and drug offenses from these indices since we do not have these crimes information for 2003.[13] We created the aggregated weighted measures using the following equation similar to Ortega et al. (2015):

[11] We exclude offenses related with the Colombian conflict, which represent the remaining 3% of these high social impact crimes (extorsion, kidnapping, and terrorism) (Policía Nacional de Colombia-DIJIN 2005)

[12] In Colombia prostitution is not a crime, therefore sexual offenses do not include it, neither soliciting it. According to official data, by 2004 the majority of sexual offense were minors (84.3%) and aggressors are usually well-known by the victim (i.e. family members) (Medicina Legal 2005)

[13] We estimated our models using an alternative measure including sexual and drug offenses, and the results do not change substantially.

$$C_{i,t} = \sum_{s \in C} \left(\frac{p_s}{\sum_{s' \in C} p_{s'}} * S_{i,t} \right) \quad (1)$$

where $C_{i,t}$ represents one of the three crime indices (total, violent, or property) in municipality i, for month t; $S_{i,t}$ is the crime rate for index s, in municipality i, during month t, expressed in terms of crime incidences per 100,000 inhabitants, and p_s is the average sentence length in years for individuals convicted of crime s.[14] Table 1 reports the weights (p_s) we use in equation (1).

[Table 1 Here]

Second, we use the yearly population projections published by the Colombian National Administrative Department of Statistics (Departamento Nacional de Estadísticas-DANE).[15] Third, the Center for Economic Development at Universidad de Los Andes (CEDE) supplied us with the municipality-year data to control for socioeconomic and demographic differences. The CEDE panel includes information on education, income, inequality, forced displacement, and rural index of each municipality from 2000 to 2008. For our analysis, we use five variables traditionally identified by the literature as determinants of crime: (i) income per capita measured as average income per capita in the municipality (Crutchfield 1989; Hipp 2007; Messner and Tardiff 1986; Verbruggen et al. 2015); (ii) institutional capacity, included as a municipality fiscal performance index (Chamlin and Cochran 1995; Messner and Rosenfeld 1997; Rosenfeld and Messner 2006); (iii) rural index (Deller and Deller 2011; Kowalski and Duffield 1990; Ladbrook 1988; Lyerly and Skipper Jr 1981; Wells and Weisheit 2004); (iv) education, using as proxy municipal expenditure on education per capita (Buonanno and Leonida 2006; Hjalmarsson and Lochner 2012; Lochner 2004; Lochner and Moretti 2004); and (v) population density (Lobonţ et al. 2017; Sampson 1983;

[14] Average sentence length was calculated using the Penal Code of 2007. To calculate the maximum sentence length, we used the maximum prison sentence for each crime corrected by the maximum prison sentenced in case of an aggravating circumstance.
[15] By the time we wrote this paper, the latest census in the country was the 2005 Colombian census.

Shichor, Decker, and O'BRIEN 1979; Shichor, Decker, and O'Brien 1980). Table 2 displays descriptive statistics of relevant variables in these three data sources.

[Table 2 Here]

Finally, the National Prosecutorial Office (Fiscalía General de la Nación-FGN) provided information related to Mechanisms 1 to 3 (see Figure 5). We temporally accessed the Prosecutorial Information System for Law 900/04 (Sistema Penal Oral Acusatorio- Ley 904 & Ley 1098-SPOA) and the Prosecutorial Information System for Law 600 (Sistema de Información Judicial Ley 600/00-SIJUF). These two systems contain case-level data since 2004 for all the Colombian municipalities. From the SPOA and SIJUF, we obtained information at the municipality-month level on (i) the number of days between procedural stages, (ii) the number of imputations, (iii) the number of active cases, (iv) the number of cases with a preventive measure (house arrest or jail detention), (v) the number of settlements (before imputation), (vi) the number of convictions (in court or guilty pleas), and (vii) the number of acquittals (in court or agreements).

VI. EMPIRICAL STRATEGY

For this analysis, we estimate the effect of introducing the U.S. adversarial model on crime rates by exploiting the variation resulting from the staggered implementation of the LACPR in Colombia. As mentioned before, we divide our study into two components. First, we explore whether the U.S. adversarial model increased or decreased crime rates in the country. Second, we study the potential mechanisms through which this model affected criminality in Colombia. To do this, we use two empirical approaches. First, we estimate an event study model following the Granger-Type Causality test structure to examine whether the control and treatment groups had parallel trends during the pre-treatment period (2003-2004). We extended this model to the post-

treatment period (2005-2008) to capture phase-in effects in addition to anticipation effects. Equation 2 represents this unconditional leads-and-lags model:

$$Y_{i,t}^{s} = \gamma_0 + \sum_{m=-1}^{m} \gamma_m D_{i,t+m} + \sum_{p=0}^{p} \gamma_p D_{i,t+p} + \delta_i + \mu_t + \varepsilon_{i,t} \quad (2)$$

where $Y_{i,t}^{s}$ represents the crime rate in municipality *i* and period *t* of one of the following types of crime *s*: (i) unweighted crime rate; (ii) crime index; (iii) violent crime index; (iv) property crime index; (v) homicides, (vi) assaults, (vii) sexual offenses; (viii) drug offenses, (ix) muggings, (x) business robberies, (xi) vehicle thefts, and (xii) home burglaries. The term $\sum_{m=-1}^{m} \gamma_m D_{i,t+m}$ denotes the sequence of lagged treatment variables *(m=-1,…,-12 months)*, capturing the potential differences in the pre-treatment period between treatment and control groups. On the other hand, $\sum_{p=0}^{p} \gamma_p D_{i,t+p}$ denotes the present and future treatment sequence *(p=0,…,12 months)*, capturing the LACPR effect in the outcome variables. This leads-and-lags structure includes municipality (δ_i) fixed effects, year-month, year and month (μ_t) fixed effects, and municipality cluster standard errors. Although the implementation uses judicial districts for treatment assignment, their number is too small and clustering at this level would not be ideal (Cameron and Miller 2015).

In addition to the unconditional evaluation of the parallel trends and phase-in effects, we estimate a conditional version of equation (2). In this modified model, we include a vector $X_{i,t}$ containing the economic, demographic, and institutional variables mentioned in the previous section to control for time-varying municipality characteristics. Additionally, we incorporate $Z_{i,t-w}$ representing the lag of police arrests, in order to avoid endogeneity problems (Listokin 2003; Pfaff 2008; Rosenfeld and Wallman 2019).

Besides the main results we obtained from equation (2), we use this model to estimate statistical differences during the pre-treatment period for vectors $X_{i,t}$ and $Z_{i,t-w}$. For variables in vector $X_{i,t}$, we utilize the yearly-municipality information provided by the CEDE between 2000-

2004 and restricted the analysis to the pre-treatment period. In contrast, for $Z_{i,t-w}$, we estimate the unconditional leads-and-lags model represented in equation (2) to explore the LACPR effects on arrest rates as a potential mechanism through which the reform affected crime (see Figure 5, Mechanism 2A).

In our second empirical exercise, we exploit the gradual reform implementation using a difference-in-difference model with variation in the timing of the treatment (Goodman-Bacon 2018; Wing, Simon, and Bello-Gomez 2018). Formally, we estimate the following model:

$$Y_{i,t}^{s} = \beta_0 + \beta_1 LACPR_{i,t} + \beta_2 X_{i,t} + \beta_3 Z_{i,t-w} + \delta_i + \mu_t + \varepsilon_{i,t} \quad (3)$$

where $Y_{i,t}^{s}$ represents the already mentioned crime outcomes for type of crime *s* in municipality *i* at period *t*. Variable $LACPR_{i,t}$ is a binary variable indicating whether the reform has been implemented in a municipality. Vector $X_{i,t}$ contains the control variables. Since the lag-structure introduced by $Z_{i,t-w}$ imposes restrictions on placebo tests during the pre-treatment period, we incorporate an alternative estimation excluding $Z_{i,t-w}$. The exclusion of $Z_{i,t-w}$ does not affect the results significantly. We present these alternative results in the methodological appendix with the robustness and placebo checks. We also include time fixed effects (μ_t) to control for national trends and cyclicality in crime rates. Likewise, municipality fixed effects (δ_i) control for non-observable and time-invariant prison-municipality characteristics, and for differences in the propensity to report data.[16] Lastly, the term $\varepsilon_{i,t}$ represents a municipality-clustered error.

To explore Mechanisms 1 to 3 (see Figure 6), we estimate equation (3) using as dependent variables (i) clearance rates, (ii) preventive measures (jail detention or house arrest), (iii) settlements, and (iv) sentences (acquittals or convictions). Recognizing the difficulties in

[16] We attempted including municipality-time fixed effects or judicial districts-time fixed effects, but the number of observations is insufficient. Considering that the parallel trends and exogeneity assumption hold without conditioning by them, we argue our estimators are unbiased.

measuring changes in judicial and prosecutorial activity (Berggren and Gutmann 2020; Marciano, Melcarne, and Ramello 2019), we proposed two types of indicators. First, we construct rates allowing for information delays and timespans between denominator and numerator (CEPEJ 2014; Marciano et al. 2019):

$$Clearance\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Imputations_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}} \quad (4)$$

$$Settlement\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Settlements_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}} \quad (5)$$

$$Acquittals\ or\ Convictions\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Acquittals\ or\ Convictions_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}} \quad (6)$$

In the second group, we propose rates controlling for the total number of cases when no logical time-gap between denominator and numerator exists:

$$Preventive\ Measures_{i,t}^{s} = \frac{Cases\ with\ Active\ Measures_{i,t}^{s}}{Cases\ with\ Imputations_{i,t}^{s}} \quad (7)$$

$$Acquittals\ or\ Convictions\ Ratio_{i,t}^{s} = \frac{Acquittals\ or\ Convictions\ in\ Court_{i,t}^{s}}{Acquittals+\ Convictions\ in\ Court_{i,t}^{s}} \quad (8)$$

Finally, in all our estimations, we use dependent variables and arrest information normalized by 100,000 inhabitants. Additionally, we use logarithmic transformations of all dependent ($Y_{i,t}^{s}$) and cofounding variables ($X_{i,t}; Z_{i,t-w}$) to allow for non-linear relationships. To avoid losing information caused by zero values, we used a ln(A+1) transformation.

VII. RESULTS

Main Results: Crime Rates

The findings from the leads-and-lags model (model 1) suggest that treatment and control groups had parallel trends, and no-anticipation effects occurred before the implementation of the reform. Figure 7 shows the outcomes for the aggregated crime rates between 2004 and 2005. Figures 8 and 10 display the results for the eight individual offenses studied over the same period.

These figures corroborate that during the pre-treatment period ($m = -1, \dots, -12$), the difference between treatment and control groups was statistically equal to zero ($\gamma_m = 0$).[17]

During the post-treatment period ($p = 0, \dots, 12$), Figure 6 reveals that the U.S. adversarial model lead to an increase across all aggregated crime rates in Colombia. We observe a seasonality effect in the property crime index, and a consistent increase in the violent crime index for $p = 0, \dots, 7$.

[Figure 7 Here]

Looking at individual offenses, we find that the reform effect varies depending on the type of crime. For violent crimes, the increase seems to be completely driven by an increase in assaults. For homicides and sex offenses, we do not see a consistent effect. Figure 8 shows an initial positive impact of the reform in sex crimes; however, since this is an unconditional model, we recognize that the variation could be potentially correlated with other confounding variables. We explore more of these effects in our difference-in-difference model. Additionally, Figure 8 reveals a decrease in drug offenses for $p = 0, \dots, 12$. These differential effects suggest that the mechanisms identified in Figure 8 affect criminal activities differently, as predicted by the literature.

[Figure 8 Here]

As mentioned previously, the report of drug offenses is different from the other crimes studied in this paper since, in most cases, they do not harm another person. Thus, these crimes are primarily dependent on police activity (Mechanism 2A, Figure 6). We estimate the unconditional leads-and-lag model to evaluate whether the LACPR altered arrest patterns that could have caused a decrease in the drug offenses report (lower probability of apprehension). We report those results in Figure 9. According to our findings, after the LACPR, the arrest rates per 100,000 inhabitants

[17] The restricted version of model 1 does not show significant differences with Figures 7, 8, and 10. We included only the unrestricted model results since they support the existence of parallel trends without any conditional adjustment.

suffered a drastic reduction. While our data does not allow us to estimate the changes in the risk of drug arrests, previous literature has identified drug offenses as generally available in terms of arrests since policing activity in these cases tends to be proactive rather than reactive (Blumstein 1995; Mitchell and Caudy 2015; Tonry 1995; Tonry and Melewski 2008; Warner and Coomer 2003).

[Figure 9 Here]

For the specific property crime offenses, we spotted the same seasonality observed in the aggregated property crime index. Figure 10 shows a seasonal increase in all four crimes; however, the effect is observed in different months of the post-treatment period. For muggings and business robberies, Figure 10 illustrates an increase at the moment the LACPR occurred and at the end of the year. In contrast, for vehicle thefts and home burglaries, the LACPR caused an effect only after the fifth month ($p = 5$). A potential explanation of these disparities within property crimes could be related to what the literature calls the "hot products" market, and the criminal opportunity discussed earlier in this paper. Differences in the "hot product" availability (Clarke and Webb 1999; Sparks 1980; Wilkins 1971), disposable facilities (Langworthy and LeBeau 1992; Sutton 1995, 2010; Sutton, Johnston, and Lockwood 1998), and removable complications (Clarke and Webb 1999; Pease 1997) shape the crime cycles of each illegal market.

[Figure 10 Here]

Before estimating our difference-in-difference specification, we use the leads-and-lags model to assess potential differences in our treatment and control groups in any of the socioeconomic factors included in model 2. Figure 11 illustrates that, except for population density, our treatment and control groups had parallel trends during the five years before the LACPR implementation. To confirm that the parallel trends assumption continues to be valid after

the inclusion of vectors $X_{i,t}$ and $Z_{i,t-w}$, we estimate model 1 including these confounding variables. Table 3 confirms the results presented in Figures 6 to 9.

[Figure 11 Here]

[Table 3 Here]

Continuing to the difference-in-difference model, Table 4 presents the results for aggregated crime measures, which confirm that the U.S. adversarial model caused an increase in crime rates in Colombia. Our findings suggest that the LACPR caused a rise of 22% in overall crime, 15% in violent crime, and 8% in property crime. After including an exposure time variable, we find that a 12-month exposure to the U.S. adversarial model caused a growth of 34% in total crime, 17% in violent crime, and 18% in property crime.

[Table 4 Here]

For each violent crime, we find that the rise in assault rates (25%) drives most of the effect of the reform in this category. Table 5 shows that no statistically significant impact occurred in sexual offenses, and a small increase of 3% in homicides happened after the reform. Moreover, our results indicate that after a 12-month exposure, the assault rate increased by 30%. In contrast to the effect on violent crimes, Table 5 ratifies the decrease in drug offenses observed in Figure 7. These findings reveal a 26% reduction in drug offenses after the LACPR implementation and a 12% reduction after exposure of 12-months.

[Table 5 Here]

For property crimes, our estimations suggest that the adversarial model caused a significant increase in all four offenses. We observe the highest impact in muggings (9%), followed by home burglaries (6%). For business robberies and vehicle thefts, we find a rise of 4% and 2% respectively (see Table 6). Furthermore, Panel B shows that after 12-months of exposure to the reform,

muggings increased 22%, business robberies 9%, and home burglaries 11%. In contrast, vehicle thefts experienced a deceleration in its increase rate after 12-months of exposure, with an increase of only 1%.

[Table 6 Here]

From our results, we conclude that besides drug and sex offenses, the U.S. adversarial model caused an increase in Colombian crime rates. In the case of drug offenses, we believe that the decrease observed in Figure 8 and Table 5 is associated with the decline in arrest rates seen in Figure 9. However, the mechanisms through which the LACPR affected other illegal activity are still unclear until we verify whether any of the nine theoretical predictions materialized in real modifications. As mentioned previously, we proposed four categories to explore the nine possible ways that could have affected criminal behavior (see Figure 6): (i) clearance rates, (ii) preventive measures, (iii) settlements, and (iv) plea bargaining. We explore each one of these mechanisms in the following subsection.

Secondary Results: Identifying Potential Mechanisms

Clearance Rates

We discussed the reform's change on the statute of limitations earlier, and we show that the number of days between opening an investigation or crime date and the imputation increased (see Figure 4). This delay in formulating an imputation postponed the formal initiation of a criminal case, which can lead to consequences in illegal activity. As explained before, previous literature associates procedural delays with higher crime rates (intertemporal discount). To explore this channel, we evaluate the reform's impact on the number of imputations. Using model 2, we

assess the effects on clearance rates (defined in Equation 4). Table 7 reveals that after the reform, the clearance rate decreased for all crimes.[18]

[Table 7 Here]

Preventive Measures

As discussed previously, the low number of yearly convictions made Colombia crime control extremely dependent on pre-trial detention, like many other countries in the region.[19] Before the LACPR, the Colombian system only resolved 21% of new cases, while pre-trial detainees represented more than 50% of the incarcerated population. Under the previous legislation, prosecutors imputed charges in the early stages of the investigation to take advantage of the pre-trial detention's incapacitation effect. However, by reducing the number of imputations, the reform could have affected the levels of pre-trial detainees. Zorro-Medina (2020a) shows that the LACPR caused a decrease in the number of pre-trial detainees in the penitentiary system; however, her results do not explore changes in the imputations numbers.

In our analysis, we evaluate what happened with pre-trial detention using the ratio expressed in Equation 7. Table 8 reveals that after the LACPR, pre-trial detention in jail decreased for all crimes, although for property crimes and sex offenses this effect is not significantly different from zero. Additionally, it shows an increase in the use of house-arrest for all crimes, including property offenses.[20]

[Table 8 Here]

[18] The FGN only provided an aggregated variable of property crimes (muggings, vehicle thefts, business robberies, and home burglaries), thus we only tested these mechanisms for these property crime measure.
[19] During the 1990s, Colombia housed 43,000 inmates in spaces with a capacity for less than 30,000 people; Peru held 28,000 people in centers with capacity of 20,000; Venezuela held between 24,000 and 27,000 in facilities built to hold 15,426 inmates; and Chile, housed sixteen inmates in cells built for six persons (Ungar 2003; Zorro Medina 2020a).
[20] We did not include exposure time in these results as introducing the exposure variable does not change the results and it is not statistically significant.

We speculate that losing the incapacitation effect of pre-trial detention in jail could have led to an increase in crime rates. However, we do not have information on whether those persons sent to house-arrest or not-held in jail re-offended during the trial. We leave this issue for further research.

Another hypothesis identified by the literature suggests that, while pre-trial detention decreased, the number of people entering jail during trial increased. Zorro-Medina (2020) shows that the number of pre-trial entries and releases to jail increased after the LACPR, explaining that the rotation increased even if the number of total detentions went down. Previous empirical work on the effect of pre-trial detention on crime rates in the U.S. suggests that while the incapacitation effect reduces criminal activity in the short-term, it might lead to new criminal charges in the long-term (Heaton, Mayson, and Stevenson 2017; Leslie and Pope 2017)

Settlements

The third channel we explore is the settlement hearing required before the formulation of imputation for property crimes and assaults. Table 9 displays the results of the difference-in-difference model using settlement rates as the dependent variable (see Equation 5). From these results, we observe an increase in settlements after the LACPR implementation.

[Table 9 Here]

As we discussed previously, the settlement hearing can affect criminal behavior by increasing the temporal gap between the event and punishment, decreasing the intertemporal value of the sanction. Additionally, since these agreements take place before a formal criminal process initiates, it removes prison or other non-monetary penalties as potential consequences from the illegal activity, reducing, even more, the severity of punishment. This new requirement creates a

channel through which offenders rapidly learn from their experiences with the criminal justice system and trigger an upward revision of the perceived probability of punishment.

Plea Bargaining

Finally, the reform introduced plea bargaining into Colombia to increase the number of convictions. As mentioned previously, the literature has associated plea negotiations with shorter sentences. Since our data does not allow for the evaluation of the effect of the reform on sentence length, we estimate whether an increase in convictions occurred and if it is associated with guilty pleas instead of trial outcomes. First, we evaluate the impact of the reform in sentencing. Table 10 presents the results using the dependent variable in Equation 6. These findings suggest that the LACPR increased both the number of acquittals and convictions. These results confirm that the reform successfully increased the efficiency of the Colombian criminal justice system.

[Table 10 Here]

While the literature has traditionally associated improvements in procedural efficiency with lower crime rates, the Colombian case seems to suggest a different story. The explanation behind this discrepancy could be related to the type of convictions obtained. Table 11 indicates that the increase in convictions occurred because of guilty pleas and not trial convictions. The results in Table 11 show that while acquittals in trial increased, trial convictions decreased for homicides or did not change for other offenses.

[Table 11 Here]

In order to confirm that after the LACPR conviction sentences occurred via guilty pleas instead of trials, we compared the changes in the number of days between opening an investigation and a conviction/acquittal sentence. Moreover, as discussed previously, Figure 5 reveals that the

gain in procedural efficiency only occurred in conviction sentences supporting the hypothesis of guilty pleas as the primary source of punishment after the LACPR.

The association between shorter sentences and plea bargaining could explain why, despite increasing the number of convictions and procedural efficiency, Colombia experienced an increase in crime. Another hypothesis connects the increase in the use of prison without resocialization programs with higher crime rates in the long-run (Pritikin 2008; Shepherd 2006; Spelman 2008). Zorro-Medina (2020b) study on the LACPR effects on incarceration rates suggests that the number of persons that enter prisons increased, specially re-offenders, confirming that an increase in imprisonment occurred in the country.

VIII. CONCLUSION & DISCUSSION

Criminology literature identifies Latin America as one of the most violent regions in the world (Bergman 2018; Müller 2018; Soares and Naritomi 2010). This has attracted attention to the study of crime in Latin American. However, most of this research has focused on how the economic (Fajnzylber et al. 1998; Soares 2004; Soares and Naritomi 2010), social (Bergman 2006; Di Tella and Schargrodsky 2004; Soares and Naritomi 2010), and institutional (Dammert 2012; Malone 2010; Müller 2018; Schultze-Kraft et al. 2018) factors influence illegal behavior. Yet, the research on Latin American crime tends to ignore the effects of criminal procedural law on crime. In this paper, we explore the most extensive Latin American procedural reform in the last two centuries and its impact on the soaring crime rates over the last 30-years. Some researchers have documented the relationship between procedural law and crime in the region (Soares and Sviatschi 2010). Nonetheless, the consequences of adopting an adversarial system using the U.S. model continue highly unexplored decades after the LACPR.

In this paper, we use the literature developed mostly for the U.S. and European cases to explore the consequences that adopting the U.S. adversarial model had on crime rates in Latin American. We identify the three mechanisms that connect procedural law and crime: i) celerity, ii) certainty, and iii) severity of punishment. The LACPR affected these three channels simultaneously since adversariality implied a radical transformation of the Latin American penal process, theoretically altering the offender's and law enforcement agents' incentives significantly.

For our analysis, we group the primary changes into nine categories (Figure 6). Using our theoretical framework, we predict that the reform caused ambiguous effects on criminal behavior. On the celerity side, we distinguish a procedural length decrease during the post-imputation phase, but an increase in the pre-imputation stage. Empirically, when we calculate the overall effect on celerity, we found that the LACPR reduced substantially the length of the process. From this perspective, we anticipated a reduction in crime rates following the previous empirical findings (Dalla Pellegrina 2008; Dušek and Traxler 2016; Soares and Sviatschi 2010).

On the certainty side, we identify three different changes in the probability of punishment. First, a reduction in the likelihood of apprehension occurred since the police responded to the stricter arrest rules with a decrease in the arrest rates (Atkins and Rubin 2003; Hausman and Kronick 2019). Secondly, a reduction in the probability of being charged occurred since the stricter imputation rules created incentives for prosecutors to settle more and present fewer formal charges. As we show an increase in settlements occurred while a reduction in the clearance rate happened after the LACPR. Lastly, we find an increase in the probability of conviction since the efficiency of the system improved. Our findings align with Zorro-Medina (2020b) results on the rise in prison convictions and entries to Colombian prisons after the LACPR.

Following the existing literature on certainty, we predicted an increase in crime rates since previous scholars associate apprehension likelihood with a stringer effect on criminal behavior (Nagin 2013a; Nagin et al. 2015; Wilson and McLaren 1972b). However, we distinguish between criminal behavior and crime rates. The first one, we associated with actual illegal activity and the second one with how societies measure crime (Black 1970). Hence, we predicted an increase in offenses linked to reactive policing work and a decrease in proactive policing.

Finally, on the severity side, we expected an increase in crime rates, particularly in those crimes with a settlement hearing requirement. The reform affected the perceived value of the sanction without changing any formal penalty in the Penal Code. By introducing alternatives forms to terminate the process, first avoiding a criminal charge and second before trial, the adversariality negotiation component opened the door to impose less severe penalties. We show that the productivity increase seems to be driven by guilty pleas instead of convictions in a trial (see Figure 5). Consequently, we expected an increase in criminal behavior.

After estimating our event study and difference-in-difference models, we confirmed that the LACPR caused an increase in overall crime of 22% and 34% after 12-month exposure, indicating that the incentives to engage in criminal activity from the decrease in certainty and severity surpassed the deterrence effect of productivity and celerity. As expected, the results vary across different offenses. For sexual crimes, we find no statistically significant impact. We hypothesize that no changes in the report of sexual crimes occurred. Moreover, we understand these offenders are notably different from other criminals since the literature has traditionally associated these offenses with cognitive distortions not found in other cases (Abel et al. 1984; Ó Ciardha and Ward 2013).

For the other violent crimes, we observe a dramatic increase in assault rates (25%-30%), while a small change in homicides occurred (3%-4%). The settlement mechanism can explain the variation between these offenses. While assault cases require a settlement hearing, homicide imputations proceed without it. These differences reveal the weight settlements potentially had on Colombian crime rates.

Moving to drug offenses, the results confirm our prediction. Comparing Figures 8 and 9, we observe similar patterns between the arrest rate contraction and drug crime rates decrease. Contrasting drug and sexual offenses, we notice the literature distinction between proactive and reactive policing in crime rates measurement. Our findings do not suggest that drug offenses dropped, rather than since these activities depend on policing activity, all we capture in these measures is detection instead of criminal behavior (Black 1970).

Finally, for property crime, our estimations reveal that the LACPR increased muggings by 9%-22%, business robberies by 4%-9%, vehicle thefts 1%-2%, and home burglaries by 6%-11%. For these offenses, we recognize different incentives operated at the same time. Since these crimes depend on reactive policing, we anticipated this increase. According to the literature, the changes in policing incentives caused a reduction in arrests, which trigged revisions on the perceived likelihood apprehension. Still, the increase in the probability of conviction and prison could have potentially deterred these criminal activities. In this case, our data does not allow us to separate both effects. However, the final observed increase suggests that for these property crimes, a decrease in the probability of apprehension or detection surpasses the weight of an increase in the likelihood of conviction and going to prison.

FIGURES

Figure 1. Latin American Criminal Procedural Revolution Implementation Years, 1990-2020

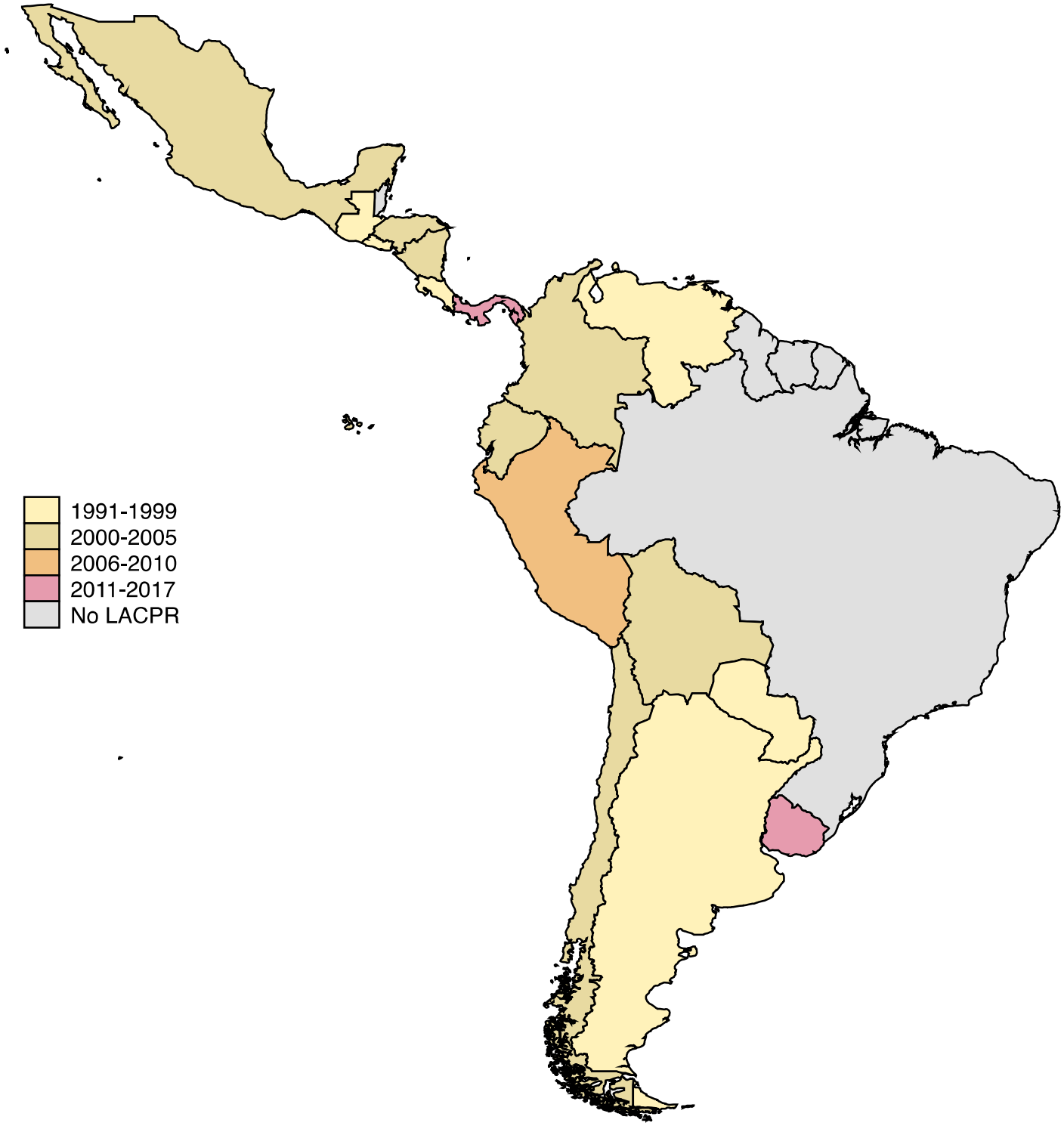


Figure 2. LACPR Implementation in Colombia, 2005-2008

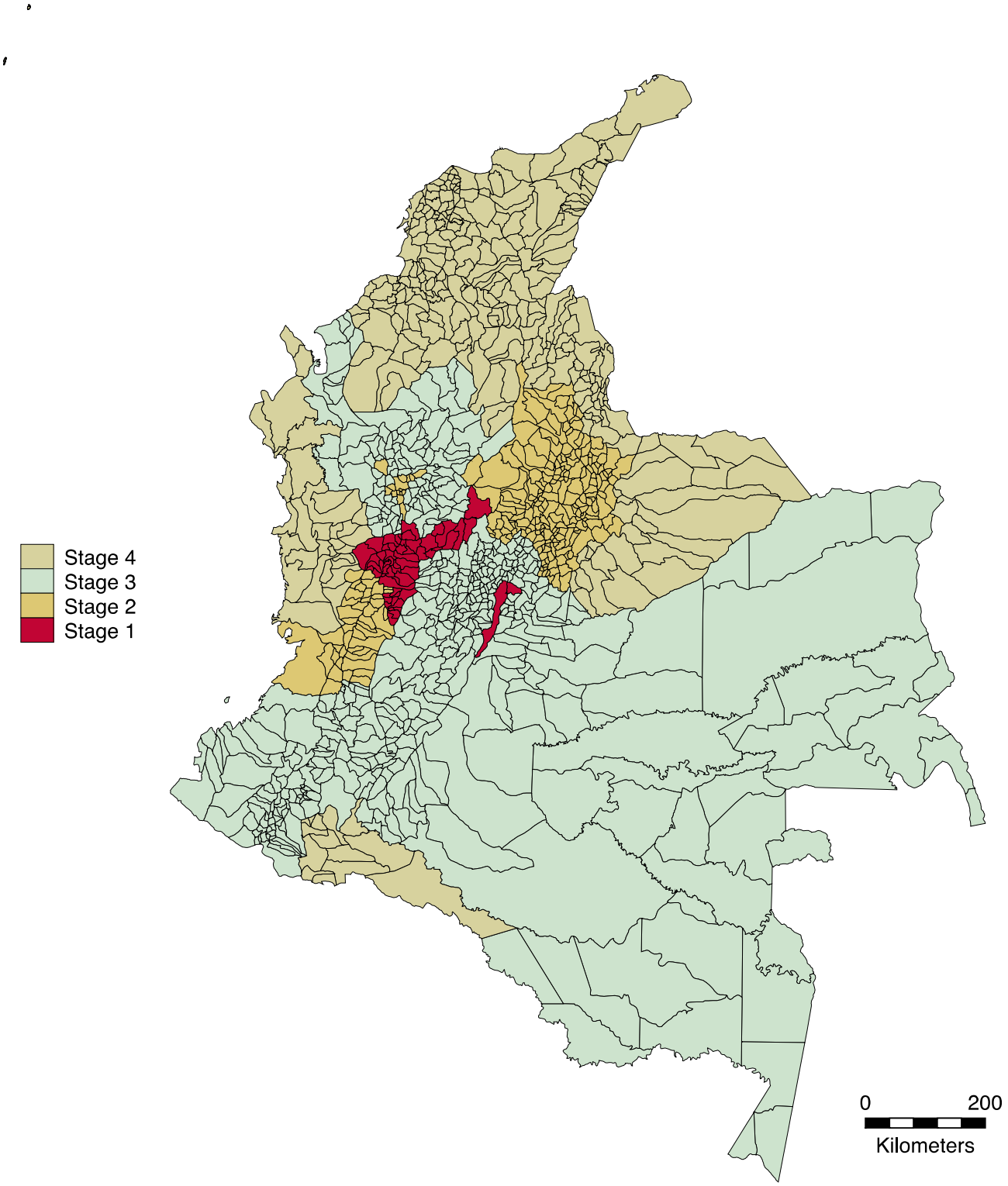


Figure 3. The LACPR in Colombia

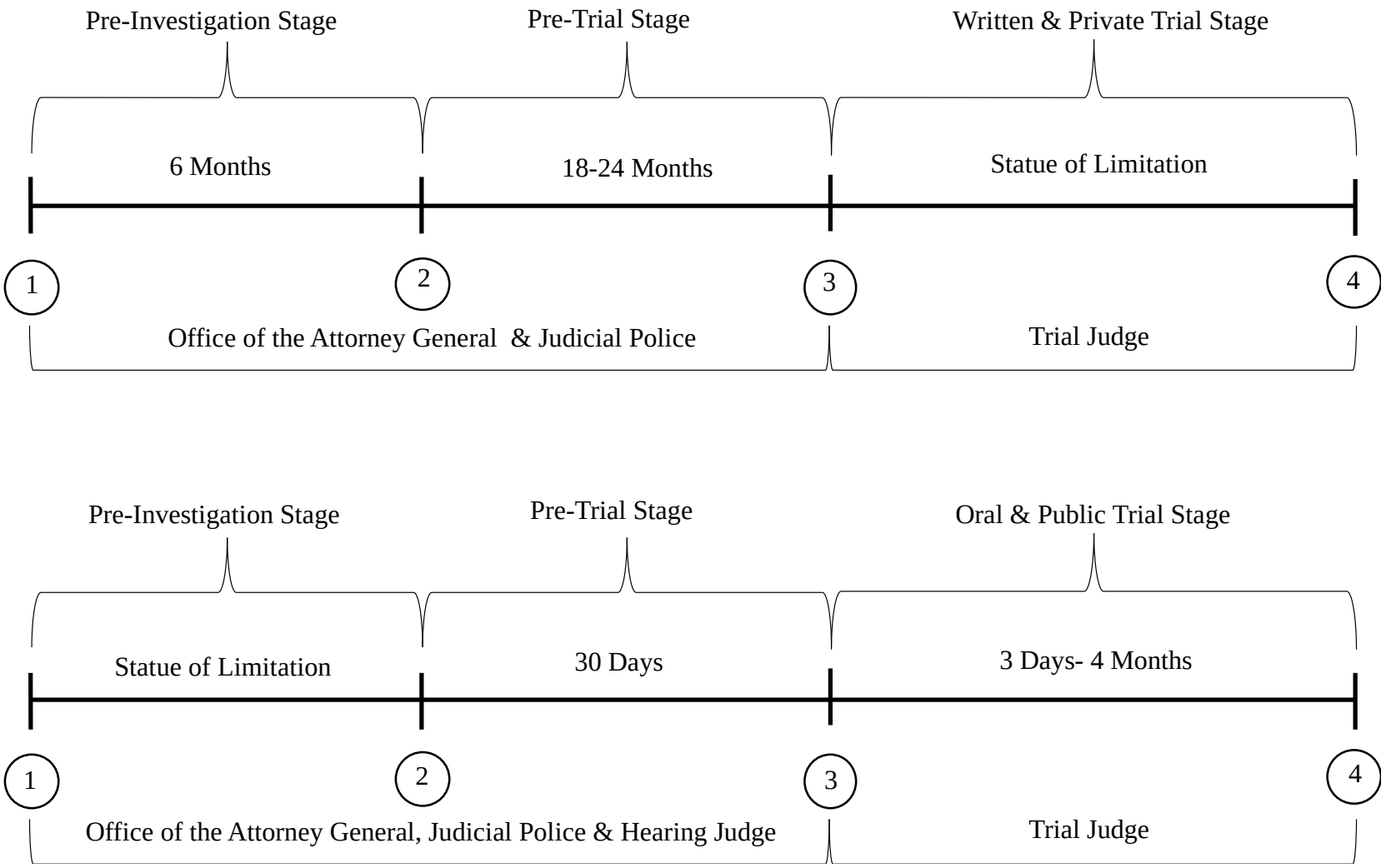
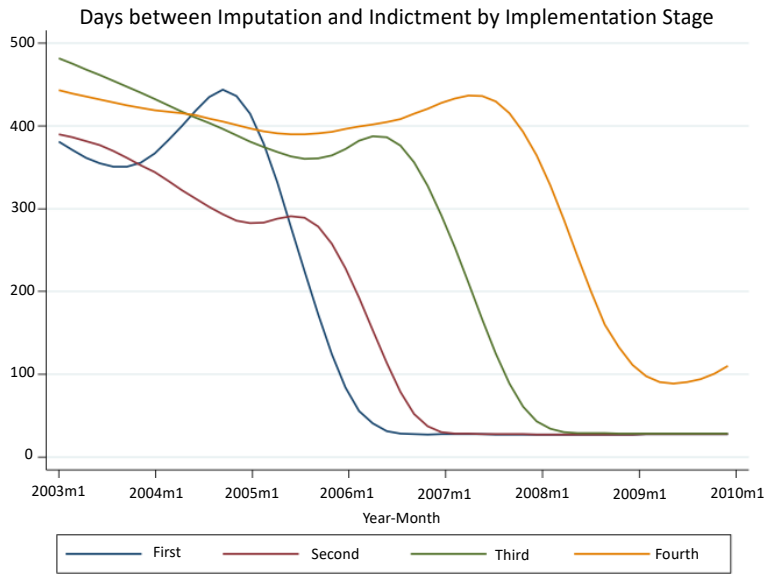
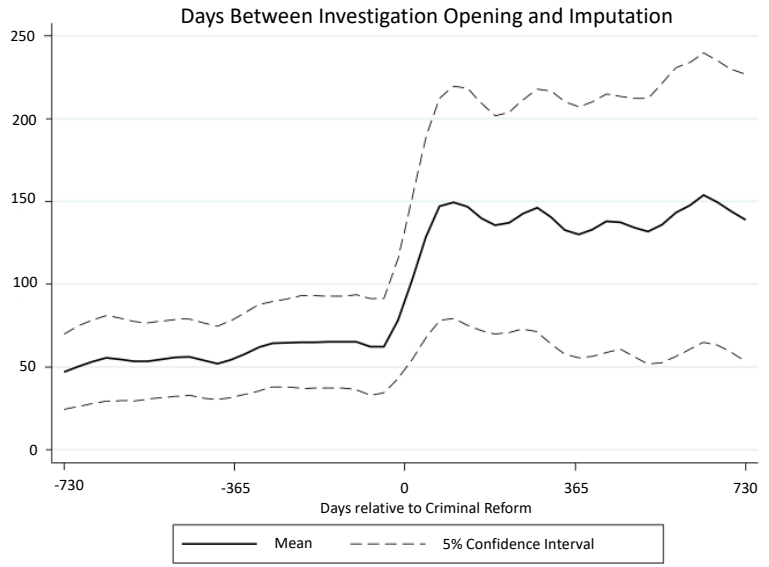
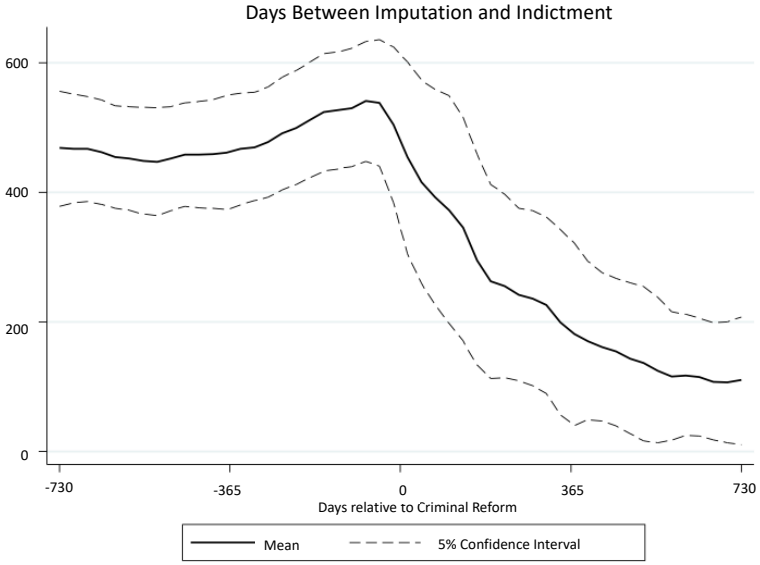


Figure 4. Procedural Length Reduction in Colombia after the LACPR







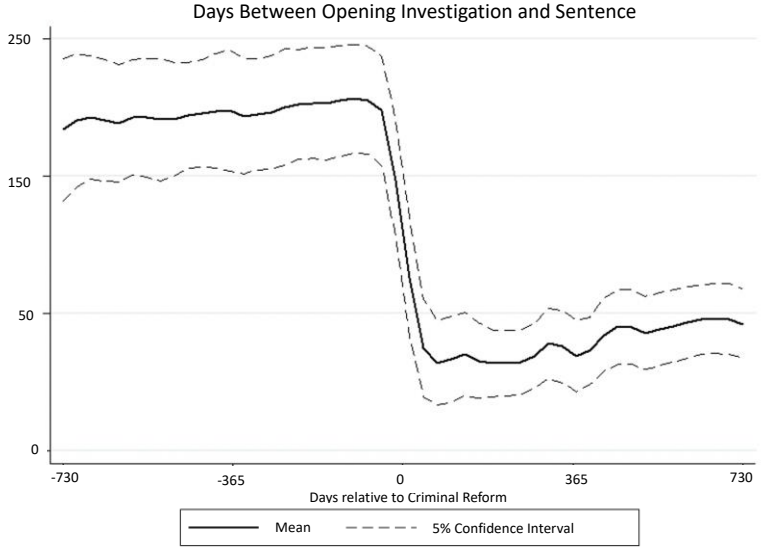
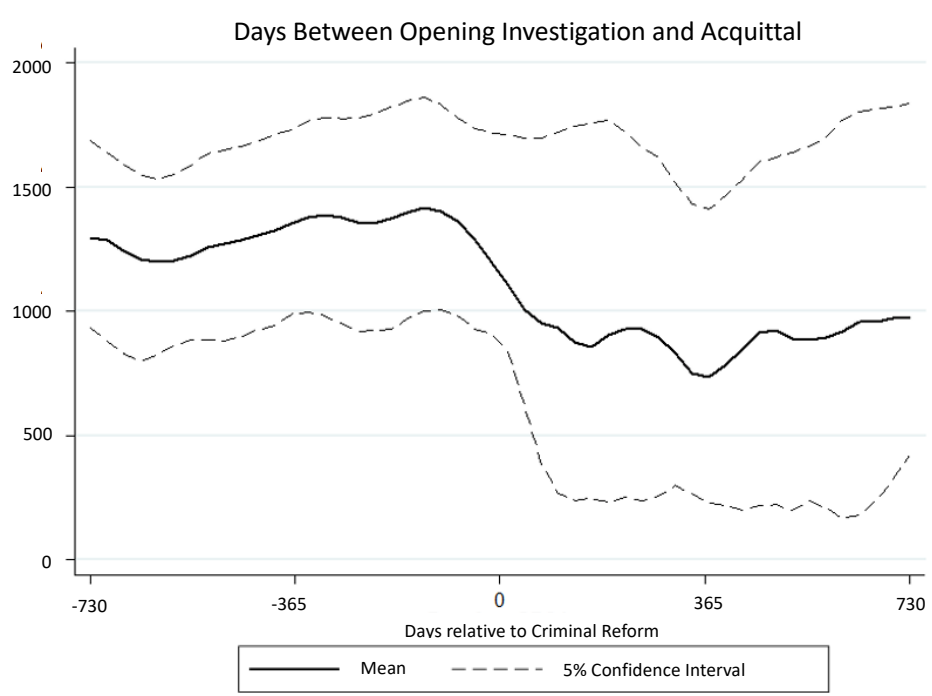


Figure 5. Procedural Length Change in Colombian Convictions and Acquittals after the LACPR



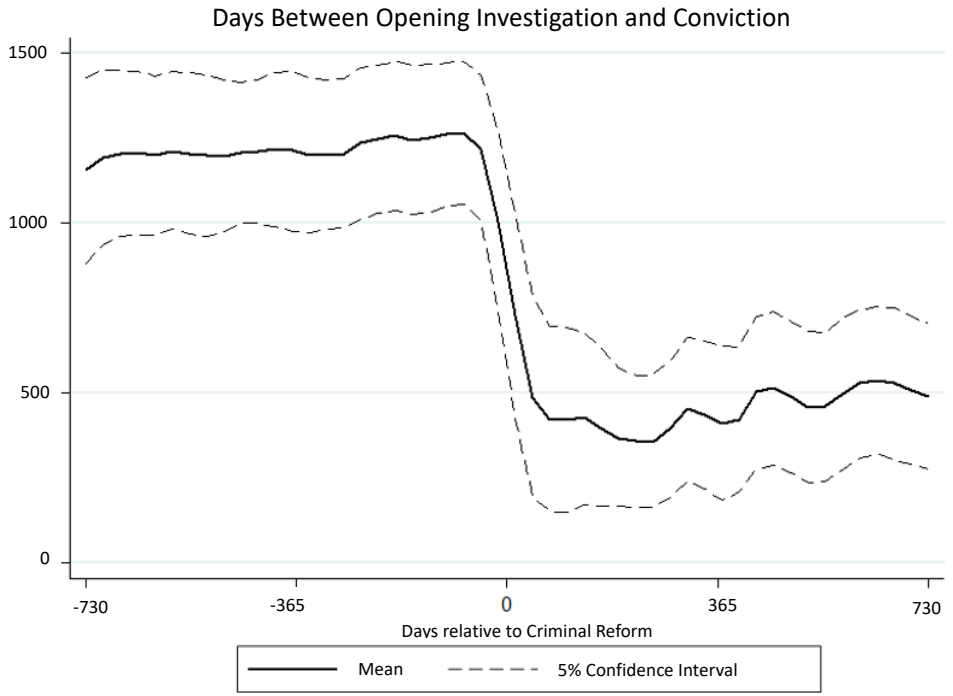


Figure 6. LACPR Potential Mechanisms Affecting Crime Rates

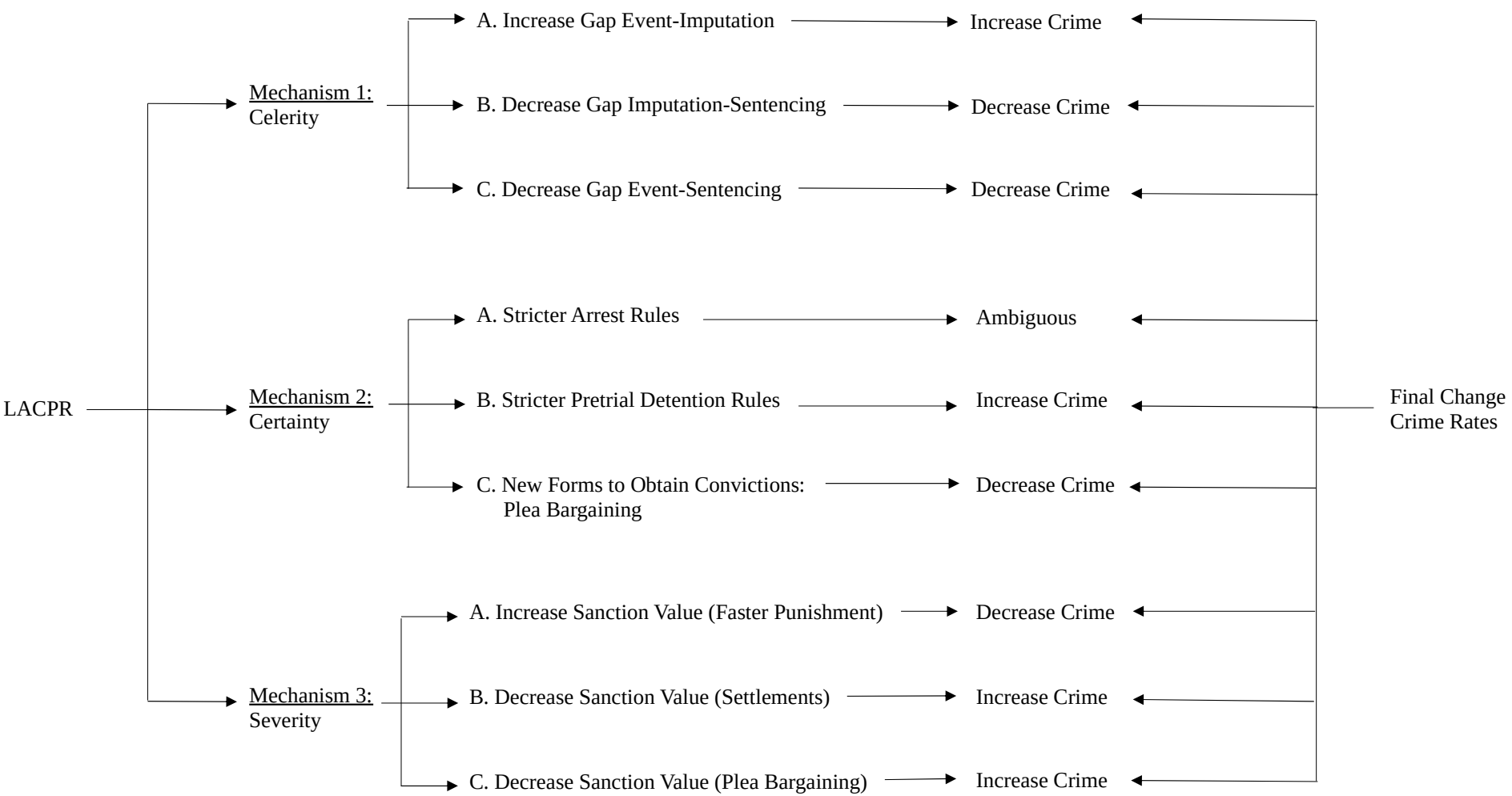


Figure 7. Leads-and-Lags Results for Aggregated Measures

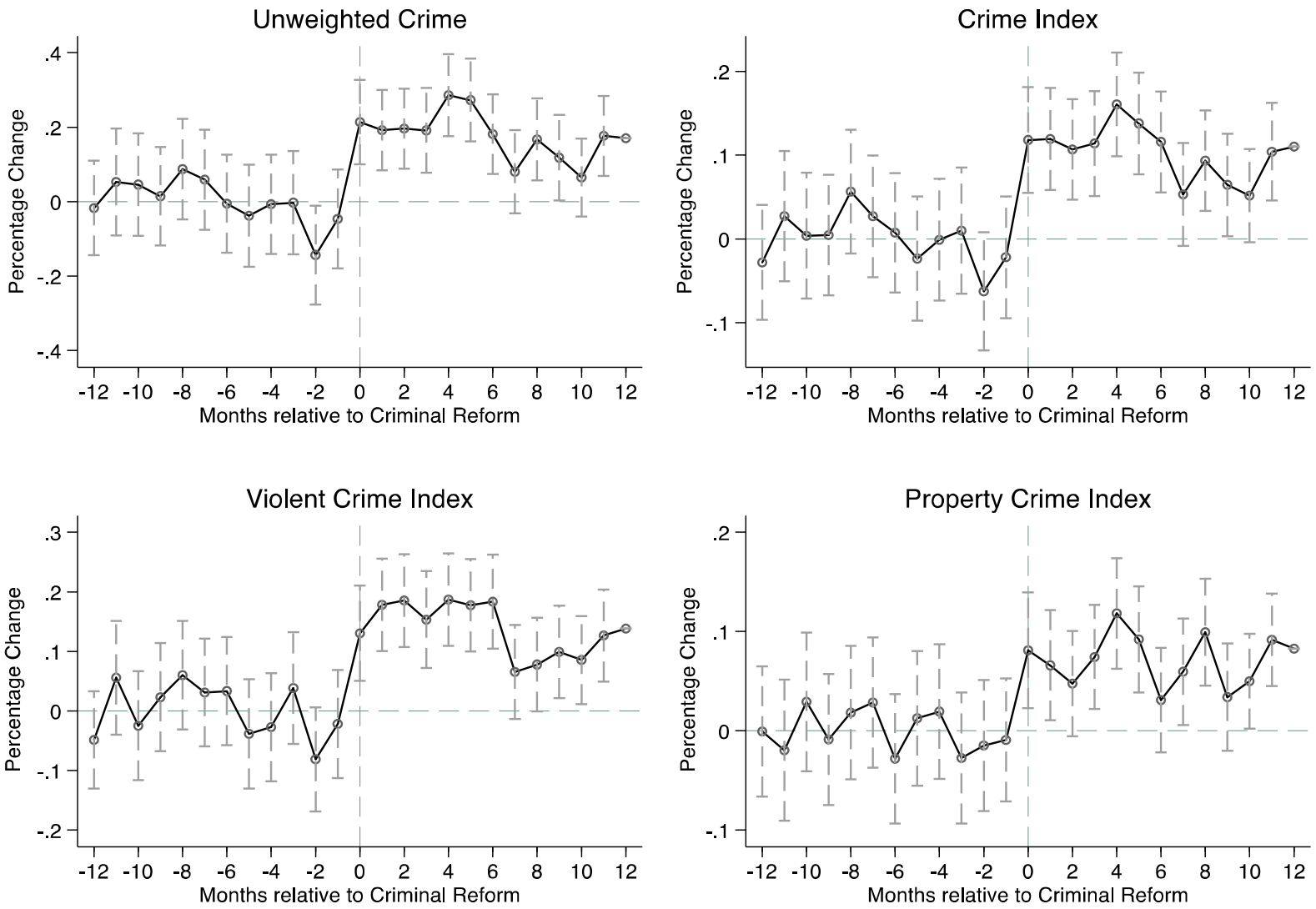


Figure 8. Leads-and-Lags Results Violent and Drug Offenses

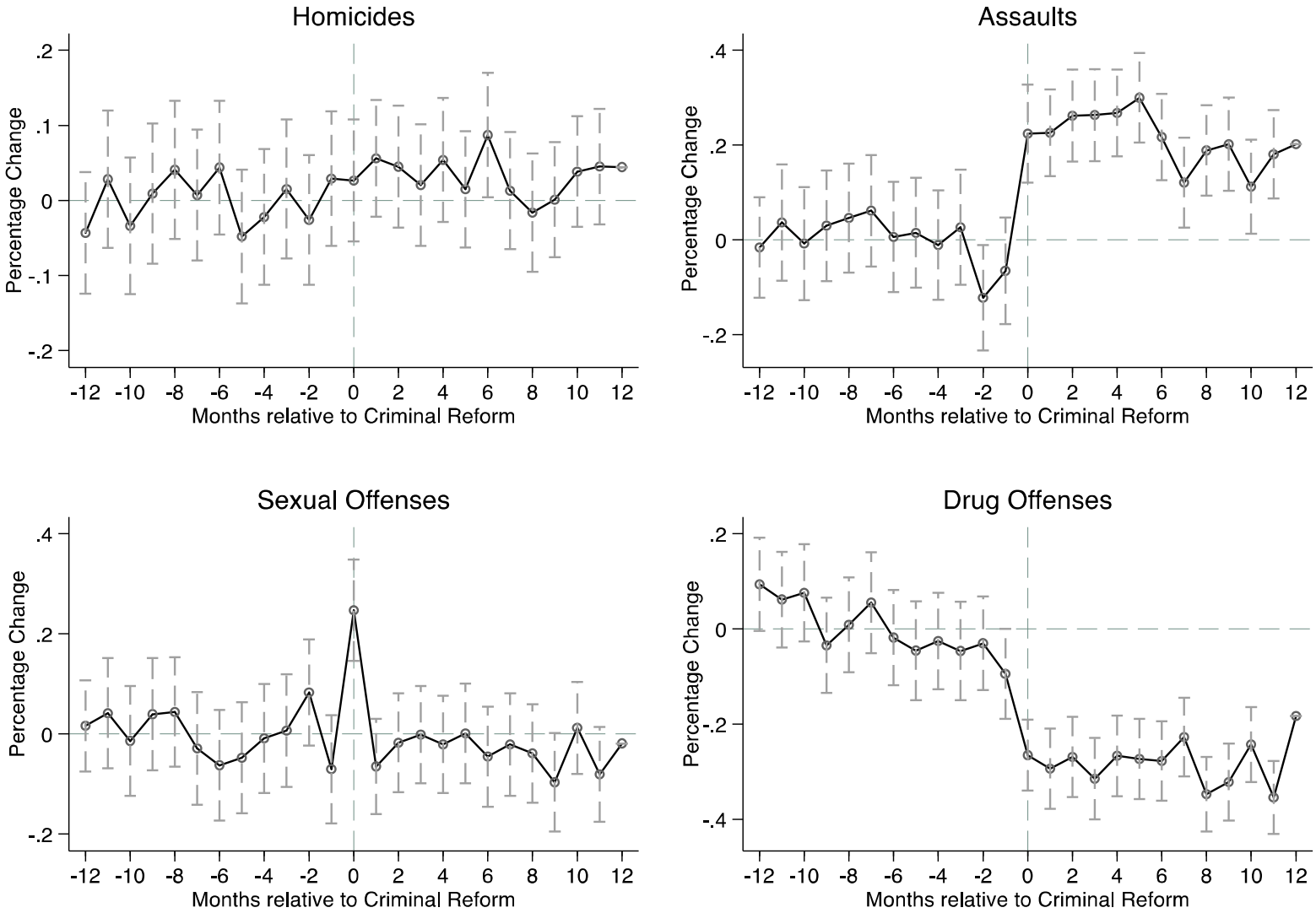


Figure 9. Leads-and-Lags Arrest Rate Per 100,000 Inhabitants

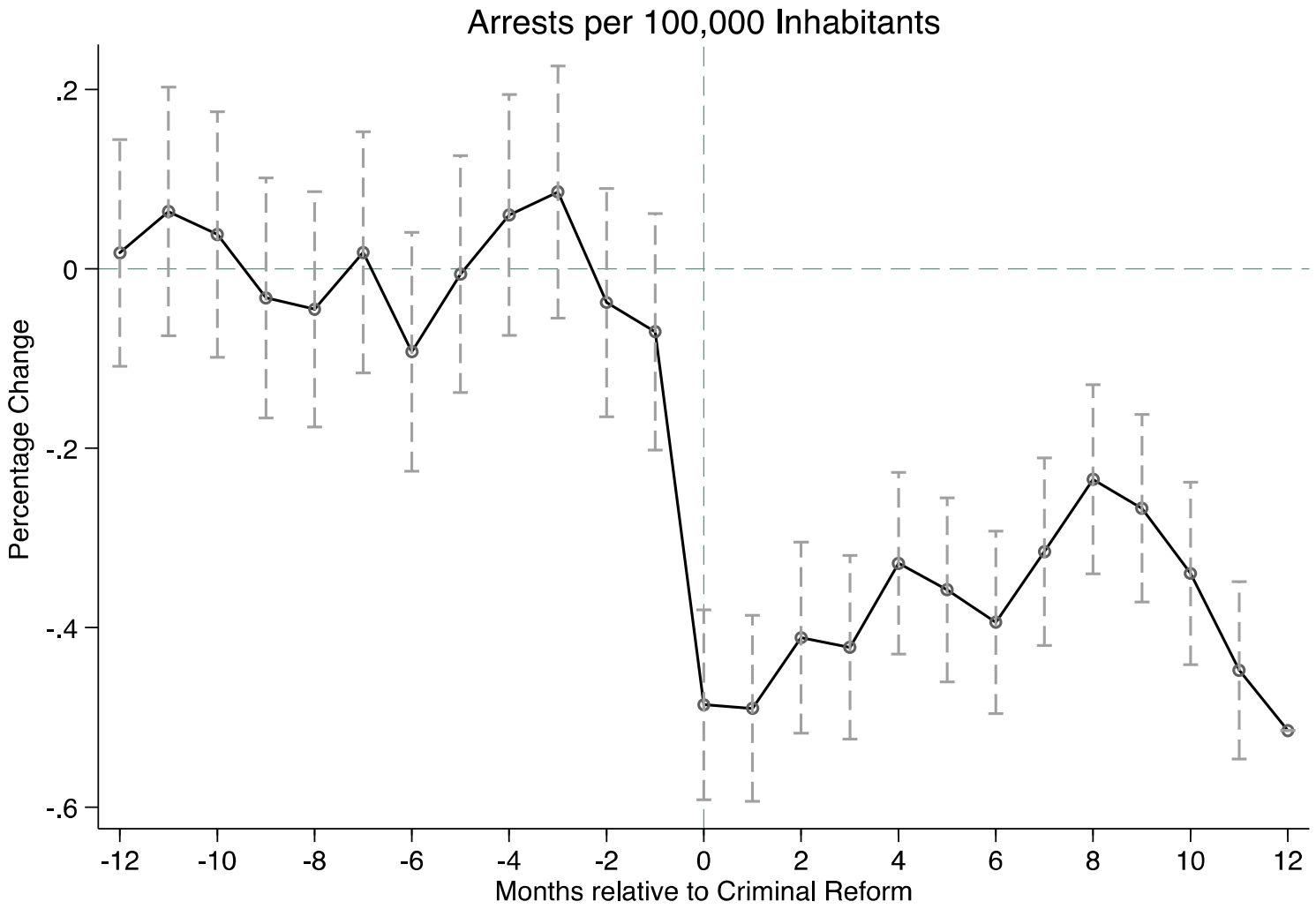


Figure 10. Leads-and-Lags Results Property Crimes

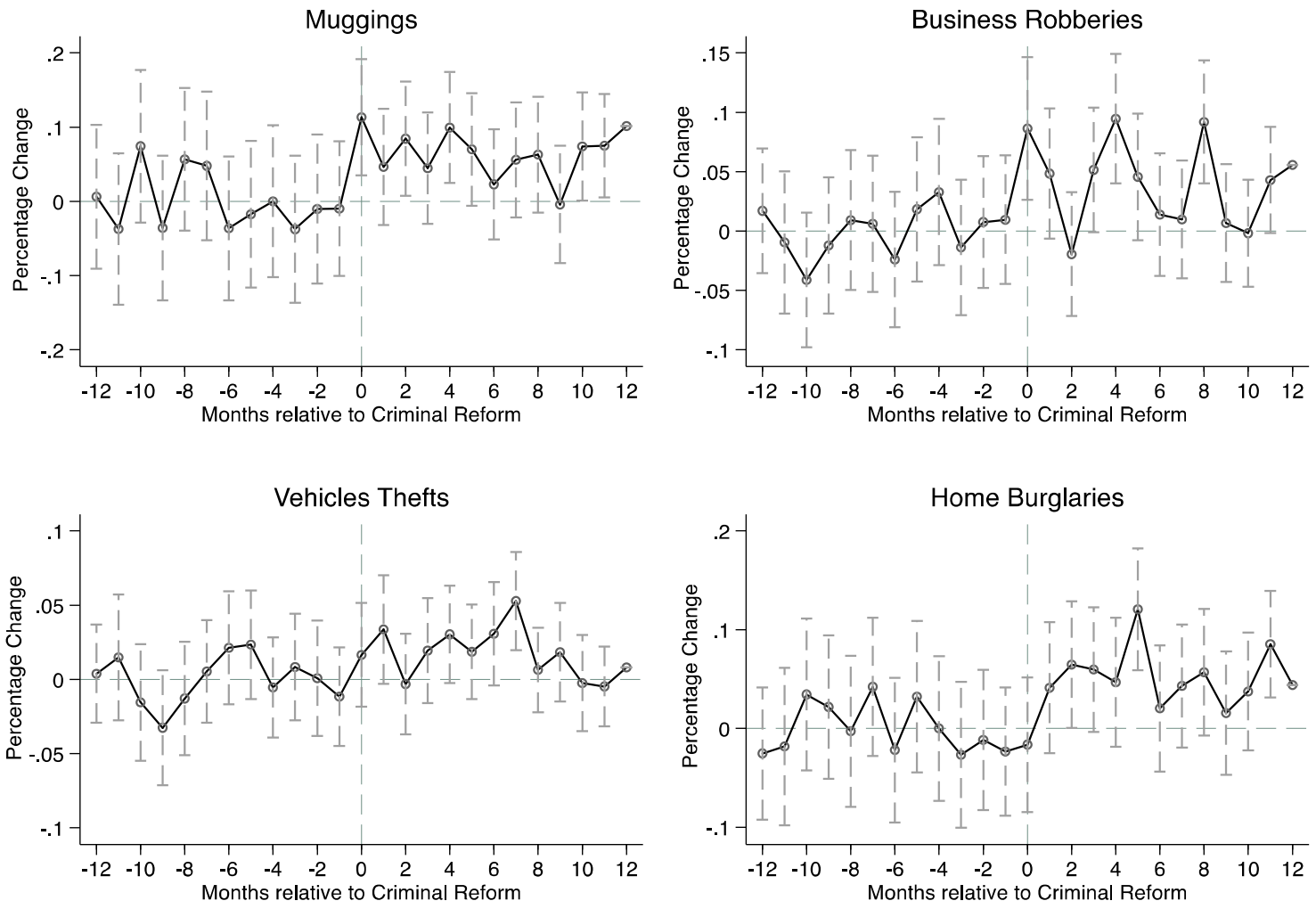


Figure 11. Leads-and-Lags Pre-Treatment Evaluation for Control Variables

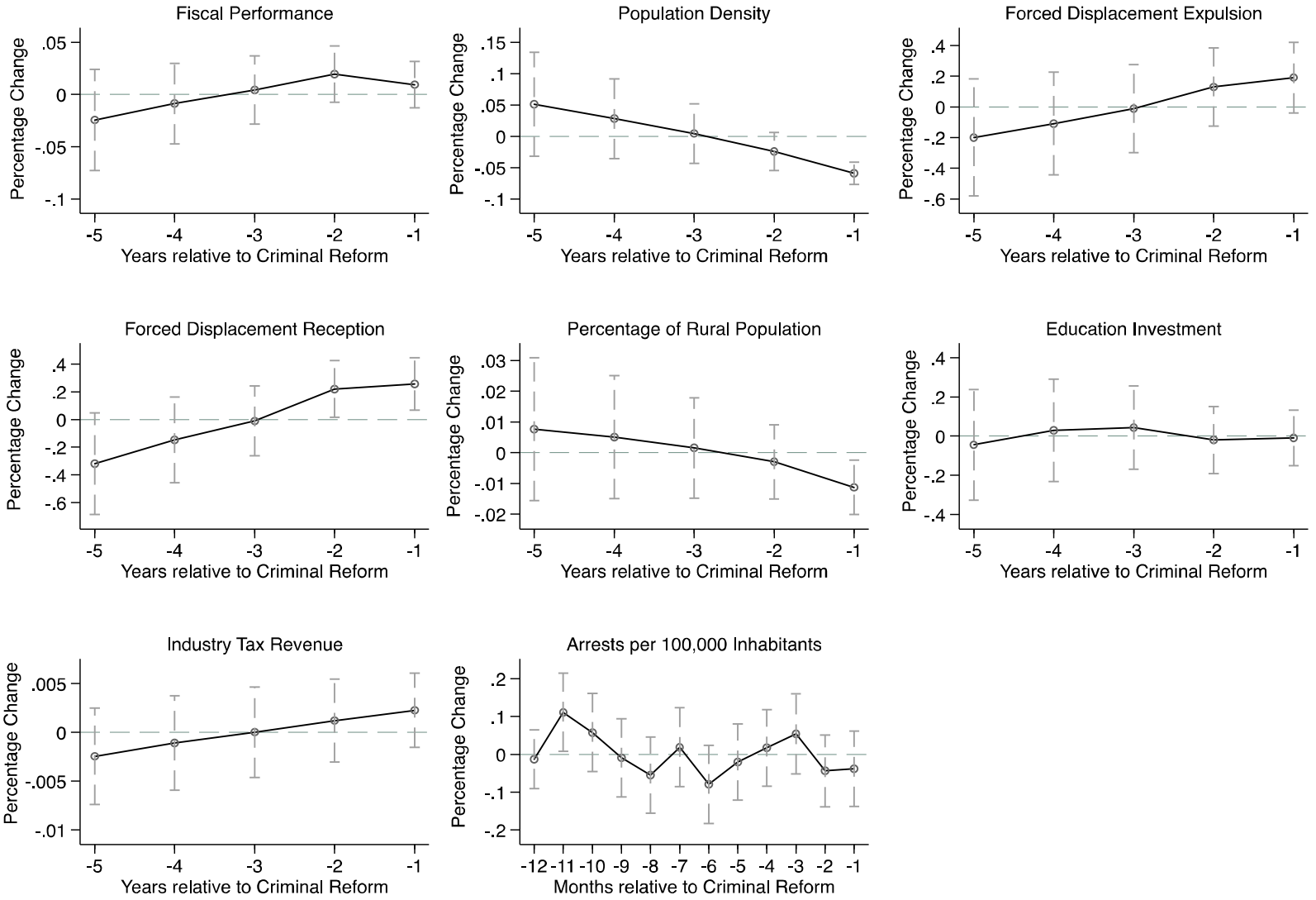


Table 1. Minimum and Maximum Sentence Length Colombian Penal Code

Crime	Min	Max	Average (p_s)
Homicides	13	40	27
Assaults	1	15	8
Muggings	1	16	9
Business Robberies	2	28	15
Home Burglaries	6	14	10
Vehicle Thefts	7	15	11
Total	30	128	80

Colombian Penal Code 2007, Articles 103, 111-116, 239-241.

Table 2. Descriptive Statistics Variables in Main Results

Variables	Observations	Mean	Std. Dev	Min	Max
Unweighted Crime*	78,894	14.99	23.58	0	834.33
Crime Index*	78,894	2.57	4.19	0	134.42
Violent Crime Index*	78,894	4.11	8.34	0	307.25
Property Crime Index*	78,894	1.37	3.03	0	93.73
Homicides*	78,894	3.76	9.76	0	374.90
Assaults*	78,894	5.29	12.57	0	834.33
Sexual Offenses*	62,977	4.49	8.44	0	232.29
Drug Offenses*	62,977	3.51	9.77	0	941.37
Muggings*	78,894	2.92	8.16	0	286.81
Business Robberies*	78,894	0.96	3.51	0	99.50
Vehicle Thefts*	78,894	0.43	2.30	0	183.53
Home Burglaries*	78,894	1.63	5.68	0	191.94
Arrests*	78,894	1.52	1.53	0	6.77
% Rural Population	78,894	1.05	0.65	0	4.09
Education Investment	77,660	3.55	0.94	0	12.34
Industry Tax Revenue	78,810	0.01	0.03	0	0.52
Fiscal Performance	77,790	4.08	0.16	2.52	4.51
Population Density	78,894	3.85	1.23	0.14	9.58
Forced Displacement Expulsion	78,918	3.83	2.11	0	9.63
Forced Displacement Reception	78,918	3.07	2.24	0	10.61

*Per 100,000 Inhabitants

Table 3. Conditional Event Study Results During the Pre-Treatment Period (2003-2004)

Months Before Implementation	(1) Unweighted Crime	(2) Crime Index	(3) Violent Crime	(4) Property Crime	(5) Homicides	(6) Assaults	(7) Sexual Offenses	(8) Drug Offenses	(9) Muggings	(10) Business Robberies	(11) Vehicles Thefts	(12) Home Burglaries
12 Months	0.045	0.039	0.068*	-0.022	0.075*	-0.069	-0.063*	-0.039	0.004	-0.002	-0.018	0.007
	(0.051)	(0.029)	(0.039)	(0.024)	(0.039)	(0.052)	(0.038)	(0.032)	(0.026)	(0.016)	(0.029)	(0.046)
11 Months	0.036	0.022	0.049	-0.002	0.007	0.089	0.016	-0.008	-0.032	0.011	0.025	0.065
	(0.066)	(0.037)	(0.049)	(0.028)	(0.052)	(0.066)	(0.046)	(0.037)	(0.032)	(0.022)	(0.037)	(0.058)
10 Months	-0.043	-0.041	-0.100**	0.041	-0.071	-0.052	0.009	0.103**	-0.032	-0.032	0.045	-0.073
	(0.068)	(0.038)	(0.050)	(0.029)	(0.051)	(0.062)	(0.047)	(0.040)	(0.031)	(0.023)	(0.036)	(0.058)
9 Months	-0.008	0.011	0.065	-0.039	0.050	0.056	-0.094*	-0.111***	0.023	-0.017	-0.009	0.059
	(0.066)	(0.037)	(0.050)	(0.029)	(0.051)	(0.063)	(0.053)	(0.040)	(0.028)	(0.021)	(0.035)	(0.056)
8 Months	0.079	0.055	0.027	0.040	0.028	0.000	0.039	0.110***	0.027	0.020	-0.020	0.004
	(0.066)	(0.037)	(0.050)	(0.027)	(0.051)	(0.063)	(0.047)	(0.039)	(0.027)	(0.020)	(0.034)	(0.057)
7 Months	-0.027	-0.028	-0.025	0.010	-0.036	-0.088	0.044	-0.020	-0.004	0.017	0.056*	0.022
	(0.062)	(0.034)	(0.048)	(0.026)	(0.049)	(0.067)	(0.049)	(0.038)	(0.027)	(0.021)	(0.033)	(0.054)
6 Months	-0.076	-0.025	-0.006	-0.057**	0.035	-0.021	-0.067	-0.077**	-0.026	0.020	-0.077**	-0.066
	(0.062)	(0.034)	(0.047)	(0.026)	(0.048)	(0.062)	(0.050)	(0.039)	(0.027)	(0.020)	(0.033)	(0.052)
5 Months	-0.018	-0.025	-0.066	0.045	-0.088*	0.013	-0.039	0.025	0.041	0.004	0.059*	0.016
	(0.066)	(0.036)	(0.048)	(0.028)	(0.049)	(0.062)	(0.049)	(0.041)	(0.028)	(0.021)	(0.033)	(0.053)
4 Months	0.015	0.014	0.006	-0.000	0.018	0.042	0.028	0.009	0.014	-0.033*	-0.039	-0.027
	(0.066)	(0.036)	(0.048)	(0.029)	(0.050)	(0.063)	(0.048)	(0.042)	(0.030)	(0.020)	(0.035)	(0.054)
3 Months	0.004	0.012	0.073	-0.054*	0.045	0.007	-0.029	-0.049	-0.051*	0.013	-0.026	0.039
	(0.063)	(0.035)	(0.047)	(0.028)	(0.048)	(0.065)	(0.048)	(0.041)	(0.029)	(0.020)	(0.035)	(0.054)
2 Months	-0.137**	-0.068**	-0.118**	0.016	-0.031	0.076	0.021	0.039	0.021	-0.007	0.012	-0.159***
	(0.062)	(0.035)	(0.047)	(0.027)	(0.049)	(0.064)	(0.047)	(0.039)	(0.029)	(0.021)	(0.033)	(0.053)
1 Month	0.093	0.035	0.048	0.012	0.036	-0.142**	-0.057	0.003	0.006	-0.007	-0.009	0.064
	(0.064)	(0.035)	(0.047)	(0.027)	(0.048)	(0.062)	(0.046)	(0.042)	(0.026)	(0.021)	(0.033)	(0.053)
Observations	75,976	75,976	75,976	75,976	75,976	60,882	60,882	75,976	75,976	75,976	75,976	75,976
R-squared	0.359	0.333	0.279	0.401	0.275	0.198	0.452	0.381	0.287	0.236	0.289	0.270
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest.
Robust cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Difference-in-Difference Results for Aggregated Measures

VARIABLES	Panel A				Panel B			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unweighted Crime	Crime Index	Violent Crime Index	Property Crime Index	Unweighted Crime	Crime Index	Violent Crime Index	Property Crime Index
LACPR	0.195***	0.110***	0.141***	0.081***	0.169***	0.097***	0.135***	0.062***
	(0.031)	(0.017)	(0.020)	(0.017)	(0.030)	(0.016)	(0.019)	(0.016)
Time Exposure LACPR					0.012***	0.006***	0.002*	0.009***
					(0.002)	(0.001)	(0.001)	(0.001)
Constant	-2.765*	-1.971**	-2.065**	-1.131	-4.306***	-2.734***	-2.381**	-2.233***
	(1.431)	(0.829)	(1.013)	(0.689)	(1.521)	(0.893)	(1.051)	(0.716)
Observations	75,976	75,976	75,976	75,976	75,976	75,976	75,976	75,976
R-squared	0.359	0.333	0.279	0.400	0.360	0.333	0.279	0.403
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T=0	13.52	2.45	4.1	1.16	13.52	2.45	4.1	1.16
Effect of LACPR	22%	12%	15%	8%	34%	18%	17%	18%

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. Exposure time effect of LACPR calculated using 12 months exposure. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Difference-in-Difference Results for Violent and Drug Crimes

VARIABLES	Panel A				Panel B			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Homicides	Assaults	Sexual Offenses	Drug Offenses	Homicides	Assaults	Sexual Offenses	Drug Offenses
LACPR	0.034*	0.221***	-0.007	-0.297***	0.035**	0.207***	-0.009	-0.318***
	(0.018)	(0.029)	(0.023)	(0.026)	(0.017)	(0.028)	(0.022)	(0.026)
Time Exposure LACPR					-0.000	0.006***	0.001	0.012***
					(0.001)	(0.002)	(0.001)	(0.002)
Constant	-2.557***	-0.366	1.869*	1.237	-2.493***	-1.168	1.740*	-0.358
	(0.946)	(1.194)	(1.000)	(1.542)	(0.964)	(1.249)	(1.012)	(1.757)
Observations	75,976	75,976	60,882	60,882	75,976	75,976	60,882	60,882
R-squared	0.274	0.269	0.197	0.450	0.274	0.270	0.197	0.452
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T=0	3.98	4.52	4.13	2.93	3.98	4.52	4.13	2.93
Effect of LACPR	3%	25%	0%	-26%	4%	30%	0%	-12%

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. Exposure time effect of LACPR calculated using 12 months exposure. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Difference-in-Difference Results for Property Crimes

VARIABLES	Panel A				Panel B			
	(1) Muggings	(2) Business Robberies	(3) Vehicles Thefts	(4) Home Burglaries	(5) Muggings	(6) Business Robberies	(7) Vehicles Thefts	(8) Home Burglaries
LACPR	0.083***	0.043***	0.016**	0.057***	0.054**	0.032**	0.018**	0.045**
	(0.025)	(0.014)	(0.008)	(0.019)	(0.024)	(0.014)	(0.008)	(0.018)
Time Exposure LACPR					0.013***	0.005***	-0.001*	0.005***
					(0.002)	(0.001)	(0.001)	(0.001)
Constant	-1.664*	-2.209***	1.241***	-1.973***	-3.372***	-2.844***	1.362***	-2.674***
	(0.989)	(0.621)	(0.359)	(0.684)	(1.067)	(0.697)	(0.388)	(0.708)
Observations	75,976	75,976	75,976	75,976	75,976	75,976	75,976	75,976
R-squared	0.381	0.287	0.235	0.289	0.383	0.288	0.235	0.290
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T=0	2.42	0.77	0.44	1.40	2.42	0.77	0.44	1.40
Effect of LACPR	9%	4%	2%	6%	22%	9%	1%	11%

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. Exposure time effect of LACPR calculated using 12 months exposure. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Difference-in-Difference Results for Clearance Rates

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Panel A					
LACPR	-4.087***	-3.187***	-11.133***	-9.592***	-5.879***
	(0.709)	(0.533)	(1.672)	(1.216)	(0.541)
Constant	47.777	91.322**	13.657	181.352**	60.042
	(46.582)	(37.415)	(121.221)	(80.524)	(43.029)
R-squared	0.387	0.484	0.416	0.431	0.603
Panel B					
LACPR	-4.069***	-3.164***	-10.883***	-9.580***	-5.886***
	(0.707)	(0.534)	(1.678)	(1.222)	(0.544)
Time Exposure LACPR	0.087	0.104**	0.347***	0.032	-0.035
	(0.058)	(0.045)	(0.132)	(0.086)	(0.051)
Constant	32.626	73.122**	-54.751	175.847**	66.105
	(47.613)	(36.102)	(119.325)	(83.018)	(44.247)
R-squared	0.388	0.486	0.417	0.431	0.603
Observations	31,760	32,064	26,267	31,169	32,035
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Mean T=0	17.21	13.50	70.74	39.03	21.94

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Difference-in-Difference Results for Pre-trial Detention

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Pre-trial Detention in Jail					
LACPR	-7.604***	-1.389*	-6.160***	-1.626	-0.996**
	(1.599)	(0.789)	(1.069)	(1.287)	(0.466)
Constant	82.727	35.572	-12.566	7.041	-2.876
	(95.869)	(50.015)	(78.294)	(76.739)	(28.874)
R-squared	0.127	0.106	0.149	0.111	0.109
Mean T=0	22.13	8.100	19.22	17.36	3.034
Pre-trial Detention in House Arrests					
LACPR	1.375***	0.938***	3.507***	1.868***	0.327**
	(0.359)	(0.266)	(0.481)	(0.408)	(0.157)
Constant	-10.166	19.811	-17.328	10.563	2.983
	(20.060)	(15.463)	(31.068)	(17.883)	(6.278)
R-squared	0.139	0.098	0.099	0.118	0.065
Mean T=0	0.156	0.187	1.335	0.671	0.0705
Total Pre-trial Detention					
LACPR	-6.229***	-0.451	-2.653**	0.242	-0.669
	(1.644)	(0.815)	(1.191)	(1.345)	(0.494)
Constant	72.561	55.383	-29.894	17.605	0.107
	(90.259)	(53.414)	(86.561)	(78.756)	(29.012)
R-squared	0.127	0.107	0.140	0.111	0.106
Mean T=0	22.28	8.287	20.55	18.03	3.105
Observations	11,200	14,857	14,004	12,098	17,554
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9. Difference-in-Difference Results for Settlements (Pre-Imputation)

VARIABLES	Panel A		Panel B	
	(1)	(2)	(3)	(4)
	Property Crimes	Assaults	Property Crimes	Assaults
LACPR	1.096***	10.445***	1.102***	10.555***
	(0.216)	(0.702)	(0.217)	(0.702)
Time Exposure LACPR			0.026*	0.516***
			(0.016)	(0.066)
Constant	2.728	122.513**	-1.822	32.007
	(14.045)	(54.729)	(14.391)	(57.720)
Observations	32,064	32,035	32,064	32,035
R-squared	0.403	0.591	0.403	0.605
Year Month & Month-Year FE	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Mean T=0	2.572	15.78	2.572	15.78

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. Exposure time effect of LACPR calculated using 12 months exposure. *** p<0.01, ** p<0.05, * p<0.1

Table 10. Difference-in-Difference Results for Total Acquittals and Convictions

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Total Acquittals					
Panel A					
LACPR	0.131**	0.010	0.580***	0.513***	0.055**
	(0.062)	(0.030)	(0.162)	(0.186)	(0.025)
Constant	0.358	5.498*	-5.461	-3.034	2.313
	(2.942)	(3.186)	(9.426)	(11.335)	(1.854)
R-squared	0.362	0.371	0.466	0.398	0.222
Panel B					
LACPR	0.135**	0.012	0.595***	0.533***	0.056**
	(0.062)	(0.030)	(0.160)	(0.184)	(0.025)
Time Exposure LACPR	0.017***	0.007**	0.021**	0.058***	0.008***
	(0.005)	(0.003)	(0.008)	(0.014)	(0.002)
Constant	-2.627	4.215	-9.511	-13.059	0.949
	(2.976)	(2.870)	(9.847)	(11.289)	(1.978)
R-squared					
Mean T=0	0.0305	0.00943	0.0363	0.0257	0.0199
Observations	31,760	32,064	26,267	31,169	32,035
Total Convictions					
Panel A					
LACPR	0.287***	0.110*	0.139*	0.166	0.125
	(0.081)	(0.058)	(0.081)	(0.112)	(0.081)
Constant	-1.682	-0.782	0.396	3.470	-3.740
	(8.421)	(5.417)	(7.305)	(8.049)	(6.501)
R-squared					
Panel B					
LACPR	0.271***	0.088*	0.166*	0.147	0.139
	(0.081)	(0.045)	(0.084)	(0.114)	(0.089)
Time Exposure LACPR	-0.003	-0.004	0.005	-0.003	0.004
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Constant	-0.559	0.958	-0.747	3.791	-4.824
	(8.249)	(5.171)	(7.160)	(8.078)	(5.944)
R-squared	0.472	0.391	0.525	0.438	0.471
Mean T=0	0.130	0.0934	0.0737	0.101	0.138
Observations	1,068	1,314	1,002	1,431	947
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11. Difference-in-Difference Results for Acquittals and Convictions in Court (Trial)

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Acquittals in Court					
Panel A					
LACPR	1.418***	1.011***	9.923***	2.978***	0.775***
	(0.208)	(0.151)	(0.855)	(0.435)	(0.119)
Constant	3.253	33.051***	182.907***	74.526***	23.305***
	(15.711)	(8.273)	(70.551)	(26.526)	(7.972)
R-squared	0.397	0.424	0.542	0.388	0.320
Panel B					
LACPR	1.445***	1.029***	10.457***	3.063***	0.782***
	(0.207)	(0.154)	(0.873)	(0.436)	(0.120)
Time Exposure LACPR	0.127***	0.081***	0.743***	0.242***	0.033***
	(0.017)	(0.010)	(0.059)	(0.031)	(0.009)
Constant	-19.001	18.940**	36.474	32.388	17.596**
	(16.089)	(7.765)	(71.087)	(25.045)	(8.059)
R-squared	0.407	0.442	0.569	0.403	0.323
Mean T=0	0.163	0.0925	0.429	0.419	0.109
Observations	31,760	32,064	26,267	31,169	32,035
Convictions in Court					
Panel A					
LACPR	-0.287***	-0.110*	-0.139*	-0.166	-0.125
	(0.081)	(0.058)	(0.081)	(0.112)	(0.081)
Constant	2.682	1.782	0.604	-2.470	4.740
	(8.421)	(5.417)	(7.305)	(8.049)	(6.501)
R-squared	0.472	0.391	0.525	0.438	0.471
Panel B					
LACPR	-0.271***	-0.088*	-0.166*	-0.147	-0.139
	(0.081)	(0.045)	(0.084)	(0.114)	(0.089)
Time Exposure LACPR	0.003	0.004	-0.005	0.003	-0.004
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Constant	1.559	0.042	1.747	-2.791	5.824
	(8.249)	(5.171)	(7.160)	(8.078)	(5.944)
R-squared	0.472	0.393	0.526	0.438	0.471
Mean T=0	0.870	0.907	0.926	0.899	0.862
Observations	1,068	1,314	1,002	1,431	947
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and police arrest. Robust cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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METHODOLOGICAL APPENDIX

Robustness & Placebo Tests

We estimated two falsification tests. Our first test consisted of evaluating our model during the pre-treatment period following the Colombian implementation order. As mentioned in the paper, the lag-structure used in the difference-in-difference model does not allow us to use the pre-treatment period directly since $Z_{i,t-1}$ is not comparable to $Z_{i,t-2}$. Thus, we estimated all our results excluding $Z_{i,t-w}$ confirming that these findings are robust to these changes. After checking these, we proceeded to evaluate our placebo test. Table 1A shows the main results excluding $Z_{i,t-w}$ and the respective calculation for the placebo treatment.

In the second falsification test, we randomly assigned all Colombian municipalities to different waves and estimated if our results come from chance. After 100-estimations, we do not find a consistent pattern indicating spurious results. Table 2A contains the average values of the 100 estimations.

Table 1A. Robustness Check without $Z_{i,t-w}$ and Falsification 2003-2004

	(1) Unweighted Crime	(2) Crime Index	(3) Violent Crime	(4) Property Crime	(5) Homicides	(6) Assaults
Panel A:						
LACPR	0.311***	0.162***	0.190***	0.119***	0.051***	0.301***
	(0.031)	(0.017)	(0.020)	(0.017)	(0.018)	(0.029)
Observations	77,094	77,094	77,094	77,094	77,094	77,094
R-square	0.431	0.379	0.302	0.441	0.276	0.320
Panel B:						
LACPR	0.280***	0.146***	0.183***	0.098***	0.051***	0.284***
	(0.030)	(0.016)	(0.019)	(0.016)	(0.017)	(0.028)
Exposure LACPR	0.013***	0.007***	0.003**	0.009***	-0.000	0.007***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	-5.147***	-3.096***	-2.913***	-2.372***	-2.647***	-2.085*
R-square	(1.484)	(0.884)	(1.044)	(0.697)	(0.960)	(1.234)
	77,094	77,094	77,094	77,094	77,094	77,094
	0.432	0.380	0.303	0.443	0.276	0.321
Panel A:						
Placebo	-0.127***	-0.078***	-0.127***	-0.003	-0.084***	-0.119***
	(0.031)	(0.019)	(0.026)	(0.014)	(0.027)	(0.028)
Observations	25,689	25,689	25,689	25,689	25,689	25,689
R-square	0.502	0.438	0.356	0.532	0.322	0.372
Panel B:						
Placebo	-0.122***	-0.074***	-0.124***	-0.002	-0.073***	-0.129***
	(0.031)	(0.019)	(0.026)	(0.013)	(0.028)	(0.028)
Exposure Placebo	0.003	0.003	0.002	0.001	0.007*	-0.007*
	(0.004)	(0.003)	(0.004)	-0.002	(0.004)	(0.004)
Observations	25,689	25,689	25,689	25,689	25,689	25,689
R-square	0.502	0.438	0.356	0.532	0.322	0.372
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index and displaced population. Robust cluster standard errors in parentheses. Exposure time of LACPR calculated using 12 months of exposure. *** p<0.01, ** p<0.05, * p<0.1

Table 1A. Robustness Check without $Z_{i,t-w}$ and Falsification 2003-2004 (continues)

	(7) Sexual Offenses	(8) Drug Offenses	(9) Muggings	(10) Business Robberies	(11) Vehicles Thefts	(12) Home Burglaries
Panel A:						
LACPR	-0.013	-0.295***	0.122***	0.063***	0.019**	0.082***
	(0.023)	(0.026)	(0.025)	(0.014)	(0.008)	(0.019)
Observations	61,671	61,671	77,094	77,094	77,094	77,094
R-square	0.197	0.450	0.404	0.300	0.236	0.304
Panel B:						
LACPR	-0.015	-0.318***	0.090***	0.051***	0.021***	0.069***
	(0.022)	(0.026)	(0.024)	(0.014)	(0.008)	(0.018)
Exposure LACPR	0.001	0.013***	0.014***	0.005***	-0.001*	0.006***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	1.786*	-0.125	-3.417***	-2.906***	1.408***	-2.852***
R-square	(1.002)	(1.776)	(1.051)	(0.707)	(0.387)	(0.707)
	61,671	61,671	77,094	77,094	77,094	77,094
	0.197	0.452	0.407	0.301	0.236	0.305
Panel A:						
Placebo	0.047	0.016	0.018	0.001	-0.026**	0.002
	(0.030)	(0.024)	(0.018)	(0.012)	(0.012)	(0.017)
Observations	19,146	19,146	25,689	25,689	25,689	25,689
R-square	0.286	0.509	0.480	0.351	0.278	0.380
Panel B:						
Placebo	0.041	0.035	0.023	-0.005	-0.028**	0.006
	(0.030)	(0.023)	(0.018)	(0.012)	(0.012)	(0.016)
Exposure Placebo	-0.003	0.011***	0.004	-0.004**	-0.002	0.003
	(0.004)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)
Observations	19,146	19,146	25,689	25,689	25,689	25,689
R-square	0.286	0.509	0.480	0.351	0.278	0.380
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index and displaced population. Robust cluster standard errors in parentheses. Exposure time of LACPR calculated using 12 months of exposure. *** p<0.01, ** p<0.05, * p<0.1

Table 2A. Randomization Falsification Test-Average Calculations 100-Estimations

VARIABLE	(1) Crime Index	(2) Violent Crime	(3) Property Crime	(4) Homicides	(5) Assaults	(6) Muggings	(7) Business Robberies	(8) Vehicles Thefts	(9) Home Burglaries
Panel A:									
Placebo	-0.027	-0.049	0	-0.024	-0.053	0.036	-0.007	-0.023	0.013
	(0.047)	(0.06)	(0.051)	(0.036)	(0.083)	(0.067)	(0.029)	(0.043)	(0.019)
Observations	76,189	76,189	76,189	76,189	76,189	76,189	76,189	76,189	76,189
R-square	0.331	0.277	0.399	0.274	0.267	0.381	0.286	0.289	0.235
Panel B:									
Placebo	-0.024	-0.048	0.003	-0.024	-0.053	0.037	-0.003	-0.02	0.009
	(0.045)	(0.058)	(0.05)	(0.034)	(0.079)	(0.066)	(0.029)	(0.042)	(0.017)
Exposure Placebo	-0.002	-0.001	-0.002	0	0	-0.001	-0.003	-0.002	0.003***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.006)	(0.004)	(0.002)	(0.003)	(0.001)
Observations	76,189	76,189	76,189	76,189	76,189	76,189	76,189	76,189	76,189
R-square	0.331	0.277	0.4	0.274	0.267	0.381	0.287	0.289	0.236
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES

Controls: Per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, and displaced population. Robust cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1