

# How Police and Crime Respond to Courts: Evidence from Colombia

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November 24, 2025<sup>§</sup>

## Abstract

How does reform of criminal prosecution and courts affect policing and crime? We study the introduction of a new code of criminal procedure in Colombia. Though the new code primarily targeted prosecutors and judges, we find that police behavior changed in response: the arrest rate dropped 45%, and arrests for minor offenses declined even more. But crime did not increase, according to administrative and survey data, and perceptions of public safety improved. We attribute this outcome to an improvement in court accuracy. Our findings underscore the value of considering strategic interaction among the police, prosecutors, and judges.

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<sup>§</sup>Special thanks to Avi Feller and Gonzalo Vazquez-Bare for extensive guidance. For comments, we thank Guy Grossman, Roman Rivera, Carolina Torreblanca, Jessie Trudeau, workshop participants at AL CAPONE, ESOC, the Penn Conference on Policing in Comparative Perspective, the USC Conference on New Directions in the Political Economy of Development, the U.C. Berkeley Methods Workshop, Tulane University, Duke University, and the Center for International Security and Cooperation at Stanford.



Scholars have long understood that police respond to other criminal justice institutions. [Wilson \(1968\)](#), for example, noted that police officers arrest people they expect prosecutors to indict (84) and judges to convict (137). [Chevigny \(1995\)](#), studying criminal justice in Latin America, observed that police officers used extralegal violence when they were impatient with courts (143, 181; see also [Brinks, 2007](#)); [Skolnick and Fyfe \(1993, 24\)](#), similarly, wrote that police in the United States use excessive force when they deem courts “too ponderous, too indolent, too unaware, or too constrained to deal with ‘the problem.’”

This work implies that reform targeting prosecutors and the courts could reshape police behavior, with consequences for crime. But we lack theory and evidence. Recent empirical research on policing focuses on policies internal to police forces (e.g. [Mummolo, 2018b](#); [Owens et al., 2018](#); [Rivera, 2025b](#); [Dube et al., 2025](#)) or on court orders that impose direct constraints on police agencies (e.g. [Trudeau, 2022](#); [Rivera and Ba, 2023](#); [Hausman and Kronick, 2023](#)). And recent work on prosecutors and courts considers effects on crime (e.g. [Ouss and Stevenson, 2023](#); [Shem-Tov et al., 2024](#)), but seldom studies police response ([Stashko and Garro, 2021](#); [Amaral et al., 2025](#), being exceptions).

How does reform of criminal prosecution and courts affect policing and crime? We use the staged rollout of a new code of criminal procedure in Colombia to study the strategic responses of police and citizens, and the consequences for public safety. Though the new code targeted the public prosecutor’s office and the courts, we find that police responses to these changes reshaped law enforcement in Colombia, with unexpected effects on crime.

Colombia’s new code of criminal procedure replaced a primarily inquisitorial system, in which judges (the eponymous inquisitors) play a large role in investigation and gathering evidence, with a primarily accusatorial system, in which prosecutors direct investigation and judges adjudicate. The new code changed the day-to-day operations of prosecutors and judges in many ways, including how they handled arrests. Under the old code, public prosecutors could issue arrest warrants and sign off on arrests made without warrants; the new code transferred these powers to judges. From the perspective of police, this shift meant that each arrest took more time (because judges asked more questions) and faced more scrutiny (because judges applied stricter procedural standards). The police and prosecutors no longer “had to convince themselves, but had to convince a judge,” as one



prominent criminal defense attorney put it (quoted in [Hartmann Arboleda et al., 2009](#), 80).

We propose a theory of how such changes affect policing and crime. In our model, reform of prosecution and courts boils down to an increase in *accuracy*: the new requirements made prosecutors and judges less likely to punish innocent suspects and more likely to punish guilty ones. All else equal, prosecutorial and judicial accuracy would strengthen deterrence and therefore reduce crime. But increasing the accuracy of the court also imposes costs on the police. In order to enable prosecutors and judges to make better decisions, officers must fill out additional paperwork, invest in preliminary investigations, or (as in our case) attend hearings with judges. These costs constitute a ubiquitous but seldom-studied consequence of reform that targets courts, and they tend to depress police effort.<sup>1</sup> That’s why the effect of reform on crime is ambiguous: court accuracy itself strengthens people’s incentives to obey the law, but the consequent decline in police activity has a countervailing effect. The same goes for the effect of reform on the arrest rate, which is jointly determined by police effort and the supply of offenses. Reform can lower the arrest rate by dampening police effort and shrinking the supply of offenses, but it can also increase the arrest rate if de-policing sparks a crime wave.

The sign of the effect of reform on arrest rates and crime, then, depends on the magnitude of the increase in accuracy relative to the costs imposed on police. Reform that substantially improves court accuracy with minimal costs for police tends to reduce crime even though it also lowers the arrest rate. More modest improvements in court accuracy, especially when they entail larger costs for police, decimate arrests and therefore cause crime. This result is not counterintuitive but does run counter to the inclination of activists who value constraints on police for their own sake.

One empirical implication of our model is that a reform targeting judicial and prosecutorial accuracy should also affect the arrest rate. We evaluate this prediction in our case, finding that officers responded to the new code of criminal procedure by making fewer arrests: the arrest rate dropped more than 45% when the new code came into effect. The decline was immediate, occurring within one month of the implementation date, and geographically widespread: arrest rates

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<sup>1</sup>If police officers value the quality of service outputs, i.e., punishment of the guilty, it is possible that a more accurate court could also increase police effort, a point that we develop below.



dropped sharply in each of the four waves of implementation, and declined in 78% of Colombia’s more than 1,000 municipalities. It is perhaps unsurprising that raising the costs (to police) of arrests would result in fewer arrests. But this fact was not documented before our work.<sup>2</sup> Because the new code primarily targeted prosecutors and the courts, effects on police were overlooked.

All else equal, we might expect this large drop in the arrest rate to increase crime. Indeed, studies on the police elasticity of crime generally estimate that a 1% decline in personnel causes a  $\approx 0.7\%$  increase in the homicide rate and a  $0.3\%$ – $1\%$  increase in assault (Chalfin and McCrary, 2018a, 179–180), meaning that it would be unsurprising if a 45% drop in the arrest rate were to generate a 30% increase in crime.<sup>3</sup>

But our theory proposes a mechanism through which a drop in the arrest rate does not increase crime: when it occurs as the result of an increase in prosecutorial and judicial accuracy. In Colombia, the new code of criminal procedure sought to improve the court’s ability to accurately distinguish innocence from guilt (Hartmann Arboleda et al., 2009); indeed, the new demands on police officers were the direct consequence of this effort. Improved accuracy itself typically deters crime.

Perhaps as a result, in Colombia, crime did not rise nearly as much as the literature might lead us to expect. Using data both from vital statistics and from the police, we find no evidence that Colombia’s new code of criminal procedure affected homicide rates or assault rates. And using survey data from the Latin American Public Opinion Project, we estimate that the new code of criminal procedure improved perceptions of neighborhood security and did not clearly affect self-reported victimization one way or the other.<sup>4</sup>

A third implication of our theory is that reform targeting prosecutors and judges can affect not only the number but the composition of arrests. For police, the new

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<sup>2</sup>For example, the thorough treatment in Hartmann Arboleda et al. (2009) does not discuss consequences for police behavior. The first quantitative study of Colombia’s new code of criminal procedure, Acosta Mejía et al. (2016), found large effects on crime and procedural outcomes but not on arrest. A subsequent draft, Zorro Medina et al. (2020), replicated our arrest result, citing a previous version of this article.

<sup>3</sup>There are fewer studies on the *arrest* elasticity of crime; we discuss this literature below.

<sup>4</sup>These results contrast with those of Acosta Mejía et al. (2023), who estimate large effects of the Colombia’s new code of criminal procedure on crime rates and conclude that there is a conflict “between due process protection and public safety;” we attribute the difference to our correction for coincident changes in crime registration. See Appendix B.



costs of reform fall especially on unjustified or under-justified arrests: those that previously escaped scrutiny. Well-founded arrests, in contrast, required significant effort even in the pre-reform period, meaning that reform entails a smaller change in police effort. In Colombia, we find that the *composition* of arrests changed dramatically as a result of the new code of criminal procedure. Arrest rates dropped much more for minor offenses like vandalism (90%) or drugs (60%) than for serious crimes; homicide arrests, for example, did not decline at all. Relatedly, arrests with warrants declined much less (20%) than arrests without warrants (50%). The ratio of convictions to arrests rose. The ratio of drug convictions to drug arrests, for example, nearly doubled.

We attribute these changes in the composition of arrests to the fact that, as our theory predicts, the reform had more bite for minor offenses than for serious ones, and more bite for unjustified than for justified arrests. Whereas homicide arrests required significant officer time and were subjected to meaningful scrutiny even under the old code of criminal procedure, arrests for low-level offenses like vandalism had required little investigation and faced minimal pushback from prosecutors. Under the new code, in contrast, even low-level arrests required officers to attend hearings with judges who often questioned and sometimes rejected the validity of the arrest. This shift in composition may also help explain why crime appears not to have risen as much as the literature would lead us to expect.

Our work is closely related to recent empirical studies on criminal procedure reform in Latin America. Like Colombia, nearly every other country in the region replaced inquisitorial with accusatorial criminal procedure, a shift that [Langer \(2007\)](#) calls the “revolution in Latin American criminal procedure.” Legal scholars, along with the international financial institutions that backed these reforms in the first place, have produced scores of qualitative studies on the consequences (e.g. [Hartmann Arboleda et al., 2009](#); [Alguíndigue and Pérez-Perdomo, 2008](#); [Alguíndigue and Pérez-Perdomo, 2013](#); [Duce and Perdomo, 2003](#)). Among the few quantitative empirical studies, [Magaloni and Rodriguez \(2020\)](#) find that Mexico’s new code of criminal procedure reduced the use of torture, and [Tiede \(2012\)](#) documents a reduction in the use of pre-trial detention in Chile. [Cattaneo et al. \(2022a\)](#) find that Uruguay’s new code caused property crime. Ours is the first of these papers to focus on police officers’ strategic response to changes in the public



prosecutor’s office and the courts.<sup>5</sup>

Our findings also contribute to recent empirical literature on the determinants of police behavior. Many recent papers consider the effect of internal policy changes and/or external mandates on police behavior (Mummolo, 2018b; Trudeau, 2022; Rivera and Ba, 2023; Hausman and Kronick, 2023; González, 2020, 270–296), and Magaloni et al. (2020) find that the introduction of “pacifying police units” in Rio reduced killings by police. Other work considers the role of officer characteristics such as race and gender (Ba et al., 2021), tendency for aggression (Rivera, 2025a), or political affiliation (Ba et al., 2023). With the exception of Stashko and Garro (2021), who study the effect of prosecutor elections on police behavior in the United States, none of this literature focuses on how police officers respond strategically to reform targeting other agents of the criminal justice system.

We also contribute to a vast literature on how the criminal justice system affects crime rates. Many studies estimate the elasticity of crime to police hiring and/or police presence (e.g. Chalfin and McCrary, 2018b, 2017; McCrary, 2002; Di Tella and Schargrodsky, 2004). Other work considers the effects of incarceration and decarceration (e.g. Buonanno and Raphael, 2013; Barbarino and Mastrobuoni, 2014; Ouss, 2020). A third group of studies focuses on police activity and tactics, such as hotspots policing (Braga et al., 2019; Collazos et al., 2021), police militarization (Mummolo, 2018a; Blair and Weintraub, 2020), or arrest activity (Cho et al., 2023; Rivera, 2024). Much of this literature suggests, contrary to earlier work on broken windows (e.g. Kelling and Coles, 1997), that governments can pull back on policing of minor offenses without harming public safety (though see Cassell and Fowles, 2018). Our findings are consistent with this conclusion. But unlike previous work, we provide evidence from outside the United States, from the staged rollout of a major policy change, and informed by theory about how police interact with other criminal justice institutions.

More generally, our findings relate to literature on incentives and behavior in public bureaucracies. We clarify how policing responds to changes in the downstream activities of prosecutors and courts, as well as how policing responds to crime. Both responses—to other public institutions, and to society—are absent in most standard accounts of bureaucratic oversight. Even though Colombia’s new code

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<sup>5</sup>Hanson and Kronick (2024) do focus on harmful police backlash to the new code of criminal procedure in Venezuela, though in that case the constraints on police were more direct.



of criminal procedure had no direct effect on police wages, selection of officers, the power of police supervisors, or politician oversight, we identify large changes in police behavior. These findings reveal the value of focusing on interaction among public-sector institutions as well as interaction between bureaucrats and citizens ([Grossman and Slough, 2022](#)). We show that these strategic interactions produce public-sector outputs (arrests) and societal outcomes (crime).

## 1 Theory: Courts and cops

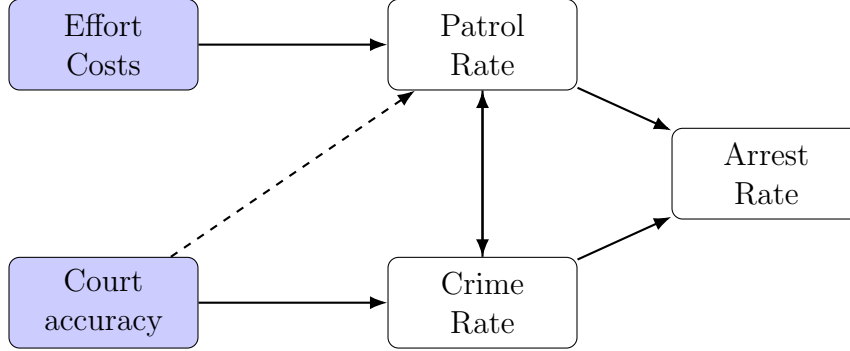
We propose a simple model for thinking about how policing and crime respond to reform of criminal prosecution and courts (formalized in [Appendix A](#)). People choose whether to work or commit crime, and the relative gains from crime vary across individuals. As in canonical theories ([Becker, 1968](#)), people who commit crimes incur a risk of arrest and, if arrested, a risk of conviction, in which case they pay a penalty. But critically, we also allow for police and courts to make mistakes: even those who do not commit crimes can be arrested and convicted.

Police choose how much to patrol. Patrol creates opportunities for making arrests but also requires costly effort. Officers benefit from arrests in two ways: because there are extrinsic career benefits of making arrests, and because they intrinsically value seeing guilty people punished. We assume that the extrinsic benefits do not depend on the true guilt or innocence of the suspect, whereas the intrinsic benefits accrue only if the suspect is actually guilty and only if the courts actually punish him; in that sense, the intrinsic benefit is analogous to public service motivation (e.g., [Francois, 2000](#); [Dal Bó et al., 2013](#); [Ash and MacLeod, 2015](#)). The model accommodates any relative weight on extrinsic vs. intrinsic benefits and treats that weight as exogenous.

We model reform as a costly change in the quality of criminal prosecution and adjudication. Conditional on arrest, reform increases the probability of punishment of offenders and lowers the probability of punishment of nonoffenders who were wrongly arrested. In other words, prosecutors and judges make fewer mistakes. We refer to this change as an increase in the *accuracy* of the court. This approach is inspired by our case, in which a new code of criminal procedure both instituted new protections for suspects and defendants (in order to prevent punishment of nonoffenders) and facilitated investigation and evidence gathering (in order to raise the probability of punishment of offenders) ([Hartmann Arboleda](#)



Figure 1: How reform affects patrol, arrests, and crime. The shaded nodes represent the two components of reform, in our model. The white nodes are equilibrium outcomes. Court accuracy affects the patrol rate directly if and only if police intrinsically value punishing the guilty (i.e., have non-zero public service motivation), hence the dashed line.



et al., 2009). In fact, one of the reasons that the new code faced minimal opposition was that advocates focused on human rights saw it as *garantista*, or protective of rights, while those focused on crime control saw it as protective of public safety (Zorro Medina, 2020).

Improving the quality of prosecutorial and judicial decisions imposes direct effort costs on police. Officers must fill out additional paperwork, or (as in our case) attend hearings, or conduct more preliminary investigation before requesting a warrant or making an arrest *in flagrancia* (without a warrant). These costs should depress police effort, following standard arguments. That high-quality prosecutorial and judicial decision-making requires additional effort from police is a very general but under-recognized reality of criminal justice reform.<sup>6</sup>

For a given level of prosecutorial and judicial quality and the resulting cost of police effort, officers choose how much to patrol and people choose whether to commit crimes. The *patrol rate*, the *arrest rate*, and the *crime rate* emerge as equilibrium outcomes. Figure 1 summarizes how reform affects these outcomes. We discuss each in turn.

**Patrol rates:** The reform affects the patrol rate in three ways. First, higher effort costs depress patrol effort, all else equal. Second, prosecutorial and judicial

<sup>6</sup>This feature of our model echoes arguments that oversight creates paperwork burdens, depressing the effort of bureaucrats (e.g., Wang, 2022).



accuracy directly deter crime, which weakens officers' incentives to patrol (because patrolling yields fewer arrests). (Of course, the patrol rate and the crime rate are determined in equilibrium, hence the bi-directional arrow in Figure 1). Third, accuracy raises officers' intrinsic reward for making arrests: an officer motivated by the prospect of punishing the guilty will gain more from each arrest when he knows that prosecutors are unlikely to bring charges against wrongfully arrested suspects or dismiss guilty ones, and that judges or juries are similarly unlikely to err. These higher rewards raise officers' optimal patrol rate. Whether the third effect dominates the first two is ambiguous ex-ante.

**Crime rates:** The effect of accuracy on crime is similarly ambiguous. Accurate prosecution and adjudication directly lowers the crime rate by increasing the probability of punishment for breaking the law and decreasing the probability of wrongful punishment of law-abiding citizens. Accuracy can also lower crime indirectly if it increases the patrol rate. But if the new costs of police effort depress the patrol rate sufficiently relative to the gains in accurate punishment, the reform can increase crime. Which of these effects dominates depends on the details of the reform. Reform that increases prosecutorial and judicial accuracy significantly while imposing minimal costs on police will lower crime; reform that increases accuracy only moderately while imposing larger costs on police will result in more crime.

**Arrest rates:** In our model, arrest is a function of the crime rate and the patrol rate. As more citizens turn to crime, arrests increase. As patrol increases, so does arrest. The ambiguous effects of the reform on crime and patrol suggest ambiguous effects on the arrest rate, too. But over much of the parameter space, higher court accuracy depresses the equilibrium arrest rate by making patrol more costly for officers and directly deterring crime.<sup>7</sup>

The effect of the reform on patrol, arrest, and crime depend substantially on the extent of changes in the cost of effort and in the accuracy of the court. Where these changes are small—e.g., because the court was already imposing greater scrutiny before the reform—we should see smaller changes in these outcomes. Where these changes are larger, we should see larger changes in outcomes. This implies that changes in the quality of court decisions affects not just the number

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<sup>7</sup>This is not a probabilistic statement as we have not made assumptions about which parameter values are more likely.



but the composition of arrests and crimes. For police, the new costs of reform fall especially on unjustified or under-justified arrests: those that previously escaped scrutiny. Well-founded arrests, in contrast, required significant effort even in the pre-reform period, meaning that reform entails a smaller change in the cost (to police) of arrests of people who have committed serious crimes for which warrants were more standard prior to reform.

In what follows, we investigate three empirical implications of our model: that reform of criminal prosecution and courts can affect the arrest rate, that it can affect the composition of arrests, and that it can affect the crime rate.

## 2 Police and criminal procedure in Colombia

**Policing in Colombia.** In the mid-1990s, ten years prior to the reform that we study, far-reaching legislation transformed the Colombian national police. What had been a corrupt, brutal, militarized, under-educated and under-trained police force embroiled in an extraordinarily violent conflict with drug cartels became a less corrupt, less violent, partially demilitarized, better-educated and better-trained agency newly focused on citizen security ([González, 2019](#)). It remained deeply flawed ([Llorente, 2005](#)). But, by the 2000s, the Colombian national police—the only major police force in the country—had experienced “an unexpected shift toward ‘democratic policing’ ” ([Moncada, 2009](#)). Even as Álvaro Uribe sent the Colombian military (and specific police units) out to fight the guerrilla, producing both a spectacular decline in lethal violence (Figure 2) and also grave human rights violations,<sup>8</sup> the national police reoriented activities around certain principles of community policing ([García et al., 2013](#)). It is revealing that domestic criticism of this program has focused on excessive centralization ([Vásquez, 2012](#)) and ineffectiveness, rather than the persistence of widespread violence or abuse.

This is all to say that, by the time of the reform that we study, the Colombian national police was “a professionalized and well-regarded police force” ([Blattman et al., 2021](#)), plausibly prepared to assimilate changes mandating new protections for suspects.

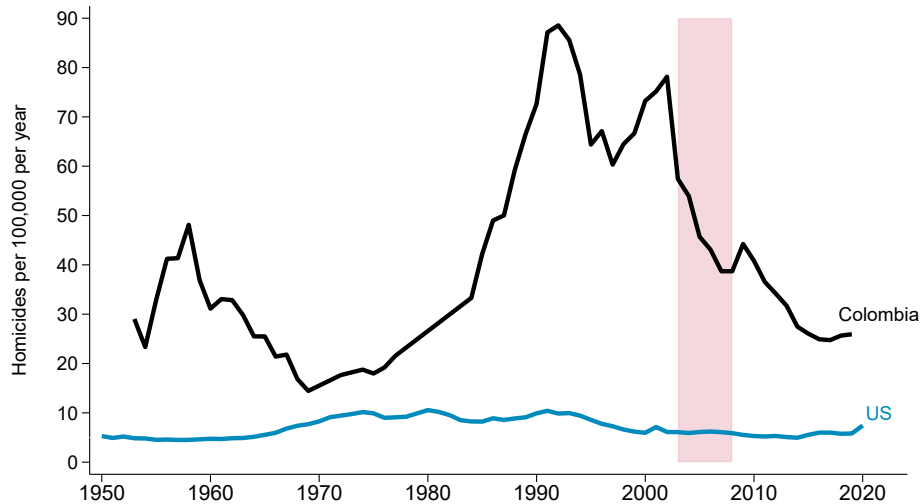
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<sup>8</sup>Most notoriously, a written policy of bonuses for military officers who killed members of the guerrilla resulted in thousands of “false positives:” innocent civilians killed and dressed up as FARC ([Acemoglu et al., 2020](#)).



Figure 2: Lethal violence in Colombia and the United States

The red shaded region marks 2005–2008, the rollout period of the Colombia’s new code of criminal procedure.



**A new code of criminal procedure.** In 2004, the second year of Uribe’s first term as president, the Colombian Congress passed legislation (Ley 906) mandating the adoption of a new code of criminal procedure. Whereas the penal code defines crimes and penalties, criminal procedure defines the rules according to which criminal cases proceed through the justice system. An international network of legal activists, with support from international financial institutions, promoted this change throughout Latin America in pursuit of due process, transparency, and efficiency ([Langer, 2007](#)); Colombia’s new code of criminal procedure was a product of this process of international diffusion, not a response to a domestic scandal about police misconduct or court malfunction.

The new code replaced what was primarily an inquisitorial system (typical of civil law) with an accusatorial system (typical of common law), changing criminal procedure in many ways. The core distinction between inquisitorial and accusatorial systems lies in the role of the judge. Under Colombia’s old code, judges participated in the process of investigation and also rendered decisions; the new code removed the judge from the process of gathering evidence, empowering public prosecutors to manage investigations ([Hartmann Arboleda et al., 2009](#)). A second major shift was the replacement of written proceedings with oral proceedings. Under the old code, cases advanced via written exchanges among attorneys and



judges, making communication slow and cumbersome. The introduction of oral proceedings dramatically reduced the time to trial (Hartmann Arboleda et al., 2009). Other major changes included the introduction of new mechanisms for case resolution (such as plea bargaining) and new restrictions on pre-trial detention (Hartmann Arboleda, 2016).

From the perspective of the police, the most salient change was the introduction of judicial control of arrests. Under the previous code, public prosecutors (*fiscales*) could write arrest warrants and sign off on warrantless arrests. This prosecutor power meant that police officers could obtain warrants quickly, and that arrests without warrants were typically declared legal—so much so that one former officer described the process as “legalizing” arrests, in scare quotes. This cursory sign-off process could conclude in fifteen minutes.

Under the new code, in contrast, *judges* wrote arrest warrants, and *judges* had to sign off on warrantless arrests. Beyond annoying the prosecutors, who “all agreed that the judges were responsible for letting subjects go” (Hartmann Arboleda et al., 2009, 85), the transfer of responsibilities (from prosecutors to judges) imposed substantial new time costs on the police. An officer making a warrantless arrest had to bring the suspect first to a prosecutor and then to a hearing before a judge; these hearings could take 45 minutes, and in some parts of the country just getting to the court was also time-consuming. Moreover, many officers perceived the judges as more *garantista* than the prosecutors, meaning that the judges were more likely to declare an arrest illegal (because officers violated due process, or because available evidence did not justify the arrest).<sup>9</sup> Under the new code, then, warrantless arrests required substantially more officer time, with a lower probability of sign-off.

Jaime Granados, the well-known criminal defense attorney quoted in the introduction, put it this way: “both the police and the prosecutors lost the power to deprive people of their liberty *just because*, as they had been accustomed to do, and they realized that if they did not have sufficient, well-founded reasons to convince a judge of the need, reasonableness, and reason for the arrest in order to get the warrant . . . then their plan falls apart.”

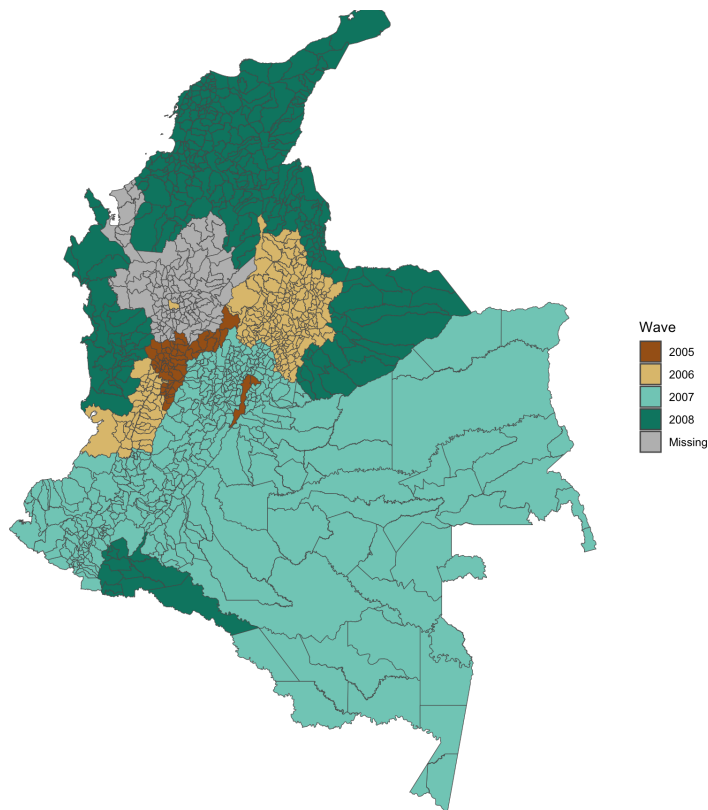
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<sup>9</sup>In response to requests for data on the number and outcome of warrantless-arrest proceedings, both the public prosecutor’s office and the judiciary provided numbers that are so incomplete as to be uninformative.



Figure 3: Rollout Map

The new code of criminal procedure came into effect on January 1, 2005 in the judicial districts of the first wave, and on January 1 of 2006, 2007, and 2008, respectively, in the second, third, and fourth waves.



Even as judicial control of arrests was meant to curb arbitrary arrests (and we find below that it did), it also reduced the time to case resolution and thereby made punishment more proximate for those who pled guilty or were convicted (Hartmann Arboleda et al., 2009, 68). For arrests *in flagrancia* (without warrants), which constituted the vast majority of arrests, the new system consolidated several steps of the process into one hearing: the legalization (or not) of the arrest, the indictment, and the plea, meaning that *flagrancia* cases were often resolved quickly (Hartmann Arboleda et al., 2009, 36). Hartmann Arboleda et al. (2009, 93) concludes that “the smooth operation of *flagrancia* cases has contributed decisively to legitimizing the new system in this country.”

Colombia’s new code of criminal procedure was rolled out in four stages. The first stage, shown in Figure 3 in brown, included Bogotá and the coffee belt; these were



the judicial districts deemed most prepared for implementing the new code. In these places, the new code came into effect on January 1, 2005, just four months after the bill passed the legislature. On January 1, 2006, the new code came into effect in the city of Medellín and in the Andean judicial districts adjacent to the coffee belt, including the city of Cali; like the judicial districts of the first wave, these districts had relatively high implementation capacity. On January 1, 2007, the new code reached the more rural judicial districts of the eastern plains and the Amazon. *De jure*, the law assigned Antioquia (the district surrounding Medellín, north of the coffee belt) to the third wave (2007), but some Antioquia courts appear to have ended up *de facto* in the 2006 wave (with Medellín); for that reason, we exclude Antioquia from the primary analysis. (Including it either in the 2006 or in the 2007 wave makes little difference; see Appendix Figure C.19). Another difference between the text of the code and implementation in practice occurred in the judicial district of Yopal, which was originally assigned to the 2006 (second) wave but ended up in the 2008 wave (Piraquive Sierra, 2007). Finally, on January 1, 2008, the new code reached the Caribbean lowlands—where the biggest city is Barranquilla, and which includes tourist regions like Cartagena—part of the Pacific coast, and much of the region that borders Venezuela. The four stages divide the Colombian population in quarters, with approximately 10 million people in each wave.

### 3 Data

To analyze the effects of the new code of criminal procedure, we use data from the police, from vital statistics, and from surveys.

**Police data.** The Colombian national police began collecting systematic data on operations and on reported crimes in 1958, via paper forms that regional offices filled out and remitted to Bogotá for tabulation, and later via Excel. Only in 2003 did the police develop and introduce dedicated software for counting crimes and documenting police activity: the System for Statistical Information on Crimes, Infractions, and Operations, or SIEDCO.

Via a freedom-of-information request, we obtained SIEDCO’s count of the number of arrests by suspected crime (such as theft or assault), by type of arrest (with a warrant or in *flagrancia*), in each municipality ( $N = 1,102$ ) in each month



between 2003 and 2011. This count includes all arrests, regardless of whether the arrest was subsequently declared legal or illegal, and regardless of whether the suspect was ultimately indicted.

We also obtained SIEDCO’s count of crimes in each municipality in each month, via freedom-of-information request. But these data suffer from three types of changes in crime registration. First, in some cities, police introduced new sources of crime counts into SIEDCO, creating the illusion of a huge increase in crime (see Appendix B). Second, in Bogotá specifically, police had previously counted thefts in SIEDCO only when the value of the stolen goods exceeded ten times the monthly minimum wage; coincident with the implementation of the new code, police began to record thefts below that threshold (Cámara de Comercio de Bogotá, 2006). And third, the incorporation of a new crime-reporting module (called SIDENCO) likely increased the number of citizen reports included in SIEDCO crime counts (Rodríguez-Ortega et al., 2018).

For some crimes, we are able to partially address the first problem—the introduction of new sources into SIEDCO—by excluding these sources from our count of crimes. The resulting measure is likely an undercount of the true number of crimes, but does not suffer from the problem of a dramatic change in crime registration (see Appendix Figure B.15). In principle, a better approach would be to add in the public prosecutor’s count of crimes for the entire period, but in practice we do not have access to these data (the public prosecutor denied our request).

For other crimes, such as car theft, the partial correction illustrated in Appendix Figure B.15 is not possible because some cities already incorporated data from the public prosecutor’s office (and other external sources) beginning in 2003.<sup>10</sup> In other words, for car theft, excluding the *public prosecutor* source would leave some cities without near-zero incident counts throughout the entire period.

These changes in registration likely did not affect SIEDCO’s count of homicides. We therefore use SIEDCO’s count of homicides in each municipality in each month as one of two measures of intentional lethal violence. Reed and Ball (2016) document certain forms of under-registration in the police count of homicides; for that

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<sup>10</sup>We also obtained data on car theft from the association of vehicle insurers. But because only one-fifth of vehicles are insured, these data are extremely sparse; three-quarters of all reported thefts occur in just three municipalities (Bogotá, Cali, and Medellín). We discuss these data in more detail in Appendix E.



reason, we also use counts from death certificates (i.e., vital statistics).

We restrict our analysis to homicide and to two major crimes for which internal police sources appear to provide a consistent (if incomplete) measure of incidence throughout the 2003–2009 period: assault (*lesiones personales*) and mugging (*hurto a personas*).

**Vital statistics.** As a second source of data on homicides, we use the mortality microdata published by Colombia’s national statistics institute. Death certificate coverage improved quickly in Colombia in the period prior to the reform that we study, climbing from an estimated 80% in the early 1990s to 99% by 2009 (Cendales and Pardo, 2018). Like the police count of homicides, the vital statistics count is incomplete; future work could supplement our analysis by conducting case studies that use multiple sources—not only police and vital statistics data but also press reports and NGO counts—to construct more complete homicide series (as in Guberek et al., 2010, for Casanare).

**Survey data.** We also use data from the Latin American Public Opinion Project (LAPOP) to measure self-reported crime victimization and perceptions of neighborhood security. The LAPOP data for Colombia identify each respondent’s municipality, allowing us to locate each respondent within a judicial district and thereby within the correct survey wave. It is not a panel; rather, the LAPOP data provide a repeated cross-section of respondents in most municipalities in most years, 2004–2009. We aggregate these data to the level of municipality-year or, for some specifications, judicial district-year. Our repeated cross sections consist of 49 municipalities and 26 judicial districts, for a total of 297 municipality-years or 156 judicial district-years.

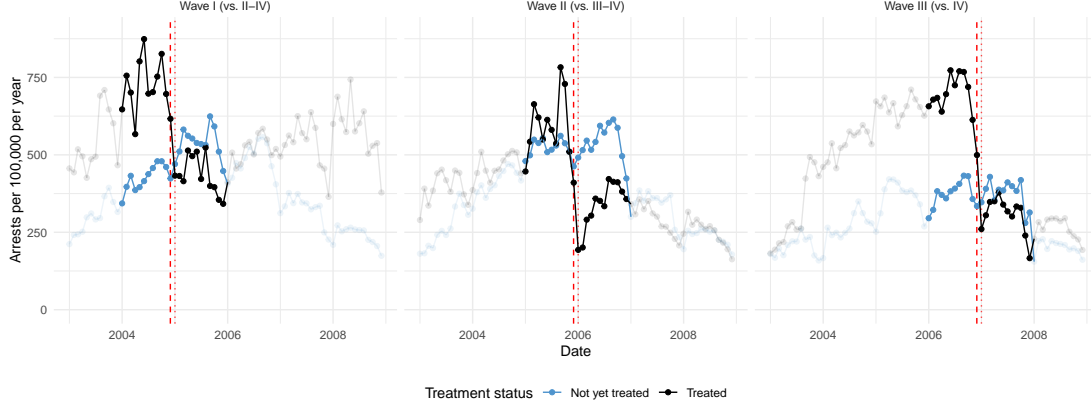
## 4 The effect of the new code on arrest rates

Though Colombia’s new code of criminal procedure primarily targeted the functioning of prosecutors and courts, we find that police also responded strategically. Police responded to new scrutiny from judges by sharply reducing the number of arrests—especially the arrests for minor offenses and arrests without warrants.



Figure 4: The Effect of the New Code on Arrest Rates

The black lines plot arrest rates in the judicial districts of Waves I, II, and III, respectively. The blue lines plot arrests rates in not-yet-treated judicial districts.



This finding is not surprising, but it was not known prior to our work.<sup>11</sup> The new code of criminal procedure is not seen or studied as a police reform. But it did reshape policing.

In 2003–2004, the two years before the new code of criminal procedure came into effect in the first judicial districts (see Figure 3 above), Colombia’s national arrest rate was approximately 750 per 100,000 per year—about half of current arrest rates in the United States, and similar to arrest rates in many countries in Latin America and Europe. When the new code came into effect, the arrest rate plummeted. In Bogotá, for example, the arrest rate declined by 50% within months, from approximately 1,300 per 100,000 per year in 2003–2004 to 600 per 100,000 per year in 2005 (see Appendix Figure C.18). In Cali, the third-largest city in Colombia, part of the second wave; Neiva, a mid-size city (population 400,000) in the third wave; and Valledupar, a city of half a million people in the fourth wave; arrest rates similarly collapsed.

These examples are not atypical. Indeed, arrests rates fell in 78% of Colombia’s 1,101 municipalities in the twelve months following the implementation of the new code, compared to the twelve months prior (Appendix Figure C.20).

To estimate the effect of the new code on arrest rates, we use a difference-in-

<sup>11</sup>Previous literature on Colombia’s new code of criminal procedure focused on how it affected pre-trial detention (Hartmann Arboleda, 2016) and the duration and outcome of court cases (Acosta Mejía et al., 2016).



differences approach. Figure 4 plots the raw data by wave. Though the data are monthly, we annualize the rates ( $\times 12$ ) so that the scale is more familiar. The left panel compares the mean arrest rate (across municipalities) in the first wave—Bogotá and the coffee belt, where the new code came into effect in January, 2005—against the mean arrest rate in Waves II, III, and IV. In 2004, arrest-rate trends were similar in these two groups (Wave I vs. Waves II–IV); then, when the new code came into effect in the judicial districts of Wave I, arrest rates diverged: they fell sharply in Wave-I municipalities while continuing to climb elsewhere in the country. This difference-in-differences suggests that the new code caused a drop in arrest rates in Bogotá and the coffee belt. A similar pattern emerges in Waves II and III.

These figures suggest that arrest rates began to fall one or two months prior to the implementation date of the new code. The second, lighter red vertical lines in each figure mark January of 2005, 2006, 2007, and 2008 (when the new code came into effect in each wave); the first, darker red vertical lines mark the previous November. This anticipation makes sense given that the implementation committee held trainings, including mock hearings, for police and prosecutors in the months leading up to the rollout ([Contraloría General de la Nación, 2010](#)). We accommodate this anticipation by coding November (rather than January) as the treatment date for our analysis of arrest rates.

To estimate the average treatment effect on the treated (ATT) at each month *relative* to when the new code came into effect, we use the estimator proposed by [De Chaisemartin and d’Haultfoeulle \(2020\)](#). Letting  $i$  index judicial districts,  $G$  index the wave of the reform ( $G \in \{1, 2, 3, 4\}$ ),  $F_G$  denote the month in which the new code comes into effect in wave  $G$  (shifted to accommodate anticipation, such that  $F_1 = \text{November, 2004}$ , for example),  $S_G$  denote the set of districts that belong to wave  $G$ ,  $Y_{i,t}$  denote outcome  $Y$  in district  $i$  at time  $t$ , and  $h \in \{-12, -11, \dots, 12\}$  index months relative to the implementation date of the reform, we calculate:

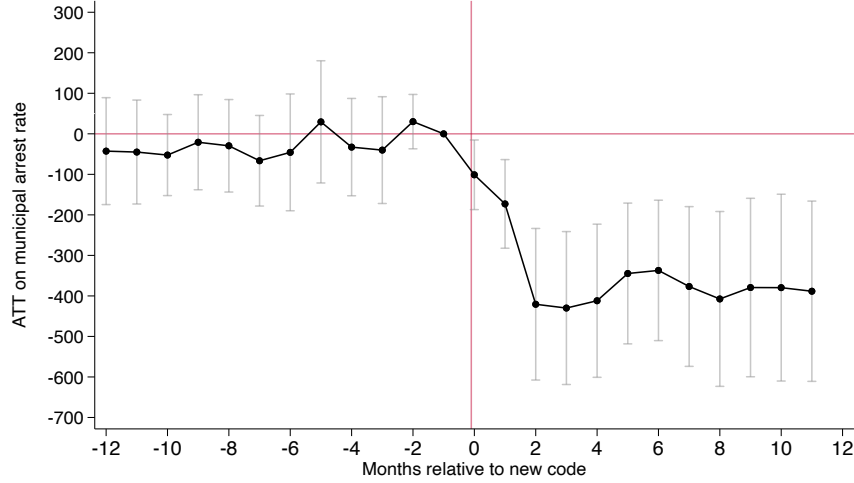
$$\widehat{\text{ATT}}_h = \sum_{G=1}^3 w_G \left( \sum_{i \in S_G} \frac{Y_{i,F_G+h} - Y_{i,F_G-1}}{|S_G|} - \sum_{\substack{i \in S_R \\ R > G}} \frac{Y_{i,F_G+h} - Y_{i,F_G-1}}{|S_R|} \right) \quad (1)$$

where  $w_G = |S_G| / \sum_g |S_g|$  are the weights given to each wave, based on the number of treated units relative to the total. As with the descriptive graphs, we annualize



Figure 5: Arrest Rates Drop As the New Code Comes into Effect

This figure plots the horizon-specific ATT of the new code on municipal arrest rates, revealing a substantively large and precisely estimated negative effect of the new code on municipal arrest rates.



Using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#). Standard errors clustered by judicial district.

the monthly rates ( $\times 12$ ) for interpretability.

Figure 5 plots the estimates. In the months prior to the introduction of the new code, the point estimates are substantively close to and statistically indistinguishable from zero (the  $p$ -value for the joint test that all pre-treatment ATTs are zero is 0.37). These estimates indicate that, consistent with Figure 4, arrest rates followed similar trends in treated and control municipalities—and that those trends diverged sharply after the new code came into effect. Figure 4 reveals a large and negative ATT on the arrest rate in the months following the introduction of the new code.

Table 1 reports the overall ATT, again following [De Chaisemartin and d'Haultfoeuille \(2020\)](#). The average municipal arrest rate dropped by 345 arrests per 100,000 per year when the new code came into effect (Column 1); relative to the average municipal arrest rate of 570 in the *pre* period (specifically, the twelve months prior to the arrival of the new code), this estimate represents a 60% decline. Weighting the observations by population (Column 2) produces an estimate of  $-431$  arrests per 100,000 per year, relative to a base of 901, or a 47% decline (this is the figure that we highlight in the abstract and introduction). Another way to



Table 1: The Effect of the New Code on Arrest Rates

This table reports estimates of Equation 1, with  $h = 1$ .

	Municipal level				Judicial district level					
	Arrest rate				Homicide		Drug		Vandalism	
	Rate	Rate	Rate	Log	Rate	Log	Rate	Log	Rate	Log
Effect of new code	-345.81 (88.66)	-431.28 (68.87)	-378.98 (91.89)	-0.47 (0.09)	-1.28 (2.49)	-0.02 (0.16)	-72.79 (39.94)	-0.62 (0.18)	-64.35 (25.69)	-1.03 (0.27)
Observations	31,056	31,056	972	972	972	972	972	972	972	972
Pre-period mean	570.78	901.25	744.22	6.48	18.06	2.76	159.22	4.55	54.64	3.21
Population weights		✓								

De Chaisemartin and d’Haultfoeuille (2020). Standard errors clustered by judicial district.

see this is to use the *log* of the arrest rate as the dependent variable (Column 4); because there are many zeros in the municipality–month panel, we aggregate to the judicial-district–month level in order to study log arrest rates (Chen and Roth, 2023).

The overall drop in arrest rates was driven by even larger declines in arrest rates for minor offenses like vandalism and drugs.<sup>12</sup> The blue line in the lower panel of Figure 6 plots the mean vandalism arrest rate in each month as a fraction of the mean vandalism arrest rate in the twelve months prior to the introduction of the new code, revealing that the mean vandalism arrest rate drops by nearly 90%. (We normalize relative to the pre-period rate in order to illustrate comparisons across crimes with very different initial arrest rates). Drug arrests (black line), too, decline more than 50% when the new code comes into effect. Homicide arrests, in contrast, do not drop as a result of the reform. Columns (6)–(10) of Table 1 confirm that similar patterns emerge using the difference-in-differences estimator described above. The declines in homicide arrest rates are substantively small and statistically indistinguishable from zero. Drug arrest rates, in contrast, fell by approximately half, and vandalism arrests collapsed almost entirely.

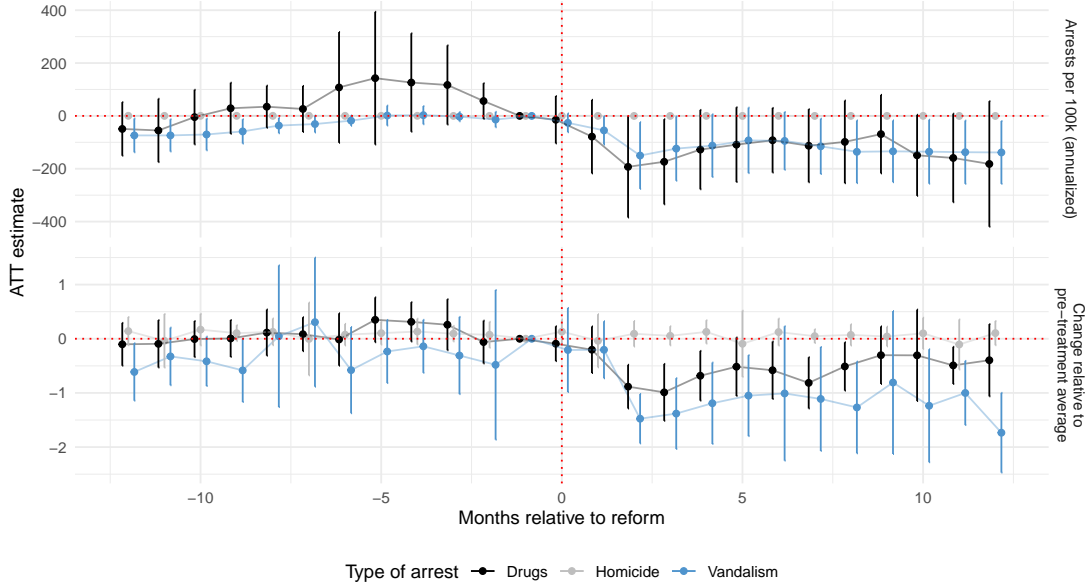
This pattern—larger declines in arrests for minor offenses than in homicide arrests—generalizes beyond the specific categories of drug and vandalism arrests. Appendix

<sup>12</sup>The Colombian penal code does not divide crimes into categories like *felony* and *misdemeanor*. Instead, the police distinguish between *crimes*, i.e., those listed in the penal code, and *infractions* (or *contravenciones*), which are violations of the Police Code. Infractions include many activities that would be classified as misdemeanors in the United States, such as public urination or disorderly conduct. Infractions in and of themselves typically do not lead to arrests. By “minor offenses,” then, we mean those crimes for which conviction would entail noncarceral punishment or short sentences.



Figure 6: Discretionary Arrests Decline, Homicide Arrests Do Not

Each line marks the crime-specific arrest rate in each month before and after the implementation of the new code, scaled relative to the mean arrest rate (for that category) in the twelve months prior to implementation.



Using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#). Standard errors clustered by judicial district.

Figure C.23 plots the size of the drop in the arrest rate against the minimum sentence length in Colombia's penal code (one measure of crime severity). For the nineteen charges with sufficient numbers of arrests to estimate a treatment effect, we observe that the arrest rate generally declines *less* for more serious crimes.

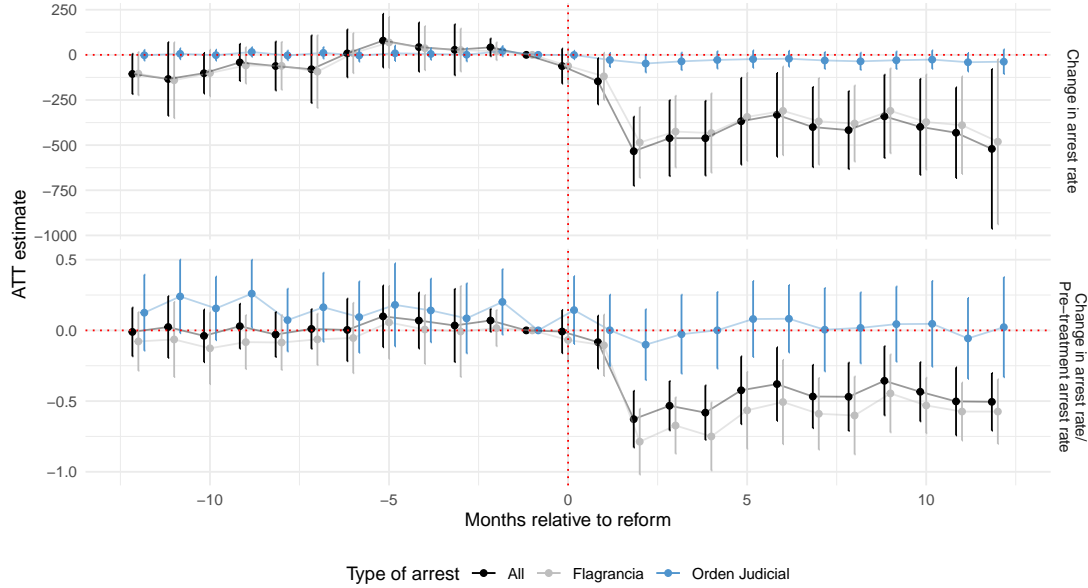
Another way to study the changing composition of arrests is to consider the share of arrests made with a warrant (as opposed to in *flagrancia*). Prior to the new code, arrests for more serious crimes were much more likely to be made with a warrant than arrests for low-level crimes: whereas just 25% of homicide arrests were made *en flagrancia* (no warrant), more than 98% of arrests for selling pirated CDs were made *en flagrancia* (Appendix Figure C.21). Overall, in the pre-period, 75% of arrests were made *en flagrancia* and 25% were made with warrants.

When the new code came into effect, arrests made with warrants declined by approximately 20% (Figure 7), while arrests in *flagrancia* (i.e., in which a suspect is caught committing a crime) declined much more, by approximately 50%. As a result, the mean (across judicial districts) warrant *share* of arrests increased by



Figure 7: Arrests Without Warrants Decline More than Arrests With Warrants

Each line marks the mode-specific arrest rate in each month before and after the implementation of the new code, scaled relative to the mean arrest rate (for that category) in the twelve months prior to implementation.



Using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#). Standard errors clustered by judicial district.

10 percentage points, from approximately 25 to 35 (Figure C.22b).

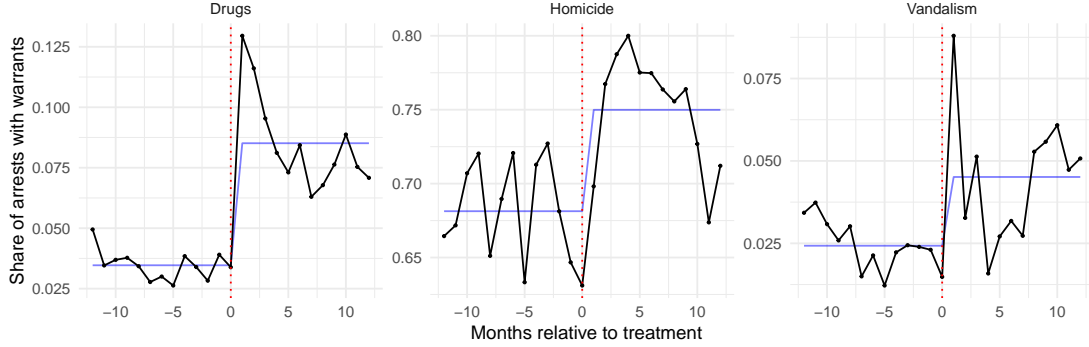
What's more, the shift toward warrant arrests is not driven entirely by the shift toward arrests for more serious offenses. The warrant share of arrests also increases sharply *within* crime (Figure 8). The warrant share of arrests increased for 25 of the 30 crimes with the highest arrest rates prior to the new code (i.e., the most common charges).

If these shifts away from arrests for low-level offenses and away from arrests *in flagrancia* (without warrants) indeed implied prioritizing more-justified arrests, we might expect that new-regime arrests would be more likely to produce convictions. Our ability to evaluate the effect of the new code on conviction rates is limited because of problems with data from the courts: Colombia's *Consejo Superior de la Judicatura* informed us that they have no records from the year 2006; that, for the year 2005, they only have records corresponding to cases considered under the old code; that they could provide only annual (not monthly) data; and, moreover, that they could not provide microdata that would allow us to follow individual



Figure 8: Warrant Share of Arrests Increases *Within* Crimes

Figure 7 above shows that arrests with warrants declined much less than arrests without warrants. Here, we show that this pattern is driven not only by the changing composition of arrests across crimes, but also by changes *within* each crime category.



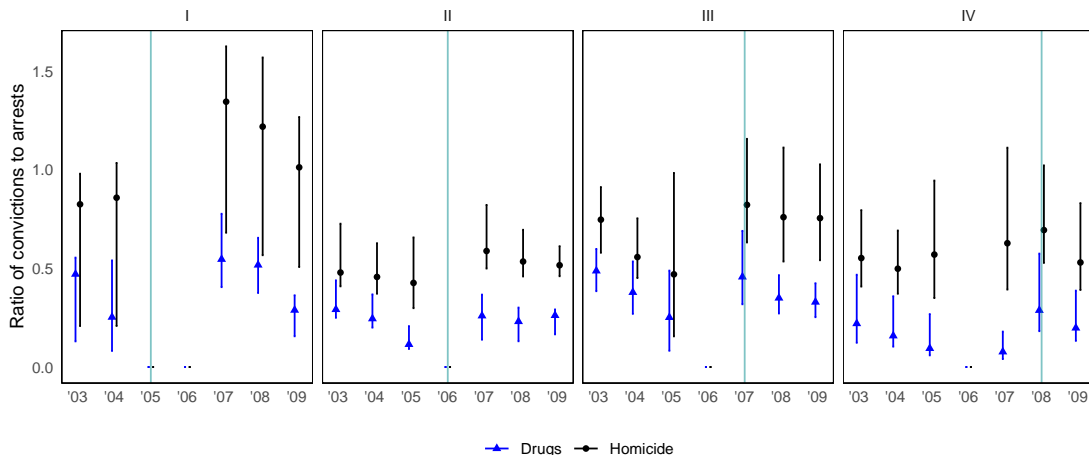
cases through the system. These limitations imply (among other issues) that we do not observe immediate pre-post values for any of the four waves of the rollout and that we cannot evaluate whether any individual arrest ends up producing a conviction. Still, we are able to calculate the ratio of convictions to arrests in each year, in each rollout wave, for each crime. Figure 9 reports the results. These values suggest that the ratio of convictions to arrests increased in both of these categories.

These differential changes in arrest rates did not occur because the new code imposed crime-specific or mode-specific restrictions on arrests. Rather, they emerged endogenously as the police re-optimized in response to the new rules. Homicide arrests, and arrests made with warrants, required significant effort and faced some scrutiny even under the old code; for that reason, the change in cost (to the police) was smaller for these crimes. Discretionary arrests, in contrast, typically faced little scrutiny under the old code; recall that one officer described the required prosecutor sign-off as “legalizing” the arrest, in scare quotes. For drug-possession arrests, vandalism arrests, and other arrests *in flagrancia* or for minor crimes, the new regime implied dramatic changes in officer time and effort—and, as a result, dramatic changes in arrest quantity. In the following section, we investigate the consequences for crime rates.



Figure 9: Ratio of Convictions to Arrests Increases

The court data are such that we cannot follow individual cases through the system. With that caveat in mind, we show here that the aggregate ratio of convictions to arrests *increased* following the implementation of the new code.



## 5 The effect of the new code on crime

Colombia's new code of criminal procedure changed the criminal justice system in many ways and therefore likely affected crime rates through multiple channels. We group these effects into two categories: (1) those that improved the accuracy of criminal prosecution, raising the probability of punishment for guilty arrestees and reducing the probability of punishment for innocent ones; and (2) those that reduced the probability of arrest.<sup>13</sup> All else equal, the increase in accuracy would likely reduce crime, while the decline in arrests would typically increase it. We find that the new code had no effect on homicide, assault, mugging, or self-reported crime victimization.

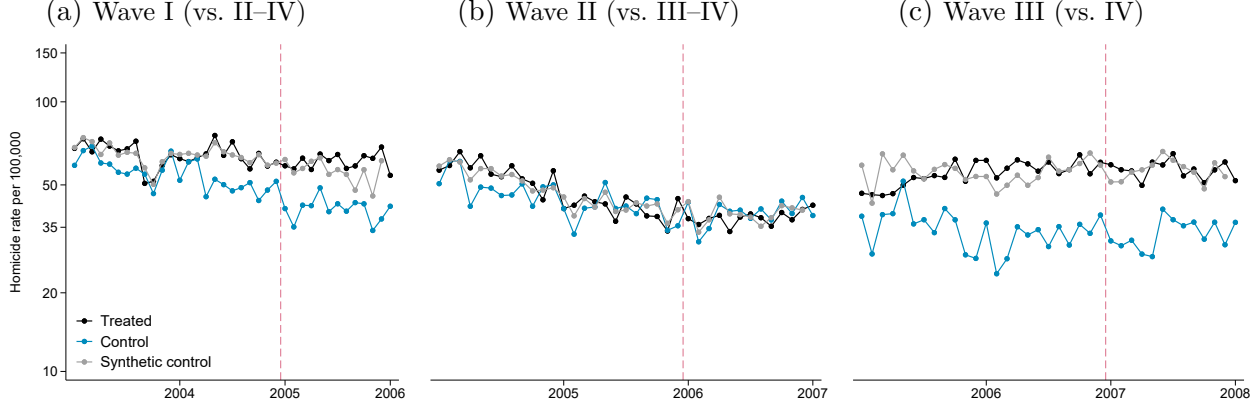
**No effect on homicide rates.** Estimating the effect of the new code on homicide rates is more difficult than estimating the effect of the new code on arrest rates. For one thing, we do not observe any obvious patterns in raw homicide-rate trends in any specific cities. Whereas the raw arrest-rate trends reveal dramatic drops in nearly every city at the moment when the new code comes into effect (see Figure D.24 for examples), raw arrest-rate trends do not reveal any obvious

<sup>13</sup>We abstract away from one major consequence of the new code, celerity, which is related to but distinct from accuracy. All else equal, celerity would tend to reduce crime.



Figure 10: The Effect of the New Code on Homicide Rates

The black lines plot arrest rates in the judicial districts of Waves I, II, and III, respectively. The blue lines plot arrests rates in not-yet-treated districts. The gray lines plot synthetic controls.



changes (Appendix Figure D.24). For another, the national homicide rate was plummeting throughout the period of the rollout of the new code (Figure 2); unsurprisingly, it declined at different rates in different places, making it difficult to account for secular trends and/or adjust for divergent pre-trends. Moreover, relative to arrest, homicide is a rare outcome, meaning that the municipality-month panel is sparse. Because there are many zeros in the municipality-month panel, we do not analyze the natural log of the outcome (Chen and Roth, 2023)—but the distribution of municipal homicide rates themselves is of course highly skewed. For that reason, we consider two panels: a municipality-month panel in which the outcome is the (highly skewed) homicide rate, and a judicial-district-month panel, in which the outcome is either the homicide rate itself or the natural log of the homicide rate.<sup>14</sup> Finally, as we discuss below, lethal violence in Colombia takes many forms, only some of which might plausibly respond to a change in arrest rates.

The black line in Figure 10a marks the average (log) homicide rate across judicial districts in the first wave of the rollout of the new code (Bogotá and the coffee belt); the blue line in that same figure marks the average (log) homicide rate in the judicial districts in Waves II–IV. They are not parallel: in the twelve months prior to the first-wave implementation of the code (i.e. the twelve months of 2004),

<sup>14</sup>At the judicial-district-month level, fewer than half of one percent of the observations are zeros, meaning that the estimates are not sensitive to whether we add 1 or 0.5 or 1.5 or another small constant to the outcome before taking the natural log.

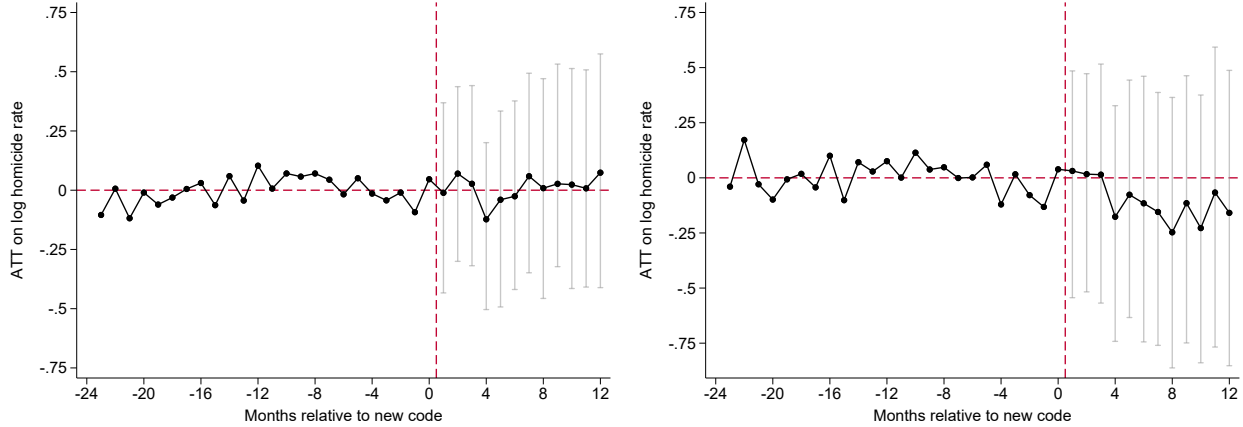


Figure 11: No Evidence that the New Code Affected Homicide Rates

This figure plots synthetic-control estimates of the horizon-specific ATT on the judicial–district-level log homicide rate.

(a) Vital statistics data

(b) Police data



Using the estimator proposed by [Cattaneo et al. \(2022b\)](#).

homicide rates declined less in Bogotá and the coffee belt than they did everywhere else. A similar pattern emerges in 2005 (the year prior to the second wave of the rollout) and 2006 (the year prior to the third wave): the gap between homicide rates in about-to-be-treated and later-treated judicial districts changes over the course of each year.

We address these not-parallel pre-trends in two ways. First, we estimate a difference-in-differences (again using the [De Chaisemartin and d’Haultfoeuille \(2020\)](#) estimator described above) that includes unit-specific linear trends; the residual trends are closer to parallel. Second, we use the methods proposed by [Cattaneo et al. \(2022b\)](#) to create synthetic control groups whose pre-trends are much closer to those of the treated units.

Table 2 reports the difference-in-differences (estimates of Equation 1, for  $h = 1$ ). The unweighted estimates (i.e., those that weight each municipality or judicial district equally), which we report in Panel A, reveal no evidence of an increase in homicide rates when the new code came into effect. In the municipality–month panel, the difference-in-differences point estimate is negative and statistically indistinguishable from zero; using the judicial-district–month panel, the point estimates are positive but very imprecisely estimated.



Table 2: The Effect of the New Code on Homicide Rates

This table reports estimates of Equation 1, with  $h = 1$ .

	Municipal level		Judicial-district level			
	Police Data	Vital Statistics	Police Data		Vital Statistics	
	Rate	Rate	Log	Log	Log	Log
<b>Panel A: Unweighted</b>						
Effect of new code	-13.78 (9.41)	-4.93 (6.24)	0.11 (0.09)	0.08 (0.11)	0.02 (0.07)	0.01 (0.09)
Observations	31,056	31,668	972	972	972	972
Pre-period mean	54.70	49.26	3.71	3.71	3.67	3.67
Unit-specific trends				✓		✓
<b>Panel B: Weighted by population</b>						
Effect of new code	-2.10 (3.00)	0.33 (1.76)	-0.02 (0.06)	-0.11 (0.07)	0.01 (0.04)	-0.05 (0.05)
Observations	31,056	31,668	972	972	972	972
Pre-period mean	55.60	49.72	3.86	3.86	3.76	3.76
Unit-specific trends				✓		✓

De Chaisemartin and d’Haultfoeuille (2020). Standard errors clustered by judicial district.

The population-weighted estimates in Panel B are also of substantive interest. Different judicial districts have very different sizes; the population of the judicial district of Bogotá, for example, was more than 6.5 million in 2003, while the population of the smallest judicial district, the Archipelago de San Andrés y Providencia, was just over 69,000. If the new code had caused an increase in homicide rates in Bogotá and one or two other large judicial districts but not elsewhere, we would want to know—but this effect might not show up in the unweighted estimates in Panel A. For that reason, we estimate population-weighted versions of the difference-in-differences. Panel B reports the results. Using the municipality-month panel with homicide rates as the dependent variable (Columns 1–2), the estimates are substantively very close to zero. Using the judicial-district-month panel, the point estimates are very close to zero in the absence of unit-specific trends, negative with unit-specific trends, and in all cases imprecisely estimated (Columns 3–6). Taken together, these results provide no evidence that the new code adversely affected homicide rates.

The gray lines in Figures 10a–10c plot the synthetic controls for the judicial dis-



tricts in each wave of the rollout, and Figure 11 reports the aggregate synthetic control ATT estimates. Neither suggests any evidence of an increase in the homicide rate.

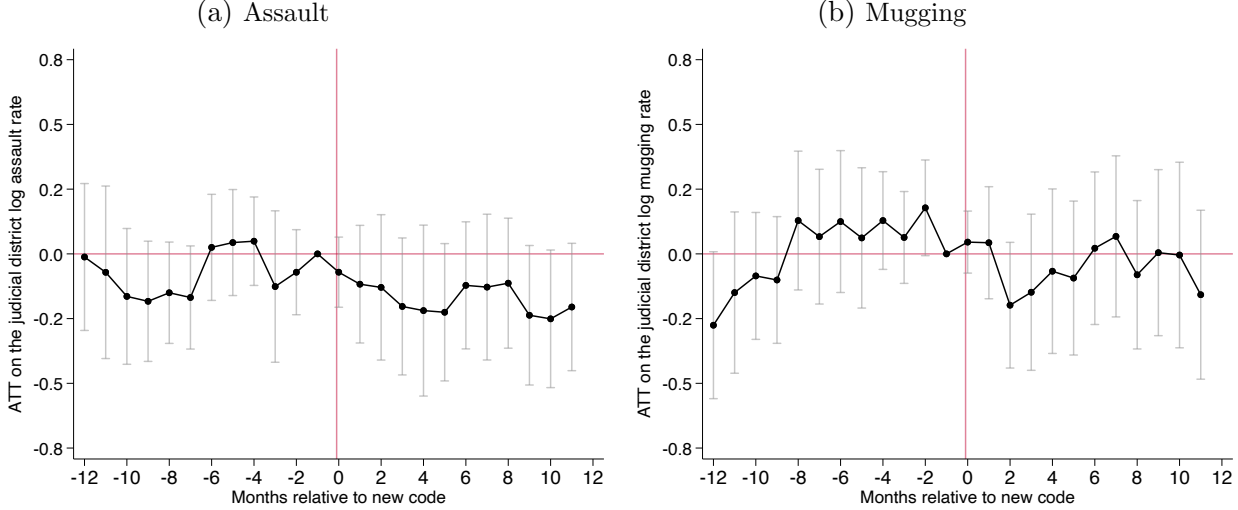
In some parts of Colombia during this period, police presence was so tenuous and the dynamics of violence so tied to war that we might not *expect* the new code to affect the homicide rate. In Saravena, for example, a city of approximately 80,000 people near the border with Venezuela, conflict between the ELN and the FARC—and between these groups and the Colombian military—produced the five or ten deaths every month (a rate of more than 100 per 100,000 per year) that led journalists call Saravena “the Colombian Sarajevo” (León, 2005); Uribe’s Democratic Security initiative did establish police presence in Saravena, but they were exempt from normal due process requirements (such as arrest warrants). It is hard to imagine how the *de jure* introduction of a new code of criminal procedure and/or the consequent change in arrest rates would affect lethal violence in places like Saravena. In the large cities of Bogotá or Medellín, in contrast, much of the lethal violence of the mid-2000s stemmed from conflict among small and consolidating local gangs as well as from community violence (such as bar fights and brawls). It is this violence could plausibly respond to arrest rates through the channels outlined above: deterrence, incapacitation, and/or the reallocation of police time across activities.

One might then wonder whether the apparent null effect of the new code on homicide rates is driven by the inclusion of war zones, in which we would not expect the new code to affect violence. We consider this possibility in three ways. First, we use the carefully constructed CERAC data set on deaths in the Colombian civil war to identify municipalities like Saravena, where the guerrilla, paramilitaries, and the armed forces were responsible for a significant fraction of all lethal violence (77% of all violent deaths in Saravena in 2003, for example). Restricting the sample to municipalities with no or few war casualties does not much affect the results (Appendix Figure D.25). Second, we simply plot the homicide rate in Colombia’s 18 largest cities (Appendix Figure D.26); these raw time trends suggest no evidence of an obvious effect of the new code except perhaps in Ibagué and Montería, cases that we believe merit further investigation. Third, we plot synthetic control difference-in-differences for each judicial district separately (Appendix D.1); we do not observe any judicial district in which the introduction of the new code appears to have affected the homicide rate. We interpret these



Figure 12: No effect of the new code on assault or mugging

These figures plot the horizon-specific ATT of the new code on (a) log assault (*lesiones*) rates and (b) log mugging rates (*hurto a personas*) at the level of judicial district. Neither figure reveals evidence of an increase in crime following the new code.



Using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#). Standard errors clustered by judicial district.

findings as evidence that the new code not only did not affect the Colombian homicide rate overall, but also that it did not affect the homicide rate in cities (where we would be more likely to observe a nonzero effect).

**No effect on assault or mugging.** As noted above, we use police counts of reported assaults (*lesiones*) and muggings (*hurto a personas*) to study how these crimes responded to the new code of criminal procedure, after removing new reporting sources that created artificial jumps in these counts (see Section 3).

We find no evidence that assault or mugging increased as a result of Colombia's new code of criminal procedure. Figure 12 plots the horizon-specific ATT of the new code on log assault (left panel) and mugging (right panel) rates. Neither set of estimates reveals an increase following the implementation of the new code. Because the municipal-level panel is very sparse, i.e., has many zeros, we aggregate to the judicial-district level in order to take the log of these crime rates.<sup>15</sup>

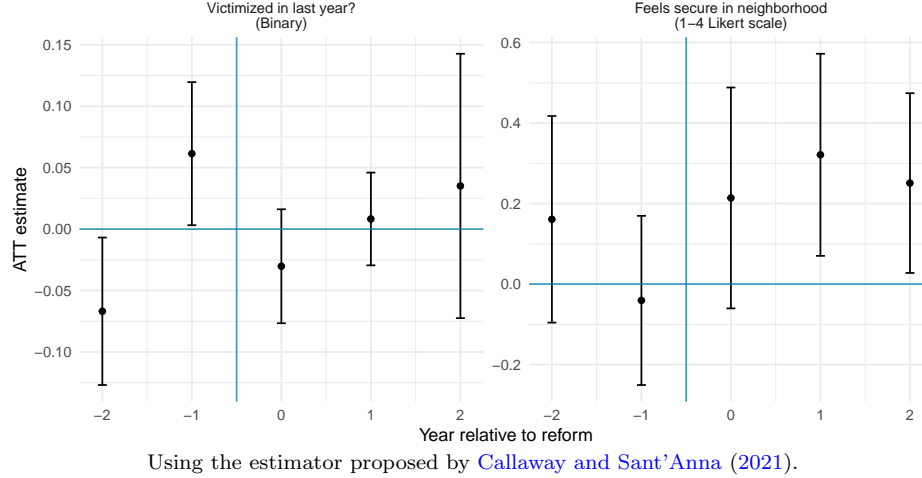
These findings contrast with those of [Acosta Mejía et al. \(2023\)](#), who find large and

<sup>15</sup>At the judicial-district-month level, just 0.5% of assault observations and 2% of mugging observations are zero.



Figure 13: No effect on self-reported victimization; + feelings of security

This figure plots difference-in-differences estimates of the effect of the new code on self-reported crime victimization and on perceptions of neighborhood security.



precisely estimated effects of the new code on these two crimes. We attribute the difference to our use of data that excludes new sources of crime reporting, which created the appearance of sharp jumps in crime in months that, in some places, coincided with the rollout of the new code. In Appendix Figure B.16, we replicate their result and then plot results from the same specification—the [Callaway and Sant'Anna \(2021\)](#) estimator with short differences, using a municipality-level panel with log rates as the dependent variable—replacing the original crime counts with counts that exclude the new sources of crime reporting. The data do not indicate that Colombia's new code of criminal procedure sparked a major crime wave.

**No effect on self-reported crime victimization.** We use data from the Latin American Public Opinion Project (LAPOP) to study the effect of the new code on self-reported crime victimization, and on perceptions of neighborhood security. Unlike the data on arrests and crime analyzed above, the LAPOP data are annual, meaning that our ability to study pre-trends or the evolution of treatment effects is more limited. But surveys have two advantages. First, they allow us to study trends in victimization from crimes *not* reported to the police. This feature is especially important given that, as noted above, the police changed their approach to crime registration in several ways during this time period. Second, they allow us to study victimization overall, rather than only for specific crimes.



If Colombia’s new code of criminal procedure had caused a major crime wave, we would expect it to show up in self-reported crime victimization. Instead, we see null effects. The left panel of Figure 13 plots the ATT of the new code on self-reported victimization rates in each year before and after the introduction of the new code. The pre-trends are not parallel; treated municipalities reported slightly lower victimization rates two years prior to adoption and slightly higher victimization rates one year prior to adoption than control municipalities. This pattern of course demands caution in interpreting the post-treatment trends. Yet the point estimates in the three years following the adoption of the new code do not suggest that crime increased. Moreover, when we aggregate these effects in Table 3, even the sign of the point estimate is unstable; two-way-fixed-effects specifications suggest an imprecisely estimated one-percentage-point increase in self-reported crime victimization (on a base of 14%); Callaway and Sant’Anna (2021) indicate a three-percentage-point *decrease* in victimization (3).

Consistent with the conclusion that the new code of criminal procedure did not cause a major crime wave, the survey data suggest a substantial (if imprecisely estimated) *improvement* in perceptions of neighborhood security (right panel, Figure 13). Here, the pre-trends in perceptions are closer to parallel, and the overall effect is somewhat more precisely estimated (Table 3). The improvement in perceptions is substantively significant, too: 0.18 points on a 1–4 scale, or approximately 6% of the pre-treatment mean and more than half of the pre-treatment standard deviation.

Overall, available data on homicide, assault, mugging, and self-reported crime victimization reveal no evidence of an increase in crime following the implementation of Colombia’s new code of criminal procedure. These findings are consistent with the notion that any criminogenic consequences of the decline in arrests was counterbalanced by the crime-reducing effects of the increase in prosecutorial and judicial accuracy.

## 6 Discussion

Most directly, our results illuminate the results of an unusual policy experiment in Colombia. Between 2005 and 2008, a new code of criminal procedure took effect in four successive geographic blocs. The new code sought to improve the accuracy of prosecutorial and judicial decisionmaking, and in doing so required more effort



Table 3: No effect on self-reported victimization; + feelings of security

This table reports the difference-in-differences estimates of the new code on self-reported crime victimization and on perceptions of neighborhood security, using both TWFE models and the estimator proposed by [Callaway and Sant’Anna \(2021\)](#).

	Victimized		Neighborhood Security	
	(1)	(2)	(3)	(4)
PANEL A: TWO-WAY FIXED EFFECT ESTIMATES				
Post-reform	0.012 (0.013)	0.007 (0.015)	0.142 <sup>+</sup> (0.082)	0.187 <sup>+</sup> (0.093)
Municipality FE	✓		✓	
Judicial district FE		✓		✓
Year FE	✓	✓	✓	✓
PANEL B: <a href="#">CALLAWAY AND SANT’ANNA (2021)</a> AGGREGATE EFFECT ESTIMATES				
Post-Reform	-0.028 (0.024)	-0.034 (0.019)	0.168 (0.106)	0.188 <sup>+</sup> (0.113)
Cross-sectional unit	Municipality	Judicial district	Municipality	Judicial district
<i>N</i> Observations	297	156	297	156
<i>N</i> Cross-sectional units	49	26	49	26
Outcome scale	[0, 1]	[0, 1]	[1, 4]	[1, 4]
Pre-reform mean (2004)	0.143	0.141	2.808	2.788
Pre-reform std. dev. (2004)	0.111	0.081	0.388	0.284

<sup>+</sup> $p < 0.1$ , \* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$

from police. As a result, we find, the police sharply curtailed the number of arrests, and in particular the number of arrests for low-level offenses like vandalism and drug possession. But while previous work finds that crime skyrocketed as a result, concluding that society faces a tradeoff between due process and public safety, we find instead no evidence of an increase in crime. The improvement in accuracy likely compensated for the drop in the arrest rate. Our theory clarifies how, why, and when such improvements outweigh the potentially criminogenic effects of reduced police activity.

More broadly, our findings speak to the value of studying interactions among different institutions of criminal justice. Despite the clear implications of Colombia’s new code of criminal procedure for the police, previous work did not focus on the implications for officer behavior. These implications are important in their own right, because policing itself is an outcome of interest. They are also critical for understanding the consequences of the new code for crime rates. Similarly, scholars studying other criminal justice policy changes that do not target the police—such



as the elimination of cash bail or new ICE priorities—must also consider police response. Police may respond to the end of cash bail for certain crimes by recommending different charges, or respond to a ramp-up in ICE activity either by avoiding discretionary contact with migrants or by seeking it out. Prosecutors also respond strategically to other agencies’ reforms, such as legislation mandating minimum sentences for certain crimes. Our point is not merely that agents of the criminal justice system may respond in unanticipated ways to incentives or constraints aimed at reshaping their behavior; that notion is well established in the literature. Rather, our point is that agents of the criminal justice system respond strategically to incentives or constraints aimed at reshaping the behavior of *other* institutions of criminal justice.

Such interactions are also relevant beyond criminal justice. Across government agencies, scholars have documented myriad examples of strategic response to direct regulation or new rules binding a given institution. Less well understood is how public bureaucracies respond to reform of the other bureaucracies with which they interact, or how that interaction affects citizen behavior. We view this as a valuable topic for future work.



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# Appendix

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## A Theory

Consider a unit mass of individuals, indexed by  $i$ . Citizens choose whether to work in the labor market or commit a crime, denoted by  $d_i \in \{W, C\}$ . By forgoing a crime, a citizen takes their market income, which we normalize to 0. By committing a crime without being caught, a citizen extracts a benefit  $\delta_i \sim U[a, b]$ . Given our normalization of income, we interpret  $\delta_i$  as the benefit of crime relative to working in the market for individual  $i$ . We denote the pdf and cdf of  $\delta$  as  $f_\delta$  and  $F_\delta$  throughout.

Police choose a patrol rate  $\lambda \in (0, 1)$ . Patrolling requires costly effort. For convenience, set the cost of patrolling to be  $\frac{c\lambda^2}{2}$ .<sup>16</sup> Police value patrolling either because it: (1) allows them to demonstrate some output—arrests—for promotion or (2) because they value taking the guilty off the street. The latter could be conceptualized as a taste for order or some type of public service motivation. We weight these two benefits by  $\mu \in [0, 1]$  (arrests) and  $1 - \mu$  (public service motivation), respectively.

Neither the police nor the courts are perfect in determining whether an individual committed a crime, however. We assume that when patrolling, police correctly arrest a guilty citizen with probability  $\rho_p \in (\frac{1}{2}, 1)$  and incorrectly arrest an innocent citizen with probability  $1 - \rho_p$ . Similarly, we assume that, conditional on reaching the court, the prosecutor accurately detains a guilty suspect with probability  $\rho_c \in (\frac{1}{2}, 1)$  and inaccurately detains an innocent suspect with probability  $1 - \rho_c$ . One can view  $\rho_p$  and  $\rho_c$  as measures of the quality of police and the court, respectively. If detained, the citizen is subject pays a penalty  $P > 0$ .

Each citizen's utility is therefore given by:

$$U_i(d_i) = \begin{cases} 0 & \text{if } d_i = W \text{ and not detained} \\ \delta_i & \text{if } d_i = C \text{ and not detained} \\ -P & \text{if subject is detained} \end{cases}$$

---

<sup>16</sup>More generally, one could set some cost function  $\kappa(\lambda)$ , where  $\kappa'(\lambda) > 0$  and  $\kappa''(\lambda) > 0$ .



Each citizen's expected utility, is therefore given by:

$$E[U_i(d_i)] = \begin{cases} \overbrace{-P\lambda(1-\rho_p)(1-\rho_c)}^{\text{Patrolled, wrongly arrested, and wrongly detained}} & \text{if } d_i = W \\ \delta_i \left( \underbrace{1-\lambda}_{\text{Not patrolled}} + \underbrace{\lambda}_{\text{Patrolled}} \left[ \underbrace{(1-\rho_p)}_{\text{Not arrested}} + \underbrace{\rho_p(1-\rho_c)}_{\text{Arrested, but not detained}} \right] \right) - \underbrace{P\lambda\rho_p\rho_c}_{\text{Patrolled, arrested, and detained}} & \text{if } d_i = C \end{cases}$$

Note that in equilibrium, some proportion of the population will commit crime, i.e.  $\Pr(d_i = C)$ . We can therefore write the police officer's objective as:

$$\max_{\lambda \in [0,1]} \lambda \left( \underbrace{\mu [\Pr(d_i = C)\rho_p + (1 - \Pr(d_i = C))(1 - \rho_p)]}_{\text{Arrests}} + (1 - \mu) \underbrace{[\Pr(d_i = C)\rho_p\rho_c]}_{\text{Guilty detained}} \right) - \frac{c\lambda^2}{2}$$

To avoid corner solutions, we impose two parametric assumptions on the support of  $\delta_i$  that are consistent with empirical observation:  $a < 0$  and  $b \geq \frac{2P}{1-\rho_c\rho_p}$ . These assumptions imply that the market wage is greater than the benefit from committing crime for some individual ( $a < 0$ ), and that perfect deterrence of crime is not possible ( $b \geq \frac{2P}{1-\rho_c\rho_p}$ ).

The sequence is as follows:

1. All individuals decide whether to commit or forego crime.
2. The police determine their patrol rate and patrol.
3. Nature determines whether a patrolled subject is arrested, and, conditional on arrest, whether they are detained. Utilities are realized.

We characterize the subgame perfect Nash equilibrium in pure strategies. Individuals' decisions are a choice  $d_i : \{W, C\}^i$ . The police's patrol rate is given by  $\lambda : \{W, C\}^i \rightarrow [0, 1]$ .

## A.1 Benchmarks: No patrolling or surveillance state

First consider crime rates under the two possible corner solutions to the police's patrol rate decision: no patrolling ( $\lambda = 0$ ) or a surveillance state in which everyone



is patrolled ( $\lambda = 1$ ). When  $\lambda = 0$ , it is trivial to see that citizens commit crime if their benefit from crime exceeds the market wage, e.g., when  $\delta_i > 0$ . The crime rate is therefore given by  $1 - F_\delta(0)$ .

In a surveillance state ( $\lambda = 1$ ), a citizen commits a crime if:

$$\begin{aligned} \delta_i ((1 - \rho_p) + \rho_p(1 - \rho_c)) - P\rho_p\rho_c &> -P(1 - \rho_p)(1 - \rho_c) \\ \delta_i &> \frac{P(\rho_p + \rho_c - 1)}{1 - \rho_p\rho_c} \end{aligned}$$

The crime rate is  $1 - F_\delta(\frac{P(\rho_p + \rho_c - 1)}{1 - \rho_p\rho_c})$ . It is clear from inspection that  $\frac{P(\rho_p + \rho_c - 1)}{1 - \rho_p\rho_c} > 0$ . Therefore, the crime rate is lower under the surveillance state than under no patrolling.

## A.2 Equilibrium characterization

The above benchmarks helps us think about the the officer's patrolling decision. Specifically, the share of the population that commits crime, can be expressed as a function of the patrol rate  $Pr(d = C) = 1 - F_\delta(g(\lambda))$ . Substitution allows us to rewrite the officer's objective as:

$$\max_{\lambda \in [0,1]} \lambda [F(g(\lambda))[\mu(1 - 2\rho_p + \rho_p\rho_c) - \rho_c\rho_p] + \rho_p(\mu + \rho_c - \mu\rho_c)] - \frac{c\lambda^2}{2} \quad (2)$$

To characterize  $g(\lambda)$ , consider the the population's decisions of whether to commit crime. A citizen commits crime if:

$$E[u_i(c)] - E[u_i(w)] > 0,$$

which we can write in terms of the threshold  $\delta_i^*(\lambda)$ :

$$\delta_i^*(\lambda) > \frac{P\lambda(\rho_p + \rho_c - 1)}{1 - \lambda\rho_p\rho_c}. \quad (3)$$

Obviously, this expression nests the two corner cases derived above. Note that  $\frac{\partial \delta_i^*}{\partial \lambda} = \frac{P(\rho_p + \rho_c - 1)}{(\lambda\rho_p\rho_c - 1)^2} > 0$ , under the above parametric assumptions. This means that as the rate of patrols increases, the marginal citizen that is indifferent to crime/foregoing crime must have a higher net benefit from committing crime ( $\delta_i$ ).



It is straightforward to see that  $\delta_i^* = g(\lambda)$ .

Substituting (3) into (2) and maximizing yields:

$$\frac{\mu(1 - 2\rho_p + \rho_p\rho_c) - \rho_c\rho_p}{b - a} \left( \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^*\rho_p\rho_c} - a + \frac{P\lambda^*(\rho_p + \rho_c - 1)}{(\lambda^*\rho_c\rho_p - 1)^2} \right) + \rho_p(\mu + \rho_c - \mu\rho_c) - c\lambda^* = 0 \quad (4)$$

To show that this interior equilibrium is unique, note the left hand side of this expression is strictly decreasing in  $\lambda^*$ . The following expression gives the second order condition with respect to  $\lambda^*$ :

$$\underbrace{2P}_{>0} \underbrace{\frac{\mu(1 - 2\rho_p + \rho_p\rho_c) - \rho_c\rho_p}{b - a}}_{<0} \underbrace{\frac{\rho_p + \rho_c - 1}{(1 - \lambda^*\rho_c\rho_p)^3}}_{>0} - c < 0$$

**Proposition 1.** *In the unique subgame perfect Nash Equilibrium, police patrol at rate:*

$$\lambda = \begin{cases} 0 & \text{if } \lambda^* < 0 \\ \lambda^* & \text{if } \lambda^* \in [0, 1] \\ 1 & \text{if } \lambda^* > 1, \end{cases}$$

where  $\lambda^*$  solves (4). Individuals commit crime if  $\delta_i > \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^*\rho_p\rho_c}$  and forego crime otherwise.

### A.3 Equilibrium Quantities of Interest

This equilibrium allows us to characterize two equilibrium outcomes that we observe empirically. First, the crime rate is given by:

$$\mathcal{C} = 1 - F_\delta \left( \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^*\rho_p\rho_c} \right). \quad (5)$$



Note that the equilibrium crime rate is *decreasing* in the patrol rate  $\lambda^*$ :

$$\frac{\partial \mathcal{C}}{\partial \lambda^*} = \frac{P(\rho_c + \rho_p - 1)}{(a - b)(\lambda^* \rho_c \rho_p - 1)^2} < 0,$$

since  $b > a$ , which demonstrates the deterrent effect of patrolling on crime.

Second, given the crime rate, the arrest rate is:

$$\mathcal{A} = \lambda^* \left[ \rho_p \left( 1 - F_\delta \left( \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^* \rho_p \rho_c} \right) \right) + (1 - \rho_p) F_\delta \left( \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^* \rho_p \rho_c} \right) \right] \quad (6)$$

$$= \lambda^* \left[ \rho_p + (1 - 2\rho_p) F_\delta \left( \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^* \rho_p \rho_c} \right) \right] \quad (7)$$

How does the arrest rate vary in the patrol rate? Let  $x = \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^* \rho_p \rho_c}$ .

$$\begin{aligned} \frac{\partial \mathcal{A}}{\partial \lambda^*} &= (\rho_p + (1 - 2\rho_p) F_\delta(x)) + \lambda(1 - 2\rho_p) F'_\delta(x) \frac{\partial x}{\partial \lambda^*} \\ &= \rho_p + (1 - 2\rho_p) \left( \frac{x - a}{b - a} + \frac{\lambda^*}{b - a} \frac{\partial x}{\partial \lambda^*} \right) \\ &= \rho_p + \frac{1 - 2\rho_p}{b - a} \left( \frac{P\lambda^*(\rho_p + \rho_c - 1)}{1 - \lambda^* \rho_p \rho_c} - a + \frac{\lambda^* P(\rho_p + \rho_c - 1)}{(1 - \lambda^* \rho_p \rho_c)^2} \right) \\ &= \underbrace{\frac{\rho_p b - a(1 - \rho_p)}{b - a}}_{>0} + \underbrace{\frac{(1 - 2\rho_p) P\lambda^*(\rho_p + \rho_c - 1)(2 - \lambda^* \rho_p \rho_c)}{(b - a)(1 - \rho_p \rho_c)^2}}_{<0} \end{aligned}$$

This expression can be positive or negative, which reflect the idea that arrest would mechanically increase in police effort (if the crime rate were fixed). However, police patrolling effort deters crime (as demonstrated above). These effects are counterveiling.

## A.4 Mapping to intervention and empirical implications

Our intervention manipulates two attributes of the above model. First, it (weakly) increases the cost for patrolling,  $c$ . As effort becomes more costly, police patrol less. This reduces deterrence of crime, (weakly) increasing the crime rate.

Second, it induces greater court/prosecutor scrutiny over arrests. One way to



interpret this is an increase in  $\rho_c$ —the courts become more accurate in assessing the true status of the defendant.

We begin by characterizing comparative statics with respect to police effort and the two equilibrium outcomes (arrest rate and crime rate) for each parameter in isolation. We then characterize comparative statics with respect to a simultaneous shock to both  $c$  and  $\rho_c$ , which we view as consistent with our intervention.

**Proposition 2.** *An increase in  $c$ , the cost of patrolling:*

1. *Weakly decreases the rate of patrol.*
2. *Weakly increases the crime rate.*
3. *Can increase or decrease the rate of arrest.*

**Proof:** Inspection of (4) shows that the LHS expression is decreasing in  $c$ . For the equality to hold, it must therefore be the case that the patrol rate  $\lambda^*$  decreases as  $c$  increases. With respect to the crime rate,  $c$  enters (5) implicitly through  $\lambda^*$ . Following the analysis above, crime decreases in the patrol rate  $\frac{\partial C}{\partial \lambda^*} < 0$ , which implies that crime increases in the cost of patrolling. Further, following the analysis above, the arrest can increase or decrease in the patrol rate  $\frac{\partial A}{\partial \lambda^*}$  can be positive or negative, which implies that the arrest rate can increase or decrease in the cost of patrolling. ■

**Proposition 3.** *An increase in  $\rho_c$ , the accuracy of the court:*

1. *Can increase or decrease the rate of patrol.*
2. *Can increase or decrease the rate of arrest.*
3. *Decreases the crime rate.*

**Proof:** Differentiation of the second order condition (4) with respect to  $\rho_c$  yields:

$$\frac{1}{a-b} \left( \frac{P\lambda^*}{(\lambda\rho_c\rho_p-1)^3} + b(\mu-1)\rho_p - \rho_p(2+4\rho_c+2\rho_p-3\lambda\rho_c^2\rho_p+\lambda^2\rho_c^3\rho_p^2)+2\lambda^2\rho_c\rho_p^4+ \right. \\ \left. \mu(2+(-6-3\lambda+4\rho_c)\rho_p+(2+\lambda^2\rho_c-3\lambda(-3+\rho_c^2))\rho_p^2+\lambda(-6+\lambda\rho_c(-3+\rho_c^2))\rho_p^3) \right)$$

This expression can be negative or positive. For example, suppose that  $a = -1, b = 3, \rho_c = \frac{17}{32}, \rho_p = \frac{273}{544}$ , and  $P = 1$ . When  $\mu = 1/2$ ,  $\lambda^* \approx 0.350$ . Evaluating the above expression yields  $\approx 0.159 > 0$ . In contrast, when  $\mu = 1$ ,  $\lambda^* \approx 0.501$



and the above expression yields  $\approx -0.001 < 0$ . Finally, consider the effect of  $\rho_c$  on crime rates.

$$\frac{\partial \mathcal{C}}{\partial \rho_c} = \frac{P(1 + \lambda^*(\rho_p - 1)\rho_p)}{(a - b)(\lambda^*\rho_c\rho_p - 1)^2} < 0,$$

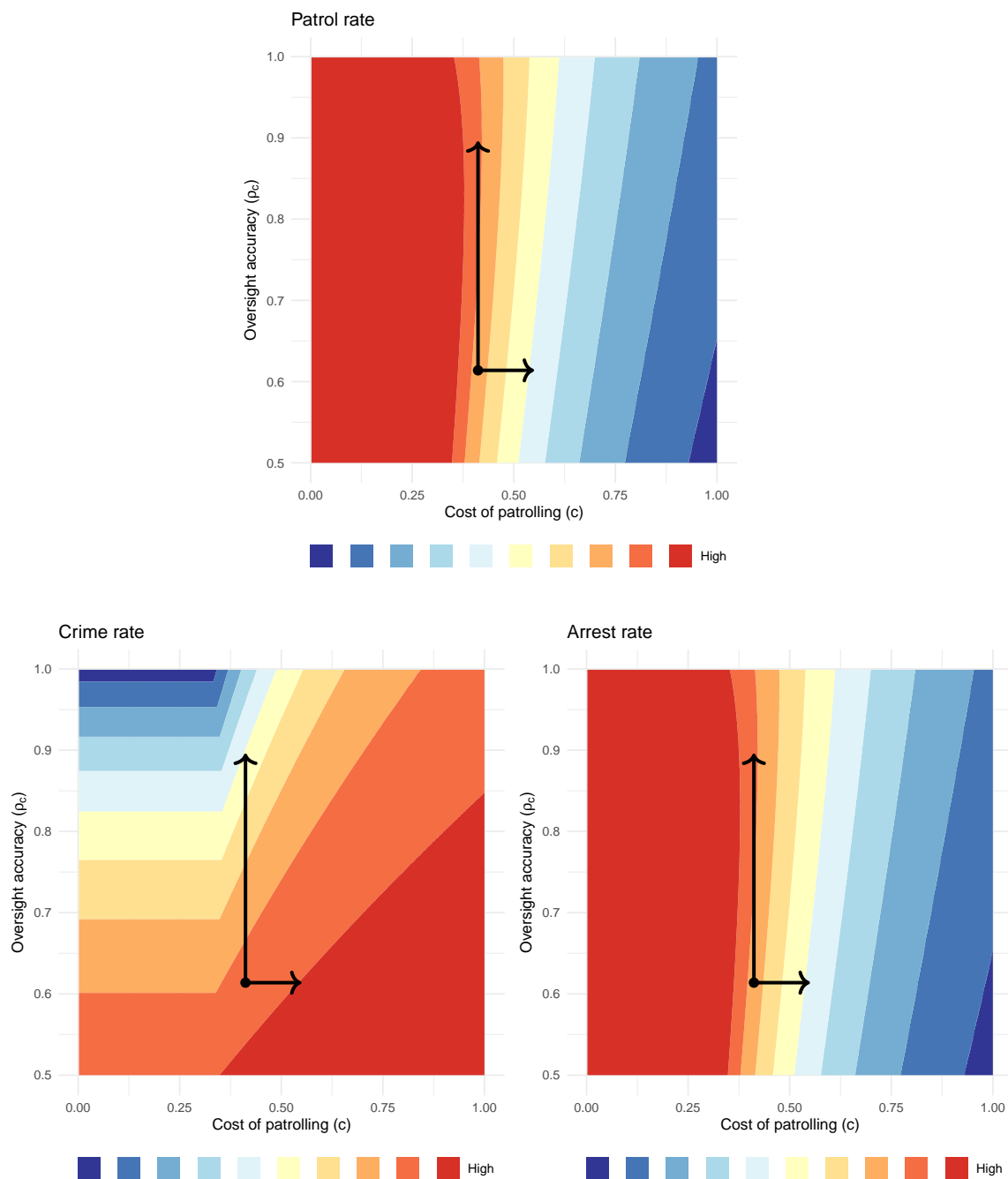
which holds for any  $\lambda^* \in [0, 1]$ . This implies that the deterrent effect on crime holds regardless of whether patrolling increases or decreases in  $\rho_c$ . ■

## A.5 Graphical Intuition

To understand the ambiguous effects that come from a simultaneous increase in  $c$ , the cost of patrolling, and  $\rho_c$ , the accuracy of the court, Figure [A.14](#) show how simultaneous increases in  $c$  and  $\rho_c$  can generate different-signed effects.



Figure A.14: Contour plots to show how patrol rates, crime rates, and arrest rates change in the cost of effort ( $c$ ) and the accuracy of the court ( $\rho_c$ ). For all plots,  $a = -25, b = 25, \rho_p = 0.7, P = 1$ , and  $\mu = 0.5$ .



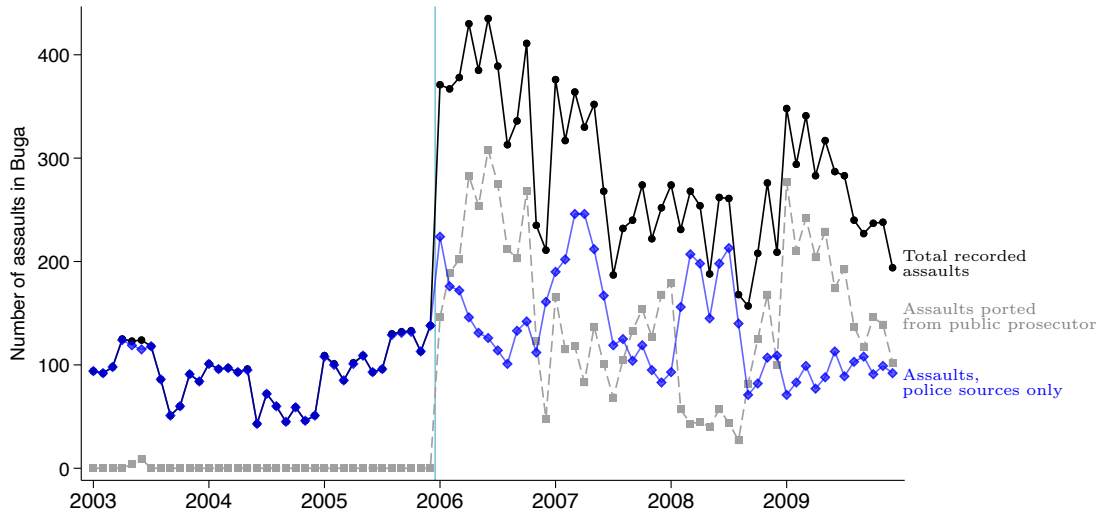


## B A change in crime registration

As mentioned in the main text, police began including new sources of crime counts in SIEDCO over the course of the rollout of the new code of criminal procedure. In the mid-size city of Buga, for example, SIEDCO previously recorded only those assaults reported to the police; coincident with the new code, SIEDCO also incorporated assaults reported to the public prosecutor's office. Ignoring this change in SIEDCO crime registration makes it look as if the number of assaults in Buga tripled within one month under the new code (see Figure B.15). The blue line in this figure plots our attempt to correct for this change by using only crime counts from internal police sources.

Figure B.15: Partial correction for a change in crime registration

The black line plots the total number of assaults recorded in the police database SIEDCO. The gray line plots the number of assaults recorded in SIEDCO that were originally reported to the public prosecutor; in 2006, in Buga, the police began incorporating the public prosecutor's data into SIEDCO, creating the illusion of a sharp increase in assault. The blue line plots a count of assaults using internal police sources only.

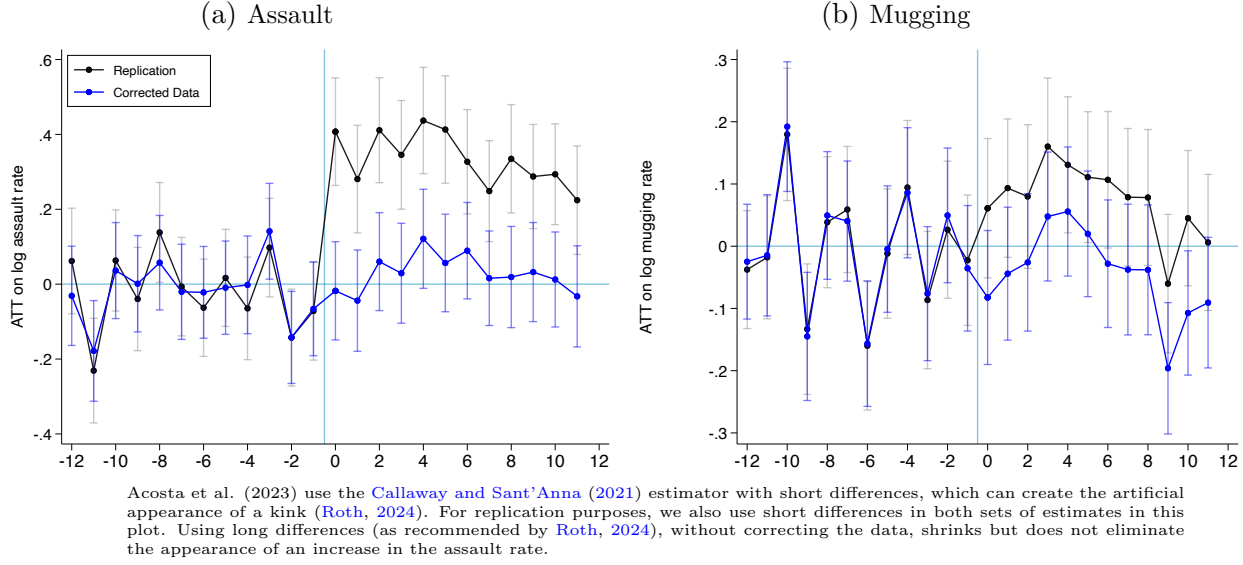


In fact, we consider two different definitions of “police sources only.” The first includes only sources that appear to be internal to police in all judicial districts, of which the biggest categories are *por información policiva*, *vigilancia* and *inspección policia*. This is the measure that we use in Figure B.16, which shows that the original result is an artifact of including these new sources of crime registration.



Figure B.16: No effect of the new code on assault or mugging

The black lines plot our replication of [Acosta Mejía et al. \(2023\)](#) (Figures A7 and A8), who find that the new code caused a large increase in assaults. The blue line plots our estimate of the effect of the new code on assaults, using data corrected for a change in crime reporting.



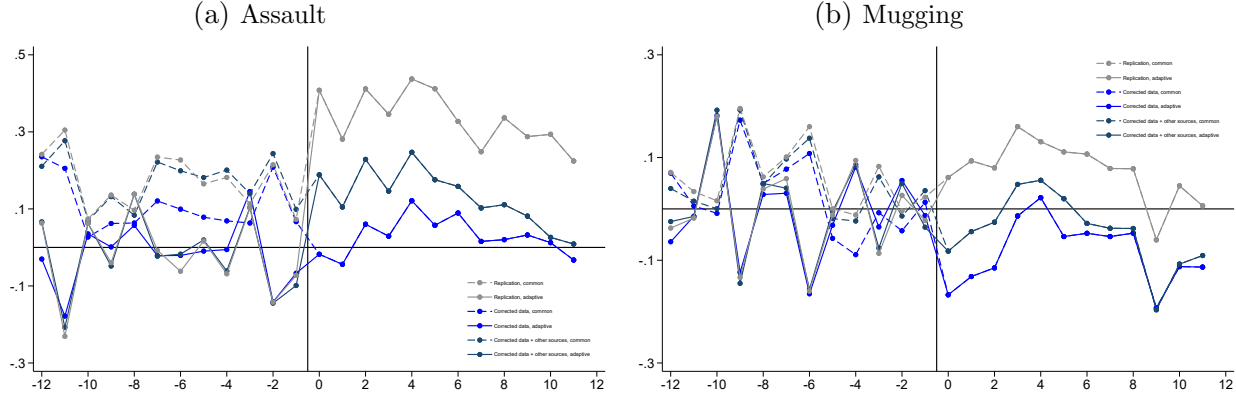
The other measure adds two additional sources: *querella* and *por denuncia*. It appears that these sources are in some places used to indicate crime reports received directly by the police, and in other places are used to indicate crime reports imported from Fiscalía. In Antioquia, for example, the *denuncia/querella* trend looks like the dotted gray line in Figure B.15: it's zero under the old code, then jumps up right in the month when the new code comes into effect. In Antioquia, unlike in Buga, there's no source called *fiscalía*, and one officer who worked in the data unit told us that the labels *denuncia/querella* could be used to indicate crime reports coming from *fiscalía*. That's why we do not include *denuncia/querella* in our principal measure in Figure B.16: it should create the artificial appearance of a jump in assaults. The problem with excluding *denuncia/querella* is that, in four judicial districts (Bogotá, Cúcuta, Sincelejo, and Villavicencio), it does *not* enter at the same time as the new code but rather exists throughout the 2003–2009 period. Excluding *denuncia/querella* in these places therefore strikes us as problematic.

For that reason, we repeat the analysis with this second measure. As expected, the measure that includes *denuncia/querella* does produce the appearance of a jump in assaults when we use the original [Acosta Mejía et al. \(2023\)](#) specification



Figure B.17: Effect of the new code on crime: Alternate measures

This figure shows that even an alternate measure of assault and/or mugging—one that, as explained in this appendix, we consider overly inclusive—does not suggest that the new code affected these crime rates, as long as ATTs are constructed symmetrically in the pre and post periods (Roth, 2024).



(see the darker blue solid line in Figure B.17a). But that apparent increase, we believe, is an artifact not only of including sources that jump from zero to a significant number exactly as the new code comes into effect but also of the problem outlined in Roth (2024): the software packages for these figures by default estimate horizon-specific ATEs asymmetrically for negative and positive horizons, which creates the artificial appearance of a kink at the treatment date. Merely switching to a symmetric formula, as recommended by Roth (2024), eliminates the appearance of a jump when the new code comes into effect (see Figure B.17a, darker blue dashed line)—even when using the more inclusive measure of “police sources only.”

Finally, for completeness, Table B.1 reports the estimates that correspond to these figures (using the Callaway and Sant’Anna, 2021, estimator). Neither our principal measure nor the expanded measure reveal evidence of an effect on these crime rates.



Table B.1: Estimates of the effect of the new code on assault and mugging rates

	Assault rate		Mugging rate	
	Replication	Corrected	Replication	Corrected
<b>Panel A: Principal measure of “police sources only”</b>				
Effect of new code	0.263 (0.098)	-0.074 (0.091)	0.068 (0.083)	-0.099 (0.062)
Pre-period mean	0.796	0.697	0.506	0.423
Observations	91,980	91,980	88,116	88,116
<b>Panel B: Expanded measure of “police sources only”</b>				
Effect of new code	0.263 (0.098)	-0.004 (0.111)	0.068 (0.083)	-0.079 (0.062)
Pre-period mean	0.796	0.755	0.506	0.488
Observations	91,980	91,980	88,116	88,116



## C Additional results on the decline in arrests

Figure C.18: The New Code and Arrest Rates in Four Cities (Examples)

This figure plots raw arrests per 100,000 people per year in four cities (one in each wave). The pale red lines mark the Januaries when the new code came into effect in each place; the darker red lines mark the preceding Novembers (when we observe anticipation effects).

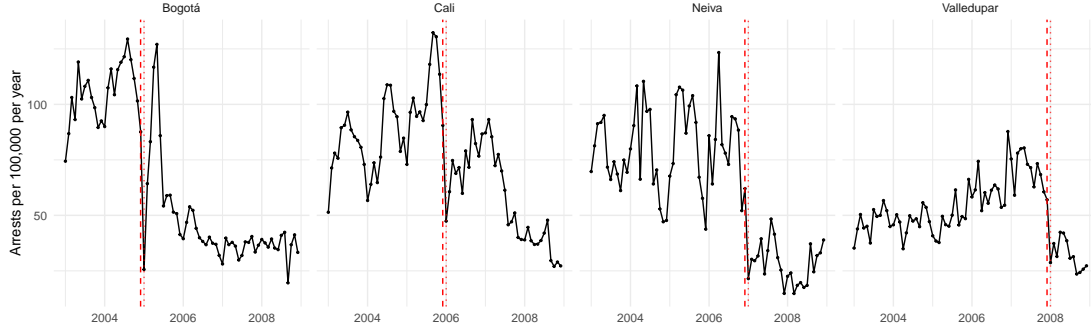
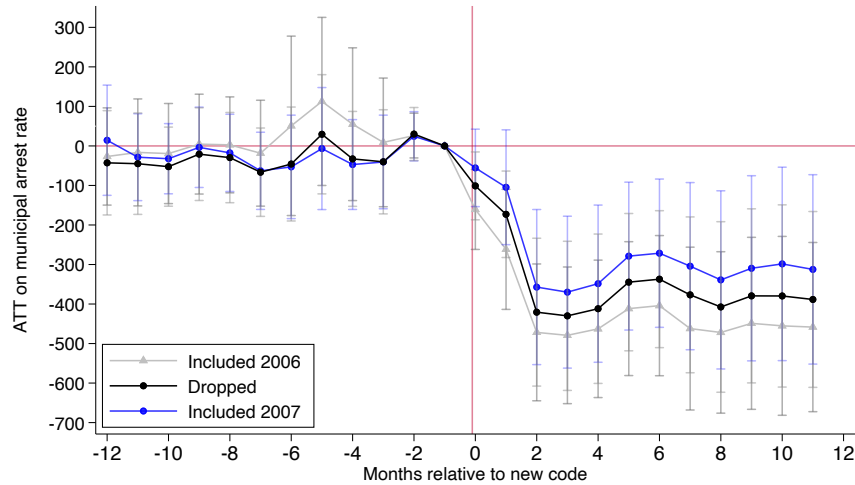


Figure C.19: Including or dropping Antioquia does not substantively change the results.

Our main results in Figure 5 drop the Antioquia judicial district given uncertainty about the timing of reform implementation. Its inclusion does not substantively change our findings with respect to the decline in arrests.



Using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#). Standard errors clustered by judicial district.



Table C.1: Estimates of the decline in arrests, using different estimators

	Municipal level				Judicial district level					
	Arrest rate				Homicide		Drug		Vandalism	
	Rate	Rate	Rate	Log	Rate	Log	Rate	Log	Rate	Log
<b>Estimates from De Chaisemartin and d'Haultfoeuille (2020) (reported in main text)</b>										
Effect of new code	-345.81 (88.66)	-431.28 (68.87)	-378.98 (91.89)	-0.47 (0.09)	-1.28 (2.49)	-0.02 (0.16)	-72.79 (39.94)	-0.62 (0.18)	-64.35 (25.69)	-1.03 (0.27)
Observations	31,056	31,056	972	972	972	972	972	972	972	972
Pre-period mean	570.78	901.25	744.22	6.48	18.06	2.76	159.22	4.55	54.64	3.21
<b>Estimates from Borusyak et al. (2024)</b>										
Effect of new code	-295.03 (24.80)	-414.90 (36.64)	-473.11 (120.89)	-0.48 (0.09)	-3.78 <sup>§</sup> (2.27)	-0.13 <sup>§</sup> (0.10)	-143.32 (58.22)	-0.58 (0.16)	-79.57 (29.95)	-1.19 (0.28)
Observations	41,314	41,314	1,303	1,303	1,303	1,303	1,303	1,303	1,303	1,303
<b>Estimates from Callaway and Sant'Anna (2021)</b>										
Effect of new code	-340.33 (26.56)	-337.90 (2,624,060.50)	-369.81 (94.28)	-0.45 (0.09)	-0.86 (2.89)	0.03 (0.18)	-70.53 (41.01)	-0.60 (0.19)	-60.93 (21.70)	-1.03 (0.27)
Observations	53,556	53,556	1,694	1,694	1,694	1,694	1,694	1,694	1,694	1,694
Population weights	✓									

Standard errors clustered at the municipal level for columns 1 and 2, and at the judiciary district for the rest.

<sup>§</sup> When studying the homicide arrest rate, estimates from Borusyak et al. (2024)'s estimator do not appear to follow parallel pre-trends; moreover, because the pre-trend estimates rely on the earliest time periods as a reference group, we include unit-specific linear trends in Panel B, Columns 5–6. Excluding them yields a somewhat larger negative point estimate of  $-0.25$  (for Column 6).

Figure C.20: Arrest Rates Decline in 78% of Municipalities

This figure plots the distribution of pre-post changes in the arrest rate across Colombian municipalities, comparing the twelve months before the reform comes into effect to the twelve months after.

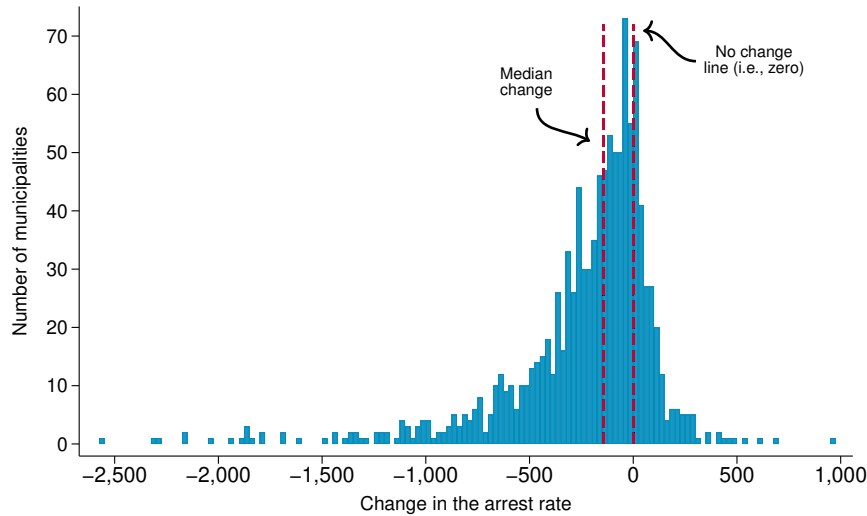




Figure C.21: Warrants Seldom Used for Minor Offenses.

This figure plots the share of arrests for common charges with and without a warrant nationwide in 2003 and 2004 (during the pre-treatment period). A charge is common if there were more than 1,000 arrests for that charge nationwide in any year between 2003 and 2008.

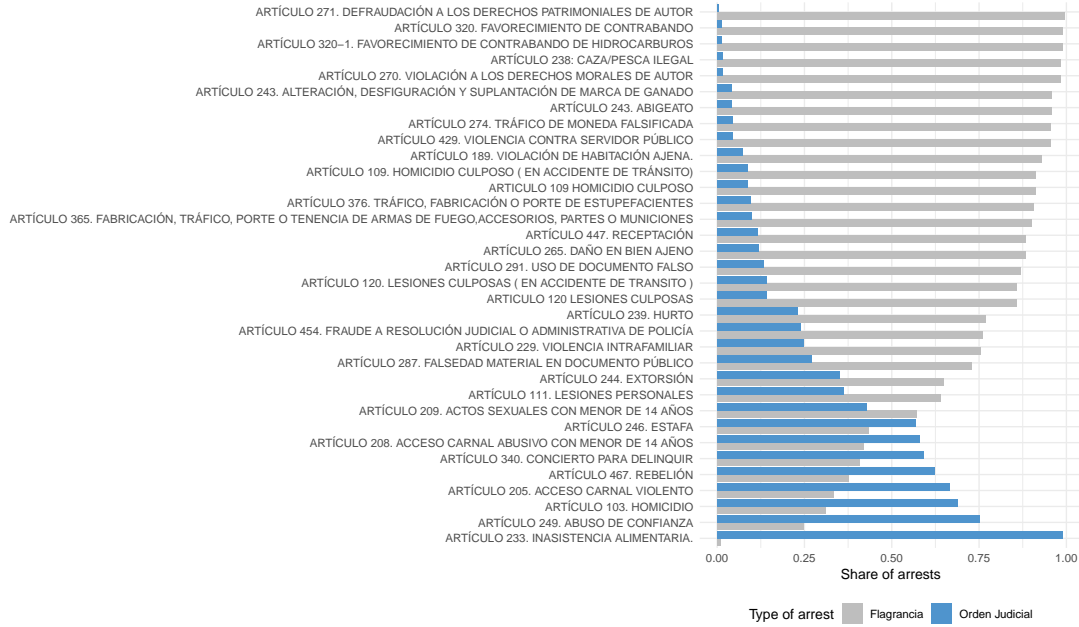
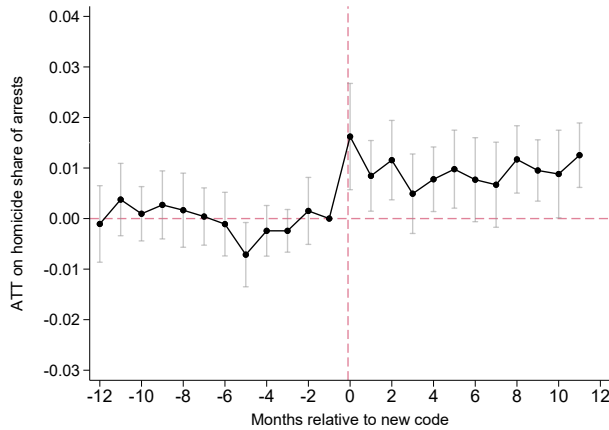


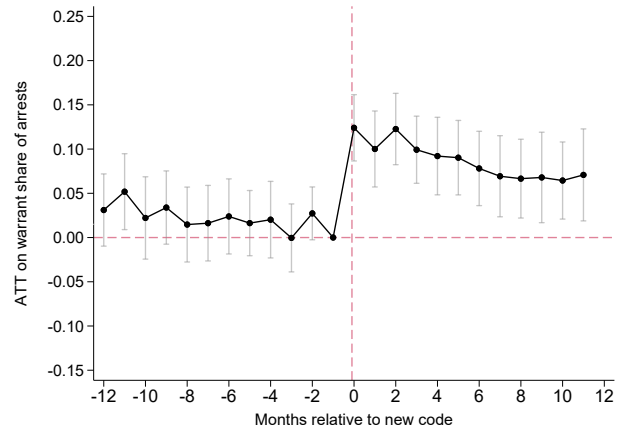
Figure C.22: The Effect of the New Code on the Composition of Arrests.

Reform increased the share of arrests for homicide (by reducing arrests for more minor offenses) and increased the share of arrests made with warrants (by reducing the share of warrantless arrests).

(a) Homicide share of arrests



(b) Warrant share of arrests

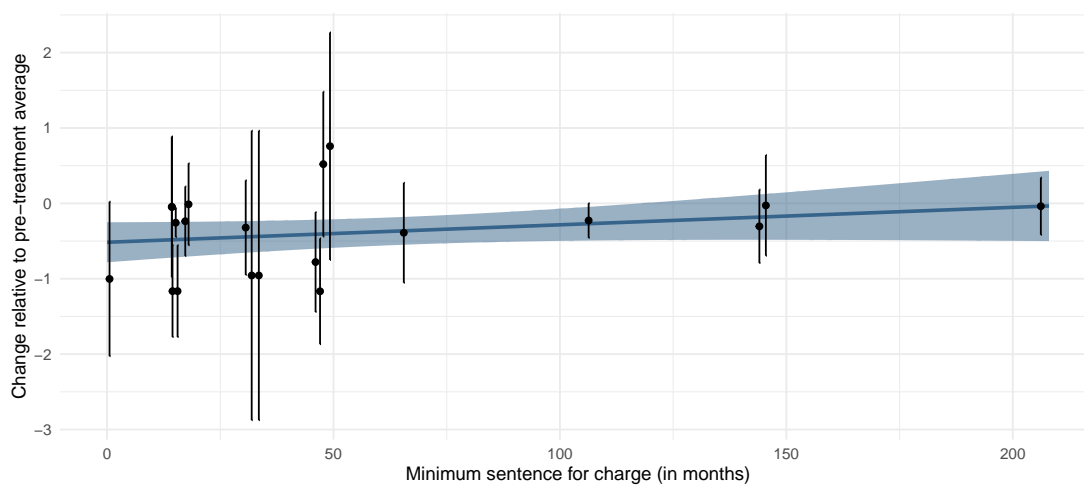


Using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#).



Figure C.23: Arrest Rate Declines Less for More Serious Crimes.

For each common charge (as defined in Figure C.21), we estimate the ATT of the reform using the estimator proposed by [De Chaisemartin and d'Haultfoeuille \(2020\)](#). We then plot these ATTs as a function of the minimum sentence for that offense, in months. Thus the severity of crimes (as defined by the Colombian penal code) is increasing on the  $x$ -axis. The blue line is estimated by random effects meta-regression in which we model the ATTs as a function of this measure of crime severity. The increasing slope suggests that the arrest rate declines less for more severe crimes.





## D Additional evidence of the effect of the new code on homicide rates

Figure D.24: The New Code and Homicide Rates in Three Cities

These figures plot the homicide rate in three cities. The pale red lines mark the Januaries when the new code came into effect in each city; the darker red lines mark the previous Novembers, in light of the fact that arrest rates began falling in anticipation.

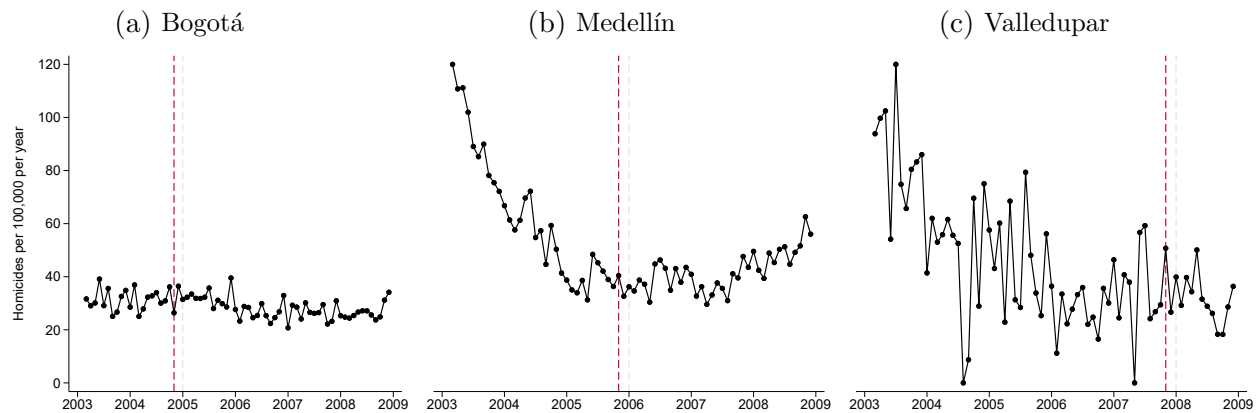
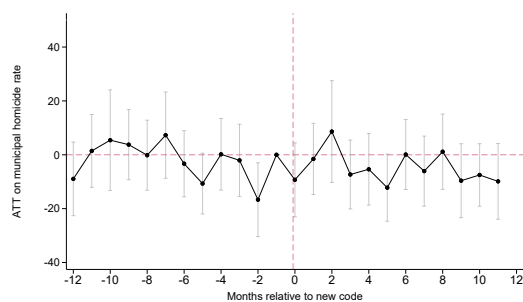
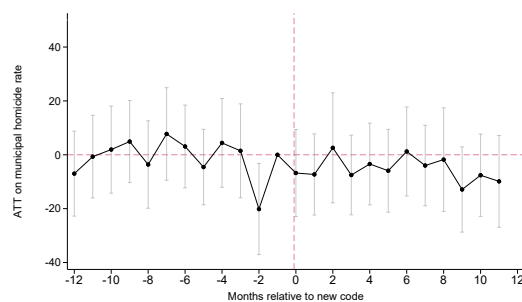




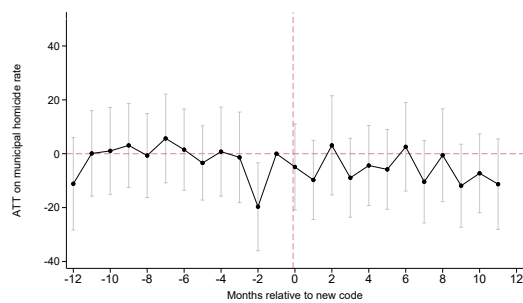
Figure D.25: Effect of the new code on homicide rates, excluding municipalities with significant war violence



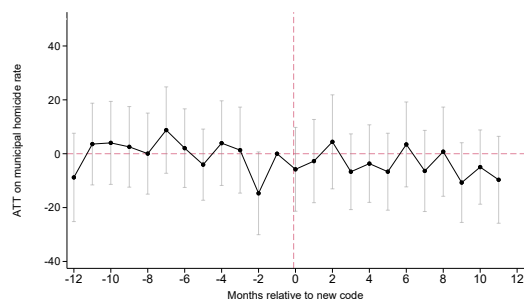
(a) Full Sample



(b) Ratio smaller than 0.05 (-482 municipalities)



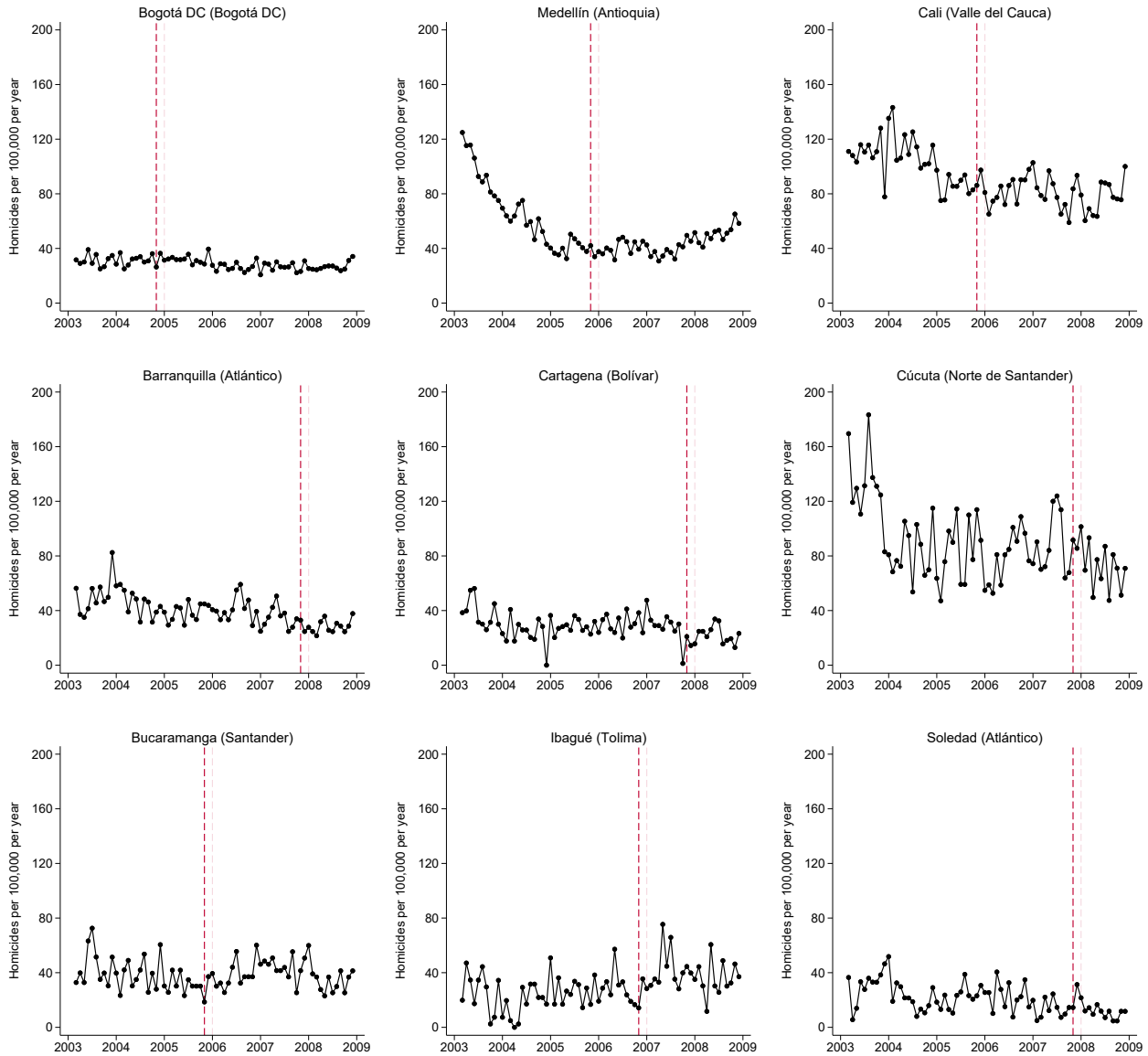
(c) Ratio smaller than 0.15 (-344 municipalities)



(d) Ratio smaller than 0.25 (-266 municipalities)



Figure D.26: Homicide Rates in Colombia's Largest Cities





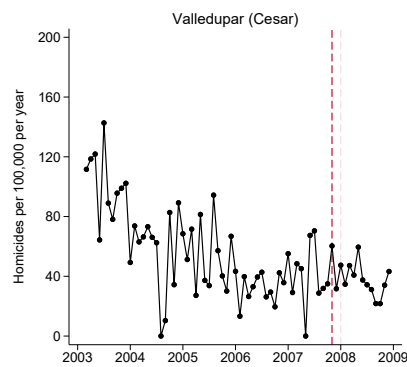
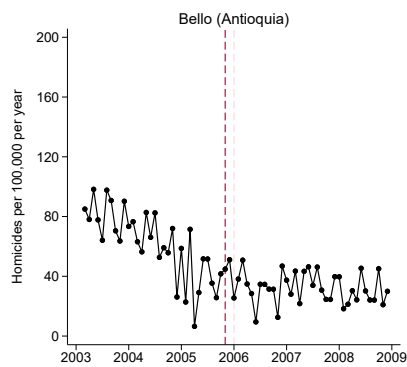
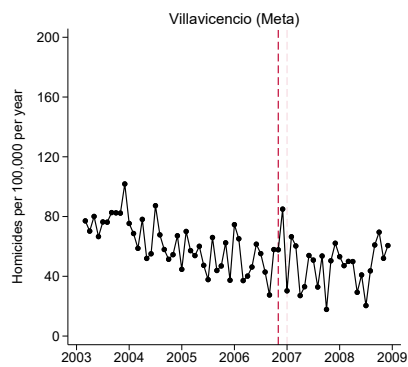
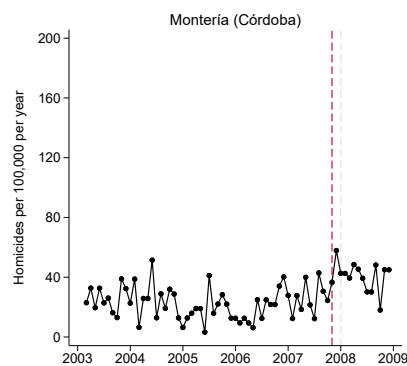
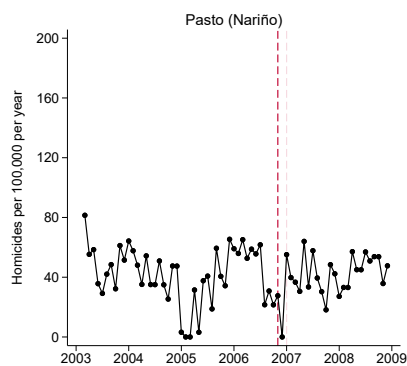
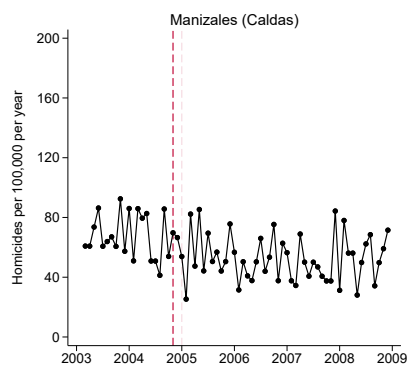
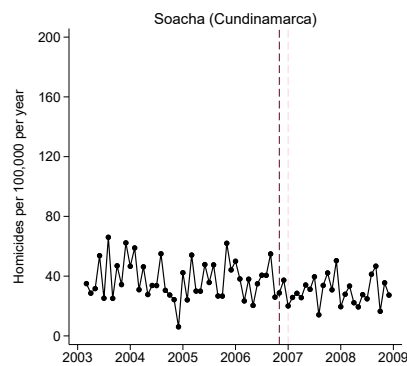
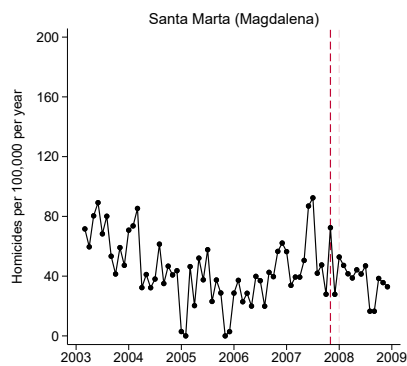
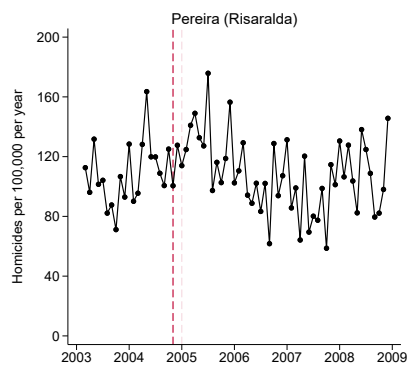
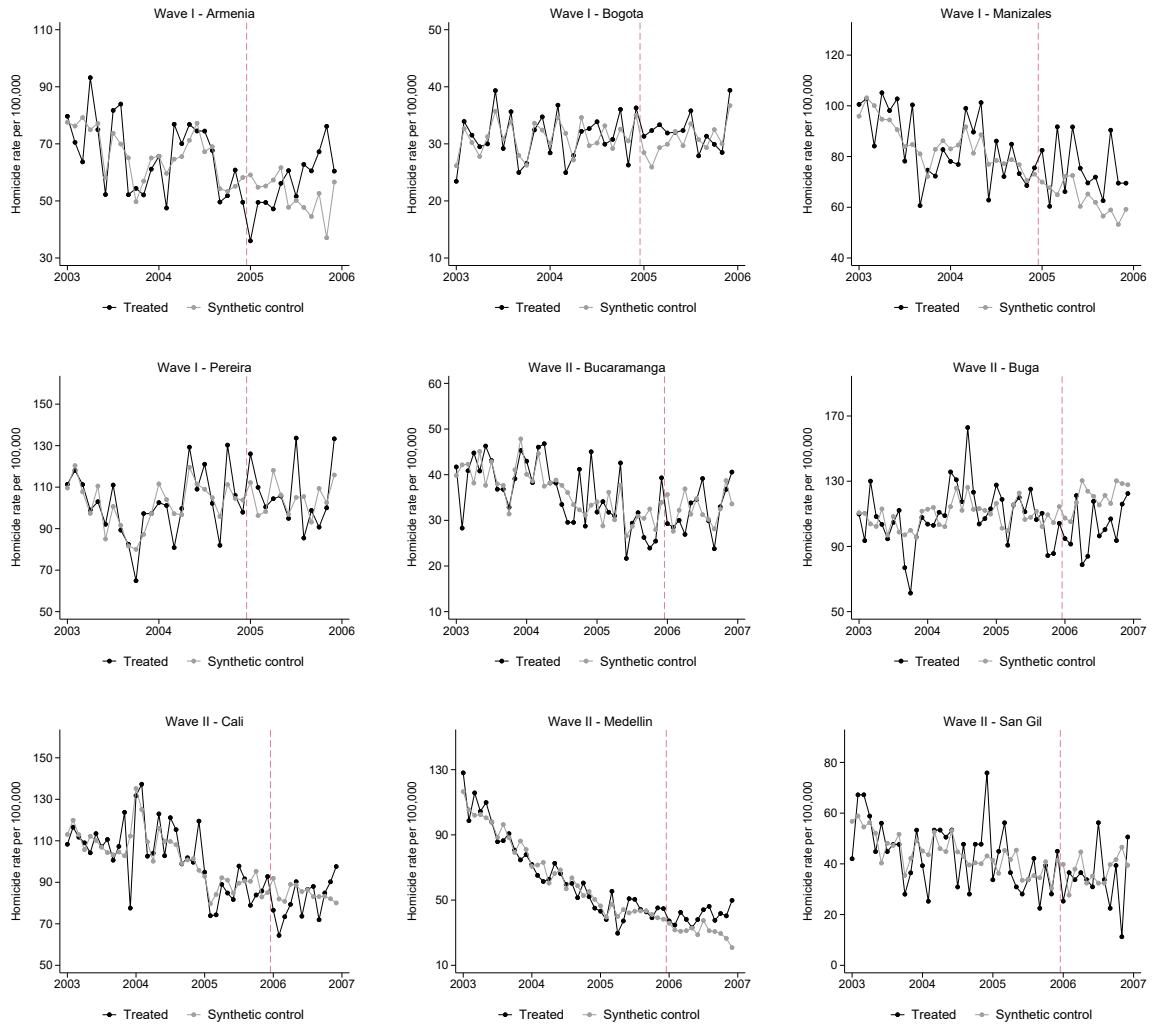
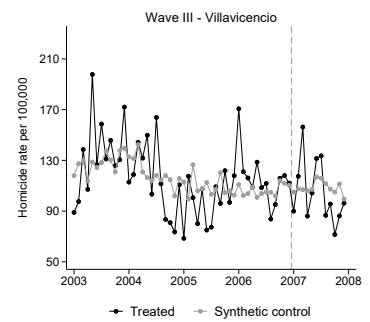
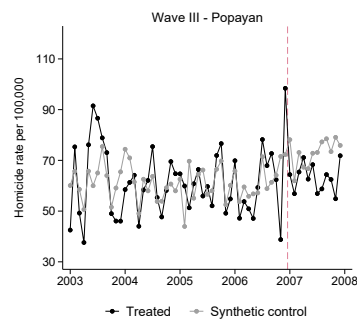
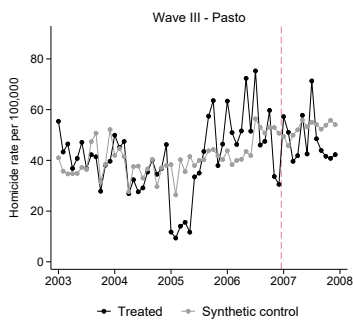
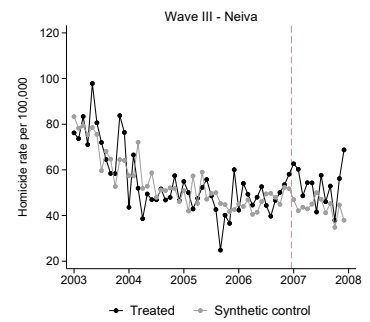
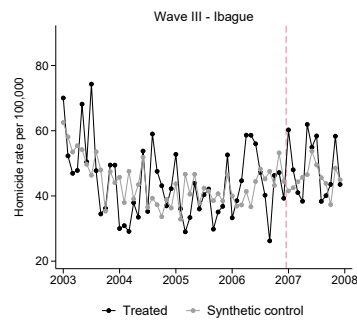
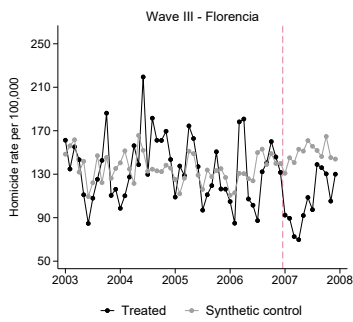
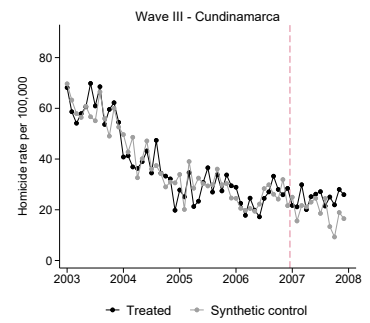
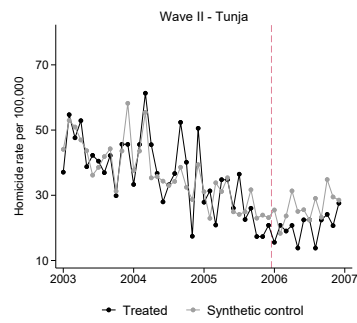
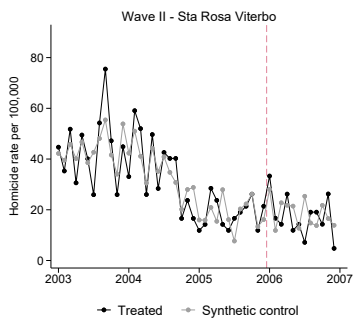




Table D.1: Synthetic Control by Judicial District









## E Vehicle theft

The data on vehicle theft (from insurers) is so sparse that just three cities contain 78% of insured vehicles: Bogotá, Cali, and Medellín. The vehicle theft rates in those three cities (vehicle thefts per 1,000 insured vehicles per year), which we plot in Figure E.28, leave open the question of whether the new code caused an increase in vehicle theft. The panel is so sparse that difference-in-differences estimates are highly unstable across reasonable specifications.

Figure E.28: The Effect of the New Code on Vehicle Theft

The gray lines mark the rate of vehicle thefts per 1,000 insured vehicles per year, according to data that we obtained from the Colombian association of insurers (Fasecolda). Black lines mark polynomial fits to the theft-rate trend in all years except the first year of the new code. Blue lines mark the rate of vehicle-theft arrests.

