On the Tension Between Due Process Protection and Public Safety: The Case of an Extensive Procedural Reform in Colombia

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Abstract

We study how large procedural reforms reshape deterrence by tracing equilibrium adjustments across multiple margins of the criminal justice system. Using the staggered rollout of Colombia's comprehensive adversarial criminal procedure reform in a civil-law setting (2005-2008), we estimate both intended administrative effects and unintended effects on enforcement and crime. The reform aimed to strengthen due process protections, reduce reliance on pretrial detention, accelerate case processing, and expand early termination mechanisms. We find that it achieved most of the objectives: pretrial detention declined by 13-28%, procedural duration fell by 19%, settlements increased by 41-65%, and adjudication rose substantially. At the same time, the reform reduced enforcement activity and case progression: arrest rates fell by 39% and clearance rates by 16-25%. Consistent with a reduction in the certainty component of deterrence, crime increased after implementation; property crime by 29% and violent crime by 15%. These findings highlight a key implication of adversarial procedural redesign: reforms that improve internal efficiency can alter enforcement and charging behavior in ways that affect public safety. More broadly, they underscore the importance of evaluating procedural reforms as bundled institutional changes and measuring their effects along the full case-processing pipeline, from complaint to case resolution, rather than inferring mechanisms from crime outcomes alone.

Keywords: Criminal procedural reform; pretrial detention; due process protection; clearance rates; crime

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I. Introduction

The literature on how criminal law affects crime has focused primarily on how changes in substantive penal law, such as sentencing rules, incarceration, and rehabilitation policies, alter the expected costs and benefits of offending. This work has been central to understanding how the criminal justice system shapes incentives to offend through sentencing (Dominguez-Rivera et al., 2019; Owens, 2009), incapacitation (Liedka et al., 2006; Lofstrom & Raphael, 2016), and rehabilitation (Escobar et al., 2023; Gaes & Camp, 2009; Tobón, 2022).

By contrast, we know much less about how procedural law, that is, the rules governing investigation, charging, adjudication, and pretrial detention, shapes crime. Existing research examines specific procedural margins, such as higher judicial productivity (Soares & Sviatschi, 2010), due process protections (Atkins & Rubin, 2003), and reductions in pretrial detention (Cepeda-Francese & Ramírez-Álvarez, 2023; Heaton et al., 2017; Leslie & Pope, 2017). However, procedural reforms are rarely narrow, isolated interventions. Major procedural redesigns typically bundle multiple institutional changes: reallocating authority across police, prosecutors, and judges; altering evidence production and timing constraints; and expanding or eliminating non-trial resolution pathways. These bundles can shift the likelihood that crime reports translate into arrests, charges, and convictions (certainty), pretrial detention rules and effective sanction intensity (severity), and time of adjudication (celerity) in different, and potentially offsetting, directions.

In this paper, we study the effects of criminal procedural reforms across multiple margins of adjustment of the criminal justice system. Rather than focusing on any single outcome in isolation, we evaluate the net impact of procedural change on the certainty, severity, and celerity of punishment by tracing how criminal justice actors re-optimize under new constraints and incentives. Procedural reforms can accelerate case processing, expand negotiated exits, and reduce detention, but those same changes can also shift enforcement efforts, prosecutorial screening, and the probability that reported offenses move from complaint to charge and then to conviction. Because these margins are jointly determined, the welfare-relevant effects of procedural reform cannot be inferred from any single margin; they must be evaluated by linking administrative outcomes to enforcement and crime outcomes within a unified framework.

A particularly consequential class of procedural reforms observed across many civil-law jurisdictions over the past several decades is the shift toward adversarial criminal procedure, in which oral hearings and prosecutorial-led investigations play a central role relative to traditional dossier-based processing (Damaska, 2001; Langer, 2007a, 2007b, 2014). These reforms are analytically useful for our purposes because they are typically bundled interventions: they change the form of adjudication (oral hearings versus written dossiers), reallocate authority across prosecutors and judges, expand early-termination mechanisms, and often alter the role and feasibility of pretrial detention. As a result, they create a multi-margin environment in which the net deterrence effect is theoretically ambiguous and must be assessed empirically. However, crime outcomes alone are not enough to characterize the new equilibrium: the same increase (or decrease) in crime could arise from changes in punishment severity driven by heightened due process protections, or from endogenous re-optimization by prosecutors and police who alter charging, detention requests, and case selection when procedural requirements change. Tracing simultaneous changes in charging, detention requests, negotiated exits, adjudication, and enforcement activity is, therefore, necessary to interpret crime effects as equilibrium responses to procedural redesigns rather than as uninterpreted outcomes.

Despite the importance of adversarial reforms, systematic evaluations of their net effects are limited, in part because bundled procedural changes are difficult to study with credible counterfactuals. This paper addresses that gap by evaluating a major adversarial procedural transformation that is representative of the broader "adversarial revolution" in civil-law settings (Langer, 2007a). Adversarial reforms are often motivated by a common set of objectives: increasing judicial productivity and reducing congestion through early-termination mechanisms (Martínez Cuéllar et al., 2008), strengthening due process protections, and reducing reliance on pretrial detention (Carranza, 2001; Duce et al., 2009; Hartmann Arboleda, 2016; McLeod, 2010) in contexts where delays and perceived impunity are salient (Dammert, 2012). At the same time, the implied deterrence effect is ambiguous ex ante, because the same reform can shift certainty, severity, and celerity in offsetting ways. For example, productivity gains may reflect greater prosecutorial selectivity (fewer cases pursued to charge), reducing clearance and raising incentives to offend. Alternatively, reductions in pretrial detention can lower crime in the long run but raise it in the short run (Chalfin & McCrary, 2017; Leslie & Pope, 2017). Finally, expanded early-termination mechanisms can reduce expected severity through sentence discounts while increasing the discounted cost of offending by shortening the time to conviction. The sign of the net effect is therefore an empirical question.

We study these tradeoffs through Colombia's adversarial reform, a setting that is unusually well-suited for identifying the effects of procedural redesign as a bundle. The reform was implemented through a staggered rollout between 2005 and 2008 that divided the country into four implementation stages, allocating roughly one quarter of the population to each stage. This staged design introduced an important quasi-experimental variation in exposure during the transition, enabling credible comparisons of municipalities before and after implementation. Colombia is also a unitary country, so substantive criminal law is uniform nationwide, and penal legislation is centrally determined, limiting cross-municipality legal heterogeneity that could otherwise confound the effects of procedural change. The shift from dossier-based processing to oral hearings before an impartial judge, alongside prosecutorial-led investigations (Damaska, 2001; Langer, 2007b, 2014), provides a concrete instance of *adversarialization* within a civil-law system.

We combine administrative data from prosecutors and courts with police data to measure the reform's effects along the full case-production process. On the institutional side, we track procedural timelines, charging activity (imputations), early-termination mechanisms, adjudication outcomes, and pretrial detention. On the enforcement and public-safety side, we examine arrests, clearance, and crime rates. This integrated measurement is central to our approach because it allows us to test whether administrative "productivity" gains coincide with changes in selection and enforcement that alter the certainty of punishment, and to interpret crime responses as the net equilibrium effect of simultaneous adjustments across margins.

Our empirical strategy exploits the staggered rollout across the four implementation stages. We estimate event-study specifications with two-way fixed effects to assess pre-trends and characterize dynamic treatment effects. As a robustness check for these event-studies, we use the doubly robust estimator proposed by Callaway and Sant'Anna (2021). We complement these estimates with a conditional difference-in-difference specification that uses the staged implementation to identify causal impacts of the reform on the outcomes of interest.

To provide an integrated account of how the reform reshaped the administration of criminal justice and how these institutional changes affected incentives to offend, we estimate its effects along the reform's main intended margins: procedural productivity, decongestion through early-termination mechanisms, adjudication, and pretrial detention. We find substantial gains in procedural efficiency. The time between the imputation of charges and the indictment hearing fell sharply after implementation, from 556 days to 229 days, while the average time between the opening of an investigation and the imputation of charges increased modestly (from 66 to 79 days). The reform also expanded negotiated exits: the share of investigations ending in a pre-imputation settlement increased by 41.7% for property crime cases and by 65.3% for assaults, with effects that grow over time after implementation. Consistent with this shift, adjudication rates increased, driven by settlements or guilty pleas rather than by trial convictions. Finally, the reform reduced reliance on jail-based pretrial detention across most offenses: the share of charged cases resulting in jail-based pretrial detention declined by 33.8% for homicides, 17.7% for property crimes, 32% for drug-related crimes, and 34.9% for assaults. Domiciliary detention increased, but it did not fully offset the decline in detention in jail; total pretrial detention (jail plus domiciliary detention) fell significantly for homicides (27.5%) and drug-related crimes (12.6%), while the net effect is close to zero for property crimes, sexual offenses, and assaults.

At the same time, we document unintended consequences for enforcement activity and crime. The reform's redesigned procedures and tighter timelines are associated with greater selectivity in case progression and declines in clearance rates, defined as the share of reported crimes in which the prosecutor files charges before a judge, across major crime categories: 23.3% for homicides, 27.3%

for assaults, 24.7% for sexual offenses, 15.6% for drug offenses, and 24.2% for property crimes. Arrest rates also declined significantly after implementation. We also find a positive net effect on crime rates: the aggregate crime index increased by 18.7%, while violent and property crime indices increased by 15.2% and 28.7%, respectively. These effects grow over time during the first 12 months after implementation. Altogether, these findings provide empirical evidence on the direction of deterrence effects in a setting where theory predicts ambiguity due to offsetting changes in the certainty, severity, and celerity of punishment.

Our results show that a comprehensive procedural reform can deliver intended within-system improvements, such as shorter timelines, greater use of negotiated exits, increased adjudication through non-trial dispositions, and reduced pretrial detention, while also changing enforcement and case progression in ways that matter for public safety. This paper contributes to the law and economics literature on how criminal justice institutions shape incentives by documenting how structural procedural redesigns can shift deterrence through adjustments across the full case production process, from enforcement and charging to detention and disposition, rather than through changes in substantive criminal law alone. Our results underscore the importance of evaluating procedural reforms jointly across administrative margins rather than inferring mechanisms from crime outcomes alone.

The rest of the paper is organized as follows. Section II discusses the existing literature on criminal procedural reforms and crime. Section III explains the institutional context of the adversarial reform and its implementation in Colombia. Section IV describes the data used to estimate the impact of the adversarial reform on the administration of criminal justice, enforcement, and crime. Section V presents the empirical strategy. Section VI presents the main results, and Section VII concludes.

II. Criminal Procedure & Crime Rates

A growing body of literature identifies criminal procedure as a key factor even when substantive criminal law is unchanged. Procedural law has the potential to reshape the certainty of punishment (including the likelihood of arrest and conviction), the severity of punishment (including pretrial detention and effective sentence length), and the celerity of adjudication (including procedural times) (Nagin, 2013; Nagin & Pogarsky, 2003; Pogarsky, 2002). Yet, understanding how procedural changes affect crime rates remains challenging because they are rarely isolated policy interventions. Procedural reforms are often implemented as institutional redesigns that relocate authority among actors (police, prosecutors, judges, defense), change constraints on the production and admission of evidence, and transform the set of case-resolution pathways (Langer, 2007, 2021). As a result, even when procedural reforms are motivated by a single objective, such as improving efficiency or strengthening due process protection, they can generate equilibrium adjustments across multiple margins of the criminal justice system, making the expected effect on crime rates theoretically ambiguous ex ante.

A useful starting point for studying the effects of procedural reforms on crime is to organize mechanisms around changes in certainty, severity, and celerity, treating them as system-level outcomes that emerge after all actors react to the reform. Procedural reforms modify the constraints that govern investigation, charging, and adjudication, altering the incentives under which police, prosecutors, judges, defense counsel, and complainants operate. One implication is that reforms can affect crime by changing the workflow of the criminal process: increases in judicial productivity can raise the expected probability of punishment and generate deterrence, with empirical evidence suggesting crime reductions when productivity gains translate into higher punishment risk (Soares & Sviatschi, 2010). However, productivity gains can also reflect selection (i.e., processing fewer cases more quickly); hence, the net effect depends on how the reform reshapes which offenses and suspects are investigated and remain in the criminal process.

Selection and substitution can arise at multiple stages of the criminal process. Prosecutors may become more selective about which cases they convert into formal charges; police may reallocate effort toward offenses that become easier to prove under new evidentiary or timing constraints; and defendants may adjust their bargaining behavior as the menu of negotiated dispositions expands. Evidence from fast-track procedural changes suggests that lowering the processing cost of minor offenses can redirect prosecutorial and policing resources toward streamlined tracks and away from more serious or more difficult cases (Dusek, 2015). In parallel, due-process-oriented procedural reforms can shift both certainty and severity by tightening the conditions under which the state may detain or incarcerate defendants, thereby affecting deterrence and incapacitation even when substantive criminal law remains unchanged (Atkins & Rubin, 2003). Consequently, reforms may increase adjudication rates and speed, conditional on cases reaching court, while reducing the likelihood that crime reports translate into arrests, charges, and convictions. The net impact on certainty, severity, and celerity, therefore, depends on how criminal justice actors re-optimize under the new constraints and incentives.

Adversariality has been part of a broader shift toward efficiency-driven procedures that reconfigure institutional roles and incentives, often expanding streamlined and non-trial tracks that can run in parallel with the intended adversarial model and, in practice, concentrate discretion in the prosecutor's office (Langer, 2007, 2021). At the same time, these reforms have been closely tied to due-process agendas and legitimacy debates. Adversarial reforms have been inspired by Anglo-adversarial ideals and adapted to local institutions, with the stated objective of strengthening fairness and transparency, while simultaneously relying on simplified procedures to keep case processing functioning at scale. As a result, in most countries adopting these reforms, the criminal process reflects a dual reality in which the courtroom-centered adversarial model applies to a relatively small share of cases, while most cases move through alternative routes that can consolidate prosecutorial power (Lewis, 2009).

Within the literature on adversarial transitions, researchers have typically focused on particular outcomes, most commonly crime rates and enforcement activity, rather than treating the reform as an institutional redesign that reconfigures the entire criminal process from initial reporting to final disposition. This narrow focus is understandable given data constraints and the complexity of these reforms. Nevertheless, studying isolated outcomes makes it difficult to fully characterize deterrence. In this sense, the relevant question about adversarial procedural reforms is not whether a single outcome "improves" or "changes," but how these reforms reshape the joint production of certainty, severity, and celerity across the sequence of interactions between complainants, police, prosecutors, defense, accused individuals, and judges.

Evidence from Mexico's adversarial reform illustrates both the value and the limits of the existing empirical work in this area. Exploiting the staggered adoption of the accusatorial reform, Amuedo-Dorantes and Ibarra-Caton (2025) link implementation to reductions in arrests and increases in homicides, and document concurrent changes in reporting behavior and trust in police and prosecutors, with stronger effects in municipalities exposed to organized criminal violence. Their analysis is informative about the reduced-form relationship between procedural reform and crime, and highlights plausible channels operating through reporting and cooperation. However, because the focus is on crime outcomes and a limited set of intermediate margins, it remains unclear how implementation reshaped prosecutorial screening, charging decisions, and the internal allocation of judicial resources that determine which cases move from crime reports to imputations, indictments, and convictions.

A complementary approach is developed by Cepeda-Francese and Ramírez-Álvarez (2023), who evaluate Mexico's transition to an adversarial procedural model using municipality-level administrative data and a generalized synthetic control strategy. They find that the reform increased homicides and coincided with reductions in the use of pretrial detention for property crimes in early-adopting municipalities, with substantially larger homicide effects in places with an established organized-crime presence. The authors also present evidence consistent with a reduced capacity to prosecute homicides effectively, using a punishment-rate proxy based on indictments per murder. Importantly for interpretation, they emphasize that identifying the reform's net effect is especially difficult when implementation overlaps with a security crisis and when the system must adjust under binding capacity constraints. However, even this broader accounting, covering crime, detention, and celerity, still leaves open how the reform altered the case-processing pipeline within prosecution and adjudication, including how prosecutors sorted cases across charging, negotiated dispositions, and trial, and how these choices interacted with judicial timelines and evidentiary standards. Studying these changes as institutionally redesigned packages matters because a procedural reform can increase speed and adjudication conditional on reaching court, while reducing clearance and formal charging through heightened selectivity, mechanisms that cannot be recovered from crime outcomes alone.

Related evidence from Mexico reinforces that the reform's effects can operate through margins that precede the courtroom and may differ from downstream outcomes. Blanco (2012) finds that the adversarial reform reduced victimization while lowering perceived security, and documents heterogeneous changes in institutional trust, reporting, and investigative outcomes across places and agencies. Taken together, the Mexican evidence suggests that the transition to an accusatorial system can simultaneously affect reporting and cooperation, enforcement activity, and internal case processing; consequently, the overall effect on crime depends on how the reform reshapes the full sequence from the initial crime report to case resolution.

In the same vein, evidence from Peru underscores the importance of time horizons and heterogeneity when interpreting transitions to an accusatorial system. Hernández (2019) evaluates the phased implementation of Peru's New Code of Criminal Procedure using a matched difference-in-differences design that contrasts early- and late-adopting areas. He finds that the average effect on crime is modest, varies across offense types, and differs in sign, suggesting that the reform reduced some crimes while increasing others, and that these effects attenuate over time, consistent with primarily short-run impacts. This study advances the procedural-reform literature by explicitly distinguishing short- from longer-run responses. However, because the analysis is organized around crime outcomes rather than the full set of administrative margins within prosecution and adjudication, it cannot identify how the observed crime dynamics translate into changes in prosecutorial screening, charging decisions, negotiated dispositions, and the composition of cases that reach adjudication, precisely the adjustments needed to interpret joint changes in certainty, severity, and celerity.

Other recent work on Uruguay's adversarial reform highlights that the implementation period itself can be consequential, both because organizational re-optimization takes time and because transitions can generate short-run frictions. Cattaneo et al. (2022) exploit a discontinuity in the reform's entry into force and document a discrete increase in police reports for common offenses in Montevideo, alongside a large decline in imputations. The authors argue that transitional coordination problems, workload shocks, and changes in adjudication pathways complicate interpretation, underscoring that early post-implementation effects may reflect adjustment dynamics rather than a new steady-state equilibrium.

A related literature on adversarial procedural reforms emphasizes that tightening due process protections when investigative capacity remains limited can induce unintended substitution in both coercive practices and evidence production. In Venezuela, Hanson and Kronick (2026) examine a procedural change that restricted arrests and document a sharp post-reform decline in arrest rates. Evidence from Mexico similarly interprets post-reform adaptation as a response to binding constraints. Magaloni and Rodríguez (2020) suggest that due-process-enhancing reforms reduce the use of torture, while militarized security interventions increase it, and that the decline in torture is weaker in organized-crime contexts. The authors further argue that adversarial reforms can reduce reliance on torture-based confessions while increasing convictions supported by fabricated or planted evidence, consistent with strategic adaptation under weak investigative capacity and pressure to punish. Taken together, these studies help explain why reforms motivated by due-process protection goals can still generate ambiguous, or even adverse, effects on crime that cannot be inferred from any single administrative margin.

Finally, the comparative literature on adversarial reforms highlights pretrial detention and trial avoidance as central institutional mechanisms through which procedural change can shift incentives. Descriptive accounts emphasize that these reforms often seek to reduce reliance on pretrial detention and strengthen defendants' rights, yet detention policy remains politically contested and can generate backlash narratives (Duce, Fuentes, & Riego, 2009).

Building on this literature, we conceptualize adversarial reforms as bundled institutional redesigns and study them from an equilibrium perspective. We document both the intended effects on the administration of justice (procedural duration, preventive measures, negotiated dispositions and settlements, and adjudication) and the unintended effects on enforcement activity, clearance, and crime rates. This design allows us to interpret changes in crime in light of the broader re-optimization that occurs along the full case trajectory, from the initial crime report to final resolution. We focus on Colombia because the reform was implemented in a staggered manner and because high-quality data make it possible to track multiple margins of adjustment. Although the estimated crime effects for Colombia may not fully generalize to every country that transitioned from an inquisitorial to an adversarial model, the contribution of our paper is the interpretive and measurement framework: a joint evaluation of how police, prosecutors, judges, defendants, and complainants adjust to new constraints and incentives introduced by the adversarial reform (Langer, 2007, 2021; Lewis, 2009).

By tracing multiple administrative margins alongside crime outcomes, the Colombian case illustrates how intended gains in efficiency and adjudication can coexist with shifts in selection and enforcement that are consequential for public safety. More broadly, the paper speaks to settings in which procedural redesign is used as a policy lever, and it clarifies why package-level measurement is necessary to identify mechanisms and diagnose unintended consequences.

III. Institutional Context: The adversarial reform in Colombia

On August 31, 2004, Colombia shifted from an inquisitorial/mixed criminal justice system (Law 600 of 2000, Old Code of Criminal Procedure) to a fully adversarial system (Law 906 of 2004, the New Code of Criminal Procedure). Under Law 600 (which was in force until the end of 2004), the criminal process was dominated by a judge-centered inquiry, in which an investigating authority gathered evidence in a mostly written dossier, and the accused played a relatively passive role. Often, the prosecutor effectively acted as an investigating judge, especially in early decisions such as whether pretrial detention was necessary or whether the evidence was sufficient to proceed to trial. In contrast, Law 906 (implemented in four phases starting in early 2005) established a U.S.-style adversarial procedure with a clear separation of functions: prosecutors and the defense present evidence before an impartial judge through oral and public hearings. The reform aimed to strengthen defendants' due process rights and transparency, making the accused an active party to the proceedings and eliminating the investigative-judge role previously performed within the prosecution, while increasing efficiency through greater prosecutorial discretion and negotiated dispositions.

Since the reform transformed the entire criminal process, assessing its effects requires a clear understanding of both the previous system (Law 600) and the new adversarial system (Law 906). We divide the analysis of the reform into three subsections to ground our empirical analysis. Figure A23 in the Online Appendix illustrates the main stages of the criminal process under Law 600 and Law 906.

a. Initiation of Investigations and Preliminary Phase

Under Law 600, a criminal case began with a preliminary inquiry (*indagación preliminar*), which was typically confidential and could be initiated *ex officio* by the state. There was no requirement for a formal accusation by a separate party to start the process, since the justice system could launch an investigation based on any credible information about the occurrence of a crime. Once a suspect was identified, the prosecutor, who held both investigative and accusatory functions, would formally link the suspect to the investigation by summoning them for an in-depth interrogation (*indagatoria*). Based on the *indagatoria*, the prosecutor would decide whether to open a formal investigation and whether preventive detention was necessary. Notably, under Law 600, the investigation phase was largely secret, as evidence was collected in writing and the defense had limited ability to intervene at this stage. The judge or investigating prosecutor could order evidence *ex officio*, blurring the line between judge and prosecutor, thus potentially compromising impartiality.

In contrast, under Law 906, a criminal case begins with a report or complaint (*denuncia* or *querella*) that triggers prosecutorial action. The prosecutor's office (*Fiscalía*) opens a case upon receiving a criminal report (*noticia criminal*), and this preliminary investigation phase remains under the prosecutor's control; generally, no judicial intervention occurs until a formal charge (imputation) is made. This initial investigation is still confidential to preserve evidence and prevent flight. Once the prosecutor has sufficient grounds, the case is brought before a judge. The investigating prosecutor no longer has a judicial role; instead, invasive investigative steps (e.g., search warrants, wiretaps, arrests) typically require approval by a judge of guarantees (*juez de garantías*), a new judicial role introduced by Law 906 to oversee the legality of pretrial actions.

Comparing the preliminary phase, whereas Law 600 allowed investigations to start and proceed largely within the prosecutor's jurisdiction with the judge as an active collaborator in fact-finding, Law 906 requires a crime report to initiate the process and introduces early judicial oversight to safeguard rights. This change implies that the state can no longer investigate and detain without judicial checks.

b. Formal Charging: Imputation of Charges vs. Indictment Resolution

A major procedural change under Law 906 was the introduction of a two-step charging process (imputation followed by formal indictment), replacing the inquisitorial system's single-step written indictment. Under Law 600, there was no public preliminary hearing equivalent to an arraignment; instead, after gathering sufficient evidence, the prosecutor would issue an indictment resolution (*resolución de acusación*). This was a written decision formally accusing the suspect of specific crimes and sending the case to trial. If the evidence was deemed insufficient, the prosecutor could instead issue a resolution declining to prosecute (*preclusión*). However, there was limited scope for informal case disposition: the process tended to either result in an indictment or be dropped for legal reasons, with few alternatives in between. Because this was a written process, the accused often learned the full charges and evidence only when the indictment was issued, frequently after a lengthy confidential investigation.

Under Law 906, the prosecutor brings charges into the courtroom earlier through the imputation stage. Once the prosecutor has enough evidence to reasonably suspect someone, they must appear before a judge of guarantees to formulate charges in a formal hearing (*audiencia de formulación de imputación*). In this hearing, the prosecutor formally informs the suspect of the charges and the essential supporting facts, in the presence of the judge and the defense. Importantly, imputation is not the final indictment: it does not yet send the case to trial, but it opens the formal prosecution and subjects the case to judicial oversight.

The next step under the adversarial system is the indictment (*acusación*). In this step, the prosecutor files an indictment document, and at an indictment hearing (*audiencia de formulación de acusación*) the case is assigned to a trial judge (*juez de conocimiento*) for the oral trial stage. Only after this second hearing does the trial phase begin.

In addition to splitting the pretrial charging stage into two steps, Law 906 introduced strict procedural time limits designed to reduce delays and prevent indefinite pretrial detention. Once charges are formally imputed, the prosecutor must present the indictment within the following 90 days. Following the indictment, the oral trial may begin within 120 days. If these deadlines are not met, the accused may be released, and the prosecution risks procedural dismissal of the case. In contrast, under Law 600, no equivalent deadlines existed: investigations could proceed without fixed timelines, and defendants could remain in pretrial detention for extended periods while prosecutors completed the written case file.

c. Implementation and rollout

Law 906 was implemented through a legally mandated gradual rollout across judicial districts between 2005 and 2008. The law specified the criteria for implementation and an ex-ante implementation schedule for each stage. Article 529 of Law 906/04 established that the implementation criteria include: (i) the number of offices and caseloads in the prosecutors' offices and criminal courts; (ii) the number of personnel trained in oral proceedings and projected training needed; (iii) projected demand for hearing rooms; (iv) demand for penal justice and public defenders; and (v) congestion levels. Article 530 then specifies the staged activation of the system, beginning on January 1, 2005, followed by additional districts entering on January 1, 2006, January 1, 2007, and January 1, 2008.[1]

This statutory rollout structure generates variation in exposure to the reform during a short transition window. Our empirical strategy leverages this staggered implementation to compare outcomes across districts before versus after implementation. A central identification concern is whether the rollout order, as determined by the criteria set out in Article 529, could be correlated with baseline differences in criminal justice or crime outcomes before implementation, or whether there were anticipatory effects. Because Article 529's criteria are directly tied to caseload and congestion, factors that may correlate with baseline crime, enforcement activity, and prosecutorial performance, we test whether rollout timing predicts differential pre-reform trends in our main outcomes and socio-economic and institutional control variables. Nonetheless, we do not find systematic evidence of such phenomena (see Figures 2 through 12, and Figures A2 through A4 in the Online Appendix), thereby

[1] Law 906/04, Article 529: *The following factors shall be taken into account for the discharge of its functions: The number of offices and cases in the Office of the General Prosecutor and in the criminal courts. The registry of officials trained in oral proceedings and the projected demand for training. The projected number of hearing rooms required. Demand for criminal justice and the need for public defense services. The level of case backlog. The rules of phased implementation are established by this law*.
Law 906/04, Article 530*: Based on the analysis of the foregoing criteria, the system shall be implemented as of January 1, 2005, in the judicial districts of Armenia, Bogotá, Manizales, and Pereira. A second stage, beginning on January 1, 2006, shall include the judicial districts of Bucaramanga, Buga, Cali, Medellín, San Gil, Santa Rosa de Viterbo, and Tunja. On January 1, 2007, the judicial districts of Antioquia, Cundinamarca, Florencia, Ibagué, Neiva, Pasto, Popayán, and Villavicencio shall enter the new system. The judicial districts of Barranquilla, Cartagena, Cúcuta, Montería, Quibdó, Pamplona, Riohacha, Santa Marta, Sincelejo, and Valledupar, as well as any others that may subsequently be created, shall begin applying the system as of January 1, 2008* (authors´ translation).

mitigating concerns about anticipatory responses and differential pre-trends. In particular, the event-study coefficients in the pre-implementation window suggest we cannot reject that they are equal to zero across arrests, clearance, and the main crime indices, supporting the parallel-trends and no-anticipation assumptions. To complement the graphical evidence, Table A19 in the Online Appendix reports joint F-tests of the null hypothesis that the pre-treatment coefficients in the event study specification are jointly equal to zero for the main outcomes.

An additional institutional feature further constrained the extent to which the reform could affect outcomes around the implementation date. The constitutional mandate and subsequent constitutional review emphasized that the new adversarial model applied only to offenses committed after the district-specific effective date, and not retroactively to offenses committed prior to the date or to the existing stock of pending cases.[2] As a result, implementation did not mechanically reclassify or transfer pre-existing cases into the new procedural regime at rollout. This feature strengthens the interpretation of the implementation date as a discrete change in the procedural environment relevant for new inflows of cases and reduces a key channel through which anticipatory behavior could operate. While non-retroactivity prevents mechanical reassignment of pending cases, anticipatory adjustments could still occur through training and enforcement reallocation; we therefore examine pre-implementation dynamics in arrests and crime, and do not detect systemic anticipatory shifts (see Table A3 and Figure A9 in the Online Appendix).

In the following section, we describe in detail the data used in our empirical analysis, including administrative records on prosecutorial and judicial activity and on crime and enforcement measures, which allow us to estimate the reform's intended and unintended effects.

IV. Data

We use data from four different sources to assess the intended and unintended consequences of the adversarial reform in Colombia. First, we use data from the General Prosecutor´s Office (*Fiscalía General de la Nación, FGN*) on judicial decisions and judicial outcomes, for both the previous inquisitorial model information system (Prosecutorial Information System for Law 600 - *Sistema de Información Judicial Ley 600/00-SIJUF*), and the new adversarial model information system (Prosecutorial Information System for Law 906/04 - *Sistema Penal Oral Acusatorio- Ley 906 & Ley 1098-SPOA*).[3] These two information systems contain case-level data since 2004 for all Colombian

[2] See the Colombian Constitutional Court Decision C-801/05 and C-1179/05.
[3] With the creation of the SPOA, the adversarial reform introduced a new module in the SIEDCO, the SIDENCO (*Sistema de Denuncias y Contravenciones*). This module allowed the National Police to send crime information to the General Prosecutor's Office, integrating the SPOA and the SIEDCO information. Although the SPOA included the information registered in the SIDENCO module, the SIEDCO information only had the National Police information during our analysis period. Moreover, the new module offered a channel to exchange information between the General Prosecutor's Office and the National Police but did not change the structure or nature of the data collected by each entity.

municipalities. From the SPOA and SIJUF, we use municipality-month information on (i) the average number of days between different procedural stages, (ii) the number of imputations of charges, (iii) the number of active cases, (iv) the number of cases with a preventive measure (house arrest or jail detention), (v) the number of settlements (before imputation), (vi) the number of convictions (at trial or guilty pleas), and (vii) the number of acquittals (at trial or agreements).

Second, the Colombian National Police (NP)[4] provided arrest and crime data by municipality and month from 2003 to 2008.[5] In a slightly unbalanced panel, we have information for 1,100 out of 1,122 municipalities in Colombia from 2003 to 2008.[6] We restrict our analysis to high-impact crimes and, within that subset, select those with fewer measurement problems: homicides, assaults, muggings, business robberies, home burglaries, and vehicle thefts (De Mello et al., 2013; Di Tella & Schargrodsky, 2004). These six categories account for 97% of high-impact crimes in Colombia in 2005 (Policía Nacional de Colombia-DIJIN, 2005).

Additionally, we include data on sexual and drug offenses as a limited diagnostic for potential reporting-related changes during the reform period. Sexual offenses are known to be substantially underreported and are plausibly more sensitive than other crimes to changes in reporting and trust in institutions (Briere, 1992; Scurich, 2020). For that reason, we do not treat sexual offenses as representative of other crime categories. Rather, we use them as a "high sensitivity" category: if reporting behavior were shifting sharply around the reform, it could plausibly show up more strongly in sexual offenses reports than in other crimes. [7]

In contrast, drug offenses are often characterized as victimless and are more directly shaped by policing activity and enforcement intensity than by reporting (Black, 1970; Campbell et al., 2022; Wijeratne et al., 2023). We therefore interpret drug offenses as a "low sensitivity to victim reporting" category, while recognizing that they are not immune to measurement issues because they respond mechanically to policing effort.

Accordingly, we use the reform's estimated effects on sexual and drug offenses reports only as suggestive evidence on the plausibility that changes in reported crime reflect reporting behavior rather than underlying offending. Importantly, we do not rely on this comparison alone to rule out reporting concerns. Throughout the paper, we interpret crime effects in conjunction with changes in

[4] The NP data came from the System of Statistical Information on Crime, Violations and Arrests (*Sistema de Información Estadístico, Delincuencial, Contravencional y Operativo* from the NPD-SIEDCO).

[5] Most of the municipalities for which we do not have information for the whole period are municipalities that were part of another one and became a formal municipality at some point during the period. As these municipalities account for less than 2% of the total and are quite small, their exclusion does not constitute a threat to our results.

[6] We restrict our study to the period between 2003 and 2008 because the SIEDCO changed in 2003, two years before the implementation of the adversarial reform (Rodríguez-Ortega et al., 2018).

[7] In Colombia, prostitution is not a crime; therefore, sexual offenses do not include prostitution or soliciting. According to official data, by 2004, the majority of sexual offense victims were minors (84.3%), and aggressors are usually well known by the victim (i.e., family members) (Medicina Legal, 2005).

enforcement and case progression, since a pure reporting shift would not, by itself, predict the joint pattern of adjustments we document across these administrative margins.

Using the crime data from the NP just described, we build four aggregate crime measures: i) an unweighted crime rate index, ii) a weighted crime rate index, iii) a weighted violent crime rate index, and iv) a weighted property crime rate index, using the average sentence for each crime from the penal code as the relative weight of each crime in each of the weighted crime indices.[8] The weighted crime index includes both violent crimes (homicides and assaults) and property crimes (muggings, business robberies, vehicle thefts, and home burglaries). From the NP, we also use arrest data. We normalize these crime rates per 100,000 inhabitants using yearly population projections from the Colombian National Administrative Department of Statistics (DANE). Figure A2 in the Online Appendix shows the evolution of the four aggregated crime measures by stage of implementation, normalizing to one the value in the month before the implementation.[9]

Finally, we use yearly municipal-level data that includes various socioeconomic and demographic covariates from the Center for Economic Development at Universidad de Los Andes (CEDE). The CEDE panel includes information on education, income, inequality, forced displacement, and a rurality index for each municipality between 2000 and 2008. For our analysis, we use five variables identified by the literature as determinants of crime: (i) income per capita (Crutchfield, 1989; Hipp, 2007; Messner & Tardiff, 1986; Verbruggen et al., 2015); (ii) institutional capacity proxied by the municipality's fiscal performance measure (Chamlin & Cochran, 1995; Messner & Rosenfeld, 1997; Rosenfeld & Messner, 2006); (iii) the rurality index (Deller & Deller, 2011; Kowalski & Duffield, 1990; Ladbrook, 1988; Lyerly & Skipper Jr, 1981; Wells & Weisheit, 2004); (iv) education, proxied by per capita municipal expenditure on education (Buonanno & Leonida, 2006; Hjalmarsson & Lochner, 2012; Lochner, 2004; Lochner & Moretti, 2004); and (v) population density (Lobonţ et al., 2017; Sampson, 1983; Shichor et al., 1979, 1980). Table A1 in the Online Appendix presents descriptive statistics for the variables of interest across these four data sources.

[8] Table A2 from the Online Appendix presents the weights we used for calculating the indices. We excluded sexual offenses and drug crimes from these indices since we do not have information for these crimes for 2003.

[9] All the panels show that, for all groups, most crime measures seem to increase after the implementation of the reform. The main increases seem to come from municipalities in stage two, followed by those in stages one and four. Average crime rates from stage three seem to increase in the first semester after the implementation of the reform, followed by a reduction after six to nine months, particularly for property crimes. Similarly, in Figure A3 from the Online Appendix we show the average value of the four aggregate crime measures for Colombia's five largest cities: Bogota (stage 1), Medellin (stage 2), Cali (stage 2), Barranquilla (stage 4), Cartagena (stage 4), Cucuta (stage 4). This figure shows that, except for Medellin, the aggregate crime indices increased for this group of cities after the implementation of the procedural reform. This increase seems to be driven by an increase in property crimes and by an increase in crimes in Cali.

V. Empirical Strategy

We estimate the effect of introducing the adversarial model on judicial decisions, judicial outcomes, and crime rates by exploiting the quasi-experimental variation resulting from the staggered rollout of the reform in Colombia between 2005 and 2008. Our empirical strategy consists of two alternative and complementary approaches. First, we estimate an unconditional event study model to assess the plausibility of the parallel trends and exogeneity assumptions. This approach allows us to test for preexisting differences in trends (pre-trends) between treated and control (not-yet-treated) municipalities, as well as the reform's dynamic effects several months after implementation. Equation (1) represents the unconditional leads-and-lags model that we estimate:

$$Y_{i,t} = \sum_{m=-12}^{m=-2} \gamma_m D_{i,t+m} + \sum_{p=0}^{p=12} \gamma_p D_{i,t+p} + \delta_i + \mu_t + \varepsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ represents the outcome variable in municipality *i* and month *t*. For judicial decisions and outcomes, $Y_{i,t}$ represents: (i) clearance rates, as the ratio of imputations to open criminal complaints; (ii) preventive measures, as the ratio of cases with preventive jail detention or house arrest to cases with imputation of charges; (iii) settlements, as the ratio between settlements and open criminal complaints; (iv) trial sentences, as the ratio of acquittals or convictions at trial over total sentences on trial, and (v) sentences, as the ratio of acquittals or convictions to open criminal complaints. Recognizing the difficulties in measuring changes in judicial and prosecutorial activity (Berggren & Gutmann, 2020; Marciano et al., 2019), we propose two types of indicators. First, rates that incorporate information delays and time spans between the denominator and numerator (CEPEJ, 2014; Marciano et al., 2019). Second, rates that control for the total number of cases when there is no logical time gap between the denominator and numerator. Further details about the construction of these variables can be found in the Online Appendix. As for crime, $Y_{i,t}$ represents: (i) unweighted crime rate index; (ii) weighted crime rate index; (iii) violent crime rate index; (iv) property crime rate index; (v) homicide rate, (vi) assault rates, (vii) sexual offenses rate; (viii) drug offenses rate, (ix) muggings rate, (x) business robberies rate, (xi) vehicle thefts rate, or (xii) home burglaries rate.

The term $\sum_{m=-12}^{m=-2} \gamma_m D_{i,t+m}$ denotes the sequence of lagged treatment variables ($m = -2, \dots, -12$ months, as $m = -1$ is the omitted category) and captures the potential differences in the evolution of each outcome variable during the pre-treatment period between treatment and control groups. The term $\sum_{p=0}^{p=12} \gamma_p D_{i,t+p}$ denotes the present and future treatment sequence ($p = 0, \dots, 12$ months), capturing the dynamic effects of the reform on the outcome variables over the 12 months after the reform was implemented. Due to the nature of our data and the implementation of the reform, we can only evaluate its impact with high statistical confidence for this period following its implementation. Nonetheless, we estimate longer-term effects using the same specification, expanding the analysis by considering 24 periods before the event and up to 36 periods after the reform was implemented. We present the longer-term results in the Online Appendix, Figures A10 to A22.

Moreover, we include year, month, and year-month fixed effects (μ_t) to control for national trends and possible seasonality in outcome variables. Likewise, we include municipality fixed-effects (δ_i) to control for non-observable and time-invariant municipality characteristics.[10] Lastly, the term $\varepsilon_{i,t}$ represents the error term, which we cluster at the judicial district and month level since the adoption of the adversarial system was assigned across judicial districts and to account for serial correlation of outcomes across municipalities within judicial districts (Abadie et al., 2023; Bertrand et al., 2004; Cameron et al., 2011). In Tables A16 to A18 in the Online Appendix, we show that results are robust to using other clusters, such as at the judicial district and calendar-month level.

This specification allows us to study pre-trends between treatment and control groups. If the parameters γ_m are statistically indistinguishable from zero for $m < 0$, we would not reject the null hypothesis that $\gamma_m = 0$. Even though the parallel trends and exogeneity assumptions are not directly empirically testable, these tests suggest that using the not-yet-treated municipalities as the control group is a sensible choice and that no-anticipation effects occurred before the implementation of the adversarial reform. As our results show, these null hypotheses cannot be rejected. Nonetheless, there are still reasons to believe that municipalities in Colombia are not comparable in other observable variables, such as differences in conflict intensity, resource availability, judicial and state capacity, among other factors. Therefore, we test pre-trends among different observable characteristics, including fiscal performance, population density, forced displacement, rurality index, tax revenues, and educational investment. Figure A4 in the Online Appendix shows that the four waves of the reform were balanced on these potentially confounding variables, thus supporting the use of not-yet-treated municipalities as controls for the treated ones.[11]

In our second approach, we exploit the gradual implementation of the reform using a two-way fixed effects (TWFE) model:

$$Y_{i,t} = \beta_1 T_{i,t} + \beta_2 X_{i,t} + \delta_i + \mu_t + \varepsilon_{i,t} \qquad (2)$$

[10] Even though the inclusion of municipality*month fixed effects or judicial districts*month fixed effects would have been more useful for identification, we do not have enough variation to estimate this large vector of fixed effects. Considering that our estimations suggest that the parallel trends and exogeneity assumptions hold without conditioning by these fixed effects, we argue our estimators are unbiased.

[11] In addition, we estimate a version of equation (1) in which we include a vector $X_{i,t}$ containing the mentioned economic, demographic, and institutional variables to control for time-varying municipality characteristics. With this estimation, we confirm that the parallel trends assumption is reasonable even after the inclusion of the vector of observable variables. Table A3 presents these results.

where $Y_{i,t}$ represents the same set of outcome variables in municipality *i* and month *t*; $T_{i,t}$ is a binary variable indicating whether the reform had been implemented in municipality *i* in month *t*. Vector $X_{i,t}$ contains economic, demographic, and institutional control variables, including the lag of police arrests in the municipality. Since outcome variables are measured in rates, $\widehat{\beta_1}$ should be interpreted as the average change in the outcome variable after the reform. Therefore, to obtain the relative effect of the reform, we divide this estimate by the mean of the dependent variable in the control group before the reform. We report both this mean and the implied effect in the results tables. Given the large number of outcomes in our specifications, we also test for multiple hypotheses using Westfall-Young stepdown-adjusted p-values, which we report in each table in Section VI.[12]

In general, our results should not be interpreted as absolute changes in the outcome variables, but rather as relative changes between our treatment and control groups. Since we include municipality fixed effects and a large array of control variables, the identification of our parameter of interest comes from changes in the outcome variables in treated municipalities relative to comparable not-yet-treated municipalities. This means that in our specification, we are not comparing remote rural areas with a substantial presence of armed groups with large urban areas. Instead, we are comparing changes across municipalities with similar levels of income per capita, institutional capacity, rurality index, education, and population density. Nonetheless, our main results are robust to different specifications.

Even though the TWFE model is the most commonly used method to estimate the effects of a policy or intervention, recent evidence has shown its limitations, especially in cases where the implementation of the policy is gradual or staggered, as is in our case (Borusyak et al., 2021; Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeuille, 2022; Goodman-Bacon, 2021). In a staggered TWFE, the estimated coefficient is a weighted average of all possible 2x2 difference-in-differences. In this average, all 2x2 comparisons between treated and not-yet-treated groups have the same weight (Goodman-Bacon, 2021). In Figure A5 of the Online Appendix, we present a graphic representation of the Goodman-Bacon (2021) decomposition, which displays all possible 2x2 difference-in-difference estimators in the data for the different outcomes of interest. This decomposition shows the large variance in the optimal weights across comparison groups and motivates the use of other estimation methods.

For this reason, we also estimate the event study specifications for the dependent variables using the doubly robust estimator proposed by Callaway & Sant'Anna (2021), which weighs these comparisons using both the variance and the centrality of the treatment. Figures A6 to A9 in the Online Appendix

[12] The Westfall-Young stepdown adjusted p-values controls for the family-wise error rate and allows for dependence amongst p-values. The individual null hypothesis corresponds to each individual hypotheses tested in our estimations being true, while the global null hypotheses correspond to all being true together.

show the results of these event study estimations. We also estimate our TWFE specification using the methodology proposed by Wooldridge (2025), who suggests that a highly flexible difference-in-difference model can account for the issues in the staggered TWFE specification and produce similar estimates to those from Callaway and Sant'Anna (2021), but with lower standard errors. We present this estimation in Table A12.

VI. Results

VI.I. Intended Consequences

In this section, we present our results examining the effects of the implementation of the procedural reform on several outcome variables that capture the reform's main stated goals, such as procedural times, pretrial detention, settlements, acquittals, and convictions. The results suggest that the reform achieved most, if not all, of its stated goals. Moreover, the results from the estimations show that, in general, there are no significant pre-trend differences between treatment and control groups in all outcomes of interest.

i. Procedural times

In Figure 1, we present the evolution of procedural times before and after the implementation of the reform between different stages of the penal process; specifically, between (i) the imputation of charges and the indictment (top two panels), (ii) the opening of the investigation and the imputation of charges (bottom left panel), and (iii) the opening of the investigation and the indictment (bottom right panel). As we explained before, the reform established very strict limits on the time that can elapse between the imputation of charges and the indictment hearing to protect due process and guarantee defendants' procedural rights. These limits led prosecutors not only to be much more selective in the criminal cases in which they decided to file charges (a result that we explore below), but also to take more time to file charges.

Panels from Figure 1 show the reallocation of time between the various stages. The top left panel shows the average number of days between the imputation of charges and indictment for each of the four implementation stages between 2003 and 2010, while the top right figure shows the evolution of the average number of days across all municipalities, normalizing to zero the implementation date of the reform. As can be seen, the number of days between the imputation of charges and the indictment was around 400 days before the reform, and after the reform, it fell to less than 90 days (the maximum time established by the new law). The lower left panel shows that the average number of days between the opening of the investigation and the imputation of charges increased, from around 60 days before the reform to around 150 days after implementation. The bottom right panel shows that the increase in the number of days between the opening of the investigation and the imputation of charges was more than offset by the reduction in the number of days between the imputation of charges and the indictment and, as a result, the number of days between the opening of the investigation and the indictment went down, from around 600 days before the reform to less than 300 days a year after the implementation of the reform.

Figure 2 presents the results from the lead-and-lags estimation (equation 1) using procedural times as the dependent variable. These figures support the conclusion that during the pre-treatment period, we cannot reject the hypothesis that the trend differences between treated and control groups were statistically equal to zero for most periods. Moreover, Figure 2 confirms the results presented in Figure 1: procedural times, measured as the number of days between the opening of the investigation and indictment, decreased by around 100 to 200 days. The bottom panel in Figure 2 shows that this reduction increased over time, at least during the first 12 months after the implementation of the adversarial reform.

Table 1 presents the results of the TWFE model (equation 2) using the three measures of procedural times between various stages of the penal process as the dependent variable. The first three columns present the results without including exposure time, while columns 4-6 (Panel B) include exposure time. The results confirm that the time elapsed between imputation and indictment decreased significantly by about 229 days, a 41.2% decrease relative to the pre-treatment mean. Yet, after the implementation of the reform, the time between the opening of the investigation and the imputation of charges increased by an average of 80 days (more than 100%). In contrast, after the reform, the total number of days between the opening of the investigation and indictment decreased by 117 days, an 18.8% decrease. Panel B confirms the dynamic evolution of these time differences presented in Figure 2.

ii. Pretrial detention

To strengthen due process, the adversarial reform aimed to increase the burden of proof to impute charges to the defendant and impose pretrial detention. Therefore, we expect to see a decrease in the use of jail-based pretrial detention after the implementation of the reform. We define our outcome variable as the number of cases with active preventive measures (jail-based pretrial detention, house arrest, and the sum of the two) divided by the total number of imputations for each crime type. Figures 3 to 5 present the results from the leads-and-lags model (equation 1) for jail-based pretrial detention, house arrests, and total pretrial detention, respectively. These three figures reveal that before the implementation of the reform, there were no significant differences in trends between treated and control municipalities for all crime categories. Moreover, the results show a decrease in the use of jail-based pretrial detention and an increase in the use of domiciliary arrests for homicides and drug crimes. Nonetheless, the estimators from the leads-and-lags model for the period after the implementation of the reform are quite imprecise and do not allow us to draw any conclusions. For this reason, we rely on the upcoming results from the TWFE.

Table 2 presents the TWFE results, which confirm the patterns shown in Figures 3 to 5. For homicide cases, the estimated reduction in the use of jail-based pretrial detention is 33.8% after the implementation of the reform; for property crimes, the estimated reduction is 17.7%, albeit less precise; and for drug offenses and assaults, the estimated reductions are 32.0% and 34.9%, respectively. For sexual crimes, the estimated reduction is not statistically significant.[13] For house arrests, the estimates are positive and smaller but highly significant, and the estimated magnitudes are extremely large for all crime categories. Under the old, inquisitorial system, house arrests were rarely used as a preventive measure, and, as a result, the estimated increase is very large in magnitude (between 266% for drug offenses and 859% for homicide cases). When we pool the two forms of preventive measures, we find reductions across most crime categories, but only for homicides (27.5%) and drug offenses (12.6%) are statistically significant.

iii. Pre-imputation Settlements

To improve the system´s efficiency and reduce congestion, the reform streamlined the possibilities for negotiated solutions. Under the new system, a settlement hearing before the imputation of charges is now mandatory for cases involving minor crimes (e.g., property crimes and assaults) and seeks to reach a reparation agreement between the alleged perpetrator and the victim so that the latter desists from the criminal complaint without formally opening a criminal case. Therefore, we should observe an increase in settlement rates and a decrease in the number of imputations for minor crimes after the implementation of the reform. Specifically, we compute settlement rates as the ratio of settlements to open criminal complaints for a specific crime in a municipality, allowing for information delays and time spans.

Figure 6 presents the results of the leads-and-lags model for settlement rates for property crimes and assaults, the two minor crimes in our data. In the months prior to the implementation of the adversarial reform, there were no significant trend differences in the rate of settlements between treated and not-yet-treated municipalities. With the implementation of the reform, and as intended, settlements increased significantly, and the effect became larger over time for both property crimes and assaults. Table 3 presents the results of the estimation of the TWFE model for settlements. The estimated percentage increase in settlements after the implementation of the reform is 41.7% for property crime cases and 65.3% for assault cases (columns 1 and 2 in Table 3). Columns 3 and 4

[13] As mentioned previously, most sexual offenses victims in Colombia are minors and the accused is usually a family member. Thus, in these cases, requesting jail-based pretrial detention has become the rule and judges tend to uphold the prosecutor's petition.

show that the increase in settlements after the implementation of the reform increased over time during the first 12 months after the reform entered into force.

iv. Adjudication rates: acquittals and convictions

Another way to evaluate the effects of the adversarial reform on the system´s efficiency is to look at adjudication rates, that is, the percentage of criminal cases that reach a final decision, either in court or through a plea deal or a settlement. We first explore the impact of the reform on total adjudication rates: acquittals or convictions. Figures 7A and 7B present the results of the leads-and-lags model for total acquittals and conviction rates respectively. These two figures show that during the pre-treatment period we cannot not reject the hypothesis that the differences in trends between treated and control groups for acquittal and conviction rates were statistically equal to zero for most periods. With the implementation of the reform, the adjudication rates increased for almost all crime categories, both for total acquittals (Figure 7A) and convictions (Figure 7B) decisions.

We present the results of the estimation of the TWFE model for acquittal and conviction rates in Table 4. The results show a very large and significant effect of the adversarial reform on the percentage of total cases that reach adjudication. For homicides, the estimated percentage increase in acquittals after the implementation of the reform is 408%, and for convictions 886%. For drug-related crimes, sexual offenses and assaults, the estimated increase is also very large and significant. For property crimes, the estimated effect of the implementation of the reform on acquittals is statistically zero, but for convictions it is, again, very large and significant.

There are at least two reasons for these extremely large, estimated effects. First, prior to the adversarial reform adjudication rates were remarkably low. For instance, right before the reform was implemented, only 0.031% of homicide cases reached an acquittal decision by a judge, and 0.16% reached a conviction decision by a judge. For property crimes, these figures are 0.009% (acquittals) and 0.093% (convictions). In other words, in the best-case scenarios, less than 1 in 200 open criminal cases reached an adjudication decision by a judge, so the space for improvement with the procedural reform was very large. The second reason is that the reform strengthened the mechanisms for early termination of the criminal process introducing agreements like plea deals. The objective of these early termination mechanisms was to reduce congestion by making it possible to obtain a sentence without going to trial.

To explore whether the increase in conviction rates was, at least in part, a result of the increase in plea deals, we estimate the leads-and-lags model using the share of acquittals and convictions in trial over total trial sentences and present the results in Figures 8A and 8B. These figures show an increase in the share of acquittals in trial over total trial sentences and a decrease in convictions in trial over total trial sentences for homicide cases. For other crime categories, the results are less conclusive.

When we estimate the TWFE model, we find a highly significant increase in the share of acquittals in trial over total trial sentences (and the corresponding decrease in the share of convictions) mainly for homicide cases and property crimes (see Table 5). Our interpretation of this result is that the introduction of plea deals made it possible to increase convictions before trial and reach these decisions faster. Presumably, in cases where the prosecution has sufficient evidence of the defendant's guilt, the defendant will seek a plea agreement with the prosecutor in exchange for a lesser sentence. In cases where the defendant knows that the prosecution does not have sufficient evidence to secure a conviction at trial, the defendant will presumably prefer not to negotiate with the prosecution and, as a result, acquittals in trial would increase as a percentage of total judgments at trial.

VI.II. Unintended consequences

In this subsection we explore three possible unintended consequences of the implementation of the adversarial reform in Colombia. As discussed earlier, the implementation of the reform not only affected the role of the agents of the criminal justice system (judges, prosecutors, police) and therefore their decision-making process and optimal actions, but also the expected costs of committing a crime. Given that some of the changes brought by the reform might have increased the expected cost of committing crimes and others might have reduced them, the net effects of the reform on crime remain an open empirical question. Before presenting the estimations of the net effects of the reform on crime, we explore the reform's effect on two other related outcomes: police arrest rates and clearance rates. We present these two effects first to offer a complete overview on how the reform affected the costs associated with committing an offense.

i. Arrest rates

One of the main objectives of the reform was to increase due process protections by imposing stricter conditions (a higher bar) for legal procedures such as the apprehension of an alleged offender by the police, the imputation of charges by the prosecutor, and the decision to impose pretrial detention on a defendant. Although the reform did not change the statutory grounds for arrest, it introduced stricter procedural scrutiny through immediate judicial review of arrest legality and stronger evidentiary safeguards. As a result, arrests that do not satisfy the evidentiary and procedural requirements necessary for the case to progress under the new adversarial system are more likely to be invalidated early in the process.

This institutional change can generate a discrete shift in arrest incentives at the time of the implementation. Once Law 906 became operative in a judicial district, arrests immediately became subject to the procedural requirements and judicial review mechanisms of the adversarial system.

The expected payoff to an arrest became more tightly linked to compliance with the new procedural requirements and prosecutorial timelines. If arrests are not supported by the evidentiary and procedural standards necessary for the case to progress to imputation under the new system, police effort becomes less productive because downstream prosecution is less likely to proceed. As a result, officers may rationally self-select away from marginal arrests immediately upon implementation, generating an abrupt decline in observed arrest rates.

In Figure 9 we present the results from estimating the leads-and-lags model using as the dependent variable the number of police arrests per 100,000 inhabitants. During the pre-treatment period we cannot reject the hypothesis that the trend differences in arrest rates between treated and control groups were statistically equal to zero for most periods. Figure 9 shows that during the post-treatment months, the adversarial reform led to a large and significant reduction in the arrest rate. The results of the TWFE model are presented in Table 6 and show that the implementation of the adversarial reform led to a 39% reduction in the arrest rate, and that this reduction increased over the first 12 months after implementation.

ii. Clearance rates

The changes introduced by the reform could have affected clearance rates (defined as the ratio between the number of imputations of charges over the total number of reported crimes, for each crime category[14]) in two ways. First, as we discussed previously, the reform made mandatory pre-imputation settlement hearings to promote closing minor criminal complaints without opening a formal penal case. As we showed in Table 3, the reform effectively led to an increase in the number of agreements between the victim and the accused before imputation. Thus, we should expect to see a decrease in the number of imputations, and therefore, a decrease in clearance rates.

Secondly, one of the ways in which the reform sought to protect due process and guarantee the rights of the accused was by limiting the time that could elapse between the imputation of charges and the indictment hearing. This might have led prosecutors to be much more selective in the cases in which they decide to file charges, presumably in those cases where prosecutors have enough evidence to advance the investigation and make it to the indictment hearing without violating the new time limits imposed by the law. Previously, we showed how the implementation of the adversarial reform increased the days between the opening of the investigation and the date of the imputation of charges (lower left panel in Figure 1).

[14] Our definition of clearance imposes a higher threshold than the one commonly used in the criminology and policing literature, where a crime is considered cleared once an arrest has been made or when a suspect has been identified and taken into custody. Because filing charges requires the prosecutor to formally present the case before a judge, our measure captures a later stage in the criminal justice process and therefore represents a stricter measure of case clearance.

We test whether the clearance rate fell upon the implementation of the reform. To do this, we use a standard definition of the clearance rate: the number of imputation of charges divided by the number of open criminal complaints for each crime category. Figure 10 presents the results of the leads-and-lags model using the clearance rate as the dependent variable. Prior to the implementation of the adversarial reform there were no significant differences in clearance rates trends for most crime categories, except for assaults. After the reform was implemented, the results clearly show a significant reduction in clearance rates for all crime categories. These findings are confirmed with the estimations of the TWFE model (Table 7), which show a significant reduction in the clearance rate for homicides of about 23%, 24% for property crimes, 16% for drug offenses, 25% for sexual offenses and 27% for assaults.

For property crimes, our data does not allow us to differentiate between the two transformations the reform introduced that potentially affected clearance rates (pre-imputation settlements and strict procedural time limits). Yet, for homicides, drug offenses and sexual offenses (crimes where pre-imputation settlements are not allowed by law), it is reasonable to assume that the effects observed in Figure 10 and Table 7 are capturing the effect of introducing procedural time limits. Thus, these reductions in clearance rates complement those results presented in the lower left panel of Figure 1 and columns 2 and 5 in Table 2, which show a significant increase in the time elapsed between the opening of the investigation and the imputation of charges.

iii. Crime rates

Finally, we now explore the overall effects of the adversarial reform on crime rates. As we have discussed extensively throughout the paper, the adversarial reform changed three key determinants of the expected cost of committing crimes in (theoretically) different directions: the certainty, severity, and celerity of punishment. While improvements in the efficiency of the judicial system in investigating and prosecuting crimes can in principle be associated with lower crime rates, a greater selectivity of the criminal cases that are likely to be prosecuted can reduce the expected probability of being charged and thus increase crime rates.

Furthermore, while increasing the protection of due process is a desirable goal because it enhances the legitimacy of the criminal justice system (Sunshine & Tyler, 2003) and reduces the risk of recidivism in the long run (Heaton et al., 2017; Leslie & Pope, 2017), it might have unintended consequences on crime rates that must be considered when enacting these reforms. For instance, increasing the burden of proof to impose pretrial detention might reduce the incapacitation effect of this measure, leading to higher crime rates in the short run (Chalfin & McCrary, 2017).

Moreover, strengthening early-termination mechanisms, such as plea deals, can shift the expected discounted cost of committing a crime in different directions. While an expected lighter sentence under a plea deal may reduce the cost of committing a crime, the reduction in time to reach a conviction may increase it. Thus, the question of what effect dominates becomes an empirical question, especially when an extensive criminal procedural reform such as the one implemented in Colombia changes the incentives of engaging in criminal activities through different channels and in different directions.

We proceed to estimate the effects of the adversarial reform on crime rates by estimating both models (equations 1 and 2) using crime rates as the dependent variables. Figure 11 presents the results of the leads-and-lags model using four crime indices (unweighted crime index, weighted crime index, weighted violent crime index and weighted property crime index). The first thing to notice is that in the 12 months prior to the implementation of the reform, there were no significant differences in the trends of these four crime indices between treated and not-yet-treated municipalities. Following the implementation of the adversarial reform, there has been a notable increase in all four indices.

We corroborate these results by estimating these specifications using the TWFE (Table 8). The estimated increase for the unweighted crime index is 30%, for the weighted aggregate crime index, it is 19%, for the violent crime index, it is 15%, and for the property crime index, it is 29%. All the estimated effects are highly significant. When we add exposure time (columns 5 – 8), the results indicate that the effects of the reform on crime rates increased over the first 12 months after implementation. When we consider the effect of the reform on the four crime indices after 12 months of implementation, the estimated increase for the unweighted crime index is 45%, 27% for the weighted crime index, 16% for the violent crime index and 58% for the property crime index.

Our results are similar in magnitude to those of other authors studying similar reforms. For example, Cepeda-Francese & Ramirez-Alvarez (2023) find that after the adoption of the adversarial reform in Mexico, homicide rates increased by 7%; while Cattaneo et al. (2022) observe an increase of 21 to 24 police reports per day in Montevideo (8% to 10% increase) after the implementation of the adversarial reform in Uruguay.

The estimates for separate crime categories are presented in Figures 12A and 12B (for the leads-and-lags model) and Tables 9A and 9B (for the TWFE model) for violent crimes and drug offenses, and property crimes, respectively. These results show that for the three crimes that compose the violent crime index (homicides, assaults, and sexual offenses), there are no discernible differences in pre-trends prior to the reform. After the implementation of the reform, our results from Figure 12A show a clear increase in the assault rate. For homicides and sexual offenses, there is no clear discernible pattern after the implementation of the adversarial reform. When we estimate the TWFE model for the three separate violent crime categories, we find a highly significant increase in the assault rate of 57%, or 70% after 12 months of implementation (columns 2 and 6). For sexual crimes and homicides, we do not find a significant effect after the implementation of the reform.[15]

Finally, we evaluate the effects on the separate crime categories that compose the property crime index: muggings, business robberies, vehicle thefts and home burglaries. For all these property crimes, the assumption of parallel trends prior to the implementation of the reform appears to hold (Figure 12B). After the reform, our estimations show a weak increase in all property crime categories. However, once we include municipality-level controls in the TWFE model, the average effect of the reform is positive for all property crimes. The average estimated increase in muggings after the reform came into force is 30%, while the increases are 34% for business robberies, 15% for vehicle thefts (but less precisely estimated), and 27% for home burglaries (Table 9B). When we include exposure time (columns 5 – 8), the estimated increase after 12 months of implementation is 74% for muggings, 63% for business robberies, 8% for vehicle thefts and 46% for home burglaries. All these effects are highly significant.

Our results on crime are highly robust to different specifications. We estimate a highly flexible difference-in-differences model (Wooldridge, 2025) to account for some of the issues in the staggered TWFE specification. Table A12 in the Online Appendix includes these results. We conduct two additional falsification tests to further assess the robustness of our results. First, in Table A3, we present the results of the conditional event study during the pre-treatment period (12 months before implementation). The results indicate that parallel trends hold. Second, we randomly assign all Colombian municipalities to different implementation stages and estimate equation (1) using this random order. We repeat this random assignment 100 times. Table A5 includes the average coefficients and standard errors across these 100 estimations. As expected, we do not observe any effect of these counterfactual reforms on crime.

Our results also hold when we exclude Bogota, Medellin, and Cali, Colombia's three largest cities, and where most crime is located (Tables A6 to A8). Moreover, we estimate the model excluding the lag of arrest (Table A4) and excluding the whole vector of covariates $X_{i,t}$, obtaining similar results (Tables A9 to A11). Finally, due to the large number of zeros in our dependent variables, we follow Chen and Roth (2024) and estimate our main model using a Poisson specification with the number of crimes of a given type in municipality i in time t ($Y_{i,t}$) as the dependent variable, while including the same right-hand side variables and additional control for the municipality's population (Tables A13 to A15).

[15] Since the empirical evidence suggests that the reform did not change sexual crime rates, we hypothesize that not observing changes in sexual crime rates might indicate that no changes in the report of these offenses occurred. In other words, there is no evidence that the reform affected the propensity to report crimes considering that the crimes more susceptible to self-reporting did not change during our study period. However, we recognize the limitations of our data to conclude that crime reporting did not change after the reform.

Overall, our estimates from the two methodological approaches show a significant increase in crime after the implementation of the adversarial reform in Colombia, ranging from 1% to 57%, depending on the crime category. These results indicate that the net effect of the implementation of the reform on the incentives to engage in criminal activities was negative: overall, our results show that the implementation of the adversarial reform reduced the expected costs of committing crimes.

VII. Concluding remarks

Most of the law and economics literature on crime control has focused on how changes in substantive penal law, such as sentencing rules, incarceration, and rehabilitation, affect criminal behavior by shifting the expected costs and benefits of offending. By contrast, we know much less about how procedural law, the rules governing investigation, charging, adjudication, and pretrial detention, shapes deterrence when substantive criminal law is held fixed. This paper contributes to that gap by evaluating a large, bundled criminal procedure reform as an institutional redesign that induces equilibrium adjustments across the full case production process, rather than as an isolated policy change affecting a single margin.

We study Colombia's transition from the inquisitorial system (Law 600/2000) to a fully adversarial system (Law 906/2004), implemented through a legally mandated staggered rollout between 2005 and 2008. The reform was explicitly intended to strengthen due process protections, reduce reliance on pretrial detention, shorten procedural times, and expand early-termination mechanisms to reduce congestion and increase adjudication. The staggered activation across judicial districts provides quasi-experimental variation during a transition window, allowing comparisons of municipalities before and after implementation, relative to not-yet-treated municipalities in the same period.

Using detailed administrative data from prosecutorial information systems under both regimes, together with police data on arrests and crime rates, we document that the reform largely achieved its intended institutional objectives. Procedural duration fell sharply in the post-imputation stage, driven by a reduction in the time between imputation and indictment, consistent with the new statutory deadlines. Specifically, the time elapsed between the imputation of charges and the indictment fell by 41.2% (557 days). The reform substantially reduced the use of jail-based pretrial detention across most offense categories and increased the use of domiciliary detention. Jail-based pretrial detention fell by 33.8% for homicides, 17.7% for property crimes, 32% for drug offenses, and 34.9% for assaults (with no statistically significant change for sexual offenses). Total pretrial detention (jail plus domiciliary) declined for homicides (27.5%) and drug offenses (12.6%), while the net effect is close to zero for property crimes, sexual offenses, and assaults.

The reform also expanded negotiated exits. Settlement rates increased markedly for minor offenses in which pre-imputation settlements are permitted: by 41.7% for property crimes and 65.3% for assaults. Consistent with this shift, adjudication rates rose substantially, with convictions increasing primarily through non-trial dispositions (settlements and pleas) rather than trial convictions. In parallel, the composition of trial outcomes shifted in ways consistent with expanded plea bargaining: for homicides (and in some specifications property crimes), the share of acquittals among trial judgments rises while the share of convictions among trial judgments falls, consistent with stronger cases exiting pretrial through pleas.

At the same time, we document unintended consequences for enforcement activity and case progression. Arrest rates declined significantly after implementation: by about 39% and roughly 52% within the first 12 months under exposure-time specifications. More importantly, clearance rates, measured as imputations relative to open criminal complaints, fell across all major crime categories, by 23.3% for homicides, 24.2% for property crimes, 15.6% for drug offenses, 24.7% for sexual offenses, and 27.3% for assaults. For property crimes and assaults, the decline is consistent with the reform's mandated pre-imputation settlement hearings, which may be diverting minor cases from formal charging. For offenses where pre-imputation settlements are not permitted by law, such as homicides, drug offenses, and sexual offenses, the reduction in clearance rates points to a different mechanism: tighter procedural timelines and higher evidentiary burdens may have increased selectivity at the formal charging stage, consistent with the reallocation of time we observe between the opening of investigations and imputations.

Taken together, our findings show that large procedural reforms should be evaluated as bundled institutional redesigns that reshape deterrence through general equilibrium adjustments across the case-production processing pipeline. Colombia's adversarial reform delivered substantial "within-system improvements" consistent with its stated objectives. Yet these gains occurred alongside pronounced declines in enforcement activity and case progression. The central lesson is that procedural redesigns can simultaneously shift celerity, severity, and certainty in offsetting directions, and that the net effect on crime depends on how criminal justice actors re-optimize under new constraints.

This equilibrium perspective also clarifies the mechanism underlying the reform's effects on crime. The reform accelerated processing and expanded settlement and plea pathways, thereby increasing the discounted expected cost of punishment through faster resolution. At the same time, the reform tightened procedural constraints and raised evidentiary and timing demands required to sustain cases through indictment, generating stronger incentives for prosecutorial screening and potentially altering police effort. In the data, these adaptations are reflected in the sharp decline in arrests and clearance rates, which are connected to the certainty of punishment. In other words, the reform increased celerity (conditional on cases reaching later stages) while reducing the probability that reported crimes translate into charges, consistent with a weakening of the expected likelihood of punishment. The observed increases in crime are consistent with this joint movement in deterrence components: efficiency gains within prosecution and adjudication can coexist with a decline in certainty as actors respond to tighter constraints by prioritizing fewer cases, reallocating effort, and relying more heavily on negotiated dispositions.

Consistent with this mechanism, we find robust evidence that the reform increased crime. The weighted aggregate crime index increased by 18.7%, the violent crime index increased by 15.2%, and the property crime index increased by 28.7%. These effects also grow over time during the first 12 months after implementation, consistent with an adjustment process rather than a one-time shift in mechanism.

Our contribution is therefore interpretive as empirical. Existing work often studies narrow procedural margins or relies primarily on crime outcomes, making it difficult to determine whether changes in crime reflect shifts in punishment severity, changes in case selection, or enforcement substitutions. By tracing administrative and enforcement margins jointly, from complaint to charge, pretrial detention, negotiated exits, and final disposition, we show why crime outcomes alone are insufficient to diagnose the mechanism of procedural reforms. A reform can increase adjudication rates and shorten downstream timelines while still reducing the system's effective certainty through upstream screening and lower enforcement activity. Evaluating reforms as packages and measuring their effects along the pipeline is necessary to interpret deterrence effects and to identify where unintended consequences arise.

The Colombian staggered rollout provides credible quasi-experimental variation to estimate these pipeline effects. The event-study evidence does not show systematic differential pre-trends in arrests, clearance, or the main crime indices prior to implementation, and the results are robust across alternative staggered adoption estimators and falsification exercises. While no design can fully eliminate every concern about policy timing, the combination of (i) a legally defined rollout schedule, (ii) dynamic pre-trend tests, and (iii) robustness across estimators and placebo assignments, supports the interpretation that the estimated post-implementation shifts reflect the reform-induced change in the procedural environment rather than pre-existing divergent trajectories.

More broadly, the results travel beyond Colombia not because every adversarial transition will increase crime, but because the underlying mechanism can be generalized beyond the Colombian case: procedural reforms can reallocate constraints and discretion across actors, including endogenous adjustments in enforcement and charging that alter the joint production of certainty, severity, and celerity. This insight is especially relevant in civil-law jurisdictions that adopt adversarial models as comprehensive packages, often with the dual goals of improving legitimacy and reducing congestion. In such settings, the key policy question is not whether reforms "increase efficiency", but whether efficiency gains are achieved in ways that preserve the pipeline's capacity to convert reports into credible charging and sanction threats.

Finally, our findings should not be read as a normative argument against due-process protection. Strengthening fairness, reducing excessive pretrial detention, and limiting coercive state power are central objectives of modern criminal justice systems. The implication is institutional rather than a "due process versus public safety" trade-off. Rights compatible with procedural reforms are most likely to preserve public safety when paired with complementary investments and monitoring that prevent collapses in certainty. If tighter timelines, higher evidentiary demands, and expanded negotiated exits operate under binding investigative and prosecutorial capacity constraints, then the system may rationally respond by screening out some cases and reducing enforcement activity, precisely the margin that most directly affects deterrence. Designing and evaluating reforms thus requires explicit attention to where the pipeline becomes constrained (investigation, charging, or adjudication) and to whether reforms shift burden onto stages that lack capacity.

In sum, we show that comprehensive procedural reform can succeed on its internal administrative objectives while still weakening deterrence through equilibrium responses that reduce enforcement and case progression. The core lesson for research and policy is to treat criminal procedure as a system-level production process. In order to understand the deterrence and public safety consequences, we must jointly measure reforms across administrative margins, rather than infer mechanisms from crime outcomes alone.

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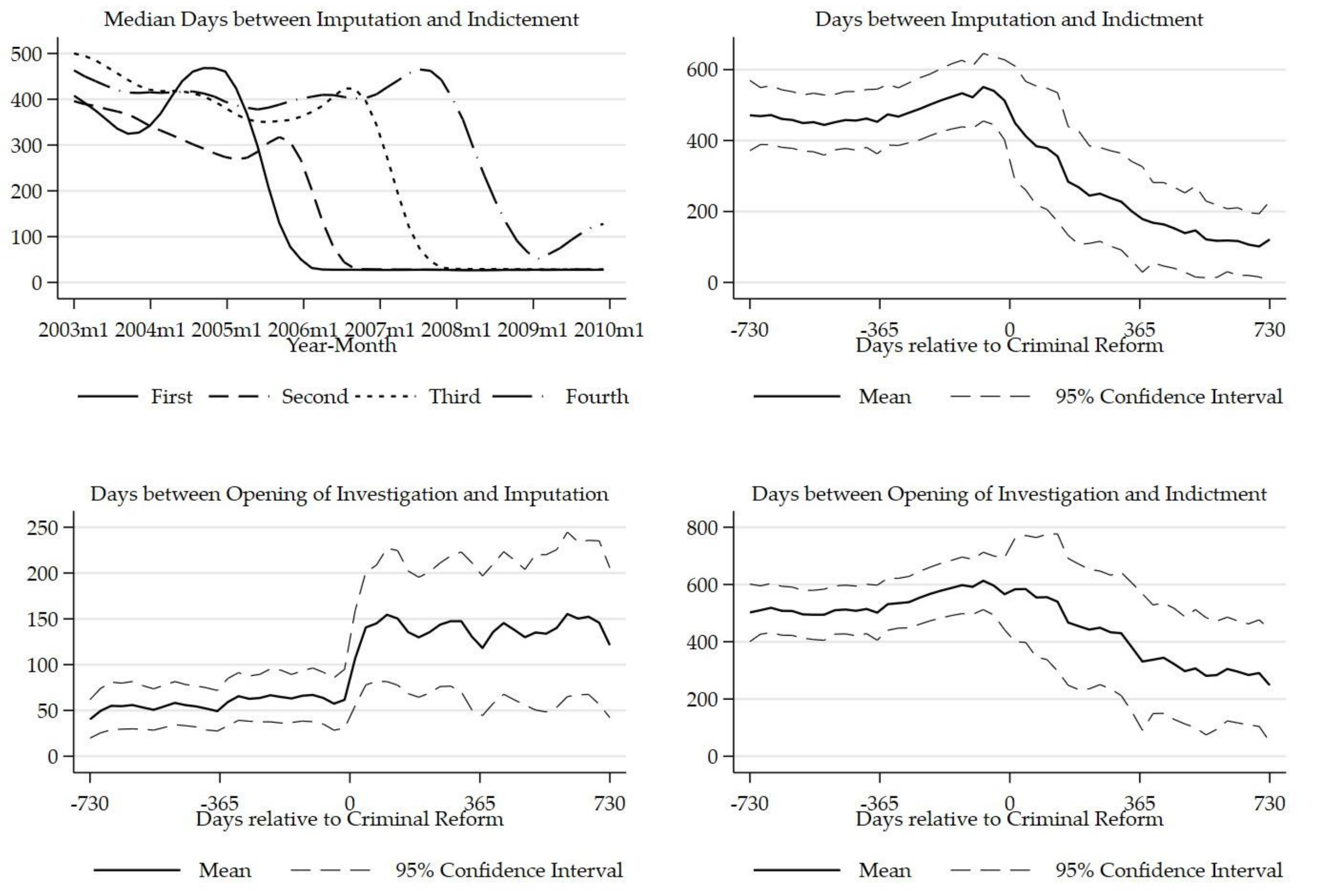
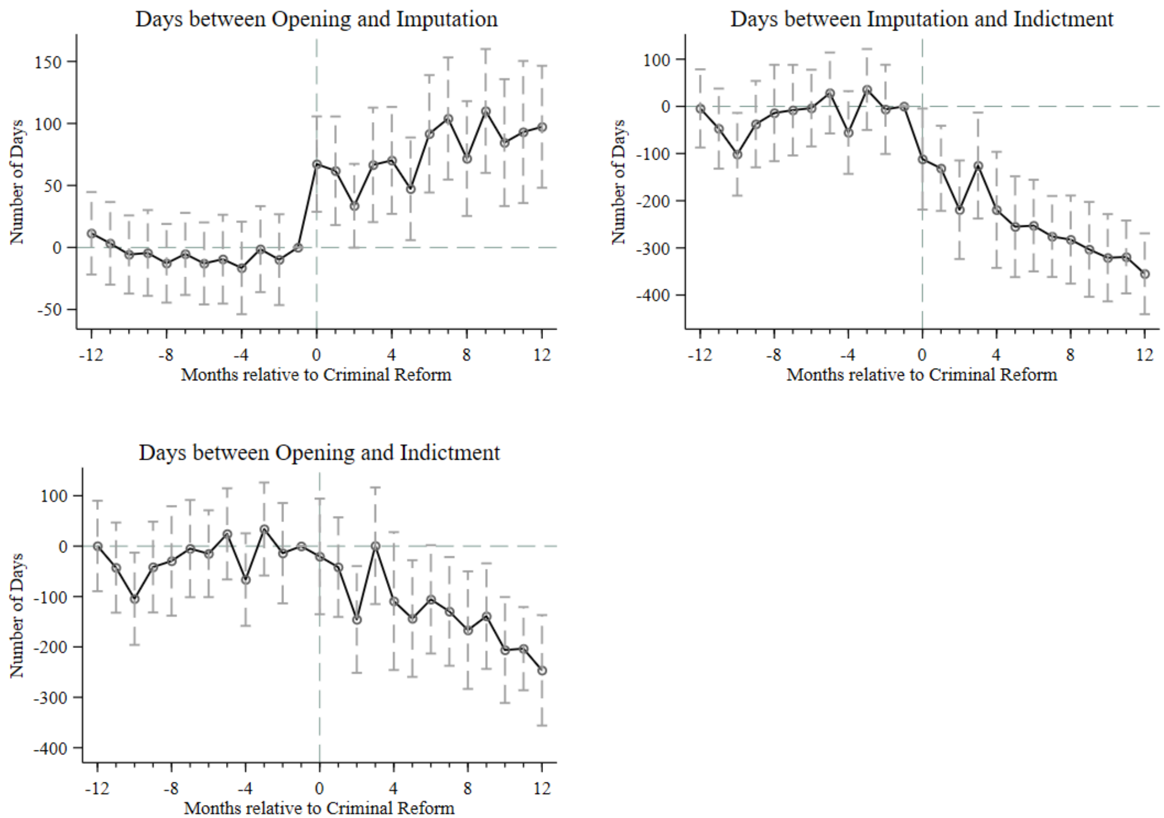
Figure 1. Procedural Length Reduction in Colombia after the Reform



Note: this figure shows the evolution of the time between different stages of the criminal process. The top-left panel shows the evolution over time of the median number of days between imputation and indictment by stage of implementation of the reform, while the other three panels show the evolution of the average of other measures across all municipalities over the time relative to the date of the implementation of the reform, including the 95% confidence intervals represented by the dashed lines.

Figure 2. Leads-and-lags model: Procedural Times



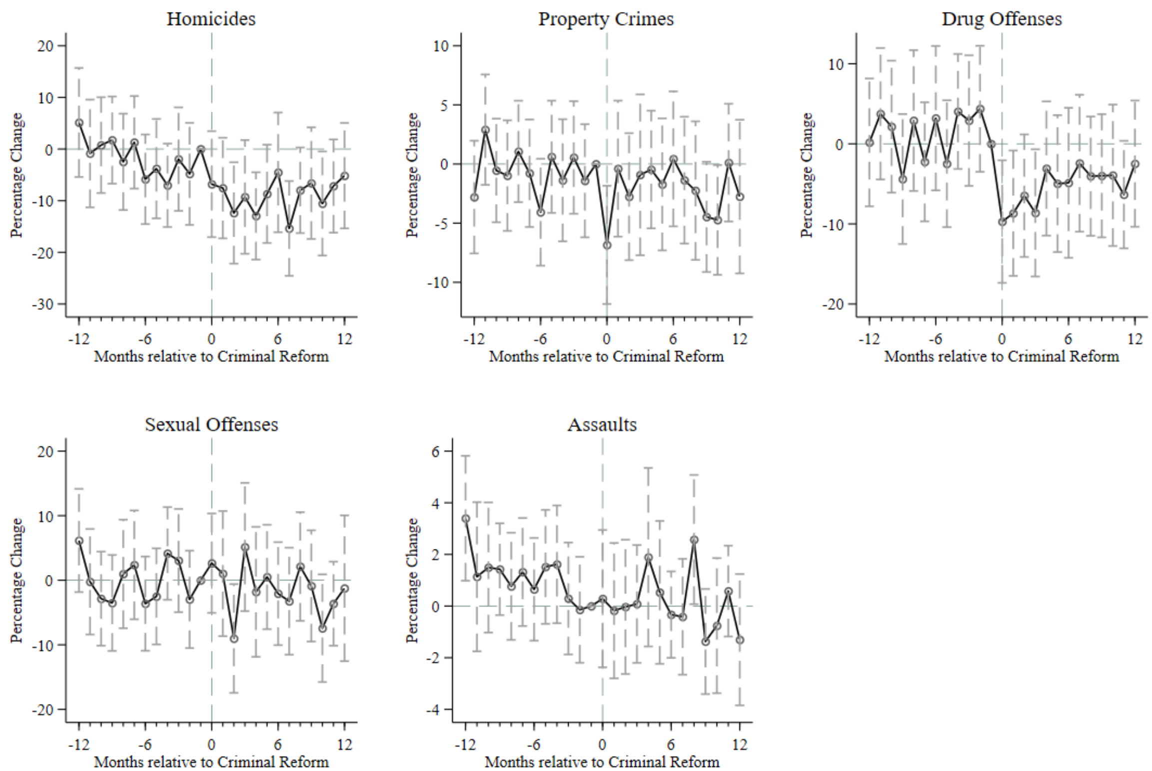
Note: this figure shows the results of an event study of the number of days between different stages of the criminal process as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors.

Table 1. Difference-in-Difference Results for Procedural Times

	Panel A			Panel B		
VARIABLES| Days between	(1) Imputation - Indictment	(2) Opening - Imputation	(3) Opening - Indictment	(4) Imputation - Indictment	(5) Opening - Imputation	(6) Opening - Indictment
T	-229.14***	79.84***	-117.11***	-197.86***	84.68***	-68.18***
	(14.62)	(5.78)	(15.42)	(14.19)	(6.45)	(15.12)
Exposure Time to T				-7.92***	-1.26***	-11.50***
				(0.96)	(0.38)	(0.97)
Constant	504.66***	37.01	468.04***	491.36***	34.97	441.37***
	(82.60)	(34.54)	(95.09)	(77.28)	(34.71)	(84.55)
Observations	17,842	17,750	17,526	17,842	17,750	17,526
R-squared	0.39	0.26	0.33	0.391	0.260	0.339
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00	0.00
Mean T = 0	556.67	66.81	622.60	556.67	66.81	622.60

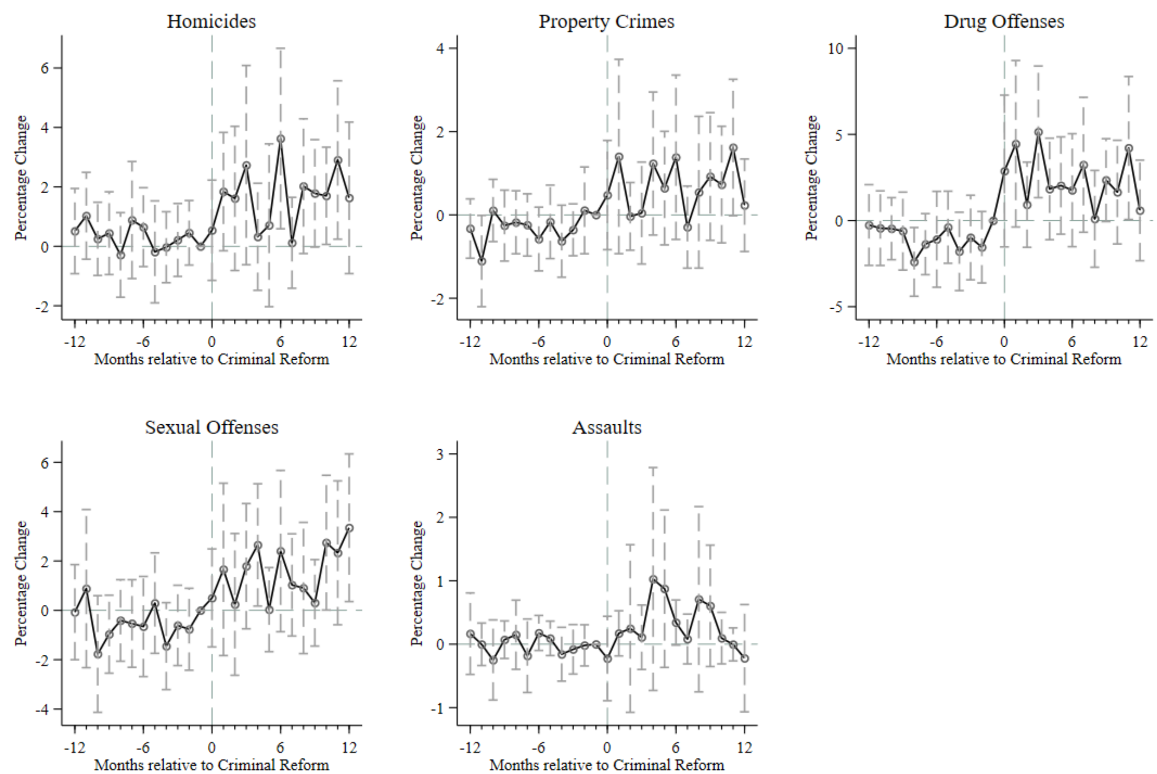
Note: this table shows the results of a two-way fixed effects regression of the days between different stages of the criminal procedure on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in each municipality and month (panel A) and the non-negative difference between a given month and the month of implementation up to 12 months (panel B). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and the lag of police arrest. Judicial district-month-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. *** p<0.01, ** p<0.05, * p<0.1

Figure 3. Leads-and-lags model: Jail-based pretrial detention



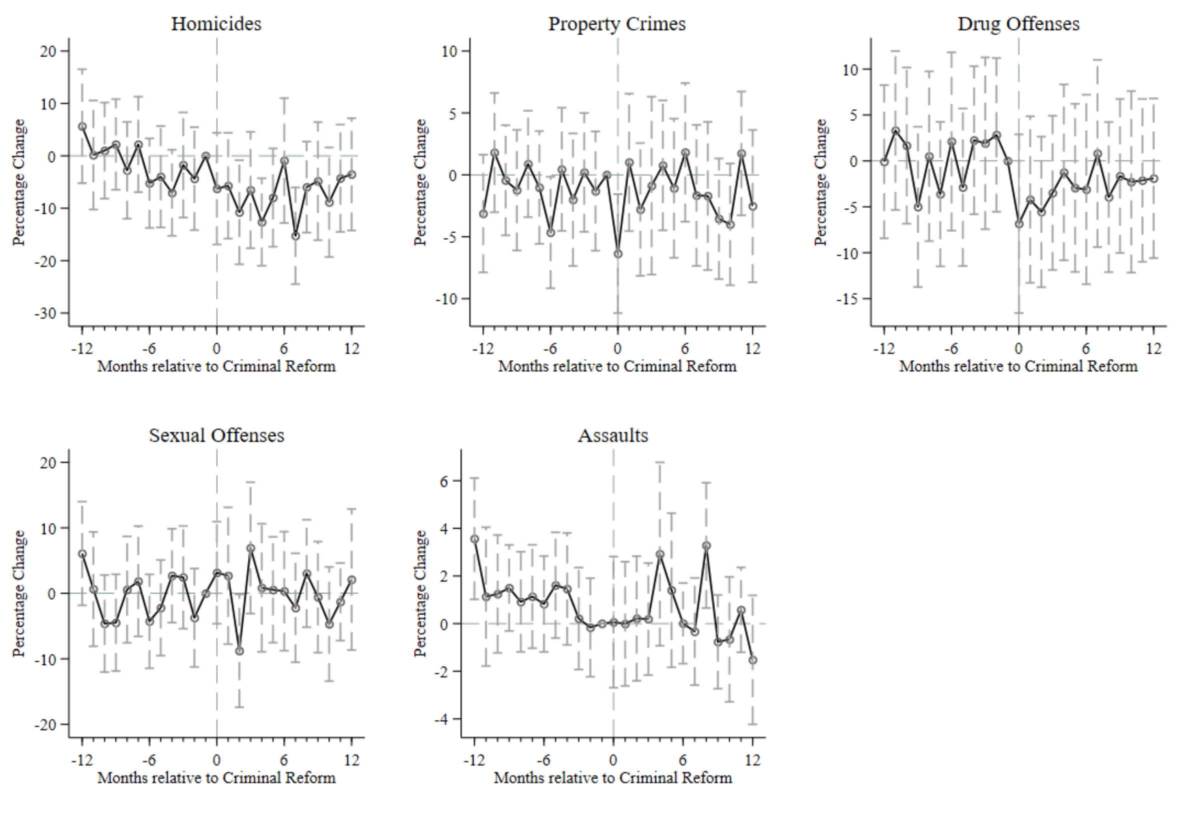
Note: this figure shows the results of an event study of jail-based pretrial detention rates for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Pretrial detention rates are computed as the ratio between the number of cases with active measures in a municipality and month relative to the total number of cases.

Figure 4. Leads-and-lags model: Pretrial house arrest



Note: this figure shows the results of an event study of the rates of pretrial house arrests for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Pretrial detention rates are computed as the ratio between the number of cases with active measures in a municipality and month relative to the total number of cases with imputations in that same municipality-month

Figure 5. Leads-and-lags model: Pretrial detention total



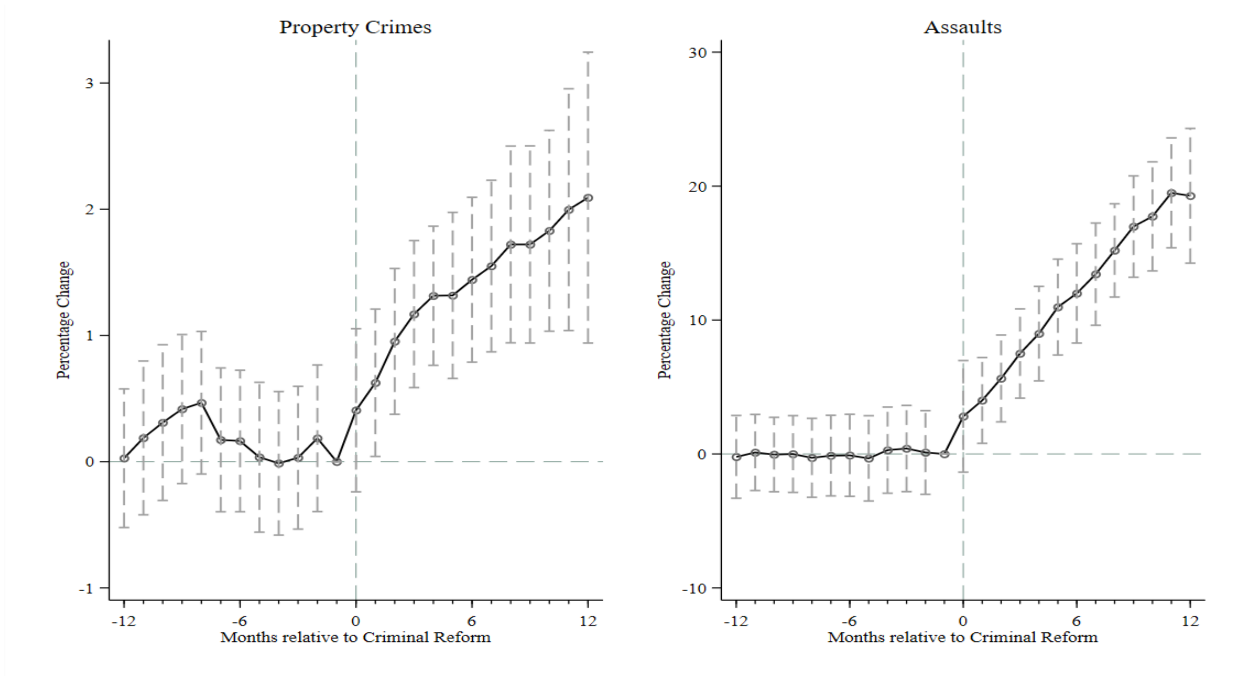
Note: this figure shows the results of an event study of the rates of total pretrial detentions for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Pretrial detention rates are computed as the ratio between the number of cases with active measures in a municipality and month relative to the total number of cases with imputations in that same municipality-month.

Table 2. Difference-in-Difference Results for Pretrial Detention

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Panel A: Jail-based Pretrial Detention					
T	-7.48***	-1.43*	-6.15***	-1.73	-1.06**
	(1.44)	(0.74)	(0.99)	(1.22)	(0.43)
R-squared	0.13	0.11	0.15	0.11	0.11
Mean if T==0	22.13	8.10	19.22	17.36	3.03
% Change after T	-33.8%	-17.7%	-32.0%	-9.9%	-34.9%
P-wyoung P-Value	0.00	0.88	0.00	0.20	0.97
Panel B: Pretrial Detention in House Arrests					
T	1.35***	0.91***	3.55***	1.93***	0.33**
	(0.29)	(0.23)	(0.48)	(0.37)	(0.14)
R-squared	0.14	0.10	0.10	0.12	0.07
Mean if T==0	0.15	0.19	1.34	0.67	0.07
% Change after T	859.0%	486.6%	266.0%	287.3%	465.2%
P-wyoung P-Value	0.00	0.01	0.00	0.00	0.33
Panel C: Total Pretrial Detention					
T	-6.13***	-0.52	-2.60**	0.20	-0.73
	(1.47)	(0.76)	(1.16)	(1.27)	(0.45)
R-squared	0.13	0.11	0.14	0.11	0.12
Mean if T==0	22.28	8.29	20.55	18.03	3.12
% Change after T	-27.5%	-6.3%	-12.6%	1.1%	-23.5%
P-wyoung P-Value	0.00	0.97	0.00	0.98	0.80
Observations	11,200	14,857	14,004	12,098	17,554
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Note: this table shows the results of a two-way fixed effects regression of the rates of jail-based pretrial detentions (panel A), house arrests (panel B) and the total (panel C) for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality. All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district - year-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Pretrial detention rates are computed as the ratio of the number of cases with active measures in a municipality-month to the total number of cases with imputations in that same municipality-month. The percentage change after treatment was calculated $\frac{Effect\ of\ T}{Mean\ T=0} * 100$. *** p<0.01, ** p<0.05, * p<0.1.

Figure 6. Leads-and-lags model for Settlements rate



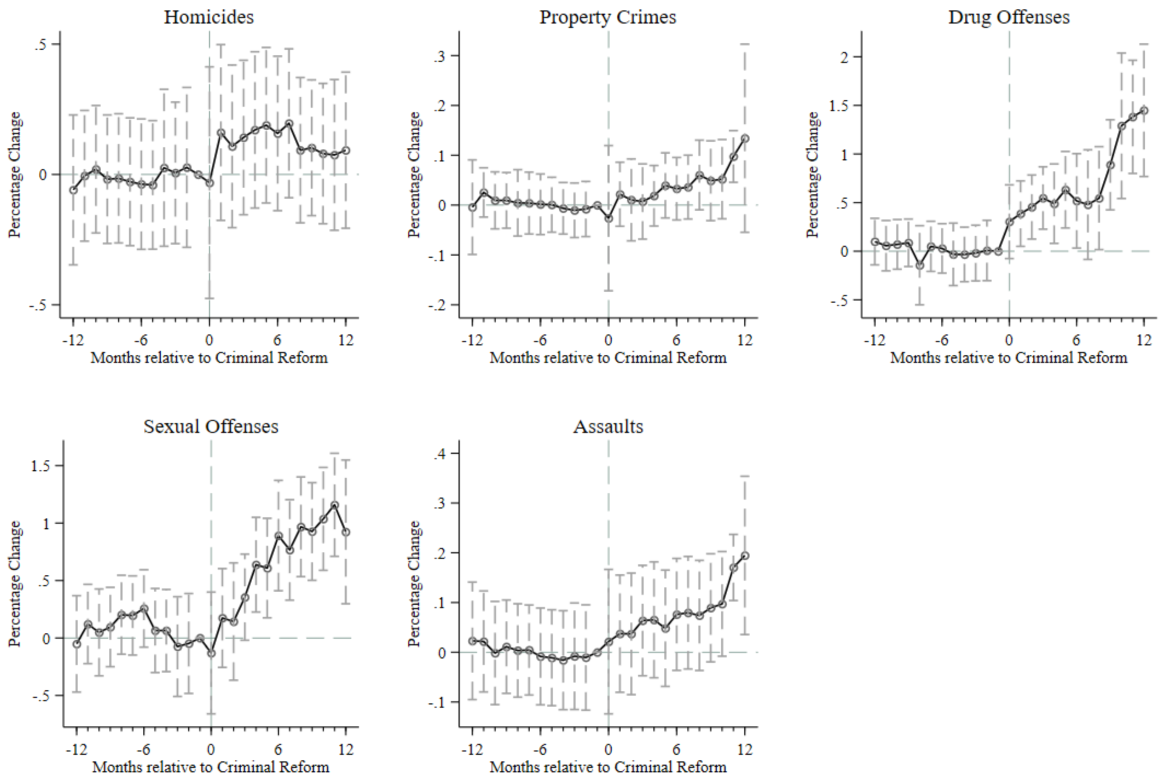
Note: this figure shows the results of an event study of settlements rates for the two minor crimes in our data for which a settlement hearing before the imputation of charges hearing is mandatory under the new system (property crimes and assault) as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Settlement rates are computed as the ratio between settlements and open cases for a specific crime in a municipality allowing for information delays and timespans, i.e., $Settlement\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Settlements_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}$.

Table 3. Difference-in-Difference Results for Settlements (Pre-Imputation)

VARIABLES	Panel A		Panel B	
	(1)	(2)	(3)	(4)
	Property Crimes	Assaults	Property Crimes	Assaults
T	1.07***	10.31***	1.08***	10.51***
	(0.12)	(0.64)	(0.12)	(0.58)
Exposure Time to T			0.03***	0.52***
			(0.01)	(0.04)
Observations	32,064	32,035	32,064	32,035
R-squared	0.40	0.59	0.40	0.61
Year Month & Month-Year FE	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Mean if T==0	2.57	15.78	2.57	15.78
% Change after T	41.7%	65.3%	54.24%	105.89%
P-wyoung P-Value	0.00	0.00	0.00	0.00

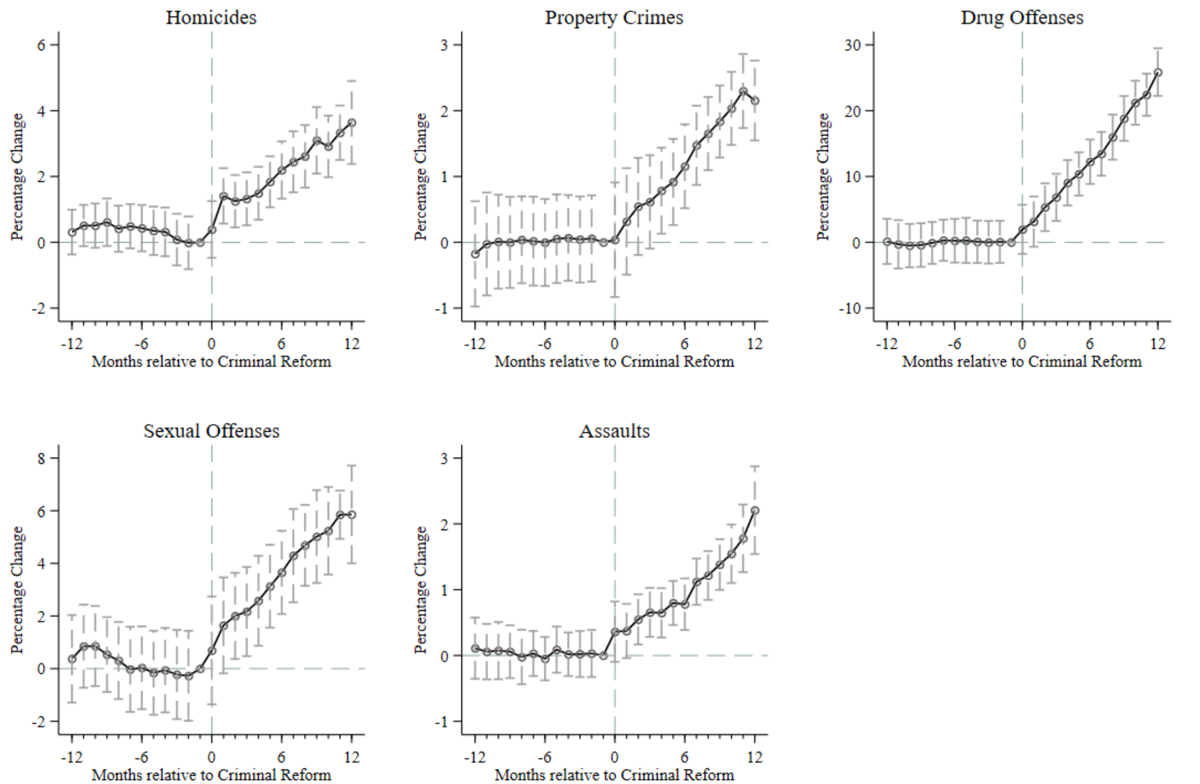
Note: this table shows the results of a two-way fixed effects regression of settlement rates for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (panel A) and the non-negative difference between a given month and the month of implementation up to 12 months (panel B). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, density of population, rural index, displaced population, and the lag of police arrest. Judicial district-year-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Settlement rates are computed as the ratio between settlements and open cases for a specific crime in a municipality allowing for information delays and timespans, i.e., $Settlement\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Settlements_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}$. The percentage change after treatment was calculated $\frac{Effect\ of\ T}{Mean\ T=0} * 100$ (panel A). To estimate the percentage change after treatment of panel B, we calculated the effect after one month of implementation as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$. ***. *** p<0.01, ** p<0.05, * p<0.1.

Figure 7A. Leads-and-lags model for acquittal rates



Note: this figure shows the results of an event study of acquittals rates for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Rates are computed as the ratio between the total number of acquittals and open cases for a specific crime in a municipality allowing for information delays and timespans, i.e., $Acquittals\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Acquittals_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}$.

Figure 7B. Leads-and-lags model for conviction rates



Note: this figure shows the results of an event study of convictions rates for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed Judicial district-month-clustered standard errors. Rates are computed as the ratio between the total number of convictions and open cases for a specific crime in a municipality allowing for information delays and timespans, i.e.,

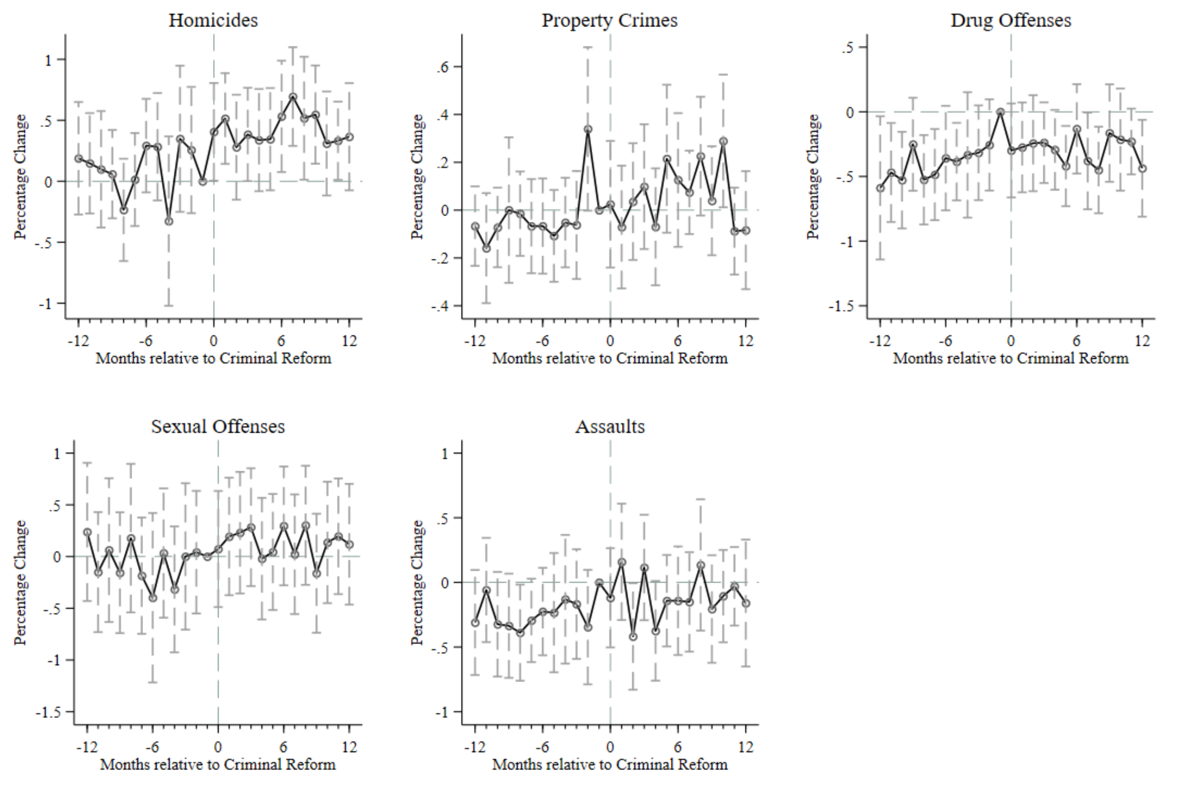
$$Convictions\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Convictions_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}.$$

Table 4. Difference-in-Difference Results for Total Acquittal and Conviction Rates

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Total Acquittals					
Panel A					
T	0.12***	0.01	0.59***	0.51***	0.06***
	(0.04)	(0.01)	(0.08)	(0.07)	(0.01)
R-squared	0.36	0.37	0.47	0.40	0.22
% Change after T	407.6%	106.0%	1617.1%	1988.3%	301.5%
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00
Panel B					
T	0.13***	0.01	0.61***	0.54***	0.06***
	(0.04)	(0.01)	(0.07)	(0.07)	(0.01)
Exposure Time to T	0.02***	0.01***	0.02***	0.06***	0.01***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
R-squared	0.36	0.37	0.47	0.40	0.22
% Change after T	1098.4%	1155.9%	2261.7%	4719.8%	799.0%
P-wyoung P-Value	0.04	0.60	0.00	0.01	0.06
Mean if T==0	0.03	0.01	0.04	0.03	0.02
Total Convictions					
Panel A					
T	1.44***	0.97***	9.69***	2.88***	0.77***
	(0.13)	(0.01)	(0.71)	(0.25)	(0.08)
R-squared	0.40	0.42	0.54	0.39	0.32
% Change after T	885.9%	1053.0%	2257.8%	687.1%	707.3%
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00
Panel B					
T	1.49***	1.01***	10.43***	3.01***	0.79***
	(0.12)	(0.10)	(0.65)	(0.23)	(0.08)
Exposure Time to T	0.12***	0.09***	0.74***	0.25***	0.04***
	(0.01)	(0.01)	(0.03)	(0.01)	(0.00)
R-squared	0.41	0.44	0.57	0.40	0.32
% Change after T	1803.1%	2204.3%	4488.8%	1429.1%	1127.5%
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00
Mean if T==0	0.16	0.09	0.43	0.42	0.11
Observations	31,760	32,064	26,267	31,169	32,035
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

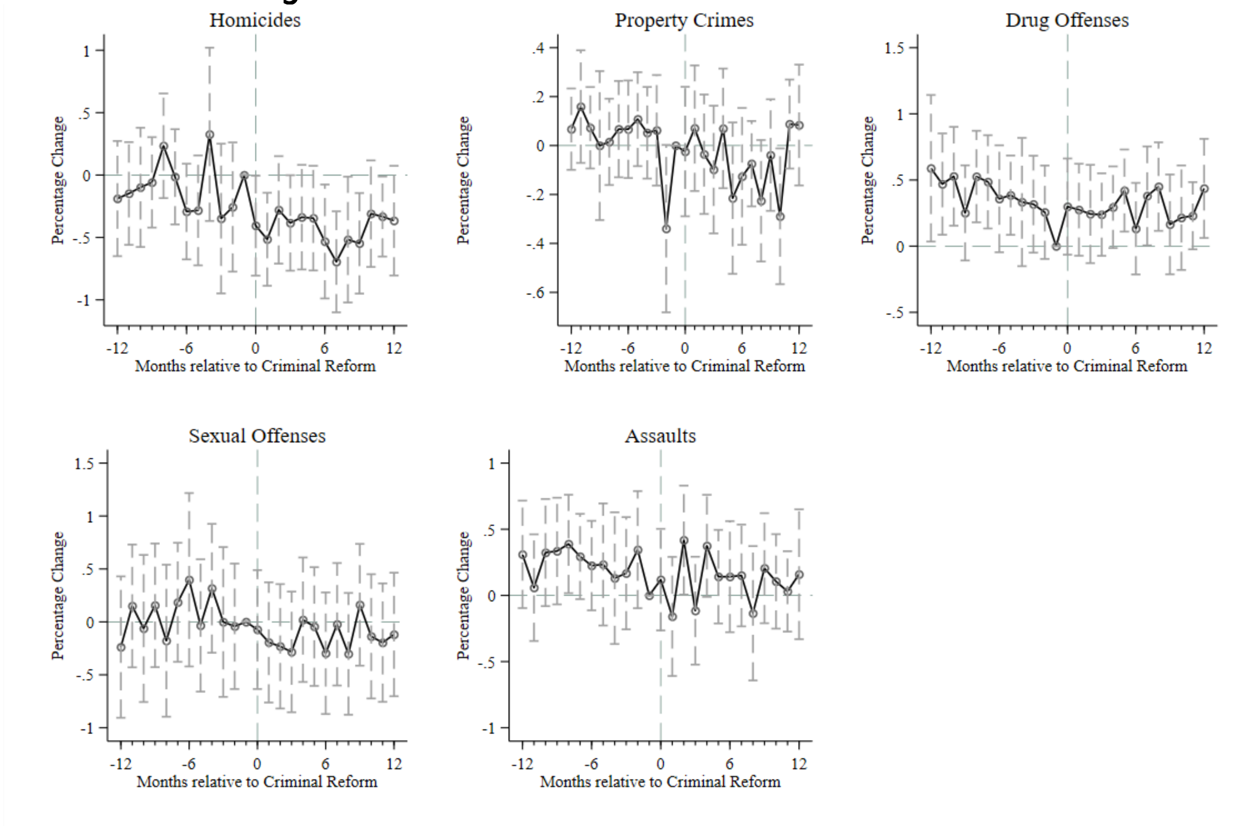
Note: this table shows the results of a two-way fixed effects regression of acquittals (top panel) or conviction rates (bottom panel) for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (panel A) and the non-negative difference between a given month and the month of implementation up to 12 months (panel B). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district - year-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Rates are computed as the ratio between the total number of acquittals or convictions and open criminal complaints for a specific crime in a municipality allowing for information delays and timespans, i.e., $Acquittals\ or\ Convictions\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Acquittals\ or\ Convictions_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Criminal\ Complaints_{i,t}^{s}}$. The percentage change after treatment was calculated $\frac{Effect\ of\ T}{Mean\ T=0} * 100$ (panel A). To estimate the percentage change after treatment of panel B, we calculated the effect after one month of implementation as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$. *** p<0.01, ** p<0.05, * p<0.1

Figure 8A. Leads-and-lags model for the share of acquittals in trial over total sentences in trial



Note: this figure shows the results of an event study of the in-trial acquittals for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Rates are computed as the ratio between the number of in-trial acquittals and open cases for a specific crime in a municipality allowing for information delays and timespans, i.e., $Acquittals\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Acquittals_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}$.

Figure 8B. Leads-and-lags model for the share of convictions in trial over total sentences in trial



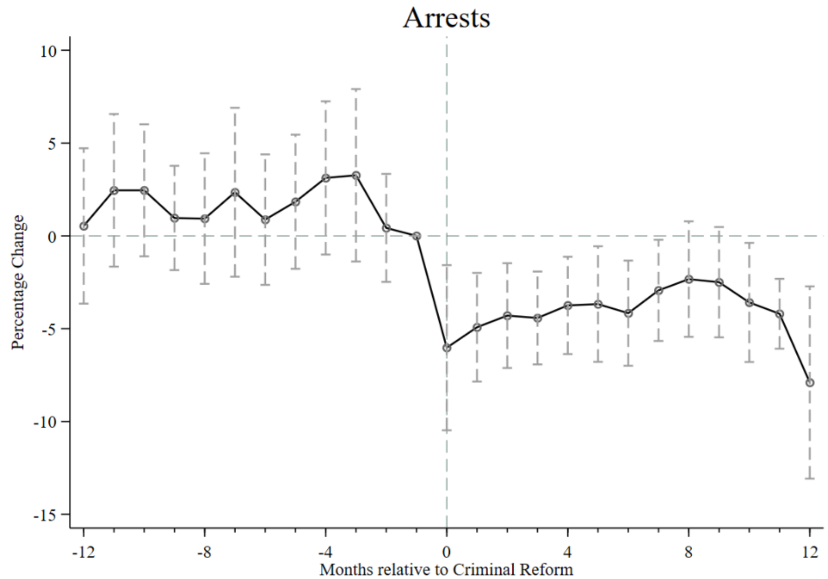
Note: this figure shows the results of an event study of the in-trial convictions for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Rates are computed as the ratio between the number of in-trial convictions and open cases for a specific crime in a municipality, allowing for information delays and timespans, i.e., $Convictions\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Convictions_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}$.

Table 5. Difference-in-Difference results for the share of in-trial acquittals and conviction over total in-trial sentences

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Acquittals in Court					
Panel A					
T	0.28***	0.11**	0.11*	0.16*	0.12*
	(0.08)	(0.05)	(0.07)	(0.08)	(0.07)
R-squared	0.47	0.39	0.52	0.44	0.47
% Change after T	216.9%	114.6%	150.6%	153.5%	89.9%
P-wyoung P-Value	0.01	0.01	0.00	0.00	0.58
Panel B					
T	0.26***	0.09*	0.14**	0.14*	0.13*
	(0.08)	(0.05)	(0.07)	(0.08)	(0.08)
Exposure Time to T	-0.00	-0.00	0.01	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R-squared	0.47	0.39	0.52	0.44	0.47
% Change after T	173.8%	43.9%	268.7%	118.8%	123.2%
P-wyoung P-Value	0.01	0.01	0.01	0.00	0.51
Mean if T==0	0.13	0.09	0.07	0.10	0.14
Convictions in Court					
Panel A					
T	-0.28***	-0.11**	-0.11*	-0.16*	-0.12*
	(0.08)	(0.05)	(0.07)	(0.08)	(0.07)
R-squared	0.47	0.39	0.52	0.44	0.47
% Change after T	-32.4%	-11.8%	-12.0%	-17.2%	-14.4%
P-wyoung P-Value	0.01	0.01	0.00	0.00	0.58
Panel B					
T	-0.26***	-0.09*	-0.14**	-0.14*	-0.13*
	(0.08)	(0.05)	(0.07)	(0.08)	(0.08)
Exposure Time to T	0.00	0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R-squared	0.47	0.39	0.52	0.44	0.47
% Change after T	-26.0%	-4.5%	-21.4%	-13.3%	-19.7%
P-wyoung P-Value	0.01	0.01	0.01	0.00	0.51
Mean if T==0	0.87	0.91	0.93	0.90	0.86
Observations	1,068	1,314	1,002	1,431	947
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Note: this table shows the results of a two-way fixed effects regression of the in-trial acquittals (top panel) or conviction rates (bottom panel) for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (panel A) and the non-negative difference between a given month and the month of implementation up to 12 months (panel B). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Judicial district-year-clustered standard errors in parentheses. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Rates are computed as the ratio between the number of in-trial acquittals or convictions and total in-trial decisions for a specific crime in a municipality allowing for information delays and timespans, i.e., $Court\ Acquittals\ or\ Convictions\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} In-trial\ Acquittals\ or\ Convictions_{i,t}^{s}}{\sum_{t=-1}^{11} In-trial\ decisions_{i,t}^{s}}$. The percentage change after treatment was calculated $\frac{Effect\ of\ T}{Mean\ T=0} * 100$ (panel A). To estimate the percentage change after treatment of panel B, we calculated the effect after one month of implementation as $\frac{\beta_1 + \beta_2 * 12}{\mathrm{Mean\ T=0}} *$. *** p<0.01, ** p<0.05, * p<0.1

Figure 9. Leads-and-lags model for arrest rates



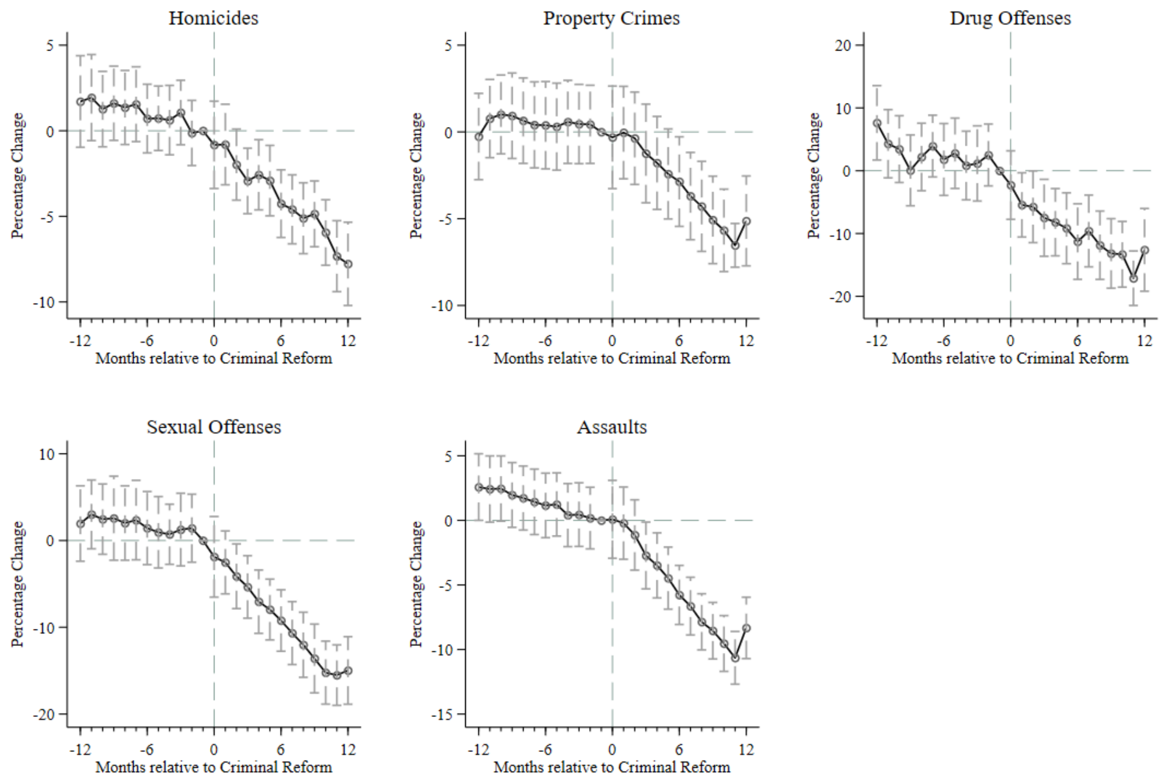
Note: this figure shows the results of an event study of the arrest rate by 100,000 inhabitants as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with judicial district-month-clustered standard errors.

Table 6. Difference-in-Difference results for arrest rates

VARIABLES	(1) **Arrest rate**	(2) **Arrest rate**
T	-5.46***	-5.10***
	(0.54)	(0.49)
Exposure Time to T		-0.19***
		(0.05)
Observations	78,894	77,094
R-squared	0.32	0.31
Year Month & Month-Year FE	YES	YES
Municipio FE	YES	YES
Controls	YES	YES
Mean T = 0	14.05	14.05
Effect of T	-38.9%	-52.4%

Note: this table shows the results of a two-way fixed effects regression of the logarithm of arrest rates on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (column 1) and the non-negative difference between a given month and the month of implementation up to 12 months (column 2). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district-year-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Effect of T is calculated as $\frac{Effect\ of\ T}{Mean\ T=0} * 100$ for column (1) and as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$ * for column (2).

Figure 10. Leads-and-lags model for clearance rates



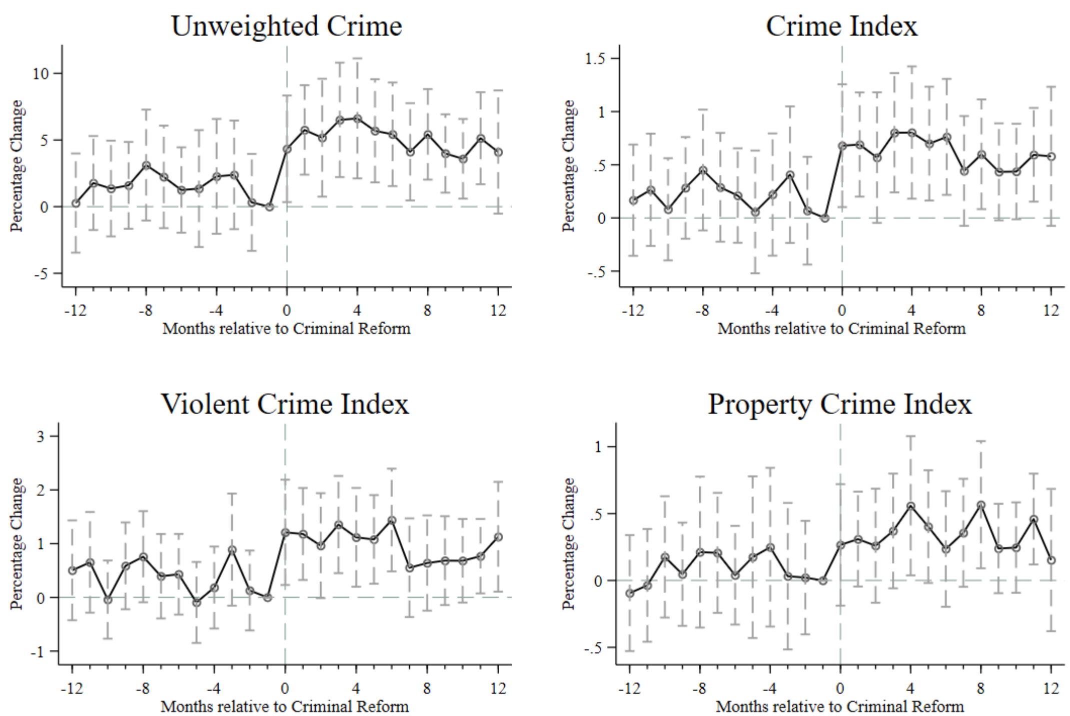
Note: this figure shows the results of an event study of the logarithm of clearance rates for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Clearance rates are computed as the ratio between imputations and open cases for a specific crime in a municipality, allowing for information delays and timespans, i.e., $Clearance\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Imputations_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Cases_{i,t}^{s}}$.

Table 7. Difference-in-Difference results for Clearance rates

VARIABLES	(1) Homicides	(2) Property Crimes	(3) Drug Offenses	(4) Sexual Offenses	(5) Assaults
Panel A					
T	-4.01***	-3.27***	-11.06***	-9.64***	-5.99***
	(0.34)	(0.29)	(0.79)	(0.59)	(0.38)
% Change after T	-23.3%	-24.2%	-15.6%	-24.7%	-27.3%
R-squared	0.39	0.48	0.42	0.43	0.60
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00
Panel B					
T	-3.98***	-3.23***	-10.71***	-9.60***	-6.00***
	(0.35)	(0.30)	(0.77)	(0.60)	(0.38)
Exposure Time to T	0.08***	0.13***	0.35***	0.07	-0.02
	(0.02)	(0.03)	(0.06)	(0.05)	(0.03)
% Change after T	-17.2%	-12.8%	-9.2%	-22.3%	-28.3%
R-squared	0.385	0.484	0.418	0.431	0.60
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00
Observations	31,760	32,064	26,267	31,169	32,035
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipality FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Mean if T==0	17.21	13.50	70.74	39.03	21.94

Note: this table shows the results of a two-way fixed effects regression of the clearance rates for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (panel A) and the non-negative difference between a given month and the month of implementation up to 12 months (panel B). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district-year-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Clearance rates are computed as the ratio between imputations and open criminal complaints for a specific crime in a municipality allowing for information delays and timespans, i.e., $Clearance\ Rate_{i,t}^{s} = \frac{\sum_{t=0}^{-11} Imputations_{i,t}^{s}}{\sum_{t=-1}^{11} Open\ Criminal\ Complaints_{i,t}^{s}}$. The percentage change after treatment was calculated $\frac{Effect\ of\ T}{Mean\ T=0} * 100$. To estimate the percentage change after treatment of panel B, we calculated the effect after one month of implementation as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$. *** p<0.01, ** p<0.05, * p<0.1

Figure 11. Leads-and-lags model for Aggregate Crime Indices



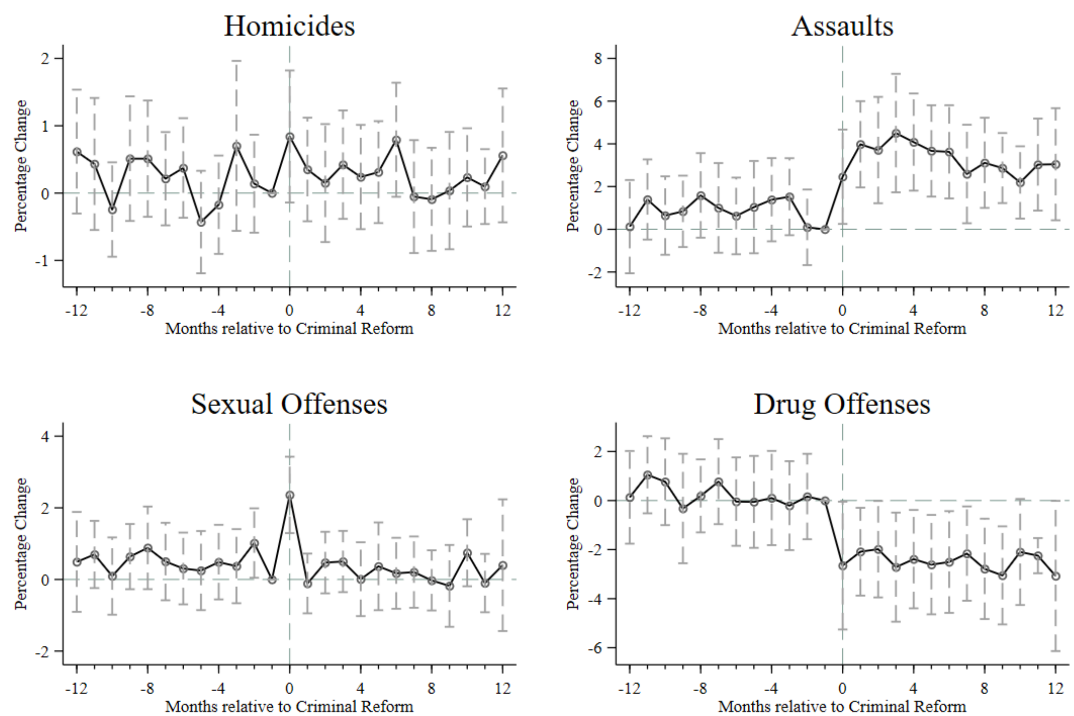
Note: this figure shows the results of an event study of the logarithm of different crime rates as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors. Crime indices are computed as the weighted average of different types of crimes, where the weights are given by the average sentence length of each type of crime.

Table 8. Difference-in-Difference results for Aggregate Crime Indices

VARIABLES	Panel A				Panel B			
	(1) Unweighted Crime	(2) Crime Index	(3) Violent Crime Index	(4) Property Crime Index	(5) Unweighted Crime	(6) Crime Index	(7) Violent Crime Index	(8) Property Crime Index
T	4.05***	0.46***	0.62***	0.33***	3.56***	0.41***	0.61***	0.25***
	(0.60)	(0.08)	(0.13)	(0.08)	(0.60)	(0.08)	(0.13)	(0.08)
Exposure Time to T					0.21***	0.02***	0.01	0.04***
					(0.03)	(0.00)	(0.01)	(0.00)
Observations	75,976	75,976	75,976	75,976	75,976	75,976	75,976	75,976
R-squared	0.30	0.22	0.15	0.35	0.30	0.22	0.15	0.35
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T = 0	13.53	2.45	4.12	1.16	13.53	2.45	4.12	1.16
Effect of T	30.0%	18.7%	15.2%	28.7%	44.5%	27.4%	16.3%	57.7%

Note: this table shows the results of a two-way fixed effects regression of different crime rates on an indicator variable that equals one if the adversarial procedural reform has been implemented in a given municipality and month (all columns) and the non-negative difference between a given month and the month of implementation up to 12 months (columns 5 to 8). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district-month-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Effect of T is calculated $\frac{Effect\ of\ T}{Mean\ T=0} * 100.$ for columns (1) to (4) and as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$ for columns (5) to (8).Crime indices are computed as the weighted average of different types of crimes, where the weights are given by the average sentence length of each type of crime and are presented in Table 2A. *** p<0.01, ** p<0.05, * p<0.1

Figure 12A. Leads-and-lags model for Violent and Drug-related crimes



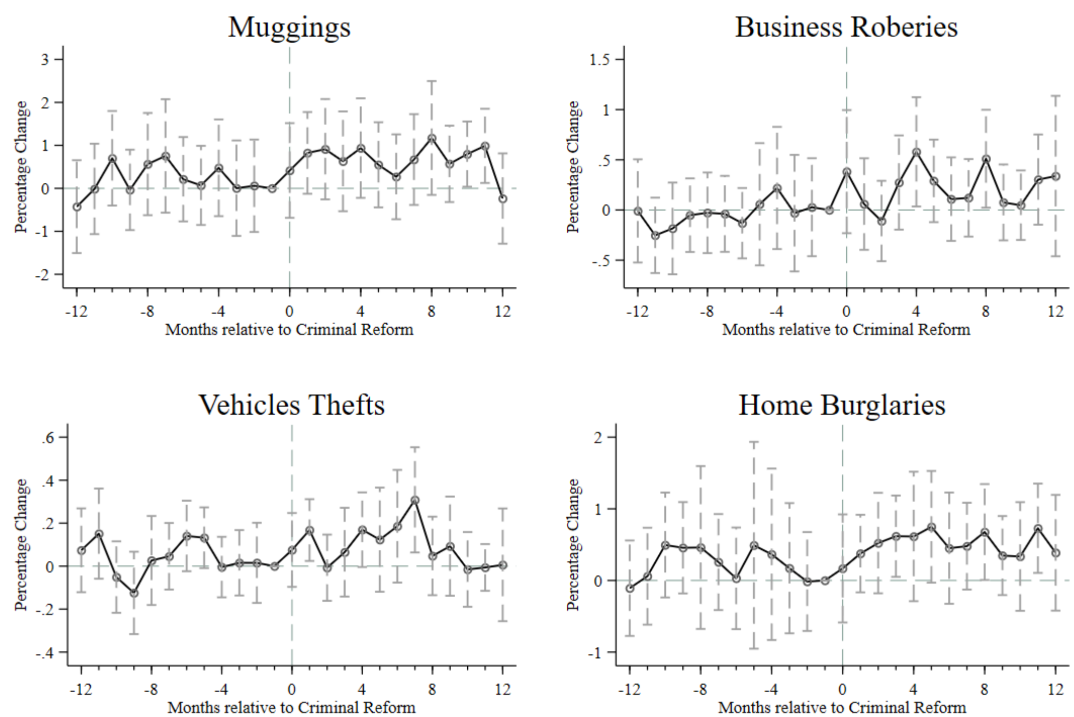
Note: this figure shows the results of an event study of different crime rates as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors.

Table 9A. Difference-in-Difference results for Violent and Drug-related crimes

VARIABLES	Panel A				Panel B			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Homicides	Assaults	Sexual Offenses	Drug Offenses	Homicides	Assaults	Sexual Offenses	Drug Offenses
T	0.04	2.58***	-0.09	-2.76***	0.07	2.43***	-0.19	-3.14***
	(0.12)	(0.33)	(0.16)	(0.25)	(0.19)	(0.32)	(0.16)	(0.27)
Exposure Time to T					-0.01	0.06***	0.02*	0.21***
					(0.01)	(0.02)	(0.01)	(0.03)
Observations	75,976	75,976	60,882	60,882	75,976	75,976	60,882	60,882
R-squared	0.14	0.17	0.13	0.30	0.14	0.17	0.13	0.31
P-wyoung P-Value	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T = 0	3.98	4.52	4.13	2.94	3.98	4.52	4.13	2.94
Effect of T	1.1%	57.0%	-2.1%	-93.9%	-1.8%	70.0%	1.8%	-21.6%

Note: this table shows the results of a two-way fixed effects regression of different crime rates on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (all columns) and the non-negative difference between a given month and the month of implementation up to 12 months (columns 5 to 8). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district - year-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Effect of T is calculated as $\frac{\beta_1}{Mean\ T=0} * 100$ for columns (1) to (4) and as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$ for columns (5) to (8). *** p<0.01, ** p<0.05, * p<0.1

Figure 12B. Leads-and-lags model for Property crimes



Note: this figure shows the results of an event study of the logarithm of different crime rates as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with Judicial district-month-clustered standard errors.

Table 9B. Difference-in-Difference results for Property crimes

	Panel A				Panel B			
VARIABLES	(1) Muggings	(2) Business Robberies	(3) Vehicles Thefts	(4) Home Burglaries	(5) Muggings	(6) Business Robberies	(7) Vehicles Thefts	(8) Home Burglaries
T	0.73***	0.26***	0.06*	0.37***	0.47**	0.21**	0.07**	0.30**
	(0.23)	(0.08)	(0.03)	(0.13)	(0.24)	(0.08)	(0.04)	(0.13)
Exposure Time to T					0.11***	0.02***	-0.00*	0.03***
					(0.01)	(0.00)	(0.00)	(0.01)
Observations	75,976	75,976	75,976	75,976	75,976	75,976	75,976	75,976
R-squared	0.30	0.14	0.11	0.19	0.31	0.14	0.11	0.19
P-wyoung P-Value	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T = 0	2.42	0.78	0.44	1.39	2.42	0.78	0.44	1.39
Effect of T	30.3%	34.1%	14.6%	26.6%	73.5%	62.5%	8.2%	45.9%

Note: this table shows the results of a two-way fixed effects regression of different crime rates on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (all columns) and the non-negative difference between a given month and the month of implementation up to 12 months (columns 5 to 8). All regressions control for municipality, year, month, and year*month fixed effects, as well as for per capita Industry and Business tax collection, per capita investment in education, fiscal performance, population density, rural index, displaced population, and the lagged police arrest rate. Judicial district-month-clustered standard errors in parentheses. P-Wyoung P-Value: P-value adjusted using Young's method (1990), used as a multiple hypothesis testing procedure to correct for dependence in errors. Mean T=0 corresponds to the mean of the dependent variable for those observations in the control group before the implementation of the reform. Effect of T is calculated as $\frac{\beta_1}{Mean\ T=0} * 100$ for columns (1) to (4) and as $\frac{\beta_1 + \beta_2 * 12}{Mean\ T=0} * 100$ * for columns (5) to (8). *** p<0.01, ** p<0.05, * p<0.1