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

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Abstract

In this paper we exploit the arguably exogenous staggered implementation of an extensive criminal procedural reform in Colombia between 2005 and 2008 to assess its intended and unintended consequences. The reform had explicit objectives, such as guaranteeing due process protection of the accused, reducing the use of pretrial detention, making the processing of criminal cases more efficient, reducing procedural times, and improving the mechanisms for early termination of criminal processes. Our results show that the reform achieved most of its goals. Namely, a significant reduction in the use of pretrial detention of about 17%-34%; a large and significant reduction in procedural times (18%); an increase in the use of mechanisms for early termination of the criminal process through settlements (43%-66%); and a large and significant increase in the percentage of cases that reach adjudication. Nevertheless, the reform also had negative unintended consequences on arrest, clearance, and crime rates. Our results indicate that arrest rates decreased by about 33% and clearance rates by 16%-27%. The reform also directly affected the incentives for criminal behavior and led to an increase in both property crimes (19%) and violent crimes (17%) as a result of the implementation of the reform. Our paper shows that well-intended reforms aimed at increasing due process protection can create unintended consequences in the administration of justice that led to increases in crime and raises the question of how to balance constitutional protections with public safety by creating special provisions and guidelines directed to mitigate potential adverse effects on crime rates.

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

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I. Introduction

The literature exploring how penal law affects crime rates has primarily focused on studying the impact of substantive penal law changes on criminal behavior. This literature has been extremely important in understanding the criminal justice system's role in shaping the incentives to engage in criminal activities. Some of the most notable work in this area addresses the impact of 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) on criminal behavior.

On the other hand, relatively few studies to date have explored how changes to criminal procedure affect crime. These studies have explored the impact of higher judicial productivity (Soares & Sviatschi, 2010), improvements in due process protection (Atkins & Rubin, 2003), and reductions in pretrial detention (Cepeda-Francese & Ramírez-Álvarez, 2023; Chalfin & McCrary, 2017; Leslie & Pope, 2017) on crime rates. However, most of the research in this area analyzes the effects of small procedural changes rather than exploring the overall impact of extensive procedural reforms, such as the ones implemented in most Latin American countries over the past three decades. A comprehensive criminal procedural reform typically involves simultaneous changes to multiple parts of the criminal process that simultaneously affect the incentives for criminal activity in different directions. Thus, in our view, assessing the overall effects of an extensive procedural reform is a first order question that, to the best of our knowledge, our paper is the first to answer.

In this paper, we take up this challenge to assess the multiple effects of one of the major criminal procedural reforms in the Americas. We focus on the adversarial reform that took place in Colombia between 2005 and 2008. More precisely, we exploit the arguably exogenous staggered implementation of Colombia's extensive criminal procedural restructuring to assess its intended and unintended consequences. The adversarial reform in Colombia took effect in four stages between 2005 and 2008, with approximately 25% of the population (and criminal cases) entering in each stage. The staggered implementation of Colombia's adversarial reform, plus access to highly detailed data on judicial decisions, outcomes, and crime rates, offers a unique opportunity to undertake a thorough evaluation to assess both its intended consequences (on judicial efficiency, procedural times, the use of pretrial detention, the use of early termination mechanisms to reduce congestion and adjudication rates) and its unintended consequences on arrest, clearance, and crime rates.

During the last 35 years, all Latin American countries except Brazil abandoned the inquisitorial criminal justice model and adopted an adversarial model (Figure 1). The inquisitorial systems had been characterized by long processes in which a trial judge decided each case based on a written

dossier that included all procedural activity and evidence presented by the prosecutor (Langer, 2007). The transition to an adversarial system eliminated the written nature of the process in favor of an oral process, with public hearings before an impartial judge (Damaska, 2001; Langer, 2014). Under the old inquisitorial model, the investigation was carried out by an examining magistrate (*juez de instrucción penal*), with no clear distinction between investigative and adjudicative functions. Under the new adversarial system, the investigation is carried out by a public prosecutor, and the role of the judge is now limited to adjudication, thus ensuring an impartial role on her part.

Although the specific changes to criminal procedure introduced by the Latin American adversarial reform varied across countries, the reforms were guided by common objectives. First, the reform aimed to improve judicial productivity, by strengthening the mechanisms for the early termination of the criminal process to reduce congestion and procedural times and increase adjudication rates. Before the reform, a criminal process rarely ended in a judicial sentence (Martínez Cuéllar et al., 2008), causing a rising perception of impunity within the criminal justice systems in the region (Dammert, 2012). Second, the reform intended to increase due process protection and guarantee defendants' rights. To achieve this, in some Latin American countries, the reform established strict limits on the time that can elapse between the filing (imputation) of charges and the indictment hearing[1]. Finally, the reform intended to significantly reduce the use of pretrial detention (Hartmann Arboleda, 2016; McLeod, 2010). Before the reform, most Latin Americans incarcerated were pretrial detainees. Between 1980 and 1990, the pretrial detention population accounted for more than 50% of incarcerated people in Latin American countries, which affected the legitimacy of the criminal justice system (Carranza, 2001; Duce et al., 2009; Hartmann Arboleda, 2016; McLeod, 2010).

The changes introduced by the adversarial reform affected the incentives to engage in criminal activities in different directions. For example, the literature has associated increases in judicial productivity and shortening of judicial processes with lower crime rates. Yet, those results rely on the assumption that productivity increases are not associated with changes in prosecutorial behavior. For instance, if part of these productivity increases comes from prosecutorial decisions not to prosecute and investigate some cases (i.e., a reduction of the clearance rate), the incentives to engage in criminal activities may increase. Moreover, reducing the use of pretrial detention has been associated with lower crime rates in the long run (Leslie & Pope, 2017) and higher crime rates in the short run (Chalfin & McCrary, 2017; Leslie & Pope, 2017). Finally, streamlining mechanisms for an early termination of

[1] The imputation of charges is done in a hearing before a supervisory judge (*juez de control de garantías*) in which the prosecutor informs the defendant that the General Prosecutor's Office (*Fiscalía*) is opening a criminal investigation for the alleged commission of a crime, without necessarily revealing the evidence it has against the defendant. The indictment is also made in a hearing before a judge, where the prosecutor investigating the case reveals the main evidence that the General Prosecutor's Office has to formally accuse the defendant of the alleged commission of the crime.

the criminal process, such as pre-imputation settlements and plea bargaining, may also change the expected discounted cost of committing a crime in different directions. While a reduced expected sentence because of a settlement or a plea deal may reduce this cost, the reduction in the time to reach a conviction may increase it. Hence, the question of what effects dominates becomes an empirical question.

We propose to answer this question by evaluating the effect of an extensive procedural reform exploiting the quasi-experimental variation resulting from the reform's staggered implementation in Colombia between 2005 and 2008. Our empirical strategy consists of two approaches. In the first approach we estimate event study models using two-way fixed effects (TWFE) to assess the plausibility of the parallel trends and exogeneity assumptions, as well as the dynamic effects of the reform after a year of implementation. As a robustness check, we estimate the event studies using the doubly robust estimator proposed by Callaway & Sant'Anna (2021). As a second approach, we estimate a conditional differences-in-difference model to obtain the causal effects of the reform on the outcomes of interest. We complement these results by estimating the model using the methodology proposed by Wooldridge (2021). Results remain quite similar with the different methodologies.

To offer an overview of the effects of the reform on the administration of criminal justice and how these changes affected crime rates, we assess the impact of the reform on procedural productivity, judicial and prosecutorial decongestion, and pretrial detention rates. Using the results on the different potential mechanisms, we theorize how the adversarial reform changed the incentives to engage in criminal activities. Thus, our paper offers an important contribution to the ongoing literature exploring the effect of procedural law on crime, as it provides a complete overview on how specific changes might dominate others under specific institutional arrangements.

Our results on procedural productivity show that the reform significantly reduced procedural times. As the reform mandated, the time elapsed between imputation of charges and indictment decreased substantially after introducing the adversarial system, going from 500 days before the implementation of the reform to about 90 days afterwards. However, the average number of days between the opening of the investigation and the imputation of charges increased, from 65 days to 145 days after the reform was implemented. Put together, the time between the opening of the investigation and the indictment hearing was substantially reduced (by about 115 days, around 4 months) with the implementation of the reform.

Regarding prosecutorial and judicial decongestion, our results show that changes associated with streamlining the mechanisms of early termination of criminal processes increased the proportion of

criminal investigations that ended up in a pre-imputation settlement.[2] For property crimes cases, the estimated increase is 42.6%, while for assaults it is 66%. Our results also indicate that this effect increased over time after the implementation of the reform. Moreover, our findings confirm that the reform achieved its goal of increasing adjudication rates (i.e., the fraction of cases that reach a conviction), due to the increase in settlements or guilty pleas, not convictions in trial. For the case of acquittal decisions, we find an increase both for settlements and in-trial.

Finally, our analysis on pretrial detention is divided in two parts. First, we focus on the changes introduced by the reform to reduce the time a person spends in jail as a pretrial detainee. The reform limited the time that could elapse between the imputation of charges and the indictment hearing. This led prosecutors to be much more selective in the cases they charged. As a result, the clearance rate (defined as the percentage of criminal cases in which the alleged perpetrator is identified and the prosecutor files charges before a judge) was negatively affected by the implementation of the reform for all crime categories. For homicides, the estimated reduction in the clearance rate is 23.7%, for assaults is 26.7%, for sexual offences 24.5% and for property crimes 23.6%. Our results also indicate that arrest rates declined significantly with the implementation of the reform.

Second, we estimate the effect of the reform on pretrial detention decisions. Our results show that the use of pretrial detention was significantly reduced with the implementation of the reform for most crimes. For homicides, our TWFE estimations show that the reduction in the percentage of cases with imputation of charges that ended up in the defendant's pretrial detention in jail of the defendant is 34.4% after the implementation of the reform; for property crime cases the reduction in the use of pretrial detention in jail is 17.5%, for drug-related crimes 31%, and for assaults 32%. For sexual offenses, we estimate a reduction in the use of pretrial detention in jail of about 9%, but the estimated coefficient is not statistically significant. We also find that the implementation of the reform led to a very large *increase* in the use of domiciliary detention for most crimes. However, the large increase in domiciliary detention rates was not enough to compensate for the decrease in pretrial detention in jail. Overall, pretrial detention (jail plus domiciliary) was significantly reduced with the implementation of the adversarial reform: by 28% for homicides and 12% for drug-related crimes. For property crimes, sexual offenses and assaults, the net effect on pretrial detention (jail plus domiciliary) was null.

As explained before, due to all the changes introduced by the adversarial reform, it is quite difficult to theoretically predict the net effect of the reform on crime rates. Thus, we take this question to the

[2] The reform also streamlined the mechanisms of early termination of criminal processes through settlements between the victim and the alleged perpetrator before the imputation of charges and only for minor crimes.

data. Our results indicate that the reform increased overall crime. For our aggregate crime index,[3] the estimated increase is 12% after the implementation of the reform; for our violent and property crime indices[4] the estimated increase is 15% and 8%, respectively. When we estimate the overall effect of the reform on individual crimes, we find a statistically significant increase in almost all crime categories: 3% for homicides, 25% for assaults, 9% for muggings, 4% for business robberies, 2% for vehicle thefts and 6% for home burglaries. Our results also show that the estimated effects of the reform on crime increased over time during the first 12 months after implementation.

Our paper contributes to the literature exploring how the criminal justice system shapes incentives to engage in criminal activities through changes in sentencing (Dominguez-Rivera et al., 2019; Owens, 2009), incapacitation (Liedka et al., 2006; Lofstrom & Raphael, 2016), and prison rehabilitation (Escobar et al., 2023; Gaes & Camp, 2009; Tobón, 2022), among others. Our results suggest that while a procedural reform might achieve desirable outcomes, like increasing conviction rates, it might have unintended consequences on policing and criminal activity. Thus, it is necessary to consider how changes to procedural law affect the incentives of all criminal justice agents to avoid undesirable effects on crime.

The rest of the paper is organized as follows. Section II discusses the existing literature exploring the effects of criminal procedural reforms on crime. Section III explains the institutional context of the adversarial reform and its implementation in Colombia. Section IV describes the data we use to estimate the impact of the adversarial reform on judicial decisions, judicial outcomes, and crime. Section V describes the two approaches used in our empirical strategy. In Section VI, we present the main results, while in Section VII we present some concluding remarks.

II. Criminal Procedure & Crime Rates

The limited but growing literature examining the relationship between criminal procedure and crime has identified three main channels through which changes in the criminal process can affect criminal behavior: (i) certainty, (ii) severity, and (iii) celerity. The "*certainty* effect" has been explored in the context of reforms aiming to improve judicial productivity. Scholars have connected increases in judicial productivity with a higher probability of punishment and, hence, a potential reduction in crime rates (Nagin, 2013; Nagin & Pogarsky, 2003; Pogarsky, 2002). Empirical evidence in this area suggests that increases in judicial productivity can reduce crime rates between 14% and 17% (Soares & Sviatschi, 2010). However, if higher judicial productivity is achieved by being more selective in the

[3] Calculated as a weighted sum of homicides, assaults, sexual offenses, muggings, business robberies, vehicle theft, and home burglaries. The weights are constructed using the average sentence established by the penal code for each crime.
[4] The violent crime index is calculated as a weighted sum of homicides, assaults, and sexual offenses. The property crime index is calculated as a weighted sum of muggings, business robberies, vehicle theft, and home burglaries.

criminal cases that are investigated and prosecuted, fewer criminal cases would be processed, and this may lead to an increase in crime.

Other studies exploring how changes in criminal procedure affect the certainty of punishment have focused on reforms that increase due process protections by creating stricter conditions to incarcerate an accused (Atkins & Rubin, 2003). In the Colombian adversarial reform, the legislature intended to increase due process protection during the imposition of pretrial detention by eliminating the prosecutor's control over this decision. Zorro-Medina (2020) shows that the Colombian reform achieved its goal of reducing the pretrial detention population in jail, yet the effect of this change on crime is still unknown. Previous empirical evidence in other contexts offers mixed results. On the one hand, greater difficulty in obtaining pretrial detention could increase crime rates by decreasing the expected certainty of imprisonment and the incapacitation effect (Chalfin & McCrary, 2017). On the other hand, lower pretrial detention rates reduce the likelihood of recidivism, leading to lower crime rates (Leslie & Pope, 2017; Walker & Herting, 2020). Other studies exploring the effects of reducing the use of pretrial detention have found no effects on crime rates (Agan et al., 2021).

Besides the certainty effect, other studies have explored how changes in criminal procedure can affect the *severity* of punishment. Previous research has linked shorter criminal processes with severer punishment and a greater deterrent effect (Chalfin & McCrary, 2017; Lee & McCrary, 2017). Dalla Pellegrina (2008) explores the impact of court delays on crime rates, concluding that longer criminal processes increase property crimes. The Colombian adversarial reform affected only procedural justice and not substantive penal law, meaning it did not directly modify the sentences associated with criminal offenses. However, the reform introduced changes that affected the *timing* of punishment. For instance, the introduction of plea bargaining opened the possibility of obtaining a conviction faster. Although plea bargaining substantially reduces the process's length, bringing punishment closer in time, it usually involves shorter prison sentences (Ulmer et al., 2010; Ulmer & Bradley, 2006). Zorro Medina (2019) suggests that although the adversarial reform increased entries to prison, it also increased prison releases. Therefore, while the adversarial reform increases the certainty of punishment, it also might decrease the length of prison sentences.

Finally, research has identified *celerity* as a crucial third factor. Scholars exploring celerity have focused on how prosecutors and police officers change their behavior when fast-track options are created. For instance, Dusek (2015) explores the effect of a fast-track processing of minor offenses implemented in the Czech Republic and shows that prosecutors and police officers transferred resources from severe crimes to less serious cases. This result suggests that shortening the

procedural length of specific offenses decreases the relative cost of pursuing them; this change in policing incentives causes a relative increase in the prevalence of other, unaffected, offenses.

Although the adversarial reform in Colombia decreased the procedural length of prosecuting all offenses, most of the effect came from creating fast-track mechanisms available for minor offenses and property crimes. The changes in criminal agreements that made pre-imputation settlement hearings mandatory for minor offenses and property crimes opened the possibility of closing criminal complaints without opening a formal criminal case. These changes might have incentivized prosecutors to seek pre-imputation settlements between the victim and the suspect to avoid opening a criminal case, thus eliminating the option to pursue a prison sentence. Thus, these fast-track mechanisms could decrease the expected probability of being punished, leading to increasing crime rates.

Based on the literature exploring the effects of criminal procedural reforms on crime rates, it is hard to anticipate the impact of the adversarial reform on crime due to all the simultaneous changes introduced. In this paper, we intend to contribute to the growing literature exploring the relationship between criminal procedure and crime by evaluating how complex reforms that simultaneously affect the severity, celerity, and certainty of punishment can impact crime rates and the behavior of criminal justice agents.

The most closely related works to our paper are three recent papers that estimate the impact of criminal procedure reforms on crime in Peru (Hernández, 2019), Mexico (Cepeda-Francese & Ramírez-Álvarez, 2023) and Uruguay (Cattaneo et al., 2022). Complex implementation processes in different countries impose empirical challenges when trying to identify the causal effect of the adversarial reform on crime. For instance, Hernandez (2019) uses a differences-in-differences strategy with matching estimators to assess the effects of the adversarial reform on crime and the perceived risk of crime in Peru. He finds that the reform slightly reduced aggregate and property crime in the group of early implementers, but that the initial observed reduction in crime was temporary and vanished three years after the implementation. For the group of late implementers, Hernandez shows that the result is the opposite: the reform *increased* crime. However, changes in the implementation schedule in Peru affect the proper comparison between treatment and control units, as several cities started their transition to the new adversarial system before their official implementation date (Ministerio Público Peru, 2005; Poder Judicial, 2022), thus affecting the causal identification of these results.

Cepeda-Francese & Ramírez-Álvarez (2023) evaluate the effects of the implementation of the adversarial model in Mexico between 1997 and 2012 using municipality level data and a generalized synthetic control group approach. They find that the implementation of the reform increased

homicides and reduced the use of pretrial detention for property crime cases. The estimated effect on homicides is much greater in municipalities with an established presence of criminal organizations. The authors also face methodological problems associated with the implementation process of the reform in Mexico, as several states only partially implemented the reform during the period of analysis (Consejo de la Judicatura Federal, 2023; Ortega, 2016). Yet, Cepeda-Francese & Ramírez-Álvarez (2023) assumed these different levels of implementation were equivalent, without considering whether their results could be capturing the effect of having an incomplete implementation of the reform, instead of a fully implemented one.

Finally, for the case of Uruguay, Cattaneo et al. (2022) find that the implementation of the adversarial reform led to an increase in police reports for all common offenses in Montevideo, the country's capital. Using a regression-discontinuity design, the authors find a significant increase in the total number of reports filed by the police of about 26% when the reform entered into force, together with a decrease of about 42% in the number of imputations of charges.

To the best of our knowledge, our paper provides the most comprehensive evaluation available to date of the intended and unintended effects of the adversarial reform in the Latin American context. Having access to very detailed data on judicial decisions and outcomes allows us not only to assess the impact of the intended objectives of the reform (on pretrial detention, the efficiency of the justice system, and adjudication rates), but also to understand better the different channels through which this type of reform may affect the incentives to engage in criminal activities. Our paper contributes to the literature by offering a thorough analysis of the effects of an extensive criminal procedural reform in Colombia and assessing its intended and unintended consequences. Our results show that while the procedural reform in Colombia might have achieved most (if not all) of its intended outcomes, it had unintended consequences on clearance rates, policing activity, and crime rates.

III. Institutional context: The adversarial reform in Colombia

On August 31, 2004, the Colombian Congress approved a new criminal procedural code that transformed the administration of penal justice from an inquisitorial model to an adversarial model. Considering the challenges of implementing the new adversarial model, the Colombian Congress designed a staggered rollout for the implementation, dividing the 33 judicial districts[5] of the country into four stages. Each stage involved approximately 25% of the total population of the entire country.

[5] A judicial district is an administrative division of the Judicial Branch in Colombia. Judicial districts do not necessarily coincide with the country's administrative division (*departamentos*), but each municipality in the country can be linked to one of the 33 existing judicial districts.

The first stage started on January 1, 2005, the second on January 1, 2006, the third on January 1, 2007, and the final stage on January 1, 2008 (see Figure A1 from the Online Appendix).

Under the old inquisitorial model (Law 600/00), prosecutors served a dual role as investigators and adjudicators in long written and private procedures. Before trial, prosecutors had control over the evidence collection and over the decision to impose pretrial detention. Once in trial, the judge decided the case based on a written dossier prepared by the prosecutor that included all procedural activities and documentary evidence (i.e., testimonies, expert testimonies, searches, seizures, etc.) (Langer, 2007). Under the Colombian inquisitorial model, a trial could last the statute of limitation, which is up to 20 years.[6] This inquisitorial model overloaded the Colombian criminal justice system. By 2004, the Colombian system produced a low number of convictions every year and excessively relied on pretrial detention as the primary form of crime control (Carranza, 2001; Duce et al., 2009; Zorro Medina, 2020).

The adversarial reform (Law 906/04) changed the structure of the criminal process before the sentencing phase. First, it eliminated the dual role of prosecutors, creating a new judge in charge of pretrial detention decisions (Supervisory Judge or *Juez de Control de Garantías*). Second, the reform limited the criminal process length, by imposing a maximum number of days between the imputation of charges (file of charges) and the indictment hearing. The reform also converted the form of proceedings from a written to an oral process, with public oral hearings before a Supervisory and a Trial Judge. Under the new adversarial model, the Trial Judge listens to all testimonies, and the defense and prosecutor cross-examine the witnesses orally in a public hearing. Moreover, the documentary evidence is now introduced by the defense and prosecutor orally during trial, and the Trial Judge examines it directly.

The adversarial reform radically transformed the country's criminal justice administration. By imposing rigid deadlines to prosecutors after the imputation of charges, not only the duration of the criminal process was affected, but also the proportion of criminal cases in which files are charged (e.g., the clearance rate) and the time that prosecutors take to file charges. In addition to these structural changes, the reform introduced two modifications to criminal settlements. First, the reform made (pre-imputation) settlement hearings between the victim and the suspect mandatory to initiate the penal process for minor crimes, such as property crimes and assaults. Before the reform, these settlements were optional. In settlement hearings, the prosecutor mediates between the victim and the alleged

[6] The statute of limitation is the maximum time after a crime within which a criminal case can be initiated. According to the Colombian Penal Code the statute of limitation depends on the maximum prison sentence established by the penal law, but it cannot be shorter than 5 years or exceed 20 years.

criminal to encourage them to settle and avoid indictment. Second, the reform introduced plea bargaining into the Colombian criminal process. Plea deals differ from pre-imputation settlements as they involve a negotiation between the prosecutor and the accused. In contrast, pre-imputation settlements are negotiations between the victim and the person being investigated and these take place before a formal criminal process begins.

Although before the adversarial reform the Colombian law contemplated agreements between the accused and the prosecutor, the reform changed the nature of these agreements considerably. First, prior to the reform, reaching an agreement did not replace the trial phase. Thus, these agreements offered no decongestion benefits for the judicial or prosecutorial systems. Second, the agreements did not involve a negotiation between the accused and the prosecutor because the law stipulated the exhaustive benefits from accepting charges. In contrast, the adversarial reform gave prosecutors enormous discretion to decide whether to offer a deal and its benefits. Moreover, the reform not only introduced plea agreements but also not guilty agreements, where the defendant could obtain an acquittal through a negotiation with the prosecutor. These changes aimed to increase the efficiency while reducing the costs of the operation of the criminal justice system. In exchange for accepting charges, under the adversarial reform, the prosecutor could offer sentencing reductions beyond those previously listed by the penal law.

Finally, the reform also set a clear goal of limiting the use of pre-trial detention except in precautionary and exceptional cases. Prior to the reform, if the crime for which an individual was charged was on a list prespecified in the law, the prosecutor could automatically order the pretrial detention of the accused without prior control by a judge. This discretionary power of the prosecutor meant that, in practice, a high percentage of individuals charged with offences on the list automatically ended up in pre-trial detention, without an objective evaluation of their dangerousness or their possibility of affecting the investigation if left free.

Prior to the 2004 reform in Colombia, a July 2001 Constitutional Court ruling had already partially limited the ability of prosecutors to send individuals to pre-trial detention by establishing basic criteria that had to be met before the prosecutor could make this decision. Nonetheless, these decisions did not have the prior control of a judge, and it was only under the 2004 reform that the prosecutor's role in the penal process changed. Now, the decision to send the defendant to pretrial detention is taken by a judge, upon formal request of the prosecutor in a formal hearing. Moreover, the new adversarial code only permits the use of pretrial detention under a highly specific set of circumstances: (i) to ensure the defendant's appearance in future hearings; (ii) to protect the integrity of the evidence; and (iii) to protect the victims and the community from the possible risk of criminal

recidivism of the defendant. By limiting prosecutorial discretion in the imposition of pretrial detention, the reform aimed to increase the burden of proof for the prosecutor to request the pretrial detention of the defendant, which must now be endorsed and granted by the judge upon a formal request by the prosecutor in a formal and public hearing.

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*) for 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*).[7] These two information systems contain case-level data since 2004 for all Colombian municipalities. From the SPOA and SIJUF, we use municipality-monthly 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 (in court or guilty pleas), and (vii) the number of acquittals (in court or agreements).

Second, the Colombian National Police (NP)[8] provided the municipality monthly arrest and crime data from 2003 to 2008.[9] In a slightly unbalanced panel, we have information for 1,100 out of 1,122 municipalities in Colombia between 2003 to 2008.[10] We restrict our analysis to high-impact social crimes, and from that subset, we select those with lower measurement problems: homicides, assaults, muggings, business robberies, home burglaries, and vehicle thefts (De Mello et al., 2013; Di Tella & Schargrodsky, 2004). These six categories represent 97% of Colombian high-impact social crimes in 2005 (Policía Nacional de Colombia-DIJIN, 2005).

[7] 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.

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

[9] 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.

[10] We restrict the 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).

Additionally, we include data on sexual and drug offenses to evaluate potential changes in crime reporting. As scholars have pointed out, sexual offenses rates rely almost entirely on subjects' reports (Briere, 1992; Scurich, 2020).[11] Thus, sexual offenses would be the crime category most susceptible to changes in self-reporting rates. In contrast, the literature has identified drug crimes as victimless offenses, and their reporting highly depends on policing activity (Black, 1970; Campbell et al., 2022; Wijeratne et al., 2023). Therefore, drug offenses are the crime category least susceptible to changes associated with self-reporting. By comparing the reform's effect on sexual and drug offenses, we evaluate whether self-reporting changes could be driving the reform's impact on crime rates. Although far from ideal, this is the best way to see changes in self-reporting patterns during our study period in Colombia.

Using the crime data, we build four aggregate crime measures: i) unweighted crime rate, ii) weighted crime index, iii) weighted violent crime index, and iv) weighted property crime 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.[12] The weighted crime index includes both violent crimes (homicides and assaults) and property crimes (muggings, business robberies, vehicles thefts and home burglaries). From the Colombian National Police, we also use the data on arrests. Figure A2 from the Online Appendix shows the evolution of the four aggregated crime measures by stage of implementation, normalizing to one the value in the month right before the implementation.[13]

Our third source of data is the yearly population projections published by the Colombian National Administrative Department of Statistics (*Departamento Nacional de Estadística*, DANE), which we use to normalize crime rates per 100,000 inhabitants. Finally, we use yearly data at the municipal level that includes different 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 traditionally identified by the literature as determinants

[11] 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).

[12] Table A2 from the Online Appendix present 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.

[13] 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.

of crime: (i) income per capita (Crutchfield, 1989; Hipp, 2007; Messner & Tardiff, 1986; Verbruggen et al., 2015); (ii) institutional capacity included as the municipality's fiscal performance measure (Chamlin & Cochran, 1995; Messner & Rosenfeld, 1997; Rosenfeld & Messner, 2006); (iii) 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 shows descriptive statistics of the variables of interest from these three data sources.

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 staggered rollout of the implementation 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 dynamic effects of the reform several months after the implementation. Equation (1) represents the unconditional leads-and-lags model that we estimate:

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

where $Y_{i,t}$ represents the outcome variable in municipality *i* and period *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 on 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 incorporating information delays and timespans between the denominator and numerator (CEPEJ, 2014; Marciano et al., 2019). Second, rates controlling 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 are in the Online Appendix.

As for crime, $Y_{i,t}$ represents: (i) unweighted crime rate; (ii) crime rate index; (iii) violent crime rate index; (iv) property crime rate index; (v) homicide rate, (vi) assaults 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 in the outcome variables over the 12-months after the reform was implemented.

Moreover, we include year, month, and year-month fixed effects (μ_t) to control for national trends and possible seasonalities in outcome variables. Likewise, we include municipality fixed-effects (δ_i) to control for non-observable and time-invariant municipality characteristics.[14] Lastly, the term $\varepsilon_{i,t}$ represents the error term, which we cluster at the municipality level. Although the treatment is assigned across judicial districts, their small number does not allow for clustering at this level (Cameron & Miller, 2015).

This specification allows us to study pre-trends between treatment and control groups. If the parameters γ_m are statistically 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 reform. As we show in our results, 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 for pre-trends among different observable characteristics, including fiscal performance, population density, forced displacement, rurality index, tax revenues, and education investment. Figure A4 in the Online Appendix shows that the four waves of the reform were balanced in these potentially confounding variables, thus supporting the use of not-yet-treated municipalities as controls for the treated ones.[15]

[14] Even though the inclusion of municipality-time fixed effects or judicial districts-time 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 pre-trends test suggests that the parallel trends and exogeneity assumptions hold without conditioning by these fixed effects, we argue our estimators are unbiased.

[15] 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

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

$$\ln(Y_{i,t}+1) = \beta_1 T_{i,t} + \beta_2 X_{i,t} + \beta_3 Z_{i,t-1} + \delta_i + \mu_t + \varepsilon_{i,t} \quad (2)$$

where $Y_{i,t}$ represents the same set of outcome variables in municipality *i* and period *t*; $T_{i,t}$ is a binary variable indicating whether the reform had been implemented in municipality *i* in period *t*. Vector $X_{i,t}$ contains economic, demographic, and institutional control variables.[16] Since outcome variables are measured in natural logarithmic terms, the coefficients should be interpreted as the average percentage change in the outcome variable after the reform.

In general, our results should not be read as absolute changes in the outcome variables but instead as relative changes in outcome variables 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 is coming from changes in the outcome variables in treated municipalities relative to not-yet-treated municipalities that are comparable in terms of the included covariates. This means that in our TWFE specification, we are not comparing remote rural areas with a substantial presence of armed group with large urban areas. Instead, we are comparing areas 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 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 from the Online Appendix, we include 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.

confirm that the parallel trends assumption is reasonable even after the inclusion of the vector of observable variables. Table A3 presents these results.

[16] We use a logarithmic transformation of all dependent ($Y_{i,t}$) and cofounding variables ($X_{i,t}; Z_{i,t-1}$) to allow for non-linear relationships. To avoid losing information due to zeros in each municipality-crime bin, we use the log + 1 transformation.

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), who weigh these comparisons using both the variance and the centrality of the treatment. Figures A6 to A8 from the Online Appendix show the results of these event study estimations. In this specification, we cluster standard errors at the municipality level using bootstrap with 1,000 iterations. We also estimate our TWFE specification using the methodology proposed by Wooldridge (2021). According to Wooldridge (2021), 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.

VI. Results

VI.I. Intended Consequences

In this section we present our results examining the effects of the procedural reform on several indicators measuring the goals of the reform, such as, procedural times, the use of pretrial detention, settlements, acquittals, and convictions. The results suggest that the reform achieved most, if not all, of its 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 the outcomes of interest.

i. Procedural times

In Figure 2, 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 2 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 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 indictment before

the reform was around 400 days, and after the implementation of 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 the reform. 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, going from around 600 days to less than 300 days a year after the implementation of the reform.

Figure 3 presents the results from the lead-and-lags estimation (Equation 1) using procedural times as the dependent variable. These figures support 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 3 confirms the results presented in Figure 2: 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 3 shows that this reduction increased over time, at least during the first 12 months after the implementation of the adversarial reform.[17]

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 230 days. Yet, after the implementation of the reform, the time between the opening of the investigation and the imputation of charges increased by 80 days on average. In contrast, after the reform, the total number of days between opening of the investigation and indictment decreased by 115 days. Panel B confirms the dynamic evolution of these time differences presented in Figure 3.

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 pretrial detention in jail after the implementation of the reform. We define our outcome variable as the number of cases with active preventive measures (pretrial detention in jail, house arrest, and their sum) divided by the total number of imputations for each type of crime. Figures 4

[17] Due to the nature of our data, we can only evaluate the impact of the reform with high statistical confidence for the 12 months following its implementation.

to 6 present the results from the leads-and-lags model (equation 1) for pretrial detention in jail, 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 pretrial detention in jail and an increase in the use of domiciliary arrests for homicides and drug crimes. Nonetheless, as can be seen in these figures, 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 (equation 2) results and confirm patterns from Figures 4 to 6. For homicide cases, the estimated reduction in the use of pretrial detention in jail is 34.4% after the implementation of the reform; for property crimes, the estimated reduction is 17.2% and is marginally significant; and for drug offenses and assaults is 32% and 32.8%, respectively. For sexual crimes, the estimated reduction is not statistically significant.[18] When we estimate the TWFE model for house arrests, the estimates are positive and smaller but highly significant, and the estimated magnitudes are extremely large for all crime categories. The reason for this is that 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 262% for drug offenses and 880% for homicide cases). When we add the two forms of preventive measures, we find reductions for most crime categories, but they are statistically significant at standard confidence levels for homicides (28%) and for drug offenses (13%).

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 hearing 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 timespans.

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

Figure 7 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 42.6% for property crime cases and 66.1% for assault cases (columns 1 and 2 in Table 3). Columns 3 and 4 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 8A and 8B present the results of the leads-and-lags model for total acquittals and adjudication 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 8A) and convictions (Figure 8B) 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 422%, and for convictions 870%. 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 measure the number of acquittals or convictions in trial divided by total trial sentences. The results of the estimation of the leads-and-lags model using the share of acquittals and convictions in trial over total trial sentences are presented in Figures 9A and 9B, respectively. 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, however, the results look 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) for homicide cases and a marginally significant increase of the share of acquittals in trial for property crimes and drug-related 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 differently 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 crimes. Given that some of the changes wrought 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 remains 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 protection by imposing stricter conditions (a higher bar) for legal procedures such the apprehension of an alleged criminal 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 directly affect the conditions for a lawful police arrest, it did raise the bar for the prosecution of the criminal case and the imputation of charges.

In Figure 10 we present the results of the estimation of the leads-and-lags model using as the dependent variable the arrest rate (the number of police arrest 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 10 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 estimation of the TWFE model are presented in Table 6. Our estimations show that the implementation of the adversarial reform led to a 32% 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 imputation of charges over the total number of reported crimes, for each crime category) in two ways. Firstly, 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 2).[19]

[19] In the imputation of charges hearing an alleged criminal must be fully identified, and the criminal process officially starts.

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 11 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.[20] 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 23.7%, of about 23.6% for property crimes, 15.7% for drug offenses, and 24.6% for sexual offences.

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 cases where pre-imputation settlements are not allowed, it is reasonable to assume that the effects observed in Figure 11 and Table 7 are capturing the effect of introducing procedural time limits. Thus, these reductions in clearance rate complement those results presented in the lower left panel of Figure 2 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

We finish by exploring the net 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,

[20] As can be seen in the last graph in Figure 11, there are some clear pre-trends differences in the clearance rate for assaults between treated and not-yet-treated municipalities before the implementation of the adversarial reform, so we need to be cautious when interpreting these results as causal.

increasing the burden of proof to impose pretrial detention might decrease the incapacitation effect of this measure, causing an increase in crime rates in the short run (Chalfin & McCrary, 2017).

Moreover, the strengthening of early termination mechanisms, such as plea deals, can change the expected discounted cost of committing a crime in different directions. While an expected lesser sentence obtained from a plea deal may reduce the cost of committing a crime, the reduction in the time to reach a conviction may increase it. Thus, the question of what effect dominates becomes and 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 the leads-and-lags model (equation 1) and the TWFE model (equation 2) using crime rates as the dependent variables. Figure 12 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. After the implementation of the adversarial reform, there is a notable increase in the four indices.

We corroborate these results by estimating the TWFE model using the four crime indices as the dependent variable (Table 8). The estimated increase for the unweighted crime index is 22%, for the weighted aggregate crime index the estimated increase is 12%, for the violent crime index is 15% and for the property crime index is 8%. All the estimated effects are highly significant. When we add exposure time (columns 5 – 8), the results indicate that the negative effects of the implementation of the reform on crime rates increased over the first 12 months after implementation of the reform. 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 37%, for the weighted crime index 18%, for the violent crime index 17% and for the property crime index 19%.

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 estimations for separate crime categories are presented in Figures 13A and 13B (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 discernable differences in pre-trends prior to the reform.[21] After the implementation of the reform, our results from Figure 13A show a clear increase in the assaults rate. For homicides and sexual offenses, there is no clear discernable 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 assaults rate of 25% and a marginally significant increase of 2% in the homicide rate. For sexual crimes, we do not find a significant effect after the implementation of the reform.[22] When we include exposure time to the reform in the estimations (columns 5 – 8), the estimated increase in the assaults rate after 12 months of implementation is 32%, and for homicides 4%, and the latter effect is now significant at standard confidence levels.

Finally, we evaluate the effects on the separate crime categories that compose the property crime index: muggings, business robberies, vehicles thefts and home burglaries. For all these property crimes, the assumption of parallel trends before the implementation of the reform seems to be fulfilled (Figure 13B). After the reform, the estimation of the leads-and-lags model shows an increase in all crimes on property. These results are corroborated when we estimate the TWFE model. The average estimated increase in muggings after the reform came into force is 9%, while the increase is 4% for business robberies, 2% for vehicle thefts, and 6% for home burglaries (Table 9B). When we include exposure time (columns 5 – 8), the estimated increase after 12 months of implementation is 23% for muggings, 10% for business robberies, 1% for vehicle thefts and 11% 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-difference model (Wooldridge, 2021) to account for some of the issues in the staggered TWFE specification. Table A12 in the Online Appendix includes these results. We estimate two falsification tests to assess our results' robustness further. First, we evaluate our model during pre-

[21] For drug-related crimes, however, the bottom right graph in Figure 13A suggests that there are significant differences in pre-trends between treated and not-yet-treated municipalities before the implementation of the reform.

[22] 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 the period studied. However, we recognize the limitations of our data to conclude that crime reporting did not change after the reform.

treatment using the reform's implementation order. In this pre-treatment test, we assume the reform started in each stage every six months, starting in January 2003 for the first group and ending in July 2004 for the fourth group. Table A4 from the Online Appendix shows no relationship between the reform's implementation and crime rates. 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). Additionally, we consider that the reform might have changed the covariates included in our analysis. To address this, we incorporate the lag of police arrests ($Z_{i,t-1}$) to avoid omitted variable biases associated with this variable (Listokin, 2003; Pfaff, 2008; Rosenfeld & Wallman, 2019). Nonetheless, the exclusion of $Z_{i,t-1}$ does not affect the results significantly. We present these alternative results in Table A4 in the Online Appendix. Moreover, we estimate equation (2) excluding vector $X_{i,t}$, obtaining similar results (see Tables A9 to A11 in the Online Appendix).

Overall, our estimations with the two methodological approaches show a significant increase in crime after the implementation of the adversarial reform in Colombia that ranges between 1% and 32%, 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 literature on crime control has focused on exploring the role of the penal law on crime rates. However, relatively few studies to date have studied the effect of changes in criminal procedure (that is, the rules by which cases are handled and processed by the criminal justice system) on crime rates. In this paper, we aimed to expand our knowledge about how a complex and extensive criminal procedural reform can transform the administration of criminal justice and shape criminal behavior. To do this, we exploit the arguably exogenous staggered implementation of an extensive procedural reform implemented in Colombia between 2005 and 2008: the adversarial reform.

While the previous inquisitorial model was characterized by long written processes where due process protection was not fully guaranteed and there was no clear distinction between the investigative and adjudication role of (inquisitorial) judges, the new adversarial model modified several aspects of the

way criminal cases are handled to protect the accused's rights, reduced congestion and improve the efficiency of the system. To meet its objectives, the reform set specific goals such as reducing procedural times, drastically reducing the use of pretrial detention, and strengthening the mechanisms for early termination of criminal proceedings to reduce congestion in the criminal justice system and increase its efficiency.

Using highly detailed data on judicial decisions and judicial outcomes from the information systems of the previous inquisitorial model and the new adversarial model, we assess whether the reform met its proposed objectives (i.e., the intended consequences). In short, the results from our estimations show that the reform largely achieved its intended effects. First, the reform significantly reduced procedural times. Our results indicate that the time elapsed between the opening of the investigation and indictment was significantly reduced by more than 50%, from an average level of 600 days before the reform to less than 300 days a year after the implementation of the reform.

Additionally, the implementation of the adversarial reform significantly reduced the use of pretrial detention in jail. For homicide cases, our estimations show a reduction in the use of pretrial detention of 34%, for property crimes of 17%, for drug-related crimes of 32% and for assaults of 32.8%. Our results also indicate an increase in the use of domiciliary detention (house arrest), but overall, this increase was not enough to compensate for the decline in pretrial detention in jail.

The implementation of the reform also met the goal of streamlining pre-imputation agreements for minor offenses. Our estimations imply that settlements increased 42.6% for property crime cases and 66.2% for assaults. Finally, the reform also led to a large and significant increase in adjudication rates (both acquittals and convictions). Our results also indicate that while the share of acquittals in trial increased significantly, that of convictions decreased with the implementation of the reform. These results could suggest that the introduction of plea bargains made it possible to increase convictions before trial and make an adjudication decision much faster, presumably in those cases where the prosecution has sufficient evidence of the defendant's guilt, so that the latter will have an incentive to seek a plea agreement with the prosecutor in exchange for a lesser sentence. In contrast, for those criminal 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 a plea deal with the prosecution and, as a result, acquittals in trial would increase as a percentage of total judgments at trial.

Overall, all the intended consequences of the reform were largely met. However, the reform also had some unintended consequences. Our findings show that arrest rates and clearance rates went down because of the implementation of the adversarial reform. More precisely, we estimate a significant

reduction in arrest rates of about 32%-35%. What is even more worrisome is the reduction in clearance rates. Our estimations show that clearance rates for all crime categories went down after the implementation of the reform: 23.7% for homicide cases, 23.6% for property crimes, 15.7% for drug-related crimes, 24.6% for sexual crimes, and 26.8% for assaults.

Regarding the effect of the reform on clearance rates, the reform had intended and unintended consequences. The reform achieved its goal of increasing pre-imputation settlements for minor crimes, which inevitably leads to a decrease in the number of imputed cases for those offenses (lower clearance rate). At the same time, the introduction of strict time limits between the imputation and indictment potentially incentivized prosecutors to prioritize easier cases to prosecute over hard cases, leading to an unintended reduction of the clearance rate.

Finally, we find highly robust evidence that the implementation of the adversarial reform in Colombia led to increases in aggregate crime by about 18%, violent crime rates by about 17% and property crime rates by 19%. When we estimate the effect of the reform on separate crime categories (homicides, assaults, burglaries, etc.), we find that the implementation of the reform led to an increase in most separate crime categories. We also find important dynamics effects: our results suggest that the negative effects of the implementation of the reform on crime rates increased over the first 12 subsequent months.

This paper addresses a recurrent question in criminal justice reform processes: how to balance constitutional protections with public safety. Increases in constitutional protections often restrict the state's ability to punish arbitrarily, creating obligations and procedures that public agents must follow to guarantee that no one will be sanctioned unjustly. However, sometimes, well-intended reforms that look to increase due process protection can create unwanted changes in the administration of justice that lead to increases in criminal activity. While our results do not support or justify deferring to public safety sacrificing constitutional rights and civil liberties, our paper intends to highlight that reforms to protect constitutional rights must include provisions and guidelines directed to mitigate potential adverse effects on crime rates. These provisions are especially important in societies with high historical crime levels and low state efficacy to prosecute and convict criminals. As the Colombian case shows, criminal justice systems that deal with high caseload levels at the policing, prosecutorial and judicial levels need to increase the resources available to law enforcement agents to prevent them from updating their priorities and sacrificing public safety goals when legal reforms that increase the protection of constitutional rights are enacted.

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Figure 1. Criminal procedure reform in Latin America by year of implementation

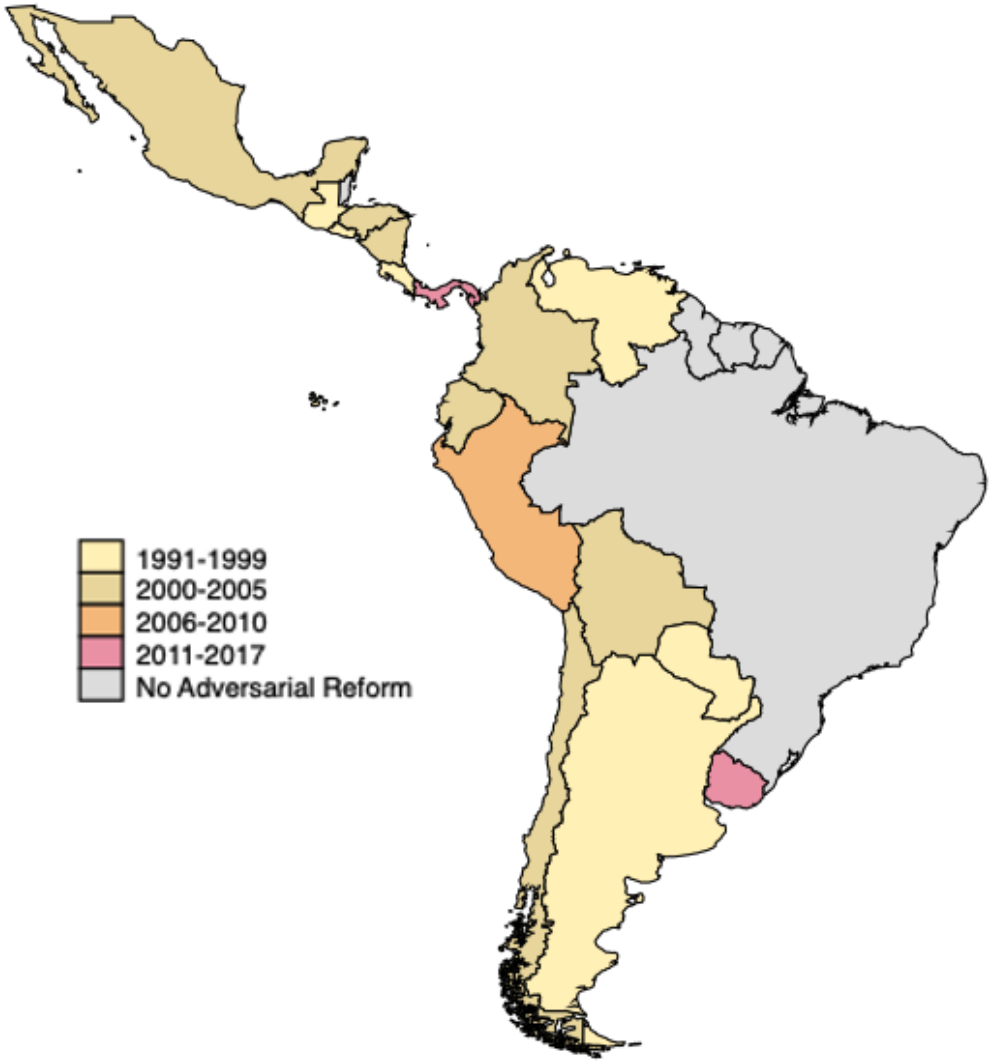
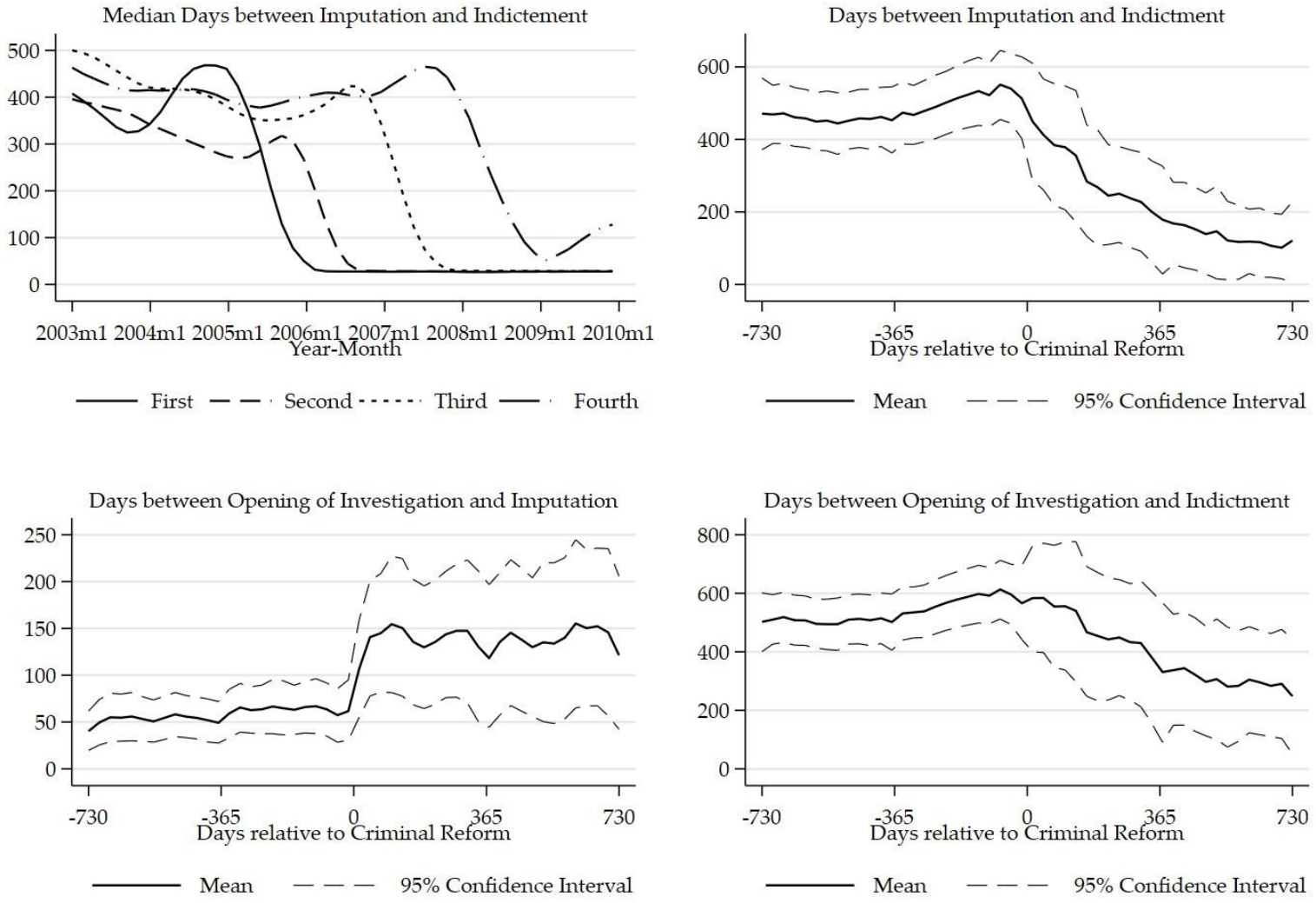
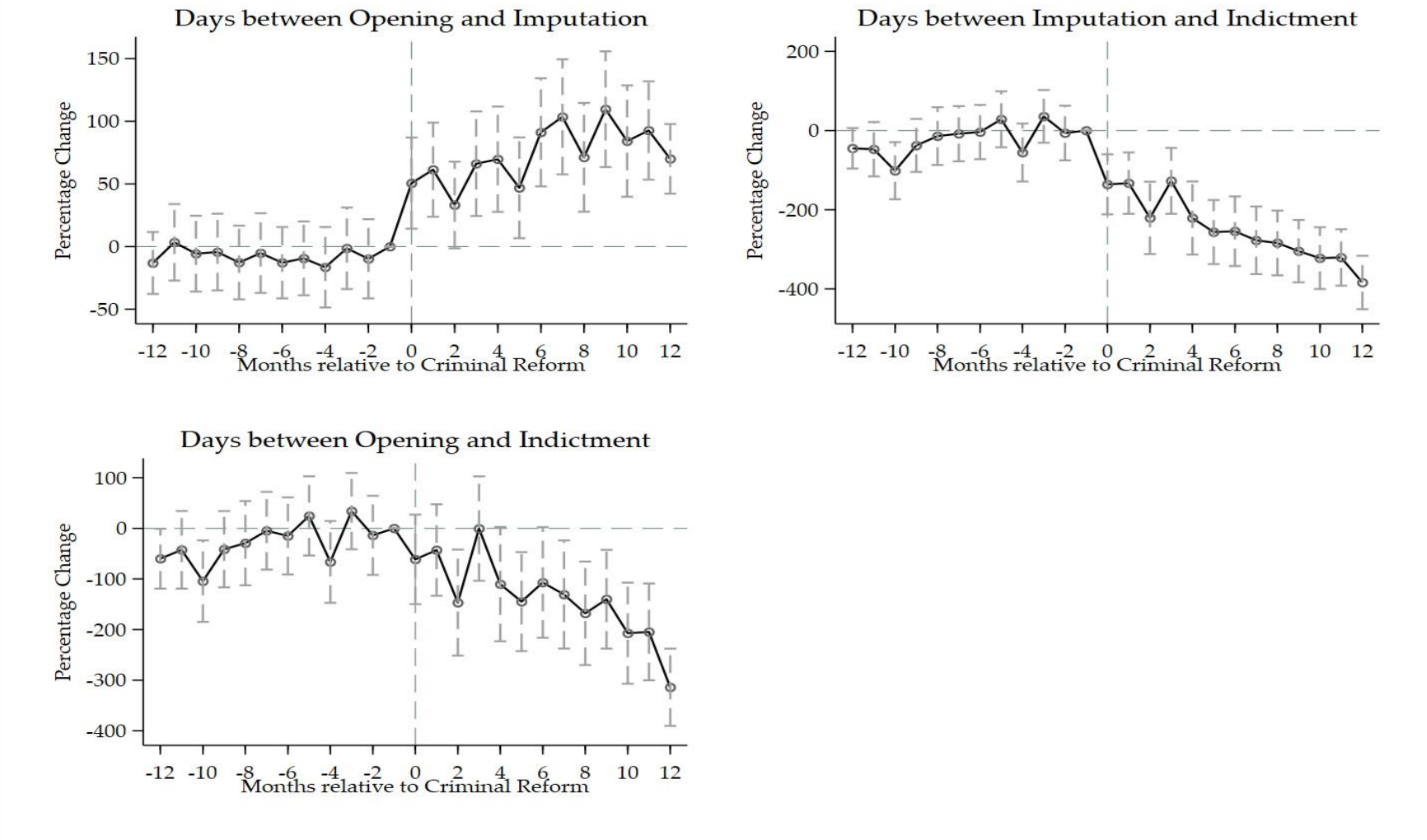


Figure 2. Procedural Length Reduction in Colombia after the Reform



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 3. Leads-and-lags model: Procedural Times



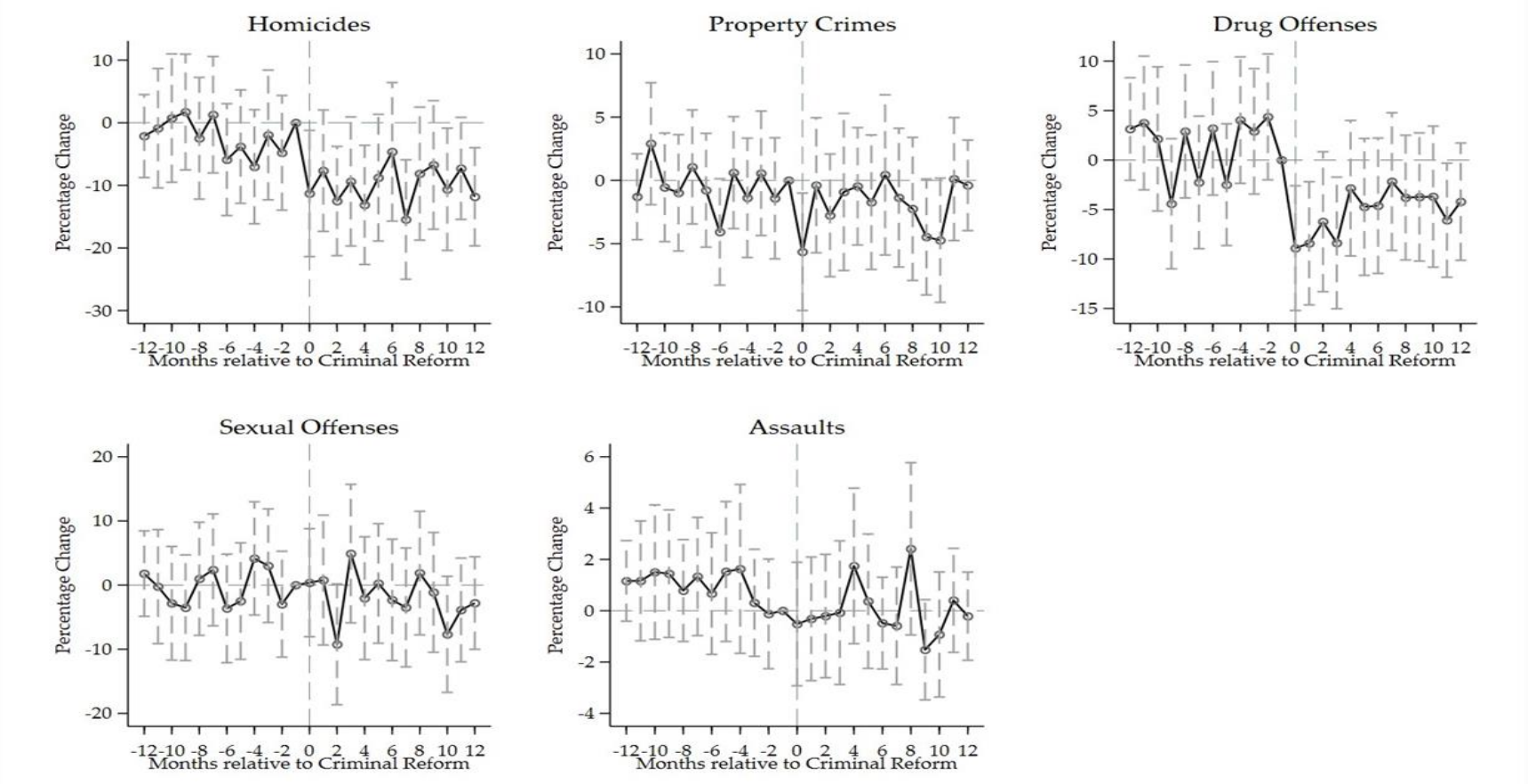
Note: this figure shows the results of an event study of the number of days between different stages of the criminal process as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered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

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

VARIABLES | Days between:	Panel A (1) Imputation - Indictment	(2) Opening - Imputation	(3) Opening - Indictment	Panel B (4) Imputation - Indictment	(5) Opening - Imputation	(6) Opening - Indictment
T	-230.624***	80.252***	-115.522***	-198.770***	84.594***	-68.082***
	(15.637)	(6.537)	(17.841)	(15.323)	(7.109)	(18.040)
Exposure Time to T				-8.106***	-1.098**	-11.256***
				(1.051)	(0.449)	(1.100)
Observations	17,842	17,750	17,526	17,842	17,750	17,526
R-squared	0.385	0.259	0.330	0.390	0.260	0.338
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
Mean T=0	556.67	66.81	622.6	556.67	66.81	622.6

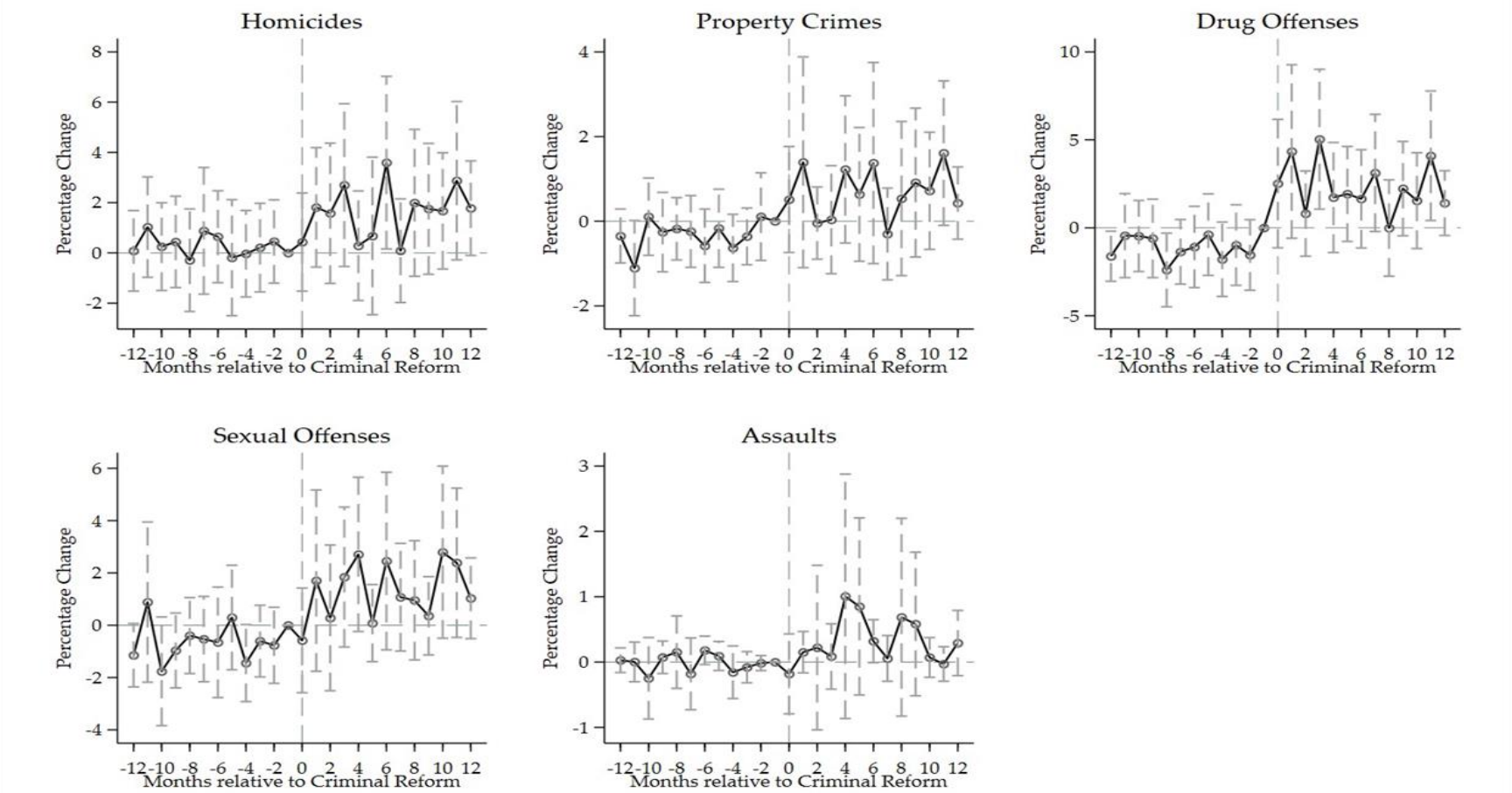
Note: this table shows the results of a two-way fixed effects regression of the days between different stages of the criminal procedure on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in each municipality and month (panel A) and the non-negative difference 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. Municipality-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. *** p<0.01, ** p<0.05, * p<0.1.

Figure 4. Leads-and-lags model: Pretrial detention in jail



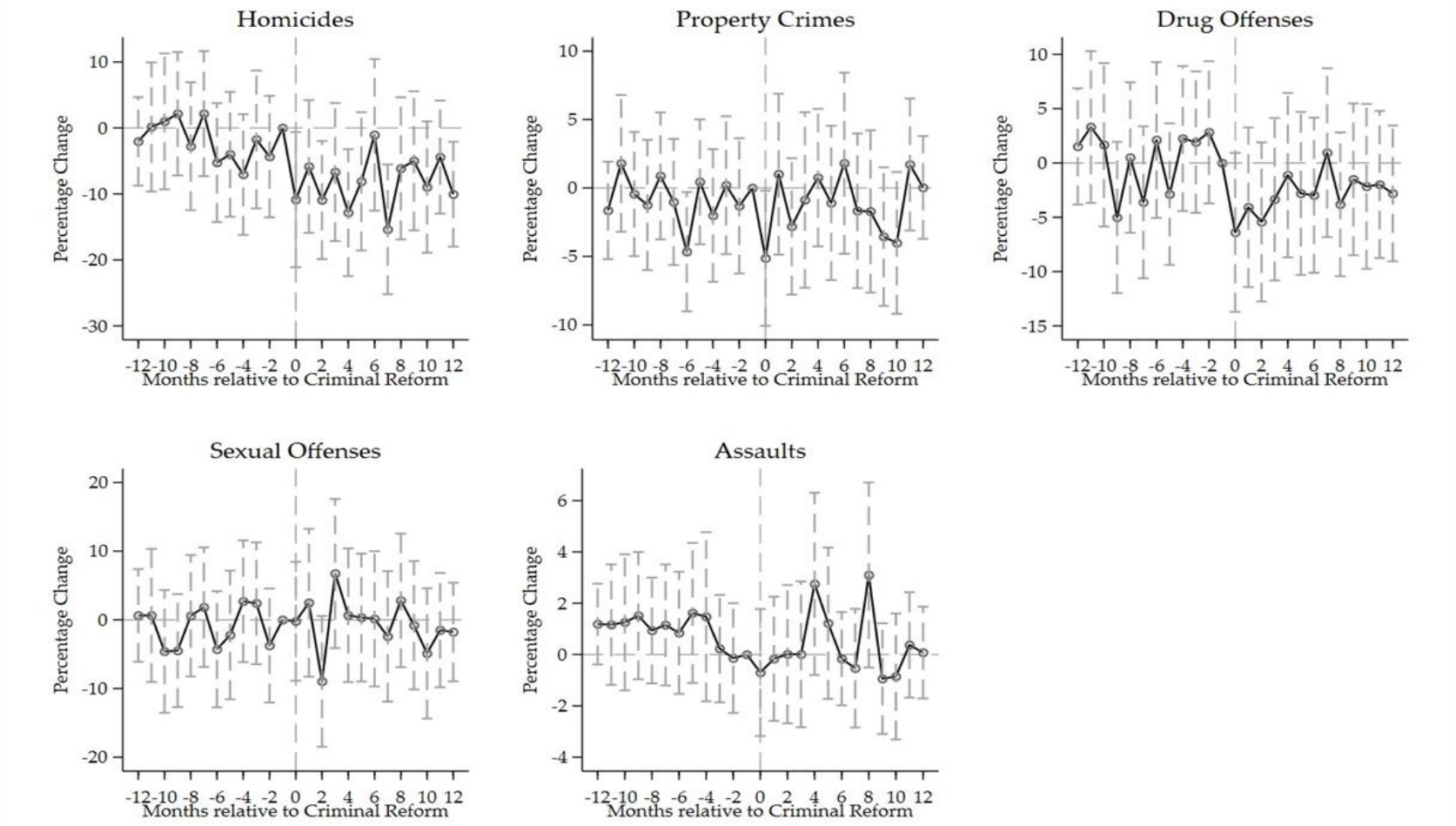
Note: this figure shows the results of an event study of the logarithm of the rates of pre-trial detentions in jail for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-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 house arrest



Note: this figure shows the results of an event study of the logarithm of the rates of pre-trial 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 municipality-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 6. Leads-and-lags model: Pretrial detention total



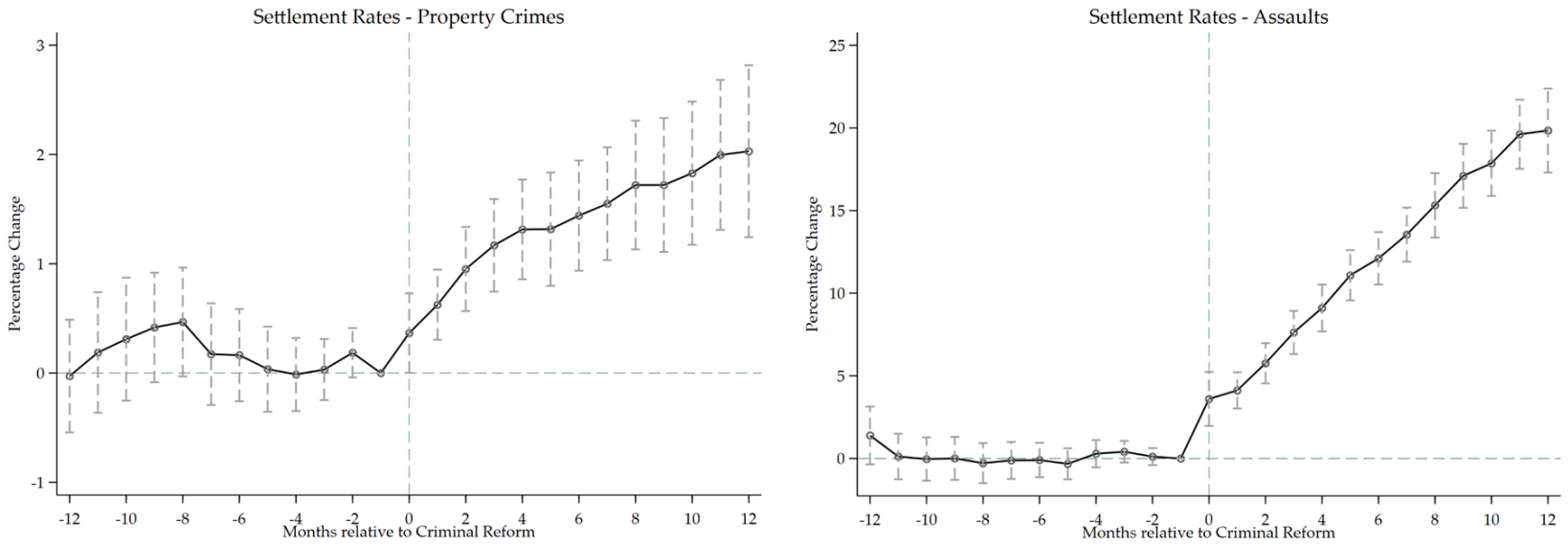
Note: this figure shows the results of an event study of the logarithm of the rates of total pre-trial detentions for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-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: Pretrial Detention in Jail					
T	-7.604***	-1.389*	-6.160***	-1.626	-0.996**
	(1.599)	(0.789)	(1.069)	(1.287)	(0.466)
R-squared	0.127	0.106	0.149	0.111	0.109
Mean T=0	22.13	8.100	19.22	17.36	3.034
% Change after T	-34.36%	-17.15%	-32.05%	0%	-32.83%
Panel B: Pretrial Detention in House Arrests					
T	1.375***	0.938***	3.507***	1.868***	0.327**
	(0.359)	(0.266)	(0.481)	(0.408)	(0.157)
R-squared	0.139	0.098	0.099	0.118	0.065
Mean T=0	0.156	0.187	1.335	0.671	0.0705
% Change after T	881.41%	501.06%	262.70%	278.39%	463.83%
Panel C: Total Pretrial Detention					
T	-6.229***	-0.451	-2.653**	0.242	-0.669
	(1.644)	(0.815)	(1.191)	(1.345)	(0.494)
R-squared	0.127	0.107	0.140	0.111	0.106
Mean T=0	22.28	8.287	20.55	18.03	3.105
% Change after T	-27.96%	0%	-12.91%	0%	0%
Observations	11,200	14,857	14,004	12,098	17,554
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

Note: this table shows the results of a two-way fixed effects regression of the rates of pre-trial detentions in jail (panel A), in 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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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. 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. 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 7. Leads-and-lags model for Settlements rate



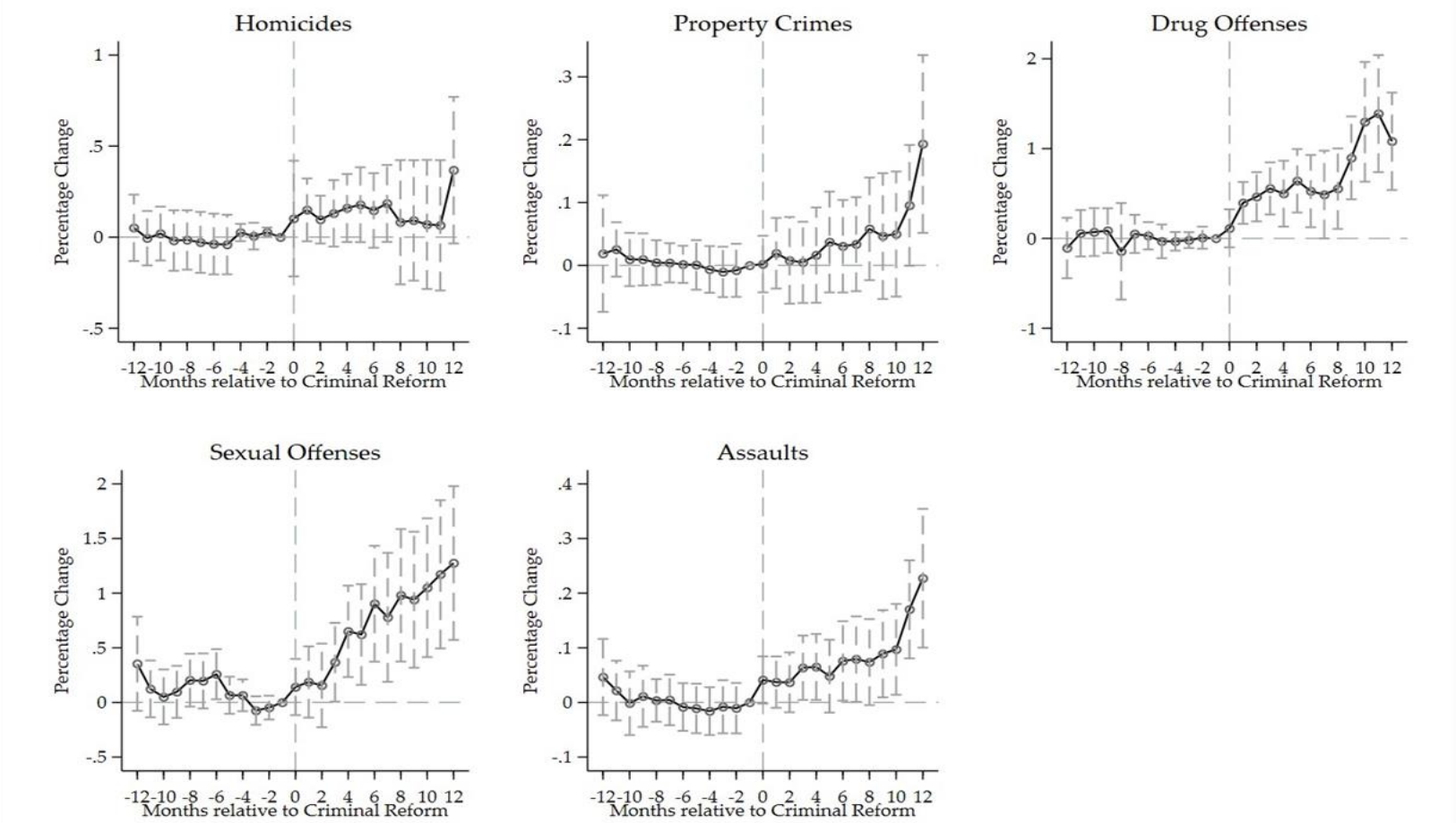
Note: this figure shows the results of an event study of settlements rates for the two minor crimes in our data for which a settlement hearing before the imputation of charges hearing is mandatory under the new system (property crimes and assault) as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-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) Property Crimes	(2) Assaults	(3) Property Crimes	(4) Assaults
T	1.096***	10.445***	1.102***	10.555***
	(0.216)	(0.702)	(0.217)	(0.702)
Exposure Time to T			0.026*	0.516***
			(0.016)	(0.066)
Observations	32,064	32,035	32,064	32,035
R-squared	0.403	0.591	0.403	0.605
Year Month & Month-Year FE	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Mean T=0	2.572	15.78	2.572	15.78
% Change after T	42.61%	66.19%	43.86%	70.16%

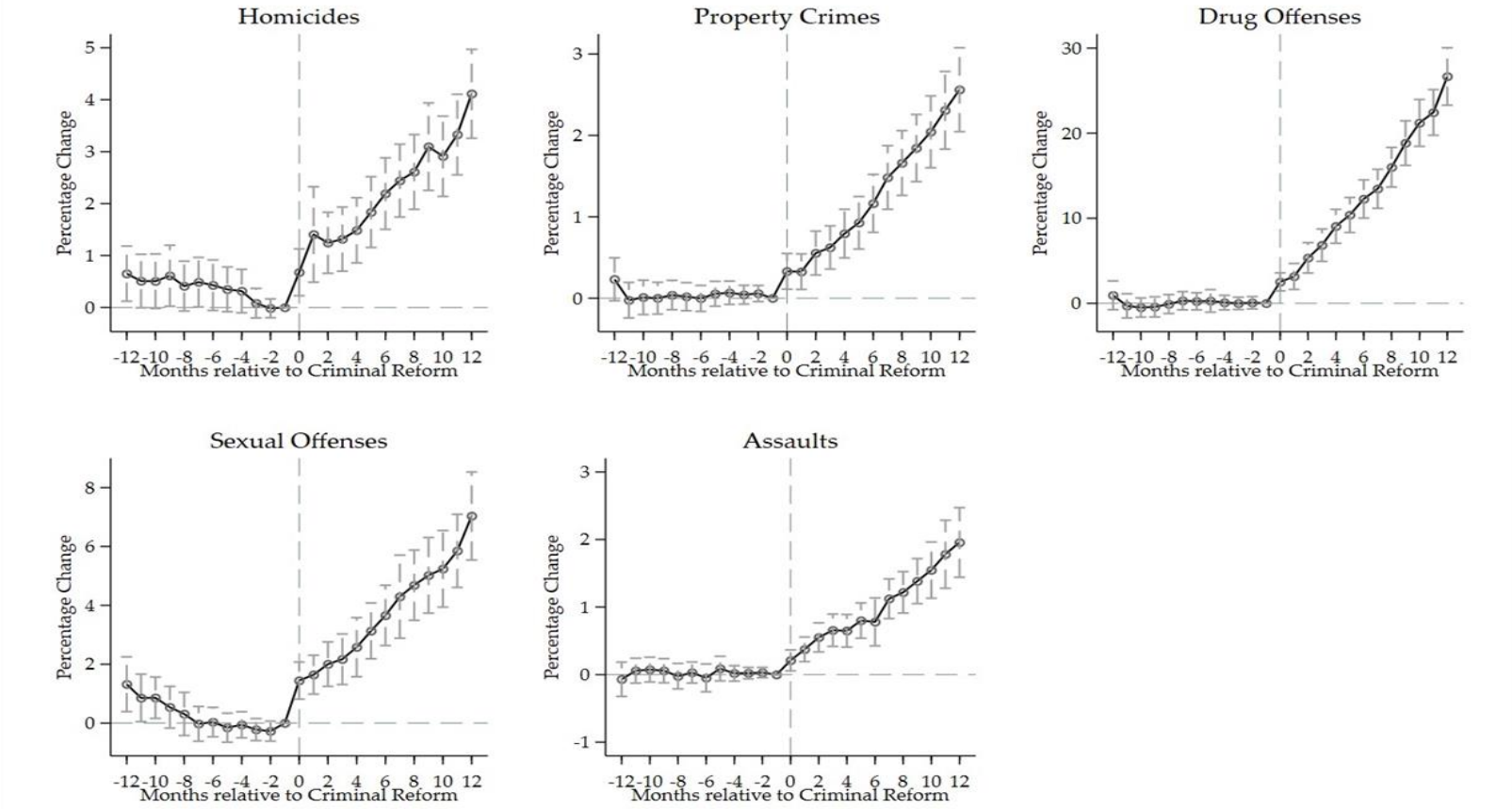
Note: this table shows the results of a two-way fixed effects regression of settlement rates for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (panel A) and the non-negative difference between a given 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. Municipality-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. 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., $Settlement\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} Settlements^{s}_{i,t}}{\sum_{t=-1}^{11} Open\ Cases^{s}_{i,t}}$. 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. *** p<0.01, ** p<0.05, * p<0.1.

Figure 8A. Leads-and-lags model for acquittal rates



Note: this figure shows the results of an event study of the logarithm 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 municipality-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 8B. Leads-and-lags model for conviction rates



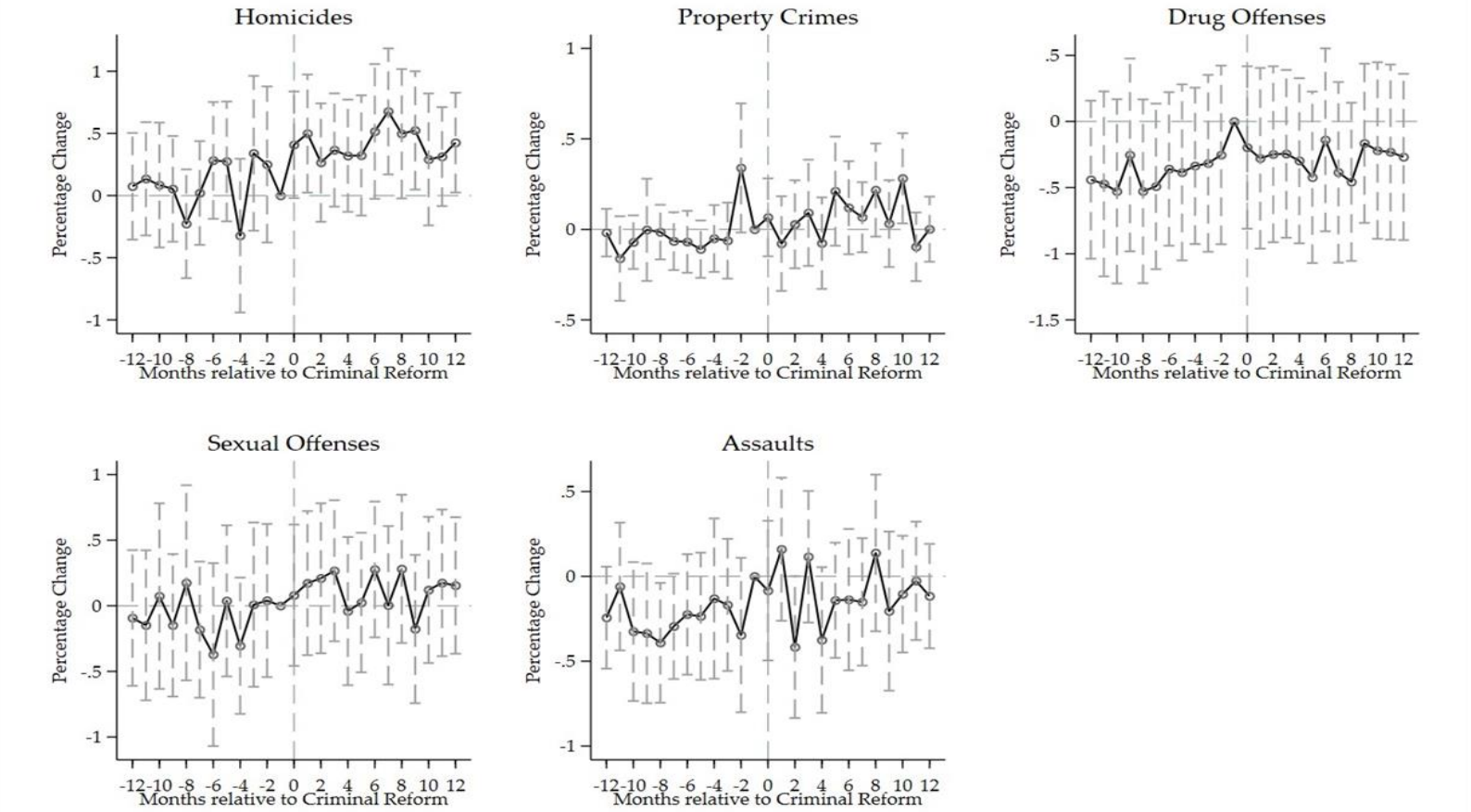
Note: this figure shows the results of an event study of the logarithm of convictions rates for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered 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., $Convictions\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} Convictions^{s}_{i,t}}{\sum_{t=-1}^{11} Open\ Cases^{s}_{i,t}}$.

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	0.131**	0.010	0.580***	0.513***	0.055**
	(0.062)	(0.030)	(0.162)	(0.186)	(0.025)
R-square	0.362	0.371	0.466	0.398	0.222
% Change after T	422.58%	0%	1611.11%	1973.08%	275%
Panel B					
T	0.135**	0.012	0.595***	0.533***	0.056**
	(0.062)	(0.030)	(0.160)	(0.184)	(0.025)
Exposure Time to T	0.017***	0.007**	0.021**	0.058***	0.008***
	(0.005)	(0.003)	(0.008)	(0.014)	(0.002)
R-square	0.364	0.373	0.467	0.402	0.224
% Change after T	490.32%	0%	1711.11%	2273.08%	320%
Mean T==0	0.031	0.009	0.036	0.026	0.020
Total Convictions					
Panel A					
T	1.418***	1.011***	9.923***	2.978***	0.775***
	(0.208)	(0.151)	(0.855)	(0.435)	(0.119)
R-square	0.397	0.424	0.542	0.388	0.320
% Change after T	869.94%	1087.10%	2313.05%	710.74%	711.01%
Panel B					
T	1.445***	1.029***	10.457***	3.063***	0.782***
	(0.207)	(0.154)	(0.873)	(0.436)	(0.120)
Exposure Time to T	0.127***	0.081***	0.743***	0.242***	0.033***
	(0.017)	(0.010)	(0.004)	(0.031)	(0.009)
R-square	0.472	0.391	0.525	0.438	0.471
% Change after T	964.42%	1193.55%	2610.72%	788.78%	173.04%
Mean T==0	0.163	0.093	0.429	0.419	0.109
Observations	31,760	32,064	26,267	31,269	32,035
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

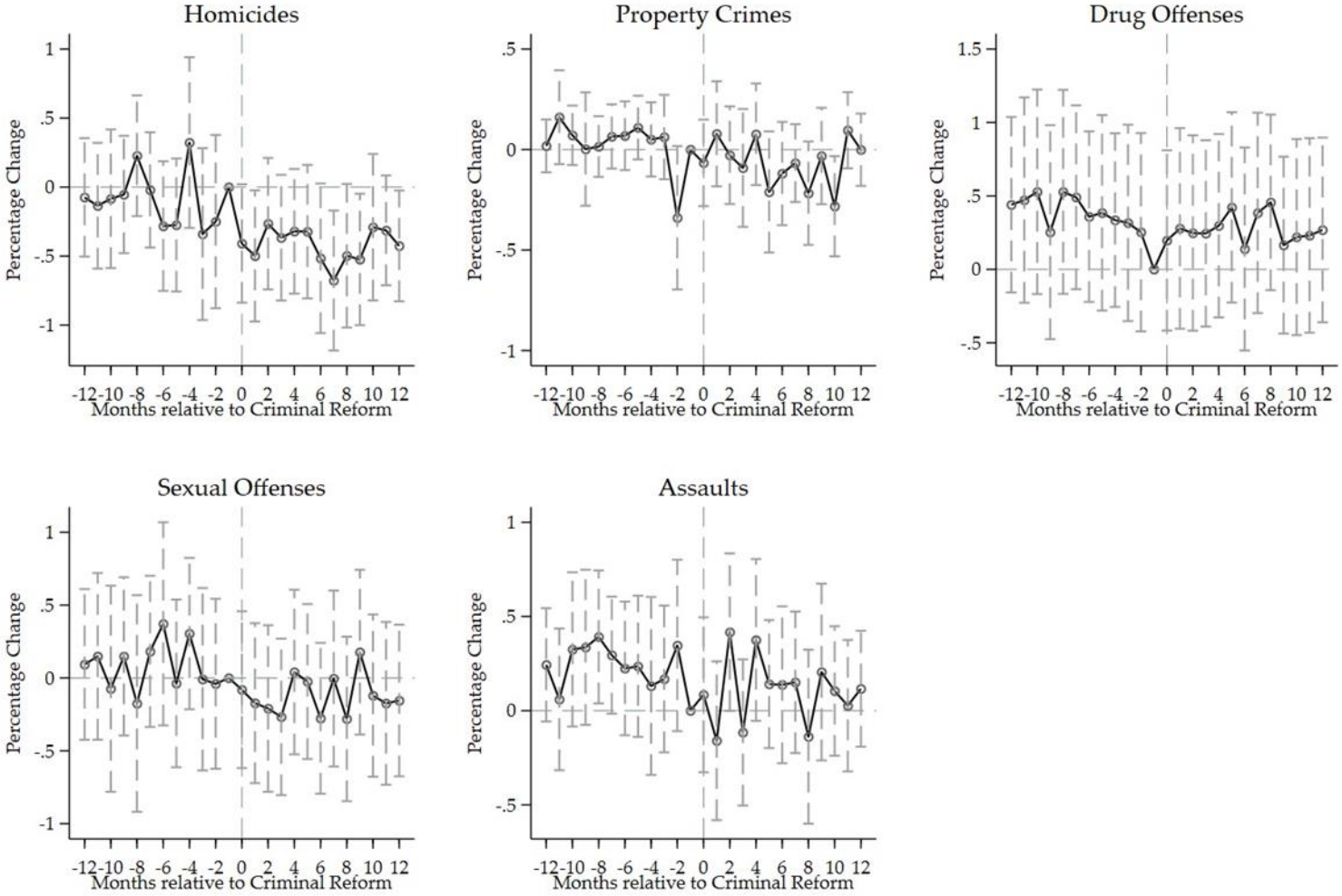
Note: this table shows the results of a two-way fixed effects regression of acquittals (top panel) or conviction rates (bottom panel) for different crimes on an indicator variable that equals 1 if the adversarial procedural reform has been implemented in a given municipality and month (panel A) and the 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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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 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., $Acquittals\ or\ Convictions\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} Acquittals\ or\ Convictions^{s}_{i,t}}{\sum_{t=-1}^{11} Open\ Criminal\ Complaints^{s}_{i,t}}$. 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. *** p<0.01, ** p<0.05, * p<0.1

Figure 9A. 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 logarithm 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 municipality-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., $Acquittals\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} Acquittals^{s}_{i,t}}{\sum_{t=-1}^{11} Open\ Cases^{s}_{i,t}}$.

Figure 9B. Leads-and-lags model for the share of convictions in trial over total sentences in trial



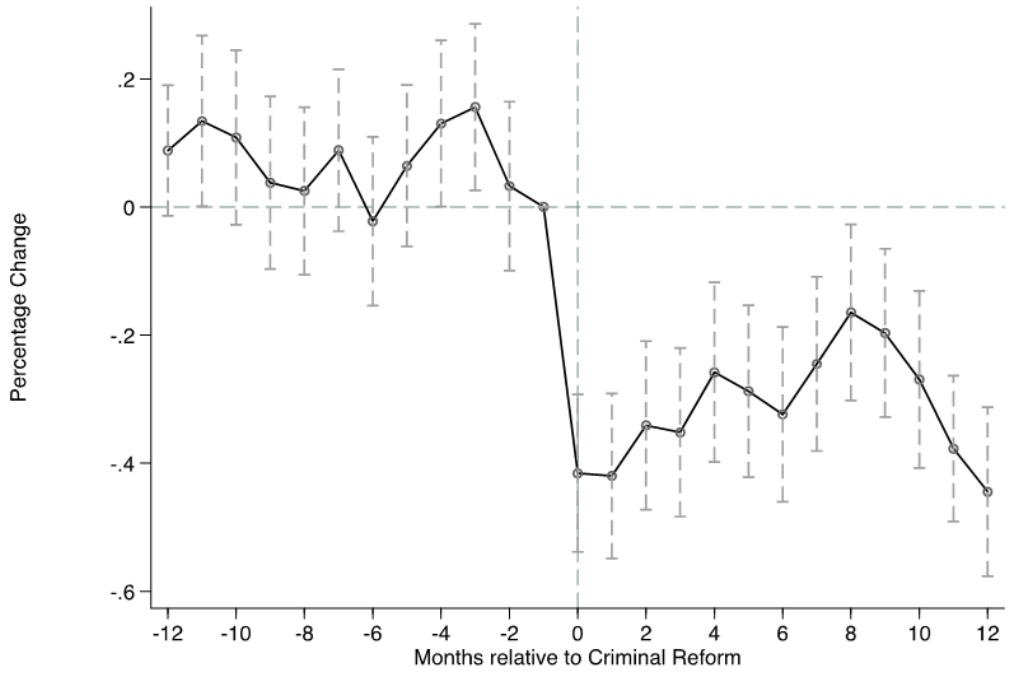
Note: this figure shows the results of an event study of the logarithm of the in-trial convictions for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered 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., $Convictions\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} Convictions^{s}_{i,t}}{\sum_{t=-1}^{11} Open\ Cases^{s}_{i,t}}$.

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	0.287***	0.110*	0.139*	0.166	0.125
	(0.081)	(0.058)	(0.081)	(0.112)	(0.081)
R-squared	0.472	0.391	0.525	0.438	0.471
% Change after T	220.77%	118.28%	187.84%	0%	0%
Panel B					
T	0.271***	0.088*	0.166*	0.147	0.139
	(0.081)	(0.045)	(0.084)	(0.0.114)	(0.089)
Exposure Time to T	-0.003	-0.004	0.005	-0.003	0.004
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
R-squared	0.472	0.393	0.526	0.438	0.471
% Change after T	106.92%	94.62%	224.32%	0%	0%
Mean T=0	0.130	0.093	0.074	0.101	0.138
Convictions in Court					
Panel A					
T	-0.287***	-0.110*	-0.139*	-0.166	-0.125
	(0.081)	(0.058)	(0.081)	(0.112)	(0.081)
R-squared	0.472	0.391	0.525	0.438	0.471
% Change after T	-32.99%	-12.13%	-15.01%	0%	0%
Panel B					
T	-0.271***	-0.088*	-0.166*	-0.147	-0.139
	(0.081)	(0.045)	(0.084)	(0.114)	(0.089)
Exposure Time to T	0.003	0.004	-0.005	0.003	-0.004
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
R-squared	0.472	0.393	0.526	0.438	0.471
% Change after T	-31.15%	-9.70%	-17.93%	0%	0%
Mean T=0	0.870	0.907	0.926	0.899	0.862
Observations	1,068	1,314	1,002	1,431	947
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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., $Court\ Acquittals\ or\ Convictions\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} In{-}trial\ Acquittals\ or\ Convictions^{s}_{i,t}}{\sum_{t=-1}^{11} In{-}trial\ decisions^{s}_{i,t}}$. 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. *** p<0.01, ** p<0.05, * p<0.1

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



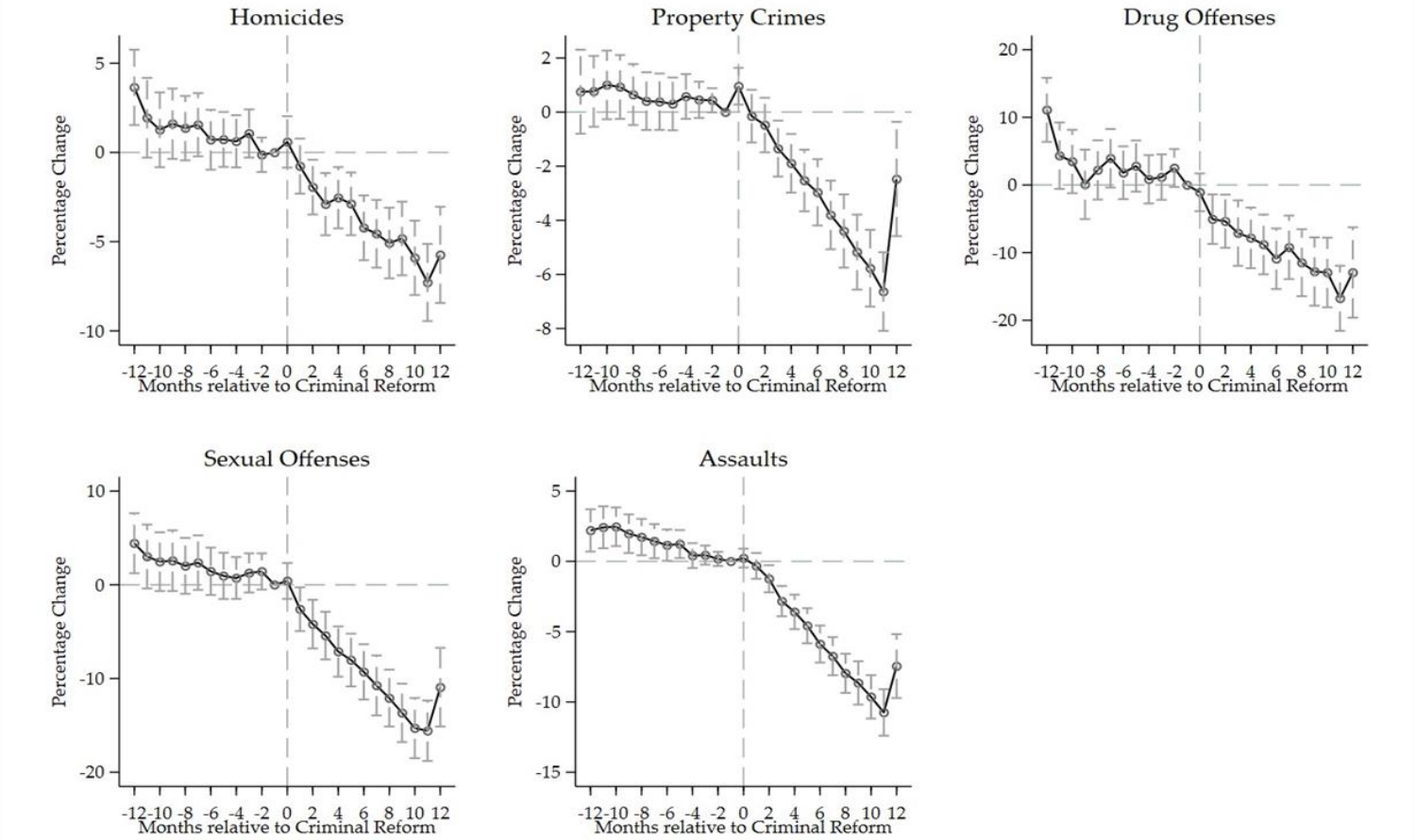
Note: this figure shows the results of an event study of the logarithm of the arrest rate by 100,000 inhabitants as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered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

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

	(1) Arrest rate	(2) Arrest rate
T	-0.380***	-0.369***
	(0.030)	(0.028)
Exposure Time to T		-0.005**
		(0.002)
Observations	77,094	77,094
R-squared	0.374	0.374
Year Month & Month-Year FE	YES	YES
Municipio FE	YES	YES
Controls	YES	YES
Mean T=0	14.05	14.05
Effect of T	-32%	-35%

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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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. *** p<0.01, ** p<0.05, * p<0.1

Figure 11. Leads-and-lags model for clearance rates



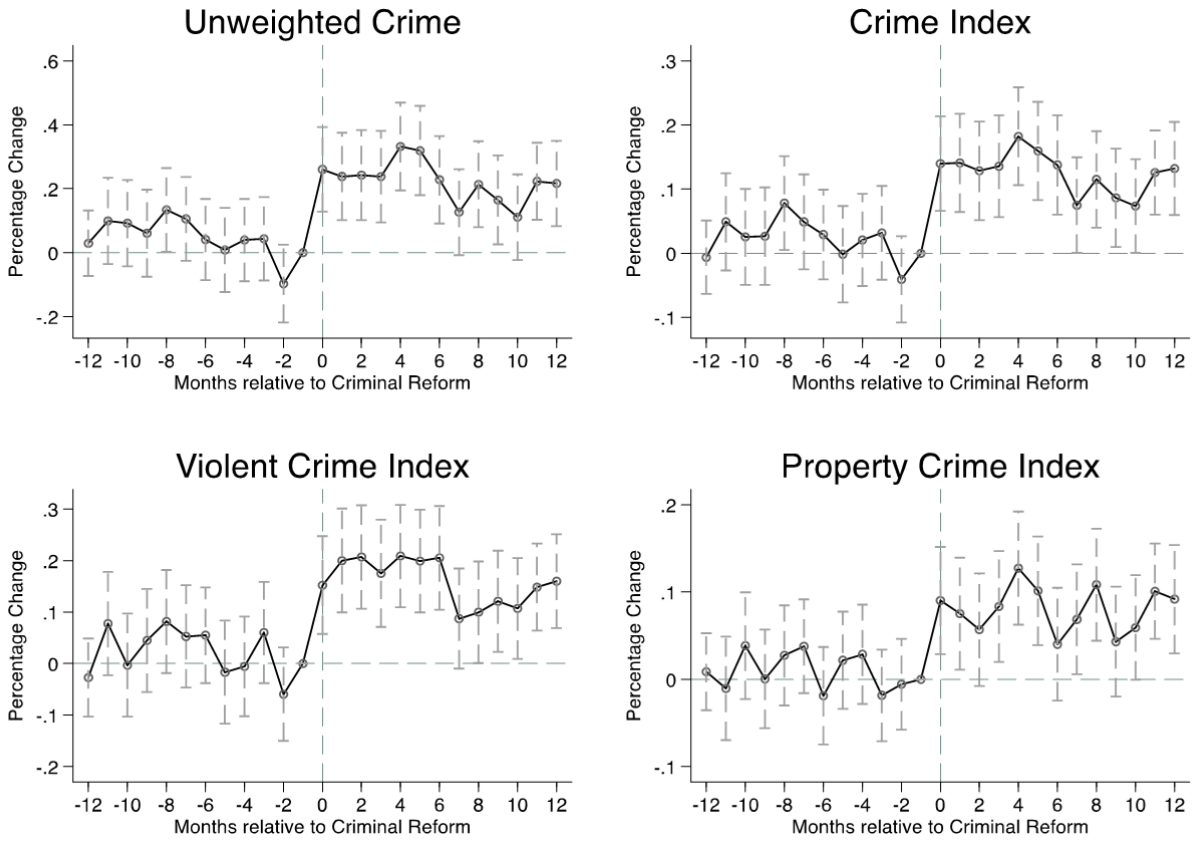
Note: this figure shows the results of an event study of the logarithm of clearance rates for different crimes as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered 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	-4.087***	-3.187***	-11.133***	-9.592***	-5.879***
	(0.709)	(0.533)	(1.672)	(1.216)	(0.541)
% Change after T	-23.75%	-23.61%	-15.74%	-24.58%	-26.80%
R-squared	0.387	0.484	0.416	0.431	0.603
Panel B					
T	-4.069***	-3.164***	-10.883***	-9.580***	-5.886***
	(0.707)	(0.534)	(1.678)	(1.222)	(0.544)
Exposure Time to T	0.087	0.104**	0.347***	0.032	-0.035
	(0.058)	(0.045)	(0.132)	(0.086)	(0.051)
% Change after T	-23.64%	-22.67%	-14.89%	-24.55%	-26.83%
R-squared	0.388	0.486	0.417	0.431	0.603
Observations	31,760	32,064	26,267	31,169	32,035
Year Month & Month-Year FE	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES
Mean T=0	17.21	13.50	70.74	39.03	21.94

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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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. 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., $Clearance\ Rate^{s}_{i,t} = \frac{\sum_{t=0}^{-11} Imputations^{s}_{i,t}}{\sum_{t=-1}^{11} Open\ Criminal\ Complaints^{s}_{i,t}}$. 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. *** p<0.01, ** p<0.05, * p<0.1

Figure 12. Leads-and-lags model for Aggregate Crime Indices



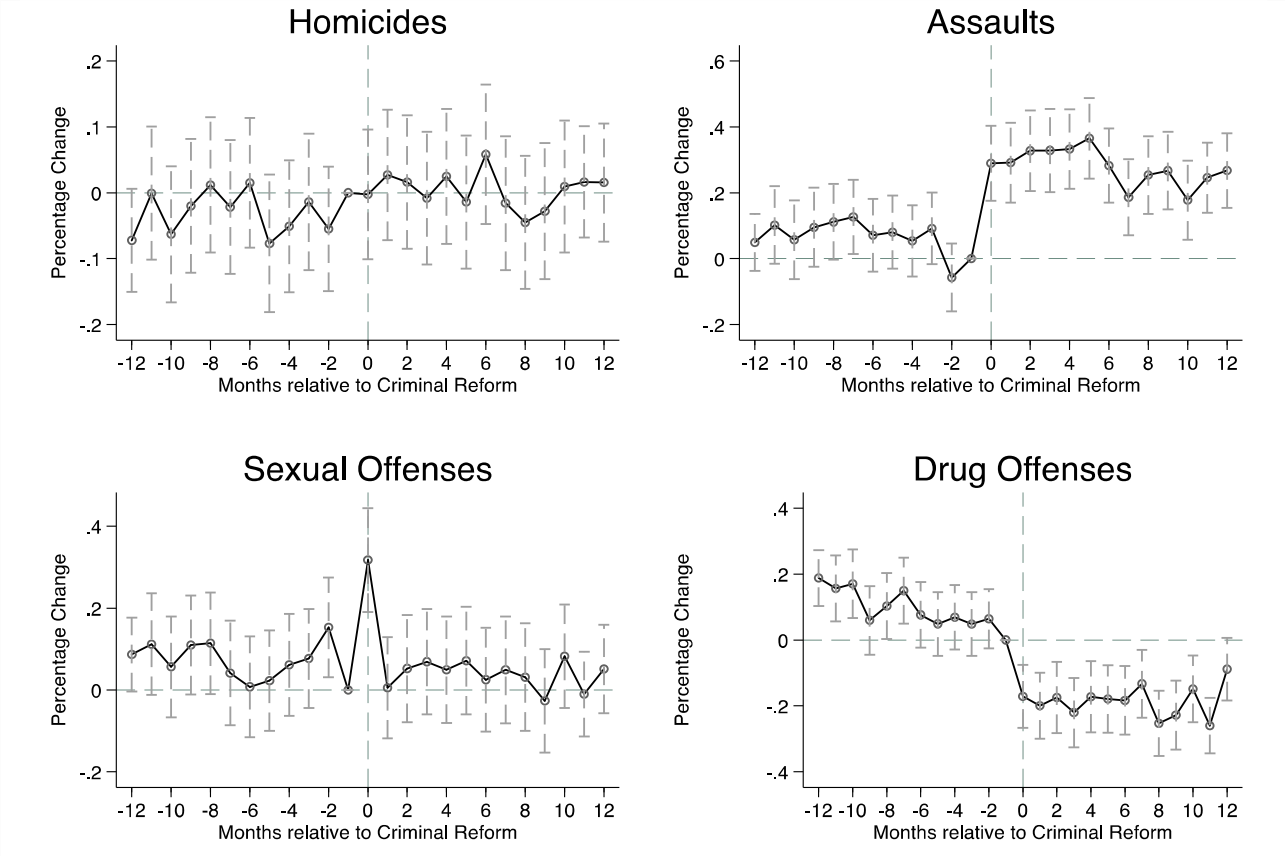
Note: this figure shows the results of an event study of the logarithm of different crime rates as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered standard errors. 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	0.195***	0.110***	0.141***	0.081***	0.169***	0.097***	0.135***	0.062***
	(0.031)	(0.017)	(0.020)	(0.017)	(0.030)	(0.016)	(0.019)	(0.016)
Exposure Time to T					0.012***	0.006***	0.002*	0.009***
					(0.002)	(0.001)	(0.001)	(0.001)
Observations	75,976	75,976	75,976	75,976	75,976	75,976	75,976	75,976
R-squared	0.359	0.333	0.279	0.400	0.360	0.333	0.279	0.403
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T=0	13.52	2.45	4.1	1.16	13.52	2.45	4.1	1.16
Effect of T	22%	12%	15%	8%	37%	18%	17%	19%

Note: this table shows the results of a two-way fixed effects regression of the logarithm of different crime rates (log+1) 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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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. Effect of T is calculated as $(\exp(\beta_1) - 1) * 100$ for columns (1) to (4) and as $(\exp(\beta_1 + 12 * \beta_2) - 1) * 100$ 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 13A. Leads-and-lags model for Violent and Drug-related crimes



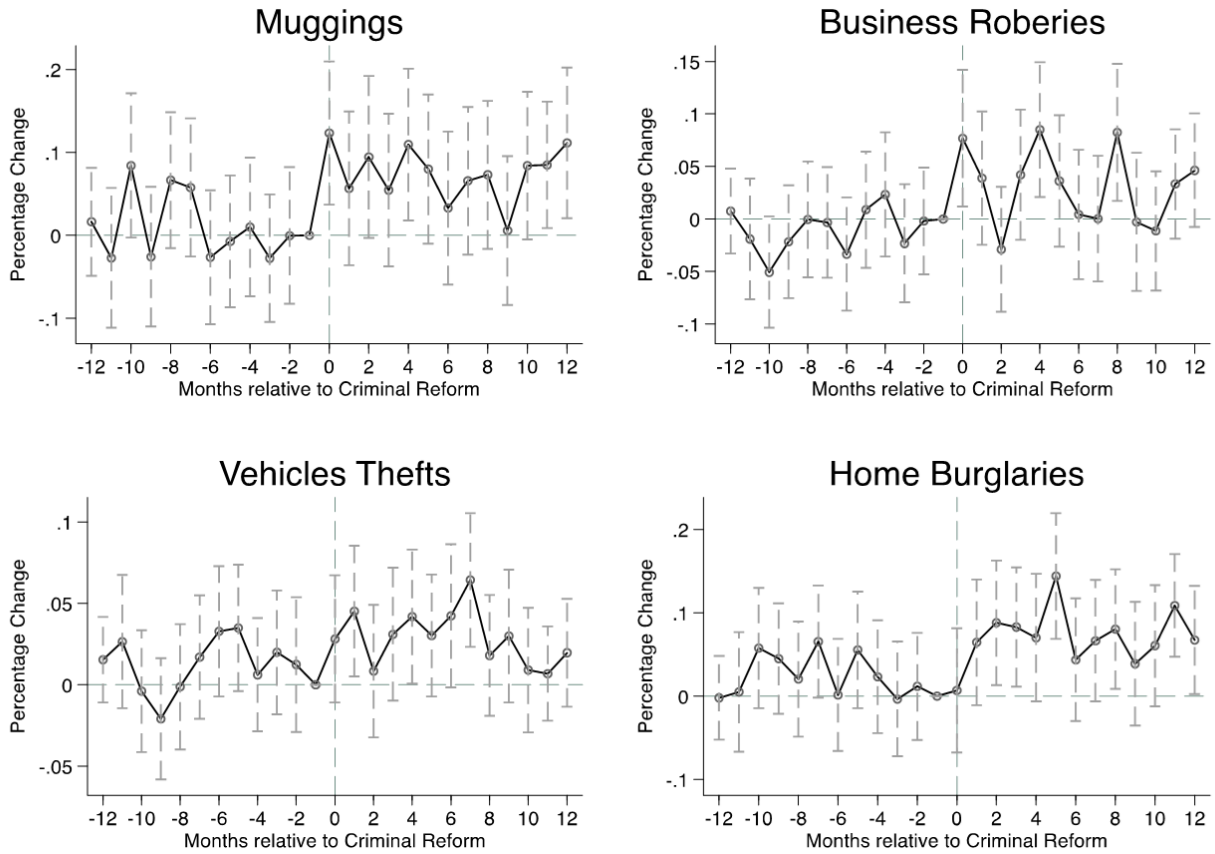
Note: this figure shows the results of an event study of the logarithm of different crime rates as a function of the leads and lags relative to the month of implementation of the reform in a municipality. Dashed lines represent 95% confidence intervals computed with municipality-clustered standard errors.

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

VARIABLES	Panel A				Panel B			
	(1) Homicides	(2) Assaults	(3) Sexual Offenses	(4) Drug Offenses	(5) Homicides	(6) Assaults	(7) Sexual Offenses	(8) Drug Offenses
T	0.034*	0.221***	-0.007	-0.297***	0.035**	0.207***	-0.009	-0.318***
	(0.018)	(0.029)	(0.023)	(0.026)	(0.017)	(0.028)	(0.022)	(0.026)
Exposure Time to T					-0.000	0.006***	0.001	0.012***
					(0.001)	(0.002)	(0.001)	(0.002)
Observations	75,976	75,976	60,882	60,882	75,976	75,976	60,882	60,882
R-squared	0.274	0.269	0.197	0.450	0.274	0.270	0.197	0.452
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T=0	3.98	4.52	4.13	2.93	3.98	4.52	4.13	2.93
Effect of T	3%	25%	-1%	-26%	4%	32%	0%	-16%

Note: this table shows the results of a two-way fixed effects regression of the logarithm of different crime rates (log+1) 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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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. Effect of T is calculated as $(\exp(\beta_1) - 1) * 100$ for columns (1) to (4) and as $(\exp(\beta_1 + 12 * \beta_2) - 1) * 100$ for columns (5) to (8). *** p<0.01, ** p<0.05, * p<0.1

Figure 13B. 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 municipality-clustered standard errors.

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

VARIABLES	Panel A				Panel B			
	(1) Muggings	(2) Business Robberies	(3) Vehicles Thefts	(4) Home Burglaries	(5) Muggings	(6) Business Robberies	(7) Vehicles Thefts	(8) Home Burglaries
T	0.083***	0.043***	0.016**	0.057***	0.054**	0.032**	0.018**	0.045**
	(0.025)	(0.014)	(0.008)	(0.019)	(0.024)	(0.014)	(0.008)	(0.018)
Exposure Time to T					0.013***	0.005***	-0.001*	0.005***
					(0.002)	(0.001)	(0.001)	(0.001)
Observations	75,976	75,976	75,976	75,976	75,976	75,976	75,976	75,976
R-squared	0.381	0.287	0.235	0.289	0.383	0.288	0.235	0.290
Year Month & Month-Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Municipio FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Mean T=0	2.42	0.77	0.44	1.40	2.42	0.77	0.44	1.40
Effect of T	9%	4%	2%	6%	23%	10%	1%	11%

Note: this table shows the results of a two-way fixed effects regression of the logarithm of different crime rates (log+1) 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, density of population, rural index, displaced population, and the lag of police arrest. Municipality-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. Effect of T is calculated as $(\exp(\beta_1) - 1) * 100$ for columns (1) to (4) and as $(\exp(\beta_1 + 12 * \beta_2) - 1) * 100$ for columns (5) to (8). *** p<0.01, ** p<0.05, * p<0.1