Alternative Justice, Unintended Harms: How Fast-Track Non-Prosecution Criminal Justice Affects Domestic Violence

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Abstract

This paper examines the unintended effects of fast-track, non-prosecution criminal procedures on domestic violence against women. Brazil provides an ideal setting for this analysis, as Law 9.099/95 established the *Juizados Especiais Criminais (JECrim)*, which mandated that all misdemeanor crimes, defined by sentencing thresholds in the Penal Code, be resolved through conciliation hearings rather than formal trials. This nationwide reform aimed to improve judicial efficiency by redirecting minor offenses outside the traditional court system. Although gender-neutral by design, the law applied to many domestic violence cases, particularly minor assaults. We exploit variation in pre-reform misdemeanor assault rates across judicial districts to estimate the impact of the policy on female homicide rates at home. Our difference-in-differences estimates indicate that, over the decade following implementation, a district with average baseline exposure experienced an increase of about 0.16 female homicides at home per 100,000 women, corresponding to roughly a 15% rise relative to the pre-reform mean. The results are robust across specifications and highlight how efficiency-oriented legal reforms can produce adverse spillovers when gender dynamics are not explicitly accounted for.

Keywords: Alternative Justice, Non-prosecution procedures, Non-custodial sanctions, Fast-track courts, Domestic Violence, Criminal Justice, Female Homicide, Law 9.099/95, Gender-Based Violence, Misdemeanor Assault, Difference-in-Differences (DiD), Unintended Consequences, Brazil.

JEL Codes: K14, J16, O15

1 Introduction

A significant and expanding body of literature in criminal justice has examined the efficacy of alternatives to traditional judicial processes (Shem-Tov et al., 2024; Agan et al., 2023; Mueller-Smith and Schnepel, 2020; Blattman et al., 2017). For nonviolent offenses, approaches for non-prosecution such as diversion programs and restorative justice have shown promise in reducing recidivism and improving outcomes relative to conventional methods. However, little is known about how these alternative mechanics perform in contexts of domestic violence, where gender-specific dynamics play a central role. This research gap is crucial because the factors influencing deterrence in domestic violence against women differ from those in other crimes, such as juvenile delinquency and armed robbery (Sherman and Berk, 1984).

Alternative mechanisms for resolving criminal cases outside the formal trial system are increasingly adopted worldwide, particularly for low-level offenses. These approaches aim to expedite case resolution, alleviate court congestion, and reduce reliance on incarceration. In Brazil, Law 9.099/1995 institutionalized such a mechanism by mandating that misdemeanor offenses be processed through *Juizados Especiais Criminais* (JECrim), special criminal courts that employ fast-track, non-custodial sanctions centered on conciliation hearings and plea agreements. The law redirects eligible cases away from formal trials, with eligibility determined by sentencing thresholds in the Penal Code, which classify misdemeanors as offenses punishable by up to two years of incarceration. Many of the cases processed through *JECrim* involve minor physical assaults, a common manifestation of domestic violence against women (Campos and Carvalho, 2006; Amorin et al., 2003; Azevedo, 2000; Faisting, 1999; Izumino, 2003). Moreover, a substantial number of these cases originate from Women's Police Stations (Debert and Oliveira, 2007), suggesting that violence against women comprises a meaningful share of the offenses handled under this framework.

The use of non-custodial sanctions in these cases, such as fines or community service, may undermine deterrence in domestic violence contexts. While the likelihood of punishment may increase under fast-track procedures, the reduced severity of sanctions can lower the perceived cost of aggression. This is particularly concerning in settings where violence tends to escalate over time (Amaral et al., 2023). Even initial, less severe assaults can evolve into more serious harm, as prior violence is a strong predictor of intimate partner homicide (Campbell et al., 2007), raising concerns that such reforms may unintentionally initiate or intensify violent behavior.

This paper assesses the impact of Brazil's judicial reform introducing non-prosecution procedures for misdemeanor crimes on female homicide rates at home, examining how this gender-neutral institutional change affected the severity of domestic violence. To estimate the causal effect of the reform, we implement a difference-in-differences design with continuous treatment, where treatment intensity is defined by each district's pre-reform misdemeanor assault rate,our measure of baseline exposure to offenses eligible for diversion. Estimates indicate that an average district, with 227 baseline misdemeanor assaults per 100,000 inhabitants, experienced an increase of about 0.16 female homicides at home per 100,000 women over the decade following the reform, corresponding to roughly a 15% rise relative to the pre-reform mean. We find no differential pre-trends in female homicide rates prior to the reform. We validate our treatment variable by showing that districts with higher baseline misdemeanor assault rates displayed no pre-reform differences in total criminal cases, but experienced larger and persistent post-reform declines in formal criminal filings, consistent with greater diversion of cases to the Special Criminal Courts (JECrim). Placebo tests show no effects on outcomes unlikely to be

affected, such as male homicides at home, female homicides outside the home, or heart-attack deaths, supporting the interpretation that the increase is specific to domestic violence dynamics. The findings suggest that efficiency-oriented, gender-neutral judicial reforms can produce unintended adverse spillovers in the context of intimate partner violence, highlighting the importance of incorporating gender sensitive design and safeguards when implementing non-prosecution procedures in domestic violence related cases.

Brazil offers a particularly relevant setting for this analysis. In 1995, Law 9.099 introduced a mandatory nationwide reform that redirected all misdemeanor crimes, including many domestic violence cases, to fast-track conciliation procedures. A decade later, the Maria da Penha Law (2006)[1] reversed this approach by mandating formal prosecution and specialized courts for all forms of gender-based violence, regardless of severity, leading to measurable reductions in female homicides (Sviatschi and Trako, 2024). This sequence of reforms is particularly revealing. While Law 9.099 sought to improve judicial efficiency, its gender-neutral design treated many domestic violence as a minor offense subject to conciliation, blurring its distinction from other misdemeanors. This approach likely weakened institutional responses to gender-based violence and delayed the effectiveness of later protections introduced by the Maria da Penha Law. We focus on São Paulo, one of the world's largest metropolitan areas (WPR, 2024), and use administrative data on criminal cases and mortality to estimate the effect of the reform on female homicides at home.

Drawing on insights from criminal justice and gender studies, this paper contributes to the growing literature on alternatives to traditional criminal justice practices and their implications for violence against women. A central concern in this debate is whether alternative interventions deter future violence or inadvertently trigger backlash. Evidence from the Minneapolis Domestic Violence Experiment, as discussed by Sherman and Berk (1984) and Angrist (2006), suggests that arrests can deter misdemeanor domestic assaults. However, subsequent studies find mixed effects, with arrests not always outperforming other responses such as mediation or separation (Dunford et al., 1990; Berk et al., 1992; Schmidt and Sherman, 1993). The effectiveness of these interventions may depend on contextual factors, including the availability of alternative measures (Berk and Sherman, 1988) and offender characteristics such as employment status (Pate and Hamilton, 1992). More recently, Amaral et al. (2023) shows that while policies like mandatory arrest may offer short-term protection, they can also increase the risk of future violence through retaliation. This concern is particularly relevant for non-custodial mechanisms, which operates at an earlier stage of the escalation cycle.

This study also complements research evaluating gender-targeted interventions, such as domestic violence laws (Ferraz and Schiavon, 2022; Stevenson and Wolfers, 2006; Amaral, 2017), women's justice centers (Sviatschi and Trako, 2024; Perova and Reynolds, 2017), and community-based mediation programs (Sonnenberg et al., 2022). While much of the existing evidence focuses on explicitly gender-sensitive policies, this paper examines the unintended consequences of a gender-blind legal reform, uniformly applied without regard to gender.

The remainder of the paper is structured as follows. Section 2 provides institutional context and describes the implementation of special criminal courts (*JECrim*) to handle misdemeanor crimes in Brazil. Section 3 outlines the theoretical framework and mechanisms through which this alternative procedure may influence the escalation of domestic violence. Section 4 describes the data, and Section 5 outlines the empirical strategy. Section 6 presents the main findings, and Section 7 concludes.

[1]Recognized by the United Nations as one of the world's most effective legal frameworks to combat violence against women, surpassed only by the laws of Spain and Chile (Women, 2011)

2 Background

Alternative justice practices are strategies for resolving disputes outside traditional court trials. Increasingly adopted worldwide, these approaches aim to ease pressure on judicial systems, accelerate case resolution, and offer more flexible responses to legal conflicts. While their design varies across countries, they generally apply to civil and low-level criminal cases and operate as substitutes or complements to prosecution. Typically non-custodial, they prioritize sanctions like fines, community service, or behavioral conditions over incarceration.

This section provides institutional background for the analysis. First, we explain the institutional framework established by Brazilian Law 9.099/95 in September 1995, which created special criminal courts (*JECrim*) to handle minor offenses through conciliation hearings. Second, we discuss the 2006 enactment of the Maria da Penha Law, a gender-targeted reform that redefined the legal treatment of domestic violence and its interaction with the earlier, gender-neutral framework.

2.1 Special Criminal Courts (JECrim)

Brazil's Law 9.099/95 established a specialized judicial system to adjudicate misdemeanor offenses (*crimes de menor potencial ofensivo*), defined as those punishable by up to two years of incarceration under the Penal Code.[2] The reform created Special Criminal Courts (*Juizados Especiais Criminais*, or JECrim), a parallel jurisdiction designed to process these cases through expedited, non-custodial procedures centered on conciliation hearings and plea agreements.

Before this reform, all criminal offenses, regardless of severity, were subject to the same formal process, including police investigation, formal charges, and a full trial. With the introduction of JECrim, once a crime is reported to the police and classified as a misdemeanor, the case is diverted from this conventional path and referred to the Special Criminal Courts, where it is processed through conciliation hearings. In these hearings, the defendant and the plaintiff sit across from one another in an informal, roundtable setting, as illustrated in Figure 1. A judge (or conciliator), accompanied by the prosecutor and a court clerk, facilitates the discussion. The objective is to encourage a mutual agreement between the parties, often involving a symbolic compensation, apology, or behavioral commitment, before formal charges are filed. If an agreement is reached, the case is dismissed without prosecution. If not, the prosecutor may offer a non-custodial sanction (*transação penal*), such as a fine or community service. Only if the defendant rejects this proposal does the case proceed to a simplified trial. The format is designed to be less adversarial than traditional courtrooms, aiming for swift and consensual resolution.

In São Paulo, the focus of our empirical analysis, JECrim units were initially incorporated into existing judicial facilities. By 1996, nearly 37% of all criminal cases in the state were processed under this new framework (author's calculation based on administrative data).

[2]The original threshold was one year and was expanded to two years by Law No. 11.313 in 2006. Common examples include minor assault, threats, and defamation. The 1988 Constitution laid the foundation for the establishment of special courts to promote a non-custodial procedures, an initiative further advanced by a 1989 federal bill that later became Law 9,099 in September 1995.[3]

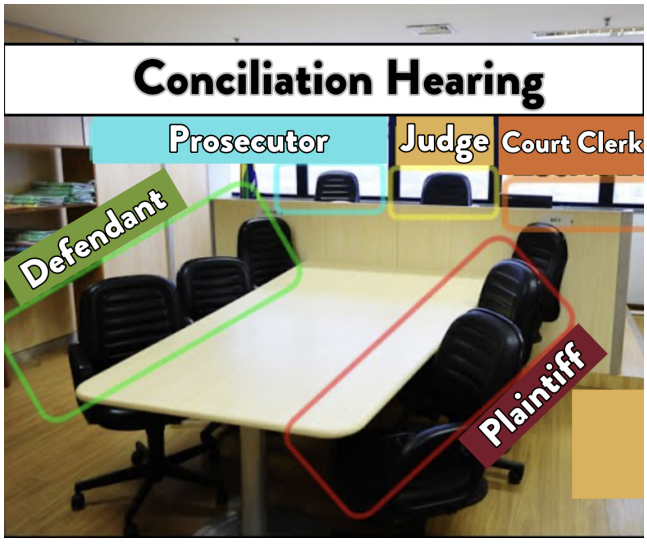


Figure 1: Typical Setting for Conciliation Hearings under JECrim

Note: This figure illustrates a typical physical setting of a conciliation hearing under the Special Criminal Courts (*Juizados Especiais Criminais*, JECrim). The layout highlights the presence of the judge (or conciliator), prosecutor, and court clerk, as well as the positioning of the defendant and the plaintiff around a shared table. Legal representation is not mandatory. Conciliation hearings aim to resolve misdemeanor cases (*crimes de menor potencial ofensivo*) through non-custodial sanctions (*transação penal*), such as fines or community service, avoiding formal prosecution when an agreement is reached. Initially, conciliation hearings were held in reserved rooms within existing judicial facilities. Over time, dedicated spaces were progressively created to accommodate this procedure.

2.2 JECrim and Domestic Violence

At the time the reform was implemented, domestic violence was not yet a legally defined category in Brazil. The primary institutional response to violence against women was the establishment of Women's Police Stations (WPS), which offered specialized support for female victims. Most cases reported to WPS involved minor assaults and threats, offenses that, under Law 9.099/95, fell within the jurisdiction of JECrim. In fact, approximately 45% of all cases processed through JECrim originated in WPS (Debert and Oliveira, 2007). Marital disputes made up more than half of all conciliation hearings (Amorin et al., 2003). This use of JECrim for domestic violence raised significant concerns. Research by de Lima et al. (2002) highlights a significant gap in judicial training, with many judges lacking the specialized expertise needed to handle domestic violence cases appropriately. As a result, these cases are often misclassified as minor family disputes, without acknowledging the specific dynamics characteristic of domestic violence. A partial response to this gap came with the enactment of Law 10.455/2002, which authorized judges to issue precautionary measures such as removing the aggressor from the household in cases of domestic violence.

In 2006, Brazil enacted the Maria da Penha Law, a landmark reform in the country's legal framework addressing violence against women. The law introduced a comprehensive set of preventive, protective, and judicial measures designed to strengthen institutional responses to domestic violence. Notably, it prohibited the use of JECrim under conciliation hearings in domestic violence cases, recognizing the power asymmetries involved and the risks of retraumatization. It also mandated the creation of specialized judicial procedures and courts to handle domestic violence cases, marking a shift toward a gender-sensitive approach in criminal justice.[4]

Although this paper focuses on domestic violence outcomes prior to the enactment of the Maria da Penha Law, the 2006 reform is important for contextualizing earlier institutional arrangements.

Figure 2 shows the evolution of JECrim case volume following the implementation of Law 9.099/95. Prior

[4]The Maria da Penha Law (Law No. 11.340/2006) is recognized by the UN as one of the world's most advanced legal frameworks to combat gender-based violence.

to the reform, all misdemeanor offenses were adjudicated in the regular criminal courts. After 1995, these cases were increasingly redirected to the newly established Special Criminal Courts (JECrim) for expedited, non-custodial resolution through conciliation hearings. This trend continued until the Maria da Penha Law mandated that all domestic violence cases, including misdemeanors, be handled by specialized domestic violence courts. The observed shift suggests that a substantial share of cases previously processed through JECrim likely involved domestic violence. While JECrim continues to operate for other misdemeanors, all domestic violence proceedings are now exclusively routed through specialized judicial channels.

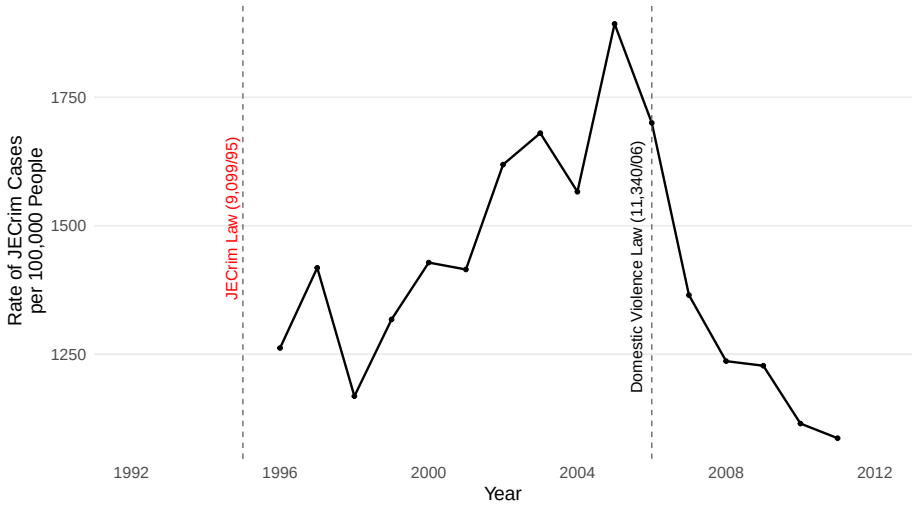


Figure 2: Use of JECrim Over the Years in São Paulo, Brazil

Note: This figure plots the rate of cases processed by the Special Criminal Courts (*Juizados Especiais Criminais*, JECrim) per 100,000 inhabitants in São Paulo over time. The vertical red dashed line marks the enactment of Law No. 9.099/1995, which created JECrim and redirected misdemeanor offenses from regular criminal courts to expedited, non-custodial procedures centered on conciliation hearings. The right vertical dotted line marks the implementation of the Maria da Penha Law (Law No. 11.340/2006), which reassigned domestic violence cases, including misdemeanors, to specialized domestic violence courts. The decline in JECrim case volume after 2006 descriptively suggests that a substantial share of cases previously processed by JECrim may have involved misdemeanor domestic violence. The figure is purely descriptive and intended to illustrate long-run trends in the use of JECrim and institutional reallocation of cases.

3 Mechanisms Linking Alternative Justice Reforms and Domestic Violence

The effect of alternative justice mechanisms on domestic violence (DV) remains ambiguous, as they may either mitigate or exacerbate violence depending on how offenders and victims respond. In the case of Brazil where the judicial reform under JECrim may have increased the likelihood of punishment by imposing non-custodial sanctions on individuals who might not have been prosecuted under the formal system,[5] it also reduced the severity of penalties. This shift raises concerns about the deterrent effect of the justice system, particularly in domestic violence cases, where lighter sanctions may fail to prevent escalation.

Victim responses to this framework are theoretically ambiguous. On one hand, faster, less intrusive procedures may encourage reporting by avoiding police investigation and formal trials. On the other hand, victims may hesitate to come forward if they perceive sanctions as too lenient, particularly when perpetrators quickly return home, raising the risk of retaliation. Offender behavior may also adjust to the new structure. Reduced sanctions could embolden some perpetrators, leading to escalation. Conversely, greater certainty of punishment, even if mild, could deter violence.

[5] See `https://www1.folha.uol.com.br/fsp/cotidian/ff240705.htm`

Importantly, under the reform, repeat misdemeanor offenders charged with the same offense become ineligible for diversion through JECrim and must be prosecuted in regular criminal courts. While this rule introduces the prospect of escalating consequences, it may not be sufficient to offset the immediate perception of leniency.

Impact on homicides. If the fast-track, non-custodial system operates as a deterrent in response to misdemeanor offenses, it could serve as an early intervention that prevents the escalation of domestic violence into lethal outcomes. Prior violence is one of the strongest predictors of intimate partner homicide (Campbell et al., 2007). Conversely, in a backlash scenario, the leniency of mild sanctions may embolden perpetrators, increasing the risk of repeated or more severe abuse. The perception of impunity may lower the cost of aggression, while emotional triggers, common in domestic disputes, can heighten the likelihood of extreme responses, potentially escalating into lethal violence (Card and Dahl, 2011).

We chose to analyze female homicides at home for four main reasons. First, women face disproportionately higher risks of intimate partner homicide in the home compared to men (UNODC, 2019). While 60% of all female homicides are committed by intimate partners or family members, only 12% of male homicides occur in the private sphere (UN Women, 2024). Second, homicide represent the most severe outcome of escalating domestic violence and are highly relevant from a public policy perspective. Third, homicide data are more reliable than other violence indicators, being less susceptible to underreporting or classification error. Fourth, alternative data sources for non-lethal violence, such as hospitalizations or police reports, are unavailable or inconsistent for the 1990s, the period covered by our analysis.

We therefore use female homicides at home as a proxy for intimate partner homicide and, more broadly, as a severe indicator of domestic violence. In contrast, male homicides at home are more likely to stem from other types of interpersonal violence. Our analysis focuses on how the shift to non-custodial justice procedures affected this specific outcome.

4 Data

We use data from São Paulo, Brazil's most populous state, which had over 37 million inhabitants in 2000 (IBGE, 2000), covering the period from 1992 to 2005. The unit of analysis is the judicial district (*comarca*), which encompasses one or more municipalities under the same jurisdiction. Although municipalities are the most granular administrative units, we rely on judicial districts because criminal cases, including those processed through JECrim, are handled at this level.[6]

To ensure geographic consistency over time, we fix boundaries to 1991 definitions and aggregate municipalities into minimum comparable judicial districts. We exclude districts with more than 200,000 residents in the 1991 census, as these areas differ substantially from smaller districts and are more likely influenced by broader crime trends. The final sample comprises a balanced panel of 242 judicial districts.

Criminal Cases We use administrative data from the São Paulo State Court of Justice (TJSP), which includes all criminal complaints filed between September 1994 and September 1995, the year immediately preceding the implementation of Law 9.099/95. The dataset provides detailed information on each case, including the date it was assigned to a courtroom, the judicial district where it was filed, and the legal classification of

[6]In Brazil, the lowest unit of court administration is the *foro*, or local courthouse, where cases are filed and processed. However, not all municipalities have their own *foro*. In such cases, judicial proceedings take place in the courthouse of a neighboring municipality within the same judicial district (*comarca*).

the offense. The last allows us to identify misdemeanor assault, the most common form of domestic violence falling under JECrim, and to construct our treatment variable based on its prevalence across judicial districts.

Mortality Records We use mortality data from the Brazilian Mortality Information System (SIM/DATASUS), maintained by the Ministry of Health, covering the period 1992–2005. All deaths in Brazil must be certified by a physician and reported to this system, ensuring high-quality administrative records. The dataset includes information on the cause of death, place of death (e.g., hospital, street, home), marital status, and age. We identify homicides using the International Classification of Diseases (ICD). Specifically, we include deaths caused by intentional injury inflicted by another person, corresponding to ICD-9 codes E960–E978 (for years before 1996) and ICD-10 codes X85–Y09 (for years 1996 onward).

Additional Sources We complement our analysis with baseline demographic and socioeconomic characteristics from the 1991 Brazilian Census conducted by the national statistics agency (IBGE). These include education levels, per capita income, access to public sanitation, urbanization rate, the share of black individuals, the female population share, the proportion of married individuals, average age, and total population. In addition, we use annual data on formal employment by gender and total municipal public revenues.

5 Research Design

5.1 The Treatment Variable

To estimate the impact of Brazil's fast-track, non-custodial justice system on female homicides at home, we exploit variation in baseline exposure to offenses eligible for the reform. Specifically, we construct a continuous treatment variable based on the judicial district's pre-reform rate of misdemeanor assault which proxies the potential volume of cases redirected to JECrim. We focus on misdemeanor assault because it is the offense most commonly associated with domestic violence and most likely to affect our outcome of interest. Higher baseline rates of misdemeanor assault indicate greater expected exposure to the non-custodial justice system, and therefore a larger anticipated impact on domestic violence-related outcomes.

Using baseline rates helps mitigate endogeneity concerns. By relying on pre-determined variation measured prior to the reform, we follow a strategy consistent with approaches used in Angrist and Krueger (1991) and Acemoglu and Finkelstein (2008) to identify causal effects. The key identifying assumption is that, absent the reform, districts with higher baseline rates of misdemeanor assault would have followed similar trends in female homicides at home as those with lower rates. After the reform, however, these high-exposure districts saw a greater share of cases redirected to JECrim, and therefore a larger shift in the treatment regime. Any differential change in female homicide rates in these districts can thus be attributed to the reform. This design reduces bias from contemporaneous shocks, such as shifts in enforcement, institutional capacity, or socioeconomic conditions, because it is based on pre-reform exposure.

Empirically, we calculate the baseline misdemeanor assault rate M_i (number of misdemeanor assaults per 100,000 inhabitants) for each judicial district i during the pre-Law implementation period, from September 1994 to September 1995.

$$M_i = \frac{\frac{1}{2}\sum_{t=1994}^{1995} \text{Misdemeanor assault cases in } (i,t)}{\text{Population in } 1991_i} \times 100,000 \tag{1}$$

The numerator captures the average number of misdemeanor assault cases, typically eligible for non-custodial resolution under the reform, recorded in the year preceding its enactment. The rate is calculated per 100,000 inhabitants using population data from the 1991 census. This continuous measure captures baseline variation in exposure to the reform.

Figure 3 shows the spatial distribution of our treatment variable, the baseline misdemeanor assault rates, across judicial districts in the state of São Paulo prior to the reform. Darker areas indicate higher rates, revealing substantial geographic variation in exposure to the non-custodial system introduced by the reform. This heterogeneity provides the basis for our identification strategy.

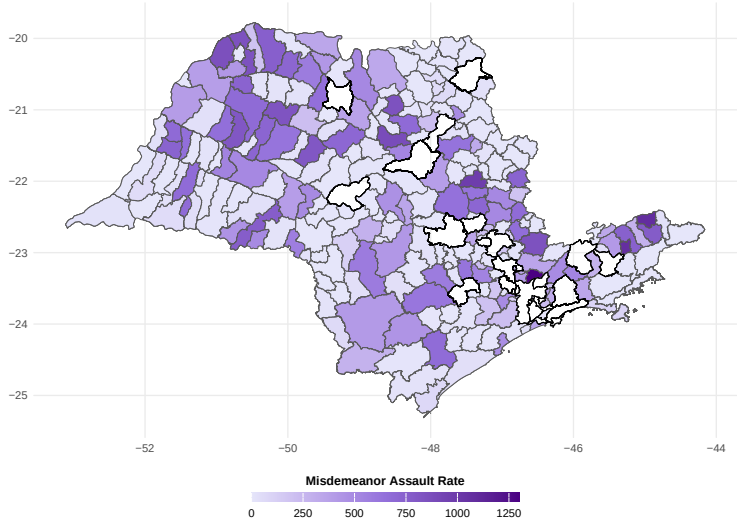


Figure 3: Intensity of Misdemeanor Assault Rate

Note: This figure maps the spatial distribution of baseline misdemeanor assault rates across judicial districts in the state of São Paulo prior to the reform. Darker shading indicates higher assault rates per 100,000 inhabitants. The map highlights substantial geographic heterogeneity in pre-reform exposure to the non-custodial criminal justice system introduced by Law No. 9.099/1995. This cross-district variation underlies the empirical strategy by providing differential baseline intensity of treatment across locations.

5.2 Empirical Estimation

As described in Section 5.1, the impact of the reform that introduced a fast-track, non-custodial justice system is expected to be greater in judicial districts with higher baseline rates of misdemeanor assault. To quantify this effect, we estimate a difference-in-differences (DiD) model that compares changes in female homicides at home before and after the reform across judicial districts i, for $t \in (1992, 2005)$.

$$y_{it} = \alpha_i + \gamma_t + \beta\,(\text{POST}_t \times M_i) + \sum_{j \neq 1995} \eta_j X_i 1\{j = t\} + \epsilon_{it} \tag{2}$$

$$\text{POST}_t = \begin{cases} 1, \text{if } t \in [1996, 2005] \\ 0, \text{otherwise.} \end{cases}$$

y_{it} is the outcome variable of interest in judicial district i at time t from 1992 to 2005. POST_t is a binary variable equal to 1 for the ten years following the implementation of JECrim through Law 9.099/95 (1996–2005).

M_i is the baseline misdemeanor assault rate, as discussed in Section 5.1, α_i and γ_t represent, respectively, the full set of judicial district fixed effects and year fixed effects, and X_i is a vector of time-invariant covariates from the 1991 Census at the judicial district level, interacted with time dummies η_j, and $\{j = t\}$ is an indicator variable for year t. These covariates include average years of schooling, per capita income, log population, urbanization rate, access to sewage, and proportions of black, female, and married populations, as well as time-variant log male and female employment, and log municipal revenue. Lastly, ϵ_{it} is the random disturbance term capturing unobserved influences. All regressions employ standard errors clustered at the district level, and the models are weighted by gender-specific population. The analysis is restricted to districts with populations under 200,000 in the 1991 Census.

We also conduct an event study to analyze the dynamic impact of the reform over time, capturing year-by-year changes in outcome.

$$y_{it} = \alpha_i + \gamma_t + \sum_{j \neq 1995} \beta_t M_i 1\{j = t\} + \sum_{j \neq 1995} \eta_j X_i 1\{j = t\} + \mu_{it} \tag{3}$$

Equation 3 extends the static specification by introducing year-specific interactions to estimate the dynamic effects of the reform. Specifically, the term $M_i 1\{j = t\}$ represents the interaction between the continuous treatment variable M_i (the pre-reform misdemeanor assault rate) and year indicators. β_j allows us to estimate, for each year $j \neq 1995$, the differential impact of baseline exposure to the reform on the outcome of interest. The omitted year is 1995, the last year prior to the implementation of Law 9.099/95, and serves as the reference category. The remaining components of the model, district fixed effects α_i, year fixed effects γ_t, and time-varying controls X_i interacted with year dummies are analogous to those included in the static model described in Equation 2.

5.3 Identification

The identification assumption is that in the absence of reform, judicial districts with different misdemeanor assault rates would not display systematically different trends in female homicides at home. In a continuous DiD setting, the parallel trends assumption is stronger compared to the binary treatment case. Let $y_{i,t}(0)$ denote the potential outcome of region i in year t without the absence of the reform. Then, identifying β requires, at minimum, that

$$E\big[y_{i,t}(0) - y_{i,t-1}(0) | M_i = m\big] = E\big[y_{i,t}(0) - y_{i,t-1}(0) | M_i = m'\big] \tag{PT.1}$$

for all $m \neq m'$. Assuming that equation (2) includes covariates with time-invariant coefficients, condition (PT.1) implies that even if the average potential outcome y(0) at time t differs between regions with exposure levels m and m', these differences in baseline misdemeanor rates do not lead to differential trends in the outcome in the absence of the reform. Then, under the additional assumption that

$$E\big[y_{i,t}(m) - y_{i,t-1}(0) | M_i = m\big] = E\big[y_{i,t}(m) - y_{i,t-1}(0) | M_i = m'\big] \tag{PT.2}$$

for all $m \neq m'$, the OLS estimate for β from equation (2) identifies the average causal response of the exposure to the reform. The condition (PT.2) states that the average causal effect of exposure m is the same for regions of any exposure level.

Given the context of this study and the nature of the data, several empirical challenges need to be carefully addressed. A key concern is that our treatment variable, based on baseline misdemeanor assault rates, may not

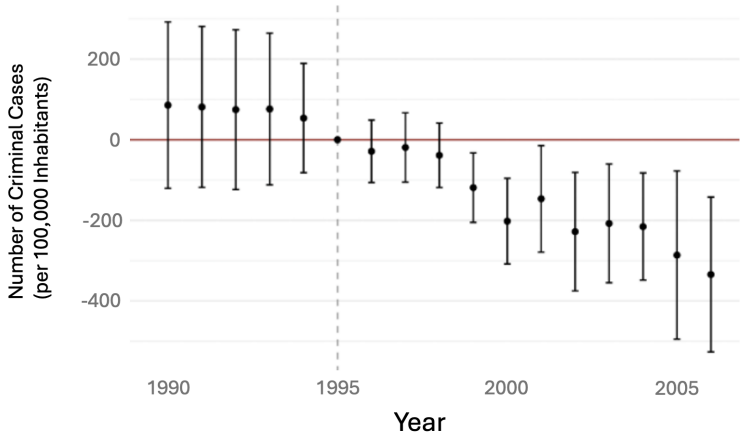


Figure 4: Dynamic Effects of Law 9.099/1995 on Formal Criminal Case Filings

Note: This figure provides a validation of the treatment intensity measure based on baseline misdemeanor assault rates. It presents event-study estimates of the dynamic effects of Law No. 9.099/1995 on the number of formal criminal case filings per 100,000 inhabitants across judicial districts in São Paulo between 1990 and 2006. The vertical dashed line marks the implementation of the reform. Points represent estimated effects by relative year, with 95 percent confidence intervals. Districts with higher baseline misdemeanor assault rates experience a larger and more persistent decline in formal criminal case filings after the reform, consistent with greater diversion of eligible cases to JECrim. This pattern indicates that the treatment variable captures differential exposure to the reform in the expected direction, supporting its relevance for the main analysis.

accurately capture differential exposure to the reform. To validate its relevance, we examine whether judicial districts with higher baseline rates experienced greater shifts in case processing following the reform. Figure 4 provides empirical support for this assumption. We estimate an event-study specification analogous to our empirical model in Equation 3, but using the number of formal criminal case as the outcome variable. The sample covers judicial districts between 1990 and 2006. Results show a significantly larger and persistent decline in formal criminal cases after the reform in districts with higher baseline misdemeanor assault rates, those more exposed to the redirection of eligible cases to JECrim. This pattern is consistent with the interpretation that more exposed districts diverted a larger share of misdemeanor offenses away from the formal system, leading to a sharper drop in formal criminal filings.

Data constraints also limit the scope of our analysis. In particular, the lack of information on defendant gender prevents a deeper examination of victim–offender dynamics, and the absence of recidivism data restricts our ability to assess whether the reform affected repeat offending.

Second, a critical identification concern is the validity of the parallel trends assumption, namely, that judicial districts with differing baseline misdemeanor assault rates did not exhibit systematically divergent trends in female homicides at home prior to the reform's implementation. This assumption is crucial to ensure that any observed effects can be causally attributed to the reform rather than pre-existing trends. To address this, we restrict the analysis to smaller judicial districts (under 200,000 inhabitants), which helps isolate the broader, distinct crime trends that may be present in larger districts.

Third, confounding factors such as other concurrent reforms or regional differences in the criminal justice system or socio-economic conditions could distort the estimated effects of the reform. For example, the implementation of the reform could coincide with broader changes in policing, victim reporting behavior, or economic conditions, all of which could influence domestic violence outcomes and potentially bias the results. To address this, we will test for these confounders by considering them as placebos. Additionally, Figure 5 shows that employment trends follow similar patterns across treatment intensity, indicating that employment-related shocks are potentially unlikely to confound the observed results. This helps mitigate concerns regarding broader economic factors driving the changes in female homicide rates. Nonetheless, we include controls for

time-varying economic conditions, such as formal employment by gender and municipal revenue, to account for residual variation in local labor markets and fiscal capacity.

Fourth, we address the concern that observed changes in outcomes might be geographically concentrated. Figure 6 displays the spatial distribution of changes in female homicide rates at home across judicial districts in São Paulo. The map compares average rates from the pre-reform period (1992–1994) to the post-reform period (2003–2005). The red-to-green color gradient reflects the direction and intensity of change, with darker red shades indicating larger increases and darker green shades indicating larger decreases in female homicide rates at home. The figure shows that these changes are widely distributed across the state rather than concentrated in a specific region, supporting the notion that the observed effects are potentially not driven by localized shocks.

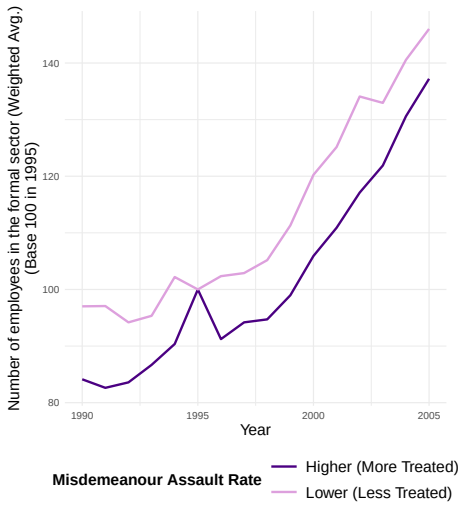


Figure 5: Employment Trends by Treatment Intensity

Note: This figure plots trends in formal employment over time across judicial districts grouped by treatment intensity. Districts are classified as more treated (top 70th percentile) and less treated (bottom 30th percentile) based on baseline misdemeanor assault rates. Employment trends evolve similarly across groups prior to and after the reform, suggesting that differential employment shocks are unlikely to drive the main results.

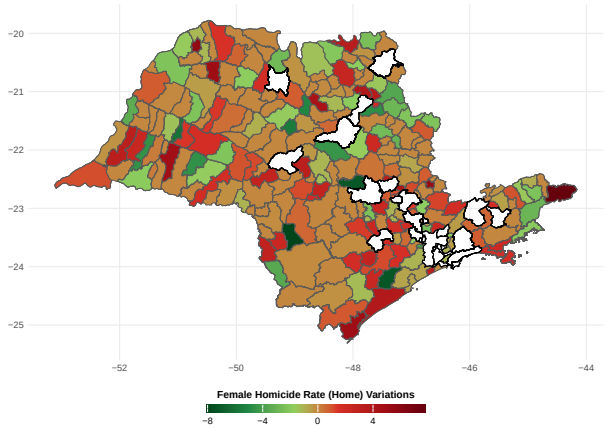


Figure 6: Variations in Female Homicide Rates (Deaths at Home) 1992–1994 vs. 2003–2005

Note: This figure maps changes in female homicide rates at home across judicial districts in São Paulo, comparing the pre-reform period (1992–1994) to the post-reform period (2003–2005). The red-to-green color gradient indicates the direction and magnitude of change, with darker red areas representing larger increases and darker green areas representing larger declines. Changes are geographically dispersed rather than concentrated in specific regions, supporting the interpretation that the estimated effects are unlikely to be driven by localized shocks. Rates are expressed per 100,000 women. Districts with fewer than 20,000 inhabitants are excluded, and transparent areas denote exclusions.

Finally, measuring domestic violence is challenging. Administrative data on female homicides at home, used as a proxy for intimate partner violence, may not capture all cases of domestic violence, as not all homicides at home are related to domestic violence. However, women are significantly more vulnerable to intimate partner homicide in domestic settings compared to men (UNODC, 2019). Consequently, the female homicide rate at home can be reasonably interpreted as a proxy for intimate partner homicide, reflecting the broader dynamics of domestic violence. Figure 7 shows trends in female homicide rates both inside and outside the home. One key observation is that homicides at home exhibit lower temporal variability (0.9–1.2) compared to those outside the home (2.0–3.5). This highlights the distinct dynamics of domestic versus non-domestic homicides. Domestic homicides are less influenced by broader crime trends, while non-domestic homicides follow São Paulo's general homicide trajectory, with a peak in the early 2000s followed by a sharp decline, driven by factors like the rise of the PCC gang and violence-reduction policies (Freire, 2018).

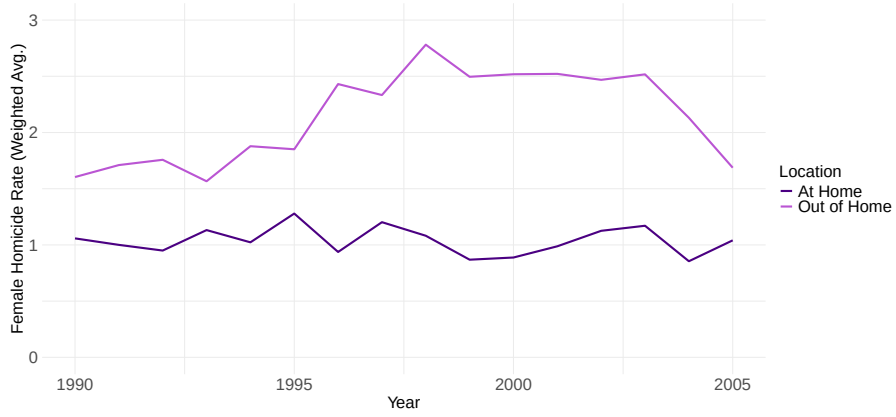


Figure 7: Trends in Female Homicide Rates (Home and Non-Home): Raw Data

Note: This figure plots trends in female homicide rates inside and outside the home in São Paulo using raw data. Rates are expressed per 100,000 women and correspond to weighted averages across judicial districts. Homicides occurring at home display substantially lower temporal variability than homicides occurring outside the home, highlighting distinct dynamics between domestic and non-domestic violence. While non-domestic homicides follow broader crime trends in São Paulo, characterized by an increase in the late 1990s and a sharp decline in the early 2000s, homicides at home evolve more smoothly over time. The figure is descriptive and intended to motivate the use of homicides at home as a more stable proxy for domestic violence, less influenced by aggregate crime cycles.

5.4 Summary Statistics

Table 1 presents summary statistics for key outcomes and treatment variables by gender in 1995 (baseline). The average female homicide rate is substantially lower than the male rate (2.7 vs. 20 per 100,000), but the gap narrows for homicides occurring at home (1.15 vs. 3.2), consistent with higher relative female victimization in domestic settings. The rate of heart attack deaths, used as a falsification outcome, is also lower for women (8.4 vs. 14). Pre-law misdemeanor assault rates are identical across gender columns because the treatment variable is defined at the judicial district level and does not vary by gender. On average, judicial districts had 227 misdemeanor assaults per 100,000 inhabitants prior to the reform.

Table 1: Summary Statistics by Gender in 1995

	N	Men		Women	
Variable		Mean	SD	Mean	SD
Outcomes					
Homicide Rate (Total)	242	20	24	2.7	4
Homicide Rate (Home)	242	3.2	5.6	1.15	2.4
Homicide Rate (Non-Home)	242	17	21	1.6	3
Homicide Rate (Heart Attack)	242	14	13	8.4	8.9
Treatment					
Misdemeanor Assault Rate (Pre-law)	242	227	294	227	294

Notes: This table reports summary statistics for key outcome and treatment variables by gender in 1995, the baseline year prior to the implementation of Law No. 9.099/1995. All rates are expressed per 100,000 inhabitants. Homicide Rate (Home) refers to deaths occurring within the household and is used as a proxy for domestic violence, while Homicide Rate (Non-Home) captures homicides occurring outside the household. Homicide Rate (Heart Attack) is included as a falsification outcome. The Misdemeanor Assault Rate (Pre-law) is measured at the judicial district level and therefore does not vary by gender. Statistics are computed across 242 judicial districts.

6 Results

This section presents the main results, focusing on the effects of the judicial reform in Brazil introduced by Law 9.099/95, which established Special Criminal Courts (JECrim) to process misdemeanor offenses through

conciliation hearings, a fast-track, non-custodial procedure. We analyze its impact on female homicide rates at home. The findings are supported by a series of robustness checks, including binary treatment definitions, difference-in-differences using Callaway and Sant'Anna's method, and placebo tests. Collectively, the evidence highlights the unintended consequences of this procedural shift, emphasizing its relevance for domestic violence outcomes and supporting the credibility and specificity of our estimates.

6.1 Main Results

We begin by discussing the static difference-in-differences specification (Equation 2), whose results are reported in Table 2. This model estimates the average effect of the reform over the ten-year post-treatment period, comparing judicial districts with varying levels of baseline exposure.

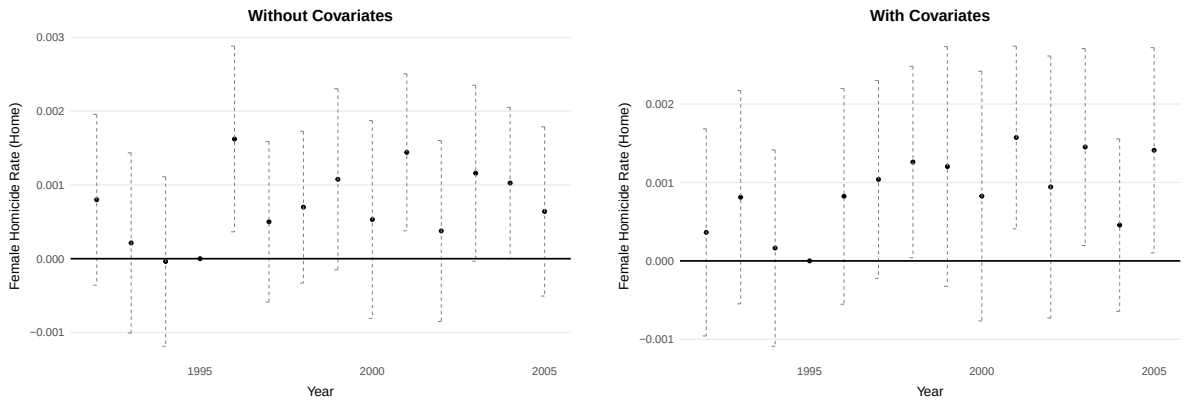
The estimates indicate that judicial districts with higher baseline misdemeanor assault rates (i.e., more treated districts) experienced significantly higher female homicide rates at home compared to less-treated districts (those with lower baseline misdemeanor assault rates). These results remain consistent even after the inclusion of controls, reinforcing the robustness of the findings. The estimates indicate that judicial districts with higher baseline misdemeanor assault rates experienced, on average, a 14% to 16% increase in the female homicide rate at home in the post-reform decade. Estimates indicate that, on average, a one-unit increase in baseline misdemeanor assault rate (per 100,000 inhabitants) is associated with 0.0007 additional female homicides at home (per 100,000 women) after the reform. Given that the average judicial district had a baseline misdemeanor assault rate of approximately 227 per 100,000 inhabitants, these estimates suggest that the introduction of the reform was associated with an increase in the female homicide rate at home of 0.16 ($\approx 0.0007 \times 227$) to 0.18 ($\approx 0.0008 \times 227$) over the first 10 years. Since the average female homicide rate at home is approximately 1.15, this corresponds to a substantial 14% ($\approx 0.16/1.15$) to 16% ($\approx 0.18/1.15$) increase in the female homicide rate at home for the average district.

Table 2: Static (TWFE) Regression Results for Female Homicide Rates at Home

Dependent Variable:	**Female Homicide Rate (Deaths at Home)**	
Treatment:	Misdemeanor Assault Rate (Pre-Law)	
Variable	**Without Controls**	**With Controls**
Treatment (Continuous) × Post	0.0007***	0.0008***
	(0.0002)	(0.0003)
Effect at mean baseline exposure (227)	0.16	0.18
Baseline mean of outcome (female homicide at home)	1.15	1.15
Change at mean (% of baseline mean)	14%	16%
Observations	**3,388**	**3,388**

Notes: This table reports estimates from a static difference-in-differences specification with two-way fixed effects (TWFE), where the dependent variable is the female homicide rate at home (deaths per 100,000 women). The treatment variable is the pre-reform misdemeanor assault rate (per 100,000 inhabitants) at the judicial-district level, interacted with a post-reform indicator. All specifications include judicial district and year fixed effects. Columns differ by the inclusion of time-varying controls. Controls consist of time-invariant district characteristics interacted with a linear time trend, including average years of schooling, per capita income, log population, urbanization rate, racial composition, marital status, and access to sewage (all based on the 1991 Census); As well as time-varying labor market indicators, such as female and male employment rates and log municipal revenue. Regressions are weighted by the gender-specific district population. Standard errors are clustered at the judicial district level. Coefficients capture marginal changes in the homicide rate associated with a one-unit increase in baseline misdemeanor assault rates. Reported "effects at mean" evaluate this marginal effect at the mean baseline exposure (227). Based on the baseline mean of the dependent variable (1.15), these correspond to an increase of approximately 14–16 percent in levels for the average district. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively

We then turn to the event-study specification (Equation 3), illustrated in Figure 8, which estimates year-specific treatment effects. This dynamic model allows us to examine the temporal evolution of the reform's impact. Figure 8a shows the estimated coefficients without covariates, while Figure 8b includes time-invariant demographic controls and time-varying labor market indicators. The pre-reform coefficients provide visual support for the parallel trends assumption: judicial districts with different baseline exposure show no systematic



(a) Female Homicide Rate (Home) - No Controls (b) Female Homicide Rate (Home) - With Controls

Figure 8: Event Study: Female Homicide Rate (Home) - With and Without Controls

Note: This figure presents event-study estimates of the effects of Law No. 9.099/1995 on the female homicide rate at home. Each panel plots year-specific coefficients from an event-study specification, normalized to the year immediately prior to the reform, capturing differential changes over time across judicial districts with varying levels of baseline misdemeanor assault rates. Panel (a) reports estimates without additional covariates, while Panel (b) includes time-invariant demographic controls and time-varying labor market indicators. Points represent estimated coefficients for each year, and dashed vertical bars denote 95 percent confidence intervals. Pre-reform coefficients are close to zero, providing visual support for the parallel trends assumption. Post-reform estimates indicate a increase in female homicides at home in more exposed districts.

differences prior to the reform. Post-reform, we observe a persistent rise in female homicide rates at home in more exposed districts. However, the trajectory is not strictly monotonic, exhibiting temporary fluctuations around an overall upward trend. These deviations may reflect idiosyncratic shocks or year-specific noise typical of rare-event outcomes, rather than systematic differences across districts. Nonetheless, the event-study estimates closely mirror the static results, confirming a robust and persistent impact of the reform.

6.2 Robustness

6.2.1 Binary Treatment

Our treatment variable is continuous, with judicial districts considered "more treated" when their baseline misdemeanor assault rates are higher. These districts are expected to have more misdemeanor domestic violence cases processed through alternative mechanisms, and thus, are likely to experience larger effects on female homicide rates at home compared to "less treated" districts with lower misdemeanor assault rates. To explore the sources of variation, we define treatment in binary terms using threshold values at the 30th, 50th, and 70th percentiles of the pre-reform misdemeanor assault rate distribution. Specifically, judicial districts are classified as treated ($= 1$) if their baseline misdemeanor assault rate exceeds the corresponding percentile cutoff.

Table 3 presents the results of this analysis. As expected, districts with the highest misdemeanor assault rates, i.e., those above the 70th percentile, are the most affected by the introduction of the judicial reform, showing the largest increases in female homicide rates at home. This supports the interpretation that higher baseline treatment intensity correlates with greater effects.

6.2.2 Difference-in-differences using Callaway and Sant'Anna

To assess the robustness of our findings, we additionally implement the difference-in-differences estimator proposed by Callaway and Sant'Anna (2021). Although our setting does not rely on staggered treatment timing, this estimator is well suited to our analysis because the package computes group-time ATT with double-robust estimation and automatically reweights treated and comparison units based on observable characteristics. We define a binary treatment indicator equal to one for judicial districts with baseline misdemeanor assault rates

Table 3: Static (TWFE) Regression Results for Female Homicide Rates at Home with Binary Treatment

Dependent Variable:	**Female Homicide Rate (Deaths at Home)**	
Treatment:	Misdemeanor Assault Rate (Pre-Law)	
Variable	**Without Controls**	**With Controls**
Treatment (>p30th) × Post	0.0979	0.2968
	(0.2175)	(0.2501)
Treatment (>p50th) × Post	-0.0258	0.1325
	(0.1820)	(0.2080)
Treatment (>p70th) × Post	0.4010**	0.3892**
	(0.1681)	(0.1585)
Baseline Mean of Dep. Var.	1.15	1.15
Observations	3,388	3,388

Notes: This table reports estimates from static difference-in-differences regressions with two-way fixed effects (TWFE), where the dependent variable is the female homicide rate at home (deaths per 100,000 women). The treatment variable is defined as a binary indicator equal to one if a judicial district's baseline misdemeanor assault rate exceeds a given percentile threshold of the pre-reform distribution. Three alternative thresholds are considered: the 30th, 50th, and 70th percentiles. The treatment indicator is interacted with a post-reform dummy. All specifications include judicial district and year fixed effects; columns differ by the inclusion of controls. Controls consist of time-invariant district characteristics interacted with a linear time trend, including schooling, per capita income, log population, urbanization, racial composition, marital status, and access to sewage, as well as time-varying female and male employment rates and log municipal revenue (1991 Census). Regressions are weighted by the gender-specific district population, and standard errors are clustered at the judicial district level. The baseline mean of the dependent variable is reported for reference. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

above the 70th percentile. The resulting estimates, constructed after adjusting the T and C groups for covariate differences, corroborate the robustness of our baseline results.

Table 4 shows the aggregate average treatment effect (ATT) over the 10 years following the reform. Judicial districts with higher treatment intensity (above the 70th percentile) experienced a significant increase in the female homicide rate at home, with an ATT estimate of 0.818. This result is comparable to the estimate of 0.392 reported in Table 2. The larger effect observed here (ATT = 0.818) may reflect the fact that the Callaway and Sant'Anna estimator reweights treated and control units based on covariates and accounts for treatment effect heterogeneity over time, potentially capturing a more pronounced cumulative impact.

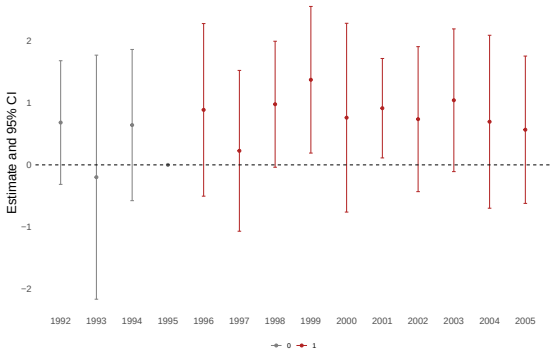
Figure 9 displays the event-study estimates, illustrating the yearly evolution of the female homicide rate at home for treated districts relative to control districts. The results suggest a persistent and positive effect of the reform on female homicides at home in treated districts, with point estimates mostly positive across post-treatment years. While the trajectory exhibits some year-to-year variability, the direction of the effects is broadly consistent with an upward shift. The aggregate average treatment effect (ATT) is positive and statistically significant, confirming that districts with higher initial exposure experienced a meaningful and lasting increase in female homicides at home following the reform. These findings reinforce the robustness of our baseline results and offer additional evidence on the unintended consequences of the judicial reform.

Table 4: Aggregate Callaway and Sant'Anna for binary treatment above the 70th percentile

Dependent variable:	Female Homicide (Home)
(Callaway & Sant'ana) ATT	0.818
	(0.3206)
95% Conf. Int.	[0.1897 1.4464]*
Mean Dep. in baseline	1.15
Controls	Yes
Observations	3,388

Notes: This table reports aggregate average treatment effects on the treated (ATT) estimated using the Callaway and Sant'Anna (2021) difference-in-differences estimator. The dependent variable is the female homicide rate at home (deaths per 100,000 women). The treatment is defined as a binary indicator equal to one for judicial districts with baseline misdemeanor assault rates above the 70th percentile of the pre-reform distribution. The estimator compares treated and control districts using doubly robust methods and reweights observations based on observable characteristics. All specifications include judicial district and year fixed effects and the same set of controls as in the baseline TWFE regressions. Regressions are weighted by the gender-specific district population, and standard errors are clustered at the judicial district level. The reported ATT represents the average post-reform effect over the sample period. The baseline mean of the dependent variable is reported for reference.

Figure 9: Event Study Callaway and Sant'Anna for Binary Treatment Above the 70th Percentile



Note: This figure presents event-study estimates based on the Callaway and Sant'Anna (2021) estimator for judicial districts with baseline misdemeanor assault rates above the 70th percentile. The figure plots year-specific treatment effects relative to the pre-reform period, illustrating the dynamic evolution of the reform's impact on female homicide rates at home. Points represent estimated coefficients for each year, and vertical bars denote 95 percent confidence intervals.

6.3 Placebo Analysis

A potential concern is that the observed effects may reflect broader trends unrelated to the reform or domestic violence. If the increase in female homicides at home were driven by general changes in crime, health, or economic conditions, we would expect to see similar effects on outcomes not plausibly affected by the reform. However, if the observed effect indeed stems from the unintended consequences of the reform on domestic violence, it should not affect outcomes unrelated to that context, such as male homicides at home, female homicides outside the home, or female deaths from non-violent causes like heart attacks. Similarly, it should not influence economic outcomes like employment or municipal revenues.

To assess this, we conduct placebo tests using outcomes unlikely to be affected by the reform. Table 5 presents the results. Panel A includes mortality outcomes: Male Homicide (Home), Female Homicide (Non-Home), and Female Heart Attack (Home). Panel B includes economic outcomes: Log Employment (Formal Sector) and Log Revenue. Across all specifications, we find no statistically significant effects, supporting the interpretation that the observed increase in female homicides at home is potentially specific to the mechanisms triggered by the reform and not driven by broader, unrelated factors.

Table 5: Static (TWFE) Regression Results for Placebo

Panel A: Death Placebos			
Dep. Var.	Male Homicide (Home)	Female Homicide (Non-Home)	Female Heart Attack (Home)
Treatment (Continuous) × Post = 1	0.0001	0.0001	-0.0010
	(0.0007)	(0.0005)	(0.0013)
Mean Dep. in Baseline	3.18	1.56	8.42
Observations	3,388	3,388	3,388
Panel B: Labor Placebos			
Dep. Var.	Log Employment (Formal Sector)	Log Revenue	
Treatment (Continuous) × Post = 1	0.0065	-0.0005	
	(0.0378)	(0.0003)	
Mean Dep. in Baseline	9302	16.5	
Observations	3,388	3,388	

Notes: This table reports estimates from static difference-in-differences regressions with two-way fixed effects (TWFE) for placebo outcomes unlikely to be affected by the reform. Panel A reports mortality outcomes: male homicide (home), female homicide (non-home), and female deaths from heart attacks (home). Panel B reports economic outcomes: log formal employment and log municipal revenue. The treatment variable is the pre-reform misdemeanor assault rate interacted with a post-reform indicator. All specifications include judicial district and year fixed effects and the same set of controls as in the baseline regressions. Regressions are weighted by the relevant population, and standard errors are clustered at the judicial district level. Mean baseline values of the dependent variables are reported for reference. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

These findings strengthen the interpretation of our main results. If the treatment variable, baseline misdemeanor assault rates, captures differential exposure to the reform, and more exposed districts exhibit larger increases in female homicides at home, then the absence of effects on unrelated outcomes provides compelling evidence that the results are not driven by confounding district-level characteristics. In other words, if the treatment were merely proxying for structural differences across judicial districts, such as economic conditions, institutional capacity, or reporting practices, we would expect to observe impacts on other outcomes as well, which we do not. This placebo analysis reinforces the view that the estimated effects likely reflect mechanisms related to domestic violence.

7 Conclusion

In this study, we examine the unintended consequences of Brazil's judicial reform introduced by Law 9.099/95, which created Special Criminal Courts (JECrim) to handle misdemeanor offenses through conciliation hearings, a fast-track, non-custodial procedure. Although designed to improve judicial efficiency, we show that this reform led to a significant increase in female homicides within the home in judicial districts with higher baseline exposure to eligible offenses. We find no effects on placebo outcomes unrelated to domestic violence and no evidence of differential pre-trends, reinforcing the interpretation that the observed effects reflect a causal impact of the reform on domestic violence outcomes.

Using a difference-in-differences framework, our analysis reveals a significant increase, around 15%, in female homicides at home in judicial districts with higher baseline misdemeanor assault rates over the ten years following the implementation of the judicial reform. This suggests that while the reform aimed to reduce court congestion by streamlining the resolution of minor offenses, it inadvertently contributed to the escalation of domestic violence.

It is important to emphasize that these effects arise in a context where the non-custodial procedure was designed to handle minor crimes in general, without incorporating the specific dynamics of domestic violence cases. Our findings align with prior criminal justice and gender studies emphasizing how the dynamics of domestic violence differ substantially from those of other crimes. Crucially, our results should not be interpreted as evidence that non-custodial measures are inherently ineffective for addressing domestic violence, or that custodial sanctions are necessarily better. Rather, the evidence suggests that applying a uniform judicial response to all offenses, without accounting for the unique dynamics and mechanism of the domestic violence context, can lead to unintended and harmful consequences.

The findings highlight the complexity of non-custodial procedures in the criminal justice system, suggesting that non-custodial penalties may embolden perpetrators, while the absence of specialized training for judges could exacerbate risks for victims. These institutional mechanisms warrant further investigation to better understand their role in shaping our results.

Our results contribute to the broader literature on the effects of legal reforms, underscoring the importance of incorporating gender-sensitive perspectives into criminal justice policy. We show that reforms aimed at improving efficiency, when designed without accounting for gender dynamics, can inadvertently perpetuate or even exacerbate violence against women. This research adds to the body of literature on the intersection between institutional design and gender-based violence, highlighting the risks of applying generic procedural reforms in contexts with high levels of domestic violence.

While our findings offer novel insights, some data constraints prevent a more granular analysis. In particular, the lack of information on defendant gender limits our ability to fully characterize victim-offender dynamics, and the absence of recidivism data restricts our capacity to assess how the reform influenced repeat offending. Future research should examine whether non-custodial justice alters reoffending patterns, especially in the context of domestic violence, and evaluate how offender and victim characteristics shape outcomes. Future work should also investigate models of alternative justice that incorporate specialized training, victim-centered safeguards, and differentiated procedures for crimes involving gendered power dynamics.

In conclusion, while non-custodial procedures can provide benefits in terms of efficiency and reducing court backlogs, their application in domestic violence cases requires careful consideration. The unintended consequences observed in this study highlight the importance of integrating gender dynamics into the design and implementation of legal reforms to effectively address and prevent domestic violence.

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