

Call It Femicide: Legal Recognition, Victim Protection, and Domestic Violence*

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Abstract

Can criminal law prevent domestic violence and change how victims use the state? We study Brazil’s 2015 recognition of femicide as qualified homicide using ten years of linked population, police, hospital, and prison records for 2.6 million women. Exploiting predetermined municipal exposure to men with histories of violence against women, we find that police-recorded non-fatal domestic violence fell disproportionately in more exposed areas. Conditional on an incident reaching police, requests for protective measures rose, while pursuit of charges did not. Legal recognition at the lethal endpoint can therefore affect both violence below that margin and survivors’ choice of state remedy.

JEL Codes: K14, K42, J12.

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1 Introduction

Violence against women is a global phenomenon shaped by local institutions. Nearly one in three women worldwide experience physical or sexual violence during their lifetime ([World Health Organization, 2024, 2021](#)), and domestic violence remains widespread in Brazil.¹ Yet the effectiveness of criminal law in this setting is uncertain. Victims and offenders often share a home, children, income, and repeated contact, so involving the state can provide safety while also risking retaliation, stigma, or the loss of a household earner.

Policies governing interpersonal violence affect two sets of choices. Offenders decide whether and how severely to offend, responding in part to expected legal costs ([Becker, 1968](#)). Victims decide whether to involve the state and which remedy to pursue, weighing safety against retaliation, stigma, and other private costs. Recorded violence is therefore an equilibrium outcome of both decisions. These responses need not move together: a law may deter violence without increasing state contact, increase reporting without changing underlying violence, or shift victims towards protective remedies that do not require prosecution. Evaluating criminal-law reforms using only offences, emergency calls, arrests, or prosecutions can consequently give an incomplete account of their effects. Distinguishing these margins requires a sharp change in law accompanied by data on both the incidence of violence and victims' choices once an incident reaches the state.

We study these trade-offs using Brazil's 2015 Femicide Law.² The law explicitly classified gender-motivated killings involving domestic and family violence as qualified homicide and placed them among heinous crimes ([Presidencia da Republica, 2015](#)). It thus combined a potential increase in expected punishment with legal naming and public salience. Although its statutory target was lethal violence, the reform may have altered behaviour earlier in an abusive relationship, i.e., before violence reached the fatal margin, and may have changed victims' willingness to use existing protective institutions.

We link at the individual level ten years of administrative population, police, hospital, and prison records in Rio Grande do Sul, the southernmost state of Brazil. The data follow 2.6 mil-

¹UN Women reports that 16.7% of Brazilian women aged 16 or older have experienced physical or sexual intimate-partner violence in their lifetime, and 3.1% did so in the previous year ([UN Women Data Hub, 2025](#)). Broader official surveys point to a larger margin of domestic and family violence: the 2023 DataSenado survey reports that 30% of Brazilian women have experienced domestic or family violence by a man, and that 76% of these victims experienced physical violence ([Instituto de Pesquisa DataSenado and Observatorio da Mulher contra a Violencia, 2023](#)). The 2019 National Health Survey reports that 19.4% of adult women experienced psychological, physical, or sexual violence in the previous twelve months; among women who suffered physical violence, 52.4% reported a partner or ex-partner as the perpetrator, and 72.8% reported that the episode occurred at home ([Instituto Brasileiro de Geografia e Estatistica, 2021](#)).

²In the Brazilian penal code, its formal definition is *Lei do Feminicídio No. 13.104/2015*.

lion women aged 18–55 and identify 257,846 domestic-violence victims in 427,835 victim-incident records. These data provide an unusual view of both sides of the institutional response. We observe whether a woman is recorded as a victim, the type and seriousness of the incident, subsequent imprisonment, and two choices made after the incident: whether she requests an urgent protective measure and whether she formally pursues charges. Protective measures and charges are legally distinct, allowing us to distinguish demand for safety from demand for prosecution.

The reform applied throughout Brazil, so we exploit predetermined differences in how strongly localities were exposed to it ([Callaway et al., forthcoming](#)). Exposure is measured via the share of men aged 18–55 in a woman’s municipality of birth who had a police record for violence against a woman at baseline. Our continuous difference-in-differences design estimates whether outcomes changed more after March 2015 for women from municipalities with greater baseline exposure. The identifying assumption is that, absent the law, outcomes would have evolved similarly across the exposure distribution. Event studies show no differential pre-trends, and the estimates survive flexible trends in predetermined municipal characteristics, diagnostics for continuous treatment, negative-control outcomes, and placebo exposure measures. Because all municipalities were covered by the Femicide Law, the design identifies differential post-reform changes associated with baseline exposure rather than the law’s average nationwide effect.

The analysis shows that recorded domestic violence fell disproportionately in more exposed municipalities. Moving from the 25th to the 75th percentile of baseline exposure is associated with a 3.8% decline in the probability that a woman is recorded as a victim. The decline is concentrated in serious non-fatal offences, i.e., bodily injury, threats, and fighting, and is absent for non-domestic violent victimization. Estimates for femicide are negative but imprecise, as lethal incidents are rare. The pattern is therefore consistent with prevention below the lethal margin. Although we cannot determine whether the non-fatal offences reduced by the reform would otherwise have escalated to femicide, these offences are consequential in their own right: bodily injuries and physical fighting impose immediate health and welfare costs, while threats can generate fear and coercive control even without realised physical harm. Threats often also signal a heightened risk of lethal escalation: prior threats to kill and threats with a weapon are among the strongest documented risk markers for intimate-partner homicide ([Campbell et al., 2003](#); [Spencer and Stith, 2020](#)).

Additional evidence does not support incapacitation as the principal mechanism. The estimated decline is similar for women with and without a recorded domestic-violence episode in the preceding two years, while neither the probability nor the duration of imprisonment rises detectably after a domestic-violence incident. The body of evidence also rules out reporting-based

explanations as the primary driver: the decline appears for serious police-recorded injuries, has the same sign in hospital records, and is not accompanied by a shift from victim-initiated reports to reports by family members.

The analysis also shows that victims’ use of the state changes. Conditional on an incident reaching the police, moving from the 25th to the 75th percentile of baseline exposure raises requests for protective measures by about 1.1% of the pre-reform mean. The strictest specification shows no corresponding increase in formal charges, and there is suggestive evidence that protection is requested sooner. Thus, the institutional response shifts toward protection rather than prosecution among recorded incidents, displaying a pattern that is consistent with victims treating protective measures as a lower-private-cost alternative to prosecution. Because this analysis conditions on post-reform incidents, it does not by itself show that the same victims changed behaviour or that protective requests caused the decline in violence. Taken together with the incidence results, however, these findings document a two-sided response: lower victimization in the population and a reallocation of victims’ state engagement toward protection among incidents reaching the police. This combination cannot be recovered from data containing only offences, emergency calls, or prosecutions, and it reveals a policy-relevant margin on which criminal-law reforms should be evaluated: not only whether violence changes, but also how victims use the remedies available to them.

The central contribution of this study is to measure these two margins jointly. Our linked records place population-level changes in recorded victimization beside victims’ choices among safety-oriented protection, formal charges, and a combination of the two, conditional on an incident reaching the police. This advances the existing literature in three ways. First, the paper contributes to research on police and legal responses to domestic violence ([Aizer and Dal Bó, 2009](#); [Iyengar, 2009](#); [Chin and Cunningham, 2019](#); [Black et al., 2023](#); [García-Hombrados et al., 2024](#); [Arteaga et al., 2025](#); [Amaral et al., 2026](#)). Existing work studies interventions that operate after a victim contacts the state or that change access to enforcement, including mandatory arrest, no-drop prosecution, realized arrests and charges, women’s police stations, and specialized courts.³ The closest study, [Amaral et al. \(2026\)](#), uses quasi-random officer assignment to show that arrest after an emergency call reduces repeat calls and subsequently increases criminal charges. We study an earlier institutional margin: a reform that changed the legal status of femicide before any particular police response. Unlike emergency-call data, our linked records

³Some articles also study the potential to combat domestic violence of divorce laws ([Stevenson and Wolfers, 2006](#); [Brassiolo, 2016](#); [García-Ramos, 2021](#); [Sanin, 2025](#)), education policy ([Gulesci et al., 2020](#)), migration policy ([Muchow and Amuedo-Dorantes, 2020](#)), national cash transfers ([Heath et al., 2020](#)), integration of women in police ([Miller and Segal, 2019](#)), all-women’s justice centres ([Kavanaugh et al., 2018](#)), women’s police stations ([Perova and Reynolds, 2017](#); [Amaral et al., 2021](#)), unilateral exit rights ([Corradini and Buccione, 2023](#)), domestic-violence criminalization ([Ferraz and Schiavon, 2022](#); [Sanin, 2025](#)), and justice centres ([Sviatschi and Trako, 2024](#)).

combine population victimization with recent first and repeat incidents, imprisonment, and victims’ separate choices of protection and charges. These data reveal a different pattern, i.e., no detectable increase in imprisonment and greater use of protection without a robust increase in charges, and therefore they identify a protection-versus-prosecution trade-off that emergency-call data do not contain.

Second, the paper speaks to the two-sided economics of domestic violence. Intimate Partner Violence (IPV) responds to economic opportunities, incentives, and shocks faced by both partners, but the effect of women’s employment or resources is theoretically and empirically ambiguous: stronger outside options may facilitate bargaining or exit, while employment may also change exposure, create opportunities for rent extraction, or provoke backlash where gender norms are restrictive (Aizer, 2010; Anderberg et al., 2016; Bhalotra et al., 2021; Erten and Keskin, 2021; Adams et al., 2024; Bhalotra et al., 2025; Kotsadam and Villanger, 2025; Shah et al., 2025). Research has also shown that IPV can respond to men’s incentives and shocks as well as women’s income, bargaining position, and ability to exit (Bloch and Rao, 2002; Angelucci, 2008; Cesur and Sabia, 2016; Díaz and Saldarriaga, 2023; Shah et al., 2025), showing the critical role in abusive relationships of economic suppression since the start of cohabitation (Adams et al., 2024) and reaching mixed conclusions on the link between a woman’s bargaining position and IPV, which appears negative in some studies (Aizer, 2010; Hidrobo and Fernald, 2013; Bobonis et al., 2013; Anderberg et al., 2016; Hidrobo et al., 2016; Buller et al., 2018; Haushofer et al., 2019) and positive in others (Alonso-Borrego and Carrasco, 2017; Erten and Keskin, 2018; Bhalotra et al., 2025).⁴ We instead study how one legal reform may change both the expected cost of violence and the value of state protection. Linked records let us place changes in population victimization beside victims’ subsequent choice between protection and prosecution. This does not separately identify the two causal channels, but it reveals an equilibrium response that is missed when data contain only victim reports, offender sanctions, or household outcomes.

Third, the paper also addresses a central measurement problem in IPV research (Shah and Barski, 2026). Household surveys may be affected by nonclassical disclosure error, with estimates varying across direct questions, list experiments, interview modes, and even interview timing. Yet administrative data are also selected: police, court, helpline, and hospital records reflect both violence and victims’ decisions to seek help, which respond to media salience, institutional accessibility, and perceived protection (Lépine et al., 2020; Agüero and Frisanchi, 2022; Cullen, 2023; Theiss, 2026; Bergvall, 2024; Colagrossi et al., 2023; García-Hombrados et al., 2024). Our linked data do not eliminate either problem, but they let us triangulate police outcomes with

⁴Other studies also show that men’s abusive behaviour increases during recessions (Schneider et al., 2016), and following unexpected losses or alcohol consumption in sporting events (Card and Dahl, 2011; Ivandić et al., 2024).

hospitalization and imprisonment and distinguish recorded incidence from victims’ conditional protection and prosecution choices.

The rest of the paper is structured as follows. Section 2 discusses a simple theoretical framework to help organize the evidence. Section 3 presents the data and the setting, while Section 4 illustrates our empirical approach. Section 5 discusses our results. Section 6 presents an array of robustness checks, while Section 7 examines the mechanisms at play. Section 8 concludes.

2 Theoretical framework

Consider a relationship in which the perpetrator chooses a level of violence and the victim chooses whether to seek state protection. Violence may provide direct gratification or serve instrumentally to control the victim’s behaviour (Tauchen et al., 1991; Adams et al., 2024), but it carries an expected cost. That cost depends on the expected legal penalty, the probability that violence is reported or detected, and the probability that the state imposes restrictions through protective measures. The victim’s decision depends on the expected protection from the state, the risk of retaliation, the social and psychological cost of involving the police, and the private cost of prosecution.

Brazil’s Femicide Law, which is empirically studied in this paper, changes both sides of this problem. On the perpetrator side, the law may raise the perceived expected cost of escalation to lethal violence. Even if threats, fighting, and bodily injuries are not the statutory object of the reform, they may form part of the same escalation dynamic. A partner who anticipates severe punishment following escalation may therefore reduce violence before it approaches the lethal margin. This behaviour would be consistent with a standard deterrence channel (Becker, 1968). More generally, evidence from sentence enhancements shows that criminal sanctions can generate an immediate deterrent response before any additional incapacitation occurs (Kessler and Levitt, 1999).⁵

On the victim side, the law can change beliefs about the state. By defining femicide as a distinct and severe crime, the state may signal that violence against women is not a private matter or a crime of passion. Economic theories of expressive law show how legal rules can shift social norms by communicating social values, while experimental evidence demonstrates that laws can causally alter perceived norms (Cooter, 1998; Lane et al., 2023). If legal recognition

⁵This channel requires potential offenders to know about the legal change. Recent evidence from France suggests that first-hand exposure to a sentencing reform can change subsequent offending even when publicity about the reform alone has little effect (Philippe, 2024).

increases the perceived legitimacy or responsiveness of the state, it can raise the expected return to asking for protection. Consistent with this logic, changes in institutional representation and accessibility can increase recorded violence by inducing greater reporting rather than greater underlying incidence (Iyer et al., 2012; Miller and Segal, 2019; Colagrossi et al., 2023; García-Hombrados et al., 2024). If protective measures become more salient or more credible, victims may seek them earlier. Earlier protection then feeds back into offender behaviour by raising the expected cost of future violence. More generally, economic models of law enforcement emphasize that formal sanctions and private reporting are jointly determined, because the effectiveness of enforcement depends partly on individuals’ willingness to report violations (Acemoglu and Jackson, 2017). In this sense, demand for state protection is not an abstract change in preferences. It is a change in the victim’s feasible set as protection becomes a more valuable action.

The framework also explains why protective measures and charges need not move concurrently. Pressing charges may impose private costs on victims who share income, children, housing, or kinship networks with the offender, particularly when legal action threatens household resources or the continuation of the relationship (Bowlus and Seitz, 2006; Aizer and Dal Bó, 2009; Iyengar, 2009). Protective measures can be attractive for marginal victims because they seek safety without the same expected litigation and incarceration costs. A reform can therefore reduce violence by increasing the use of protection even if it does not increase prosecution. This mechanism is close in spirit to models in which services and legal commitment devices affect violent relationships (Farmer and Tiefenthaler, 1997; Aizer and Dal Bó, 2009), but the key state action here is the credibility of protection after a criminal law reform.

The model does not imply that requests for protection mediate the contemporaneous decline in recorded violence. These choices are inherently dynamic: current abuse can reveal information about the partner’s type and affect subsequent employment, exit, and exposure to violence (Bowlus and Seitz, 2006; Anderberg et al., 2023; Adams et al., 2024). Rather, the reform may jointly change perpetrators’ expectations and victims’ valuation of existing remedies. A request observed after an incident can affect subsequent recurrence, while the anticipated availability of protection may influence behaviour before an incident. The empirical analysis therefore treats deterrence, salience, and protection credibility as competing interpretations and uses the timing and composition of responses to assess them.

The model yields three empirical implications. First, under the assumption that the reform changes perceived legal costs or the credibility of protection more strongly where baseline exposure to violent men is greater, recorded violence should decline more in those locations. Second, if incapacitation is the main channel, effects should be stronger for repeat cases and should co-

incide with longer or more frequent imprisonment. Third, if victim demand for state protection is central, victims should request protection more often and earlier. The evidence is consistent with the first and third predictions and does not support the second. Overall, this pattern is consistent with deterrence operating partly through victims’ increased willingness to use state protection.

3 Data & Setting

3.1 A Lei do Feminicídio (Lei No 13.104/2015)

This study focuses on Rio Grande do Sul (RGS), Brazil’s southernmost state. Its capital, Porto Alegre, is the largest urban center in the state. With nearly 11 million inhabitants, about 5% of Brazil’s population in 2022, RGS is the country’s fifth most populous state. Up until 2015, “femicide” was not explicitly included in the Brazilian Penal Code. Depending on the circumstances and discretion of the agents involved (victims, offenders, lawyers, prosecutors, judges), the homicide of an intimate partner could be treated either as a simple homicide, for which the Penal Code of Brazil provides for a prison sentence of 6 to 20 years, or as a qualified homicide, for which it provides for a prison sentence of 12 to 30 years. The variability in legal treatment and sanctioning consequences in the context of such grave events had, for several years, sparked a serious debate around the suitability of the existing legislation in serving as an effective mechanism of protection for the victim and punishment for the offender.⁶

Legislation aimed at explicitly protecting victims of domestic violence in Brazil had first been introduced in 2006 with *Lei Maria da Penha* (see [Ferraz and Schiavon \(2022\)](#)). The 2006 reform stated that, regardless of whether the victim or someone else contacted the police in relation to a Domestic Violence (DV) incident, every victim of DV in Brazil would be eligible for urgent protective measures starting from the date of the incident. Such measures include restraining orders prohibiting contact or proximity between the aggressor and the victim, and the removal of the aggressor from the shared household.⁷ Furthermore, the *Lei Maria da Penha* also advocated

⁶Although the Maria da Penha Law (2006) represented an important strengthening of the legal framework around domestic violence, concerns remained over whether the existing legislation was sufficiently effective in dealing with the most severe forms of violence against women, particularly killings in domestic and gender-based contexts. Over time, questions around inconsistent legal classification, uneven punishment, and broader institutional failures became increasingly central to public and legislative debate. The Parliamentary Commission of Inquiry on Violence Against Women ([Brasil. Senado Federal, 2013](#)), for instance, documented significant shortcomings in prevention, protection, and enforcement, helping shape the policy environment that ultimately led to *Lei 13.104/2015*, which formally recognised femicide as a qualified form of homicide.

⁷Courts may also prohibit the aggressor from visiting specific locations, suspend firearm possession, and establish temporary custody or financial support arrangements

for the creation of specialized police stations for women’s assistance, shelters, women’s reference centres, and courts for domestic and family violence against women. However, several of these resources remained out of reach to a significant part of the population - as of 2018, 8.3% of the municipalities in Brazil had specialized police stations, and 2.3% had women’s shelters.⁸

Against the backdrop of dissatisfaction with the existing legal framework in dealing with severe domestic violence cases and the persistence of high levels of the phenomenon, the Femicide Law (*Lei No. 13.104/2015*) was enacted in March 2015 with a clear link to the *Lei Maria da Penha*. At the core of the reform was the explicit inclusion for the first time in the Penal Code of Brazil of “femicide” as a new crime category. The reform defined “femicide” as a qualified homicide and it placed it on the list of heinous crimes, with penalties ranging from 12 to 30 years. Its goal was to remove any ambiguity on where the homicide of an intimate partner would fall within the Brazilian Penal Code, eliminating the scope for an interpretation of femicide as an “honor” or “passion” crime. More explicitly, the reform defines as “femicide” any attempted or consummated murder that involves domestic and family violence, as well as disregard or discrimination against the victim’s status as a woman.⁹

Before 2015, some intimate-partner killings could already be prosecuted as qualified homicide and therefore face the same statutory 12–30 year range. The reform’s central legal change was to remove ambiguity by explicitly defining femicide as a qualified homicide and heinous crime. We therefore interpret the reform as changing expected punishment, legal salience, and state credibility jointly, rather than as providing a clean first stage on sentence length alone.

The purpose of the reform was not just to prevent homicides of intimate partners but also, more broadly, to prevent the escalation of violence in domestic settings (i.e., whether fatal or non-fatal violence). In the words of supervising judge Taís Culau de Barros, responsible for the State Coordination Office for Women in Situations of Domestic and Family Violence (Cevide) of the Court of Justice of the State of Rio Grande do Sul (TJRS),

“Some forms of violence against women are still normalized in our society, and this leads to silence. That is why it is so important to talk about these crimes, so that girls and women, at the first sign of violence, can recognize the wrongful behaviour of their aggressors and leave relationships before it is too late. The visibility that the Femicide Law has brought to femicide crimes is very important. [...] Drawing attention to the crime helps women find the courage to report the violence they suffer and to know that they will have support.”¹⁰

⁸IBGE, Pesquisa de Informacoes Basicas Municipais.

⁹The penalty for femicide is further increased from 33% to 50% if the crime is committed during pregnancy or in the 3 months following birth; against a person under 14 years of age, over 60 years of age, or with a disability; or in the presence of a descendant or ascendant of the victim.

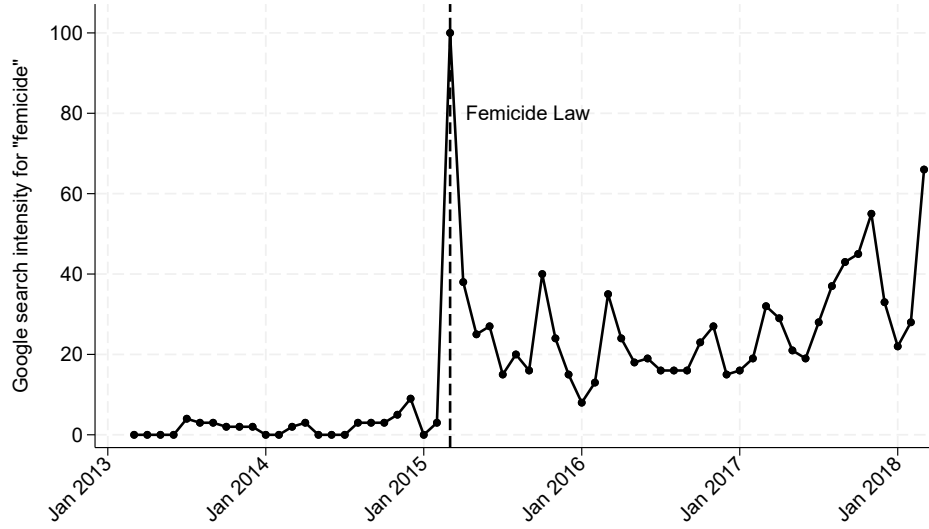
¹⁰Her full interview is available here (last accessed on 04 May 2025): [Brasil de Fato, April 1, 2025](#).

At the time of its implementation in March 2015, the law became known to the public through extensive news coverage, culminating in a spike in public interest in the law. Figure 1 shows the Google search intensity of the “femicide” keyword in Brazil between 2013 and 2019.¹¹ Reflecting both the sharp public salience at enactment and the lack of widespread prior knowledge of the law upon its introduction, a spike in searches for “femicide” appears exactly in March 2015, the date of its enactment, from a prior baseline of close to zero searches. The legislative framework introduced by the *Lei Maria da Penha* remained unchanged in March 2015 despite minor amendments in the following years, none of which related to the legal recognition of femicide.¹²

¹¹Figure B1 in Appendix B additionally shows the Google search intensity also for the “Domestic Violence” and “Maria da Penha Law” keywords in Brazil for the years 2004 to 2024. Interestingly, the *Lei Maria da Penha* shows a similar pattern in search surge close to its implementation date from a very low prior level; moreover, the search intensity for the general term “Domestic Violence” shows significant time-series correlation with both laws, suggesting an intrinsic link between the social phenomenon and the substance of the enacted laws. Additionally, Figure B1 does not show a particular rise in searches for the *Lei Maria da Penha* around the introduction of the *Lei do Feminicídio*, arguably mitigating the concern that the older law might act as a potential policy confounder during the period of our analysis.

¹²The 2015 reform was followed by three later amendments of relevance during our study period: *Law 13.505* of November 2017, which strengthened rights to specialized, continuous police and forensic service for women experiencing domestic violence, *Law 13.641* of April 2018, which created a separate crime for breaching an urgent protective measure, punishable by three months to two years, and *Law 13.827* of May 2019, which allowed judicial or police authorities to grant urgent protective measures in specified circumstances and required a national protective-measure database. Only in October 2024, well beyond the end of our study period, *Law 14.994* introduced another revision to the *Lei do Feminicídio*, further raising penalties and turning femicide into an autonomous offence (Article 121-A) with increased penalties from 12–30 years to 20–40 years. Sections 5, 6 and 7 present event-study estimates that show significant effects of the 2015 reform before any of these other legislative changes occurred and that none of the main results are driven by the 2018–2019 period.

Figure 1: Google search intensity for the term “femicide” in Brazil (2013-2018)



Notes: The figure plots monthly Google Trends search intensity for the term “femicide” between March 2013 and March 2018. Search intensity is normalized by Google Trends to range from 0 to 100 within the selected period, where 100 corresponds to the peak search volume. The dashed vertical line marks March 2015, when Brazil’s Femicide Law was enacted.

3.2 Data and Descriptive Analysis

The empirical analysis of this reform uses administrative registers linked at the individual level from 2010 to 2019 on family linkages and domestic violence cases recorded by the police for the universe of individuals that ever resided in RGS between 2010 and 2019. As such, our analysis utilizes administrative records on the entire population of the state. These data were obtained via a data-sharing agreement with the Governor of RGS and several public ministries of RGS and public sector organizations. To be precise, administrative records of family linkages of all individuals come from the Instituto Geral de Pericias (*IGP*), i.e., the identity public registry, police records originate from the police authorities (*Secretaria de Seguranca Publica - Policia Civil*) and prison records are provided by the prison authorities (SUSEPE/Polícia Penal).

Our administrative dataset includes 8,803,274 individuals born in RGS after 1920 and, if dead, dead after 2010. It contains information on gender, ethnicity, date of birth and death, nationality, schooling history, and civil status. Of these individuals, 10.42% are black, 50.42%

are females, 29.94% completed compulsory education or above, and 24.89% are married.¹³ The police records include 15,594,108 incidents reported to the police between 2010 and 2020.¹⁴ For each incident, the data contain information on the type of participation, the date of the event and its registration, the outcome of the crime, whether protective measures were applicable, requested, and whether charges were pressed.

Information about whether protective measures were applicable is used to identify Domestic Violence (DV) cases. In Brazil, as in many other settings, “DV” is not recorded as a distinguishable crime category from others. However, since urgent protective measures are available only to victims of DV and they can be identified in our data, these enable us to identify DV cases in the police records. In Rio Grande do Sul, anyone can call the police to report a DV incident. However, an eligibility marker is registered only if the victim is identified and brought to the police station to testify about the occurrence, as this is the only way the police can offer protection and forward the case for court proceedings.

After the domestic violence incident is reported, the victim has two legally distinct choices to make. The first is whether to formally pursue legal action against her abusive partner: while some domestic violence offences during our period of analysis could legally proceed without formal victim representation (for example, bodily injury cases), victim engagement remained central in practice, as reporting, cooperation, and sustained participation often shaped whether cases effectively entered and progressed through the criminal justice system. This was particularly relevant because offences such as threats and minor assault, which account for about 60% of DV cases in our data, generally could not proceed without the victim formally initiating or sustaining the complaint. Secondly, a DV victim needs to choose whether to request protection from the police. In RGS since 2006, if a DV victim requests protective measures, the police officer has to bring the case to court within 48 hours. Upon receiving the request, the judge in charge has a further 48 hours to decide on the protective measures to be applied.¹⁵

The focus of our empirical analysis is on female victims: for the period between March 2010 and February 2020, our data contain 3,393,522 unique occurrences with at least one female victim, 27% of which were identified as DV. Figure 2 shows that, during our study period, the incidence of DV remained fairly constant in RGS while the incidence and share of DV cases where protective measures were requested increased over time. Figures C2, C3A. and C3B. show this even more clearly, as they show that the count and share of DV cases where the victim requested protective measures and omitted to press charges increased over time, while

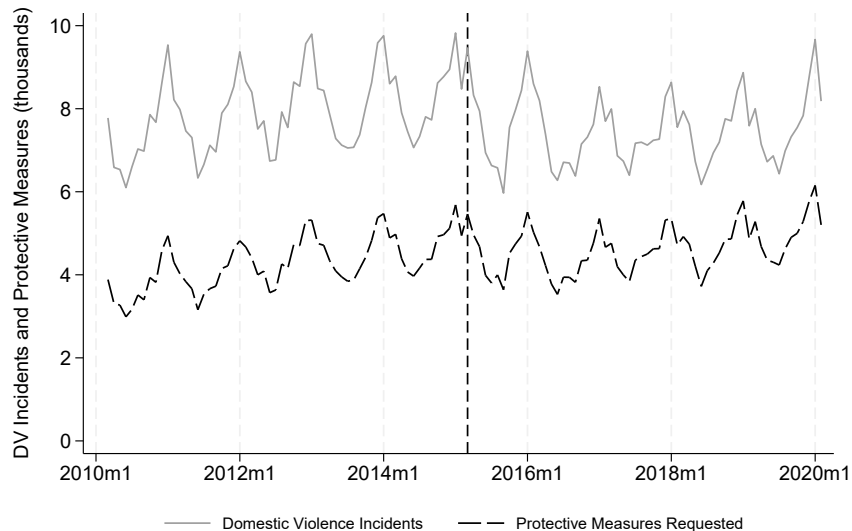
¹³Schooling history and civil status are missing for 26% and 0.5%, respectively.

¹⁴To avoid COVID-19, we use the period up to February 2020.

¹⁵In cases of immediate danger to life, the police can put in place preventive measures and make an urgent request to the court for provisional measures.

the cases in which the victim pressed charges without requesting protection became increasingly rare after 2015.¹⁶ All figures display clear seasonality, indicating that the incidence of DV cases peaks in December and January, i.e., during and after the Christmas break when families spend more time together and off from work. Figure C1 displays the age profiles of female victims and male offenders aged 18 or older, showing that female victims of DV tend to be younger than female victims of any crime, while male offenders are on average older in cases of DV compared to non-DV offences.

Figure 2: Trends in Domestic Violence Incidents and Protective Measures



Notes The figure displays the monthly counts of total DV incidents in Rio Grande do Sul between 2010 and 2020, as well as monthly counts of incidents for which protective measures were requested.

For our empirical analysis, we derived from the raw administrative data a panel dataset for the period 2012-2020 that includes all women aged 18-55 to investigate whether they became victims of DV in a given year. We focus on the age range 18-55 firstly because this sample represents the overwhelming majority of DV victims (i.e., as illustrated in Figure C1), and secondly because we want to avoid capturing family violence against minors. We further restrict

¹⁶In particular, Figure C2 shows that the count of cases for which the victim both pressed charges and requested protection did not really change over time; likewise, the count of cases for which the victim neither pressed charges nor requested protection remained quite stable too. In contrast, the count (and share) of DV cases where the victim requested protective measures and omitted to press charges increased, while the cases in which the victim pressed charges and did not request protection decreased. Figures C3A. and C3B. also display the monthly counts of DV incidents distinguishing between those for which protective measures were requested and those for which charges were pressed, respectively. They show that DV victims requested protective measures in a growing share of cases, while the share of DV cases where the victim pressed charges was relatively stable in the first five years of our study period and it started to decrease from 2015.

the end of our analysis to February 2020 in order not to capture the disruption caused by the COVID pandemic which significantly influenced the dynamics of DV in terms of its nature, catalysts, and reporting.¹⁷ As a result, the final sample is composed of 2,557,114 women and 17,991,919 woman-period observations.¹⁸ At the incident level, we considered only domestic violence incidents and our analysis includes 427,835 occurrence-victim observations of domestic violence, as we consider incidents with up to 10 victims. The number of domestic violence incidents is 420,897. The majority of cases have only one victim (414,338, i.e. 98.44%), 6,232 (1.48%) occurrences have two victims, and the remaining fraction of approximately 0.08% of occurrences have three or more victims.

Overall, our analysis includes 257,846 unique female DV victims. Table C1 reports descriptive statistics for the entire sample of 2,557,114 women included in the econometric analysis. The Table indicates that, on average, 0.77% of males appeared in the police records for violent crimes against females in 2010, 1.98% of women become victim of DV and 5.03% become victim of other violent crimes in a given year in our study period. On average, femicides involved a very small 0.01% of women in our analysis, while 0.57% of women experienced bodily injuries, 0.95% experienced serious threats, 0.23% became victims of fights and 0.16% of disorderly conduct. Reflecting how widespread DV is in RGS, fewer males, i.e., 0.52% of males, appeared in the police records for violent crimes against males in 2010. Table C2 shows that, among RGS municipalities, approximately 7% of the population is black, roughly 68% of the working age population is employed, 17% of the population is young, i.e., 15-24, and nearly 3% of the local population live under the poverty line.

Table C3 complements this descriptive analysis by displaying descriptive statistics for the DV occurrences studied in this paper. As we allow for multiple victims, the unit of observation here is the occurrence-victim. Table C3 shows that, on average, DV victims pressed charges in almost 70% of the cases and requested protective measures in 60.65% of the cases. While charges and protective measures are both requested in approximately 51% of the cases, in 20.83% of the cases do victims not request either. In 8.28% of cases in our sample, the victim ended up severely injured, hospitalized or dead.¹⁹

¹⁷Overall, the majority of research indicates an increase in domestic violence during the Covid-19 pandemic, although some studies report mixed or contradictory results (Arenas Arroyo et al., 2020; Béland et al., 2020; Campedelli et al., 2020; Miller et al., 2020; Payne et al., 2020; Leslie and Wilson, 2020; Silverio-Murillo et al., 2020; Bullinger et al., 2021; McCrary and Sanga, 2021; Ravindran and Shah, 2023).

¹⁸Of the 2,557,114 women in the panel, 256,398 appear as victims of DV in at least one period. The slightly larger figure of 257,846 unique female DV victims reported below corresponds to the occurrence-victim sample, which applies the age restriction at the time of the incident rather than period-by-period and is therefore not exactly aligned with the panel definition.

¹⁹Details of data sources and sample construction can be found in Appendix A.

4 Econometric Modelling

4.1 Treatment Exposure: Male violence against females

The goal of our empirical analysis is to quantify the impact of the *Lei do Feminicídio* on domestic violence (DV) in Rio Grande do Sul (RGS). Because the legislative framework introduced in March 2015 by the *Lei do Feminicídio* applied to all the citizens of Brazil, no pure control group exists for this reform, either in RGS or in other Brazilian states - more formally, there are no never treated units. To quantify the causal impact of the reform on the dynamics of domestic violence, we therefore define a continuous treatment variable based on the local, predetermined exposure of women in RGS to men with a history of violence against women. This measure can be interpreted as a “dosage” of treatment (Callaway et al., forthcoming) - similar designs have been used in settings with national adoption such as national minimum wage (Haanwinckel (2025)) or arrival of vaccines and medical treatments (Bhalotra et al., 2026).

Our empirical design implements a Difference-in-Differences (DiD) framework that exploits cross-sectional variation in our exposure measure across the 226 municipalities of RGS prior to the policy change.²⁰ For woman i , treatment exposure is defined as the share of men in her municipality of birth m who appear in police records as offenders in a violent crime against a woman in the baseline period, March 2010 to March 2011.²¹ Formally, for municipality m , we define:

$$EXP_{m(i)}^f = \frac{VM_m^{2010}}{M_m^{2010}}$$

where VM_m^{2010} denotes the number of men aged 18–55 who committed at least one violent crime against a woman between March 2010 and March 2011 in municipality m , and M_m^{2010} is the total number of men aged 18–55 in the same municipality and period. This measure captures the *ex ante* local supply of men who may perpetrate violence against women, and therefore the baseline intensity of exposure to the margin directly targeted by the *Lei do Feminicídio*. Defining exposure as the share of men captures the fact that the reform did not change the population

²⁰Although RGS comprises 497 municipalities, many are very small in population terms, and including them would introduce substantial measurement error. Reassuringly, robustness tests show that our findings are robust to using the full set of municipalities.

²¹We define treatment exposure using a fixed baseline measure from 2010/11, rather than averaging exposure over the immediate pre-reform period (March 2012 to March 2015), to avoid incorporating transitory local shocks, measurement error, or differential pre-trends into the treatment measure itself. Using an earlier baseline helps isolate predetermined cross-sectional variation in exposure and reduces concerns that short-run fluctuations mechanically shape treatment intensity, in line with the discussion in Haanwinckel (2025).

at risk, but rather increased the salience of violent behaviour against women and the expected sanction associated with it. As such, its impact should be strongest in environments where interactions with potentially violent offenders are more frequent. Our exposure measure thus provides a proxy for the intensity with which women are affected by the reform, by capturing pre-existing local variation in the prevalence of male violence against women.

We construct this measure using the municipality of birth rather than current municipality of residence in order to mitigate concerns related to endogenous migration.²² If migration responds to local conditions, women may selectively move towards areas with lower levels of violence against women. In that case, defining exposure at the municipality of residence would tend to attenuate measured risk for those more likely to relocate, potentially biasing estimates upward by overstating relative improvements in lower-exposure areas. By anchoring exposure to the municipality of birth, we instead rely on a predetermined measure that is not mechanically affected by endogenous mobility decisions. In Section 6, we compare results using exposure defined at both the municipality of birth and the municipality of residence.

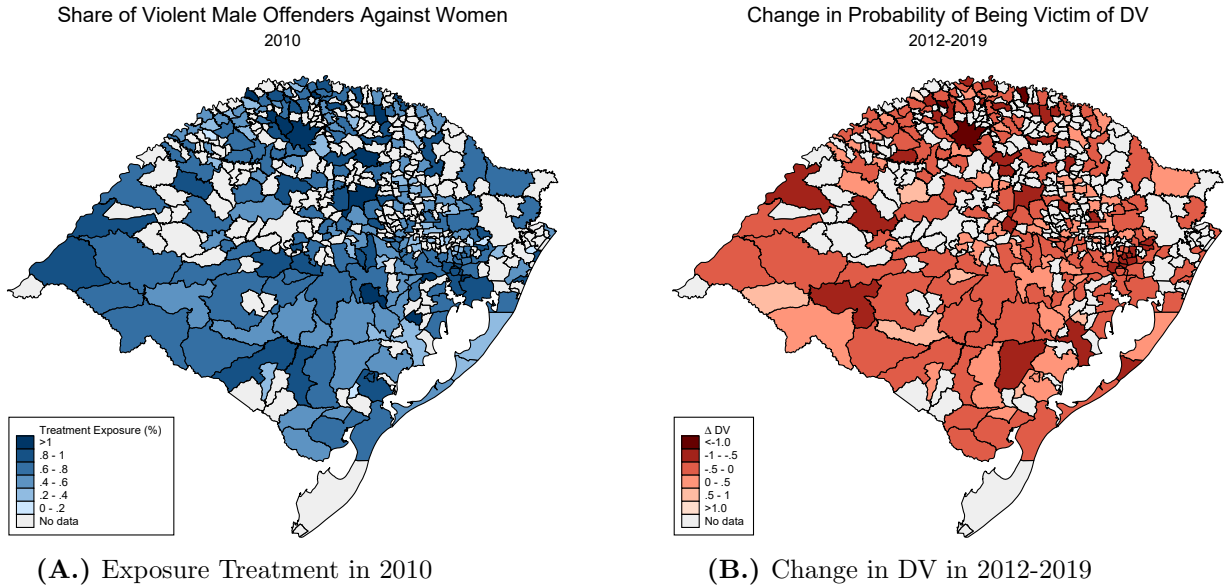
Figure 3 shows two maps of municipalities of RGS. In particular, Figure 3A. illustrates the cross-sectional variation in the treatment measure ($EXP_{m(i)}^f$) and it displays significant dispersion of the treatment exposure across areas. While a woman born in the municipality corresponding to the 25th percentile of treatment exposure (Sant’Ana do Livramento) was, on average, at risk of encountering 6.66 violent men per thousand in 2010, a woman born in Porto Alegre (75th percentile of treatment exposure) was exposed to an average of 8.78 violent men per thousand. This treatment measure is fixed at the 2010 baseline and is therefore not affected by subsequent migration decisions. Figure 3B. shows the change in domestic violence victimisation rate across the municipalities of RGS between 2012 and 2019. A visual inspection of the two maps arguably suggests a negative correlation between treatment exposure across areas and the relative reduction in DV cases observed from 2012-2019. While, in Figure 3, municipalities that were too small to properly calculate our treatment measure in 2010 are left blank, the same conclusion emerges from the visual inspection of Figure B2 in Appendix B that shows the same two maps of municipalities of RGS including all the municipalities in the analysis.

The scatter plots in Figure 4 display the change in DV victimisation against the treatment exposure ($EXP_{m(i)}^f$) separately for the *pre*- and *post*-reform periods, thus providing a more transparent view of the underlying variation in DV across localities and over time. Figure 4A. shows no discernible correlation in the pre-reform period (2012–2014) between exposure to treatment and the change in DV victimisation over time, suggesting that areas with different

²²Nonetheless, more than half of our sample continues to reside in their municipality of birth.

levels of exposure followed similar trends prior to the policy change. In contrast, Figure 4B. reveals a negative and significant association between exposure to treatment and changes in DV victimisation in the post-reform period (March 2015–2019), with higher-exposure municipalities experiencing larger relative declines.

Figure 3: Share of Violent Male Offenders against Women in 2010 and Change in Risk of Domestic Violence in 2012-2019

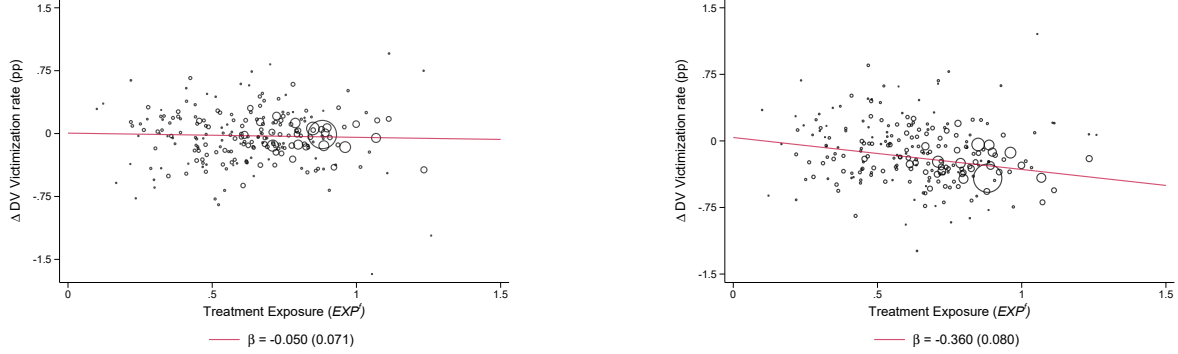


Notes: The map on the left displays the spatial variation in the share of males aged 18-55 with a record of violence against a female victim in the baseline period (March 2010 - February 2011) across the 226 municipalities used in the empirical approach. The map on the right shows the spatial variation in the change in the probability of female victimisation from domestic violence between 2012 and 2019 across the same set of municipalities. Corresponding figures including all municipalities in Rio Grande do Sul are presented in (Appendix B).

4.2 Differences-in-Differences Model and Identification

Building on these descriptive findings, in the econometric analysis we exploit the richness of our data and define a set of Difference-in-Differences (DiD) specifications at the individual level. This approach allows us to control not only for baseline characteristics of the treatment units (municipalities), but also for a detailed set of individual attributes that may influence the probability of a woman becoming victim of domestic violence (DV), beyond the variation captured by treatment exposure. The following equation can be estimated to retrieve the causal

Figure 4: Treatment Exposure vs Change in Risk of Domestic Violence Before and After the *Lei do Feminicídio*



(A.) Before the *Lei do Feminicídio*

(B.) After the *Lei do Feminicídio*

Notes: The left panel plots municipality-level changes in the female domestic violence victimisation rate between 2012 and 2014 (pre-reform period) against baseline treatment exposure, defined as the 2010 share of males with a record of violence against a female victim. The right panel presents the corresponding relationship for the post-reform period, comparing changes between 2015 and 2019. Each observation represents a municipality and is weighted by the municipality cell size in 2010. The fitted lines show the linear association between treatment exposure and changes in domestic violence victimisation.

impact of the reform on our outcomes of interest:

$$Y_{it} = \alpha + \beta_1 Post_t + \beta_2 EXP_{m(i)}^f + \beta_3 (Post_t \times EXP_{m(i)}^f) + \theta_t X_{imt} + \delta_{im} + \omega_{icmt} \quad (1)$$

where Y_{it} is the outcome of interest, $Post_t$ is a dummy for the period after March 2015, $EXP_{m(i)}^f$ is the exposure measure defined above, X_{imt} are a set of time invariant individual characteristics and municipality level controls measured at baseline, δ_{im} indicates individual or municipality fixed effects (depending on the specification) and ω_{icmt} is the error term for woman i , born in municipality m , in period t . Since the treatment is defined at the municipality level, we cluster the standard errors for inference at this geographical level throughout the empirical analysis. Following [Wooldridge \(2021\)](#), we allow baseline municipality-level socio-economic and demographic controls in X_{imt} to have time-varying effects by interacting them with period indicators. This flexible specification helps relax the unconditional parallel trends assumption by allowing for differential trends across municipalities with different baseline characteristics.

Identification of DiD models with continuous treatment as the one outlined in Equation (1) has received renewed attention by [Callaway et al. \(forthcoming\)](#) and [de Chaisemartin et al.](#)

(2026). Under the standard assumptions of no anticipation effects, parallel trends, and unconfoundedness, the coefficient β_3 captures how outcomes respond to changes in exposure to the reform. However, in contrast to the binary treatment case, β_3 does not, in general, recover a single average causal effect. In settings where treatment effects vary across levels of exposure (heterogeneous effects), the estimated coefficient reflects a weighted average of local causal responses and the weights are determined by the variation in exposure in the data. These weights, when estimating Equation (1) via two-way fixed effects (TWFE) regressions, do not in general coincide with simple exposure shares and may place disproportionate weight on particular segments of the exposure distribution (Callaway et al. (forthcoming), de Chaisemartin et al. (2026)). In the presence of heterogeneous treatment effects, this can lead the estimate to differ from the underlying average causal response.

Interpreting β_3 as an average causal response (ACR) therefore requires a stronger condition, namely that the marginal effect of exposure is similar across municipalities, regardless of their initial level of exposure - the “strong parallel trends” assumption in Callaway et al. (forthcoming).²³ Under this assumption, differences in outcome changes across exposure levels can be interpreted as tracing out a common dose-response relationship to the reform.

In order to assess the plausibility of this stronger assumption in our setting, we draw directly on recent work that highlights testable conditions supporting its validity and the use of a linear TWFE specification. In particular, we explore a set of diagnostic tests that address the main sources of concern in this type of modelling approach. First, we study how pre- and post-treatment effects evolve across the distribution of exposure to provide support for the strong parallel trends assumption. Second, we explicitly test whether post-treatment effects are approximately linear with respect to treatment exposure levels and that evolution of outcomes in pre-periods is mean independent of treatment exposure, following de Chaisemartin et al. (2026). Third, we evaluate the sensitivity of our estimates to very low levels of exposure, which may effectively act as control units in our setting. This speaks directly to the existence of valid quasi-untreated units, as discussed by de Chaisemartin et al. (2026) and the support conditions required for identification. Lastly, we produce estimates using a binary version of the treatment, namely above- and below-median exposure. This provides a more transparent benchmark that does not rely on the same continuous treatment structure and allows us to assess the robustness of our results in a simpler setting. These diagnostics provide a transparent way of assessing whether the conditions under which a linear specification as in Equation (1) yields a meaningful average causal response effect are likely to be approximately satisfied in our setting. We present

²³de Chaisemartin et al. (2026) show that, under this type of assumption, linear DiD specifications are most naturally interpreted when outcome changes are approximately linear in the treatment dose and display no systematic relationship with treatment exposure in the pre-reform period.

the corresponding evidence in Section 5.1.1.

5 Results

5.1 Incidence of Domestic Violence (DV)

Table 1 presents our main results on the impact of the *Lei do Feminicídio* on the probability of becoming victim of domestic violence (DV) among women in Rio Grande do Sul (RGS). Column (1) reports estimates from the baseline specification, which includes municipality of birth fixed effects. Column (2) extends this specification by adding a set of municipality-level socio-economic and demographic controls, measured in 2010 and interacted with period fixed effects. These controls include median income, the share of employed individuals, education levels, the share of young individuals, the poverty rate, and an urbanisation index based on the share of the population living in urban areas. Interacting these predetermined characteristics with period effects allows for differential trends across municipalities with different baseline conditions, helping to account for time-varying factors that may be correlated with both exposure and DV outcomes. Column (3) presents estimates analogous to Column (1), but replaces municipality of birth fixed effects with individual fixed effects, thereby controlling for time-invariant individual heterogeneity. Finally, Column (4) augments this specification by adding the same set of municipality-level controls interacted with period fixed effects as in Column (2), while maintaining individual fixed effects. In all specifications, we include a quadratic function of age and fixed effects for race, year of birth and period.

Regardless of the set of controls and fixed effects included in the regression, the estimated effect of treatment exposure on the probability of becoming a DV victim is negative and statistically significant at all conventional levels. The results in Table 1 indicate that more exposed women to violent men witnessed a disproportionate reduction in their risk of DV victimisation following the *Lei do Feminicídio*. In particular, estimates in Table 1 indicate that, after the reform, the incidence of DV fell by a disproportionate 2.4% to 5.4% (depending on the specification) in municipalities at the 75th percentile of the distribution of our treatment exposure *vis-à-vis* municipalities at the 25th percentile of the distribution of our treatment exposure.

Table 1: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence

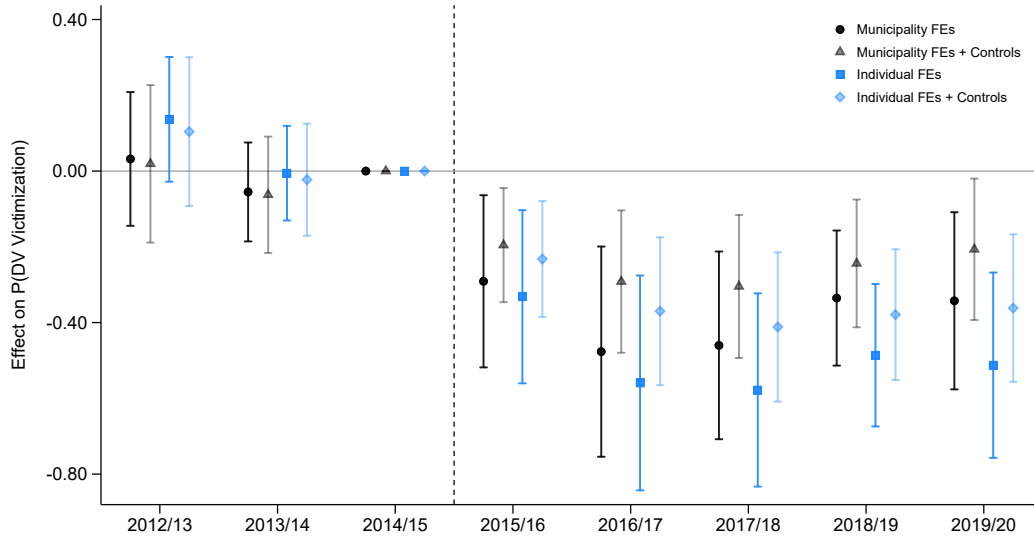
	Probability of DV			
	(1)	(2)	(3)	(4)
Post $\times$ Exposure	-0.384*** (0.095)	-0.241*** (0.054)	-0.532*** (0.099)	-0.372*** (0.056)
Observations	17,991,919	17,991,919	17,991,919	17,991,919
Pre Mean of Outcome	2.119	2.119	2.119	2.119
Range of Exposure	0.215	0.215	0.215	0.215
Effect wrt Mean	3.900	2.443	5.400	3.778
Municipality Count	226	226	226	226
Municipality FE	Yes	Yes	No	No
Individual FE	No	No	Yes	Yes
Municipality Controls	No	Yes	No	Yes

Notes: This table reports estimates of the impact of the *Lei do Feminicídio* on the probability of domestic violence victimisation. The dependent variable is an indicator equal to one if woman i is recorded as a victim of domestic violence in period t . The coefficient of interest corresponds to the interaction between the post-reform indicator and the measure of treatment exposure. Individual-level baseline characteristics (race and birth cohort) and a quadratic in age are included in all specifications. Columns (1)–(2) incorporate municipality of birth fixed effects and Column (2) adds municipality-level controls (median income, share of employed individuals, education index, share of young individuals, poverty rate, and an urbanisation index) measured in 2010 interacted with period indicators. Columns (3)–(4) include individual fixed effects and Column (4) adds the same municipality-level controls as in Column (2) interacted with period indicators. Coefficients and pre-reform means are multiplied by 100 and can therefore be interpreted in percentage points. The pre-reform mean of the outcome is reported for reference. The range of exposure corresponds to the difference between the 25th and 75th percentiles of the treatment distribution. “Effect wrt Mean” reports the implied percentage change in the outcome, relative to the pre-reform mean, from moving across this interquartile range of exposure, computed as the estimated coefficient multiplied by the exposure range and scaled by the pre-reform mean. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

As mentioned in Section 4, the causal interpretation of these results relies on several identifying assumptions. One of those assumptions is that of parallel trends: outcomes in more and less exposed units would have evolved similarly in the absence of the policy intervention. Although the standard parallel trend assumption does not suffice to ensure consistent estimates of the average causal response effects as discussed in Section 4.2, we start by testing the absence of pre-trends as a necessary condition for at least this weaker assumption to be met. Figure 5 displays the event-study analogue to the DiD estimates in Columns (1) to (4), respectively, of Table 1. Reassuringly, estimated coefficients appear numerically small and statistically insignificant prior to the reform, thus providing empirical evidence that more and less exposed municipalities to our treatment followed similar trends prior to the reform. In contrast, starting from March

2015, a disproportionate reduction in the risk of DV appears in more exposed municipalities to our treatment. Estimates are always statistically significant at 5% and they are numerically much larger than those in the pre-period, thus reiterating that the *Lei do Feminicídio* disproportionately reduced the risk of DV in municipalities with higher shares of violent men against women.

Figure 5: Event Study Estimates of Exposure to Violent Men on DV Victimization



Notes: This figure reports event-study estimates and the corresponding 95% confidence intervals of the effect of treatment exposure on the probability of domestic violence victimisation, corresponding to the four specifications presented in Table 1. Each panel displays the dynamic coefficients associated with relative time to the reform, with the year prior to the reform omitted. Coefficients are scaled by 100 (percentage points). Standard errors are clustered at the municipality level.

5.1.1 Assessing Strong Parallel Trends

Since the standard test on parallel trends is useful but not sufficient to ensure the identification of the causal responses of interest and, in particular, to make comparisons across the treatment exposure distribution, we now show a set of diagnostic tests that support the use of the TWFE estimator in our setting.

Firstly, we estimate event-study specifications across bins of the treatment exposure distribu-

tion. If the strong parallel trends assumption holds, we would expect similar pre-trends across exposure bins and a larger post-reform effect in bins with higher levels of exposure. Figure B3 shows the corresponding event-study estimates across deciles of the treatment distribution. Although the estimates are somewhat noisy due to the discretisation of treatment, pre-trends appear broadly similar across deciles, and there is a clear gradient in post-reform effects: individuals in higher-exposure bins experience larger reductions in the probability of DV victimisation.

Secondly, we follow de Chaisemartin et al. (2026) and test non-parametrically whether changes in the outcome, ΔY_{it} , are conditionally independent of treatment exposure, $EXP_{m(i)}^f$, in the pre-reform period.²⁴ Panel A of Table 2 shows that we do not reject the null of mean independence in any of the pre-reform periods, which is consistent with the absence of differential pre-trends across exposure levels. In addition, we test linearity with respect to treatment exposure and report it in Panel B of Table 2. We test whether post-reform changes in outcomes are approximately linear in treatment exposure. We fail to reject linearity at the 5% level in all post-reform periods. One period (2018/2019) shows marginal evidence against linearity at the 10% level, but the overall pattern is consistent with a linear approximation. Taken together, these results provide supportive evidence for the assumption of a common (or approximately homogeneous) causal response to treatment.

²⁴Because treatment varies at the municipality level, while the main specification is estimated at the individual level, we implement these diagnostics using municipality-level residualised outcome changes. We first partial out controls from the individual-level change in the outcome and then average the residuals within municipality-period cells. The tests are conducted separately by period at the municipality level, which reflects the level of treatment variation and avoids relying on procedures that do not accommodate clustering or weights.

Table 2: Diagnostic Tests: Pre- and Post-Reform Periods

Period	p-value	N
<i>Pre-reform</i> (Order 0: Mean independence of ΔY and EXP_m^f)		
2012/13	0.278	226
2013/14	0.909	226
2014/15	0.129	226
<i>Post-reform</i> (Order 1: Linearity in EXP_m^f)		
2015/16	0.108	226
2016/17	0.413	226
2017/18	0.146	226
2018/19	0.087	226
2019/20	0.904	226

Notes: This table reports heteroskedasticity-robust (Yatchew, 1997) tests following the adaptation in (de Chaisemartin et al., 2026). In the pre-reform period, we test whether changes in the outcome are mean independent of treatment exposure (order 0 polynomial). In the post-reform period, we test whether outcome changes are linear in treatment exposure (order 1 polynomial). P-values correspond to the null hypotheses described above. Tests are conducted at the municipality level.

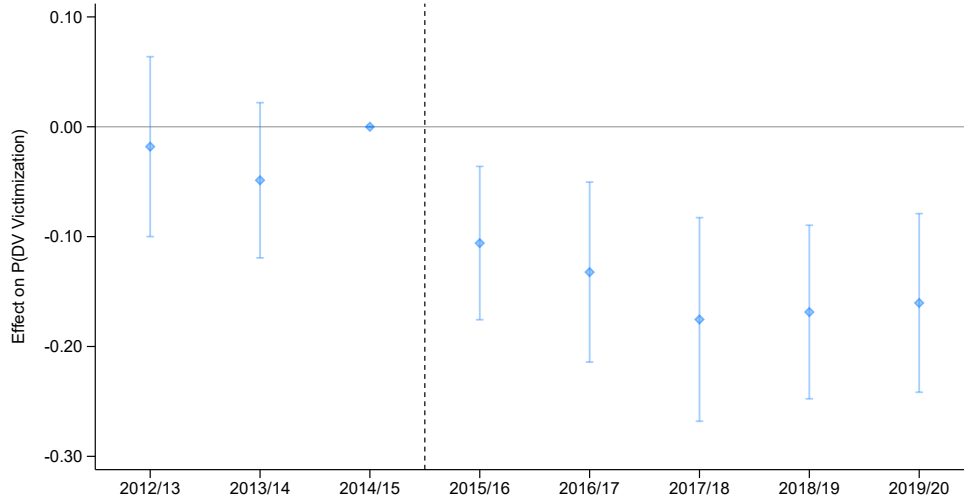
Thirdly, we exclude municipalities with zero exposure, as these may, *de facto*, reflect measurement error and mechanically act as control units in our setting, while accounting for a negligible share of the sample.²⁵ This restriction is also in line with the recent DiD literature with continuous treatments. As emphasised by de Chaisemartin et al. (2026), identification can be sensitive to a small number of quasi-untreated observations that sit at the lower tail of the exposure distribution and effectively act as control across units. In our setting, municipalities with zero exposure may play this role despite being very few in number. Re-estimating Column (4) of Table 1 after excluding these observations yields a coefficient of -0.381, virtually unchanged relative to the baseline.

Lastly, we complement the continuous-treatment specification with a binary version of treatment, defined as above- and below-median exposure. This allows us to estimate an average treatment effect on the treated (ATT) under the standard parallel trends assumption - assum-

²⁵Only three municipalities have zero exposure. These are small municipalities, representing around 0.1% of the estimation sample and 0.1% of individuals in the panel.

ing that DV victimisation in highly treated municipalities would have followed similar trends to those observed in less treated municipalities in the absence of the law. The estimated effect under this specification is consistent with the continuous-treatment results, with a coefficient of -0.124 (0.031).²⁶ In Figure 6, we present the estimates corresponding to the event study with binary treatment which closely tracks the dynamic effects of Figure 5.

Figure 6: Event Study Estimates of Exposure to Violent Men on DV Victimization - Binary Treatment



Notes: This figure reports event-study estimates and the corresponding 95% confidence intervals of the effect of high treatment exposure (above-median municipalities) on the probability of domestic violence victimisation. Each point corresponds to the estimated coefficient for a given relative time period, with the omitted category being the year prior to the reform. The specification corresponds to Column (4) of Table 1. Coefficients are scaled by 100 (percentage points). Standard errors are clustered at the municipality level.

5.1.2 Falsification and Placebo Tests

Despite the absence of differential pre-trends, both in means and across the treatment distribution, and the robustness of our estimates to the inclusion of varying sets of predetermined controls, one may still be concerned that our exposure measure reflects underlying socio-economic factors that are not fully captured. In particular, it may pick up unobserved time-invariant differences across municipalities, such as variation in local infrastructure or service provision for

²⁶This result is presented in Table E2

victims, or time-varying shocks, such as targeted local interventions aimed at reducing violence. While we cannot fully rule out these possibilities, we provide additional evidence suggesting that the estimates in Table 1 and Figure 5 are unlikely to be driven by such confounding factors.

First, we consider other forms of female victimisation, namely violent crimes against women that are not classified as domestic violence, as alternative outcomes in the same DiD framework. This serves as a falsification exercise using a negative control outcome (NCO) in the sense of [Danieli et al. \(2026\)](#). In the absence of spillovers between DV and non-DV crimes, these outcomes should not respond to variation in treatment exposure, and we therefore expect no systematic effect. Second, we assess identification by constructing a placebo exposure that is not expected to affect DV victimisation once we condition on the true treatment exposure.²⁷ Under the identifying assumptions, such variables should be unrelated to the outcome conditional on the true treatment exposure.

Falsification Test. Table 3 shows the results of our falsification test. Panel A of Table 3, which models violent crimes against women that are not classified as DV, presents the results using specifications in columns 2 and 4 from Table 1. If our results reflected some unobserved policy aimed at reducing criminality more broadly, or differential trends in underlying socio-economic conditions across municipalities of RGS, we would expect to find similar effects in this setting. This is not what we find. The estimates in Table 3 are small in magnitude and statistically insignificant, suggesting that the post-2015 decline is specific to DV rather than female victimisation more broadly.

Figure 7A. presents the corresponding event-study estimates for Columns (1) and (2) of Table 3. The estimates are centred around zero throughout and do not display a systematic post-reform pattern. While there is some mild negative correlation between non-DV victimisation and treatment exposure in the pre-reform period, this does not persist after the reform. Taken together, these results suggest that the main findings are not capturing broader changes in crime, but are specific to domestic violence.

²⁷This type of placebo exposure corresponds to a negative control instrument (NCI) in the framework of [Danieli et al. \(2026\)](#)

Table 3: Placebo and Falsification Tests

	(1)	(2)
<i>Panel A: Probability of non-DV</i>		
Post $\times$ Exposure	0.110 (0.168)	-0.004 (0.163)
Observations	17,991,919	17,991,919
Pre Mean of Outcome	5.308	5.308
Range of Exposure	0.215	0.215
Effect wrt Mean	0.447	0.016
<i>Panel B: Probability of DV</i>		
Post $\times$ Exposure	-0.231*** (0.066)	-0.378*** (0.066)
Post $\times$ ExposureM	-0.030 (0.092)	0.014 (0.091)
Observations	17,991,919	17,991,919
Pre Mean of Outcome	2.119	2.119
Range of Exposure	0.215	0.215
Effect wrt Mean	2.342	3.838
Municipality Count	226	226
Municipality FE	Yes	No
Individual FE	No	Yes
Municipality Controls	Yes	Yes

Notes: Panel A reports estimates of the effect of the *Lei do Feminicídio* on the probability of non-domestic violence (non-DV) victimisation, while Panel B reports the corresponding estimates for domestic violence (DV). The dependent variable is an indicator equal to one if woman i is recorded as a victim of the relevant outcome in period t . In Panel B, we additionally include the interaction between the post-reform indicator and the placebo exposure based on violence against men ($Post_t \times EXP_{m(i)}^m$), alongside the baseline exposure to violence against women. Specifications (1) and (2) correspond to Columns (2) and (4) of Table 1, respectively. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

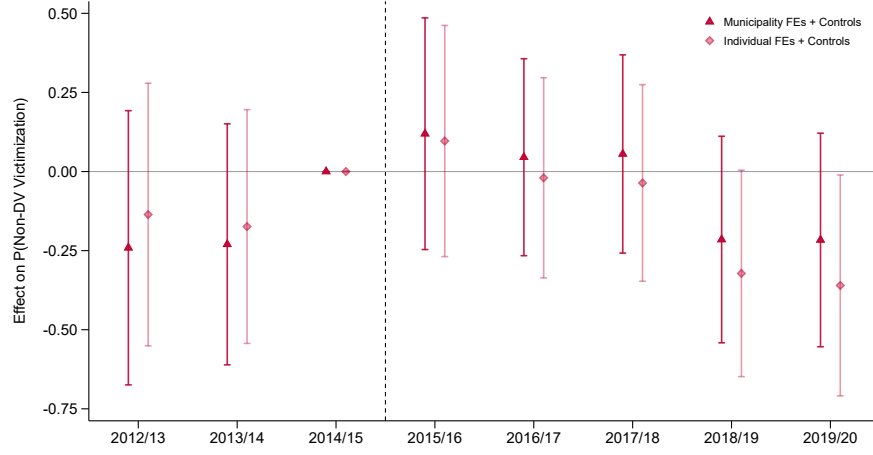
Placebo Test. A complementary approach is to construct an alternative exposure measure that captures similar underlying factors but should not affect DV victimisation directly. We do so by constructing the male analogue of our treatment exposure, defined as the share of violent male offenders against men at baseline, $EXP_{m(i)}^m$. If our baseline exposure $EXP_{m(i)}^f$ primarily reflected underlying socio-economic conditions, then this alternative measure should display a similar relationship with DV outcomes, or attenuate the estimated effect of the main treatment.

Even though the two exposure measures are positively correlated, $\rho(EXP_{icm}^f, EXP_{icm}^m) = 0.64$, suggesting that they capture some common underlying factors, the results in Panel B of Table 3 show that including this placebo exposure does not alter our main findings. The interaction

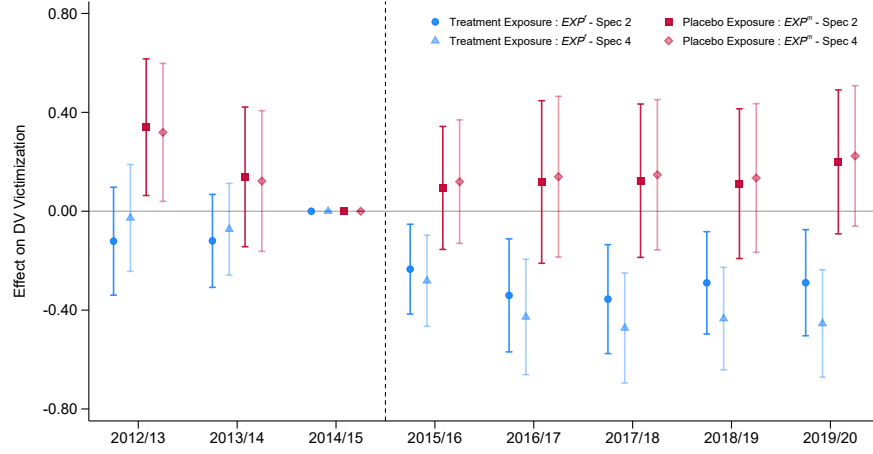
term $Post_t \times EXP_{m(i)}^f$ remains negative and precisely estimated, with very similar magnitudes to those in Table 1. In contrast, $Post_t \times EXP_{m(i)}^m$ is small and statistically insignificant in both specifications, and if anything, estimates are slightly positive.

Figure 7B. presents the corresponding event-study estimates. The event-study coefficients associated with the baseline exposure show no evidence of differential pre-trends and a persistent decline in DV victimisation following the reform, consistent with the main results. In contrast, the placebo exposure displays no comparable pattern. Estimates are generally centred around zero in the post-reform period and do not indicate any systematic relationship with DV outcomes.

Figure 7: Event Study Estimates for the Falsification and Placebo Tests



(A.) Event Study Estimates of Impact of the *Lei do Feminicídio* on Risk of Other Female victimisation (i.e., Non-DV)



(B.) Event Study Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence Using Male Exposure

Notes: Event-study analogues for the falsification and placebo tests. Figure 7A. corresponds to Panel A of Table 3, while Figure 7B. corresponds to Panel B. The specifications follow Columns (2) and (4) of Table 1. Coefficients are scaled by 100 (percentage points). Confidence intervals of 95% are included. Standard errors are clustered at the municipality level.

5.2 Crime Types

Table 4 shows results disaggregated by DV category. Also in this case, the estimated specifications follow Column (4) of Table 1, i.e., our most stringent specification. While Column 1 of Table 4 reproduces the negative and statistically significant coefficient in Column (4) of Table 1, subsequent columns show results when the dependent variable is disaggregated from the most severe form of DV, i.e., femicides, to the least severe, i.e., other types of DV.²⁸ Columns (2)-(7) of Table 4 indicate that the DV reduction in Table 1 was not driven by femicides, which are very rare events for which the estimated coefficient appears negative but numerically small and statistically insignificant, but rather by serious, non-fatal DV, including bodily injuries, serious threats and fighting.²⁹ For each of these DV categories, estimates in Table 4 indicate that, after the reform, the incidence of DV reduced by a disproportionate 3.9% to 5.6% (depending on the outcome variable) in municipalities at the 75th percentile of the distribution of our treatment exposure *vis-à-vis* municipalities at the 25th percentile of the distribution of our treatment exposure. While Column (7) indicates that less serious forms of DV, such as defamation against women by their partners, followed the opposite dynamic, Columns (3)-(5) indicate that municipalities with a higher share of violent men against women experienced a disproportionate reduction in the risk of serious, non-fatal DV after the introduction of the Femicide Law.

Table 4: Estimates of Impact of the *Lei do Feminicídio* by Crime Types

	(1) Any DV	(2) Femicide	(3) Bodily Injuries	(4) Threat	(5) Fighting	(6) Disorderly Conduct	(7) Other
Post×Exposure	-0.372*** (0.056)	-0.001 (0.003)	-0.158*** (0.028)	-0.188*** (0.036)	-0.065*** (0.021)	-0.012 (0.016)	0.051** (0.020)
Observations	17,991,919	17,991,919	17,991,919	17,991,919	17,991,919	17,991,919	17,991,919
Pre Mean of Outcome	2.119	0.011	0.610	1.032	0.259	0.159	0.048
Range of Exposure	0.215	0.215	0.215	0.215	0.215	0.215	0.215
Effect wrt Mean	3.778	1.162	5.560	3.921	5.384	1.582	22.725
Municipality Count	226	226	226	226	226	226	226
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Column (1) reproduces the main effect reported in Column (4) of Table 1. Columns (2)–(7) disaggregate domestic violence episodes by offence category. The specification in all columns is identical to that of column (4) in Table 1. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

²⁸These include, for example, defamation and false claims against one’s partner.

²⁹The types of crime in Columns (2) to (6) account for close to 90% of all DV crimes in our analysis, with the remaining corresponding to Column(7) residual category

5.3 Reporting

A natural alternative interpretation of our findings is that the law may have reduced reporting of DV. The effect this reform may have had on reporting behaviour is ambiguous *ex ante*. On the one hand, the increased salience of femicides and domestic violence generated by the law may have increased the likelihood for DV victims to report an abusive relationship. On the other hand, by raising the perceived penal consequences associated with lethal escalation, the reform may have inadvertently discouraged DV victims from reporting an abusive relationship.

We test these competing hypotheses, and therefore whether our results merely reflect changes in victims' reporting behaviour, in three ways. First, we use the hospitalisation of DV victims as the dependent variable in our analysis. Hospitalisation data were obtained from Brazil's SIH (*Sistema de Informações Hospitalares*) and matched to individuals in our analysis using exact information on date of birth, sex, and municipality of birth from the IGP baseline data, as well as municipality of residence from Datasus. The main outcome therefore measures the cell-level average number of hospitalisations due to injuries (i.e., ICD S- and T-codes). Consistent with a true reduction in DV incidence, Panel A of Table 5 shows that the reform is associated with fewer injury-related hospitalisations among women, although the estimate is not statistically significant when individual fixed effects are included.

Second, using the administrative records from the police of RGS, we restrict the definition of DV to the most serious types of DV with injuries. These include instances where the victim got seriously injured, hospitalized, or died. These are unlikely to be affected by reporting dynamics among DV victims, as serious non-fatal or fatal injuries are less likely to be affected by discretionary reporting or classification once the case enters the police system. Panel B of Table 5 shows that negative and statistically significant estimates of the effect of the Femicide Law on the likelihood of DV still appear.

Third, the richness of the police records allows us to test whether the reform changed who contacted the police in DV incidents. In particular, we model whether the caller was a family member rather than the victim herself. If the reform had substantially reduced victims' willingness to contact the police directly, we might expect an increase in reporting by family members. Panel C of Table 5 shows no detectable effect on this margin.

Table 5: Estimates of Impact of the *Lei do Feminicídio* on Reporting

	(1)	(2)
<i>Panel A: Hospitalisation</i>		
Post×Exposure	-0.096** (0.044)	-0.060 (0.040)
Observations	17,991,919	17,991,919
Pre Mean of Outcome	0.229	0.229
Range of Exposure	0.215	0.215
Effect wrt Mean	9.021	5.669
Municipality Count	226	226
Municipality FE	Yes	No
Individual FE	No	Yes
Municipality Controls	Yes	Yes
<i>Panel B: Serious DV at Individual Level</i>		
Post×Exposure	-0.076*** (0.019)	-0.104*** (0.021)
Observations	17,991,919	17,991,919
Pre Mean of Outcome	0.237	0.237
Range of Exposure	0.215	0.215
Effect wrt Mean	6.906	9.448
Municipality Count	226	226
Municipality FE	Yes	No
Individual FE	No	Yes
Municipality Controls	Yes	Yes
<i>Panel C: Caller is a Family Member</i>		
Post×Exposure	0.015 (0.026)	0.037 (0.029)
Observations	427,835	427,835
Pre Mean of Outcome	0.046	0.046
Range of Exposure	0.170	0.170
Effect wrt Mean	5.380	13.460
Municipality Count	226	226
Municipality FE	Yes	Yes
Crime Type	Yes	Yes
Police Station	No	Yes
Municipality Controls×Period	No	Yes

Notes: This table tests whether the main results in Table 1 reflect changes in reporting behaviour rather than incidence. Panel A models hospitalisations for injuries (ICD codes S and T) from Brazil’s SIH records, matched to individuals using date of birth, sex, and municipality of birth from the IGP baseline data, as well as municipality of residence from Datasus. Panel B restricts DV to the most serious cases (victim seriously injured, hospitalised, or killed). Panel C uses the occurrence-victim sample and models an indicator equal to one if the caller is a family member. In Panels A and B, specifications (1) and (2) follow Columns (2) and (4) of Table 1. In Panel C, Column (1) includes municipality and offence-type fixed effects; Column (2) adds police-station fixed effects and municipality controls interacted with period. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6 Robustness

In our main analysis, the outcome of interest is a binary indicator capturing the probability of being a victim of domestic violence. Panel A of Table E1 in the Appendix replicates the main results when the dependent variable is the number of DV episodes per victim in a period in order to quantify both the extensive and intensive margins of victimisation. All specifications indicate that the crime-reducing effect documented in Table 1 is robust to this alternative definition of our dependent variable.

One concern might be that the exclusion of small municipalities may bias our estimates. Table E2 reproduces the main results from Table 1 in Column (1) and then shows results from the same specification when all municipalities are included in the analysis in Column (2). In both cases, results are very similar. An additional concern might be that our treatment exposure may be comparing individuals from intrinsically different geographies and, therefore, capturing unobserved differences between rural and urban areas rather than a true causal effect of treatment exposure. To that end, we enriched our analysis by restricting the sample to women born in urban areas only. Results are displayed in Column (3) of Table E2 and they look like a replication of our main results in Table 1. Indeed, in urban areas, regardless of the sets of controls included in the regression, higher exposure is associated with a larger post-reform decline in the risk of DV. In particular, estimates indicate that, after the reform, the incidence of DV reduced by a disproportionate 3.2% in urban municipalities at the 75th percentile of the distribution of our treatment exposure *vis-à-vis* urban municipalities at the 25th percentile of the distribution of our treatment exposure. Urban municipalities with a higher share of violent men against women experienced a disproportionate reduction in the risk of DV after the introduction of the reform. This result is helpful in mitigating the potential bias arising from comparing treatment exposures that may capture quite distinct unobserved characteristics of municipalities.

In Table E2, we also consider alternative measures of exposure to the reform. In Column (4) we consider the binary version of our exposure measure discussed in Section 5.1.1. While the main analysis defines treatment exposure as the share of men who committed at least one violent crime against women at baseline, Column (5) displays results when treatment exposure is defined as the share of men aged 18-55 that committed at least one DV crime against a woman. Column (6) displays results when treatment exposure is defined as the share of men aged 18-55 who committed at least one *violent* DV crime against a woman. In all cases, results are robust to these alternative definitions of treatment, indicating a strong decrease in the risk of DV.

Lastly, to address potential concerns about potential migration, we reproduced the results on

DV by using the municipality of *residence* of women to define exposure to the treatment. In this exercise, the sample includes all women born and currently residing in RGS. Interestingly, column (7) of Table E2 shows that the estimated effect using the treatment based on the municipality of residence is negative, statistically significant and consistent with our main results.

7 Mechanisms

Why did Brazil’s *Lei do Feminicídio* reduce the incidence of non-fatal domestic violence (DV) in Rio Grande do Sul? One natural hypothesis is that this reform may have acted via an incapacitation effect on offenders. By increasing the sentences for perpetrators of fatal DV, the reform may have allowed the judicial authorities of Rio Grande do Sul (RGS) to impose harsher prison sentences on abusive men and force them in prison for longer, thus generating an incapacitation effect. The lack of significance in Table 4 in the estimated effect of the reform on femicides arguably makes this an unlikely mechanism, since the reform introduced harsher sentences for fatal DV and not for other, non-fatal DV categories. However, a set of empirical tests is presented here to further establish whether an incapacitation effect might be at play.

7.1 Incapacitation. First vs Repeat Occurrences

A first set of tests distinguishes between first-time victims and victims who have previously experienced domestic violence. If incapacitation effects were the main driver of our results, one would expect disproportionately larger effects for repeated DV episodes and much smaller effects for first-time victims, since serial offenders should, on average, be more likely to be imprisoned following an offence. DV episodes were classified using information on victimisation in the preceding two years.³⁰ Table 6 shows that results for first-time DV victims and repeat DV victims appear substantially comparable, thus providing little empirical support to the notion that serial offenders may have been affected more harshly by the reform and thus an incapacitation of these serial offenders might be behind our results.

7.2 Incapacitation. Imprisonment

A complementary set of tests to establish whether an incapacitation effect might be at play is presented in Table 7, where administrative data from the prison police of RGS are used to

³⁰Because we focus on individuals from age 18 onward, this two-year window was not always available for younger women. To address this, we verified that excluding all observations for which the full two-year history is not observed does not affect our results.

Table 6: Heterogeneous Estimates of Impact of the *Lei do Feminicídio* on Risk of DV Victimization for First-Time Victims and Repeat Victims

	(1) Probability of Any DV	(2) First DV Episode	(3) Repeated DV Episode
Post×Exposure	-0.372*** (0.056)	-0.271*** (0.044)	-0.102*** (0.025)
Observations	17,991,919	17,991,919	17,991,919
Pre Mean of Outcome	2.119	1.594	0.526
Range of Exposure	0.215	0.215	0.215
Effect wrt Mean	3.778	3.652	4.154
Municipality Count	226	226	226
Individual FE	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes

Notes: Column (1) reproduces the main effect reported in Column (4) of Table 1. Columns (2) and (3) distinguish between first-time and repeated domestic violence victimisation episodes. A first episode is defined as a DV victimisation for which no prior DV victimisation is observed in the previous two years; repeated episodes correspond to cases with at least one prior DV victimisation during this period. The specification in all columns is identical to that of Column (4) in Table 1. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

study whether the reform may have increased either the likelihood or the duration of prison stays in the state. In particular, Column (1) of Table 7 uses our main specification to test whether the reform may have increased the likelihood of imprisonment following a DV event; Column (2) does likewise but modelling the average prison stay for the individuals who were imprisoned during our study period; and Column (3) models the likelihood of imprisonment for more than 30 days for the individuals who were imprisoned during our study period, thus ignoring short stays in the measurement of the impact of interest of the reform. In all cases, estimates in Table 7 provide no empirical support to the notion that the reform that is studied here may have altered either the likelihood or the duration of prison stays in RGS. This is perhaps not surprising, given the lack of significance in Table 4 in the estimated effect of the reform on femicides. However, taken together, the results in Table 4 for femicides, in Table 6 and in Table 7 provide no empirical support to the idea that the reform may have reduced DV via an incapacitation effect on offenders.

7.3 Victims’ Institutional Response: Protection versus Prosecution

The preceding results narrow the set of explanations for the decline in recorded DV. Higher-exposure municipalities experienced larger reductions in serious non-fatal violence, yet the reform did not detectably increase either the probability or the duration of imprisonment, and the proportional decline was similar for women with and without a recorded DV episode in the preceding two years. This pattern weighs against broad incapacitation as the principal explana-

Table 7: Estimates of Impact of the *Lei do Feminicídio* on Imprisonment Outcomes

	(1)	(2)	(3)
	Any Imprisonment	Average Stay (Days)	More Than 30 Days
Post $\times$ Exposure	-0.174 (0.234)	19.360 (39.230)	0.041 (0.075)
Observations	427,835	6,725	6,725
Pre Mean of Outcome	1.361	187.183	0.563
Range of Exposure	0.170	0.156	0.156
Effect wrt Mean	2.180	1.610	1.130
Municipality Count	226	186	186
Municipality FE	Yes	Yes	Yes
Crime Type	Yes	Yes	Yes
Police Station	Yes	Yes	Yes
Municipality Controls $\times$ Period	Yes	Yes	Yes

Notes: This table tests whether the *Lei do Feminicídio* reduced domestic violence through an incapacitation channel. The unit of observation is the occurrence–victim record; prison outcomes are measured at the occurrence level and are therefore common to all victims of the same occurrence. A prison stay is linked to the occurrence that most recently preceded the offender’s entry into prison: among all occurrences involving that offender, the stay is assigned to the last one occurring on or before the entry date, and stays beginning before the occurrence are not linked. Column (1) models an indicator equal to one if at least one offender linked to the occurrence served a prison stay, expressed in percentage points. Column (2) models the mean stay across linked offenders, measured in days and top-coded at 1,825 days (five years). Column (3) models an indicator equal to one if that mean stay exceeds 30 days, expressed as a proportion. Columns (2) and (3) samples are restricted to individuals with at least one completed linked spell. All specifications include individual-level baseline characteristics (race and birth cohort), a quadratic in age, municipality of birth fixed effects, offence-type fixed effects, police-station fixed effects, and municipality-level controls measured in 2010 interacted with period indicators. The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

tion, but it does not by itself distinguish a direct response by potential offenders from a change in how victims use the state.

We therefore turn to the second margin made observable by the linked incident records. Conditional on a DV incident reaching the police, victims make legally distinct choices about whether to request an urgent protective measure and whether to formally pursue charges. Comparing these outcomes allows us to determine whether post-reform state engagement shifted toward immediate safety or toward prosecution. These estimates describe the composition of engagement among recorded incidents; the population-panel results separately capture the change in recorded victimization. Placing the two margins side by side is useful because greater demand for protection is consistent with the reform increasing the salience or perceived value of an existing remedy, while anticipated protective intervention may also enter offenders' expectations.

We investigate these issues by focusing on DV incidents during our study period, i.e., from 2012 to 2019, in RGS. The same DiD specification was estimated again, modelling as dependent variables a set of dummy variables that capture whether the victim of DV requested protective measures, whether she pressed charges against her abusive partner, or any combination of the two. This analysis is at the incident-victim level, i.e., with an intensive margin, to better investigate the specific events of DV.³¹ Importantly, one victim of DV does not need to press charges to be able to request and access protective measures. Therefore, these responses are not mechanically connected. However, they can, of course, be correlated as, for instance, requesting protective measures may act as a compensating mechanism to avoid pressing charges for a woman who might be financially dependent on her abusive partner.

For this analysis, we expanded the specification to include offence-type fixed effects in the set of controls (i.e., fixed effects for, namely, threat, bodily injuries, fighting, and disorderly conduct). Therefore, this can be viewed as a within-crime-type analysis of how the victim's response to DV changed after the reform. This extension in the specification enables us to avoid capturing crime-type compositional effects as those shown in Table 4 that can result from the policy and, therefore, influence the probability of requesting protective measures and/or pressing charges based on the nature of their severity and risk to life. Furthermore, to account for potential differences in policing across treatment units, we also included police station fixed effects, which absorb time-invariant differences in resources and specialization of police forces.

Panel A of Table 8 shows results for the likelihood of a DV victim requesting protective measures from the police to keep her safe from her abusive partner after a DV incident. Columns (1)

³¹Appendix D shows the results for behavioural responses when using the same design of the first part, i.e., the panel structure of the data.

to (4) progressively add control variables to the estimated equation. Estimates show that, regardless of the set of controls, the propensity of victims to seek protective measures increased due to the reform. In particular, the likelihood of DV victims requesting protection from the abusive partner in municipalities at the 75th percentile of our treatment exposure measure increased by a disproportionate 1.14% to 1.86% compared with municipalities at the 25th percentile of our treatment exposure measure.

Table 8: Estimates of Impact of the *Lei do Feminicídio* on Requesting Protective Measures and Pressing Charges

	(1)	(2)	(3)	(4)
<i>Panel A: Requested Protective Measures</i>				
Post×Exposure	4.751*** (1.453)	5.390*** (1.438)	6.169*** (1.692)	3.796** (1.900)
Observations	427,835	427,835	427,835	427,835
Pre Mean of Outcome	56.411	56.411	56.411	56.411
Range of Exposure	0.170	0.170	0.170	0.170
Effect wrt Mean	1.430	1.620	1.860	1.140
<i>Panel B: Pressed Charges</i>				
Post×Exposure	-3.346** (1.690)	-2.992* (1.745)	-2.559 (2.012)	-1.968 (2.004)
Observations	427,835	427,835	427,835	427,835
Pre Mean of Outcome	71.596	71.596	71.596	71.596
Range of Exposure	0.170	0.170	0.170	0.170
Effect wrt Mean	0.790	0.710	0.610	0.470
Municipality Count	226	226	226	226
Municipality FE	Yes	Yes	Yes	Yes
Crime Type	No	Yes	Yes	Yes
Police Station	No	No	Yes	Yes
Municipality Controls×Period	No	No	No	Yes

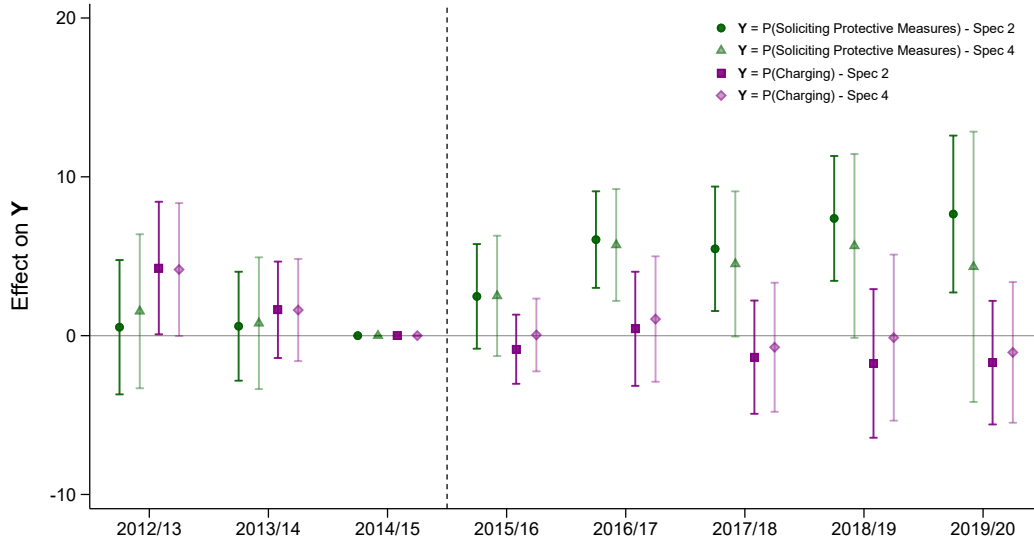
Notes: This table reports estimates of the impact of the *Lei do Feminicídio* on victims’ behavioural responses following a DV incident. The analysis is conducted at the occurrence-victim level. Panel A models an indicator equal to one if the victim requested protective measures from the police. Panel B models an indicator equal to one if the victim pressed charges against the offender. All specifications include individual-level baseline characteristics (race and birth cohort), a quadratic in age, municipality of birth fixed effects. Column (2) adds offence-type fixed effects. Column (3) further adds police station fixed effects. Column (4) additionally includes municipality-level controls (median income, share of employed individuals, education levels, share of young individuals, poverty rate, and an urbanisation index) measured in 2010 interacted with period indicators. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Panel B of Table 8 shows results for the likelihood of a DV victim pressing charges against her abusive partner after a DV incident. While a negative sign appears in this case, suggesting that the harsher sentences implied by the reform may have discouraged some DV victims from pressing charges against their partners, estimated effects are smaller and not statistically significant in columns (3) and (4). Therefore, while *ex ante* the increase in protective measures might have appeared as a potential compensating mechanism for the potential reduction in charges following

the reform, the results in Table 8 do not provide empirical support to this hypothesis.³² Table D2 also shows that, due to the reform, increased protective measures were requested following threats and bodily injuries, while fewer charges were pressed following fighting.

Figure 8 represents the event-study analogue for the specifications in Columns (2) and (4) of Table 8. Regardless of the specification chosen, these event-study estimates substantially confirm the conclusion from Table 8 that the reform increased the likelihood of a DV victim to request protective measures from the police and left the likelihood of pressing charges against abusive partners unchanged. The absence of differential pre-trends supports this interpretation.

Figure 8: Event Study Estimates of Impact of the *Lei do Feminicídio* on Requesting Protective Measures and Pressing Charges



Notes: This figure reports event-study estimates and the corresponding 95% confidence intervals of the effect of treatment exposure on the probability of requesting protective measures (in green) and pressing charges (in purple). Each point corresponds to the estimated coefficient for a given relative time period, with the omitted category being the year prior to the reform. The specifications follow Columns (2) and (4) of Table 8. Coefficients are scaled by 100 (percentage points). Standard errors are clustered at the municipality level.

Taken together, Table 8, Figure 8 and Table D1 show a reallocation in the form of victims' state engagement. Requests for protective measures increased, the strictest specification shows

³²Table D1 also shows our estimated effects for the four possible combinations of protective measures and charges. Results indicate that the reform generated a statistically significant reduction in the likelihood to press charges without requesting protective measures and a statistically significant increase in the likelihood to request protective measures without pressing charges.

no detectable change in the overall probability of formally pursuing charges, requests for protection without charges increased, and charges without protection declined. The data therefore point to a shift toward a safety-oriented remedy rather than a general increase in punitive engagement. Even though the records do not distinguish changes in victims’ beliefs from changes in information, police practice, or case composition, this pattern is consistent with protective measures becoming more salient or more valuable to victims after the reform.

Table 9: Estimates of Impact of the *Lei do Feminicídio* on Waiting Behaviour and Reporting Time

	(1) Wait Before PM	(2) N of DV Before PM	(3) Days to PM	(4) Time to Report
Post×Exposure	-0.015* (0.008)	-0.018 (0.012)	-5.246* (2.716)	0.016 (0.023)
Observations	263,273	263,273	263,273	427,829
Pre Mean of Outcome	0.139	0.173	39.001	1.733
Range of Exposure	0.170	0.170	0.170	0.170
Effect wrt Mean	1.820	1.730	2.290	0.160
Municipality Count	226	226	226	226
Municipality FE	Yes	Yes	Yes	Yes
Crime Type	Yes	Yes	Yes	Yes
Police Station	Yes	Yes	Yes	Yes
Municipality Controls×Period	Yes	Yes	Yes	Yes

Notes: This table reports estimates of the impact of the *Lei do Feminicídio* on the timing of victims’ interaction with the police. Columns (1) to (3) restrict the sample to DV victims who ever requested protective measures, retaining the occurrence at which the request was made. All three outcomes are defined within the two-year spell preceding the request. Column (1) models an indicator equal to one if the victim experienced at least one earlier DV episode in that spell before requesting protective measures. Column (2) models the number of DV episodes in the spell preceding the request, measured in episodes and equal to zero when protection is requested at the first episode. Column (3) models the number of days between the first DV episode of the spell and the request for protective measures. Column (4) models the log of one plus the number of days between the DV incident and its registration with the police, winsorised at the 99th percentile; occurrences with a negative interval between the two dates are treated as data errors and excluded. Dependent variables are reported in their original units and are not rescaled. All specifications include municipality-of-birth, offence-type and police-station fixed effects together with municipality-level characteristics measured in 2010 interacted with period indicators, following Column (4) of Table 8. The exposure range and the effect relative to the mean (“Effect wrt Mean”) are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We next examine timing. If the shift toward protection extended to when victims used the remedy, requests should occur earlier in the sequence of recorded abuse. This analysis was conducted at the incident level and was restricted to DV victims who ever requested protective measures. Table 9 presents our results. Column (1) shows that Brazil’s *Lei do Feminicídio* decreased the likelihood that women wait beyond the first recorded DV episode before requesting the state’s protection; put differently, the reform increased the likelihood that DV victims seek protective measures at the earliest occurrence. Column (2) models the dependent variable as the continuous count of DV episodes before protection is sought. The point estimate suggests

that victims waited fewer DV episodes before seeking protection, although the estimate is not statistically significant. Similarly, Column (3) models the dependent variable as the continuous count of days starting from the DV episode before protective measures are requested. As a result of the reform, DV victims waited fewer days before seeking protection. Finally, Column (4) extends the analysis to all women, i.e., not only those who sought protective measures, and models the time between the DV incident and the date of reporting to the police as dependent variable. The estimated coefficient is positive and insignificant, indicating that reporting delay does not change, which weighs against a general acceleration of police contact in more treated areas in our study period as the explanation for the results in Columns (1)-(3).

8 Conclusion

The legislation governing domestic violence operates within relationships in which the person whom the state may punish is also someone with whom the victim may share a home, children, income, and repeated contact. A legal reform can therefore affect more than an offender's expected sanction. It can also change the value victims attach to different forms of state intervention. This paper studies both margins in the context of Brazil's 2015 Femicide Law, which formally recognized femicide as a qualified homicide and heinous crime.

Linking population, police, hospital, and prison records for 2.6 million women in Rio Grande do Sul, we find that recorded domestic violence fell more after the reform among women from municipalities with greater predetermined exposure to men with a history of violence against women. Moving from the 25th to the 75th percentile of baseline exposure is associated with a 3.8% decline relative to the pre-reform mean. The decline is concentrated in serious non-fatal offences, i.e., bodily injury, threats, and fighting, and does not extend to non-domestic violent victimization. The same pattern appears in hospital records. By contrast, neither the probability nor the duration of imprisonment rises detectably, and the proportional decline is similar for women with and without a recorded domestic-violence episode in the preceding two years.

The linked incident records reveal a second response. Conditional on a domestic-violence incident reaching the police, the requests of urgent protective measures rose differentially, while the strictest specification shows no corresponding increase in the formal pursuit of charges. The increase in protective requests is concentrated in requests made without charges, and there is suggestive evidence that victims sought protection earlier in the sequence of recorded abuse. The reform was therefore followed not simply by more or less contact with the justice system, but by a change in the form of that engagement toward immediate protection. Together with

the incidence results, this documents a two-sided institutional response to the reform.

These findings broaden the policy margin on which criminal-law reforms should be judged. A law directed at the lethal endpoint of gender-based violence was followed, in more exposed municipalities, by less recorded violence below that endpoint and by more protection-oriented engagement among incidents that reached the state. The policy implication is not that protective requests are a substitute for enforcement, but that the accessibility, processing, and enforcement of protective remedies are central components of the institutional response. Evaluating domestic-violence policy solely through sentences, incarceration, or case filings would miss this margin. In this setting, the law mattered before the worst outcome occurred: through behaviour below the fatal margin and through victims' choice of how to seek the state's protection.

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Appendix A Data Construction

This section describes the administrative sources used in the empirical analysis and the construction of the main analysis samples. Replication code is available upon request.

A.1 Administrative Sources

The empirical analysis combines several administrative sources for the universe of individuals that ever resided in Rio Grande do Sul (RGS) between 2010 and 2019, obtained via a data-sharing agreement with the Cabinet of the Governor of RGS and several public agencies of the state.

Records of family linkages and individual characteristics come from the Instituto Geral de Perícias (IGP), the public identity registry of RGS. The dataset reports gender, ethnicity, date of birth and death, nationality, schooling history, civil status, municipality of birth, and municipality of residence, and contains 8,803,274 unique individuals born in RGS after 1920 and, if deceased, deceased after 2010. Records on criminal occurrences come from the Secretaria de Segurança Pública–Polícia Civil and contain 20,066,180 incidents reported between 2010 and 2024. For each occurrence we observe the type and date of the event, the police station in charge, an indicator for attempted versus consummated crimes, the type of participation of each individual involved, the protective-measure status, the representation status (i.e. whether the victim pressed charges), and the participant’s physical condition. To avoid the disruption to DV dynamics generated by the COVID-19 pandemic, we restrict the sample to occurrences taking place between March 2010 and February 2020.

We classify crimes following the categorisation adopted by the RS Seguro programme of the Government of RGS. We classify a crime as violent if it belongs to the official CVLI category (*Crimes Violentos Letais Intencionais*, intentional lethal violent crimes) or to any robbery-related offence category. CVLI includes crimes such as intentional homicide, femicide, and bodily injury followed by death. Robberies are also included because, even when they are not lethal, they involve violence or the threat of violence against the victim. Domestic violence (DV) cases are identified through the eligibility marker for urgent protective measures, which is recorded only when the victim is identified, brought to the police station, and testifies about the occurrence. Within DV, four offence categories — threat (*ameaça*), bodily injury (*lesão corporal*), fighting (*vias de fato*), and disorderly conduct (*perturbação da tranquilidade*) — account for close to 90% of all DV cases during the period of analysis.

Femicide presents a specific measurement challenge because it was introduced as a separate crime category in the police record system only in 2018, and the number of cases coded as femicide in the police occurrences is mechanically zero before that date and very low in the early post-reform years. We complement the police records with the official list of femicides between 2010 and 2018 maintained by the security agencies of RGS, and we construct three alternative femicide indicators based on (i) all murders with a DV marker, (ii) murders in offender–victim dyads with a prior history of DV, and (iii) the union of the two. The DV marker is set to one for any occurrence flagged as femicide under any of these definitions. Our main results use the most comprehensive measure that combines the different ways of identifying likely femicides in police records.

Predetermined municipality-level controls — median monthly income, an education index, the share of employed individuals, the share of urban residents, and the poverty rate — come from the IBGE 2010 population census and complementary sources, and are available for all 497 municipalities of RGS. The share of young individuals (aged 15–24) is computed from the IGP data at the 2010 baseline. Hospitalisation records come from Brazil’s Sistema de Informações Hospitalares (SIH) and are matched to individuals in our analysis using exact information on date of birth, sex, and municipality of birth from the IGP baseline data, as well as municipality of residence from Datasus. We retain hospitalisations whose primary diagnosis falls in chapters S and T of the International Classification of Diseases.

A.2 Analysis Samples

The main analysis is conducted at two units of observation: an individual-period panel of women, used to study the incidence of DV (Sections 5–6), and an occurrence-victim sample, used to study victims’ behavioural responses (Section 7).

The individual-period panel is constructed from the IGP file, expanded into ten twelve-month periods running from March to February of the following calendar year. Period 1 covers March 2010 to February 2011 and period 10 covers March 2019 to February 2020; the indicator $Post_t$ takes value one from period 6 onward, since the reform date (March 2015) coincides with the start of that period. We restrict the panel to women aged 18–55 in each period, which covers the overwhelming majority of female DV victims (Figure C1) and excludes minors. Outcomes are constructed by collapsing the occurrence-level data at the individual–period level. The resulting sample contains 2,557,114 unique women and 17,991,919 woman-period observations, of which 356,043 correspond to women with at least one DV episode in the relevant period.

The occurrence-victim sample is built from the participant–occurrence records merged with individual characteristics, the crime classification, and protective-measure information. We apply the same age and gender restrictions as for the individual-period panel and additionally drop occurrences with more than ten victims or ten offenders, occurrences in which the same individual appears as both victim and offender, and occurrences in which the gender of any participant is missing. The resulting sample contains 420,897 DV occurrences and 427,835 occurrence-victim observations corresponding to 257,846 unique female DV victims, with one row per victim of one DV occurrence. The vast majority of occurrences (98.44%) have a single victim, 1.48% have two, and 0.08% have three or more.

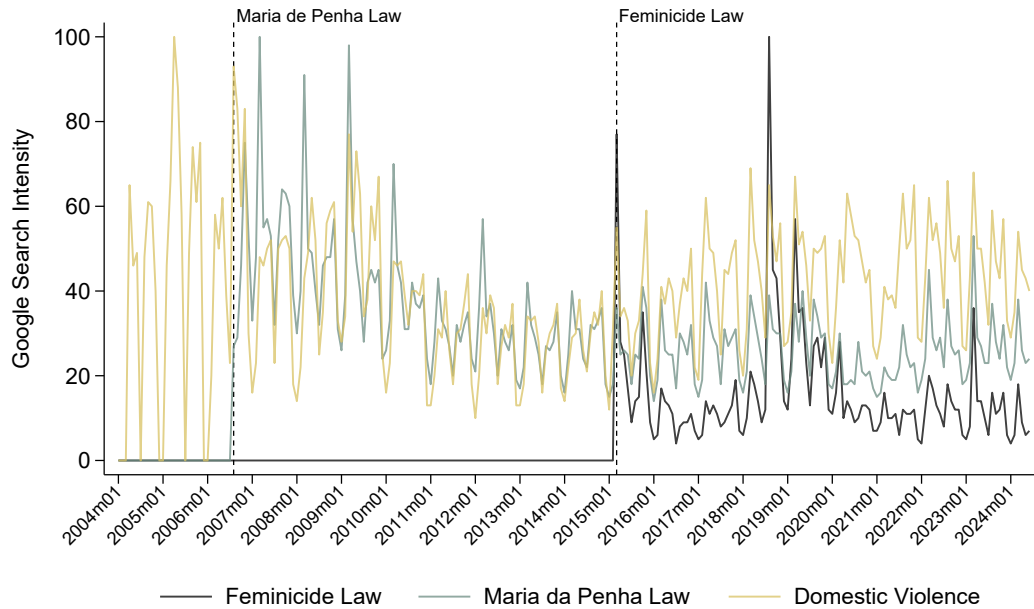
For Panel C of Table 5, which models the identity of the caller, we further merge this sample with kinship indicators from administrative records that report the link between any two individuals in the data (“(ex-)partner”, “father”, “mother”, “son/daughter”, “sibling”). The outcome modelled in Panel C corresponds to an indicator for the caller being any family member of the victim.

A.3 Baseline Exposure Measure

The baseline exposure measure $EXP_{m(i)}^f$ is constructed from the participant–occurrence records in period 1 (March 2010–February 2011), restricted to male offenders aged 18–55 of violent crimes against women. For each municipality of birth, we count the number of unique such offenders and divide by the male population aged 18–55 in the municipality at baseline. The resulting share is multiplied by 100. Each woman in the panel is assigned the value of this share for her municipality of birth. Three small municipalities have zero exposure because no male offender of a violent crime against a woman was recorded there in 2010; the main results are not sensitive to dropping them (Section 5.1.1). Of the 497 municipalities of RGS, 226 have sufficient population size to support reliable measurement of exposure and constitute the baseline sample; the robustness check in Column (2) of Table E2 shows that the results are not sensitive to including all municipalities. Analogous exposure measures based on DV crimes and on violent DV crimes against women are constructed in the same way and are used in the robustness analysis in Table E2.

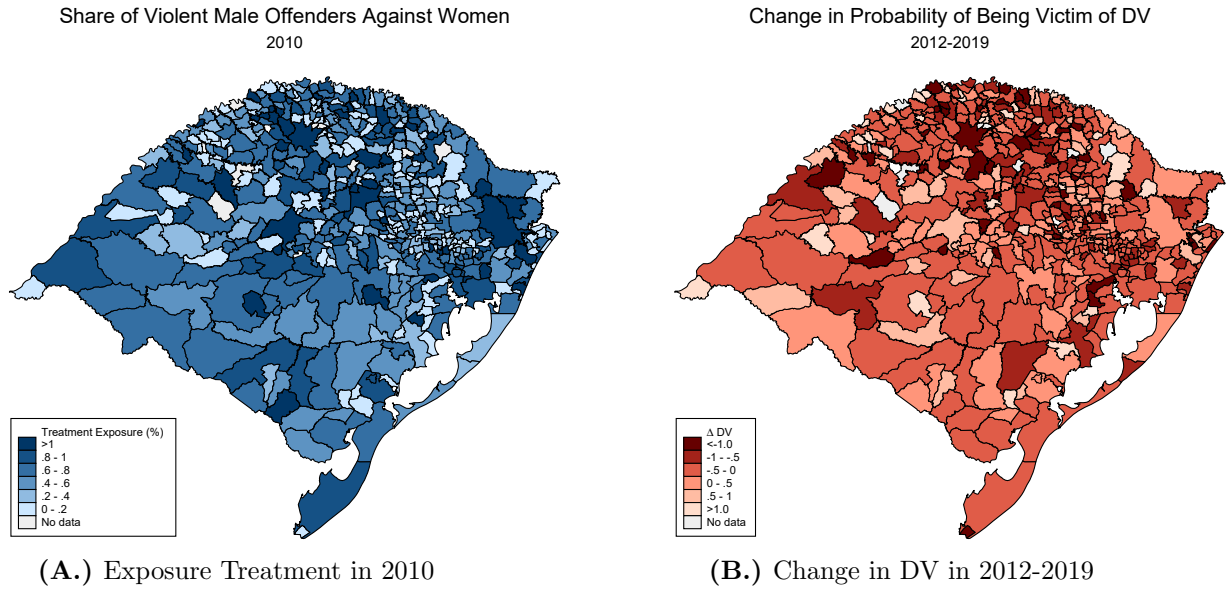
Appendix B

Figure B1: Google Search Intensities for Maria da Penha Law, Femicide Law & Domestic Violence



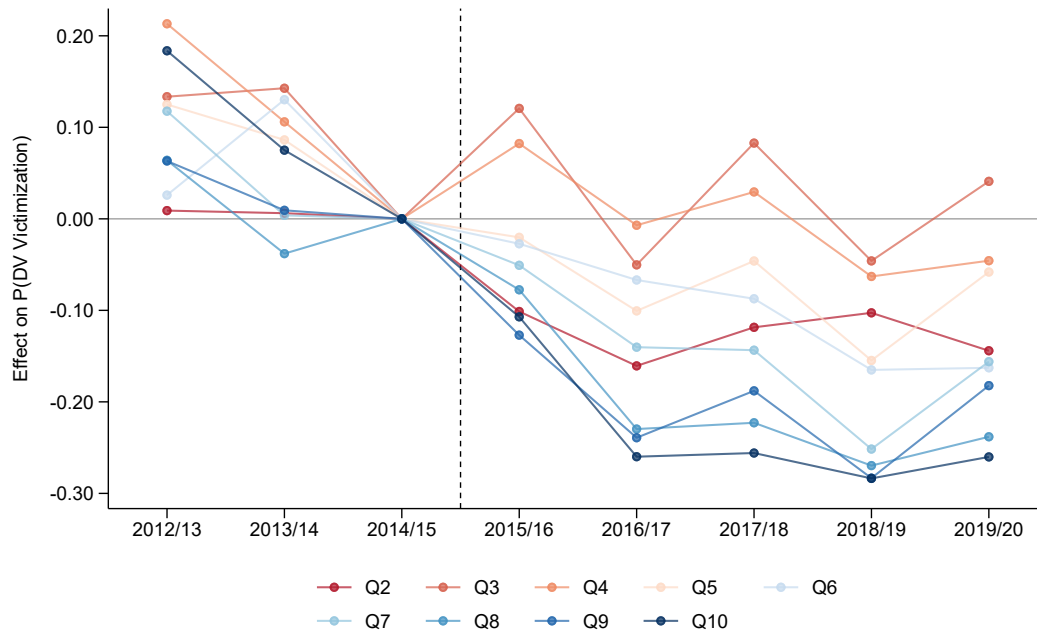
Notes: The figure plots monthly Google Trends search intensity for the terms “Femicide Law”, “Maria da Penha Law”, and “domestic violence” between March 2004 and March 2024. Search intensity is normalized by Google Trends to range from 0 to 100 within the selected period, where 100 corresponds to the peak search volume. The dashed vertical lines mark the enactment of the Maria da Penha Law (August 2006) and of the Femicide Law (March 2015).

Figure B2: Share of Violent Male Offenders against Women in 2010 and Change in Risk of Domestic Violence in 2012-2019 (All Municipalities)



Notes: The map on the left displays the spatial variation in the share of males aged 18-55 with a record of violence against a female victim in the baseline period (March 2010 - February 2011) across all the 497 municipalities in Rio Grande do Sul - with 13 municipalities with missing data marked separately. The map on the right shows the spatial variation in the change in the probability of female victimisation from domestic violence between 2012 and 2019 across the same set of municipalities.

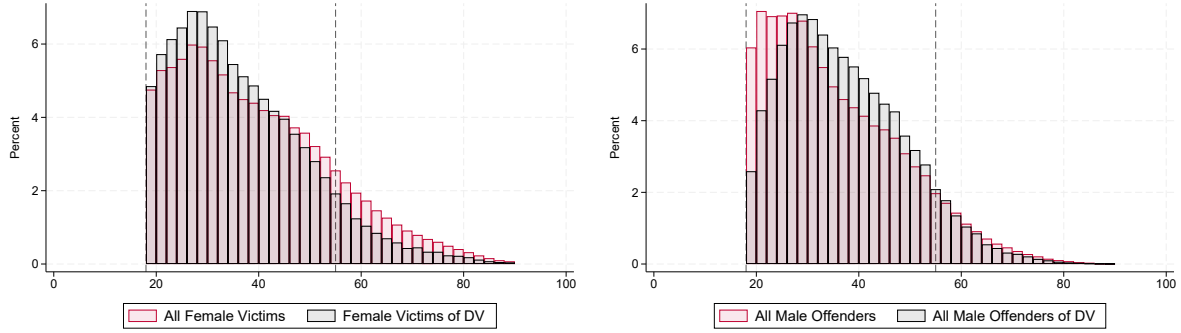
Figure B3: Event Study Estimates by Deciles of Treatment Exposure



Notes: This figure reports event-study estimates of the effect by decile of treatment exposure compared to DV victimisation in the lowest decile. Treatment groups are calculated based on deciles of baseline female exposure to violent men. Each point corresponds to the estimated coefficient for a given relative time period, with the year prior to the reform omitted. The specification corresponds to Column (4) of Table 1. Coefficients are scaled by 100 (percentage points). Standard errors are clustered at the municipality level.

Appendix C

Figure C1: Age Profile of Victims & Offenders for Any Offence



Notes: The figure displays the age profile of female victims and male offenders for any offence. Notice that here we consider everyone above 18 y.o. In red all crimes are considered, in black DV crimes.

Table C1: Descriptive Statistics (Individual-Level Panel)

Variable (%)	Mean	SD
Exposure	0.77	0.185
Exposure-Male	0.52	0.118
Probability of Any DV	1.98	13.927
Femicide	0.01	1.089
Bodily Injury	0.57	7.496
Threat	0.95	9.689
Fighting	0.23	4.745
Disorderly Conduct	0.16	4.029
Probability of Non-DV	5.03	21.850
First DV Episode	1.49	12.132
Repeated DV Episode	0.48	6.946
Observations	17,991,919	

Notes: The table displays the mean and standard deviation for the main variables used in the empirical analysis considering the full sample of 17,991,919 women-period observations.

Table C2: Descriptive Statistics (Municipalities)

Variable (Measured in 2010)	Mean	SD
Share Black	0.07	0.048
Median Monthly Income	465.64	110.307
Education Index	0.64	0.077
Share of Employment	0.68	0.069
Share of Youth	0.17	0.070
Poverty Rate	2.91	2.879
Urban Index	0.50	0.501
Observations	226	

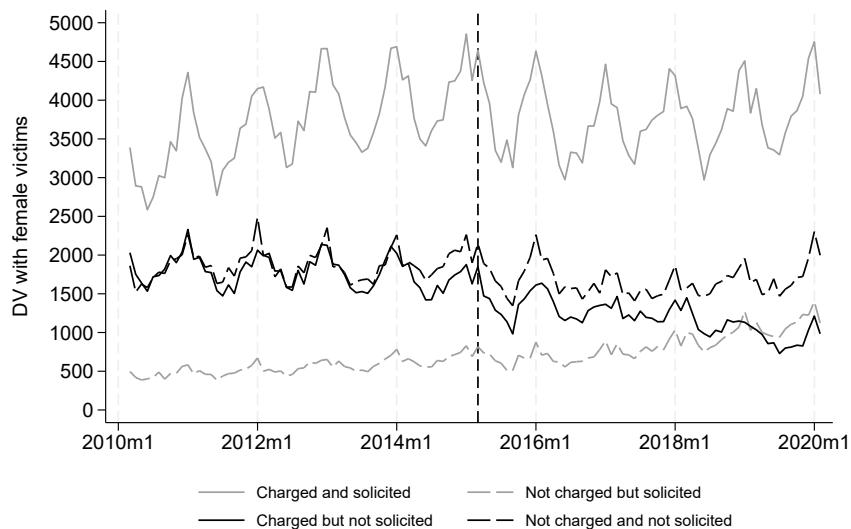
Notes: The table presents descriptive statistics for the 226 municipalities in the state of Rio Grande do Sul included in our main specification. The urban index is an indicator equal to one if more than 75% of a municipality's population resides in urban areas. All variables, except the share of young individuals and the education index, are constructed using the 2010 Brazilian Census. The share of youth (aged 15–24) is computed using IGP data from the 2010 baseline. The education index is the education component of the Rio Grande do Sul Socioeconomic Development Index (Idese), based on indicators of school enrollment, student achievement, and adult educational attainment.

Table C3: Descriptive Statistics (Occurrence-Victim Level)

Variable (%)	Mean	SD
Caller is a family member	0.04	2.034
Pressed Charges	69.94	45.850
Solicited Protection	60.65	48.853
Pressed Charges & Solicited Protection	51.42	49.980
Not Pressed Charges but Solicited Protection	9.23	28.941
Pressed Charges but Not Solicited Protection	18.52	38.849
Not Pressed Charges and Not Solicited Protection	20.83	40.608
Victim is severely injured, hospitalised or dead	8.28	27.555
Observations	427,835	

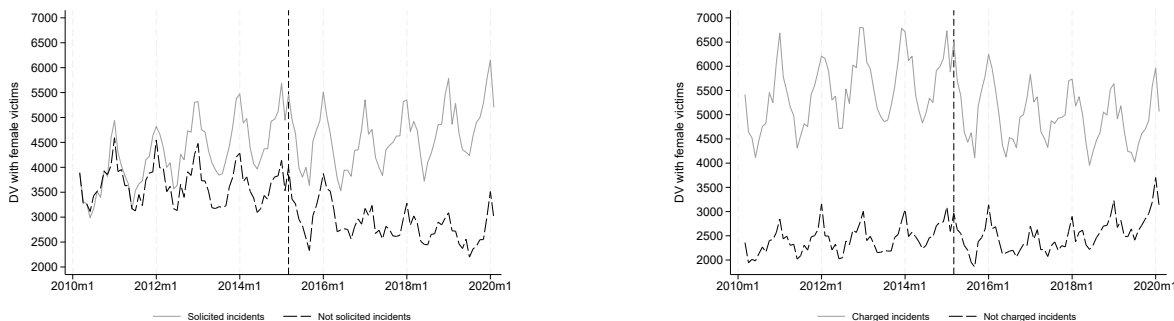
Notes: The table displays descriptive statistics for the sample of 427,835 occurrence-victim pairs included in our analysis.

Figure C2: Incidents when Charges (Protective Measures) were Pressed (Requested) vs Not



Notes: The figure displays the monthly counts of DV incidents distinguishing between incidents when charges (protective measures) were pressed (requested) vs not.

Figure C3: Trends of Requested and Charged Incidents vs Not



(A.) Requested Incidents vs Not

(B.) Pressed Charges vs Not

Notes: The figure displays the monthly counts of DV incidents distinguishing between those for which protective measures were requested and those for which charges were pressed, respectively.

Appendix D Mechanisms

Table D1: Estimates of Impact of the *Lei do Feminicídio* on Combination of Behaviours

	(1) Not Charged Not Requested	(2) Charged Not Requested	(3) Not Charged Requested	(4) Charged Requested
Post×Exposure	-0.256 (1.549)	-3.538** (1.494)	2.225* (1.131)	1.569 (2.047)
Observations	427,835	427,835	427,835	427,835
Pre Mean of Outcome	21.453	22.136	6.950	49.460
Range of Exposure	0.170	0.170	0.170	0.170
Effect wrt Mean	0.200	2.720	5.440	0.540
Municipality Count	226	226	226	226
Municipality FE	Yes	Yes	Yes	Yes
Crime Type	Yes	Yes	Yes	Yes
Police Station	Yes	Yes	Yes	Yes
Municipality Controls×Period	Yes	Yes	Yes	Yes

Notes: This table reports estimates of the impact of the *Lei do Feminicídio* on the four mutually exclusive combinations of victims' behavioural responses, complementing the marginal estimates in Table 8. The analysis is conducted at the occurrence-victim level. Each dependent variable is an indicator equal to one if the victim's response falls in the corresponding combination: Column (1) neither pressed charges nor requested protective measures; Column (2) pressed charges but did not request protective measures; Column (3) did not press charges but requested protective measures; Column (4) both pressed charges and requested protective measures. All specifications follow Column (4) of Table 8. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean ("Effect wrt Mean") are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table D2: Estimates of Impact of the *Lei do Feminicídio* on Behaviour By Crime Type

	(1)	(2)	(3)	(4)
	Threat	Bodily Injury	Fighting	Disorderly Conduct
<i>Panel A: Requested Measure</i>				
Post×Exposure	2.432** (0.975)	1.400*** (0.481)	-0.127 (0.315)	-0.315 (0.335)
Observations	427,835	427,835	427,835	427,835
Pre Mean of Outcome	26.942	16.365	5.717	3.349
Range of Exposure	0.170	0.170	0.170	0.170
Effect wrt Mean	1.530	1.450	0.380	1.600
<i>Panel B: Pressed Charges</i>				
Post×Exposure	-1.316 (1.398)	-0.146 (0.399)	-1.004** (0.399)	0.289*** (0.091)
Observations	427,835	427,835	427,835	427,835
Pre Mean of Outcome	34.618	22.209	7.543	0.399
Range of Exposure	0.170	0.170	0.170	0.170
Effect wrt Mean	0.650	0.110	2.260	12.310
Municipality Count	226	226	226	226
Municipality FE	Yes	Yes	Yes	Yes
Crime Type	Yes	Yes	Yes	Yes
Police Station	Yes	Yes	Yes	Yes
Municipality Controls×Period	Yes	Yes	Yes	Yes

Notes: This table reports estimates of the impact of the *Lei do Feminicídio* on victims' behavioural responses, disaggregated by DV crime type. The analysis is conducted at the occurrence-victim level. The dependent variable in Panel A is an indicator equal to one if the victim requested protective measures and the incident is recorded under crime type j ; in Panel B it equals one if the victim pressed charges and the incident is recorded under crime type j . Columns index the crime type: threat (1), bodily injury (2), fighting (3), and disorderly conduct (4). All specifications follow Column (4) of Table 8. Coefficients and pre-reform means are scaled by 100 (percentage points). The exposure range and the effect relative to the mean ("Effect wrt Mean") are defined as in Table 1. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix E Robustness Checks

Table E1: Estimates of Impact of the *Lei do Feminicídio* on Count of Domestic Violence

	Number of DV Episodes			
	(1)	(2)	(3)	(4)
Post×Exposure	-0.005*** (0.001)	-0.003*** (0.001)	-0.007*** (0.001)	-0.005*** (0.001)
Observations	17,991,919	17,991,919	17,991,919	17,991,919
Pre Mean of Outcome	0.025	0.025	0.025	0.025
Range of Exposure	0.215	0.215	0.215	0.215
Effect wrt Mean	4.230	2.587	5.807	3.999
Municipality Count	226	226	226	226
Municipality FE	Yes	Yes	No	No
Individual FE	No	No	Yes	Yes
Municipality Controls	No	Yes	No	Yes

Notes: This table reproduces the main analysis presented in Table 1 using the number of domestic violence episodes per individual-period as the dependent variable, rather than a dummy for any domestic violence victimisation (not scaled by 100). The specifications in Columns (1)–(4) correspond to the ones in Columns (1)–(4) in Table 1. The dependent variable is the count of domestic violence episodes per individual-period (not scaled by 100). The range of exposure and the pre-reform mean are reported in their original units. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E2: Robustness Checks

	Sample Selection			Alternative Exposure Measures			
	(1) Main Specification	(2) No Municipality Filter	(3) Urban Municipalities	(4) Binary Exposure	(5) Using Exposure to DV	(6) Using Exposure to Violent DV	(7) Using Exposure Based on Residence
Post*Exposure	-0.372*** (0.056)	-0.286*** (0.047)	-0.430*** (0.066)				
Post*High Exposure				-0.124*** (0.031)			
Post*Exposure DV					-0.248*** (0.028)		
Post*Exposure Violent DV						-0.432*** (0.065)	
Post*Exposure (Residence)							-0.351*** (0.063)
Observations	17,991,919	18,693,278	15,072,262	17,991,919	17,991,919	17,991,919	12,132,556
Pre Mean of Outcome	2.119	2.100	2.198	2.119	2.119	2.119	1.930
Range of Exposure	0.215	0.216	0.165	-	0.294	0.151	0.200
Effect wrt Mean	3.778	2.946	3.226	5.868	3.446	3.08	3.635
Municipality Count	226	484	114	226	226	226	225
Municipality FE	No	No	No	No	No	No	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Column (1) reproduces the main result reported in Column (4) of Table 1. Column (2) includes all municipalities without missing data in the analysis, without applying the municipality filter used in the baseline sample. Column (3) restricts the sample to women born in urban municipalities only. Columns (4) to (7) consider alternative measures of exposure to the reform. Column (4) considers a binary treatment variable that takes one if the municipality has a baseline share of violent men that is above median exposure. In Column (5), treatment exposure is defined as the baseline share of men aged 18–55 who committed at least one domestic violence offence against a woman. In Column (6), treatment exposure is defined as the baseline share of men aged 18–55 who committed at least one violent domestic violence offence against a woman. Column (7) defines treatment exposure based on the municipality of residence rather than municipality of birth. All specifications include individual fixed effects and municipality-level controls interacted with period fixed effects. Coefficients, pre-reform means, and the range of exposure are multiplied by 100. Standard errors are clustered at the municipality level and can be found in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.