

Call It By Its Name: Legal Recognition and the Prevention of Domestic Violence*

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Abstract

Can harsher criminal sanctions reduce violence inside intimate relationships? We study Brazil's 2015 Femicide Law, a reform that clarified the legal treatment of gender-based killings, classified femicide among heinous crimes, and increased the public salience and expected punishment of severe violence against women. Using ten years of linked administrative records for the universe of individuals in the state of Rio Grande do Sul, we compare women born in municipalities with different pre-reform exposure to men with a history of violence against women. We find that the reform reduced domestic violence, with effects concentrated in serious non-fatal offences: bodily injuries, threats, and fighting. The decline does not appear to be explained by reporting changes or incapacitation. Instead, the evidence points to deterrence reinforced by victims' use of the state: after the reform, women requested protective measures more often and did so earlier in the sequence of abuse. The results show that criminal law can reduce domestic violence before the fatal margin, not only by threatening offenders, but by making protection a more credible option for victims.

JEL Codes: K14, K42, J12.

Keywords: Gender-related violence; Violence against women; Crime; Punishment.

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1 Introduction

Violence against women is a global phenomenon shaped by local institutions. WHO estimates indicate that about one in three women worldwide have experienced physical or sexual intimate-partner violence, non-partner sexual violence, or both; among ever-partnered women aged 15–49, 27 percent have experienced physical or sexual violence by an intimate partner ([World Health Organization, 2024, 2021](#)). Brazil fits this global pattern. UN Women reports that 16.7 percent of Brazilian women aged 16 or older have experienced physical or sexual intimate-partner violence in their lifetime, and 3.1 percent 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 percent of Brazilian women have experienced domestic or family violence by a man, and that 76 percent 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 percent of adult women experienced psychological, physical, or sexual violence in the previous twelve months; among women who suffered physical violence, 52.4 percent reported a partner or ex-partner as the perpetrator, and 72.8 percent reported that the episode occurred at home ([Instituto Brasileiro de Geografia e Estatistica, 2021](#)).

The prevalence of domestic violence poses a central question for policy makers: what legal measures can reduce violence inside intimate relationships? Whether and how formal legal measures will reduce domestic violence is a complicated question. Domestic violence differs from many crimes because the victim and offender often share a household, children, finances, social networks, and repeated contact. Reporting an abusive partner may provide protection, but it could also trigger retaliation, stigma, loss of income, or the incarceration of a household member. A higher expected sanction may deter offenders ([Becker, 1968](#)), but it could also change victims' willingness to seek help by increasing the perceived effectiveness of state protection. The effect of sentencing severity therefore depends on two margins: the behaviour of potential offenders and the behaviour of victims who decide whether, when, and how to involve the state. These margins are often difficult to separately identify empirically.

This paper studies whether legal recognition and increased sentencing severity can reduce domestic violence, carefully disentangling the effects on perpetrator offending and victim reporting. We examine the recent Brazilian law on femicide that was implemented in March 2015, the *Lei do Feminicídio* (*Lei No. 13.104/2015*), which amended the Penal Code by defining femicide as a qualified homicide and placing it among heinous crimes. Before the reform, the killing of an intimate partner could either be treated as simple homicide, with a prison sentence of 6 to

20 years, or as qualified homicide, with a sentence of 12 to 30 years. The 2015 law removed this ambiguity for gender-based killings involving domestic and family violence and raised the expected sanction for the most severe form of violence against women ([Presidencia da Republica, 2015](#)). Although the law targeted fatal violence, its stated broader purpose was to prevent escalation of violence in domestic settings.

We use ten years of linked administrative records for the universe of individuals residing in the state of Rio Grande do Sul, Brazil. The data combine identity-register information with police and prison records. The police records identify domestic-violence cases through eligibility for urgent protective measures, allowing us to observe not only victimisation but also the victim’s subsequent choices: whether she requested protective measures and whether she pressed charges. Our main sample contains 2.6 million women aged 18–55, 257,846 domestic-violence victims, and 427,835 occurrence-victim observations from 2012 to 2019.

Because the reform was implemented nationally, there is no untreated comparison group within Brazil. We therefore estimate difference-in-differences models using a continuous treatment intensity measure ([Callaway et al., 2024](#)). Treatment exposure is defined as the predetermined share of men aged 18–55 in a woman’s municipality of birth who had committed at least one violent crime against a woman at baseline. The design compares the evolution of domestic violence after March 2015 between women born in municipalities with different pre-reform exposure to men with a recorded history of violence against women. The identifying assumption is that, absent the reform, high- and low-exposure municipalities would have followed parallel trends in domestic violence. We test this assumption using event studies, non-parametric diagnostics for continuous treatment designs, falsification outcomes, and placebo exposure measures.

Our findings show that the reform reduced domestic violence incidence. In our preferred specification, which accounts for individual heterogeneity via fixed effects and flexible trends in predetermined municipality controls, moving from the 25th to the 75th percentile of baseline exposure reduced the probability of domestic-violence victimisation by 3.8% relative to the pre-reform mean. The effect appears after the law, with no differential pre-trend across localities differentially exposed to the reform. It is robust to a variety of sample selection choices (including all municipalities, restricting the sample to urban municipalities), using alternative exposure measures based on explicit domestic violence cases at baseline, and defining the treatment at municipality of residence instead of that of birth. The estimated effect is specific to domestic violence: we find no comparable reduction in non-domestic violent victimisation of women, and a placebo exposure measure based on men who committed violent crimes against men does not predict the post-reform evolution of domestic violence.

The reduction is concentrated in serious non-fatal violence, namely offences classified as bodily injuries, threats, and fighting. Because fatal events are extremely rare, estimates for femicide are negative but imprecise. This pattern demonstrates that a law that increased penalties for femicide successfully reduced violent acts that fall short of femicide. The evidence therefore points to prevention of escalation rather than only punishment after fatal violence has occurred.

We further test whether an incapacitation effect, deterrence effect, or even both, might be at play. If the reform reduced domestic violence by removing offenders from households through longer prison spells, the effects should be larger for repeat victimisation and should coincide with changes in imprisonment. We find that the effects are similar for first and repeat domestic-violence episodes, and administrative prison records show no detectable increase in the probability or length of imprisonment after domestic-violence incidents. Together with the imprecise effects on femicide, these results suggest incapacitation is not the main channel and, instead, are more consistent with a deterrence interpretation.

We further analyze whether the effects materializes through reduced perpetration, reduced victim reporting, or both. This distinction is important: if the law simply reduced reporting, then true rates of domestic violence may be unchanged despite the reduction in reported violence. We find that victims in more exposed areas were more likely to request protective measures after the reform. They also requested protection earlier, with a higher probability of doing so at the first recorded episode. In contrast, the strictest specifications show no increase in pressing charges. This suggests that the reform changed how women engaged with the state, increasing demand for protection without a corresponding rise in criminal prosecution. This response is consistent with a model in which the law increased women’s perceived protection from the state, raised the expected cost of escalation for abusive partners, and allowed victims to seek safety while limiting the private cost of prosecution.

Our paper contributes to four literatures. First, it contributes to the economics of intimate-partner violence. Recent work studies how violence responds to women’s labour-market opportunities, income, household bargaining power, relationship dynamics, and male economic shocks (Aizer, 2010; Anderberg et al., 2016; Adams et al., 2024; Bhalotra et al., 2025b). This literature documents the critical role played in abusive relationships by economic suppression since the start of cohabitation (Adams et al., 2024) and it reaches mixed conclusions on the link between a woman’s bargaining position in the household and DV, which appears negative in some studies (Aizer, 2010; Hidrobo and Fernald, 2013; Anderberg et al., 2016; Bobonis et al., 2013; Hidrobo et al., 2016; Buller et al., 2018; Haushofer et al., 2019) and positive in others (Hidrobo and Fer-

nald, 2013; Alonso-Borrego and Carrasco, 2017; Erten and Keskin, 2018; Bhalotra et al., 2025b). Other studies also show that men’s abusive behaviour increases during recessions (Schneider et al., 2016), during natural disasters (Catarino et al., 2015), following unexpected losses in sporting events (Card and Dahl, 2011; Ivandić et al., 2024). Some articles also study the potential to combat domestic violence of education policy (Gulesci et al., 2020), migration policy (Muchow and Amuedo-Dorantes, 2020), divorce laws (Brassiolo, 2016), national cash transfers (Heath et al., 2020), integration of women in police (Miller and Segal, 2019), the creation of all-women’s justice centres (Kavanaugh et al., 2018) or women’s police stations (Perova and Reynolds, 2017; Amaral et al., 2021), mandatory arrest laws (Iyengar, 2009; Chin and Cunningham, 2019), and no-drop policies that require the prosecutor to continue the prosecution even if the victim expresses a desire to drop charges (Aizer and Dal Bó, 2009). Related evidence from Puerto Rico shows that credible judicial protection and access to specialized DV courts can reduce repeat violence and increase the probability that judges issue a protection order (Arteaga et al., 2025). Our paper complements these studies as it investigates the scope for a different policy, i.e., legal recognition of femicides and increased sentences in court, to combat domestic violence. As such, this study adds novel evidence on a criminal-law margin: the legal recognition and severity of sanctions for gender-based violence.

Second, the paper contributes to the literature on laws, institutions, and violence against women. Existing work studies divorce laws, unilateral exit rights, domestic-violence criminalization, women’s police stations, female police officers, and justice centres (Stevenson and Wolfers, 2006; Brassiolo, 2016; Garcia-Ramos, 2021; Corradini and Buccione, 2023; Ferraz and Schiavon, 2022; Perova and Reynolds, 2017; Amaral et al., 2021; Miller and Segal, 2019; Sviatschi and Trako, 2024). A related paper by Sanin (2025) studies when domestic-violence laws work in Rwanda and Uganda, emphasizing divorce, bride-price refunds, and the social cost of exiting violent marriages. Our setting and mechanism are different. We study a legal-recognition reform in Brazil, focus on serious non-fatal violence, and observe victims’ use of protective measures in administrative police records. The results show that criminal law can matter even when the main behavioural response is not marital exit, but earlier recourse to state protection.

Third, the paper speaks to measurement. The recent review by Shah and Barski (2025) emphasizes that IPV research is constrained by underreporting, survey measurement, and the difficulty of distinguishing changes in incidence from changes in reporting. Our administrative data do not eliminate underreporting, but they let us test reporting-based explanations directly. We show that the reform does not reduce the probability that the victim is the caller, does not generate similar effects for non-domestic violence, and also reduces cases of serious domestic violence and hospitalisations. The evidence is therefore hard to reconcile with a pure reporting

story.

Fourth, the paper informs the design of legal protection. The DataSenado survey reports that almost half of victims who requested protective measures experienced a breach by the aggressor ([Instituto de Pesquisa DataSenado and Observatorio da Mulher contra a Violencia, 2023](#)). Protective measures are therefore not only an outcome; they are a policy margin whose credibility can shape behaviour. Our results suggest that legal recognition and sentencing severity can raise the value of this margin by changing beliefs about state protection and by increasing the expected cost of escalation. The law reduced non-fatal violence not only because offenders faced a harsher legal environment, but because victims acted sooner and sought protection more often. Because our research design identifies the reduced-form effect of the Femicide Law and the reform combined changes in statutory classification, expected punishment, public salience, and legal recognition, the estimates should not be interpreted as isolating sentencing severity from these other components.

The rest of the paper is structured as follows. Section 2 presents the main theoretical considerations underpinning our empirical analysis. Section 3 presents the data and the setting, while Section 4 illustrates our empirical approach. Section 5 discusses our results for the incidence of DV. Section 6 presents an array of robustness checks, while Section 7 examines the mechanisms at play. Section 8 concludes.

2 Theoretical considerations

Before delving into the technicalities of our empirical analysis, this section discusses a simple framework to help organize the evidence. Consider a relationship in which the perpetrator chooses a level of violence and the victim chooses whether to seek state protection. Violence gives the perpetrator control or expressive utility, 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, it raises the expected cost of escalation to lethal violence. Even if threats, fighting, and bodily injuries are not the statutory object of the reform, they are part of the same escalation dynamic. A partner who expects severe punishment after escalation may reduce violence before it approaches the lethal margin. This behaviour would be consistent

with a standard deterrence channel (Becker, 1968).

On the victim side, the law can change beliefs about the state. By defining femicide as a distinct and severe crime, the state signals that violence against women is not a private matter or a crime of passion. This can increase the expected return to asking for protection. 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. In this sense, empowerment 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. 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 sentencing-severity reform.

The model yields three empirical implications. First, violence should fall most in places with higher baseline exposure to violent men. Second, if incapacitation is the main channel, effects should be stronger for repeat cases and should coincide with longer or more frequent imprisonment. Third, if victim empowerment is central, victims should request protection more often and earlier. The evidence supports the first and third predictions and rejects the second. Overall, this pattern is most 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.¹

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 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 household². Furthermore, the *Lei Maria da Penha* also advocated 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 remain 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.³

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 Law 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

¹Although 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.

²Courts may also prohibit the aggressor from visiting specific locations, suspend firearm possession, and establish temporary custody or financial support arrangements

³IBGE, Pesquisa de Informacoes Basicas Municipais.

discrimination against the victim’s status as a woman.⁴

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 (Cevid) 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.”⁵

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 2019.⁶ 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 severity of

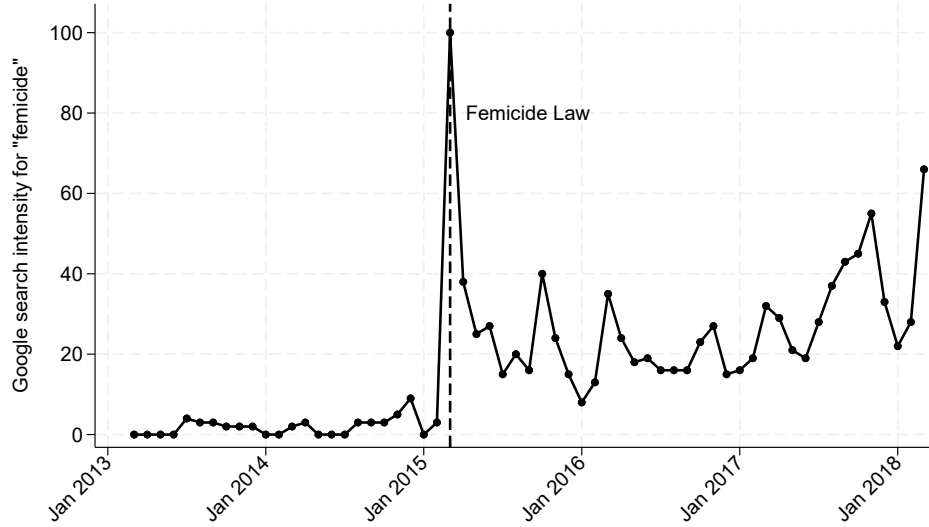
⁴The penalty for femicide is further increased from 33% to 50% if the crime is committed during pregnancy or in the 3 months following birth; against a person under 14 years of age, over 60 years of age, or with a disability; or in the presence of a descendant or ascendant of the victim.

⁵Her full interview is available here (last accessed on 04 May 2025): [Brasil de Fato, April 1, 2025](#).

⁶Figure 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 Femicídio*, arguably mitigating the concern that the older law might act as a potential policy confounder during the period of our analysis.

sanctions.

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 “feminicide” 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 brought to the police by victims 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 ⁷. The police records include 15,594,108 incidents reported to the police between 2010 and 2020.⁸ 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, solicited, 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 in RGS. 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 independent 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 accordance with the 2006 Law, once a victim of DV requests protective measures, the police officer should forward the case to the courts within 48 hours, and upon reception of the request, the judge in charge has a further 48 hours to decide on the protective measures to be applied.⁹

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.

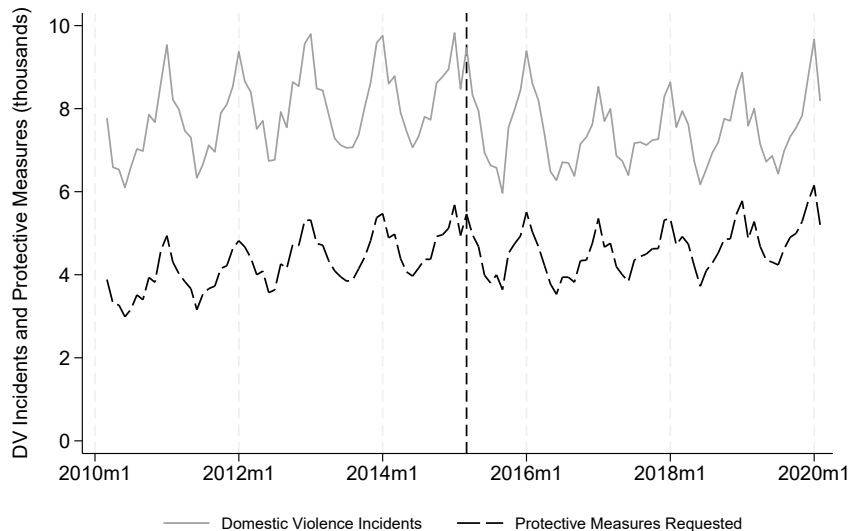
⁷Schooling history and civil status are missing for 26% and 0.5%, respectively.

⁸To avoid COVID-19, we use the period up to February 2020.

⁹In 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.

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 the cases in which the victim pressed charges without requesting protection became increasingly rare after 2015.¹⁰ 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

¹⁰In particular, Figure C2 shows that the count of cases for which the victim both pressed charges and solicited protection did not really change over time; likewise, the count of cases for which the victim neither pressed charges nor solicited 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 solicited 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.

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 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.¹¹ As a result, the final sample is composed of 2,557,114 women and 17,991,919 woman-period observations.¹² 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 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

¹¹Overall, the majority of research indicates an increase in domestic violence during the Covid-19 pandemic, although some studies report mixed or contradictory results (Campedelli et al., 2020; Payne et al., 2020). McCrary and Sanga (2021) find a 12% average increase in domestic violence in 14 US cities during the pandemic, with a significant rise in first-time abuse incidents. Leslie and Wilson (2020) observe a 7.5% increase in domestic violence calls in the US during Spring 2020; while Bullinger et al. (2021) show that the lockdown led to more police calls for domestic violence but fewer official reports and arrests in Chicago, suggesting a discrepancy between reported incidents and police action. Miller et al. (2020) highlight heterogeneous effects of the pandemic on domestic violence in Los Angeles, with increases in calls but decreases in arrests and crime incidents. Internationally, Ravindran and Shah (2020) find significant increases in domestic violence complaints in Indian districts with the strictest lockdowns, while Silverio-Murillo et al. (2020) report stable or increased hotline calls but initially decreased police reports in Mexico City. Arenas Arroyo et al. (2020) document a 23% rise in intimate partner violence in Spain during the lockdown, while Beland et al. (2020) find that the inability to meet financial obligations and maintain social ties significantly increases domestic violence in Canada.

¹²Of 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.

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, only in 9.23% 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.¹³

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., 2024) - 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., 2025a).

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.¹⁴ 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 2011¹⁵. Formally, for municipality m , we define:

¹³Details of data sources and sample construction can be found in [Appendix A](#).

¹⁴Although 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.

¹⁵We 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 \(2025\)](#).

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

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* risk of encountering men who 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 at risk, but rather increased the expected sanction associated with violent behaviour against women. 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.¹⁶ 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.

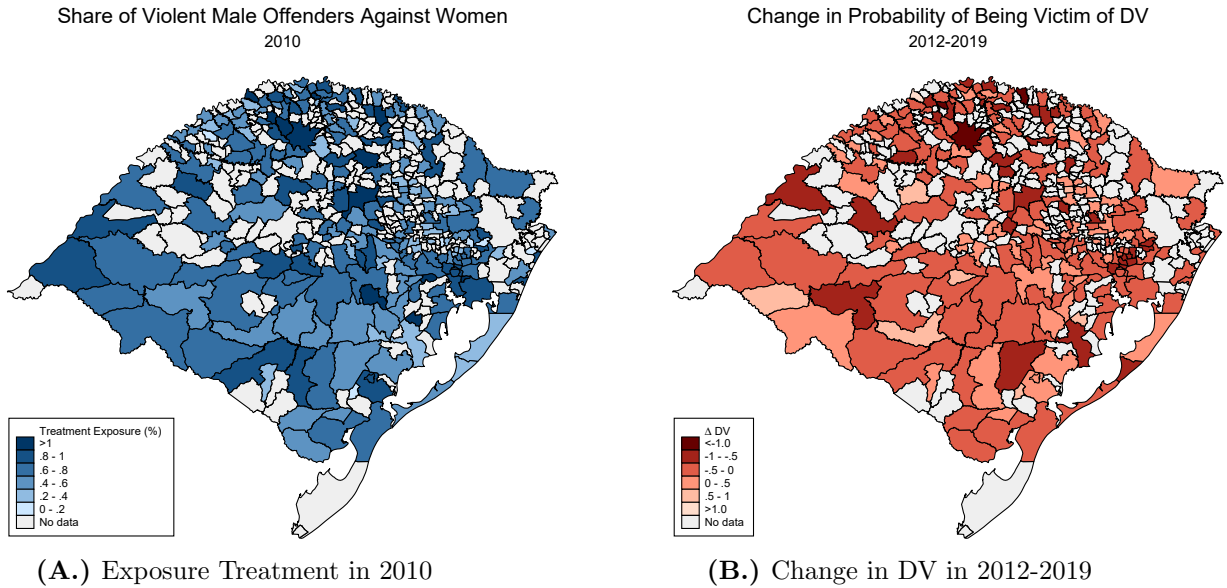
Figure 3 shows two maps of municipalities of RGS. In particular, Figure 3A. illustrates the cross-sectional variation in the treatment measure ($EXP_{m(i)}^f$) and it displays significant dispersion of the treatment exposure across areas. While a woman born in the municipality corresponding to the 25th 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

¹⁶Nonetheless, more than half of our sample continues to reside in their municipality of birth.

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.

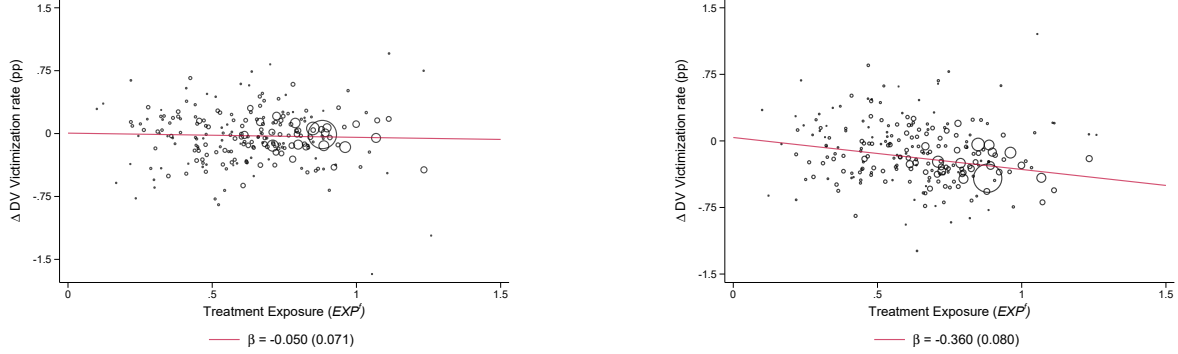
The scatter plots in Figure 4 display the change in DV victimisation against the treatment exposure ($EXP_{m(i)}^f$) separately for the *pre*- and *post*-reform periods, thus providing a more transparent view of the underlying variation in DV across localities and over time. Figure 4A. shows no discernible 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 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 (Appendix B).

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.

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 being a victim of domestic violence, beyond the variation captured by treatment exposure. The following equation can be estimated to retrieve the causal 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. \(2024\)](#) 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. \(2024\)](#), [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. \(2024\)](#).¹⁷ 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. \(2026\)](#).

¹⁷[de 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.

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 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 a 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 domestic violence 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

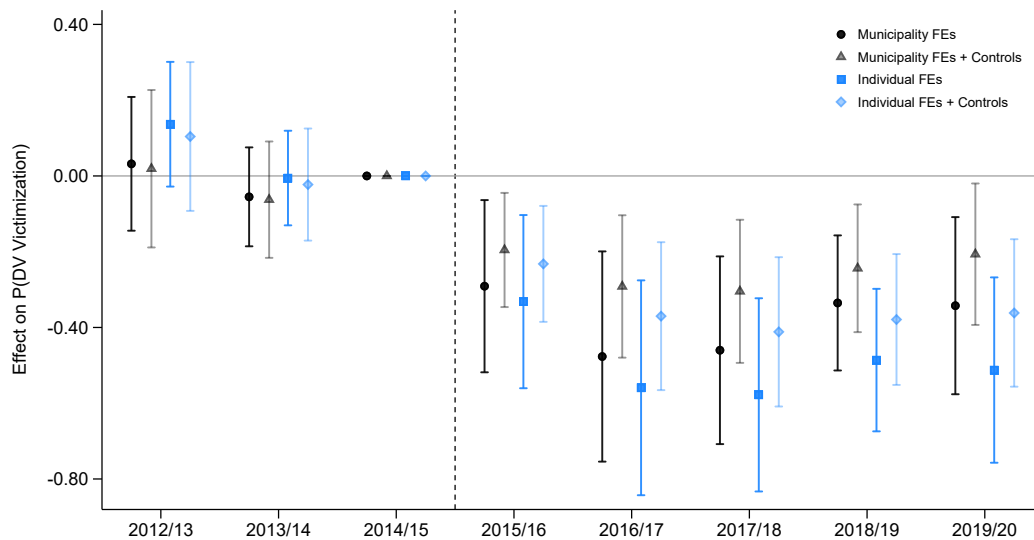
	Probability of DV			
	(1)	(2)	(3)	(4)
Post×Exposure	-0.384*** (0.095)	-0.241*** (0.054)	-0.532*** (0.099)	-0.372*** (0.056)
Observations	17,991,919	17,991,919	17,991,919	17,991,919
Pre Mean of Outcome	2.119	2.119	2.119	2.119
Range of Exposure	0.215	0.215	0.215	0.215
Effect wrt Mean	3.900	2.443	5.400	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 levels, 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. Standard errors are clustered at the municipality level. Coefficients, pre-reform means, and the range of exposure 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 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 dis-

plays 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 test on standard parallel trends is useful but not enough 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 distribution. 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.¹⁸ 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 (2017/2018) 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.

¹⁸Because 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 de Chaisemartin and D’Haultfoeuille (2024). 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.

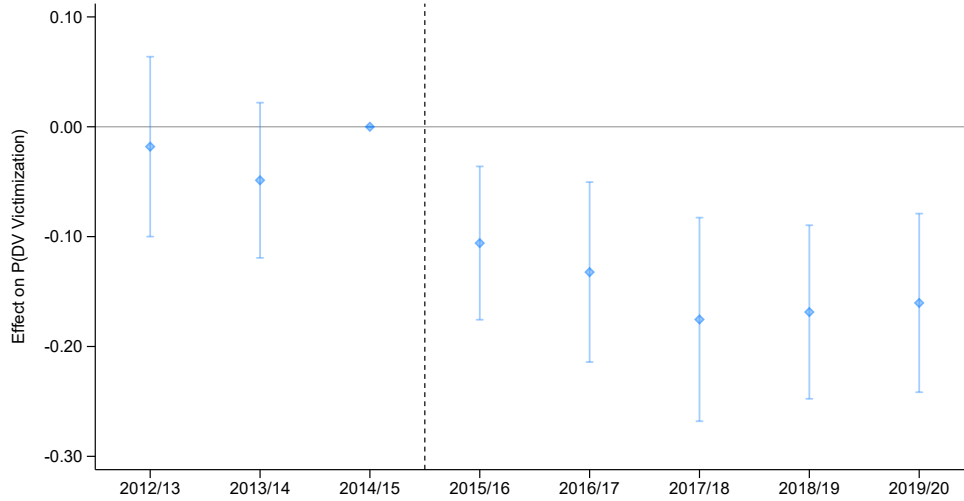
Thirdly, we exclude municipalities with zero exposure, as these may, *de facto*, reflect measurement error and mechanically act as control units in our setting, while accounting for a negligible share of the sample.¹⁹ This restriction is also in line with the recent DiD literature with continuous treatments. 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-

¹⁹Only 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)²⁰. 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

²⁰This 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.²¹ 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.

²¹This 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, pre-reform means, and the range of exposure are scaled by 100 (percentage points). The range corresponds to the interquartile range. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean. Standard errors are clustered at the municipality level. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

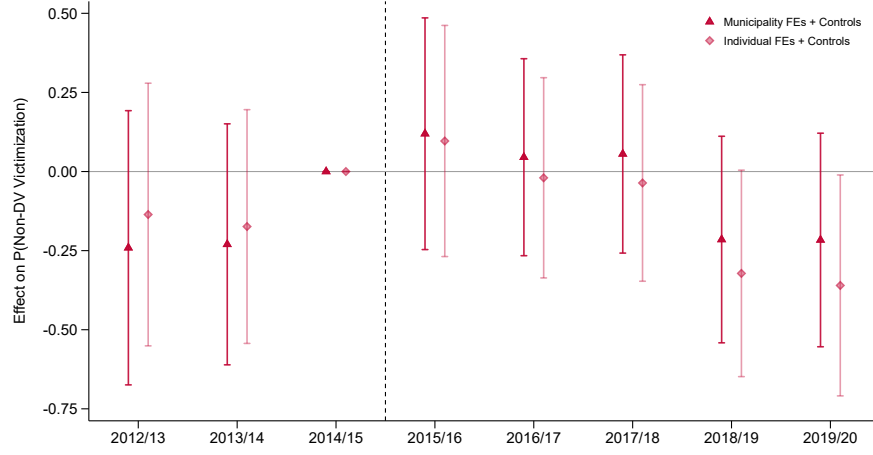
Placebo Test. A complementary approach is to construct an alternative exposure measure that captures similar underlying factors but should not affect DV victimisation directly. We do so by constructing the male analogue of our treatment exposure, defined as the share of violent male offenders against 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 Ta-

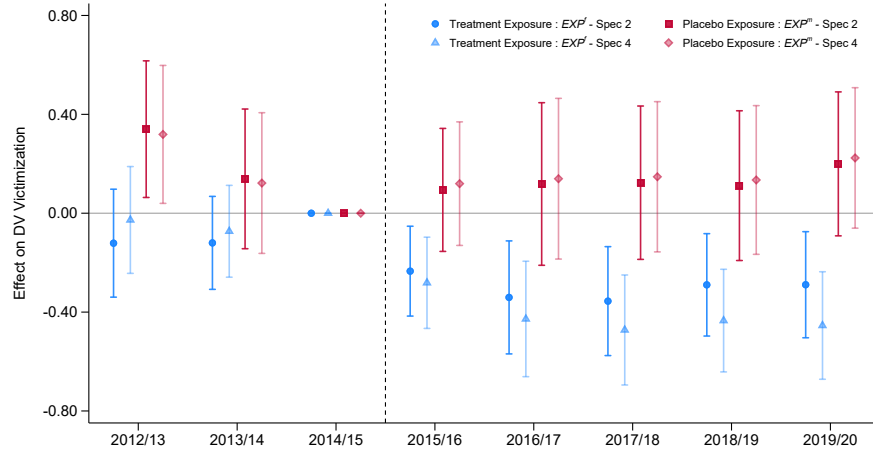
ble 3 show that including this placebo exposure does not alter our main findings. The interaction term $Post_t \times EXP_{m(i)}^f$ remains negative and precisely estimated, with very similar magnitudes to those in Table 1. In contrast, $Post_t \times EXP_{m(i)}^m$ is small and statistically insignificant 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 centered 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.²² 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.²³ 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, thus arguably pointing in the direction of some backlash in DV generated by this reform, 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)	(2)	(3)	(4)	(5)	(6)	(7)
	Any DV	Femicide	Bodily Injuries	Threat	Fighting	Disorderly Conduct	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, pre-reform means, and the range of exposure are multiplied by 100. The range corresponds to the interquartile range. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean. Standard errors are clustered at the municipality level. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

²²These include, for example, defamation and false claims against one’s partner.

²³The 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 attention in the political debate and in the media to 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 introducing harsher sentences for DV offences, 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)²⁴. Panel A of Table 5 shows that negative and numerically consistent estimates of the effect of the reform on the likelihood that a DV victim is hospitalised for injuries still appear, albeit not statistically significant when individual fixed effects are included in the analysis.

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.

²⁴Our baseline specification uses the full sample, while robustness checks exclude cells in the top 5% and top 1% of the distribution of cell size (approximately 6 million and 3 million observations, respectively). Results are consistent across these alternative samples.

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 hospitalisation for injuries (ICD codes S and T), from Brazil's SIH records matched to individuals by date of birth, sex and municipality. 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 rather than the victim herself. In Panels A and B, specifications (1) and (2) follow Columns (2) and (4) of Table 1. In Panel C, the specifications include offence-type and police-station fixed effects, consistent with Section 7. Coefficients, pre-reform means, and the range of exposure are multiplied by 100. Standard errors are clustered at the municipality level. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6 Robustness

In our main analysis, the outcome of interest is a dummy 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, the 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 strong 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 domestic violent 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, our results are robust to these alternative definitions of treatment, indicating a strong decrease in the probability 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 very consistent with our main results.

7 Mechanisms

7.1 Incapacitation, Deterrence, or Both?

Why did Brazil’s *Lei do Feminicídio* reduce the incidence of non-fatal domestic violence 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.

7.2 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.²⁵ 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.

²⁵Because 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, pre-reform means, and the range of exposure are multiplied by 100. Standard errors are clustered at the municipality level. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

7.3 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 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 feminicides. However, taken together, the results in Table 4 for feminicides, 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.

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

	(1) Any Imprisonment	(2) Average Stay (Days)	(3) 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: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. This table tests whether the *Lei do Feminicídio* reduced domestic violence through an incapacitation channel. Column (1) models the probability of imprisonment following a DV occurrence. Column (2) models the average prison stay (in days), and Column (3) models the probability of imprisonment for more than 30 days; Columns (2) and (3) are estimated conditional on imprisonment during the study period, which explains the smaller sample and reduced municipality count. The dependent variables in Columns (2) and (3) are winsorised at 5 years. 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. Coefficients, pre-reform means, and the range of exposure are multiplied by 100. The range corresponds to the 25th–75th percentile difference. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean from moving across this interquartile range. Standard errors are clustered at the municipality level.

7.4 Deterrence. Behavioural Responses

One alternative hypothesis is that Brazil’s Femicide Law may have signaled to victims that the state had started to take domestic violence more seriously, thereby increasing trust in public authorities and reducing fears of retaliation by abusive partners following the decision to press charges. This, in turn, may have exerted a deterrence effect on offenders and may have prevented the escalation of violence by abusive men. The significance in Table 4 in the estimated effect of the reform on non-fatal, serious categories of DV (i.e., bodily injuries, threats and fighting) arguably makes this a likely mechanism, since the reform introduced harsher sentences for fatal DV and not for these, non-fatal DV categories. In the absence of a deterrence effect that generated spillovers on non-fatal violence not directly targeted by this reform, one would not expect to find significant estimates for non-fatal DV crimes. In addition, a set of empirical tests is presented here to further establish whether a deterrence effect might be at play and why.

The current section investigates 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, as well as any combination of the two. This analysis is done at the incident-victim level, i.e., with an intensive margin, to better investigate the specific events of DV.²⁶ 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 introduction of the reform. This extension in the modelling 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 will absorb time-invariant differences in resources and specialization of police forces.

²⁶ [Appendix D](#) shows the results for behavioural responses when using the same design of the first part, i.e., the panel structure of the data.

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) to (4) progressively add control variables to the estimated equation. These estimates indicate that, regardless of the set of controls, the propensity of victims to seek protective measures increased due to the introduction of the *Lei do Feminicídio*. 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 1.17% to 1.97% compared with municipalities at the 25th percentile of our treatment exposure measure. 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 substantive empirical support to this claim.²⁷ 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 solicit 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.

A natural interpretation of the findings in Table 8 and Figure 8 is that Brazil’s Femicide Law may have made women feel more protected by the state. By removing any ambiguity in the treatment for abusive partners and formally introducing the femicide offence in the Brazilian penal code, the reform plausibly signaled to victims that the state had started to take DV seriously, which may have increased victims’ perceived protection from public authorities and request more protective measures in response to DV episodes. This, in turn, helps rationalize the significance in the results for non-physical forms of DV too, i.e., threats, in Table 4: the greater empowerment of DV victims evidently exerted a deterrence effect on offenders that prevented the escalation of both physical and non-physical non-fatal violence by abusive men.

An additional way to appraise the validity of this conclusion is to test whether the reform

²⁷Table 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 soliciting protective measures and a statistically significant increase in the likelihood to request protective measures without pressing charges.

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

	(1)	(2)	(3)	(4)
<i>Panel A: Solicited 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

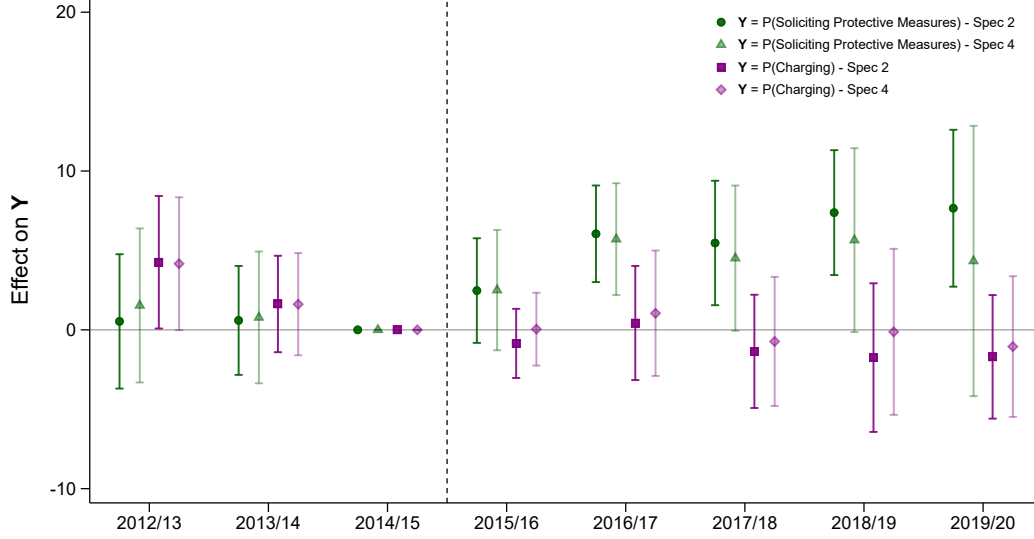
Notes: This table reports estimates of the impact of the *Lei do Feminicídio* on victims' behavioural responses following a DV incident. The analysis is conducted at the occurrence-victim level. Panel A models an indicator equal to one if the victim solicited protective measures from the police. Panel B models an indicator equal to one if the victim pressed charges against the offender. The two outcomes are not mechanically connected, since a victim need not press charges to request protective measures. 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, pre-reform means, and the range of exposure are multiplied by 100. The range corresponds to the 25th–75th percentile difference. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean from moving across this interquartile range. Standard errors are clustered at the municipality level. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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, which explains the smaller sample size. Column (1) models an indicator equal to one if the victim waited beyond the first DV episode before requesting protective measures; a negative coefficient indicates that victims sought protection earlier in the sequence of abuse. Column (2) models the count of DV episodes preceding the request for protective measures. Column (3) models the number of days between the DV incident and the request for protective measures. Column (4) extends the sample to all DV occurrences and models the time elapsed between the DV incident and the date of reporting to the police, providing a falsification test for whether the results in Columns (1)–(3) reflect a generalised change in reporting behaviour rather than the timing of protection-seeking. All specifications follow Column (4) of Table 8. The estimated effect on the days between consecutive DV episodes within the two-year spell is not statistically significant, suggesting that the results do not simply reflect a mechanical compression of the time window. Coefficients, pre-reform means, and the range of exposure are multiplied by 100. The range corresponds to the 25th–75th percentile difference. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean from moving across this interquartile range. Standard errors are clustered at the municipality level. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 8: Event Study Estimates of Impact of the *Lei do Feminicídio* on Soliciting 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.

may have induced DV victims to wait less before requesting protective measures. For this, the analysis was conducted at the incident level and was restricted to DV victims who ever requested protective measures. Table 9 shows the results of this exercise. In particular, Column (1) shows that Brazil's *Lei do Feminicídio* decreased the likelihood that women wait until after the first DV episode to request a protective measure; put differently, this result indicates that the reform increased the likelihood that DV victims seek protective measures at the earliest occurrence of DV. Column (2) models the dependent variable as the continuous count of DV episodes before protective measures are requested. The point estimate suggests that victims waited for fewer DV episodes before seeking protective measures, 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. It shows that, as a result of the reform, DV victims started to wait for fewer days before seeking protective measures. 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 the results in Columns (1)-(3) are not picking up a generalized change in reporting behaviour in more treated areas in our study period.

8 Conclusion

Domestic violence is common, costly, and hard for the state to deter. It occurs within relationships in which victims and offenders may share a home, children, income, and social ties. For this reason, criminal-law reforms can have effects that differ from those predicted by standard models of crime. Formal legal recognition and harsher sanctions may deter offenders, but they may also change whether victims seek help, request protection, or press charges. This paper studies these margins in the context of Brazil’s 2015 Lei do Feminicídio.

We use linked administrative records for the universe of individuals in Rio Grande do Sul and exploit cross-municipality variation in women’s predetermined exposure to men with a history of violence against women. Our findings indicate that domestic violence fell more after the reform among women born in municipalities with higher baseline exposure to men with a history of violence against women. The decline is concentrated in serious non-fatal violence, including bodily injuries, threats, and fighting. The result is robust across specifications, samples, exposure measures, placebo tests, falsification outcomes, and event-study diagnostics.

We find limited evidence of a broad incapacitation response in the recorded data. Although the null imprisonment and repeat-episode results do not rule out incapacitation among a narrower group of high-risk offenders directly affected by femicide charges, the incapacitation of abusive partners does not appear to be the key ingredient behind the fall in non-fatal domestic violence documented here. Instead, our results indicate that the reform changed victims’ interaction with the state. Women became more likely to request protective measures and did so earlier in the sequence of abuse. These responses are consistent with a mechanism in which the law made victims feel more protected by the state, raised the expected cost of escalation for offenders, and helped prevent violence before it reached the fatal margin. Because the reform also changed the incidence of domestic violence, these estimates should be interpreted as strongly suggestive, but not definitive, evidence that the same victims changed behaviour.

The results contribute to the economics of domestic violence by showing that sentencing severity can reduce violence within intimate relationships, but through a channel that depends on victims’ agency. The law does not appear to have worked only by threatening offenders with harsher punishment after femicide. It also changed the set of actions available to women before

violence escalated. In this sense, the reform operated not only as a deterrence policy, but also by strengthening victims' willingness and ability to seek protection earlier.

This distinction also matters for policy. While the reform produced robust and coherent reductions in domestic violence, the magnitudes are relatively modest, suggesting that legal recognition and harsher sentencing alone is unlikely to be a sufficient response to gender-based violence. Rather than diminishing the importance of the reform, this helps clarify where its most meaningful effects may have emerged. Our findings suggest that much of the law's impact operated less through large changes in offender behaviour and more through changing how victims perceived and used state protection, particularly by increasing awareness, perceived protection, and earlier engagement with the justice system. This is particularly relevant given that the *Lei do Feminicídio* was revised again only recently, further increasing statutory penalties²⁸. If so, repeatedly raising sanctions may face limited returns unless accompanied by policies that strengthen victims' practical ability to act. The more effective margin may lie not only in harsher punishment, but in making legal protection more accessible, credible, and actionable through stronger enforcement, clearer information, and lower barriers to reporting and protection. Sentencing severity may still matter, but its effectiveness is likely to depend on whether victims can realistically use the institutions designed to protect them before violence escalates.

The findings also speak to a broader question in the economics of law and gender. Legal reforms can reshape private behaviour even when enforcement is imperfect. By naming femicide as a specific crime and reducing ambiguity in its legal treatment, the Brazilian state changed both the expected consequences of violence against women and how victims responded to it. In our setting, this shift reduced serious non-fatal violence and led victims to seek protection sooner. The results suggest that law can matter before the worst outcome occurs: not only by punishing violence, but by changing behaviour in ways that help prevent escalation.

²⁸In October 2024, Law No. 14.994 introduced another revision to the *Lei do Feminicídio*, further raising penalties and reinforcing the central role of sentencing severity in Brazil's legislative response to gender-based violence. It transformed femicide into an autonomous offence (Article 121-A), increased penalties from 12–30 years to 20–40 years, and expanded related sanctions for other gender-based crimes.

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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 8,453,076 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, feminicide, 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 the individuals in our analysis based on date of birth and municipality. 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.

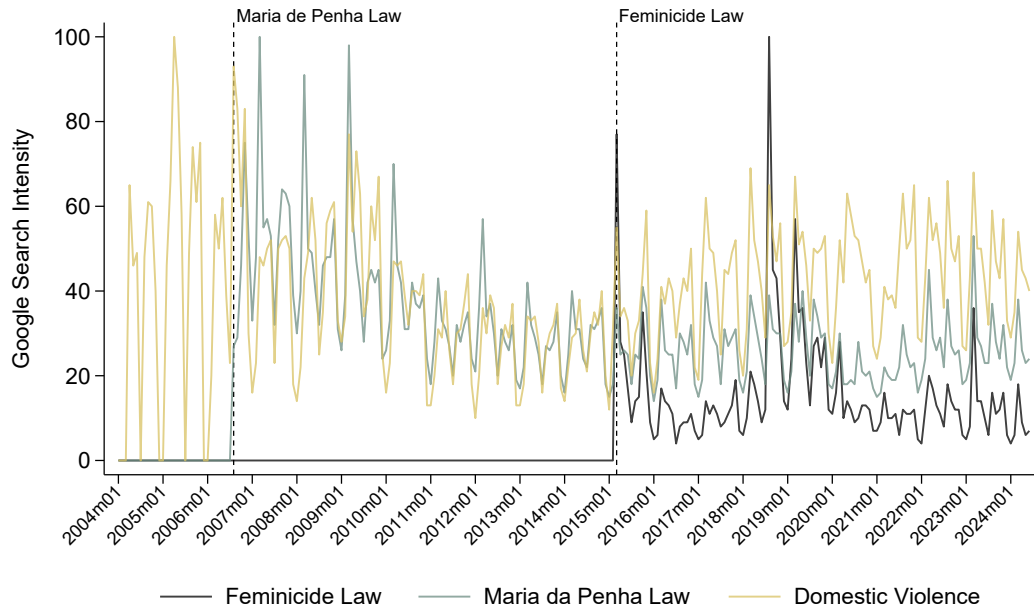
For Panel C of Table 5, which models the identity of the caller, we further merge this sample with kinship indicators from administrative records that report the link between any two individuals in the data (“(ex-)partner”, “father”, “mother”, “son/daughter”, “sibling”). The outcome modelled in Panel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

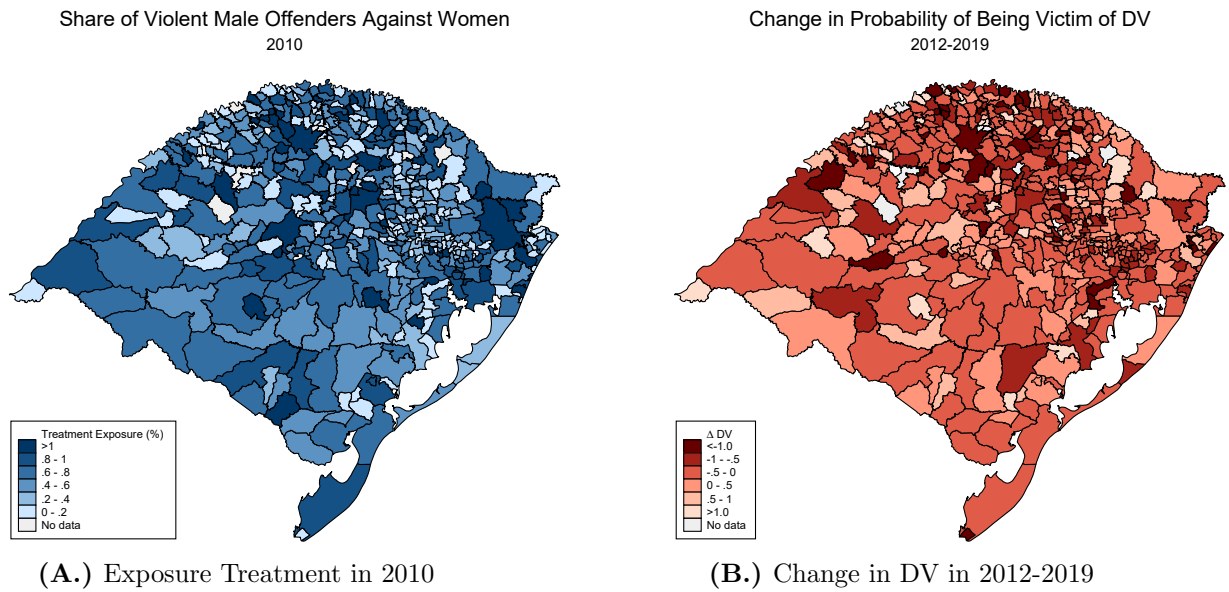
Appendix B

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 marks 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. 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.

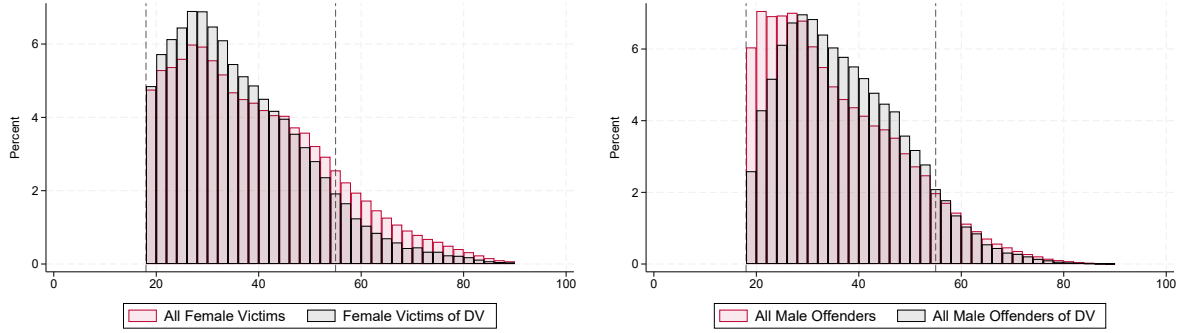
Figure B3: Event Study Estimates by Deciles of Treatment Exposure



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	

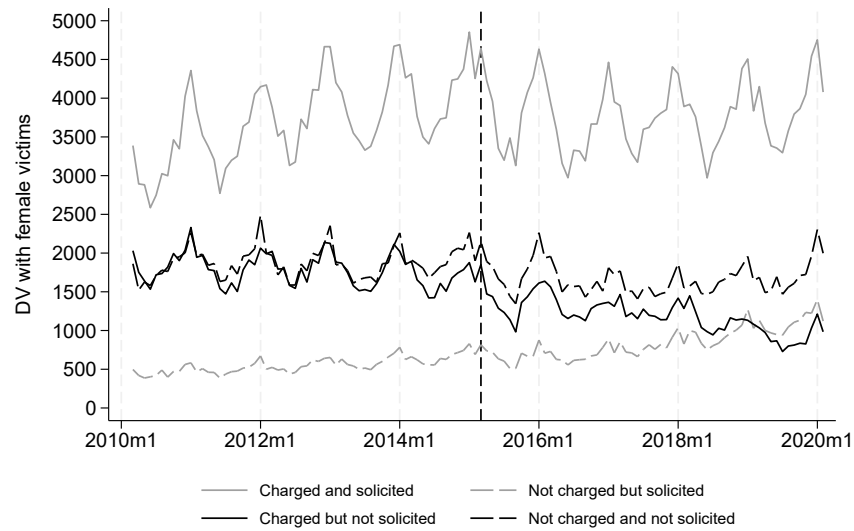
Notes: The table displays descriptive statistics for the 226 municipalities in Rio Grande do Sul used in our main specification. This information comes from the 2010 Brazilian Census.

Table C3: Descriptive Statistics (Incident-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	

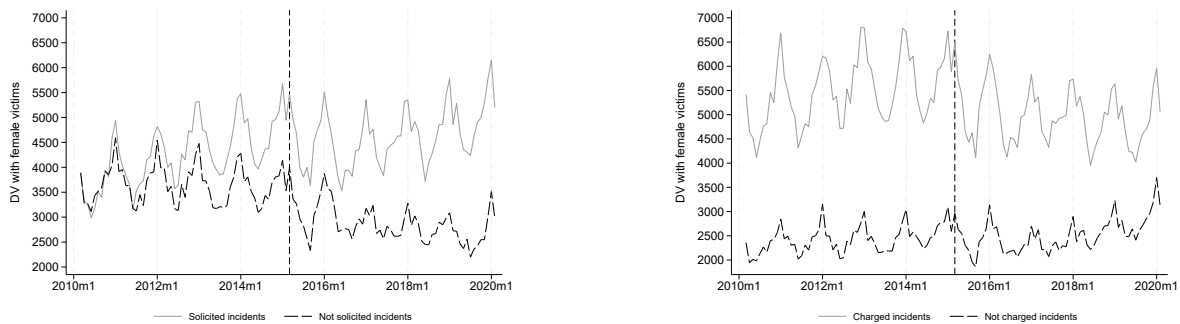
Notes: The table displays the mean and standard deviation for the sample of 427,835 domestic violence incidents explored in our incident-level analysis.

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



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

Figure C3: Trends of Solicited and Charged Incidents vs Not



(A.) Solicited 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 solicited 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)	(2)	(3)	(4)
	Not Charged Not Solicited	Charged Not Solicited	Not Charged Solicited	Charged Solicited
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: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. 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 solicited protective measures; Column (2) pressed charges but did not solicit protective measures; Column (3) did not press charges but solicited protective measures; Column (4) both pressed charges and solicited protective measures. All specifications follow Column (4) of Table 8. Coefficients, pre-reform means, and the range of exposure are multiplied by 100. The range corresponds to the 25th–75th percentile difference. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean from moving across this interquartile range. Standard errors are clustered at the municipality level.

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: Solicited 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: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. 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. Panel A models an indicator equal to one if the victim solicited protective measures, and Panel B an indicator equal to one if she pressed charges. Each column corresponds to a specific DV category: threat (1), bodily injury (2), fighting (3), and disorderly conduct (4). All specifications follow Column (4) of Table 8. Coefficients, pre-reform means, and the range of exposure are multiplied by 100. The range corresponds to the 25th–75th percentile difference. “Effect wrt Mean” reports the implied percentage change relative to the pre-reform mean from moving across this interquartile range. Standard errors are clustered at the municipality level.

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: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. 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.

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: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Column (1) reproduces the main result reported in Column (4) of Table 1. Column (2) includes all municipalities 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.