Don't Stick a Spoon in Marital Disputes? Sentencing Severity and Domestic Violence

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Preliminary and incomplete. Please do not circulate.

Abstract

We estimate the impact of sentencing severity on the dynamics of domestic violence. The study uses ten years of merged individual-level administrative registers on domestic violence cases brought to the police and family linkages for the entire state of Rio Grande do Sul (Brazil). Leveraging Brazil's "Lei do Feminicídio", which was implemented in March 2015 to include the crime of "femicide" in the Brazilian penal law, we find that sentencing severity significantly affects the behavior of both offenders and victims of domestic violence. While the policy change seemingly deterred potential offenders by reducing the incidence of domestic violence, victims of domestic violence became more likely to ask for protective measures and more reluctant to press charges against their abusive partners as a framework of compensating mechanisms would predict. For a policymaker seeking to design effective sentences to combat domestic violence, the tension between these outcomes appears critical.

1   Introduction

Domestic violence is one of the most widespread violations of human rights worldwide, with as many as one in three women experiencing domestic violence (DV) at some point in their lives (Garcia-Moreno et al., 2006). In Brazil, the empirical setting of our study, 4,762 female homicides and 4.8 female killings for every 100,000 people occurred in 2013 (Avila, 2018), averaging 13 per day and marking Brazil as fifth in the global ranking for female homicides. According to the National Secretary of Policies for Women in Brazil (2016), 4.7 million calls were made to the domestic violence hotline from 2005 to 2015, and 552,748 reports of domestic violence were filed in the same years. Police records show a rise from 4,258 cases of intimate partner violence (IPV) in 2008 to 13,100 in 2016, and recent evidence also suggests that 27% of 10,000 women surveyed in Northern Brazil experienced at least one act of DV in their lives, with 11.9% reporting such incidents in the last year alone (Carvalho and Oliveira, 2016).

Domestic violence is both a consequence and a driver of gender inequality in modern society. Still, unlike other dimensions of gender discrimination, such as the gender pay gap, it has received remarkably little attention from economists (Bhalotra et al., 2021). One reason for the limited amount of causal research on DV is that large-scale systematic data on it combined with policy reforms that can be evaluated are rare.

This paper studies the impact of sentencing severity on the dynamics of domestic violence. In particular, we study the effect of sentencing severity on both the incidence of DV and the behavior of DV victims. Studying the behaviour of DV victims is essential because, unlike most other crimes, DV is a type of crime where the victim and the offender typically share a family linkage, they may share children as well as a close personal relationship and financial interdependence. For these reasons, pressing charges against the offender typically entails a tough decision for a victim of DV. Moreover, especially for some forms of violence, such as controlling behavior and psychological violence, DV can be difficult to recognize by its victims. Even when victims realize the violence, they may face barriers to reporting, such as retaliation from the perpetrators, social sanctions of reporting, lack of knowledge about the existing reporting services, inability to access the reporting services, and low expectations about the outcomes of reporting. Therefore, the link between sentencing severity and DV does not appear obvious *a priori*, as harsher sentences might dissuade potential offenders from committing acts of domestic violence, but they may also dissuade victims from pressing charges.

We investigate the tension between these outcomes by examining the recent Brazilian law on femicide that was implemented in March 2015, the *Lei do Feminicídio* (*Lei No. 13.104/2015*).

This law, which is linked with Brazil's *Lei Maria da Penha* from 2006 (Ferraz and Schiavon, 2022), introduced the crime of *femicide* in the Brazilian law and made femicide equivalent to a qualified homicide, placing it on the list of heinous crimes with higher penalties ranging from 12 to 30 years. Our study uses ten years of administrative registers merged at the individual level on family linkages and DV cases brought to the police for the entire state of Rio Grande do Sul in Brazil (RGS). Detailed administrative records from the Instituto Geral de Pericias (IGP) and police reports from the police authorities (*Secretaria de Seguranca Publica - Policia Civil*) during the 2010-2019 period for the entire population of RGS are used to define a set of Difference-In-Differences (DiD) specifications with a continuous treatment exploiting the predetermined distribution of violent men against women across the municipalities of RGS. To be precise, our measure of treatment exposure to violent crime against women is defined as the share of men aged 18-55 who committed at least one violent crime against a woman at baseline in a woman's municipality of birth.

Our findings show that the *Lei do Feminicídio* deterred potential offenders as it caused a reduction in the incidence of DV. The effect is robust to different model specifications and sampling restrictions, as we reach the same conclusion regardless of whether the analysis includes all municipalities or is restricted to urban areas that display less measurement error and concentrate most DV cases. We probe the validity of the empirical modeling through a set of falsification and placebo tests that mitigate the potential concern that our treatment exposure measure may pick up some unobserved municipality-year-specific shock. The reduced incidence of DV aligns with the predictions of the Becker model of crime (Becker, 1968) where, *ceteris paribus*, harsher sentences should act as a deterrent and reduce crime.

Additional analysis reveals that the behavior of DV victims was also affected by the policy. In particular, our results show that DV victims increased their likelihood to request protective measures and became, to a certain extent, more reluctant to press charges against their abusive partners. Most of the change in the behavior of DV victims is driven by the increased propensity to solicit protective measures without pressing charges and, at the same time, by the reduced propensity to charge without soliciting protective measures. We rationalize these behavioral responses as a form of compensating mechanism whereby victims attempt to preserve their physical and psychological security while trying to avoid the financial loss and negative consequences of the extended incarceration period for their abusive partner. Therefore, unlike cases where the victim presses charges and solicits protective measures (or those in which the victim does not do either), we interpret these responses as marginal cases that are particularly representative of the complier population of DV victims.

Further analysis also shows that our results are not explained by a compositional change in the types of DV offenses observed over time, as including crime-fixed effects does not alter our main conclusion. Threats and bodily injuries are the main drivers of the reduced incidence of DV and they drive the increased propensity to request protective measures without pressing charges.

The contribution of this paper is twofold. First, it contributes to the literature on the dynamics of abusive relationships by presenting the first rigorous evaluation of the impact of sentencing severity on the incidence of DV. This literature documents the critical role played in abusive relationships by economic suppression since the start of cohabitation (Adams-Prassl et al., 2023) 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 Fernald, 2013; Alonso-Borrego and Carrasco, 2017; Erten and Keskin, 2018; Bhalotra et al., 2021). Other studies also show that men's abusive behavior increases during recessions (Schneider et al., 2016), during natural disasters (Catarino et al., 2015), following unexpected losses in American football (Card and Dahl, 2011) or following football games in England (Ivandić et al., 2024). By exploiting a policy change for identification, our paper retrieves a policy parameter that is of direct relevance for a policymaker interested in combating all these forms of DV.

Some papers study the potential to fight domestic violence for education policy (Gulesci et al., 2020), migration policy (Muchow and Amuedo-Dorantes, 2020), divorce laws (Brassiolo, 2016), national cash transfers (Heath et al., 2020), the integration of women in policing (Miller and Segal, 2019), the creation of all-women's justice centers (Kavanaugh et al., 2018) or women's police stations (Perova and Reynolds, 2015; Amaral et al., 2019), mandatory arrest laws (Iyengar, 2009; Chin and Cunningham, 2019), no-drop policies that compel the prosecutor to continue with prosecution even if the victim expresses a desire to drop the charges (Aizer and Dal Bó, 2009). Our paper complements these studies as it investigates the scope for a different policy, i.e., increased sentences in court, to combat domestic violence.

The second key element of the contribution of this study lies in the analysis of the behavioral response by DV victims to a reform that introduced the crime of femicide in the criminal justice system of Brazil. The existing evidence suggests that the repercussions of DV can extend far beyond the pain of the victim, and they can impose significant societal costs. These consequences comprise poorer labour market outcomes for victimized women (Bindler and Ketel, 2022; Folke and Rickne, 2022; Adams-Prassl et al., 2023), reduced birth weight and increased

under-5 mortality (Aizer, 2011; Currie et al., 2022; Rawlings and Siddique, 2020). A study of the behavioral response by the victims to a reform that increases the severity of sentencing is required because DV is a very particular type of crime where the behavior of the victim and her collaboration with the criminal justice system are critical.

In sum, we contribute to the literature by providing the first empirical study of the effect of sentencing severity on the dynamics of DV and by showing that sentencing severity can affect the behavior of both offenders and victims. The evidence presented here indicates that harsher sentences for DV can deter potential offenders, but they can also have unintended consequences, such as discouraging victims from pressing charges. For a policymaker seeking to design effective sentences to combat domestic violence, striking a balance between these outcomes is of primary importance.

The rest of the paper is structured as follows. Section 2 presents the data and the setting, while Section 3 illustrates our empirical approach. Section 4 discusses our results for the incidence of DV. Section 5 analyzes potential heterogeneities in terms of different types of crime, age groups, and first and repeated offenses. Section 6 performs robustness checks on the DV result, while Section 7 takes a different perspective, looking at the specific occurrences and investigating the behavior of DV victims. Section 8 examines types of crime, severity of DV, and family involvement. Section 9 concludes.

2   Data & Setting

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

The setting of this study is the Brazilian state of Rio Grande do Sul (RGS), the southernmost state of Brazil. Porto Alegre is its capital and largest urban centre and, with nearly 11 million inhabitants (i.e., approximately 5% of the Brazilian population in 2022), RGS is the fifth-most populous state in the country. Up until 2015, "femicide" was not explicitly included in the Penal Code of Brazil. 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 the Penal Code of Brazil provides for a prison sentence of 12 to 30 years. The variability in the 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.

Legislation aiming to explicitly protect victims of domestic violence in Brazil had first been introduced in 2006 with the *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. Furthermore, the *Lei Maria da Penha* also advocated the creation of specialized police stations for women's assistance, shelters, women's reference centers, 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.[1]

Against the backdrop of the unsatisfactory existent legal framework in dealing with severe domestic violence instances and the persistence of high levels of the phenomenon, the *Lei do Feminicídio* (*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 *Lei do Feminicídio* was the explicit inclusion for the first time in the Penal Code of Brazil of "femicide" as a new crime category. The *Lei do Feminicídio* defined "femicide" as a qualified homicide and it placed it on the list of heinous crimes with higher penalties ranging from 12 to 30 years. The goal of the *Lei do Feminicídio* 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 *Lei do Feminicídio* defines as "femicide" any attempted or consummated murder that involves domestic and family violence, as well as disregard or discrimination against the victim's status as a woman. Moreover, the penalty for femicide is further increased from 33% to 50% if the crime is committed in any of the aggravating circumstances enumerated below:

- 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;

- in the presence of a descendant or ascendant of the victim.

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), hence acting as a deterrent. At the time of its implementation in March 2015, the *Lei do Feminicídio* 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 keywords "Domestic Violence", "Maria da Penha Law" and "Femicide Law" in Brazil for the years 2004 to 2024. It becomes clear that a spike in searches for the *Lei do Feminicídio*

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[1]IBGE, Pesquisa de Informacoes Basicas Municipais.

occurred exactly in March 2015, the date of its enactment, from a prior baseline of close to zero searches. This demonstrates both the public interest salience of the femicide law upon its introduction and the lack of major widespread anticipatory knowledge of it. Two additional interesting observations can be made about Figure 1: firstly, the *Lei Maria da Penha* shows a similar pattern in search surge close to its implementation date from a very low level prior; secondly, 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. An important point to make is that 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 sanctions. Additionally, Figure 1 does not show a particular rise in searchers for the *Lei Maria da Penha* around the introduction of the *Lei do Feminicídio*. The previous points help in reducing concerns about the older law being seen as potential policy confounder during the period of our analysis.

Figure 1: Google Search Intensities for Maria da Penha Law, Femicide Law & Domestic Violence
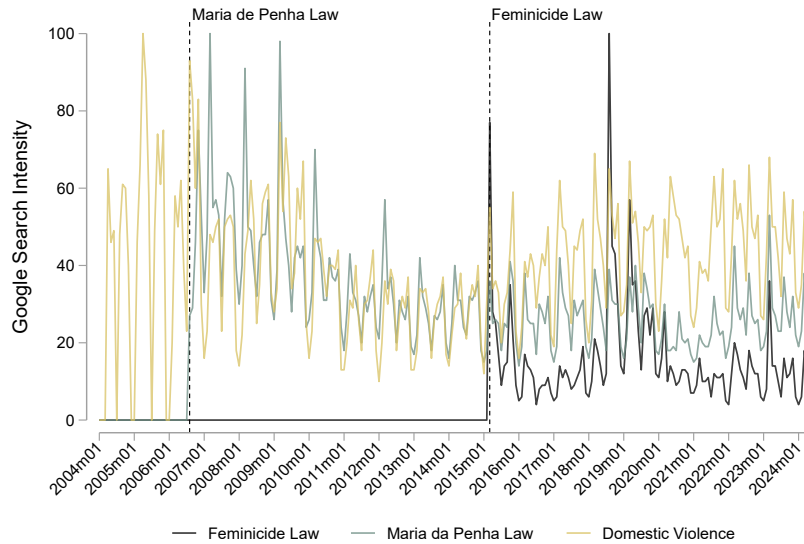
Notes: The figure displays Google search intensities for different keywords: in yellow, "Domestic Violence"; in grey, the "Maria da Penha Law" and, in black, the "Femicide Law".

2.2 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, while police records originate from the police authorities (*Secretaria de Seguranca Publica - Policia Civil*).

Our administrative dataset includes 8,453,076 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.[2] Of these 8,453,076 individuals, 11.54% are black, 51.28% are females, 39% completed compulsory education or above, and 24.89% are married. The police records include 20,066,180 incidents reported to the police in 2010-2024.[3] For those, we have 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, or not, and whether charges were pressed or not. It is worth remarking here that the information about whether protective measures were applicable is what we use to be able to identify DV cases. Indeed, in Brazil, there is not a crime type "DV" as in many other countries. However, urgent protective measures are available only to victims of DV, and therefore, this measure enables us to identify these cases in the police records. In Rio Grande do Sul, anyone can call the police to report a DV incident in RGS. Notice, however, that we would only have an eligibility marker 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 one is whether to press charges against her abusive partner: indeed, in Brazil, with the exception of particular crimes,[4] the victim of a DV incident needs to actively choose to press charges against the offender in order for the criminal case to be brought to the court. This applies in particular to crimes such as threat and fighting which compose a non-negligible proportion of DV cases.[5] 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.[6]

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[2]Schooling history and civil status are missing for 23% and 0.8%, respectively.

[3]To avoid COVID-19, we use the period up to February 2020.

[4]For example, crimes against children or the elderly do not require charges to be pressed by the victims to be prosecuted.

[5]As shown in Table 7, they constitute more than 56% of DV cases against females in our sample pre-reform.

[6]In 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.

We focus on female victims: for the period March 2010-February 2020, we have 3,391,450 unique occurrences with at least one female victim, of which 27% are DV. Figure 2 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. What changed was really the count (and share) of DV cases where the victim requested protective measures and omitted to press charges. Figure 2 shows that the cases in which the victim charged and did not request protection decreased markedly, while the opposite occurred for cases in which the victim requested protection and did not press charges. Figures 3A. and 3B. display the monthly counts of DV incidents distinguishing between those for which protective measures were solicited and those for which charges were pressed, respectively. Figure 3A. shows that DV victims requested protective measures in a growing share of cases; while Figure 3B. shows that 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. 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. Appendix A shows additional descriptive statistics from RGS.
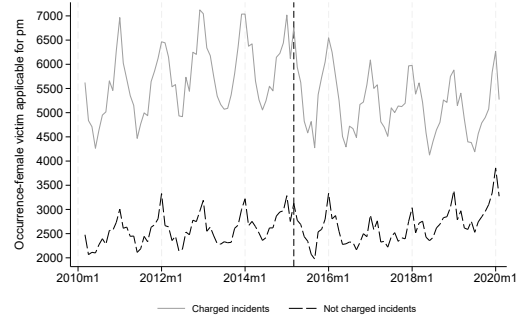
Figure 2: Incidents when Charges (Protective Measures) were Pressed (Solicited) vs Not
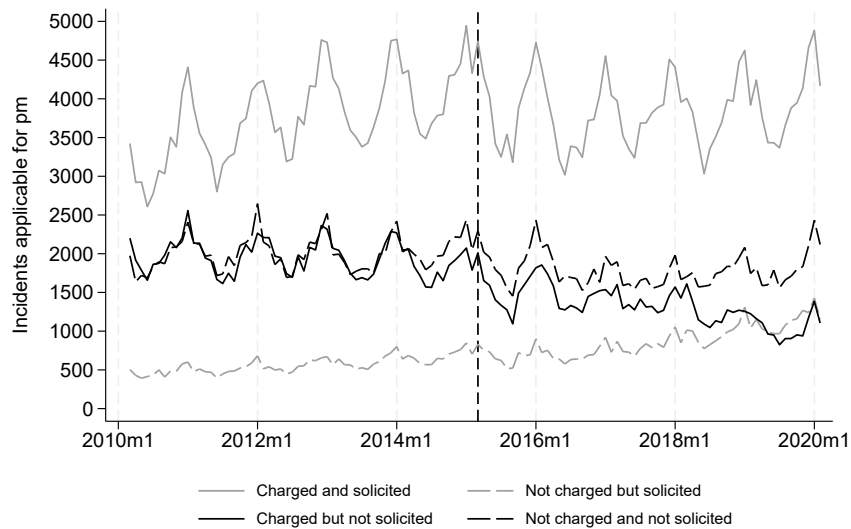
Notes: The figure displays the monthly counts of DV incidents distinguishing between incidents when charges (protective measures) were pressed (solicited) vs not.
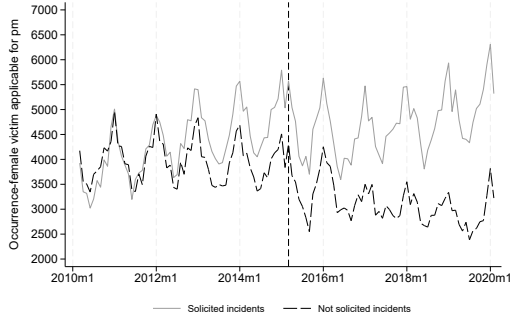
| **(A.)** Solicited Incidents vs Not | **(B.)** Pressed Charges vs Not |

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.
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Figure 3: Trends of Solicited and Charged Incidents vs Not

For our econometric analysis, first, we build a panel dataset for the period 2012-2019 where we include all women aged 18-55 from the administrative data to investigate whether they were victimized in a given year (DV or Non-DV). We choose to focus on the age range 18-55, firstly because this sample represents the prime ages of DV victims as illustrated in Figures A1 and A2, respectively for first offense and any offense,[7] and secondly because we want to avoid capturing family violence against minors which is not our focus in this study. We further restrict the end of our analysis to 2019 in order not to capture the disruption caused by the COVID pandemic which has significantly influenced the dynamics of DV both in terms of its nature and catalysts, but also in terms of reporting.[8] As a result, in this part, the final sample is composed of 2,820,199 women, 271,736 domestic violence victims,[9] 19,180,075 woman-period observations.[10] Then, to better analyze the specific occurrences, we consider only domestic violence incidents. We have

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[7]Moreover, Figures A3 and A4 report respectively the ages of victims and offenders for any DV offense by level of schooling and by marital status.

[8]Overall, 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.

[9]Notice that this is not a count of DV episodes. It is how many women reported being victimized at least once in a given year.

[10]Of these total observations, 376,784 are the women-period with at least one DV episode.

450,668 occurrence-victim observations of domestic violence, as we consider incidents with up to 10 victims. The number of domestic violence incidents is 443,265. The majority of cases have only one victim (436,262, i.e. 98.42%)[11], 6,656 (1.50%) occurrences have two victims, 306 (0.07%) occurrences have three victims, 31 (0.01%) occurrences have four victims, 9 (0.00%) occurrences have five victims, 1 (0.00%) occurrence has seven victims. Overall, we have 271,736 unique women (as in the first part).

Table A1 reports descriptive statistics for the sample of women used in the econometric analysis. The Table indicates that, on average, 0.75% of males appeared in the police records for violent crimes against females in 2010, 1.96% of women become victim of DV and 4.97% become victim of other violent crimes in a given year in our study period. Reflecting how widespread DV is in RGS, fewer males, i.e., 0.51% of males, appeared in the police records for violent crimes against males in 2010. Panel B shows that, in RGS, approximately 10% of women are black, roughly 67% of the working age population are employed, 18% of the population is young, i.e., 15-24, and nearly 2% of people live under the poverty line.

Table A2 displays descriptive statistics for occurrences of domestic violence. As we allow for multiple victims, observations are at the occurrence-victim level. For this part, we focus on whether each victim decides to solicit protection from the police and/or press charges against the abusive partner. Around 70% of occurrence-victim pairs are charged, and in 60% of cases, women ask for protection. The mean percentages for family involvement are very low: the probability that a caller is a family member is around 0.04%, while that a witness is a family member is 0.23%. Table A2 shows also municipal controls for this part.

3 Econometric Modelling

3.1 Female Exposure to Male Violence as treatment exposure

The legislative framework introduced in March 2015 by the *Lei do Feminicídio* applied to all the citizens of Brazil. Therefore, no pure control group exists for this reform neither in Rio Grande do Sul (RGS) nor in other states of Brazil - more formally, there are no never treated units. In order to quantify the causal impact of this reform on the dynamics of domestic violence, we define a continuous treatment variable based on the predetermined exposure of women in RGS to men who were violent against women, which can also be interpreted as "dosage" of treatment (Callaway et al., 2024). Our analysis applies a Difference-in-Differences

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[11]Remember that we restrict the age of victims and offenders to be between 18 and 55, so child victims are not accounted for here.

(DiD) model that exploits the geographical variation in violent men against women across the 497 municipalities of RGS prior to the policy change. For woman i, the exposure to treatment is defined as the share of men in the municipality of birth of woman i who appear in the police register with a violent criminal record against a female in the baseline year, i.e., from March 2010 to March 2011. Formally, our treatment measure for woman i of cohort c in municipality m can be expressed as follows:

$$EXP_{icm}^{f} = \frac{VM_{2010}}{M_{2010}}$$

where VM_{2010} for woman i of cohort c is the count of men aged 18-55 who committed at least one violent crime against a woman in the period March 2010-March 2011 in woman i's municipality of birth m. M_{2010} for woman i is the total count of men aged 18-55 in the same period in woman i's municipality of birth m. Insofar as this measure captures the *ex ante* risk of encountering men exerting violence against women, this represents a measure of exposure to the *Lei do Feminicídio*. We choose to consider the municipality of birth of women and not of residence to avoid issues of endogenous migration.[12] We would expect - if there is any difference - that, by using the municipality of residence, our estimates would be upward biased, as it might be that women choose to migrate towards municipalities with less violence against women. Section 6 compares the main results when using the exposure at the municipality of residence or municipality of birth levels.

(A.) Exposure Treatment in 2010   **(B.)** Change in DV in 2012-2019
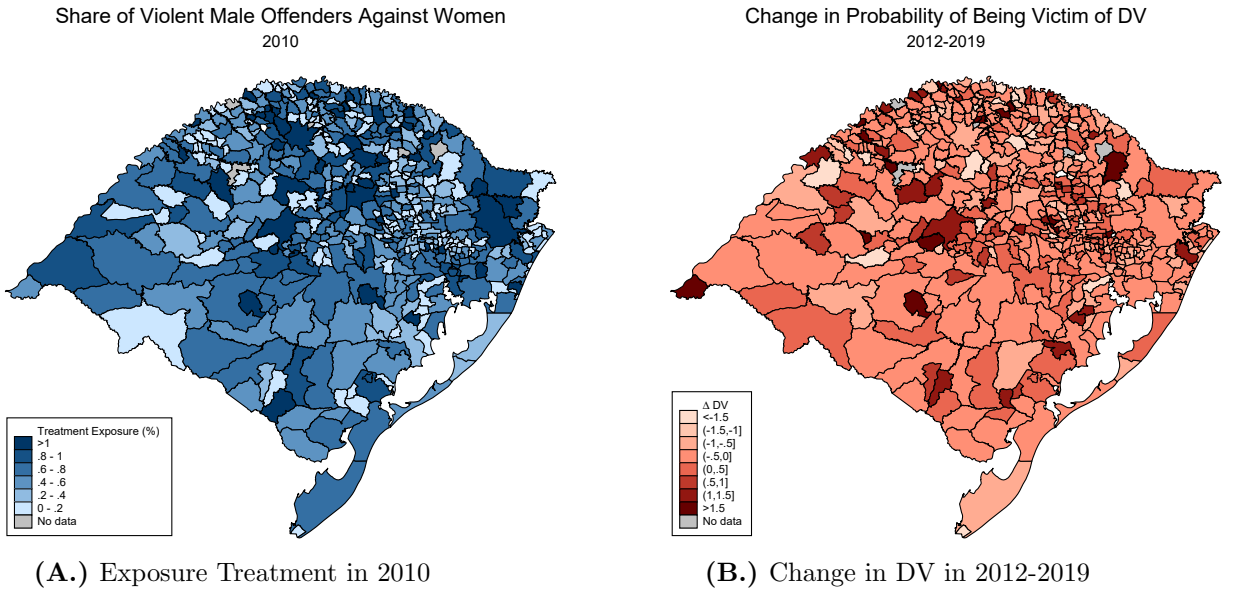
Figure 4: Share of Violent Male Offenders against Women in 2010 and Change in Risk of Domestic Violence in 2012-2019

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[12]However, more than half of our sample resides in the municipality of birth.

Figure 4 shows two maps of municipalities of RGS. Figure 4**A.** shows the statistical variation that can be exploited from the treatment measure (EXP^f_{icm}). One can observe that there is significant dispersion of the treatment exposure across areas: while a woman born in the municipality corresponding to the 25th percentile of treatment exposure (Triunfo and Nonoai) was, on average, at risk of encountering 6.38 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. While the treatment measure is fixed at the baseline year 2010 and does not include potential endogenous migration of individuals across localities, the analysis uses data from 2012 to 2019. Figure 4**B.** shows the change in domestic violence victimization rate across the municipalities of RGS between 2012 and 2019. A quick visual assessment of color gradients of the two maps points to a negative correlation between areas more exposed and a relative reduction in domestic violence cases, as one would expect if the law implementation acted as a deterrent.

The scatter plots of Figure 5, which display the change in DV victimization against the treatment exposure (EXP^f_{icm}) for the *before* and *after* periods, help to look at the relationship between outcome and treatment more explicitly. While Figure 5**A.** shows no correlation in the years 2012-2014, i.e., prior to the reform, between our treatment and the change in DV over time, Figure 5**B.** shows that our treatment negatively predicts the change in DV after the reform, i.e., from March 2015 to 2019. These descriptive results suggest that a) the treatment had no significant correlation with the evolution of DV victimization prior to the law suggesting that parallel trends are observed b) after the law was implemented DV disproportionately decreased in areas with a greater share of violent men against women (higher treatment exposure).

(A.) Before the *Lei do Feminicídio*

(B.) After the *Lei do Feminicídio*

Figure 5: Treatment Exposure vs Change in Risk of Domestic Violence Before and After the *Lei do Feminicídio*

3.2 Estimation Strategy

While the descriptive analysis previously presented offers valuable insights and promising signs of causality between the implementation of the law and a reduction in DV cases, a more formal modeling and estimation is granted. In order to do so, we explore the richness of the data and formalize a DiD model using data at the ind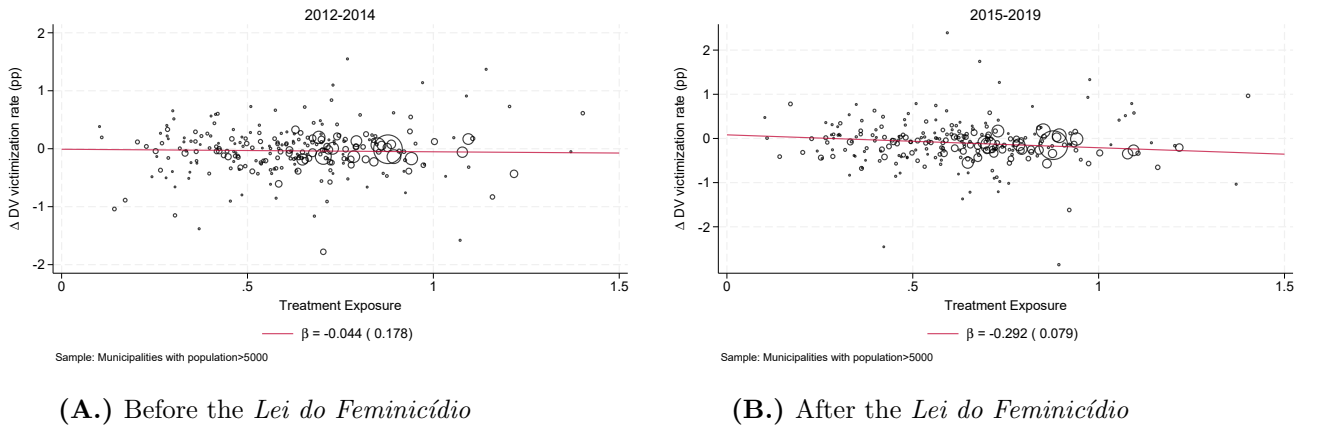ividual level which allows us to not only control for baseline socio-economic features of the units of treatment (municipalities) but also a detailed array of individual characteristics which can affect the probability of woman to be a victim of domestic violence beyond the treatment exposure proposed. The following equation can be estimated to retrieve the causal impact of the reform on our outcomes of interest:

$$Y_{icmt} = \alpha + \beta_1 Post_t + \beta_2 EXP_{icm}^f + \beta_3(Post_t \times EXP_{icm}^f) + \theta_t X_{icmt} + \omega_{icmt} \tag{1}$$

where Y_{icmt} is the outcome of interest, $Post_t$ is a dummy for the period after March 2015, EXP_{icm}^f is the women's exposure measure defined above, X_{icmt} are a set of baseline and time-varying controls, and ω_{icmt} is the error term for woman i, from birth cohort c, 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. Under the assumptions of no anticipation effects, parallel trends, and unconfoundness, the parameter β_3 identifies the average treatment effect on the treated (ATT) over different levels of exposure. In order to provide supportive evidence and indirect testing of some of the assumptions, we later expand the model of Equation 1 to allow for dynamic effects of treatment exposure in an event study design. Following Wooldridge (2021), we allow baseline controls of municipalities' socio-economic and demographic features in X_{icmt} to have time-varying effects, helping to relax the unconditionality of the parallel trends assumption.

Before proceeding to the results, and in line with recent econometric developments on parameter identification in differences-in-differences models with continuous treatment and absence of never-treated units, valid comparisons across the treatment exposure (dosage) distribution and the marginal effects on the outcomes of interest of a change in treatment exposure require a stronger assumption concerning the heterogeneity of the treatment effects (Callaway et al., 2024; de Chaisemartin et al., 2024; Sun and Shapiro, 2022). By ruling out selection-on-gains into particular levels of treatment exposure, we assume that the observed outcome changes for a given exposure group reflect what would have happened to all other groups had they been exposed to that same level of treatment - this assumption is referred as "Strong Parallel Trends" according to Callaway et al. (2024). Note that this assumption does not impose full homogeneity

of treatment effects but rather that the treatment effect response as a function of exposure is common to different treated units.

4   Results

4.1   Incidence of Domestic Violence (DV)

Table 1 shows our main results for the impact of the *Lei do Feminicídio* on the probability of being a domestic violence victim among women in Rio Grande do Sul (RGS). In all specifications, we include fixed effects for race, year of birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year-fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with year-fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)), based on the share of population living in an urban area within the municipality.

Regardless of the sets of controls included in the regression, results in Table 1 indicate that the *Lei do Feminicídio* reduced the risk of DV in RGS. In particular, estimates in Table 1 indicate that, after the reform, the incidence of DV reduced by 3.56% to 2.46% more (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. Municipalities with a higher share of violent men against women experienced a disproportionate reduction in the risk of DV after the introduction of the *Lei do Feminicídio*.

As mentioned in Section 3, 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 at the same rate in the absence of the policy intervention. Although the parallel trend assumption is not directly testable in the data, we can test for the absence of pre-trends as a necessary condition for this assumption to hold. For this reason, Figure 6 displays the event-study analogue to the DiD estimates in Columns (1) and (6), respectively, of Table 1, i.e., with the basic and full set of controls in the estimated equation. It shows numerically small and statistically insignificant coefficients prior to the reform, indicating that more and less exposed municipalities to our treatment exposure shared parallel pre-trends prior to the reform. In contrast, starting from March 2015, a disproportionate reduction in the risk of DV appears in municipalities more exposed to our treatment exposure. Estimates are always statistically significant at 5% and they are numerically much larger than those in the

pre-period, reiterating that the *Lei do Feminicídio* reduced the risk of DV disproportionately in municipalities with a higher share of violent men against women.

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

	Probability of DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.295***	-0.239***	-0.237***	-0.237***	-0.235***	-0.204***
	(0.0688)	(0.0398)	(0.0398)	(0.0403)	(0.0398)	(0.0379)
N of Observations	19,180,075	19,179,550	19,179,550	19,179,550	19,176,518	19,176,518
N of Municipalities	492	491	491	491	487	487
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	1.988	1.988	1.988	1.988	1.988	1.988
Range of Exposure	.24	.24	.24	.24	.24	.24
Percentage Effect	3.56	2.89	2.86	2.86	2.84	2.46
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is the probability of DV. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

4.2 Falsification and Placebo Tests

Even in the absence of differential pre-trends and robustness of the estimates to pre-determined controls, one may worry that our treatment exposure measure might carry socio-economic components and, therefore, capture unobserved time-invariant factors in the different municipalities (e.g., poverty) or unobserved time-varying municipality-year shocks (e.g., a localized intervention to reduce violence). While we cannot fully rule out these possibilities, we can present supportive evidence that our estimates in Table 1 and Figure 6 are not the result of other unobserved factors. To this end, first, we model other female victimization, i.e., violent crimes with female victims that are not DV, as the outcome of the same DiD specification estimated in the main

Figure 6: Event Study Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence
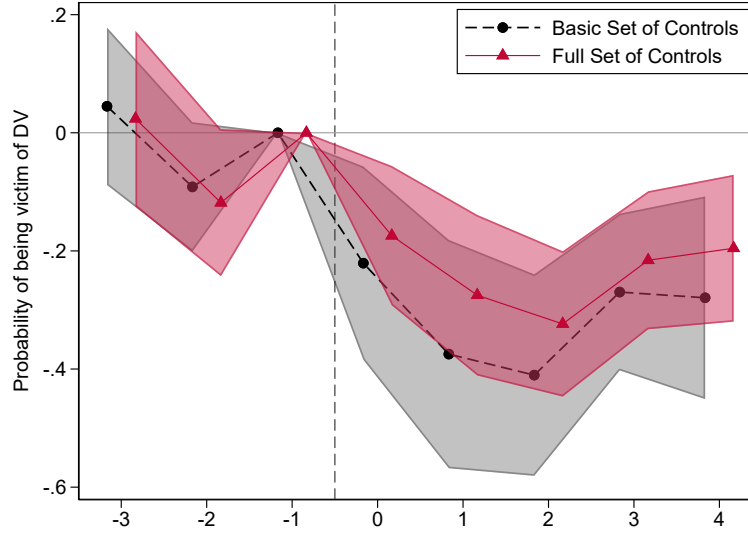
Notes: Event-study analogue of Table 1. The basic set of controls reproduces Column (1) of Table 1, while the full set Column (6). Standard errors are clustered at the municipality level.

analysis. This can be seen as a falsification test since, in the absence of spillovers between DV and Non-DV crimes against women, there should be no effect of the law on Non-DV crimes. Secondly, we construct a potential confounder exposure, which should not have an effect on the DV outcome conditional on the true treatment exposure. This can be interpreted as a placebo test.

Falsification Test. Table 2, which models violent crimes against females that are Non-DV, is organized in the same way as Table 1 and the same set of controls is gradually added. If our results reflected some unobserved policy aimed to reduce criminality, or if they reflected some differential underlying trend in the socio-economic circumstances of different municipalities of RGS (e.g., differential unemployment trends across regions), some significance in the estimated effects should appear also in Table 2. However, that is not what is found here. Table 2 displays a set of numerically small and statistically insignificant coefficients, suggesting that only DV decreased disproportionately after 2015 in municipalities more exposed to violent men against women. Figure 7**A.** displays the event-study analogue to the falsification test in Columns (1) and (6) of Table 2, showing again no detectable impact of the *Lei do Feminicídio* on the risk of other forms of female victimization in RGS. Moreover, Figure B2 shows the change in Non-DV victimization against the treatment exposure for the *before* and *after* periods. While Figure B2**A.** shows a slightly negative correlation between Non-DV and treatment exposure in the

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years 2012-2014, i.e., before the reform, Figure B2**B.** shows that our treatment does not predict the change in Non-DV after the reform, i.e. from March 2015 to 2019. This should mitigate the concern that our analysis is not isolating the effect of the *Lei do Feminicídio* on the risk of DV but rather capturing the simultaneous effects on crime more broadly defined.

Table 2: Estimates of Impact of the *Lei do Feminicídio* on Risk of Other Female Victimization (i.e., Non-DV)

	Probability of Non-DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.144	0.0113	-0.00202	0.00102	-0.00713	-0.0105
	(0.199)	(0.132)	(0.127)	(0.118)	(0.118)	(0.122)
N of Observations	19,180,075	19,179,550	19,179,550	19,179,550	19,176,518	19,176,518
N of Municipalities	492	491	491	491	487	487
R-squared	0.004	0.004	0.004	0.004	0.004	0.004
Pre Mean of Outcome	5.12	5.12	5.12	5.12	5.12	5.12
Range of Exposure	.24	.24	.24	.24	.24	.24
Effect wrt Mean	.68	.05	.01	0	.03	.05
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is the probability of Non-DV. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Placebo Test. Another way of testing whether our results in Table 1 and Figure 6 might reflect some unobserved factor is to construct a potential confounder treatment exposure which, conditional on the true treatment measure EXP_{icm}^f, should have no effect on the actual outcome of interest, therefore, acting as a placebo treatment. In order to achieve this, we construct the male version of our treatment exposure: we measure the share of violent male offenders against men at baseline EXP_{icm}^m displayed in Figure B1. If, for instance, our treatment exposure EXP_{icm}^f is mostly picking up poor socio-economic circumstances, insofar as the higher share of violent men against men is a similar proxy for crime-inducing socio-economic circumstances, then

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the male version of exposure EXP_{icm}^m should also predict a similar reduction in DV victimization across women born from different areas of RGS after 2015 or render the effect of our treatment measure insignificant.

Table 3: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence Using Male Exposure

	Probability of DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.310***	-0.255***	-0.252***	-0.251***	-0.253***	-0.222***
	(0.0788)	(0.0477)	(0.0468)	(0.0472)	(0.0465)	(0.0467)
Post*ExposureM	0.0363	0.0372	0.0379	0.0338	0.0423	0.0403
	(0.0662)	(0.0598)	(0.0584)	(0.0578)	(0.0569)	(0.0564)
N of Observations	19,180,075	19,179,550	19,179,550	19,179,550	19,176,518	19,176,518
N of Municipalities	492	491	491	491	487	487
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	1.988	1.988	1.988	1.988	1.988	1.988
Range of Exposure	.24	.24	.24	.24	.24	.24
Range of ExposureM	.119	.119	.119	.119	.119	.119
Effect wrt Mean	3.74	3.07	3.05	3.03	3.05	2.67
Effect wrt Mean for M	.22	.22	.23	.2	.25	.24
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is probability of DV. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Although both treatment measures do correlate with which other, $\rho(EXP_{icm}^f, EXP_{icm}^m)$=0.62,[13] suggesting that they share some common underlying factors, the results of Table 3 show that including this placebo treatment measure does not invalidate our previous findings. Our treatment measure, i.e., $Post_t \times EXP_{icm}^f$ in Table 3, is still a powerful and negative predictor of the

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[13]This is measured in the panel data before keeping only women. If we keep only women, i.e. for the sample of our analysis, this number is 0.6094. For the part of occurrence-victim level, it is still around 60%.

evolution of DV after March 2015; in contrast, the share of violent men against men at baseline, i.e., $Post_t \times EXP_{icm}^m$ in Table 3, does not significantly predict the evolution of DV in RGS after 2015. If anything, estimates are numerically positive, thus clearly not predicting a reduction in DV unlike our treatment measure.

This conclusion is confirmed in Figure **7B.**, which displays event study estimates of the impact of the share of violent men against men at baseline, i.e., $Post_t \times EXP_{icm}^m$ in Table 3, on the risk of female DV. No negative detectable effect appears also in this case, confirming that the share of violent men against men at baseline in the municipalities of RGS does not predict the evolution of DV after 2015 and thus suggesting that our results are not simply picking up the differential evolution of DV over time between women born in areas of the state with unobserved crime-inducing characteristics. Furthermore, Figure B3 shows the change in DV victimization against the placebo exposure (EXP_{icm}^m) for the *before* and *after* periods. While Figure B3**A.** shows a slightly negative correlation between the placebo exposure and DV victimization before the law, Figure B3**B.** displays no correlation for the after periods thus confirming that the placebo treatment does not predict the change in DV after the reform.
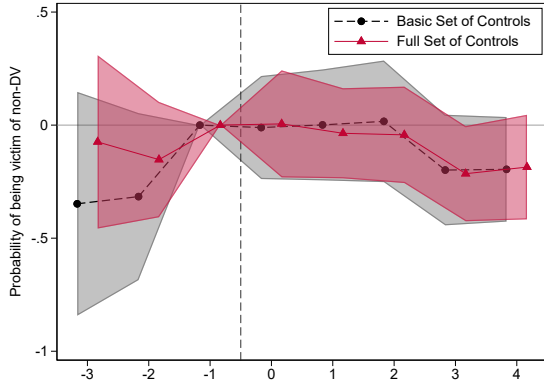
(A.) Event Study Estimates of Impact of the *Lei do Feminicídio* on Risk of Other Female Victimization (i.e., Non-DV)
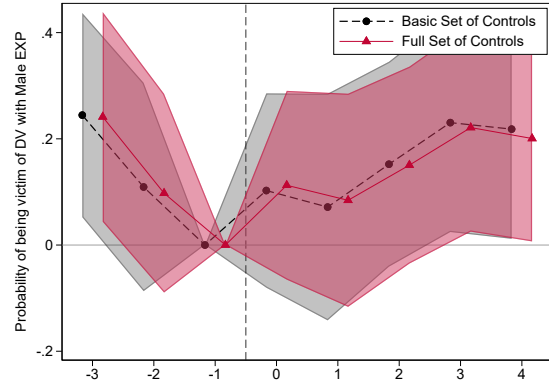
(B.) Event Study Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence Using Male Exposure

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

Notes: Event-study analogues for the Falsification and Placebo Tests. Figure **7A.** reproduces Table 2; while Figure **7B.** follows Table 3. The basic set of controls reproduces Column (1), while the full set Column (6). Standard errors are clustered at the municipality level.

Finally, another useful test is to study what happened to male DV victimization. Male DV victimization is a very rare event. Table 4 shows that our female treatment exposure, i.e., $Post_t \times EXP_{icm}^f$, is an insignificant predictor of male DV victimization after 2015. However, in contrast, the share of violent men against men at baseline, i.e., $Post_t \times EXP_{icm}^m$, is a negative and statistically significant predictor of the evolution of male DV victimization after March 2015.

This could reflect the presence of homosexual couples, or of violence against sons or fathers, which we need to investigate further.

Table 4: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence for Men

	Probability of DV for men					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.0141	-0.0100	-0.00980	-0.00988	-0.00962	-0.00523
	(0.00874)	(0.00913)	(0.00892)	(0.00910)	(0.00890)	(0.00890)
Post*ExposureM	-0.0312**	-0.0309**	-0.0280**	-0.0317**	-0.0305**	-0.0310**
	(0.0143)	(0.0139)	(0.0141)	(0.0140)	(0.0141)	(0.0140)
N of Observations	18,557,117	18,556,616	18,556,616	18,556,616	18,554,282	18,554,282
N of Municipalities	487	486	486	486	482	482
R-squared	0.000	0.000	0.000	0.000	0.000	0.000
Pre Mean of Outcome	.081	.081	.081	.081	.081	.081
Range of Exposure	.24	.24	.24	.24	.24	.24
Range of ExposureM	.122	.122	.122	.122	.122	.122
Effect wrt Mean	4.19	2.97	2.9	2.93	2.85	1.55
Effect wrt Mean for M	4.7	4.65	4.22	4.77	4.59	4.67
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is probability of DV for men. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

5 Heterogeneity

5.1 DV results by Age Decomposition

It is interesting to investigate whether the results of DV might be heterogeneous across age groups of victims. Table 5 presents the regression results estimating the impact of the *Lei do Feminicídio* on the probability of domestic violence (DV) for women of different age groups, i.e., aged 18-25; 26-35; 36-45; 46-55. The full set of controls was included in the model for each age group. The coefficients of the variable of interest ($Post_t \times EXP_{icm}^f$) are all negative and statistically significant at 1% level for the first three age groups (18-25, 26-35, 36-45) and at 5% level for the 46-55 age group, indicating a reduction in the probability of DV post-law

implementation. Specifically, after the reform, the incidence of DV reduced by 2% more 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 for women aged 18-25. The percentage effects are even higher for older women: 2.21% for those aged 26-35, 2.93% for the age range 36-45, and finally 2.55% for 46-55.

Table 5: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence - Age decomposition

	Probability of DV			
	Age 18-25	Age 26-35	Age 36-45	Age 46-55
	(1)	(2)	(3)	(4)
Post*Exposure	-0.207***	-0.226***	-0.237***	-0.116**
	(0.0718)	(0.0627)	(0.0615)	(0.0485)
N of Observations	4,135,247	5,646,407	4,526,713	4,069,828
N of Municipalities	464	475	470	471
R-squared	0.002	0.002	0.001	0.001
Pre Mean of Outcome	2.232	2.347	1.956	1.245
Range of Exposure	.216	.23	.242	.274
Percentage Effect	2	2.21	2.93	2.55
Full Set of Controls	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is the probability of DV by age group. In all Columns, we include the full set of municipal controls as in Column (6) of Table 1. Column (1) investigates victims aged 18-25, Column (2) 26-35, Column (3) 36-45; and Column (4) 46-55. Standard errors are clustered at the municipality level.

Overall, the results suggest a consistent and significant reduction in the probability of domestic violence across different age groups following the implementation of the *Lei do Feminicídio*, with the most substantial percentage effect observed in the 36-45 age group. Detailed tables are displayed in Appendix C.

5.2   DV results by First vs Repeated Occurrences

The richness of our data allows us to distinguish between first-time victims and victims who have already experienced domestic violence. An important limitation to consider relates to the fact that our data only starts in 2010 for the police reports. Hence, we suffer from censoring prior to that date, which results in several false positive labeling of first-time victims.

Table 6: Estimates of Impact of the *Lei do Feminicídio* on First DV and Repeated DV Victimization by Age

	Totals		Age 18-25		Age 26-35		Age 36-45		Age 46-55	
	First DV	Repeated DV	First DV	Repeated DV	First DV	Repeated DV	First DV	Repeated DV	First DV	Repeated DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post*Exposure	-0.265***	0.0613*	-0.202***	-0.00488	-0.385***	0.160***	-0.283***	0.0460	-0.141***	0.0252
	(0.0445)	(0.0337)	(0.0645)	(0.0377)	(0.0768)	(0.0596)	(0.0506)	(0.0482)	(0.0436)	(0.0282)
N of Observations	19,176,518	19,176,518	4,135,247	4,135,247	5,646,407	5,646,407	4,526,713	4,526,713	4,069,828	4,069,828
N of Municipalities	487	487	464	464	475	475	470	470	471	471
R-squared	0.002	0.002	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	1.988	0	2.232	0	2.347	0	1.956	0	1.245	0
Range of Exposure	.24	.24	.216	.216	.23	.23	.242	.242	.274	.274
Percentage Effect	3.2	.	1.95	.	3.77	.	3.5	.	3.11	.
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of first DV and of repeated DV, for all and by age group. In all Columns, we include the full set of municipal controls as in Column (6) of Table 1. Columns (1) and (2) investigate first vs repeated victims for all ages; Columns (3) and (4) for ages 18-25, Columns (5) and (6) for ages 26-35, Columns (7) and (8) for ages 36-45; and Columns (9) and (10) for ages 46-55. Standard errors are clustered at the municipality level.

To this end, Table 6 presents the regression results estimating the impact of the *Lei do Feminicídio* on the probability of first-time and repeated domestic violence (DV) incidents across different age groups. The analysis is divided into total effects and specific age groups: 18-25, 26-35, 36-45, and 46-55. For total effects, the law significantly reduces the probability of first DV and slightly increases repeated DV, both statistically significant. In the age-specific results, the coefficients for first DV are all negative and significant at the 1% level. For repeated DV, significant increases are seen only in ages 26-35. The percentage effects indicate a reduction in first DV by 3.2% overall and 1.95%, 3.77%, 3.5%, and 3.11% for the respective age groups. Notice that we cannot compute the percentage effects of repeated DV as, by construction, the pre-mean of the variable is zero. A full set of controls was included in each model. These results suggest a substantial and significant reduction in first-time DV incidents across all age groups following the implementation of the *Lei do Feminicídio*, with a slight increase in repeated DV for the 26-35 age group. Detailed tables are displayed in Appendix C. These results point to the relevant fact that possibly the law deterred more first-time offenders, than offenders who have already committed a DV act. However, we have to take this result with caution as we are very likely to capture mechanical censoring rather than a true null effect or even a positive one on repeated offenses.

5.3   DV results by Crime Type

Another interesting dimension to investigate is the type of DV crime committed and how that can change as a response to the policy. Table 7 shows descriptive statistics for the most frequent

crime types of domestic violence and is based on data from the years prior to the reform. It shows that threats constitute nearly half of all the DV incidents in our dataset, followed by bodily injuries, which account for roughly one in four incidents, fights, and disorderly conduct.

Table 7: Most Frequent Crime Types of Domestic Violence

	(1)	(2)	(3)
	Frequency	Percent	Cumulative Percentage
Threat	133572	46.11	46.11
Bodily Injuries	74773	25.81	71.92
Fighting	29120	10.05	81.97
Disorderly Conduct	17631	6.09	88.06
Observations	289,681		

Notes: Most frequent crime types of DV with female victims in the pre-reform period.

Table 8 displays the DiD estimates by crime type for domestic violence. A similar reduction in the incidence of DV occurred for threats, bodily injuries and fights, while no significance is found for the impact of the reform on the incidence of disorderly conduct. These results are interesting in showing that the law seems to have had a reducing effect on forms of DV which, despite not being as serious as homicides, can be seen as precursors in the dynamics of escalation of violence in the domestic context.

Table 8: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence By Crime Type

	Probability of DV			
	Threat	Bodily Injuries	Fighting	Disorderly Conduct
	(1)	(2)	(3)	(4)
Post*Exposure	-0.119***	-0.0708***	-0.0323*	-0.00984
	(0.0239)	(0.0200)	(0.0181)	(0.0108)
N of Observations	19,176,518	19,176,518	19,176,518	19,176,518
N of Municipalities	487	487	487	487
R-squared	0.001	0.001	0.001	0.000
Pre Mean of Outcome	0.985	0.605	0.219	0.125
Range of Exposure	0.24	0.24	0.24	0.24
Effect wrt Mean	2.9	2.81	3.54	1.89
Full Set of Controls	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is the probability of DV. In all Columns, we include the full set of municipal controls as in Column (6) of Table 1. Column (1) investigates threats, Column (2) bodily injuries, Column (3) fighting; and Column (4) disorderly conduct. Standard errors are clustered at the municipality level.

6 Robustness

In our main analysis, the outcome of interest is 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 victimization. All specifications indicate that the decrease found in Table 1 is robust. A reasonable concern is that our treatment exposure is 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 enrich our analysis by restricting the sample to women born in urban areas only. Results are displayed in Panel B of Table E1 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, *Lei do Feminicídio* reduced the risk of DV in RGS. In particular, estimates indicate that, after the reform, the incidence of DV reduced by 2.12% to 2.64% more (depending on specification) 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 *Lei do Feminicídio*. 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 look at alternative exposures. Remember that what we use in the main analysis is the share of men who committed at least one violent crime against women at baseline. The first alternative is the share of men aged 18-55 that committed at least one domestic violent crime against a woman. The second exposure we use as a robustness check is the share of men aged 18-55 who committed at least one violent DV crime against a woman. Regardless of the set of controls used, the main results are robust to both alternative treatments, indicating a strong decrease in the probability of DV, and no effect on the probability of Non-DV.

Lastly, to address concerns about potential migration, we reproduce the results on DV by using the municipality of residence of women and comparing them to the results with the municipality of birth. For this part, the sample is all women born and currently residing in RGS. Interestingly, the treatment effect using the treatment allocated at the municipality of residence is negative with a smaller magnitude but not significant, as shown in Table E3, while results for treatment allocated at the municipality of birth remain negative and statistically significant. This result points to the fact that when using exposure at the municipality of residence level, the coefficient suffers from a positive bias. Table E4 distinguishes between non-migrant women, i.e.,

those women whose municipality of residence coincides with that of birth, and migrant women, i.e., those that reside in a different municipality than the one of birth. When looking at the non-migrant sample, there is no difference in the results as the treatment measure is effectively the same. When looking at the migrant women, we see that the results are not significant when using the municipality of residence, but they are still negative and significant for the exposure at the municipality of birth level supporting the argument of potential endogenous selection of migration decisions consistent with a downward bias affecting estimates when treatment allocation reflects such migration choices.

7 Occurrence-victim level data

7.1 Behavioral Responses by Occurrences of Domestic Violence (DV)

The previous section demonstrated that women born in municipalities in Rio Grande do Sul (RGS) characterized by a higher share of violent men against women prior to the *Lei do Feminicídio* experienced a steeper reduction in the risk of falling victim to domestic violence. This conclusion is consistent with the Becker model of crime that describes the decision of the offender to engage in a criminal activity once the expected costs and benefits of the criminal act have been weighted against the expected benefit and cost of a lawful alternative. In our context, the determinant of the crime participation equilibrium decision that changes is that of the sanction costs facing offenders being higher as a result of the introduction of the law. Assuming the other determinants are kept constant or controlled for appropriately, the increased sanction cost reduces the expected net benefit of participating in crime and tilts the decision of offenders away from engaging in criminal acts through deterrence.

However, the previous section only focused on the impact of the *Lei do Feminicídio* on the behavior of the offenders. But what happened to the behavior of victims? The harsher sentences in relation to DV that resulted from this policy change may have affected the behavior of victims too. One possible channel is that the policy intervention may have signaled to victims that the state is now taking DV more seriously, which might make victims trust the public authority more and fear less the potential retaliation by the abusive partner in response to their decision to press charges. This, in turn, may have encouraged victims to press more charges against their abusive partners. On the other hand, the harsher sentences implied by the policy change may have as well discouraged victims from pressing charges against their abusive partner. This may have happened, for instance, if victims shared children with the abusive partner and/or if victims were financially dependent on their abusive partner. The belief of an increased probability of harsher

punishment entailing incarceration may deter victims from charging their intimate partner in order to limit direct and indirect non-monetary and monetary costs in the absence of adequate state welfare support. In these and other similar cases, the victim of DV may have displayed a greater hesitation towards charges against the abusive partner after March 2015.

The current section investigates these issues by focusing on occurrences of DV during our study period, i.e., from 2012 to 2019, in RGS. The same DiD specification is estimated again, modeling as dependent variables a set of dummy variables that capture whether the DV victim of an occurrence pressed charges against her abusive partner, whether she solicited protective measures, as well as any combination of the two. This analysis is done at occurrence-victim level, i.e., an intensive margin, to better investigate the specific events of DV.[14]

In this part of the analysis, we expand the specification to include offense-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 the DV changed after the introduction of the reform. This extension in the modeling is particularly relevant to avoid capturing crime-type compositional effects as those shown in Table 8 that can result from the policy and, therefore, influence the probability of charging and/or soliciting protective measures based on the nature of their severity and risk to life. Furthermore, to account for potential differences in policing across treatment units, we also include police station fixed effects, which will absorb time-invariant differences in resources and specialization of police forces.

Columns (1) and (2) of Table 9 show results for the likelihood of a DV victim soliciting protective measures by the police to keep her safe from her abusive partner after a DV incident. Importantly, one DV victim needs not 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. The first specification corresponds to Column (1) in Table 1, the second one to Column (6) in Table 1. These estimates indicate that, regardless of the set of controls, the propensity of victims to solicit protective measures increased due to the introduction of the *Lei do Feminicídio*. In particular, the likelihood of DV victims soliciting protection from the abusive partner in municipalities at the 75th percentile of our treatment exposure measure increased by 1.46% to 2.18% more than in municipalities at the 25th percentile of our treatment exposure measure.

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[14]Notice that in Section D.2 we show the results for behavioral responses when using the same design of the first part, i.e., the panel structure of the data.

Table 9: Estimates of Impact of the *Lei do Feminicídio* on Soliciting Protective Measures and Pressing Charges

	Soliciting Protection		Pressing Charges	
	(1)	(2)	(3)	(4)
Post*Exposure	5.561***	3.723**	-1.900	-1.603
	(1.475)	(1.588)	(1.524)	(1.527)
N of Observations	450,565	450,530	450,565	450,530
N of Municipalities	432	429	432	429
R-squared	0.173	0.173	0.303	0.303
Pre Mean of Outcome	50.933	50.933	70.8	70.8
Range of Exposure	.2	.2	.2	.2
Effect wrt Mean	2.18	1.46	.54	.45
Black	Yes	Yes	Yes	Yes
Crime/police station FE	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Income/employment	No	Yes	No	Yes
Education	No	Yes	No	Yes
Share of youth	No	Yes	No	Yes
Poverty rate	No	Yes	No	Yes
Urban index	No	Yes	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of soliciting protection and of pressing charges. Columns (1) and (3) report the estimate for the basic specification, i.e., with fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects; crime and police station fixed effects. Columns (2) and (4) report the estimate for the full specification, i.e., adding also the median income, the share of employed people, the level of education, the share of young people, the poverty rate, and an urban index, interacted with year fixed effects. Standard errors are clustered at the municipality level.

Figure 8**A.** represents the event-study analogue, with two specifications, respectively Column (1) and Column (2) of Table 9. Columns (3) and (4) of Table 9 show results for the likelihood of a DV victim to press charges against her abusive partner after a DV incident. Figure 8**B.** shows the event-study analogue, with two specifications, respectively Column (3) and Column (4) of Table 9. Estimates in Table 9 indicate that, regardless of the set of controls, the propensity of victims to press charges decreased due to the introduction of the *Lei do Feminicídio*. However, the coefficients are not statistically significant. In particular, the likelihood of DV victims pressing charges against the abusive partner in municipalities at the 75th percentile of our treatment exposure measure decreased by 0.5% more than in municipalities at the 25th percentile of our

treatment exposure measure.

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

(A.) Probability of Soliciting Protection

(B.) Probability of Pressing Charges
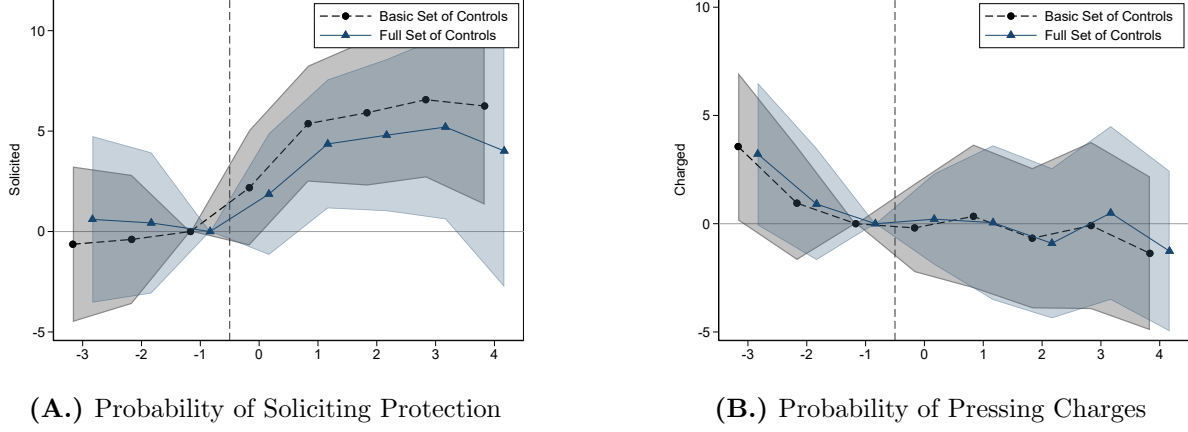
Notes: Event-study analogues of Table 9. Subfigure A reproduces Columns (1) and (2); while Subfigure B Columns (3) and (4). Standard errors are clustered at the municipality level.

Even though these results are not significant, the fact that women might be less willing to press charges could go in line with not wanting to penalize their abusers too much now that the law is in place. At the same time, it seems that they are aware of the danger they are facing and are more prone to ask for protection. What if we consider combinations of these two behaviors? From previous results, it seems that they might be substitutes as the likelihood of soliciting protection increases, while the probability of pressing charges decreases. Hence, it seems interesting to look at what happens at the combination of the two, which we do in Table 10.

First, consider the probability of not pressing charges but soliciting protection by the police, in Columns (3) and (4). This is interesting as it combines the two mechanisms we found above: the coefficient is always positive and statistically significant at 10% level. The percentage effect with respect to the mean is around 5-6%. The positive effects are a composition of the negative effect on the likelihood of pressing charges and the positive on the probability of soliciting protection. The same composition of effects can be detected in in Columns (5) and (6) Table 9, which examines the probability of pressing charges but not soliciting protection from the police. The coefficient of interest is always negative and statistically significant, indicating a significant reduction in charges without protection solicitation post-law implementation. The estimates in Table 9 still compare municipalities at the 75th percentile of our treatment exposure measure with those at the 25th percentile, and they indicate that the likelihood of victims pressing

charges without requesting protection from their abusive partner is reduced by a sizeable 2.8% to 4.4%.

Table 10: Estimates of Impact of the *Lei do Feminicídio* on Victims' Behavior

	Charged and Solicited		Not Charged but Solicited		Charged but Not Solicited		Not Charged and Not Solicited	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Exposure	3.839**	2.023	1.722*	1.689*	-5.739***	-3.633***	0.178	-0.0853
	(1.706)	(1.644)	(0.896)	(0.880)	(1.381)	(1.250)	(1.357)	(1.284)
N of Observations	450,565	450,530	450,565	450,530	450,565	450,530	450,565	450,530
N of Municipalities	432	429	432	429	432	429	432	429
R-squared	0.223	0.223	0.248	0.248	0.121	0.122	0.173	0.173
Pre Mean of Outcome	44.768	44.768	6.164	6.164	26.031	26.031	23.036	23.036
Range of Exposure	.2	.2	.2	.2	.2	.2	.2	.2
Effect wrt Mean	1.72	.9	5.59	5.48	4.41	2.79	.15	.07
Black	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crime/police station FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	No	Yes	No	Yes	No	Yes
Education	No	Yes	No	Yes	No	Yes	No	Yes
Share of youth	No	Yes	No	Yes	No	Yes	No	Yes
Poverty rate	No	Yes	No	Yes	No	Yes	No	Yes
Urban index	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are all combinations of behavior. Columns (1), (3), (5), and (7) report the estimate for the basic specification, i.e. with fixed effects for race, year-of-birth, year and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects; crime and police station fixed effects. Columns (2), (4), (6), and (8) report the estimate for the full specification, i.e. adding also the median income, the share of employed people, the level of education, the share of young people, the poverty rate and an urban index, interacted with year fixed effects. Standard errors are clustered at the municipality level.

The other two combinations, i.e., the probability of both pressing charges and soliciting protection and the probability of doing neither, are displayed respectively in Columns (1) and (2), and Columns (7) and (8). The fact that the coefficients in Columns (1) and (2) are positive seems to indicate that the positive effect on soliciting protection is more than compensating the negative one on the probability of pressing charges. The coefficients in Columns (7) and (8) are mostly not significant. Figure 9 shows the event-study analogues of all these combinations, while Tables D1 and D2 show the entire set of specifications for respectively Tables 9 and 10.

8 Heterogeneity at the Occurrence-Victim level

8.1 Behavioral results by Crime Type

The results in the previous section are consistent with two different explanations. The first, which we have hypothesized already in the paper, is that victims may have been discouraged by the harsher sentences of the *Lei do Feminicídio* from pressing charges against their abusive partner either as a form of consideration for their children or because of concerns about the

Figure 9: Event Study Estimates of Impact of the *Lei do Feminicídio* on Victims' Behavior

(A.) Probability of Soliciting Protection and Pressing Charges

(B.) Probability of Soliciting Protection but Not Pressing Charges

(C.) Probability of Not Soliciting Protection but Pressing Charges

(D.) Probability of Not Soliciting Protection and Not Pressing Charges
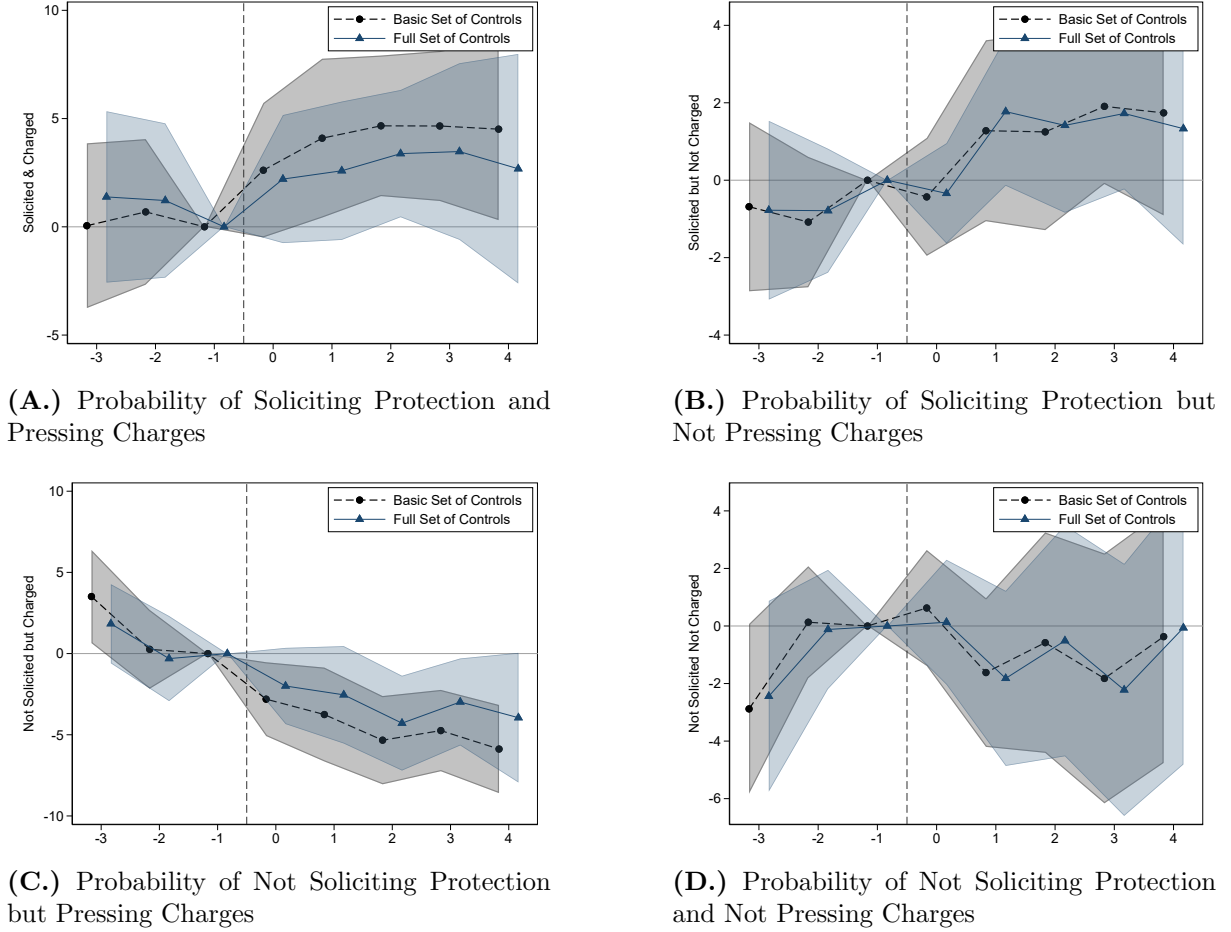
Notes: Event-study analogues of Table 10. Subfigure A reproduces Columns (1) and (2); Subfigure B reproduces Columns (3) and (4); Subfigure C reproduces Columns (5) and (6); Subfigure D reproduces Columns (7) and (8). Standard errors are clustered at the municipality level.

financial circumstances of the household (or a bit of both). In this scenario, soliciting more protective measures may function as a compensating mechanism that Domestic Violence (DV) victims resort to as an alternative to charging their abusive partner.

However, an alternative explanation is also possible. The *Lei do Feminicídio* may have reduced not only the incidence of DV, i.e., the extensive margin of DV, but also its intensity, i.e., the intensive margin of DV. Victims may view the request for protective measures as a first step and pressing charges as a last resort that is to be used only under extreme circumstances. In this scenario, our results so far may also reflect the fact that the *Lei do Feminicídio* might have reduced both the incidence of DV and the escalation of violence in domestic settings (i.e., the intensity of DV), thus causing a reduction in charges (i.e., in more serious cases) and an increase

in the requests of protective measures (i.e., in less serious cases). From a policy perspective, this would be arguably a more desirable outcome.

Table 11: Estimates of Impact of the *Lei do Feminicídio* on Behavior By Crime Type

	Panel A: Soliciting Protection				**Panel B:** Pressing Charges			
	Threat	Bodily Injuries	Fighting	Disorderly Conduct	Threat	Bodily Injuries	Fighting	Disorderly Conduct
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Exposure	2.483***	1.308***	-0.0444	-0.242	-0.975	-0.211	-0.839***	0.212**
	(0.824)	(0.412)	(0.274)	(0.278)	(1.103)	(0.333)	(0.307)	(0.0879)
R-squared	0.530	0.638	0.569	0.562	0.651	0.840	0.693	0.058
Pre Mean of Outcome	24.433	15.328	4.693	2.726	34.504	22.349	6.892	.361
Range of Exposure	.2	.2	.2	.2	.2	.2	.2	.2
Effect wrt Mean	2.03	1.71	.19	1.78	.561	.19	2.43	11.72
	Panel C: Soliciting Protection but not Pressing Charges				**Panel D:** not Soliciting Protection but Pressing Charges			
	Threat	Bodily Injuries	Fighting	Disorderly Conduct	Threat	Bodily Injuries	Fighting	Disorderly Conduct
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Exposure	1.512**	0.244	0.473**	-0.321	-1.946***	-1.273***	-0.321	0.133***
	(0.694)	(0.181)	(0.191)	(0.273)	(0.720)	(0.411)	(0.264)	(0.0482)
N of Observations	450,530	450,530	450,530	450,530	450,530	450,530	450,530	450,530
N of Municipalities	429	429	429	429	429	429	429	429
R-squared	0.075	0.030	0.067	0.522	0.144	0.187	0.181	0.020
Pre Mean of Outcome	1.716	0.769	0.37	2.516	11.787	7.79	2.569	0.151
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	17.62	6.36	25.56	2.55	3.3	3.27	2.5	17.55
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of soliciting protection in Panel A, of pressing charges in Panel B, of soliciting protection but not pressing charges in Panel C, and of not soliciting protection but pressing charges in Panel D. In all Columns, we consider the full set of controls from Tables 9 and 10, except the crime fixed effects. Columns (1) and (5) investigate threats, Columns (2) and (6) bodily injuries, Columns (3) and (7) fighting; and Columns (4) and (8) disorderly conduct. Standard errors are clustered at the municipality level.

To shed light on this, Table 11 reproduces Table 9 in Panel A (likelihood of soliciting protective measures), and Panel B (likelihood of pressing charges) by crime type. Similarly to the main results, a significant increase in the likelihood of requesting protective measures for threats and bodily injuries, while a significant decrease in the likelihood of pressing charges is observed for threats, bodily injuries, and fighting. Panel C and Panel D of Table 11 reproduce Table 10 to analyze the behavior response by crime type. The main results are still present for threats, bodily injuries, and fighting. The other combinations, i.e., the probability of both soliciting protection and pressing charges and the probability of doing neither, are displayed in Table D3.

Therefore, while we cannot fully rule out an effect of the *Lei do Feminicídio* on the intensity of violence and therefore on the composition of DV incidents, the consistency in the results within these three crime categories appears more coherent with the hypothesis that the behavior of victims changed, not the types of DV crimes that were committed after March 2015.

8.2 Serious DV and Family Involvement

Table 12: Estimates of Impact of the *Lei do Feminicídio* on Serious DV and Family Involvement

	Serious DV		Family is Involved		Caller is a Relative		Witness is a Relative	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Exposure	-1.428**	-0.786	0.0741	0.105	0.00646	0.0208	0.0677	0.0842
	(0.571)	(0.538)	(0.0732)	(0.0716)	(0.0254)	(0.0280)	(0.0681)	(0.0654)
N of Observations	450,565	450,530	450,565	450,530	450,565	450,530	450,565	450,530
N of Municipalities	432	429	432	429	432	429	432	429
R-squared	0.212	0.212	0.006	0.006	0.006	0.006	0.006	0.006
Pre Mean of Outcome	10.777	10.777	0.215	0.215	0.029	0.029	0.186	0.186
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	2.65	1.46	6.9	9.77	4.45	14.34	7.28	9.06
Controls	Basic	Full	Basic	Full	Basic	Full	Basic	Full

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of a serious DV, that family is involved, that the caller is a family member, and that the witness is a family member. Columns (1), (3), (5) and (7) report the estimates for the basic specification, i.e., with fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects; crime and police station fixed effects. Columns (2), (4), (6) and (8) report the estimates for the full specification, i.e., adding also the median income, the share of employed people, the level of education, the share of young people, the poverty rate and an urban index, interacted with year fixed effects. Standard errors are clustered at the municipality level.

An interesting analysis we can perform when looking at specific occurrences is to consider the gravity of them. For each occurrence-victim, we build a variable that considers whether the victim was seriously injured, hospitalized, or died. For this type of outcome, reporting should not be an issue, and results are shown in Columns (1) and (2) of Table 12. Column (1) shows a statistically significant and negative effect of the law, indicating a decrease in serious domestic violence incidents. In the full model, the coefficient is still negative but not statistically significant. The effect sizes relative to the mean outcome vary between 1.46% and 2.65%, demonstrating that the estimated impact of the law on serious DV is substantial but diminishes slightly as additional control variables are added. Overall, the results alleviate the concern that changes in reporting are driving our main results. Panel A of Figure 10**A.** shows the event-study analogue.

Another dimension we can investigate is whether family involvement in occurrences changed due to the *Lei do Feminicídio*. In particular, considering the same occurrences as for the behavior of victims we add information on other participants, i.e. callers and witnesses. We link participants to relationship data, from where we retrieve information on whether the participant is a father, mother, son/daughter or a sibling to the victim. In particular, Columns (3) and (4) of Table 12 consider whether the law had an effect on the probability that the family of the

victim is involved, either as a caller or a witness. Columns (5) and (6) of Table 12 consider only the case of the caller, while Columns (7) and (8) of Table 12 look at witnesses. All Columns report insignificant coefficients for the variable of interest, which leads to thinking that family involvement did not change after the law. Figure 10**B.**, Figure 10**C.** and Figure 10**D.** display the event-study analogues of Table 12. Moreover, Table D7 and Table D8 show the entire set of specifications for these outcomes.

Figure 10: Event Study Estimates of Impact of the *Lei do Feminicídio* on Serious DV and Family Involvement

(A.) Injured, hospitalized or dead

(B.) Witness or caller is a family member

(C.) Caller is a family member

(D.) Witness is a family member
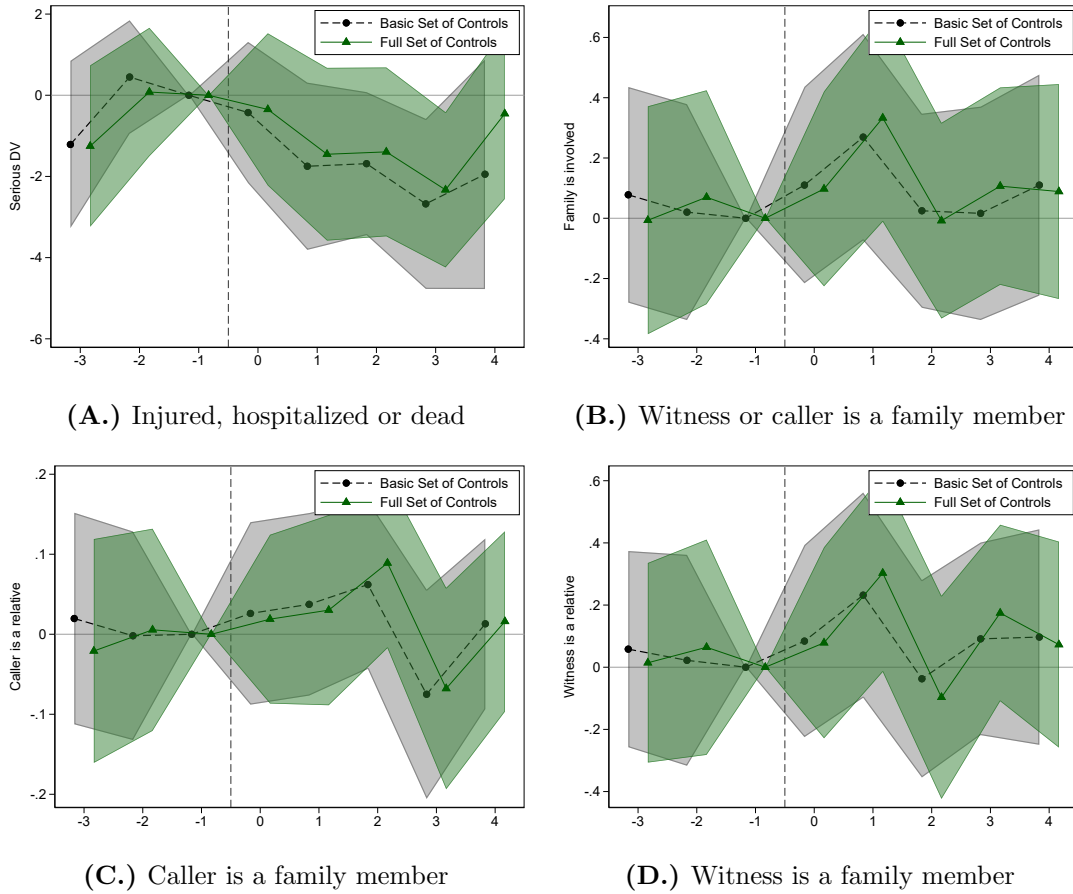
Notes: Event-study analogues of Table 12. Subfigure A reproduces Columns (1) and (2). Subfigure B reproduces Columns (3) and (4); Subfigure C Columns (5) and (6); Subfigure D Columns (7) and (8). Standard errors are clustered at the municipality level.

9   Conclusions

Increasing the severity of sentences can act as a deterrent for potential criminals. This notion has driven policy in the US and many other settings where sentences have been increased in an attempt to fight waves of crime and restore peace and order. However, in the particular case of Domestic Violence (DV), which has increased dramatically in many countries in recent decades and where victims typically have an intimate relationship with the offender, increases in severity may have unintended and undesirable effects as they may make victims more hesitant to press charges. This may obviously lead to undesirable outcomes for a policymaker seeking to guarantee order and public safety to her citizens.

We use administrative records from the Instituto Geral de Pericias (IGP) and police reports from the police authorities (*Secretaria de Seguranca Publica - Policia Civil*) during the 2010-2019 period for the entire population of RGS to empirically examine the *Lei do Feminicídio* that introduced the homicide category of *femicide* in the Penal Code of Brazil in March 2015. The reform aimed to tackle domestic violence and prevent the escalation of violence in domestic settings. To do so, we employ a set of Difference-In-Differences (DiD) specifications with a continuous treatment exposure. Our measure of exposure is defined as the share of men aged 18-55 who committed at least one violent crime against a woman at baseline in a woman's municipality of birth.

First, we show that the *Lei do Feminicídio* effectively deterred potential offenders, resulting in a significant reduction in the incidence of domestic violence. This finding is robust across various model specifications and sampling restrictions, and it is reinforced by a series of falsification and placebo tests, addressing concerns that our treatment exposure measure might capture unobserved municipality-year-specific shocks. The observed reduction in domestic violence is consistent with the predictions of Becker's model of crime (Becker, 1968), which posits that, *ceteris paribus*, harsher penalties serve as a deterrent to criminal behavior.

Secondly, we find that the *Lei do Feminicídio* also influenced the behavior of domestic violence (DV) victims. Specifically, our findings reveal that DV victims were more likely to request protective measures while becoming somewhat more hesitant to press charges against their abusive partners. We interpret these behavioral responses as a compensating mechanism, where victims strive to maintain their physical and psychological security while avoiding the financial loss and adverse consequences associated with the extended incarceration of their abusive partners.

Further analysis also shows that our results are not explained by a compositional change in

the types of DV offenses that are observed over time, as the inclusion of crime-fixed effects does not alter our main conclusion. Threats and bodily injuries are the main drivers of the reduced incidence of DV and they drive the increased propensity to request protective measures without pressing charges.

Therefore, the findings that emerge from the empirical study of this reform highlight a critical aspect of domestic violence legislation: the complex personal and financial ties that often exist between the victim and the perpetrator. Laws designed to combat domestic violence should be crafted with careful consideration of their potential impact on victims' willingness to come forward. Legislation should aim to guarantee sufficient deterrence against potential offenders while also ensuring that victims feel supported and empowered to report crimes. Careful consideration of this delicate balance is crucial to ensure that laws can effectively protect victims without inadvertently discouraging them from pursuing justice.

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Appendix A

In this section, we display the age profiles of female victims and male offenders who are above 18 years old. Note that these do not correspond to our final sample, as we restrict victims and offenders to be within the age range of 18-55. These figures are useful in demonstrating that by limiting the age to below 55, we capture the majority of cases.

Figure A1: Age Profile of Victims & Offenders for First Offence
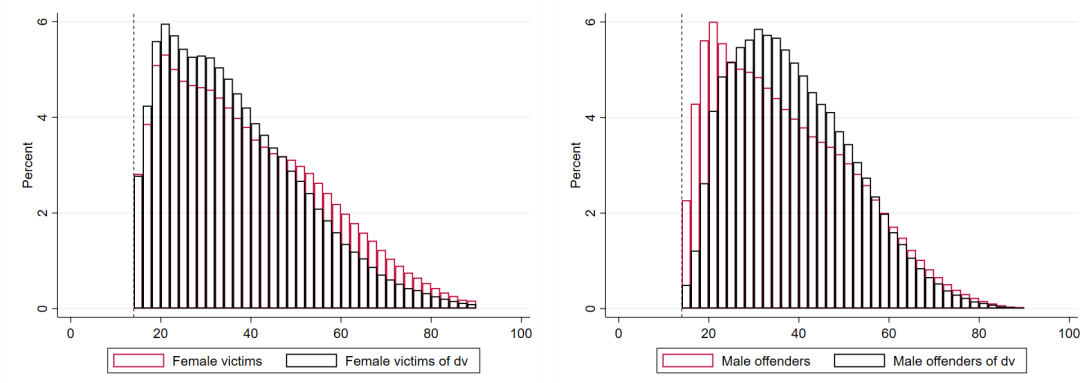
Notes: The figure displays the age profile of female victims and male offenders for first offence. Notice that here we consider everyone above 18 y.o. In red all crimes are considered, in black DV crimes.

Figure A2: Age Profile of Victims & Offenders for Any Offence
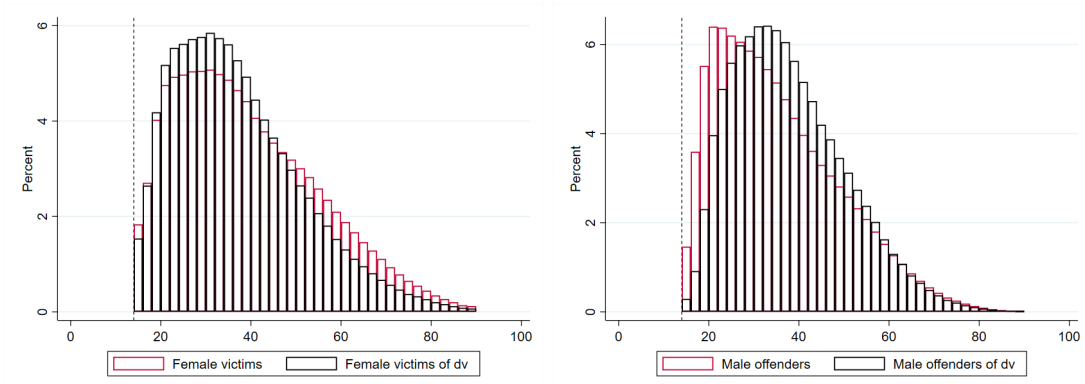
Notes: The figure displays the age profile of female victims and male offenders for any offence. Notice that here we consider everyone above 18 y.o. In red all crimes are considered, in black DV crimes.

Figure A1 shows the age profiles of female victims and male offenders for their first offense, categorized by domestic violence (DV) and non-DV offenses. Female victims of DV tend to be younger than female victims of any crime when considering their first victimization. Conversely,

male offenders are older when committing their first DV crime compared to their first general crime.

Figure A2 displays the age profiles for any offense. Once again, female victims of DV tend to be younger than female victims of any crime, while male offenders are older in cases of DV compared to non-DV offenses.

Figure A3: Age Profile of DV Victims & Offenders by Level of Schooling
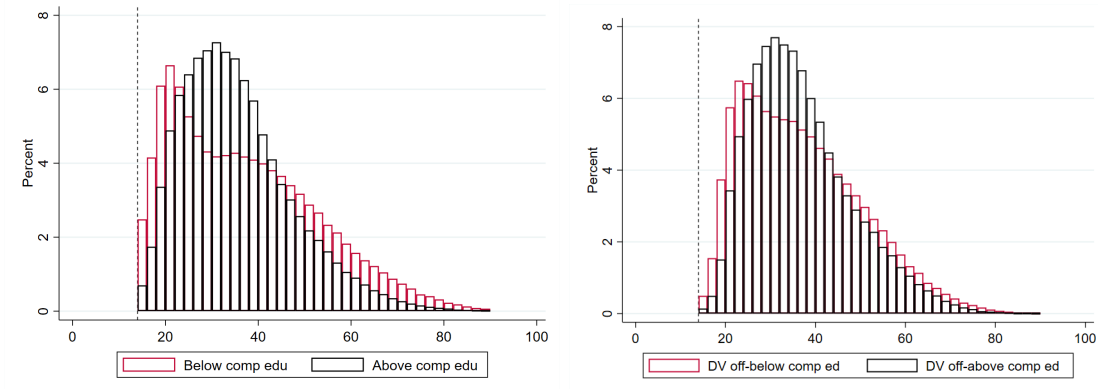
Notes: The figure displays the age profile of female victims and male offenders for any DV offence by level of schooling. Notice that here we consider everyone above 18 y.o. In red victims and offenders are below the compulsory education, in black above.

Figure A4: Age Profile of DV Victims & Offenders by Marital Status
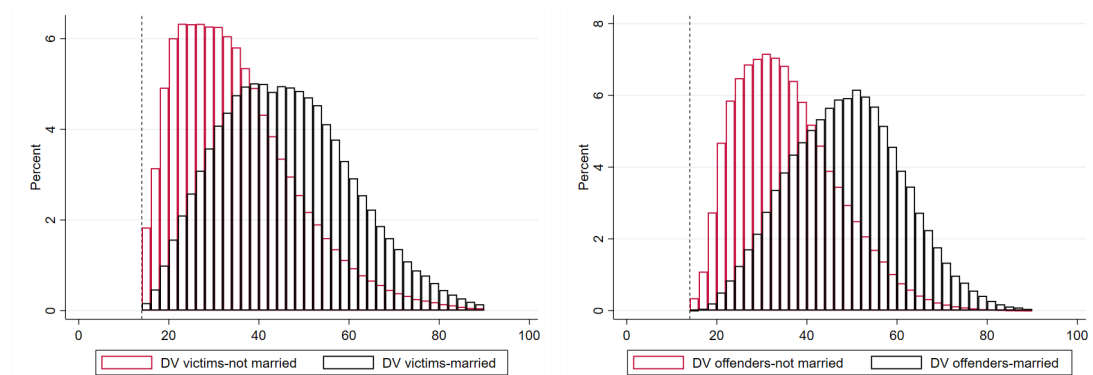
Notes: The figure displays the age profile of female victims and male offenders for any DV offence by marital status. Notice that here we consider everyone above 18 y.o. In red victims and offenders are non-married, in black married.

Figure A3 shows the age profiles of female victims and male offenders of DV, categorized by their level of schooling. Both female victims and male offenders tend to be younger when they have not completed compulsory education compared to when they are more educated. This may be due to a mechanical reason: individuals who are more educated are generally older. However,

it is important to note that compulsory education in Brazil requires individuals to stay in school until they are 17 years old.

Figure A4 displays the age profiles of female victims and male offenders of DV, categorized by marital status. Both female victims and male offenders are significantly younger when they are unmarried compared to when they are married. These age profiles likely reflect the fact that people are more likely to be married when they are relatively older.

Table A1: Descriptive Statistics for the Entire Sample

Panel A: Exposure and Outcomes			**Panel B:** Municipal Controls		
Variable	Mean	SD	Variable	Mean/Proportion	SD
Exposure (%)	0.75	0.228	Black	0.10	0.303
P75-P25 Range of Exposure	0.24	0.000	Median Monthly Income in 2010	539.19	136.067
Exposure-Male (%)	0.51	0.154	Education Index in 2010	0.66	0.059
P75-P25 Range of Exposure M	0.12	0.000	Share of Employment in 2010	0.67	0.058
Probability of DV (%)	1.96	13.878	Share of Youth in 2010	0.18	0.047
Number of DV	0.02	0.183	Poverty Index in 2010	2.01	2.145
Probability of non-DV (%)	4.97	21.722	Urban Index in 2010	0.81	0.394
Probability of first DV (%)	1.24	11.048			
Probability of repeated DV (%)	0.73	8.504			
Observations	19,180,075		Observations	19,180,075	

Notes: The table shows descriptive statistics for our final sample of women aged 18-55. The observation unit is a woman-period.

Table A2: Descriptive Statistics for Domestic Violence Incidents

Panel A: Exposure and Outcomes			**Panel B:** Municipal Controls		
Variable	Mean	SD	Variable	Mean/Proportion	SD
Exposure (%)	0.78	0.215	Black	0.16	0.366
P75-P25 Range of Exposure	0.20	0.000	Median Monthly Income in 2010	543.00	136.955
Exposure- Male (%)	0.52	0.141	Education Index in 2010	0.66	0.058
P75-P25 Range of Exposure M	0.12	0.000	Share of Employment in 2010	0.67	0.056
Witness is a family member (%)	0.23	4.773	Share of Youth in 2010	0.18	0.043
Caller is a family member (%)	0.04	2.053	Poverty Index in 2010	1.98	2.090
Charged individually (%)	69.88	45.879	Urban Index in 2010	0.84	0.366
Protection Solicited individually (%)	60.74	48.834			
Charged & Solicited (%)	51.48	49.978			
Solicited but Not Charged (%)	9.26	28.986			
Not Solicited but Charged (%)	18.40	38.747			
Not Solicited & Not Charged (%)	20.86	40.634			
Observations	450,668		Observations	450,668	

Notes: The table shows descriptive statistics for our final sample of domestic violence incidents. The observation unit is an occurrence-victim.

Appendix B

Figure B1: Share of Violent Male Offenders against Men in 2010

Share of Violent Male Offenders Against Men
2010
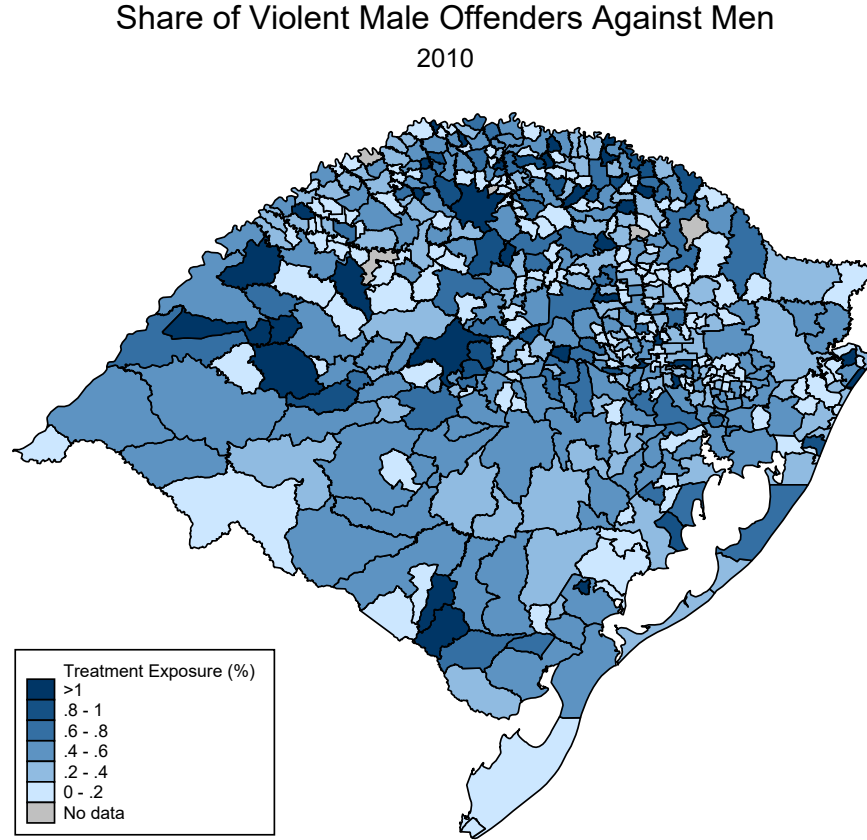
Figure B1 shows the map of the share of violent male offenders against men at baseline: EXP_{icm}^m. Figure B2 shows the change in Non-DV victimization against the treatment exposure for the *before* and *after* periods. Figure B3 shows the change in DV victimization against the placebo exposure (EXP_{icm}^m) for the *before* and *after* periods.
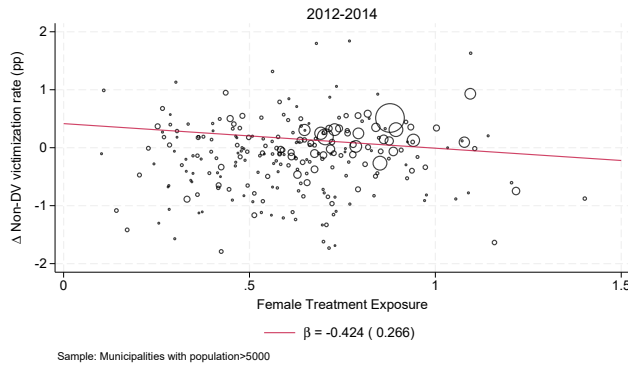
(A.) Before the *Lei do Feminicídio*
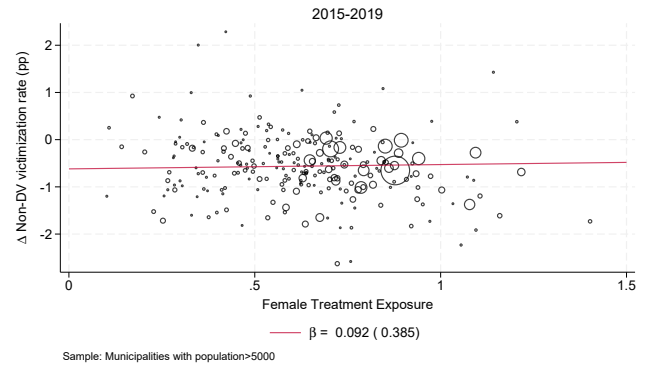
(B.) After the *Lei do Feminicídio*

Figure B2: Treatment Exposure vs Change in Risk of Non-Domestic Violence Before and After the *Lei do Feminicídio*
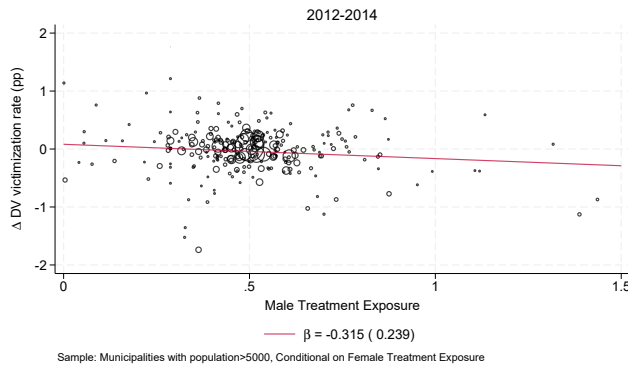
(A.) Pre-reform Exposure M
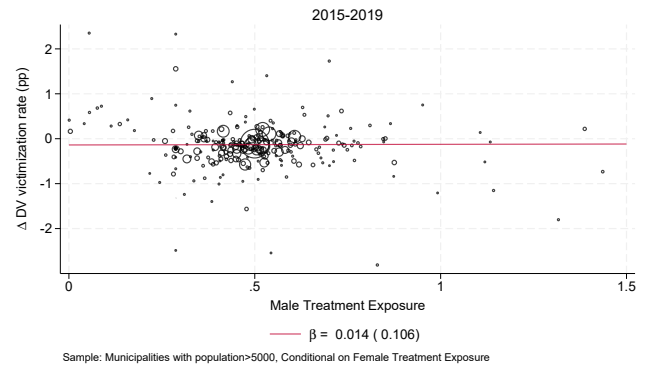
(B.) Post-reform Exposure M

Figure B3: Treatment Exposure M vs Change in Risk of Domestic Violence Before and After the *Lei do Feminicídio*

Appendix C

Table C1: Estimates of Impact of the *Lei do Feminicídio* on Risk of First and Repeated Domestic Violence

Panel A:	Probability of first DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.340***	-0.311***	-0.310***	-0.310***	-0.308***	-0.265***
	(0.0634)	(0.0505)	(0.0504)	(0.0504)	(0.0501)	(0.0445)
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	1.988	1.988	1.988	1.988	1.988	1.988
Range of Exposure	.24	.24	.24	.24	.24	.24
Effect wrt Mean	4.11	3.76	3.74	3.75	3.72	3.2
Panel B:	**Probability of repeated DV**					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.0451	0.0718**	0.0732**	0.0735**	0.0731**	0.0613*
	(0.0389)	(0.0324)	(0.0323)	(0.0318)	(0.0318)	(0.0337)
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	0	0	0	0	0	0
Range of Exposure	.24	.24	.24	.24	.24	.24
Effect wrt Mean	.	.	.	.	.	.
N of Observations	19,180,075	19,179,550	19,179,550	19,179,550	19,176,518	19,176,518
N of Municipalities	492	491	491	491	487	487
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability, for all women, of first DV in Panel A, and of repeated DV in Panel B. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table C2: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence - women aged 18-25

Panel A:	Probability of DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.357***	-0.271***	-0.268***	-0.282***	-0.280***	-0.207***
	(0.104)	(0.0775)	(0.0775)	(0.0805)	(0.0806)	(0.0718)
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	2.232	2.232	2.232	2.232	2.232	2.232
Range of Exposure	0.216	0.216	0.216	0.216	0.216	0.216
Effect wrt Mean	3.45	2.62	2.59	2.72	2.71	2.00
Panel B:	**Probability of first DV**					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.333***	-0.275***	-0.272***	-0.277***	-0.276***	-0.202***
	(0.0926)	(0.0742)	(0.0737)	(0.0759)	(0.0757)	(0.0645)
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	2.232	2.232	2.232	2.232	2.232	2.232
Range of Exposure	0.216	0.216	0.216	0.216	0.216	0.216
Effect wrt Mean	3.23	2.66	2.63	2.69	2.68	1.95
Panel C:	**Probability of repeated DV**					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.0234	0.00429	0.00422	-0.00402	-0.00400	-0.00488
	(0.0379)	(0.0356)	(0.0358)	(0.0351)	(0.0352)	(0.0377)
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	0	0	0	0	0	0
Range of Exposure	0.216	0.216	0.216	0.216	0.216	0.216
Effect wrt Mean	.	.	.	.	.	.
N of Observations	4,135,375	4,135,355	4,135,355	4,135,355	4,135,247	4,135,247
N of Municipalities	468	467	467	467	464	464
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability, for women aged 18-25, of DV in Panel A, of first DV in Panel B, and of repeated DV in Panel C. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table C3: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence - women aged 26-35

Panel A:	Probability of DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.323***	-0.232***	-0.228***	-0.227***	-0.223***	-0.226***
	(0.118)	(0.0693)	(0.0680)	(0.0676)	(0.0647)	(0.0627)
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	2.347	2.347	2.347	2.347	2.347	2.347
Range of Exposure	.23	.23	.23	.23	.23	.23
Effect wrt Mean	3.17	2.28	2.24	2.22	2.19	2.21

Panel B:	Probability of first DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.480***	-0.431***	-0.431***	-0.430***	-0.427***	-0.385***
	(0.0965)	(0.0796)	(0.0793)	(0.0791)	(0.0787)	(0.0768)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	2.347	2.347	2.347	2.347	2.347	2.347
Range of Exposure	.23	.23	.23	.23	.23	.23
Effect wrt Mean	4.71	4.22	4.22	4.21	4.18	3.77

Panel C:	Probability of repeated DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.157**	0.197***	0.202***	0.203***	0.204***	0.160***
	(0.0771)	(0.0642)	(0.0639)	(0.0643)	(0.0633)	(0.0596)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	0	0	0	0	0	0
Range of Exposure	.23	.23	.23	.23	.23	.23
Effect wrt Mean	.	.	.	.	.	.
N of Observations	5,646,804	5,646,728	5,646,728	5,646,728	5,646,407	5,646,407
N of Municipalities	480	479	479	479	475	475
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability, for women aged 26-35, of DV in Panel A, of first DV in Panel B, and of repeated DV in Panel C. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table C4: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence - women aged 36-45

Panel A:	Probability of DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.296***	-0.257***	-0.253***	-0.251***	-0.244***	-0.237***
	(0.0783)	(0.0577)	(0.0578)	(0.0592)	(0.0592)	(0.0615)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	1.956	1.956	1.956	1.956	1.956	1.956
Range of Exposure	.242	.242	.242	.242	.242	.242
Effect wrt Mean	3.67	3.17	3.13	3.1	3.02	2.93

Panel B:	Probability of first DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.336***	-0.314***	-0.314***	-0.318***	-0.311***	-0.283***
	(0.0679)	(0.0526)	(0.0526)	(0.0527)	(0.0520)	(0.0506)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	1.956	1.956	1.956	1.956	1.956	1.956
Range of Exposure	.242	.242	.242	.242	.242	.242
Effect wrt Mean	4.16	3.89	3.88	3.94	3.85	3.5

Panel C:	Probability of repeated DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.0396	0.0586	0.0608	0.0673	0.0671	0.0460
	(0.0502)	(0.0491)	(0.0487)	(0.0487)	(0.0487)	(0.0482)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	0	0	0	0	0	0
Range of Exposure	.242	.242	.242	.242	.242	.242
Effect wrt Mean	.	.	.	.	.	.
N of Observations	4,527,740	4,527,636	4,527,636	4,527,636	4,526,713	4,526,713
N of Municipalities	475	474	474	474	470	470
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability, for women aged 36-45, of DV in Panel A, of first DV in Panel B, and of repeated DV in Panel C. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table C5: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence - women aged 46-55

Panel A:	Probability of DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.151***	-0.158***	-0.156***	-0.148***	-0.150***	-0.116**
	(0.0457)	(0.0480)	(0.0481)	(0.0487)	(0.0492)	(0.0485)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	1.245	1.245	1.245	1.245	1.245	1.245
Range of Exposure	.274	.274	.274	.274	.274	.274
Effect wrt Mean	3.33	3.48	3.44	3.26	3.3	2.55

Panel B:	Probability of first DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.154***	-0.171***	-0.168***	-0.164***	-0.164***	-0.141***
	(0.0435)	(0.0446)	(0.0446)	(0.0450)	(0.0455)	(0.0436)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	1.245	1.245	1.245	1.245	1.245	1.245
Range of Exposure	.274	.274	.274	.274	.274	.274
Effect wrt Mean	3.38	3.76	3.71	3.61	3.62	3.11

Panel C:	Probability of repeated DV					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.00213	0.0128	0.0122	0.0158	0.0143	0.0252
	(0.0247)	(0.0245)	(0.0245)	(0.0250)	(0.0250)	(0.0282)
R-squared	0.001	0.001	0.001	0.001	0.001	0.001
Pre Mean of Outcome	0	0	0	0	0	0
Range of Exposure	.274	.274	.274	.274	.274	.274
Effect wrt Mean	.	.	.	.	.	.
N of Observations	4,071,702	4,071,397	4,071,397	4,071,397	4,069,828	4,069,828
N of Municipalities	476	475	475	475	471	471
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability, for women aged 46-55, of DV in Panel A, of first DV in Panel B, and of repeated DV in Panel C. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Appendix D

D.1 Behavioral Responses for Occurrence-Victim Analysis

Table D1: Estimates of Impact of the *Lei do Feminicídio* on Soliciting Protective Measures and Pressing Charges - All Specifications

Panel A:	Soliciting Protection					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	5.561***	3.679**	3.647**	3.693**	3.660**	3.723**
	(1.475)	(1.602)	(1.607)	(1.578)	(1.595)	(1.588)
R-squared	0.173	0.173	0.173	0.173	0.173	0.173
Pre Mean of Outcome	50.933	50.933	50.933	50.933	50.933	50.933
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	2.18	1.44	1.43	1.45	1.44	1.46
Panel B:	**Pressing Charges**					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-1.900	-1.891	-1.925	-1.818	-1.900	-1.603
	(1.524)	(1.562)	(1.562)	(1.557)	(1.541)	(1.527)
R-squared	0.303	0.303	0.303	0.303	0.303	0.303
Pre Mean of Outcome	70.8	70.8	70.8	70.8	70.8	70.8
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	0.54	0.53	0.54	0.51	0.54	0.45
N of Observations	450,565	450,546	450,546	450,546	450,530	450,530
N of Municipalities	432	431	431	431	429	429
Black	Yes	Yes	Yes	Yes	Yes	Yes
Crime/police station FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of soliciting protection in Panel A and pressing charges in Panel B. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. We also include crime and police station fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table D2: Estimates of Impact of the *Lei do Feminicídio* on Victims' Behavior
All Specifications

	Charged and Solicited						Not Charged but Solicited					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	3.839**	2.019	1.980	2.016	2.001	2.023	1.722*	1.659*	1.667*	1.678*	1.662*	1.689*
	(1.706)	(1.637)	(1.636)	(1.609)	(1.622)	(1.644)	(0.896)	(0.910)	(0.907)	(0.899)	(0.880)	(0.880)
R-squared	0.223	0.223	0.223	0.223	0.223	0.223	0.248	0.248	0.248	0.248	0.248	0.248
Pre Mean of Outcome	44.768	44.768	44.768	44.768	44.768	44.768	6.164	6.164	6.164	6.164	6.164	6.164
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	1.72	0.9	0.88	0.9	0.89	0.9	5.59	5.38	5.41	5.44	5.39	5.48

	Charged but Not Solicited						Not Charged and Not Solicited					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-5.739***	-3.911***	-3.906***	-3.833***	-3.901***	-3.633***	0.178	0.232	0.259	0.142	0.239	-0.0853
	(1.381)	(1.346)	(1.323)	(1.323)	(1.321)	(1.250)	(1.357)	(1.324)	(1.312)	(1.301)	(1.293)	(1.284)
R-squared	0.121	0.122	0.122	0.122	0.122	0.122	0.173	0.173	0.173	0.173	0.173	0.173
Pre Mean of Outcome	26.031	26.031	26.031	26.031	26.031	26.031	23.036	23.036	23.036	23.036	23.036	23.036
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	4.41	3.01	3.00	2.95	3.00	2.79	0.15	0.20	0.22	0.12	0.21	0.07
N of Observations	450,565	450,546	450,546	450,546	450,530	450,530	450,565	450,546	450,546	450,546	450,530	450,530
N of Municipalities	432	431	431	431	429	429	432	431	431	431	429	429
Black	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crime/police station FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the combinations of behaviors. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. We also include crime and police station fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table D3: Estimates of Impact of the *Lei do Feminicídio* on Behavior By Crime Type

	Probability of Pressing Charges and Soliciting Protection				Probability of not Pressing Charges and not Soliciting Protection			
	Threat	Bodily Injuries	Fighting	Disorderly Conduct	Threat	Bodily Injuries	Fighting	Disorderly Conduct
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Exposure	0.973	1.063**	-0.517*	0.0789	-0.537	-0.0364	0.367*	0.110
	(1.069)	(0.428)	(0.285)	(0.0646)	(0.834)	(0.254)	(0.212)	(0.288)
N of Observations	450,530	450,530	450,530	450,530	450,530	450,530	450,530	450,530
N of Municipalities	429	429	429	429	429	429	429	429
R-squared	0.464	0.606	0.507	0.041	0.136	0.083	0.214	0.398
Pre Mean of Outcome	22.717	14.558	4.323	0.21	10.23	3.927	2.215	2.647
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	0.86	1.46	2.39	7.52	1.05	0.19	3.32	0.831
Full Set of Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of pressing charges and soliciting protection, and of doing neither by crime type. In all Columns, we consider the full set of controls from Table 10, except the crime fixed effects. Columns (1) and (5) investigate threats, Columns (2) and (6) bodily injuries, Columns (3) and (7) fighting; and Columns (4) and (8) disorderly conduct. Standard errors are clustered at the municipality level.

D.2 Behavioral Responses with Panel Data Structure

Table D4: Descriptive Statistics on Behavior for the Entire Sample

	Mean	SD
Share of Solicited DV (%)	62.40	46.203
Share of Charged DV (%)	69.91	44.281
Share of Solicited and Charged DV (%)	52.94	47.769
Share of Not Solicited but Charged DV (%)	16.96	35.668
Share of Solicited but Not Charged DV (%)	9.46	28.226
Share of Not Solicited and Not Charged DV (%)	20.64	38.960
Observations	376784	

Table D4 provides descriptive statistics for the behavior of victims of domestic violence, when considering the panel data design of the first part.

Panel A of Table D5 shows results for the likelihood of a DV victim soliciting protective measures by the police to keep her safe from her abusive partner after a DV incident. The same set of fixed effects and controls is gradually added across Columns as in Table 1. Estimates in Panel A of Table D5 indicate that, regardless of the set of controls, the propensity of victims to solicit protective measures increased due to the introduction of the *Lei do Feminicídio*. In particular, the likelihood of DV victims soliciting protection from the abusive partner in municipalities at the 75th percentile of our treatment exposure measure increased by 0.96% to 1.7% more than in municipalities at the 25th percentile of our treatment exposure measure.

Panel B of Table D5 shows results for the likelihood of a DV victim pressing charges against her abusive partner after a DV incident. The same set of fixed effects and controls is gradually added across Columns as in Table 1. Estimates in Panel B of Table D5 indicate that, regardless of the set of controls and even with the panel data structure, the propensity of victims to press charges decreased as a result of the introduction of the *Lei do Feminicídio*. In particular, the likelihood of DV victims pressing charges against the abusive partner in municipalities at the 75th percentile of our treatment exposure measure decreased by 0.6% more than in municipalities at the 25th percentile of our treatment exposure measure.

Table D5: Estimates of Impact of the *Lei do Feminicídio* on Likelihood to Solicit Protective Measures and Pressing Charges - Panel Structure

Panel A:	Share of Solicited					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	4.378***	2.540**	2.526**	2.580**	2.552**	2.463*
	(1.200)	(1.267)	(1.266)	(1.267)	(1.257)	(1.274)
R-squared	0.092	0.092	0.092	0.092	0.092	0.092
Pre Mean of Outcome	51.542	51.542	51.542	51.542	51.542	51.542
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	1.7	0.99	0.98	1.0	0.99	0.96
Panel B:	**Share of Charged**					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-2.052	-2.308*	-2.220*	-2.122	-2.090	-2.017
	(1.274)	(1.323)	(1.338)	(1.328)	(1.320)	(1.280)
R-squared	0.250	0.250	0.250	0.250	0.250	0.250
Pre Mean of Outcome	70.128	70.128	70.128	70.128	70.128	70.128
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	0.59	0.66	0.63	0.61	0.60	0.58
N of Observations	376,732	376,720	376,720	376,720	376,705	376,705
N of Municipalities	434	433	433	433	431	431
Black	Yes	Yes	Yes	Yes	Yes	Yes
Crime FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the share of solicited and charged incidents per year. In all specifications, we include fixed effects for race, year-of-birth, year and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. We also include crime fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table D6: Estimates of Impact of the *Lei do Feminicídio* on Victims' Behavior - Panel Structure

	Panel A: Share of Solicited and Charged						Panel B: Share of Charged but Not Solicited					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	2.748*	0.773	0.740	0.785	0.796	0.682	-4.799***	-3.030**	-2.959**	-2.907**	-2.886**	-2.705**
	(1.652)	(1.429)	(1.416)	(1.415)	(1.393)	(1.408)	(1.428)	(1.242)	(1.221)	(1.222)	(1.217)	(1.187)
R-squared	0.160	0.160	0.160	0.160	0.160	0.160	0.069	0.069	0.069	0.069	0.069	0.070
Pre Mean of Outcome	45.308	45.308	45.308	45.308	45.308	45.308	24.82	24.82	24.82	24.82	24.82	24.82
Range of Exposure	.2	.2	.2	.2	.2	.2	.2	.2	.2	.2	.2	.2
Effect wrt Mean	1.21	.34	.33	.35	.35	.3	3.87	2.44	2.38	2.34	2.33	2.18

	Panel C: Share of Solicited but Not Charged						Panel D: Share of Not Solicited and Not Charged					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	1.630	1.760*	1.786*	1.795*	1.756*	1.782*	0.421	0.542	0.432	0.327	0.333	0.235
	(1.007)	(0.992)	(0.983)	(0.981)	(0.949)	(0.950)	(0.990)	(0.944)	(0.956)	(0.957)	(0.962)	(0.940)
R-squared	0.219	0.219	0.219	0.219	0.219	0.219	0.120	0.120	0.120	0.120	0.120	0.120
Pre Mean of Outcome	6.234	6.234	6.234	6.234	6.234	6.234	23.638	23.638	23.638	23.638	23.638	23.638
Range of Exposure	.2	.2	.2	.2	.2	.2	.2	.2	.2	.2	.2	.2
Effect wrt Mean	5.23	5.65	5.73	5.76	5.63	5.72	.36	.46	.37	.28	.28	.2
N of Observations	376,732	376,720	376,720	376,720	376,705	376,705	376,732	376,720	376,720	376,720	376,705	376,705
N of Municipalities	434	433	433	433	431	431	434	433	433	433	431	431
Black	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Crime FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the share of all the combinations of behaviors per year. In all specifications, we include fixed effects for race, year-of-birth, year and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. We also include crime fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table D6 models as dependent variables the various combinations of charges and protective measures. Estimates indicate that the *Lei do Feminicídio* had the greatest influence on the likelihood by DV victims to press charges without requesting protective measures. Results for this outcome are displayed in Panel B of D6, which is organized in the same way as Table D5. Estimates in Panel B of Table D6 still compare municipalities at the 75th percentile of our treatment exposure measure with those at the 25th percentile, and they indicate that the likelihood of victims pressing charges without requesting protection from their abusive partner reduced by a sizeable 2.18% to 3.87%.

D.3 Serious DV and Family Involvement

Table D7: Event Study Estimates of Impact of the *Lei do Feminicídio* on Serious DV and Family Involvement

Panel A:	Serious Domestic Violence					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-1.428**	-0.982*	-0.992*	-0.972*	-0.983*	-0.786
	(0.571)	(0.531)	(0.527)	(0.531)	(0.528)	(0.538)
R-squared	0.212	0.212	0.212	0.212	0.212	0.212
Pre Mean of Outcome	10.777	10.777	10.777	10.777	10.777	10.777
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	2.65	1.82	1.84	1.8	1.82	1.46
Panel B:	Caller or Witness is a Family Member					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.0741	0.0650	0.0640	0.0638	0.0655	0.105
	(0.0732)	(0.0770)	(0.0766)	(0.0763)	(0.0736)	(0.0716)
R-squared	0.006	0.006	0.006	0.006	0.006	0.006
Pre Mean of Outcome	0.215	0.215	0.215	0.215	0.215	0.215
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	6.9	6.05	5.95	5.94	6.101	9.77
N of Observations	450,565	450,546	450,546	450,546	450,530	450,530
N of Municipalities	432	431	431	431	429	429
Black	Yes	Yes	Yes	Yes	Yes	Yes
Crime/police station FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of a serious DV and that family is involved. In all specifications, we include fixed effects for race, year-of-birth, year and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. We also include crime and police station fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Table D8: Estimates of Impact of the *Lei do Feminicídio* on Likelihood that Caller or Witness is a Family Member

Panel A:	Caller is a Relative					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.00646	0.0140	0.0141	0.0134	0.0143	0.0208
	(0.0254)	(0.0275)	(0.0275)	(0.0275)	(0.0275)	(0.0280)
R-squared	0.006	0.006	0.006	0.006	0.006	0.006
Pre Mean of Outcome	0.029	0.029	0.029	0.029	0.029	0.029
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	4.45	9.68	9.73	9.211	9.86	14.34
Panel B:	**Witness is a Relative**					
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.0677	0.0510	0.0499	0.0505	0.0512	0.0842
	(0.0681)	(0.0698)	(0.0693)	(0.0689)	(0.0668)	(0.0654)
R-squared	0.006	0.006	0.006	0.006	0.006	0.006
Pre Mean of Outcome	0.186	0.186	0.186	0.186	0.186	0.186
Range of Exposure	0.2	0.2	0.2	0.2	0.2	0.2
Effect wrt Mean	7.28	5.48	5.36	5.43	5.51	9.06
N of Observations	450,565	450,546	450,546	450,546	450,530	450,530
N of Municipalities	432	431	431	431	429	429
Black	Yes	Yes	Yes	Yes	Yes	Yes
Crime/police station FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability that the caller is a family member in Panel A and that the witness is a family member in Panel B. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. We also include crime and police station fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

Appendix E   Robustness Checks

E.1   Number of DV Episodes and Urban Municipalities

Table E1: Estimates of Impact of the *Lei do Feminicídio* on Number of Episodes of DV in All Municipalities and on Risk of Domestic Violence in Urban Municipalities

Panel A:	All Municipalities					
	Number of DV (x100)	Number of DV (x100)	Number of DV (x100)	Number of DV (x100)	Number of DV (x100)	Number of DV (x100)
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.366***	-0.291***	-0.288***	-0.288***	-0.287***	-0.244***
	(0.0938)	(0.0527)	(0.0524)	(0.0527)	(0.0523)	(0.0493)
N of Observations	19,180,075	19,179,550	19,179,550	19,179,550	19,176,518	19,176,518
N of Municipalities	492	491	491	491	487	487
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	2.388	2.388	2.388	2.388	2.388	2.388
Range of Exposure	.24	.24	.24	.24	.24	.24
Effect wrt Mean	3.68	2.92	2.89	2.89	2.88	2.45
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes
Panel B:	**Urban Municipalities**					
	Probability of DV	Probability of DV	Probability of DV	Probability of DV	Probability of DV	Probability of DV
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.315***	-0.259***	-0.252***	-0.260***	-0.261***	-0.265***
	(0.0743)	(0.0538)	(0.0538)	(0.0552)	(0.0537)	(0.0537)
N of Observations	15,491,081	15,490,556	15,490,556	15,490,556	15,490,556	15,490,556
N of Municipalities	142	141	141	141	141	141
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	2.07	2.07	2.07	2.07	2.07	2.07
Range of Exposure	.174	.174	.174	.174	.174	.174
Effect wrt Mean	2.64	2.18	2.12	2.19	2.19	2.23
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Income/employment	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Share of youth	No	No	No	Yes	Yes	Yes
Poverty rate	No	No	No	No	Yes	Yes
Urban index	No	No	No	No	No	Yes

Notes: Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the number of episodes of DV in a year, multiplied by 100 in Panel A, and the probability of DV in Urban areas in Panel B. In all specifications, we include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. From Column (2) to Column (6), we gradually add extra controls measured in 2010 at the municipality level interacted with the year fixed effects: these are, namely, the median income and the share of employed people (Column (2)), the level of education (Column (3)), the share of young people (Column (4)), the poverty rate (Column (5)) and an urban index (Column (6)). Standard errors are clustered at the municipality level.

E.2 Different Exposures

Table E2: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence and Non with Different Exposures

Panel A:	Probability of DV					
	Basic Set of Controls			**Full Set of Controls**		
	Exposure	Exposure DV	Exposure DV violent	Exposure	Exposure DV	Exposure DV violent
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	-0.295***			-0.204***		
	(0.0688)			(0.0379)		
Post*Exposure DV		-0.161***			-0.135***	
		(0.0330)			(0.0220)	
Post*Exposure DV violent			-0.325***			-0.228***
			(0.0796)			(0.0453)
N of Observations	19,180,075	19,180,075	19,180,075	19,176,518	19,176,518	19,176,518
N of Municipalities	492	492	492	487	487	487
R-squared	0.002	0.002	0.002	0.002	0.002	0.002
Pre Mean of Outcome	1.988	1.988	1.988	1.988	1.988	1.988
Range of Exposure	0.24	0.345	0.153	0.24	0.345	0.153
Effect wrt Mean	3.56	2.79	2.5	2.46	2.34	1.76
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Controls*year	No	No	No	Yes	Yes	Yes

Panel B:	Probability of Non-DV					
	Basic Set of Controls			**Full Set of Controls**		
	Exposure	Exposure DV	Exposure DV violent	Exposure	Exposure DV	Exposure DV violent
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Exposure	0.144			-0.0105		
	(0.199)			(0.122)		
Post*Exposure DV		0.0257			-0.0146	
		(0.0918)			(0.0677)	
Post*Exposure DV violent			0.126			0.00440
			(0.219)			(0.160)
N of Observations	19,180,075	19,180,075	19,180,075	19,176,518	19,176,518	19,176,518
N of Municipalities	492	492	492	487	487	487
R-squared	0.004	0.004	0.004	0.004	0.004	0.004
Pre Mean of Outcome	5.12	5.12	5.12	5.12	5.12	5.12
Range of Exposure	.24	.345	.153	.24	.345	.153
Effect wrt Mean	.68	.17	.38	.05	.1	.01
Black	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Controls*year	No	No	No	Yes	Yes	Yes

Notes: Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variables are the probability of being a victim of DV in Panel A, and of non-DV in Panel B. Columns (1), (2), and (3) include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. Columns (4), (5), and (6) add controls measured in 2010 at the municipality level interacted with the year fixed effects. Standard errors are clustered at the municipality level.

E.3 Municipality of Birth vs Municipality of Residence

Table E3: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence for all women born and residing in RGS

	Probability of DV for all women born and residing in RGS			
	Basic Set of Controls		Full Set of Controls	
	(1)	(2)	(3)	(4)
Post*Exposure-Residence	-0.0672		-0.0386	
	(0.0446)		(0.0280)	
Post*Exposure-Birth		-0.346***		-0.251***
		(0.0699)		(0.0409)
N of Observations	16,352,833	16,352,833	16,338,511	16,349,480
N of Municipalities	493	492	488	487
R-squared	0.002	0.002	0.002	0.002
Pre Mean of Outcome	1.988	1.988	1.988	1.988
Range of Exposure	.24	.242	.24	.242
Effect wrt Mean	.811	4.21	.47	3.05
Black	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes
Municipality of Residence FE	Yes	Yes	Yes	Yes
Municipality of Birth FE	Yes	Yes	Yes	Yes
Municipal Controls*year	No	No	Yes	Yes

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is the probability of DV. Columns (1) and (2) include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. Columns (3) and (4) add controls measured in 2010 at the municipality level interacted with the year fixed effects, respectively for municipality of residence and of birth. Standard errors are clustered at the municipality level.

Table E4: Estimates of Impact of the *Lei do Feminicídio* on Risk of Domestic Violence for all women born and residing in RGS - Non-migrants vs migrants

	Non-migrant				Migrant			
	Basic Set of Controls		Full Set of Controls		Basic Set of Controls		Full Set of Controls	
	Probability of DV	Probability of DV	Probability of DV	Probability of DV	Probability of DV	Probability of DV	Probability of DV	Probability of DV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Exposure-Residence	-0.442***		-0.301***		-0.0194		-0.00993	
	(0.0880)		(0.0595)		(0.0246)		(0.0150)	
Post*Exposure-Birth		-0.442***		-0.301***		-0.263***		-0.203***
		(0.0880)		(0.0595)		(0.0681)		(0.0451)
Observations	8,939,232	8,939,232	8,938,174	8,938,174	7,413,599	7,413,599	7,400,335	7,411,304
R-squared	0.002	0.002	0.002	0.002	0.003	0.003	0.003	0.003
Municipality	482	482	477	477	493	493	488	493
Pre Mean of Outcome	1.988	1.988	1.988	1.988	1.988	1.988	1.988	1.988
Range of Exposure	.24	.242	.24	.242	.24	.242	.24	.242
Effect wrt Mean	5.33	5.38	3.63	3.66	.23	3.2	.12	2.47
Black	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort/period/interaction FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality of Residence FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Controls*year	No	No	Yes	Yes				

Notes: Standard errors in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Dependent variable is the probability of DV for migrant and non-migrant women. Columns (1), (2), (5) and (6) include fixed effects for race, year-of-birth, year, and municipality, as well as the interaction of year-of-birth fixed effects with year fixed effects. Columns (3), (4), (7) and (8) add controls measured in 2010 at the municipality level (of residence for Columns (3) and (7), of birth for Columns (4) and (8)) interacted with the year fixed effects. Standard errors are clustered at the municipality level.