

CAF - WORKING PAPER #2025/12

This version: October 31, 2025

Women in Office: The Impact of Female Politicians on Gender-Based Violence Reporting

Veronica Frisancho¹ | Evi Pappa² | Camila Ramírez³ | Chiara Santantonio⁴

¹CAF - Development Bank of Latin America and the Caribbean.
vfrisancho@caf.com

²Universidad Carlos III de Madrid and CEPR. ppappa@eco.uc3m.es

³World Bank.
cramirez3@worldbank.org

⁴University of Bath.
cs2983@bath.ac.uk

Gender-based violence in the U.S. is a silent epidemic. Twenty percent of women experience rape, yet only one in three reports it. Using FBI data and a regression discontinuity design, we examine the impact of female U.S. House Representatives on reported rapes and femicides. Our findings suggest an increase in reporting, rather than higher levels of violence. Our setting and additional analysis allow us to rule out policy channels. We argue that female politicians serve as role models, influencing reporting through symbolic and social pathways. Congressional speech data support this argument: female legislators advocate more against gender-based violence, and their speeches correlate with higher reporting in their districts.

KEYWORDS

mixed-gender race, gender-based violence, crime data

Small sections of text that are less than two paragraphs may be quoted without explicit permission as long as this document is acknowledged. Findings, interpretations and conclusions expressed in this publication are the sole responsibility of its author(s) and cannot be, in any way, attributed to CAF, its Executive Directors or the countries they represent. CAF does not guarantee the accuracy of the data included in this publication and is not, in any way, responsible for any consequences resulting from its use.

CAF - DOCUMENTO DE TRABAJO #2025/12

Esta versión: Octubre 31, 2025

Mujeres en el Cargo: El Impacto de las Mujeres en Política en el Reporte de Violencia de Género

Veronica Frisancho¹ | Evi Pappa² | Camila Ramírez³ | Chiara Santantonio⁴

¹CAF - Development Bank of Latin America and the Caribbean.
vfrisancho@caf.com

²Universidad Carlos III de Madrid and CEPR. ppappa@eco.uc3m.es

³World Bank.
cramirez3@worldbank.org

⁴University of Bath.
cs2983@bath.ac.uk

La violencia de género en EE. UU. es una epidemia silenciosa. El veinte por ciento de las mujeres sufre violación, pero solo una de cada tres lo denuncia. Utilizando datos del FBI y un diseño de regresión por discontinuidad, examinamos el impacto de las Representantes femeninas de la Cámara de EE. UU. sobre las violaciones y los femicidios denunciados. Nuestros hallazgos sugieren un aumento en la denuncia, más que niveles más altos de violencia. Nuestro entorno y análisis adicionales nos permiten descartar canales políticos/normativos (policy channels). Argumentamos que las mujeres políticas sirven como modelos a seguir, influenciando la denuncia a través de vías simbólicas y sociales. Los datos de discursos del Congreso respaldan este argumento: las legisladoras abogan más en contra de la violencia de género, y sus discursos se correlacionan con un mayor nivel de denuncias en sus distritos.

KEYWORDS

carrera de género mixto, violencia de género, datos de criminalidad

Pequeñas secciones del texto, menores a dos párrafos, pueden ser citadas sin autorización explícita siempre que se cite el presente documento. Los resultados, interpretaciones y conclusiones expresados en esta publicación son de exclusiva responsabilidad de su(s) autor(es), y de ninguna manera pueden ser atribuidos a CAF, a los miembros de su Directorio Ejecutivo o a los países que ellos representan. CAF no garantiza la exactitud de los datos incluidos en esta publicación y no se hace responsable en ningún aspecto de las consecuencias que resulten de su utilización.

1 | INTRODUCTION

Gender-based violence (GBV) is a pervasive and often unspoken crisis in the United States (U.S.). Nearly 20 percent of women in the country have experienced rape during their lifetime, and one in four has endured severe physical violence from a partner (NCADV, 2014). Despite its prevalence, rape remains one of the least reported crimes, with roughly 63 percent of sexual assaults going unreported to law enforcement (NSVRC, 2015). Underreporting is not unique to the U.S.: ample evidence documents similar patterns in both developed and developing countries (Palermo et al., 2014; Agüero and Frisanch, 2022; Cerqua et al., 2024). Reporting is nonetheless vital for prevention and deterrence, as formal complaints enable protective interventions and reduce future incidents. For example, Amaral et al. (2023) show that arrests triggered by reporting reduce future domestic violence calls by as much as 51% in the ensuing year, demonstrating a significant deterrence effect. Understanding what encourages survivors to come forward is thus a critical policy priority.

This paper investigates whether women's political representation can help close the GBV reporting gap through a role model effect. We ask whether the visibility and leadership of female politicians can shift social norms, legitimize survivors' voices, and empower women to report abuse. Using a regression discontinuity (RD) design in close mixed-gender U.S. House races from 1970 to 2017 and official FBI crime data, we find that districts narrowly electing a female Representative experience a roughly 40 percent increase in reported rapes relative to those electing men. This rise does not reflect higher violence, as fatal crimes involving women remain unchanged, but instead greater empowerment and willingness to report. Because legislative authority and funding decisions occur at the federal level, while crime data vary at the district level, we interpret this increase as arising from women's visibility and influence as public role models rather than from direct policy action.¹ Although Members of Congress lack direct authority over local law enforcement, their public engagement can shape social attitudes and institutional responsiveness indirectly. We therefore examine changes in police activity following the election of a Congresswoman (Iyer et al., 2012; Miller and Segal, 2018; Amaral et al., 2021) and find no evidence of increased law enforcement effort in gender-based crime cases, if anything, the share of solved female homicides declines slightly.

Supporting the role model effect, we analyze 1.8 million Congressional speeches from 1971 to 2012 (Gentzkow et al., 2018) and find that Congresswomen are substantially more likely than men to address GBV-related issues. The frequency of such speeches correlates positively with rape reports in their districts, consistent with greater awareness and survivor empowerment rather than local policy changes. Finally, using survey data from the American National Election Studies (ANES), we show that women in districts represented by a Congresswoman express stronger support for reporting harassment and more favorable views towards the police. For men, on the other hand, attitudes toward the police remain unchanged, while some evidence points to more favorable views on reporting harassment. We find no indication of backlash or rising violence, reinforcing the interpretation that the observed effects operate through social and cultural channels rather than political polarization.

To ensure robustness, we conduct extensive sensitivity tests. Our results remain consistent across alternative outcome definitions, samples, and bandwidths. We also rule out confounding by partisanship: although women are more likely to be Democrats, electing a

¹The role model channel is vividly illustrated by public testimonies. For instance, during the legislative debate over Ohio's fetal heartbeat bill on April 10, 2019, State Representative Lisa Sobecki shared her own experience of sexual assault. Following her speech, a man later approached her to say that his wife, deeply moved by Sobecki's words, had disclosed her own rape for the first time in forty-one years (Times, 2019).

Democrat over a Republican does not significantly affect reported rapes.

Evidence from developing countries shows that electing female leaders often improves outcomes for women. Female politicians can challenge gender-biased norms, inspire girls' aspirations, and promote redistributive and child-oriented policies (Beaman et al., 2009, 2012; Priyanka, 2020; Brollo and Troiano, 2016; Bruce et al., 2022; Clots-Figueras, 2012; Bhalotra et al., 2022; Bhalotra and Clots-Figueras, 2014). They also prioritize public goods that benefit women, advance women-friendly laws, and actively address gender-specific issues (Chattopadhyay and Duflo, 2004; Clots-Figueras, 2011; Gerrity et al., 2007). In contrast, studies from advanced economies provide mixed evidence. Hessami and da Fonseca (2020) and Carozzi and Gago (2023) find limited effects of female representation on spending patterns, while Bagues and Campa (2021) show that gender quotas alone do not overcome institutional barriers to women's influence. Yet more recent work demonstrates substantive impacts: Lippmann (2022) finds that, in France, legislators are more active on women's issues, while Brogaard et al. (2024) shows that female politicians increase the share of U.S. government procurement contracts awarded to women-owned firms.

Within this broader literature, our paper contributes to understanding how female representation affects GBV. Prior research has largely focused on specific policy interventions, such as cash transfers, alcohol restrictions, enhanced property rights, or the creation of women's police stations (Hidrobo et al., 2016; Luca et al., 2015; Amaral, 2017; Amaral et al., 2021), while the political dimension has received less attention. A small but growing set of studies explores this link. In Indonesia, Kuipers (2020) finds a significant negative relationship between women's presence in local councils and the share of women who approve of a husband assaulting his wife, showing that female political representation can lower acceptance of GBV. In India, Anukriti et al. (2024) show that exposure to female legislators increase intimate partner violence. The authors attribute this link to expanded access to modern contraception, which exacerbates spousal conflict. In Brazil, Delaporte and Pino (2022) and Bochenkova et al. (2023) find that electing female mayors reduces domestic violence by nearly half. These previous studies analyze local executives with direct control over policy implementation. In contrast, we study U.S. Representatives who legislate federally but have no authority over local policing or welfare programs—allowing us to isolate role model effects from policy effects.

Our work also builds on Iyer et al. (2012), who show that female representation in local governments in India has led to increased recorded crimes against women. They leverage data on fatal crimes and rape records and show that this increase is primarily due to greater reporting rather than greater incidence of GBV. However, the use of the staggered enactment of quotas may conflate the impact of increased representation with the broader effects of policy shifts and changing societal attitudes (Beaman et al., 2009; Clots-Figueras, 2012). For example, quotas may be introduced in a policy environment where there is already a big push for gender equality, which may introduce contextual confounders. Quotas can also reflect the consequences of the policy itself – affecting the characteristics of the women who choose to compete under quotas vs open elections— or the nature of political competition (Chattopadhyay and Duflo, 2004). Our research design has an advantage over the use of quotas, identifying the causal impact of women winning office in a politically competitive environment. Our results thus speak to contexts without formal political interventions.

The U.S. context also strengthens the interpretation of our results. Because federal laws apply nationally but outcomes are observed at the district level, any differential reporting across districts following the election of a woman cannot stem from new federal legislation. This institutional “mismatch” provides a rare opportunity to disentangle symbolic and cultural influences from formal policy mechanisms. Finally, we go beyond previous studies by linking Congressional speech records and survey data to GBV outcomes, offering novel

evidence on the communicative and attitudinal channels through which female leaders may empower victims and reshape social norms.

The rest of the paper is organized as follows: Section 2 describes the data, Section 3 outlines the empirical methodology, and Section 4 presents the main results and robustness checks. Section 5 explores potential mechanisms, and Section 6 concludes.

2 | CONTEXT AND DATA

2.1 | The U.S. House of Representatives

The U.S. House of Representatives is one of the two chambers of the U.S. Congress alongside the U.S. Senate. It is composed of 435 voting members, known as Representatives (or Congressmen and Congresswomen), each elected to serve a two-year term representing one congressional district.² All seats are contested every two years, and members are elected directly by citizens. Representatives carry out a broad range of work, including introducing bills, offering amendments, voting on legislation, but also serving on committees, and engaging with constituents and other groups.

The House of Representatives shares its legislative powers with the Senate: any member may introduce a bill, but legislation must be approved by both chambers – and signed by the President – to become law. It also holds the so-called “power of the purse”, meaning it originates and approves spending and taxation measures.³

2.2 | Elections

Data on the U.S. House elections from 1970 to 2016 was sourced from CQ Press, and supplemented with information on the sex of all candidates from the Republican and the Democratic party from [PoliticalParity \(2015\)](#), the Center for American Women and Politics (CAWP), and [Pettigrew et al. \(2014\)](#).⁴

As mentioned in Subsection 2.1, elections for the U.S. House of Representatives are held biennially across 435 Congressional districts, with one Representative being appointed per district. This results in a total of 10,440 races during our sample period. Mixed-gender races (female vs. male) account for 18.2% of this sample (1,895 races) and are more often won by men (1,081 male vs. 814 female wins, see Table A1). These races also appear to exhibit a partisan imbalance, with female winners more likely to come from the Democratic party and male winners more often representing Republicans. A geographical analysis reveals that mixed-gender races are more frequent in coastal states (see Panel A in Figure A1). However, female candidates win mixed-gender elections across the country (see Panel B in Figure A1). We match election data with district-level characteristics (e.g., demographics, income, education, and inequality) for 1972–2016 from [Foster-Molina \(2017\)](#). Additional details on the election data are available in [Appendix A.1](#).

²Congressional districts are smaller geographical areas than states. Each state is assigned a number of districts based on its population, with every state guaranteed at least one. States are responsible for drawing the boundaries of their own districts but cannot determine the number allocated to them.

³Article I, Section 7, of the Constitution provides that all bills for raising revenue shall originate in the House of Representatives but that the Senate may propose, or concur with, amendments. By tradition, general appropriation bills also originate in the House of Representatives.” (see [How Our Laws Are Made](#))

⁴These datasets do not include the sex of candidates from other parties. Because of this restriction and because of the strong bipolarity of the American party system, we focus on the sex of Republican (R) and Democratic (D) candidates only. On average, in all elections in which the first and second places correspond to R and/or D, the share of votes for these two candidates is above 98 percent.

2.3 | Crimes

Crime data come from the FBI's Uniform Crime Report (UCR), specifically the Offenses Known and Clearances by Arrest (OKCA) files (1970–2017) and Supplementary Homicide Report (SHR) files (1976–2017) (Kaplan, 2019b,a).

The OKCA files include monthly crime data by individual law enforcement agency, capturing victim counts and clearances. These records do not include detailed information on the victim or the perpetrator, such as their relationship or their sex. We retrieve the number of female victims raped by men (forcible and attempted) and the number of rape offenses cleared between 1970 and 2012. Throughout this period, such crime was explicitly defined as “carnal knowledge of a female forcibly against her will” by a male (FBI, 2013).⁵ We also retrieve the number of offenses and offenses cleared for other felonies that are not specifically gender-based, such as assault, robbery, and vehicle theft.⁶

The SHR provides incident-level data on fatal crimes. From these records, we obtain the number of female victims of murder and non-negligent manslaughter (i.e., women killed). We construct two additional outcome measures by restricting this number to the number of female victims when all offenders are male, and the number of female victims with a relationship to the offender (e.g., wife, girlfriend, or daughter), i.e., number of victims of intimate femicide. In addition, we retrieve data on other fatal crimes, such as the number of men killed and the total number of murder victims. Crime statistics are aggregated at the year–district level using geographical matching, based on agency coordinates and district maps from Lewis et al. (2013) and the Census Bureau.

As noted in Appendix A.2, the records available provide data on rape cases using the reporting date while homicides cases are recorded using the occurrence date. While these constraints are imposed by the data, we deem it appropriate given our focus on reporting behavior. If Congresswomen become a role-model that fosters reporting, some rapes from the past may be reported after the election of a woman. Moreover, we argue that comparing recorded rapes vs occurrences of intimate femicides is also helpful to disentangle actual increases in GBV from changes in reporting behavior.

To address data quality challenges (e.g., missing data, frequency of reporting, representativeness), we focus on local agencies (e.g., city police) reporting data consistently for all 12 months of the year.⁷ In our analysis, crimes are normalized per 100,000 district residents. We also examine the sensitivity of our results when we normalize rape and fatal crimes involving women by the size of the female (voting) population in the district.

Crime data are aggregated at the term level for each winning candidate. For instance, total crimes committed between 2011 and 2012 are linked to a 2010 election. We also aggregate crimes at the annual level within each two-year term for additional robustness checks. Additional details on the crime data are available in Appendix A.2 and Appendix B.

⁵This definition was expanded in 2013, after which the term forcible was dropped, and victims and perpetrators of both genders were considered. Since the OKCA files do not include information about the sex of the victim or the perpetrator, we cannot identify these counts for more recent years. See the FBI website at ucr.fbi.gov for more details.

⁶For crimes against the person (e.g., homicide, rape), one offense is counted for each victim. For crimes against property (e.g., robbery), one offense is counted for each distinct operation, with the exception of motor vehicle theft for which one offense is counted for each stolen vehicle (FBI, 2013).

⁷Focusing on local agencies (while excluding, for example, those with state-wide jurisdiction) ensures that crimes are more accurately counted within the boundaries of a given district.

2.4 | Congressional Speeches

We gather data on Congressional speeches between 1971 and 2012 from the Congressional Records dataset compiled by [Gentzkow et al. \(2018\)](#). This dataset contains processed text from the bound and daily editions of the U.S. Congressional Records. It includes transcripts of individual floor interventions in each chamber of Congress. It also provides speaker information such as name, district, and sex.⁸

To identify speeches related to GBV, we focus on those that contain predefined key terms: rape, femicide, domestic violence, intimate partner violence, violence against women, gender-based violence, sexual assault, sexual violence, sexual harassment, and battered women.⁹ However, a speech may mention a key term only circumstantially: for example, a Representative may mention “rape” in the context of general crime trends, rather than as part of a discourse on GBV. To address this, we apply a second filter to these pre-selected speeches. We rely on latent Dirichlet allocation (LDA), a topic modeling technique, to determine which of the speeches are most likely centered on GBV. LDA is commonly used in natural language processing to identify latent topics in textual data. We applied this analysis to speeches containing key terms and not to the full body of speeches because only a small proportion of speeches are associated to GBV (less than 1% across the different definitions used) and, therefore, GBV does not come up as a topic when the analysis is applied to all the speeches in the data without a previous filter. We use LDA to extract topics in the collection of speeches, identify those that are GBV-related, and estimate the probability that a given speech belongs to a GBV-related topic. Finally, we filter out speeches for which this probability, denoted as Q_i , falls below the t -th percentile of the overall distribution of Q . In the results discussed in Section 5, we have set $t = 75$, so that we keep the 25% of speeches most likely related to GBV. To select this threshold, we selected a random sample of 50 speeches among those containing key words, read them entirely, and manually classified them as “relevant” or “not relevant”. In this random sample, the proportion of relevant speeches was $\sim 25\%$. Moreover, when evaluating the classification accuracy using the manually coded sample, we found that applying the 75th percentile threshold yielded a more accurate identification of GBV-related speeches than alternative cutoff points. With this procedure, we filter out all speeches that: a) do not contain a key term or b) contain key terms but are among the least likely to be GBV-related. The results discussed in Section 5 are robust to other choices of the threshold. We present a robustness test using the 50th percentile in Table D10 in Appendix D.

This approach enables us to better comprehend the content of the speeches and refine our dataset by excluding those texts that incidentally mention key terms but do not focus on GBV. Using this filtered set, we identify districts in which a Representative made at least one GBV-related intervention in a given year. In addition, we calculate the proportion of GBV-related speeches delivered by each Representative annually.

3 | EMPIRICAL STRATEGY: REGRESSION DISCONTINUITY DESIGN

We estimate the causal effect of female representation using an RD design in a sample of close mixed-gender races. Our identification strategy relies on the assumption that, in close races between a man and a woman, it is mostly random factors that tilt the scale in favor

⁸We retrieve the speech data from the bound edition (Congresses 92nd-96th) and from the daily edition (Congresses 97th-112th). We use district and year identifiers to match the speeches with the crime data.

⁹We obtained this list iteratively, by selecting an original list of key terms, categorizing the speeches that contained these terms in topics, and expanding the list with keywords and bigrams that were among the most frequent for speeches related to GBV.

of one of the candidates. Therefore, a district in which a man wins against a woman by a narrow margin of victory becomes a good counterfactual for a district in which the opposite occurs. In our setting, close mixed-gender races are defined as those in which the top two places are occupied by a male and a female, irrespective of the total number of candidates, and where the election results are tightly contested.

We first compute the female margin of votes, i.e., the difference between the votes received by the female and the male candidates as a share of the total votes, for each race taking place in district d , state s , and year t :

$$MV_{d,s,t} = \left(\frac{F_{d,s,t} - M_{d,s,t}}{Votes_{d,s,t}} \right) \times 100$$

We define our assignment to treatment as $W_{d,s,t} = 1[MV_{d,s,t} > 0]$, an indicator variable that takes a value of one if the winner of the (mixed-gender) race is female. The effect of having a woman in power can then be estimated with the local polynomial of the p -th order:

$$y_{d,s,t} = \gamma_s + \lambda_t + \sum_{k=0}^p \eta_k MV_{d,s,t}^k + W_{d,s,t} \sum_{k=0}^p \beta_k MV_{d,s,t}^k + X'_{d,s,t} \alpha + \varepsilon_{d,s,t} \quad (1)$$

in the sample of races in which the female margin of votes lies within the interval $[-h, +h]$. In equation (1), $y_{d,s,t}$ is our measure of gender-based crimes in district d , in state s , in year t , while γ_s and λ_t denote state and year fixed effects. $X_{d,s,t}$ is a vector of pre-determined district-covariates. The vector η captures the relationship between the running variable (margin of votes) and the outcome, absent treatment. In other words, it determines the slope of the relationship between y and MV to the left of the cutoff. Finally, our vector of interest is β_k . In particular, the main parameter of interest is β_0 , which identifies the effect of electing a female Representative at the threshold, i.e., a *local effect*.

Following the literature (Imbens and Lemieux, 2008; Broockman, 2014; Brollo and Troiano, 2016; Cattaneo et al., 2020), our main and preferred specification is a non-parametric local linear regression. Specifically, we estimate a polynomial of order 1 where h is chosen according to the procedure developed by Calonico et al. (2014b) and Calonico et al. (2014a), using MSE-optimal bandwidths. Since one district can contribute multiple mixed-gender elections over the years, standard errors are clustered at the district level.

In this RD design, identification rests on two main assumptions. First, around the threshold $MV = 0$, randomness determines the election result. Second, candidates are not able to control the probability of falling on either side of the threshold; that is, they are not able to manipulate the outcome of an election. We refer the reader to Appendix C for a discussion of the validity of our approach. In particular, we show that pre-determined characteristics are balanced (see Table C1), and the McCrary test of discontinuity around the threshold indicates that there is no evidence of manipulation (see Figure C2).

Recent work by Marshall (2022) shows that, unless additional strong assumptions are invoked, RD estimates may be asymptotically biased. In our setting, isolating the effect of gender would require that the gender of the candidate does not affect vote shares *or* that other predetermined potential confounders (e.g., competence, or social preferences) do not affect the outcome of interest (i.e., rapes). It is hard to argue that gender does not affect the margins of victory. However, we claim that voters in the U.S. are not likely to select Congress Representatives to reduce GBV, which implies that no compensating differential affects the outcome of interest. Indeed, the Pew Research Center instrument to identify top voting issues in the U.S. Congressional elections does not include sexual abuse or gender issues in general as an option. Data from ANES (2019) also seem to support this claim.

When asked to identify the most important problem facing the U.S., overall, only around 13% of the respondents surveyed between 1970 and 2000 mention “public order”, a category that includes crime, drugs, women’s rights, and gun control, among other issues.

Still, we acknowledge the possibility that this assumption (i.e., compensating differentials induced by variation in gender do not affect the outcome of interest) may fail. Therefore, we interpret our estimates of β_0 as a compound treatment effect that incorporates the effects of gender as well as the effects of all compensating differentials induced or altered by conditioning on close races (Marshall, 2022). We thus describe β_0 as the causal effect of electing a female Representative as opposed to the causal effect of gender. For instance, one of the main differentials between female and male candidates in close races is their party affiliation. In this case, we conduct a robustness check with a similar RD design to evaluate the effect of party affiliation on rapes (see Section 4.1, and Table 2). Reassuringly, this placebo test yields null impacts, which suggests that the main estimate of the impact of electing a female Representative on rapes is not driven by party affiliation.

Another potential challenge is highlighted in Eggers et al. (2015), who document the presence of an incumbent advantage in close election for the U.S. House of Representatives. This advantage raises concerns about the validity of the RD design since winners and losers in close races may not be comparable. To address this issue, we follow the approach proposed in Eggers et al. (2015) and test for the presence of an incumbency advantage in our sample of mixed-gender races. We define a candidate as an incumbent if they are running for the party that holds the seat, i.e., a party-based definition of incumbency. We then test for a discontinuity in the probability of being an incumbent at the vote margin threshold (see Table C2). We find no evidence of such an advantage. If anything, incumbents are less likely to win close elections.¹⁰

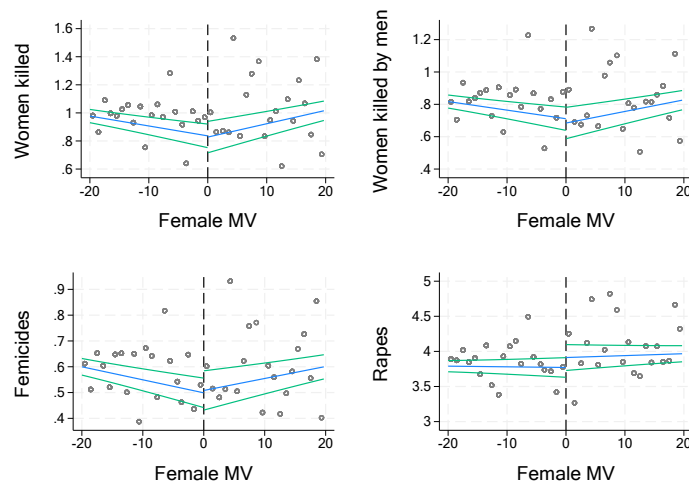
4 | RESULTS

Figure 1 presents a graphical analysis of the impact of electing a female Representative on fatal crimes against women and rapes during their time in office. The plot shows the (log-transformed) values, averaged within bins of 1% of the female vote margin, against the margin of victory (MV). The fitted values from a first-order polynomial in MV are also included. The graph reveals that electing a female Representative appears to be associated with an increase in reported rapes, but does not seem to have an effect on fatal crimes.

This initial finding is further supported by the results presented in Table 1, where we estimate equation (1) with $p = 1$. The model includes time and state fixed effects, as well as district-level covariates: macro-area dummies, a Democratic Party winner indicator, the percentages of Black and Hispanic populations, the unemployment rate, and the proportion of law enforcement agencies reporting data to the FBI.¹¹ Column (1) examines the impact of female representation on recorded rapes, while Columns (2) through (4) explore the impact on fatal crimes against women: women killed, women killed by men, and intimate femicides, respectively. All outcome measures are expressed as logs of the number of victims per 100,000 residents. We find a significant increase in recorded rapes in districts electing

¹⁰Eggers et al. (2015) find no compelling explanation for the mechanisms driving the documented incumbent advantage. They conclude that “the unusual success of incumbents in very close House elections might result from chance”, and consequently “evidence of incumbent dominance in close U.S. House elections does not pose a general threat to the validity of RD designs in electoral settings”.

¹¹Macro-area dummies include West, Northeast, and Midwest (South omitted category). Reporting agency shares vary by outcome: for rapes, the percentage of agencies providing non-missing OKCA data is used; for fatal crimes involving women, the share of agencies with non-missing SHR data is considered. Shares are averaged over the first and second years of the term.



Note: Measures on the y-axis are expressed as the (log) number of victims per 100,000 residents. *Female MV* is the margin of votes of the female candidate in district d in year t . $MV > 0$ when the winning candidate is female and $MV < 0$ when the winning candidate is male. Scatter points represent values averaged within bins of 1% of MV. The blue line represents fitted values from a first-order polynomial in the MV, fitted separately on each side of the threshold, while the green lines are the 95% confidence interval of that polynomial. Values refer to the whole term.

FIGURE 1 Gender-based violence and margin of victory

a Congresswoman compared to those in which a man is elected — close to a 40% change ($\approx (e^{.335} - 1) * 100$). This increase is concentrated in the second year of the term, as shown in Panels A and B of Table D1. Importantly, we do not find a wider change in the level of crime in the district (see Table D8), suggesting that there is no overall deterioration in law and order conditions and underscoring that the effect is only found for gender-based crimes.

An increase in reported rapes may reflect either higher reporting rates or a rise in incidents. The “male backlash effect” suggests increased GBV following women’s empowerment (e.g., (Tur-Prats, 2021; Zhang and Breunig, 2021; Guarnieri and Rainer, 2018)). In our case, the election of a female Representative might signal an increase in women’s political power, which could be seen as a threat to the status quo wherein positions of power are predominantly held by men. We argue that this backlash effect is unlikely to be driving our results. If this effect were present, we would expect increased fatal crimes against women as well. However, Table 1 shows no significant change in such crimes, including intimate femicides. Since fatal crimes are less prone to reporting bias, this finding supports the idea that the observed increase in rapes reflects higher reporting rather than a rise in violence. Survey evidence presented in Subsection 5.4, showing no significant worsening in men’s views, further counters the backlash hypothesis.

Our estimates align with the findings of Iyer et al. (2012), who report a 46% rise in documented crimes against women following the introduction of female quotas in Indian local councils, including a 23% increase in reported rapes. They also find no effect on fatal crimes against women. The magnitude of the impact of female political leaders on rapes in India and in the U.S. can be explained both by the context as well as the reliance on quotas in the case of Iyer et al. (2012). In the Indian setting, gender norms may be more entrenched and harder to shift, which could also help explain why the effect appears larger in our context. As discussed in the Introduction, quota-systems may alter the nature

of political competition, by reserving seats for women and excluding men from running, somewhat restricting voters' choice. Such a system may thus generate a backlash effect from males (Beaman et al., 2009) which could mitigate the positive effect of female political representation on reporting.

TABLE 1 The Effect of Female Representation on GBV

	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.335	.082	.084	.04
SE	.187	.135	.105	.09
P-value	.035	.436	.308	.546
h	18.261	14.693	14.504	15.091
N	303	275	266	267
Average Outcome	72.385	2.776	1.885	1.104
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as logs of number of victims per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

4.1 | Robustness

To validate our main findings, we perform several robustness checks. Results are robust to normalizing our measures of GBV by the size of the female (voting) population, rather than by total population (Table D2). The results are also robust to defining the outcome variables as the number of victims divided by total population in levels, instead of considering a logarithmic transformation (Table D3).

To assess the sensitivity of our findings to the bandwidth selection, Table D4 replicates the analysis using different bandwidths.¹² The results remain consistent across these specifications. Similarly, the results are robust to an alternative specification using a second-order polynomial, as shown in Table D5.

Another important robustness check concerns the sample of agencies included in the analysis. While our baseline results rely on local law enforcement agencies with complete 12-month reporting, we expand the dataset to include non-local agencies (e.g., state police) with full reporting (Table D6) and local agencies with incomplete monthly records (Table D7; see Appendix B for details on data quality and representativeness). Across all specifications, our main findings remain qualitatively unchanged.

Our results are also robust to the exclusion of any single district or year from the sample. Figure D2 in Appendix D shows the distribution of coefficient estimates obtained by sequentially omitting one district or one year. The coefficients remain tightly clustered around the full-sample estimate, confirming that our findings are not driven by any specific observation or period.

As outlined in Section 3, focusing on close elections may capture not only the effect of gender but also compensating differentials, such as other candidate traits influencing

¹²To facilitate comparison, we use three common bandwidths: the average of those optimally selected in our main analysis across all measures (16.5), the average of those optimized for fatal crime measures (15), and the bandwidth optimized specifically for rapes (18).

close races. For instance, when trying to isolate the impact of gender in close elections where there is voter bias against women, competence or ability could act as a compensating differential among female candidates. Previous research suggests that women in politics are often more educated, effective, and skilled at securing funding for their constituencies (Baltrunaite et al., 2014; Black and Erickson, 2003; Bauer, 2020; Anzia and Berry, 2011). These patterns may reflect barriers such as voter bias or party strategies that lead only highly qualified women to run in or win elections (Anzia and Berry, 2011; Le Barbanchon and Sauvagnat, 2021; Esteve-Volart and Bagues, 2012; Cella and Manzoni, 2019). Anticipating discrimination, women may also work harder once elected. Although data on candidate characteristics are limited, we observe that women in close mixed-gender races are more often Democrats (68%) compared to men (32%). To test if the observed effects are due to electing a Democrat rather than electing a woman, we conduct a placebo test using an RD design on “mixed-party races” between Democrats and Republicans. Results in Table 2 show no significant effects from electing a Democrat, confirming that our findings are not driven by party affiliation. These results suggest that “gender” is more important than “ideology” for raising awareness and empowering survivors to report.

TABLE 2 The Effect of Party Affiliation on GBV - Democratic vs Republican Party

	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	-.042	-.013	0	.012
SE	.089	.055	.05	.04
P-value	.593	.67	.908	.869
h	16.971	22.186	21.1	23.515
N	1391	1747	1639	1769
Average Outcome	67.172	2.757	1.906	1.13
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. The RD design looks at close races between a Democratic and a Republican candidate. Dependent variables are expressed as logs of number of victims per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for gender of the winning candidate, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-party races.

5 | CHANNELS AND MECHANISMS

5.1 | Policy Effect

A female Representative may influence gender-based crime levels and reporting through legislative action. Given the differences in policy preferences between men and women, female legislators may be more likely to propose and pass laws addressing GBV. They can also influence police efforts to address gender-based crimes, either directly (e.g., by applying media pressure) or indirectly (e.g., by challenging gender-biased stereotypes held by local law enforcement leaders). Anzia and Berry (2011) find that Congresswomen secure more federal discretionary program funding, however these allocations are not exclusively tied to “women’s issues” such as education or related programs. In the Brazilian context, the results from Delaporte and Pino (2022) and Bochenkova et al. (2023) suggest that the effect of female representation operates indeed through such policy-channels.

Our setting greatly differs from the Brazilian context and the channels operating in our

setting are not necessarily applicable to the Brazilian one (see, e.g., [Anukriti et al. \(2024\)](#)). More importantly, our institutional setting allows us to distinguish policy effects from role model effects. U.S. Representatives legislate federally, whereas GBV outcomes are measured locally at the district level. This separation between legislative authority and electoral constituency limits their ability to implement district-specific policies. Even if female Representatives support national initiatives to combat GBV, such measures apply uniformly across all districts. Consequently, any observed local variation in reported violence following a woman's election is more likely to stem from her symbolic influence and visibility rather than direct policy changes.¹³ This contrasts with the contexts studied by [Delaporte and Pino \(2022\)](#) and [Bochenkova et al. \(2023\)](#), where local or regional leaders possess executive authority and can directly affect service delivery and enforcement.

Crime-related offices in the U.S. (e.g., judges or sheriffs) are at times allocated via elections. Therefore, a potential concern is that our results could be confounded by such elections. If the same factors driving the election of a woman to the U.S. House drive the election of, e.g., a female judge, the observed effect on GBV and reporting could be due to a combination of more women in the Congress and in the judicial system.

In [Appendix E](#) we examine this hypothesis, focusing on the institutional features of four offices: state judges, U.S. attorneys, district attorneys (DAs), and sheriffs. We analyze how these figures are selected (e.g., elected vs appointed), the length of their terms, and the jurisdiction they oversee. We argue that systematic confounding is unlikely. First, these offices typically have longer terms than U.S. Representatives. Second, they also have broader geographical jurisdictions. Finally, for sheriffs and district attorneys in particular, election are typically non-competitive and dominated by men.

5.2 | Police Effort

Police are often the first responders in reporting and handling violent crimes, including GBV. Congresswomen may influence police performance both directly, through policies impacting funding or operational scope, and indirectly, by shaping public discourse. For instance, they may publicly criticize or commend police efforts, thereby affecting law enforcement's priorities and responsiveness.

While formal and informal pressure is likely to be exerted at the national level, elected Congresswomen can try to influence local police offices in their own districts. To explore whether these dynamics are relevant in our context, [Table 3](#) examines police effort to address crimes targeting women. To proxy effort, we consider the ratio between clearances and crimes reported, that is, a measure of *relative effort*. This allows us to evaluate how effort changes, while taking into account changes in caseloads. Column (1) examines the ratio of cleared rapes to total reported rapes.¹⁴ Using SHR data, we also analyze solved murders as an indicator of effort. Column (2) assesses the effect of electing a female Representative on the proportion of solved female homicides relative to total female homicides. Column

¹³Suggestive heterogeneity analysis also supports a role model effect. We argue that the visibility of a woman in office should have greater effects where women's presence in politics has been historically limited. We identify whether a *state* had ever elected a woman to the U.S. House before 1970, and split our sample of districts accordingly. Consistent with this hypothesis, the coefficient for rape is larger in districts located in states that had *not* previously elected a woman (results available upon request).

¹⁴Clearances include crimes cleared by arrest and those cleared by exceptional means. Arrest clearances require three criteria: at least one person is (i) arrested, (ii) charged, and (iii) turned over for prosecution. Exceptional clearances occur when external factors prevent arrests, such as the offender's death (see FBI guidelines at ucr.fbi.gov). For crimes against individuals, each victim counts as a separate offense ([FBI, 2013](#)). A murder is considered solved if the offender's gender is known, as this information is available only when the perpetrator is apprehended ([Fox, 2004](#)).

TABLE 3 Police Effort: Clearances and Solved Murders

	Rape Cleared/Total	W killed Solved/Total	M killed Solved/Total	Murders Solved/Total
	(1)	(2)	(3)	(4)
Coefficient	.013	-.044	.016	-.002
SE	.019	.025	.026	.02
P-value	.631	.054	.357	.939
h	17.16	16.583	13.571	16.933
N	285	314	251	320
Average Outcome	.467	.818	.755	.724
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Values refer to the overall term and are expressed in logs. MSE-optimal bandwidths and a triangular kernel are used. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. Robust p-values are reported. *W killed, solved* is computed by counting female victims of murder whose offenders were (all) known. We consider an offender as such if the sex was *known* (see Fox (2004)). *M killed, solved* counts male victims of solved murders. *Murders, solved* counts all murders solved, both of women and men. *Average Outcome* is the mean of the outcome variables (shares) computed across all districts with mixed-gender races.

(3) conducts a similar analysis for male homicides, and Column (4) considers all reported murders. Once again, all outcomes are expressed in logs.¹⁵

The results in Table 3 suggest that police do not change their level of effort in addressing rapes. If anything, police become *less* likely to clear the homicide of a woman after a female Representative is elected. These findings imply that police forces do not make extra effort to solve gender-related crimes after the election of a female politician in the district.

We also consider the hypothesis that electing a woman could lead to increased female representation in the police force, thereby encouraging more women to report crimes (Amaral, 2017; Miller and Segal, 2018). However, our analysis finds no significant changes in police force composition, such as the number or share of female officers or the gender ratio, following the election of a Congresswoman, as suggested by Tables D12 and D13 – likely due to the lack of direct influence of House Representatives on the police and the longer time frame required for shifts in recruitment and police demographics to materialize.

5.3 | Inspiring Words

A politician's voice is a powerful tool, strategically used to spotlight key issues. Women in politics are often vocal about topics such as GBV. Research by the Pew Research Center shows that women in the U.S. Congress are more likely than men to address sexual misconduct on social media, and they do so more frequently.¹⁶ Similarly, studies have found that female

¹⁵To make our results directly comparable to Iyer et al. (2012), Table D9 in Appendix D repeats this analysis with the (log) number of cleared cases normalized by population. The results show a significant increase in clearances for rape, which is consistent with the findings in Iyer et al. (2012). However, it is important to note how the two measures capture different dimensions of police effort. Normalizing by population allows us to capture changes in the absolute levels of police response, but does not adjust for changes in caseload. If reported cases rises, the number of clearances might also increase, even if the police are merely “keeping pace” with the increased workload, rather than exerting more effort per case. In contrast, looking at shares allows us to assess whether the proportion of solved cases improves, despite the larger number of cases. We regard this as a more direct indicator of police effort, and therefore our preferred measure. This difference in measurement offers a complementary but distinct perspective on police behavior following the election of female leaders.

¹⁶See this [link](#).

Senators speak more often than their male counterparts on issues such as crimes against women and women's health (Osborn and Mendez, 2010).

Discussions on GBV go beyond advocacy: they play a transformative role in reshaping societal narratives, dismantling stigma, and empowering victims to speak up. A systematic review on public stigma toward women victims of GBV highlights that effective strategies to combat stigma frequently center on ending social silence and making violence visible by increasing dialogue, which helps shift the perception of violence from a private to a public matter (Murvartian et al., 2023). Crucially, the visibility of stories of overcoming violence is recommended, as survivors act as role models who empower other women to break their silence. Jaffe et al. (2021) offers direct evidence that such broad public discourse influences personal narrative. The authors found that survivors of unwanted sexual experiences were increasingly likely to recognize and label their past experiences as "sexual assault" with more time post #MeToo, demonstrating that the social context reshapes personal understanding and facilitates victim empowerment and acknowledgment.

Congressional speeches, in particular, can directly inspire victims to report crimes. Notably, women in Congress have courageously shared personal stories of abuse to break the silence and foster solidarity.¹⁷ These powerful testimonies can have broader societal impacts, as seen in Ohio following Representative Sobecki's revelation (Times, 2019). Moreover, the fact that a female representative addresses GBV in Congress can be a signal for her introducing this topic in the political agenda more generally, and discussing it in other instances when it can reach her constituents as well. The act of speaking on an issue such as GBV can raise awareness and shape public discourse, even if it does not translate directly into new laws.

To examine gender differences in addressing GBV, we analyzed Congressional speeches from 1971 to 2012 (Gentzkow et al., 2018), as detailed in Subsection 2.4. Figure D1 plots the share of districts whose House Representative made at least one GBV-related speech in a given year by the sex of the politician. The trends show that female-represented districts are more likely to have a leader speak about GBV. Table D11 in Appendix D reinforces this claim: using "female winner" as a treatment and the proportion of GBV-related speeches in Congress as the outcome, we find that districts in which women win have their Representatives speaking up more about GBV.

Table 4 examines the relationship between these speeches and reported rapes by regressing recorded rapes in year t and district d on the proportion of GBV-related speeches by the district's Representative in the previous year. This lagged specification helps mitigate concerns related to reverse causality, and allows for a lagged effect as is suggested by our main analysis. Both measures are expressed in logs. The regression includes a dummy for whether the Representative giving the speech was a woman, and an interaction with the proportion of GBV-related speeches. We also add the same controls and fixed effects included in our baseline regression, and cluster standard errors by district. The estimate of Column (1) reveals a positive association between speeches by female Representatives and reported rapes, with a 1% increase in GBV-related speeches linked to a $\sim 4.7\%$ rise in reported rapes when the Representative is a woman. The effect for districts with men, on the other hand, is negative and insignificant. This analysis provides suggestive evidence that having a female Representative who addresses GBV in her speeches positively impacts the reporting of rapes, while male representatives do not have this effect, even when addressing GBV.¹⁸

¹⁷For example, Rep. Barbara Lee and Rep. Gwen Moore shared their experiences with domestic violence and sexual assault (see YouTube references, Lee (2012) and Moore (2012)).

¹⁸We investigate whether the effect differs when the speech is given by a Democratic or Republican Congresswoman by replicating the regressions presented in Table 4 in the subset of Democratic winners only and

TABLE 4 Speeches on GBV and Recorded Rapes

	Lagged model	Lagged model (excluding bill-related speeches)
	(1)	(2)
Proportion of GBV speeches (lag)	−0.189 (1.592)	−1.729 (2.389)
Female representative (lag) * Proportion of GBV speeches (lag)	4.848* (2.821)	6.911* (3.689)
Female representative (lag)	0.084 (0.065)	0.092 (0.065)
Average Outcome	33.495	33.495
Num. Obs.	13521	13521
Time FE	Yes	Yes
State FE	Yes	Yes
Controls	Yes	Yes

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$ Dependent variables are expressed as the logs of the number of victims of rapes per 100,000 residents. Proportion of relevant speeches is the log of the proportion of speeches in a given district and year that addressed GBV/sexual violence. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for gender of the winning candidate, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA). Standard errors are clustered at the district level. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

One potential concern is that the impact of speeches on rapes might also reflect a policy effect, e.g., a female Representative introducing a bill on GBV and subsequently discussing it. We examined the number of House bills addressing GBV and find that only a very small number of such bills were introduced and became law during our period of analysis and only 5 were sponsored by a Congresswoman. Indeed, [Volden et al. \(2013\)](#) argue that bills have a very low passing rate, particularly when focusing on women issues: only 4 percent of all bills become law, and this rate drops to 2 percent for women's issues and to only 1 percent for women's issue bills sponsored by women themselves.

Still, Column (2) in Table 4 tries to address this concern. We present the same model estimated in Column (1), but using a subset of the data that excludes all speeches given during a period in which "GBV-related bills" were being discussed. We considered "GBV-related bills" as those focused on combating GBV or including dispositions affecting GBV related policies. To identify GBV-related speeches, we used the website congress.gov. First, we searched all bills approved in the U.S. House of Representatives that became law and had "crimes against women" as at least one of the subtopics during the period of interest. Second, and because the classification by subtopics has not been consistent throughout the whole period of study, we conducted a second search to identify those bills that contained GBV-related key terms in their title. Third, we read the titles and summary of all the bills, and kept only those aimed at combating GBV or containing provisions that affect GBV-related policies (excluding, for example, bills focused on foreign policies). Our main parameter of interest is still positive and significant, and the point estimate is larger than in the model for the full sample. Our results are overall robust to an alternative definition of relevant speeches, as discussed in Subsection 2.4 and presented in Table D10.

Another key question is whether congressional speeches actually reach and resonate with

Republican winners only, respectively. Although the estimates lose precision given the smaller sample, the main parameter of interest (the interaction of female representative and proportion of relevant speeches) is positive and very similar between the two subsamples, confirming that the effect of female representatives addressing GBV on reporting is similar for Republican and Democratic Congresswomen. Results available upon request.

the population. In this respect, we note the presence of C-SPAN, a TV-network dedicated to broadcasting legislative activities in the House and the Senate, including roll-call and debates. Since 1979 – when the company launched its first channel dedicated exclusively to the U.S. House – C-SPAN has provided direct and unfiltered access to congressional speeches. Most importantly, C-SPAN audience is non-negligible: [Gennaro and Ash \(2023\)](#) report that the “share of regular viewers [of C-SPAN] increased from 8.6% of the U.S. population in 1994, to 12% in 2004, to 14.8% in 2013”, and that “according to a 2021 survey, 60 million U.S. adults have watched C-SPAN in the last week, 73 million in the last month, and 85 million in the last 6 months”. Beyond direct broadcast, congressional speeches also reach citizens through mainstream media, e.g., local television stations running C-SPAN clips of local member ([Maltzman and Sigelman, 1996](#)). Moreover, as discussed above, when women address GBV in their congressional speeches, it may signal their engagement with the issue in other public settings as well.

Given the evidence above, it could be argued that the visibility and awareness raised by female Representatives’ speeches extend beyond their district borders. However, we claim that female Representatives’ influence is stronger among their own constituents, since their public interventions are likely to have greater local resonance, e.g., in local media outlets. For example, [Hayes and Lawless \(2016\)](#) show that local media play a crucial role in shaping voters’ exposure to congressional candidates and officeholders, especially female politicians, who tend to be covered more in local outlets than in national ones.

5.4 | Attitudes Towards Reporting and Towards the Police

Female leadership may affect GBV by shaping gender-related social norms. Both men and women may change their perceptions about gender equality when a female from the district becomes a visible political leader. Women may become more empowered, and thus be more likely to stand up to violence and to increase their rate of reporting when they are victimized. At the same time, men may also display more positive views about women in society, making them less likely to commit gender-based crimes. However, female leadership may also provoke resentment among men, yielding a backlash effect that may translate into greater levels of violence against women, e.g., as a way to express their frustration or to intimidate female leaders and voters ([Iyer et al., 2012](#)).

To shed light on these mechanisms, we retrieve data from the Time Series Cumulative Data Files of the American National Election Studies (ANES) series for the period 1970-2016 ([ANES, 2019](#)), and from individual studies for 2004 and 2008 ([ANES, 2005, 2015](#)). ANES is a nationally representative study conducted in the U.S. approximately every election year, around the time of the elections. Surveys can take place both before and after an election is held. In our analysis, we only use the information whenever it is collected post-election. The final dataset allows us to estimate effects on attitudes in the very short run as the outcome variables are measured, at the earliest, on the day of the election and, at the latest, around 90 days after the election.

There are several limitations with regards to these series. First, they are not representative at the district level. Yet, if sampling is orthogonal to specific characteristics of the survey respondents, our results should continue to be unbiased. Second, the survey instrument is not consistent over time, which generates gaps in the series for certain questions. Nevertheless, ANES remains a valuable and unique source of information on a vast array of issues.

In particular, in 2004 and 2008, survey respondents were asked to give their opinion about the statement “women who complain about harassment cause more problems than they solve.” Although this question does not specifically relate harassment or discrimination

to the use of violence against women, it is still a good indicator of how women perceive reporting. Original records are collected using Likert-type questions. Based on these data, we define two outcome variables. First, a dummy indicator that is equal to one whenever the individual disagrees with the statement. Second, we compute the corresponding standardized indices based on the original (non-binary) response values. To estimate the impact of female leadership on these views, we rely on local randomization methods ([Cattaneo et al., 2016, 2018a](#)), which we deem more appropriate to deal with the limited sample size and the masspoints we identify in the ANES data ([Cattaneo et al., 2017](#)).

Our estimates indicate that the election of a female Representative has a positive effect on women's views about reporting. Columns (1) and (2) in Table 5 present the results by sex of the respondent, using a first order transformation. These results provide supporting evidence of women becoming more likely to hold positive views about denouncing harassment. In turn, Panel B shows that the election of a Congresswoman has no impact on men's views about this subject when using the dummy specification, while there is a significant and positive effect when using the standardized index. This mixed evidence for men suggests that, if anything, their views about denouncing harassment may become more positive as well, which provides evidence against the existence of a backlash effect.

TABLE 5 The Effect of Female Representation on Views and Attitudes towards Reporting and the Police

Panel A: Women				
	Harassment		Police	
	Dummy	Index	Dummy	Index
	(1)	(2)	(3)	(4)
Coeff	.406	.611	.464	.495
p-value	.006	.028	0	.092
lWindow	9.439	9.439	3.997	3.997
rWindow	22.309	22.309	4.209	4.209
N-	21	21	16	16
N+	31	31	35	35

Panel B: Men				
	Harassment		Police	
	Dummy	Index	Dummy	Index
	(1)	(2)	(3)	(4)
Coeff	.213	.538	-.004	.118
p-value	.161	.021	.821	.365
lWindow	3.783	3.783	13.682	13.682
rWindow	26.478	26.478	13.917	13.917
N-	23	23	77	77
N+	32	32	94	94

Notes: Dependent variables are indicator variables (odd columns) or standardized values of the original responses (even columns), as described in Section 5.4. Results are from local randomization (rdrandinf). Individual characteristics used for balance-test: black, Hispanic, age, Democratic. N+ refers to the number of observations above the threshold MV=0, N− to those below. lWindow and rWindow are the left-and-right limit of the interval considered. Values for the difference in mean, and p-values are reported.

The ANES Cumulative Data Files also allow us to measure the effect of elected female leaders on views about the police. Respondents are asked to rate their feelings towards police on a scale from 0 to 100, where higher values are associated with more favorable and warmer feelings.¹⁹ Based on these data, we construct an indicator variable equal to one if the respondent rated their feelings above 50 as well as a standardized index based on the original responses. Columns (3) and (4) in Panel A shows that women do view the police more favorably after a female leader is elected. On the other hand, Panel B shows that there is no such change among men.

Overall, these findings reinforce the interpretation that the documented increase in rapes

¹⁹This question is asked post-election in the following years: 1970, 1972, 1974, 1976, 1992 and 2016.

is driven by increased reporting. These results also rule out a backlash effect since men's attitudes are not affected negatively by the election of a female Representative, and may actually have more positive attitudes towards reporting.

6 | CONCLUSION

Several studies have examined how female political representation influences the incidence and reporting of GBV. We contribute to this literature by focusing on the United States, a developed country with persistently high GBV levels, and by isolating the role model effect from direct policy effects. Unlike studies in developing contexts that rely on quota-based reforms or local executives with policymaking power, our setting uses close elections to the U.S. House of Representatives, where laws are enacted at the federal level but outcomes are observed at the district level. This separation between legislative authority and electoral constituency allows us to distinguish symbolic and cultural influences from policy-driven mechanisms.

Using detailed FBI crime data, we show that districts narrowly electing a female Representative experience a substantial increase in reported rapes, consistent with improved reporting rather than heightened violence. We find no significant change in police activity following a woman's election, suggesting the effect does not operate through enforcement. Supporting evidence from Congressional speeches indicates that Congresswomen are more likely to discuss GBV-related issues, and that their attention to these topics correlates with higher reporting in their districts. Moreover, survey evidence from ANES reveals that women develop more favorable attitudes toward reporting harassment and greater trust in law enforcement after a female Representative takes office, while men's views are mostly unaffected. Together, these findings highlight that female political representation in the U.S. influences GBV outcomes primarily through symbolic and social channels. By increasing visibility and challenging gender norms, female leaders appear to empower survivors to come forward.

ACKNOWLEDGEMENTS

We thank the late Jorge Agüero for his guidance during the early stages of this project. We are grateful to Johanna Rickne (editor) and two anonymous referees for their insightful suggestions, and to Juanjo Dolado, Roberto Galbiati, David K. Levine, Andrea Mattozzi, Paola Profeta, and Giulio Zanella, as well as seminar participants at LUISS and EUI, for valuable comments. We also thank Nelson Oviedo, Alejandro Herrera, and Olivia Masi for excellent research assistance. Frisancho acknowledges financial support from the Inter-American Development Bank (IDB). Pappa acknowledges support from MICIU/AEI (Proyecto gran CEX2021-001181-M, PGC2018-094321-B-I00) and Comunidad de Madrid (grant EPUC3M11, V PRICIT).

REFERENCES

- Adler, E. S. (2003) Congressional district data file, [92-105 congress]. University of Colorado, Boulder, CO.
- Agüero, J. and Frisancho, V. (2022) Measuring Violence against Women with Experimental Methods. *Economic Development and Cultural Change*.
- Amaral, S. (2017) Do improved property rights decrease violence against women in india? *Working Paper*. Available at SSRN: <https://ssrn.com/abstract=2504579> or <https://dx.doi.org/10.2139/ssrn.2504579>.
- Amaral, S., Bhalotra, S. and Prakash, N. (2021) Gender, crime and punishment: Evidence from women police stations in india. *CESifo Working Paper No. 9002*. Available at SSRN: <https://ssrn.com/abstract=3827615> or <https://dx.doi.org/10.2139/ssrn.3827615>.
- Amaral, S., Dahl, G. B., Endl-Geyer, V., Hener, T. and Rainer, H. (2023) Deterrence or backlash? arrests and the dynamics of domestic violence. NBER Working paper 30855.
- ANES (2005) The american national election studies, 2004 time series study.
- (2015) The american national election studies, 2008 time series study.
- (2019) The american national election studies, time series cumulative data file (1948-2016).
- Anukriti, S., Erten, B. and Mukherjee, P. (2024) Female Leaders and Intrahousehold Dynamics: Evidence from State Elections in India. Working paper.
- Anzia, S. F. and Berry, C. R. (2011) The jackie (and jill) robinson effect: Why do congress-women outperform congressmen? *American Journal of Political Science*, **55**, 478–493. URL: <https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1540-5907.2011.00512.x>.
- Bagues, M. and Campa, P. (2021) Can gender quotas in candidate lists empower women? evidence from a regression discontinuity design. *Journal of Public Economics*, **194**, 104315. URL: <https://www.sciencedirect.com/science/article/pii/S0047272720301791>.
- Baltrunaite, A., Bello, P., Casarico, A. and Profeta, P. (2014) Gender quotas and the quality of politicians. *Journal of Public Economics*, **118**, 62 – 74. URL: <http://www.sciencedirect.com/science/article/pii/S0047272714001480>.
- Bauer, N. M. (2020) *The Qualifications Gap: Why Women Must Be Better than Men to Win Political Office*. Cambridge University Press.
- Beaman, L., Chattopadhyay, R., Duflo, E., Pande, R. and Topalova, P. (2009) Powerful women: Does exposure reduce bias? *The Quarterly Journal of Economics*, **124**, 1497–1540. URL: <http://www.jstor.org/stable/40506265>.
- Beaman, L., Duflo, E., Pande, R. and Topalova, P. (2012) Female leadership raises aspirations and educational attainment for girls: A policy experiment in india. *Science*, **335**, 582–586. URL: <https://science.sciencemag.org/content/335/6068/582>.
- Bhalotra, S. and Clots-Figueras, I. (2014) Health and the political agency of women. *American Economic Journal: Economic Policy*, **6**, 164–97. URL: <https://www.aeaweb.org/articles?id=10.1257/pol.6.2.164>.
- Bhalotra, S. R., Clarke, D., Gomes, J. F. and Venkataramani, A. (2022) Maternal mortality and women’s political power. *Working Paper 30103*, National Bureau of Economic Research. URL: <http://www.nber.org/papers/w30103>.
- Black, J. and Erickson, L. (2003) Women candidates and voter bias: do women politicians need to be better? *Electoral Studies*, **22**, 81 – 100. URL: <http://www.sciencedirect.com/science/article/pii/S0261379401000282>.
- Bochenkova, A., Buonanno, P. and Galletta, S. (2023) Fighting violence against women: The role of female political representation. *Journal of Development Economics*, **164**, 103140. URL: <https://www.sciencedirect.com/science/article/pii/S0304387823000950>.

- Brennan Ctr. for Justice (2025) Judicial selection: An interactive map. Dataset. Available online from <https://brennancenter.org/judicial-selection-map> (September).
- Brogaard, J., Gerasimova, N. and Rohrer, M. (2024) The effect of female leadership on contracting from capitol hill to main street. *Journal of Financial Economics*, **155**, 103817. URL: <https://www.sciencedirect.com/science/article/pii/S0304405X24000400>.
- Brollo, F. and Troiano, U. (2016) What happens when a woman wins an election? evidence from close races in brazil. *Journal of Development Economics*, **122**, 28–45.
- Broockman, D. E. (2014) Do female politicians empower women to vote or run for office? a regression discontinuity approach. *Electoral Studies*, **34**, 190 – 204. URL: <http://www.sciencedirect.com/science/article/pii/S0261379413001583>.
- Bruce, R., Cavgias, A., Meloni, L. and Remígio, M. (2022) Under pressure: Women’s leadership during the covid-19 crisis. *Journal of Development Economics*, **154**, 102761. URL: <https://www.sciencedirect.com/science/article/pii/S0304387821001243>.
- Calonico, S., Cattaneo, M. D. and Titiunik, R. (2014a) Robust data-driven inference in the regression-discontinuity design. *Stata Journal*, **14**, 909–946(38). URL: <http://www.stata-journal.com/article.html?article=st0366>.
- (2014b) Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, **82**, 2295–2326. URL: <https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA11757>.
- Carozzi, F. and Gago, A. (2023) Who promotes gender-sensitive policies? *Journal of Economic Behavior Organization*, **206**, 371–405. URL: <https://www.sciencedirect.com/science/article/pii/S0167268122004425>.
- Cattaneo, M., Titiunik, R. and Vazquez-Bare, G. (2016) Inference in regression discontinuity designs under local randomization. *Stata Journal*, **16**, 331–367. URL: <https://EconPapers.repec.org/RePEc:tsj:stataj:y:16:y:2016:i:2:p:331-367>.
- Cattaneo, M. D., Idrobo, N. and Titiunik, R. (2018a) *A Practical Introduction to Regression Discontinuity Designs: Volume II*. Prepared for: Elements in Quantitative and Computational Methods for the Social Sciences.
- (2020) *A Practical Introduction to Regression Discontinuity Designs: Foundations*. Elements in Quantitative and Computational Methods for the Social Sciences. Cambridge University Press.
- Cattaneo, M. D., Jansson, M. and Ma, X. (2018b) Manipulation testing based on density discontinuity. *The Stata Journal*, **18**, 234–261. URL: <https://doi.org/10.1177/1536867X1801800115>.
- (2019) Simple local polynomial density estimators. *Journal of the American Statistical Association*, **0**, 1–7. URL: <https://doi.org/10.1080/01621459.2019.1635480>.
- Cattaneo, M. D., Titiunik, R. and Vazquez-Bare, G. (2017) Comparing inference approaches for rd designs: A reexamination of the effect of head start on child mortality. *Journal of Policy Analysis and Management*, **36**, 643–681. URL: <https://onlinelibrary.wiley.com/doi/abs/10.1002/pam.21985>.
- Cella, M. and Manzoni, E. (2019) Gender bias and women political performance. *Working Papers 414*, University of Milano-Bicocca, Department of Economics. URL: <https://ideas.repec.org/p/mib/wpaper/414.html>.
- Cerqua, A., Giannantoni, C., Letta, M. and Pinto, G. (2024) Femicides, anti-violence centers, and public salience. Available at SSRN: <https://ssrn.com/abstract=4899917> or <http://dx.doi.org/10.2139/ssrn.4899917>.
- Chattopadhyay, R. and Duflo, E. (2004) Women as policy makers: Evidence from a randomized policy experiment in india. *Econometrica*, **72**, 1409–1443. URL: <https://onlinelibrary.wiley.com/doi/abs/10.1111/j.1468-0262.2004.00539.x>.

- Clots-Figueras, I. (2011) Women in politics: Evidence from the indian states. *Journal of Public Economics*, **95**, 664 – 690. URL: <http://www.sciencedirect.com/science/article/pii/S0047272710001775>.
- (2012) Are female leaders good for education? evidence from india. *American Economic Journal: Applied Economics*, **4**, 212–244. URL: <http://www.jstor.org/stable/41419430>.
- Coppolo, G. C. A. (2003) States that elect their chief prosecutors. Connecticut General Assembly. Report 2003-R-0231. Available online from <https://www.cga.ct.gov/2003/rpt/2003-R-0231.htm>.
- Delaporte, M. and Pino, F. (2022) Female Political Representation and Violence against Women: Evidence from Brazil. IZA Discussion Paper No. 15365.
- Eggers, A. C., Fowler, A., Hainmueller, J., Hall, A. B. and Snyder Jr., J. M. (2015) On the validity of the regression discontinuity design for estimating electoral effects: New evidence from over 40,000 close races. *American Journal of Political Science*, **59**, 259–274. URL: <https://onlinelibrary.wiley.com/doi/abs/10.1111/ajps.12127>.
- Esteve-Volart, B. and Bagues, M. (2012) Are women pawns in the political game? evidence from elections to the spanish senate. *Journal of Public Economics*, **96**, 387 – 399. URL: <http://www.sciencedirect.com/science/article/pii/S0047272711001848>.
- FBI, C. J. I. S. D. (2013) *Summary Reporting System (SRS) User Manual*. U.S. Department of Justice - FBI.
- Foster-Molina, E. (2017) Historical congressional legislation and district demographics 1972-2014. URL: <https://doi.org/10.7910/DVN/CI2EPI>.
- Fox, J. A. (2004) Missing data problems in the shr: Imputing offender and relationship characteristics. *Homicide Studies*, **8**, 214–254. URL: <https://doi.org/10.1177/1088767904265592>.
- Gennaro, G. and Ash, E. (2023) Televised debates and emotional appeals in politics: Evidence from cspan. URL: <https://elliottash.com/wp-content/uploads/2023/02/Gennaro-Ash-Televised-Debates-Emotional-Appeals-Politics-Evidence-CSPAN.pdf>. Working paper, available at <https://elliottash.com/wp-content/uploads/2023/02/Gennaro-Ash-Televised-Debates-Emotional-Appeals-Politics-Evidence-CSPAN.pdf>.
- Gentzkow, M., Shapiro, J. M. and Taddy, M. (2018) Congressional record for the 43rd-114th congresses: Parsed speeches and phrase counts. URL: https://data.stanford.edu/congress_text. Palo Alto, CA: Stanford Libraries [distributor].
- Gerrity, J. C., Osborn, T. and Mendez, J. M. (2007) Women and representation: A different view of the district? *Politics & Gender*, **3**, 179–200.
- Guarnieri, E. and Rainer, H. (2018) Female empowerment and male backlash. *Tech. rep.*, CESifo Working Paper.
- Hayes, D. and Lawless, J. L. (2016) *Women on the run: Gender, media, and political campaigns in a polarized era*. Cambridge University Press.
- Hessami, Z. and da Fonseca, M. L. (2020) Female political representation and substantive effects on policies: A literature review. *European Journal of Political Economy*, **63**, 101896. URL: <https://www.sciencedirect.com/science/article/pii/S0176268020300446>.
- Hidrobo, M., Peterman, A. and Heise, L. (2016) The effect of cash, vouchers, and food transfers on intimate partner violence: Evidence from a randomized experiment in northern ecuador. *American Economic Journal: Applied Economics*, **8**, 284–303. URL: <https://www.aeaweb.org/articles?id=10.1257/app.20150048>.
- Imbens, G. and Lemieux, T. (2008) Regression discontinuity designs: A guide to practice. *Journal of Econometrics*, **142**, 615–635.

- Iyer, L., Mani, A., Mishra, P. and Topalova, P. (2012) The power of political voice: Women's political representation and crime in india. *American Economic Journal: Applied Economics*, **4**, 165–193. URL: <http://www.jstor.org/stable/23269746>.
- Jaffe, A. E., Cero, I. and DiLillo, D. (2021) The #metoo movement and perceptions of sexual assault: College students' recognition of sexual assault experiences over time. *Psychol Violence*, **11**, 209–218.
- U.S. Dept. of Justice, F. B. o. I. (2000) Uniform crime reporting program data: [united states], 1975-1997 [supplementary homicide reports, 1982] [computer file]. compiled by the u.s. dept. of justice, federal bureau of investigation. 2nd icpsr ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor].
- (2005) Data, uniform crime reporting program data [united states]: 1975-1997. Inter-university Consortium for Political and Social Research [distributor].
- U.S. Dept. of Justice, Office of Justice, P. B. o. J. S. (1992) Directory of law enforcement agencies, 1986: [united states]. 1992-02-16. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor].
- Kaplan, J. (2019a) Jacob kaplan's concatenated files: Uniform crime reporting (ucr) program data: Supplementary homicide reports, 1976-2017. URL: <https://doi.org/10.3886/E100699V7>. Inter-university Consortium for Political and Social Research [distributor].
- (2019b) Uniform crime reporting (ucr) program data: Raw data. URL: <https://doi.org/10.3886/E108361V1>. Inter-university Consortium for Political and Social Research [distributor].
- Koteen, B., Baranouski, E., Ruttenberg, J. and Stafford Stein, C. (2014) The fast track to a u.s. attorney's office. Harvard Law School, report. Available online from <https://hls.harvard.edu/content/uploads/2008/06/fast-track-final.pdf>.
- Kuipers, N. (2020) The effect of electing female candidates on attitudes toward intimate partner violence. *The Journal of Politics*, **0**, 000–000. URL: <https://doi.org/10.1086/708685>.
- Le Barbanchon, T. and Sauvagnat, J. (2021) Electoral Competition, Voter Bias, and Women in Politics. *Journal of the European Economic Association*, **20**, 352–394. URL: <https://doi.org/10.1093/jeea/jvab028>.
- Lee, B. (2012) Congresswoman barbara lee speaking about the violence against women act. YouTube video. URL: https://www.youtube.com/watch?v=Qg_cYdMNa8M&ab_channel=Rep.BarbaraLee. Available at https://www.youtube.com/watch?v=Qg_cYdMNa8M&ab_channel=Rep.BarbaraLee.
- Lewis, J. B., DeVine, B., Pitcher, L. and Martis, K. C. (2013) Digital boundary definitions of united states congressional districts, 1789-2012. Retrieved from <http://cdmaps.polisci.ucla.edu>.
- Lim, C. S. H., Snyder, James M., J. and Strömberg, D. (2015) The judge, the politician, and the press: Newspaper coverage and criminal sentencing across electoral systems. *American Economic Journal: Applied Economics*, **7**, 103–35. URL: <https://www.aeaweb.org/articles?id=10.1257/app.20140111>.
- Lippmann, Q. (2022) Gender and lawmaking in times of quotas. *Journal of Public Economics*, **207**, 104610. URL: <https://www.sciencedirect.com/science/article/pii/S0047272722000123>.
- Loftin, C., McDowall, D. and Fetzer, M. D. (2008) A comparison of shr and vital statistics homicide estimates for u.s. cities. *Journal of Contemporary Criminal Justice*, **24**, 4–17. URL: <https://doi.org/10.1177/1043986207312585>.
- Lublin, D. (1997) Congressional district demographic and political data, 1972-1994.

- Luca, D. L., Owens, E. and Sharma, G. (2015) Can alcohol prohibition reduce violence against women? *American Economic Review*, **105**, 625–29. URL: <https://www.aeaweb.org/articles?id=10.1257/aer.p20151120>.
- Maltz, M. D. (1999) Bridging gaps in police crime data. **Report No. NCJ-1176365. Bureau of Justice Statistics, Office of Justice Programs, U.S. Department of Justice, Washington, DC, September, 1999.** URL: <http://www.ojp.usdoj.gov/bjs/pub/pdf/bgpcd.pdf>.
- Maltz, M. D. and Targonski, J. (2002) A note on the use of county-level ucr data. *Journal of Quantitative Criminology*, **18**, 297–318. URL: <https://doi.org/10.1023/A:1016060020848>.
- (2004) Making ucr data useful and accessible. URL: <https://www.ncjrs.gov/pdffiles1/nij/grants/205171.pdf>.
- Maltzman, F. and Sigelman, L. (1996) The politics of talk: Unconstrained floor time in the u.s. house of representatives. *The Journal of Politics*, **58**, 819–830. URL: <https://doi.org/10.2307/2960448>.
- Marshall, J. (2022) Can Close Election Regression Discontinuity Designs Identify Effects of Winning Politician Characteristics? *American Journal of Political Science*.
- McCrary, J. (2008) Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, **142**, 698 – 714. URL: <http://www.sciencedirect.com/science/article/pii/S0304407607001133>. The regression discontinuity design: Theory and applications.
- Miller, A. R. and Segal, C. (2018) Do Female Officers Improve Law Enforcement Quality? Effects on Crime Reporting and Domestic Violence. *The Review of Economic Studies*, **86**, 2220–2247. URL: <https://doi.org/10.1093/restud/rdy051>.
- Moore, G. (2012) Gwen moore recounts personal story of domestic violence during motion to recommit on vawa. YouTube video. URL: <https://www.youtube.com/watch?v=bP5WGOcHHnQ>. Available at <https://www.youtube.com/watch?v=bP5WGOcHHnQ>.
- Murvarian, L., Saavedra-Macías, F. J. and Infanti, J. J. (2023) Public stigma toward women victims of intimate partner violence: A systematic review. *Aggression and Violent Behavior*, **73**, 101877.
- NCADV, N. C. A. D. V. (2014) Statistics. URL: <https://ncadv.org/STATISTICS>.
- NSVRC, N. S. V. R. C. (2015) Statistics about sexual violence. URL: https://www.nsvrc.org/sites/default/files/publications_nsvrc_factsheet_media_packet_statistics-about-sexual-violence_0.pdf. Available at https://www.nsvrc.org/sites/default/files/publications_nsvrc_factsheet_media_packet_statistics-about-sexual-violence_0.pdf.
- Osborn, T. and Mendez, J. M. (2010) Speaking as women: Women and floor speeches in the senate. *Journal of Women, Politics & Policy*, **31**, 1–21. URL: <https://doi.org/10.1080/15544770903501384>.
- Palermo, T., Bleck, J. and Peterman, A. (2014) Tip of the Iceberg: Reporting and Gender-based Violence in Developing Countries. *American Journal of Epidemiology*, **179**, 602–612.
- Pettigrew, S., Owen, K. and Wanless, E. (2014) U.S. House Primary Election Results (1956–2010). URL: <https://doi.org/10.7910/DVN/26448>.
- PoliticalParity (2015) Dataset for the report *Where Women Win: Closing the Gap in Congress*, by Shauna Shames. Available at <https://www.politicalparity.org/research/where-women-win/>.
- Priyanka, S. (2020) Do female politicians matter for female labor market outcomes? evidence from state legislative elections in india. *Labour Economics*, **64**, 101822. URL: <http://www.sciencedirect.com/science/article/pii/S0927537120300282>. European Association of Labour Economists, 31st annual conference, Uppsala Sweden, 19–21 September 2019.

- Reflective Democracy Campaign (2019) Tipping the scales: Challengers take on the old boys club of elected prosecutors. Report. Available online from <https://wholeads.us/wp-content/uploads/2019/10/Tipping-the-Scales-Prosecutor-Report-10-22.pdf>.
- Reflective Democracy Campaign (2020) Confronting the demographics of power: America's sheriffs. Report. Available online from <https://wholeads.us/wp-content/uploads/2020/06/reflectivedemocracy-americaassheriffs-06.04.2020.pdf>.
- Riedel, M. and Regoeczi, W. C. (2004) Missing data in homicide research. *Homicide Studies*, 8, 163–192. URL: <https://doi.org/10.1177/1088767904265447>.
- Siczewicz, P. (2011) U.s. historical counties. Dataset. Emily Kelley, digital comp. Atlas of Historical County Boundaries, ed. by John H. Long. Chicago: The Newberry Library, 2011. Available online from <http://publications.newberry.org/ahcbp>.
- Targonski, J. R. (2011) *A Comparison of Imputation Methodologies in the Offenses Known Uniform Crime Reports*. Ph.D. thesis, University of Illinois at Chicago. URL: <https://www.ncjrs.gov/pdffiles1/nij/grants/235152.pdf>.
- Times, L. A. (2019) Female lawmakers speak about rapes as abortion bills advance. URL: <https://www.latimes.com/nation/la-na-abortion-women-lawmakers-20190518-story.html>. Available at <https://www.latimes.com/nation/la-na-abortion-women-lawmakers-20190518-story.html>.
- Tur-Prats, A. (2021) Unemployment and intimate partner violence: A cultural approach. *Journal of Economic Behavior & Organization*, 185, 27–49. URL: <https://www.sciencedirect.com/science/article/pii/S0167268121000615>.
- Volden, C., Wiseman, A. E. and Wittmer, D. E. (2013) When are women more effective lawmakers than men? *American Journal of Political Science*, 57, 326–341. URL: <https://onlinelibrary.wiley.com/doi/abs/10.1111/ajps.12010>.
- Zeigermann, L. (2018) Opencagegeo: Stata module for forward and reverse geocoding using the opencage geocoder api. URL: <https://EconPapers.repec.org/RePEc:boc:bocode:s458155>.
- Zhang, Y. and Breunig, R. (2021) Gender norms and domestic abuse: evidence from australia. *Tech. rep.*, IZA Discussion Papers.

Appendix A | DATA DETAILS

Appendix A.1 | Election Data

As mentioned in Subsection 2.2, elections for the U.S. House of Representatives occur at the Congressional district (CD) level, with each district electing one Representative. Every two years (in all even years), all 435 seats in the U.S. House are up for re-election, generating a total of 10,440 races between 1970 and 2016. Table A1 shows a break-down of elections by type of race, defined by the sexes of the top candidates and the winning party. Specifically, we look at races in which the top two candidates are either from the Democratic party (D) or the Republican party (R), and divide our sample into the following main categories: man running against a man, woman against woman, and man against woman, i.e., a mixed-gender race. Mixed-gender races are further divided by sex of the winning candidate.²⁰ Overall, mixed-gender races represent 18.2% of the whole sample (1,895) and appear to be more frequently won by men (1,081 vs 814 wins).

While Democrats and Republicans do not differ substantially in the total number of mixed-gender races won (956 vs 939), they do not seem to end up electing the same candidates: out of all the elections in which a woman defeats a man, the number of Democrat winners is more than twice that of Republican winners, with Democratic Congresswomen representing 68% of women elected in this type of race (559 vs 255).²¹ Instead, the opposite holds for mixed-gender elections won by men, in which a male Republican is more likely to be elected.

Given the correlation between political affiliation and geography in the U.S. (e.g., southern states leaning Republican, and coastal states leaning Democrat), Figure A1 reports the geographical distribution of mixed-gender races. For each state, Panel (a) shows the share of mixed-gender races for the U.S. House held between 1970 and 2016 with respect to the total number of elections held during the same period. These figures are aggregated at the state level, as district boundaries and assignment to a given state may change over time. Panel (b) depicts the share of mixed-gender races won by women. As we can see, coastal states tend to have more mixed-gender races. However, we also find some inner states among those with the highest share of mixed-gender races won by women, suggesting that, even though these states are less likely to have mixed-gender races, a female candidate is more likely to win when they do occur.

We match these data with district characteristics for the period 1972–2016 (93rd to 115th U.S. Congress) using the files from Foster-Molina (2017). Specifically, we consider the percentage of older citizens; the percentage of Black or Hispanic population; the percentage of people with a high-school degree and with a Bachelor's degree; the unemployment rate; median family income; and the Gini index.²²

²⁰We also include one-candidate races, *Unopposed*. Those listed in CQ Press as Democrat Farmer-Labor (for Minnesota) or as Democrat-Open Primary are counted as Democrats. Similarly, those listed as Independent Republican (for Minnesota), Republican Write-in, or Republican-Open Primary are counted as Republicans. Races in which candidates from the same party occupy both first and second place are also included.

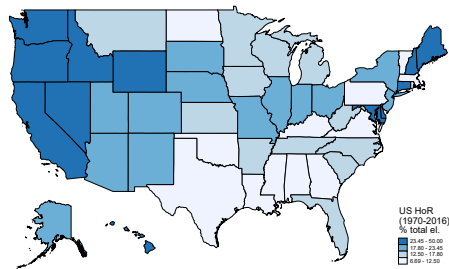
²¹These numbers include mixed-gender races in which candidates belong to the same party. If we consider mixed-gender races between a Democrat and a Republican, results are virtually unchanged: out of 1,880 races, 43% are won by a woman (805). Out of these, 550 are won by a Democratic woman and 255 by a Republican woman. A similar proportion is found when considering all races: out of a total of 1,063 races appointing a woman to the U.S. House, irrespective of the type of election, 69% are won by a woman belong to the Democratic party (736) and 31% to the Republican party (327).

²²The original data span from the 93rd to 113th Congress. We expanded the dataset by inputting data from the 113th Congress to the 114th and 115th Congresses. The cutoff to determine "old age" share in the district in Foster-Molina (2017) is 60 or 62 years of age, depending on the age brackets used by the Census for that decade.

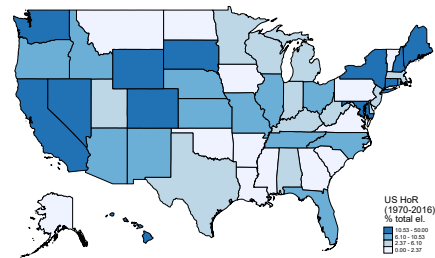
TABLE A1 Races by Sex of the Top Candidates and Winning Party

Race	Winning party				Total	
	D		R			
	No.	%	No.	%	No.	%
Man vs man	3,721	65.6	3,260	68.6	6,981	66.9
Man vs woman, M	397	7.0	684	14.4	1,081	10.4
Man vs woman, W	559	9.9	255	5.4	814	7.8
Unopposed	894	15.8	499	10.5	1,393	13.4
Woman vs woman	103	1.8	56	1.2	159	1.5
Total	5,674	100.0	4,754	100.0	10,428	100.0

Note: Races span from 1970 to 2016. The number or share of elections won by type of race (defined by sex of the top candidates) and winning party. During this period, 12 races were won by an Independent, to which no gender was assigned. Note that *Unopposed* includes cases in which a Democrat or Republican has defeated a third-party opponent, in addition to actual one-candidate races. Instances in which candidates from the same party occupy both first and second places are also considered.



(a) Share of Mixed-Gender Races



(b) Share of Mixed-Gender Races Won by Women

FIGURE A1 Share of mixed-gender races as a percentage of the total number of elections per state

Note: Data on elections from 1970 to 2016, source: CQ Press. Panel A: share of mixed-gender races, by state. Panel B: share of mixed-gender races won by women, by state. Both shares are computed with respect to the total number of elections occurring in a given state.

Appendix A.2 | Crime Data

The Uniform Crime Report of the FBI, henceforth UCR, is a voluntary program under which law enforcement agencies provide high-frequency reports on crime statistics corresponding to their jurisdictions.²³ For our analysis, we consider the Offenses Known and Clearances by Arrest (OKCA) files for the period 1970–2017 and the Supplementary Homicide Report (SHR) files for the period 1976–2017, by Kaplan (2019b) and Kaplan (2019a).

The OKCA provides data at the month–agency level for several felonies. These statistics capture information such as the number of victims and whether the report was cleared, i.e., considered closed or solved for crime reporting purposes. The month recorded corresponds to the reporting date, but the occurrence date is not observed. They do not include information on the relationship between the victim and the perpetrator. From these files, we retrieve the number of rape victims, including both forcible and attempted rapes, between 1970 and 2012 – when this crime was explicitly defined as “carnal knowledge of a female forcibly against her will” by a male (FBI, 2013).²⁴ Other felonies such as assault, robbery, and motor vehicle theft are also included in these files.

The SHR is an incident-based dataset providing detailed information on single cases of homicide, such as the month in which it occurred and the sex of both victim(s) and offender(s), whenever known. From this, we retrieve different measures of fatal crimes involving women. First, we compute the total number of women killed by counting the number of victims of murders and non-negligent manslaughter in which all victims are women, irrespective of the identity of the offender. Since not all killings of women constitute a gender-based crime, we then restrict our attention to cases in which all offenders are men.

We further focus on those in which the victim–offender relationship is one of the following: ex-wife, wife, common-law wife, girlfriend, daughter, mother, sister, stepdaughter, or stepmother. We refer to these cases as *intimate femicides*.²⁵

The UCR poses at least three challenges: data might not be representative; there may be missing observations and other anomalies in the reporting; and assumptions need to be made regarding how to appropriately measure crime at the CD level. Appendix B discusses in detail how we deal with these three issues, and we summarize the key points of our strategy below.

To have a more precise measurement of crimes occurring within the boundaries of a given CD, we restrict our focus to local law enforcement agencies, such as a city police department or a sheriff’s office. Moreover, since there is great variation in the frequency of reporting (e.g., some agencies report semi-annual data twice a year), in the main analysis, we restrict our sample to agencies reporting monthly data for all 12 months²⁶ and aggregate the data at the year–district level. To do so, we augment these files with information on the geographical location of the agencies. Using the addresses listed in the OKCA, we recover their spatial coordinates and determine the district in which an agency was located in each year, using district maps from Lewis et al. (2013) and the Census Bureau. We then compute the number of female victims of murders – total, by males, and victims of femicide – and of rape reported by every agency a in district d in year t , and normalize it by the population

²³Despite its voluntary nature, the sample of agencies included in the UCR covers the vast majority of the U.S. population (approximately 94% according to recent statistics from the U.S. Department of Justice, see also Appendix B).

²⁴As noted, after 2013 the definition was expanded to include victims and perpetrators of both genders, which prevents us from using more recent data.

²⁵The SHR contains information on the relationship between what is registered as *first* victim and the offender(s), where up to 11 offenders can be listed. However, the vast majority of cases involve only one victim and/or one offender.

²⁶As a robustness check, we also consider a sample including agencies without complete monthly reporting. See also Appendix B.

size of the district in year t (per 100,000 residents), retrieved from [Adler \(2003\)](#) and the Census Bureau. A similar procedure is followed to measure other crimes (e.g., assaults, burglaries) and clearances in a given district. Crimes against individuals record the number of victims, while crimes against property (e.g., motor vehicle thefts) record the number of occurrences. For fatal crimes involving women and for rapes, we consider an additional normalization in terms of female population.²⁷

Crime data are then linked to the race happening in district d in year t by summing all crimes reported between $t + 1$ and $t + 2$, that is, over the full elected term. We deemed this appropriate, as the candidates who win a November election are officially sworn into Congress and thus start their term the following January. We additionally consider all crime rates reported in year $t + 1$ and in year $t + 2$ separately, accounting for crimes in each year of the term.

A final note pertains to the issue of CD boundaries, which can be redrawn from one session of Congress to another. This implies that a district in year $t + 2$, despite maintaining the same name (e.g., Texas 13th CD), might cover a different area than it did in year t . While using yearly maps helps us to keep track of the different districts where agencies are located, the changes in their boundaries might still generate inconsistencies when aggregating for longer periods. Adopting a more conservative approach, we use the geographical boundaries from [Lewis et al. \(2013\)](#), rather than their denomination, to define districts. In other words, we consider two areas to be the same CD only if they have the same boundaries. If a district changes boundaries between elections, we are still able to merge the corresponding crime data for the following year, $t + 1$, with our election data, but we are unable to compute the overall level of crime registered, since new boundaries have emerged in $t + 2$.

²⁷We use the population of women of voting age from [Lublin \(1997\)](#) and from the Census Bureau.

Appendix B | NOTES ON THE FBI DATA

A novelty of our work relative to the existing literature is that we use rich and detailed FBI data. However, this came at a cost in terms of the time and effort needed to collect these data and make them usable for academic purposes. In this section, we discuss some of the main challenges encountered in the use of the UCR data and how we dealt with them. These challenges include: (i) representativeness; (ii) identification of missing observations and frequency of the reporting, which all call for a serious reflection on (iii) how to correctly measure crime at the CD level. We then provide more details on the procedure used to geo-localize our law enforcement agencies.

Appendix B.1 | Representativeness

With respect to representativeness, the problem is threefold. On the one hand, the UCR records only those incidents that are reported to the police, which inevitably represents a subset of actual crime in the U.S. This is common when dealing with this type of data, and unfortunately there is no easy way to account for the so-called *dark figure* of crime. However, it should be noted that fatal crimes, such as those included in the SHR, should not be subject to under-reporting to the same extent as other types of felonies, as also argued for example by [Fox \(2004\)](#).

On the other hand, representativeness is undermined by the fact that participation in the reporting program is voluntary, making the list of law enforcement agencies contributing to the UCR *de facto* a subset of U.S. law enforcement agencies that might arguably be non-random. In this regard, the substantial level of actual participation and the extensive population covered should make this matter less concerning. For example, according to the [U.S. Dept. of Justice \(2000\)](#), as of 1976, over 16,000 law enforcement agencies representing a population in excess of 220 million participated in this program. For comparison, the first available census of law enforcement agencies of 1986 ([U.S. Dept. of Justice, 1992](#)) counted 16,707 agencies in the U.S. in that year. Moreover, more recent statistics from the Department of Justice (DoJ) highlight that agencies in the UCR provide services to approximately 94% of the U.S. population. The DoJ also recognizes that coverage is volatile across time and data items, e.g., “the proportion of the US population covered by agencies reporting arrest information ranged from 63% to 94% between 1980 and 2008, with 2008 coverage of 76%. Generally, the coverage of reported crimes (offenses) data exceeds that of arrests (see [ojdp.gov](#)).

At the same time, the representativeness of participating agencies does not necessarily imply that the UCR is representative of crimes in the country. As highlighted in [Maltz \(1999\)](#), the fact that the FBI cannot mandate agencies to provide data on time or at all also affects the quality of the data. Among the reasons reducing agencies’ ability to report consistently, [Maltz and Targonski \(2002\)](#) point to natural disasters, budgetary restrictions, personnel changes, inadequate training, and conversion to new computer or crime reporting systems, as well as having very few crimes to report. Despite these issues, [Riedel and Regoeczi \(2004\)](#) note that, as of 1999, 25 states legally require reporting by their law enforcement agencies.

The SHR file presents the additional issue of it being *requested but not required* to all agencies that register some homicides in a given month, i.e., those with a non-zero value in the corresponding field in the OKCA files. In other words, some agencies might register a murder but, although they include it in their monthly aggregate in the OKCA, they might not provide additional information. This effectively makes the SHR a subsample of reported homicides. In our data, we find that the SHR information is complete for around 90% of the

cases in this respect.²⁸ This is consistent with the results of a similar analysis by Fox (2004). Comparing the victim counts from the SHR with the national FBI estimates for the period 1976–2001, Fox finds that “the SHR file appears to be slightly more than 90% complete, with annual rates of completeness ranging from a high of 96.7% in 1983 to a low of 83.7% in 1998.”

In conclusion, it is worth pointing out that this program remains, to our knowledge, the most complete and reliable source of information on crime in the U.S.

Appendix B.2 | Frequency of Reporting and Missing Observations

As mentioned, even when participating in the program, agencies decide *when* and *how* to send their reports to the FBI. Although it should be done on a monthly basis, some law enforcement agencies record quarterly data, whereas others report annual or semi-annual data; still others only report monthly data for some months. This poses several challenges with respect to the aggregation of our crime data, especially for those coming from OKCA (not incident-based).

First, arguably the lack of reporting is not always motivated by the lack of crime.²⁹ Second, these instances are not flagged clearly in the files in which gaps are filled in with zeros.

This all calls for a careful investigation to correctly distinguish missing observations, real zeros, and aggregated values. A great deal of effort has been put into such an investigation by Maltz and Targonski (2004) and Targonski (2011), who have thoroughly investigated the OKCA reports from 1977 to 2000. While we do not attempt to replicate their meticulous analysis, we rely on their methodology to identify missing values at the month–agency level in our OKCA data. More specifically, true missing values are found using the field *date-of-last-update*, which the criminologists explain is filled in only for those months in which the agency submits a report to the FBI, and hence “can serve as a proxy for whether or not [a given month’s] crime data are missing” (Targonski, 2011). Accordingly, we set all variables as missing for agency *a* in month *m* if, in that month, (i) *date-of-last-update* is either missing or 000000, and (ii) the sum of all crimes reported is zero.³⁰

The SHR needs to be treated differently, as this report should be filled in only by those agencies that report any homicide in the OKCA. In this case, we proceed as follows to identify zeros and true missing values. First, we use the same SHR and compare the number of total murders and the number of femicides at the month–agency level: if *a* reports a non-zero value of murders but no value for femicides for a given month *m*, then femicides are set to zero in that month. Then, we compare these data with the OKCA: if zero murders are reported in the OKCA and *a* is not included in the SHR – that is, it is missing according to our calculation based on the SHR data – then both homicides and femicides are set to zero. Otherwise, observations remain missing. The same procedure is followed for other types of killings, e.g., men killed by men or total number of women killed.

With respect to the aggregation of values time-wise, closer inspection revealed a great deal of variation both across and within agencies, making it challenging to combine these

²⁸ Out of a total of 10,434,084 agency–month observations in the OKCA files, we have non-missing values on the number of murders registered by an agency for 7,506,789 observations, and 6,865,154 also have non-missing information in the SHR files. In 6,836,734 cases, the two reports coincide, implying that a corresponding supplementary report is filed for every murder registered and recorded by an agency in the OKCA.

²⁹ The same FBI guidelines specify that “if no offenses have occurred during the month, the reporting agency submits the Return A with zeros in the Grand Total row” (FBI (2013)).

³⁰ According to their methodology, a given month is missing if *date-of-last-update* is missing and if all fields in the OKCA file are zero. They also note that there are cases in which there is no “date updated,” but the agency has actually reported crime data (Maltz and Targonski, 2004).

data appropriately. Rather than unpack data to build monthly estimates, as our main dataset, we restrict the sample to those agencies with complete 12-month reporting and sum data yearly. Since there might be other agencies reporting complete yearly data, albeit not every month, as a robustness check, we build an additional dataset without such restriction. It is important to note that, in this second case, we are not augmenting our measures in any way, and we do not attempt to fill in the gaps. Consequently, the level of crime that we calculate is likely lower than the FBI estimates, as these follow a different procedure. For example, when aggregating at the county–year level, the Bureau adopted the following strategy (until 1994): for agencies reporting between 3 and 11 months of data – say x months – the number of crimes reported was multiplied by the relative weight $12/x$; for those reporting less than 3 months, crime data from similar agencies were imputed.

Appendix B.3 | Measuring Crime at the CD Level

The main concern for our analysis is the use of spatial aggregation to retrieve CD estimates of crime, as the location of the agency and the location where the crime occurred might not coincide perfectly. If this were the case, the number of incidents recorded by a given law enforcement agency would not necessarily be indicative of the crimes reported in a given area. In this respect, the Summary Reporting System User Manual of the FBI clearly states that “jurisdictional guidelines provide for the most local reporting. Whenever possible, the local LEA of the geographical area in which the crime occurred should report the data” (FBI, 2013). Loftin et al. (2008) also point out that, for the SHR, “victim records are associated with the place where the underlying assault occurred, regardless of where the death occurred or where the victim or offender resided.” This should also apply in cases of multiple agencies investigating the same crime. Note that, in such cases, the FBI guidelines further specify that “a crime that occurs in one jurisdiction should not be counted by another city or county even though more than one agency participated in the investigation or the arrest of the subject of that crime” (U.S. Dept. of Justice, 2005).

A possible threat to the validity of our methodology comes from agencies *covered by* other ones, a term used to indicate law enforcement agencies using the services of another agency that also reports data to the FBI. As long as the two are located in the same CD, however, our measures are not affected by such relationships. With regard to this issue, we note that, in general, a city uses the police services of the sheriff of the county (or of another city, usually nearby). Moreover, the FBI (2013) states that “as a rule, cities having their own police department report their own crime data.”

Most importantly, agencies have different – and at times overlapping – jurisdictions. In fact, other than more local agencies (e.g., a police station covering a city), we find some agencies being responsible for an entire county (e.g., sheriff) and some with state-wide jurisdictions. To avoid double counting, the FBI guidelines say that (i) police should count crimes that occur within the city limits, and (ii) county and state law enforcement agencies should count crimes that happen in the county outside the city limits (U.S. Dept. of Justice, 2005). Nevertheless, simply summing the number of incidents recorded by all law enforcement agencies located in a given CD might still incorrectly estimate the number of crimes actually occurring in the area when we also include non-local agencies in our sample. To avoid inputting values of crime occurring in another part of the state to a given CD, we exclude from our main analysis all agencies that we could identify as state-wide (e.g., state patrol) or that had a special jurisdiction (e.g., park police). We include, instead, county-wide agencies (sheriff’s offices) because, in the majority of cases, a county is included in one CD only. Overlapping the maps of county boundaries for the period 1970–2015 (Siczewicz (2011) and Census Bureau) with maps of the corresponding CD boundaries, we find, in

fact, that on average only around 10% of counties are divided into more than one district.³¹ Consistent with the criteria according to which the population must be equally divided across the different districts assigned to a given state, we also note that counties divided into multiple CDs are often the most populated ones, e.g., Los Angeles County is divided into 17/18 districts. This constitutes the dataset of what we refer to as, for simplicity, *local agencies*, which is the one we use in our main analysis.

A final concern relates to the possibility that our panel of agencies might not be balanced, in other words, that more agencies might start to report their data to the FBI after the election of a woman. If reporting is orthogonal to the agency being located in a CD where there is a mixed-gender race leading to election of a candidate of a specific gender, it should not affect our estimates of the effect of female representation on gender-related violence. In Table B1, we test this hypothesis by estimating the effect of the election of a woman on the number of agencies reporting non-missing data, using our preferred specification. We consider the number of agencies reporting SHR data in Column (1), and its logs in Column (2). In Columns (3) and (4), we replicate the exercise considering those reporting OKCA data. In all cases, we take the average between the first and second year in office. We detect no significant effect on reporting, making us more confident that our panel of agencies is indeed balanced.

TABLE B1 Number of Agencies Reporting Non-Missing Data

	SHR		OKCA	
	Levels	Logs	Levels	Logs
	(1)	(2)	(3)	(4)
Coefficient	1.767	-.087	1.406	-.12
SE	5.431	.151	5.514	.15
P-value	.663	.565	.727	.375
h	17.477	25.061	17.032	22.341
N	382	573	385	531
Average Outcome	26.226	2.913	26.46	2.929
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. MSE-optimal bandwidths and a triangular kernel are used. Dependent variables are number of agencies reporting non-missing data (2-year average), expressed either in levels or logs. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, and unemployment rate. Standard errors are clustered at the district level. Robust p-values are reported. *Avg Outcome* is computed across all districts with mixed-gender races.

Appendix B.4 | FBI Data Geography

The imputation of the correct CD for a given agency is, of course, another crucial issue for our study. Agencies might change their location, and redistricting might occur over the years, changing the CD of reference. While the FBI provides additional datasets with some geographical information – namely Crosswalk files, Census of Local and State Law Enforcement Agencies, and Directories of Law Enforcement Agencies – these rarely include data on the CD associated with a given agency, or its geographical coordinates. More importantly, these data only partially cover the period we are considering, which would force us to make important approximations. For example, the first Census took place in

³¹Specifically, 16,269 out of 144,182 observations are divided across more than one district (yearly share: mean 0.112 (11%), min 0.042, max 0.144). From these computations, we exclude counties that overlap with a CD by less than 10 km² to eliminate possible mistakes due to discrepancies in the shapefiles.

1986 and has since been taken every 4 years; however, the first Crosswalk file only dates back to 2000.

For this reason, we use the OKCA files to recover the geographical location for each law enforcement agency included in the files. Specifically, we use their reported address to retrieve their spatial coordinates through the *OpenCage* package by [Zeigermann \(2018\)](#). Overlapping our yearly agency maps with maps of the CD boundaries by [Lewis et al. \(2013\)](#) for the 92nd–114th Congressional Sessions, and by the Census Bureau for the 115th Session of Congress, we are able to retrieve the CD in which each agency was located in a given year.

The performance of the package is, inevitably, only as good as the addresses provided, the quality of which varies across the years. For example, we find spelling mistakes or agencies reporting only the name of a city (or, at times, just a county). In the latter case, the program will return the coordinates of the center of the city (county). To minimize these occurrences, we perform several checks and corrections of our original address dataset. We also drop from the analysis those observations that could not be correctly located (e.g., an agency of the state of New York whose address was listed as New Jersey, or whose address was impossible to find). Nevertheless, this remains a small sample, mostly of non-local agencies.

As a check of our strategy, we compare our results with the Crosswalk files for 2012, which include information related to the agencies' CD.³² We merge this information with our geographical data for 2012, obtaining a dataset of 22,030 agencies participating in the UCR, of which 3,087 are listed under more than one CD. Comparing the CD that we associated with (any of) those recorded in the Crosswalk, we find that we have associated our agency with a different district in 1,393 cases (6.32%). If we further exclude state-wide agencies, we obtain a dataset of 18,565 observations; in only 430 cases (2.32%), the CD we have listed is not the same as the CD listed in the Crosswalk. The fact that most of the mistakes are found in non-local agencies – which can be explained by the fact that at times they are all located in a given city (e.g., the state capital), even when responsible for a different area – reinforces the validity of the restriction adopted in our main analysis.

³²The data claim to be reporting CDs as of 2010; however, we believe this to be a simple mistake in the FBI data, which actually refer to the CD in 2012. For instance, among the values listed in the FBI's CDs, we find the 34th CD of Texas. However, this district saw its first election in 2012, as it was created as a result of the 2010 Census (see the website of the Congressman representing the district [here](#), as well as the Wikipedia page for the district [here](#)). Therefore, this discrepancy could just be due to the time references adopted.

TABLE B2 Year and Congress-CD

Year	Congress	Duration of Session	Election
1970	92nd	Jan 3, 1971 to Jan 3, 1973	Nov 3, 1970
1971	92nd		
1972	93rd	Jan 3, 1973 to Jan 3, 1975	Nov 7, 1972
1973	93rd		
1974	94th	Jan 3, 1975 to Jan 3, 1977	Nov 5, 1974
1975	94rd		
1976	95th	Jan 3, 1977 to Jan 3, 1979	Nov 2, 1976
1977	95th		
1978	96th	Jan 3, 1979 to Jan 3, 1981	Nov 7, 1978
1979	96th		
1980	97th	Jan 3, 1981 to Jan 3, 1983	Nov 4, 1980
1981	97th		
1982	98th	Jan 3, 1983 to Jan 3, 1985	Nov 2, 1982
1983	98th		
1984	99th	Jan 3, 1985 to Jan 3, 1987	Nov 6, 1984
1985	99th		
1986	100th	Jan 3, 1987 to Jan 22, 1989	Nov 4, 1986
1987	100th		
1988	101th	Jan 3, 1989 to Jan 28, 1991	Nov 8, 1988
1989	101th		
1990	102nd	Jan 3, 1991 to Jan 9, 1993	Nov 6, 1990
1991	102nd		
1992	103rd	Jan 3, 1993 to Jan 3, 1995	Nov 3, 1992
1993	103rd		

Year	Congress	Duration of Session	Election
1994	104th	Jan 3, 1995 to Jan 3, 1997	Nov 4, 1994
1995	104th		
1996	105th	Jan 3, 1997 to Jan 3, 1999	Nov 5, 1996
1997	105th		
1998	106th	Jan 3, 1999 to Jan 3, 2001	Nov 3, 1998
1999	106th		
2000	107th	Jan 3, 2001 to Jan 3, 2003	Nov 7, 2000
2001	107th		
2002	108th	Jan 3, 2003 to Jan 3, 2005	Nov 5, 2002
2003	108th		
2004	109th	Jan 3, 2005 to Jan 3, 2007	Nov 2, 2004
2005	109th		
2006	110th	Jan 3, 2007 to Jan 3, 2009	Nov 7, 2006
2007	110th		
2008	111th	Jan 3, 2009 to Jan 3, 2011	Nov 4, 2008
2009	111th		
2010	112th	Jan 3, 2011 to Jan 3, 2013	Nov 2, 2010
2011	112th		
2012	113th	Jan 3, 2013 to Jan 3, 2015	Nov 6, 2012
2013	113th		
2014	114th	Jan 3, 2015 to Jan, 3 2017	Nov 4, 2014
2015	114th		
2016	115th	Jan, 3 2017 to Jan 3, 2019	Nov 8, 2016
2017	115th		

Source: *Wikipedia*. The table indicates the years associated to a given Session of Congress. For each Session of Congress, it additionally specifies the election of reference, as well as the start and end date of the Session.

Appendix C | RD DESIGN – VALIDITY TESTS

In the RD design, identification rests on the assumption that, around the threshold $MV = 0$, randomness determines the election result. One way to support this assumption is to show that there are no significant differences around that threshold among all relevant factors that might affect the probability of electing either one of the candidates. This assumption makes it possible to compare two districts represented by Congresspersons of different genders. To test for this assumption, we use data from [Foster-Molina \(2017\)](#) to look for the presence of discontinuities in a wide range of district characteristics, using the specification in equation (1) in Section 3 for each attribute. Specifically, we consider the percentage of people defined as older adults in the district (%Old), the percentage of Black (%Black) and Hispanic (%Hispanic) population, the percentage of people with a high-school degree (%HS) and with a Bachelor's degree (%BA), and the percentage of people who are unemployed (%Unem), as well as the median family income (MedInc), the Gini index (Gini), and the population size (Population).

Results are reported in Panel A of Table C1. As we can see, none of the coefficients are statistically significant, supporting the comparability of CDs around the cutoff. To control for possible differences related to the geographical location of the district, we also check for differences in the macro-region in which the race took place. Results are reported in Panel B of Table C1. Again, the probability of electing either one of the candidates is not significantly different across macro-regions.

Another important assumption on which the RD design hinges is that candidates are not able to control the probability of falling on either side of the threshold, which, in our setting, translates into being able to manipulate the outcome of an election. In other words, the validity of our identification strategy relies on the continuity of the female margin of votes around this threshold. Therefore, we first check for possible spikes in the distribution of MV by plotting its frequency in Figure C1. Then, more formally, we implement a McCrary test ([McCrary, 2008](#)) in Figure C2. We also perform an additional discontinuity test, as suggested in [Cattaneo et al. \(2018b\)](#) and [Cattaneo et al. \(2019\)](#), reported in Figure C3. Taken together, we fail to reject the null hypothesis of the continuity of that distribution.

As mentioned in Section 3, the results from [Eggers et al. \(2015\)](#), documenting the presence of an incumbent advantage in close U.S. House elections, may raise concerns about the validity of the RD design in electoral settings. To address this concern, in Table C2 we test for the presence of an incumbency advantage in our sample following [Eggers et al. \(2015\)](#). Specifically, we define a candidate as an incumbent if they are running for the party holding the seat, and test for a discontinuity in the probability of being an incumbent at the vote margin threshold, focusing on mixed-gender races. The results show no evidence of an incumbency advantage. If anything, incumbents appear less likely to win close elections.³³

³³We also replicate our main analysis restricting the sample to mixed-gender races without incumbents. While this naturally reduces statistical power, the estimated coefficients, in particular on rape, remain similar to our main estimates both in magnitude and sign (results available upon request).

TABLE C1 Balance Checks: District Characteristics

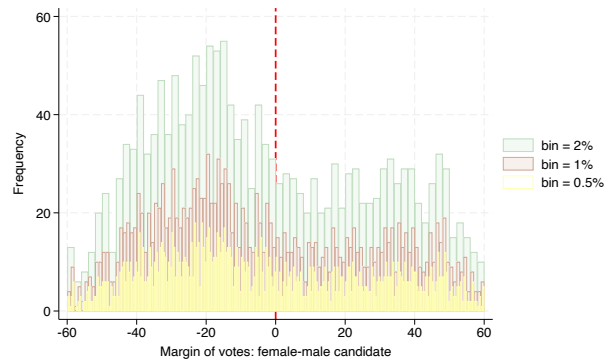
Panel A: district characteristics									
	%Old	%Black	%Hisp	%HighSchool	%BA	%Unemp	Median Inc	Gini	Population
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Coeff	-0.364	-0.844	0.238	-0.347	0.237	0.077	-0.558	-0.003	-0.079
p-value	0.542	0.521	0.845	0.808	0.799	0.788	0.9	0.569	0.302
h	25	20	22	27	27	21	29	26	24
N	769	658	715	896	878	685	866	875	808
Panel B: macro-region									
	Midwest	Northeast	South	West					
	(1)	(2)	(3)	(4)					
Coeff	-0.125	0.035	-0.011	0.094					
p-value	0.133	0.529	0.898	0.438					
h	28	23	28	25					
N	916	790	938	839					

Note: Results are from a local linear polynomial specification. MSE-optimal bandwidths and a triangular kernel are used. Robust p-values are reported. N denotes the total number of observations. Time fixed effects are included, and standard errors are clustered at the district level. Population is expressed in 100,000 residents, and median income in thousands of dollars.

TABLE C2 Testing for incumbency advantage

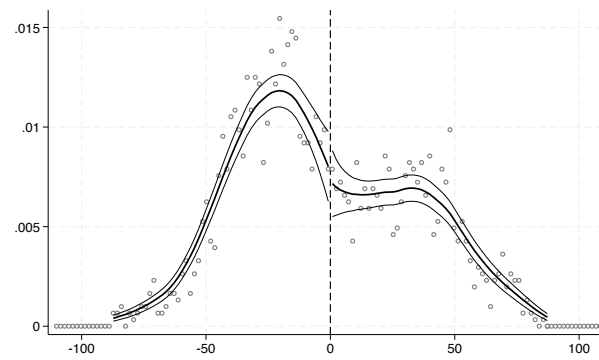
	Incumbent
	(1)
Coefficient	-.133
SE	.087
P-value	.065
h	.119
N	341
Average Outcome	.444
Time FE	Yes
State FE	Yes
Controls	Yes

Note: Results are from a local linear polynomial specification. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, percentage of Black population, percentage of Hispanic population, unemployment rate. Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported.



Frequency plot. The bars indicate the (absolute) frequency of the races within a given bin, computed according to three different bandwidths: 2 percent, 1 percent, and 0.5 percent.

FIGURE C1 Frequency of female margin of votes, MV



McCrary plot: Discontinuity estimate, point estimate: -0.07 (SE 0.177).

FIGURE C2 McCrary density plot

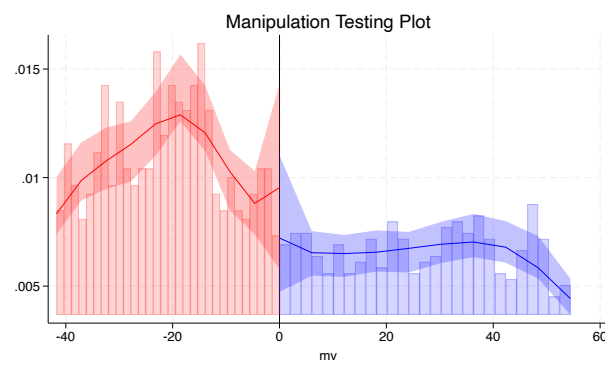


FIGURE C3 RD-density plot

Appendix D | ADDITIONAL TABLES AND FIGURES

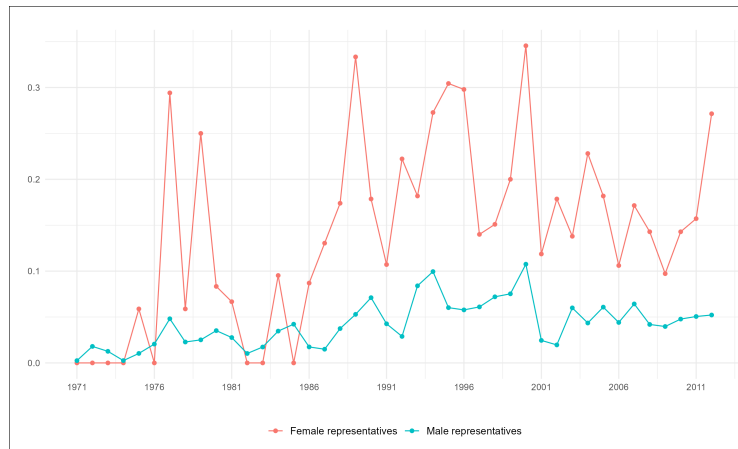


FIGURE D1 Districts and Representatives addressing VAW, by gender

Note: The figure shows the proportion of districts whose Representative addressed VAW in a given year, from 1971 to 2012. Districts are grouped according to the gender of the Representative.

TABLE D1 The Effect of Female Representation on GBV – Year 1 and Year 2

Panel A: First year				
	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.032	-.052	-.041	-.079
SE	.205	.075	.064	.053
P-value	.79	.613	.652	.148
h	13.869	22.519	18.59	19.141
N	313	640	500	504
Average Outcome	37.371	1.459	.983	.573
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Panel B: Second year				
	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.329	.061	.072	.001
SE	.181	.1	.085	.064
P-value	.031	.513	.428	.985
h	17.327	14.884	10.83	15.525
N	296	294	196	294
Average Outcome	36.665	1.414	.97	.568
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as logs of number of victims per 100,000 residents. Values refer to the first year (Panel A) and to the second year (Panel B) in office. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D2 The Effect of Female Representation on GBV – By Female Population

Overall term				
	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.385	.15	.147	.033
SE	.197	.169	.147	.121
P-value	.022	.255	.205	.695
h	15.782	14.258	14.477	19.858
N	258	262	264	372
Average Outcome	196.884	7.491	5.093	2.991
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as logs of number of victims per 100,000 women (of voting age). Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D3 The Effect of Female Representation on GBV – Levels

Overall term				
	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	27.68	-.008	.16	-.007
SE	13.717	.558	.33	.223
P-value	.023	.976	.543	.969
h	11.657	12.44	12.269	13.655
N	184	223	216	237
Average Outcome	72.385	2.776	1.885	1.104
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as number of victims per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables and is computed across all districts with mixed-gender races.

TABLE D4 The Effect of Female Representation on GBV – Same Bandwidths

Overall term												
	Rapes			Women Killed			Women Killed by Men			Femicides		
Bandwidth:	16.5	15.0	18.0	16.5	15.0	18.0	16.5	15.0	18.0	16.5	15.0	18.0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Coefficient	.361	.388	.338	.086	.083	.085	.076	.081	.067	.03	.04	.02
SE	.257	.26	.254	.174	.177	.169	.136	.138	.133	.12	.124	.117
P-value	.061	.068	.086	.583	.623	.583	.344	.361	.365	.573	.603	.637
h	16.5	15	18	16.5	15	18	16.5	15	18	16.5	15	18
N	273	242	301	314	281	348	310	276	344	297	264	330
Average Outcome	72.385	72.385	72.385	2.776	2.776	2.776	1.885	1.885	1.885	1.104	1.104	1.104
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as logs of number of victims per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. Bandwidths are manually selected to be 16.5, 15, and 18, and a triangular kernel is used. Robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D5 The Effect of Female Representation on GBV – Second-Order Polynomial

Overall term				
	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.457	.11	.116	.026
SE	.275	.178	.144	.111
P-value	.061	.527	.334	.748
h	20.614	21.886	21.596	26.366
N	350	439	433	496
Average Outcome	72.385	2.776	1.885	1.104
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local quadratic polynomial specification. Dependent variables are expressed as logs of number of offenses cleared per 100,000 residents. Values refer to the whole term in office. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D6 The Effect of Female Representation on GBV – Extended Sample, Including Non-Local Agencies

Overall term				
	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.335	.082	.084	.04
SE	.187	.135	.105	.09
P-value	.035	.436	.308	.546
h	18.261	14.693	14.504	15.091
N	303	275	266	267
Average Outcome	72.385	2.776	1.885	1.104
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. The sample includes *all* law enforcement agencies with complete 12-month reporting. Dependent variables are expressed as logs of number of victims per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D7 The Effect of Female Representation on GBV – Extended Sample, Local Agencies with Non-Complete Reporting

	Rapes	Women Killed	Women Killed by Men	Femicides
	(1)	(2)	(3)	(4)
Coefficient	.238	.112	.038	-.011
SE	.169	.133	.111	.093
P-value	.093	.334	.616	.979
h	22.096	14.589	14.416	14.526
N	378	280	270	265
Average Outcome	72.385	2.776	1.885	1.104
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. The sample includes *all local* law enforcement agencies, including those without complete 12-month reporting. Dependent variables are expressed as logs of number of victims per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D8 The Effect of Female Representation on Other Crimes

Overall term							
	Violent Crimes			Non-Violent Crimes			
	Murders	M killing M	Aggr ass	Robbery	Burglary	Larceny	Mv Theft
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Coefficient	.172	.114	.185	.093	.047	.095	.15
SE	.172	.161	.204	.234	.158	.131	.186
P-value	.209	.312	.269	.56	.622	.356	.35
h	19.5	12.697	16.385	15.352	19.998	20.467	22.235
N	436	227	346	326	450	461	504
Average Outcome	14.001	6.183	617.424	372.841	1914.284	5276.459	920.429
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as logs of number of victims (or offenses) per 100,000 residents. Values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races. *M killing M* is the number of men killed by men.

TABLE D9 The Effect of Female Representation on Clearances – By Population

Overall term				
	Rape	W killed	M killed	Murders
	Cleared	Solved	Solved	Solved
	(1)	(2)	(3)	(4)
Coefficient	.335	.048	.284	.264
SE	.175	.114	.182	.189
P-value	.03	.586	.048	.078
h	15.433	15.212	12.21	12.837
N	251	286	224	226
Average Outcome	33.258	2.125	7.099	9.292
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Dependent variables are expressed as logs of number of offenses cleared per 100,000 residents. Values refer to the whole term in office. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA or SHR). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D10 Speeches on VAW and Recorded Rapes - Robustness

	Lagged model	Lagged model (excluding bill-related speeches)
	(1)	(2)
Proportion of GBV speeches (lag)	−0.234 (1.095)	−2.542 (1.688)
Female representative (lag) * Proportion of GBV speeches (lag)	4.437** (2.066)	7.299*** (2.522)
Female representative (lag)	0.072 (0.064)	0.085 (0.064)
Average Outcome	33.495	33.495
Num. Obs.	13521	13521
Time FE	Yes	Yes
State FE	Yes	Yes
Controls	Yes	Yes

Note: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$ Dependent variables are expressed as the logs of the number of victims of rapes per 100,000 residents. Proportion of relevant speeches is the log of the proportion of speeches in a given district and year that addressed VAW/sexual violence. Relevant speeches were defined in an alternative way to that in the main table, as discussed in Section I.D. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for gender of the winning candidate, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (OKCA). Standard errors are clustered at the district level. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D11 The Effect of Female Representation on GBV-related speeches in Congress

	Full data		Excluding GBV bills	
	Proportion of GBV speeches	Proportion of GBV speeches (definition 2)	Proportion of GBV speeches	Proportion of GBV speeches (definition 2)
	(1)	(2)	(3)	(4)
Coefficient	.003	.008	.004	.005
SE	.002	.005	.002	.003
P_value	.138	.043	.044	.034
h	22.073	17.624	15.099	13.606
N	538	427	356	312
Avg	.0019	.0043	.0006	.0015
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. The dependent variable is the proportion of GBV speeches in district d over congress session C , thus values refer to the overall term. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D , percentage of Black population, percentage of Hispanic population, and unemployment rate. Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome across all districts with mixed-gender races.

TABLE D12 The Effect of Female Representation on Police Composition

Overall term									
	Both Genders			Males			Females		
	Total	Officers	Civilians	Total	Officers	Civilians	Total	Officers	Civilians
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Coeff	.049	.073	.088	.051	.046	.111	.065	.067	.062
p-value	.572	.458	.545	.558	.584	.535	.534	.533	.622
h	23	20	18	23	22	18	20	20	18
N	543	463	402	543	526	422	460	460	406
Average Outcome	309.71	224.235	85.475	231.789	200.834	30.955	78.131	23.549	54.582
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

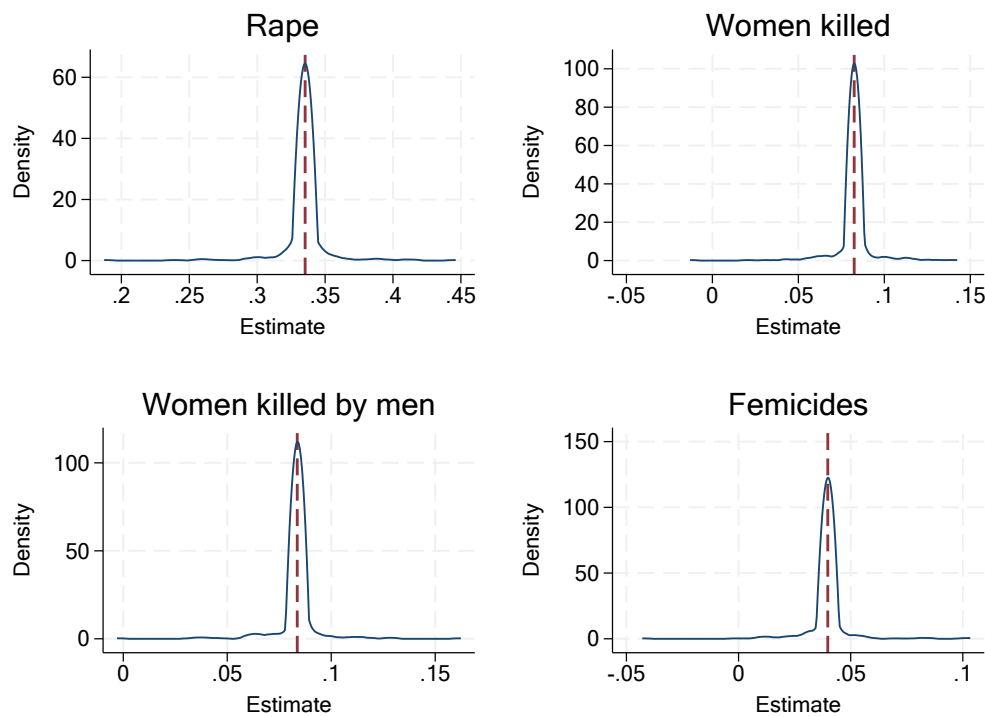
Note: Results are from a local linear polynomial specification. Dependent variables are normalized per 100,000 residents and expressed in logs. Values refer to the whole term in office. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D , percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (Law Enforcement Officers Killed and Assaulted, LEOKA). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables (per 100,000 inhabitants) and is computed across all districts with mixed-gender races.

TABLE D13 The Effect of Female Representation on Police Composition - Ratios

Overall term						
	Gender Ratio			Female Share		
	All employees (1)	Officers (2)	Civilians (3)	All employees (4)	Officers (5)	Civilians (6)
Coefficient	-.003	.005	.064	-.001	.004	-.006
SE	.01	.007	.207	.006	.005	.017
P-value	.732	.379	.588	.858	.401	.908
h	14	16	20	14	16	16
N	293	350	472	306	354	367
Average Outcome	.315	.098	2.205	.234	.088	.655
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: Results are from a local linear polynomial specification. Values refer to the second year in office. All regressions include a set of controls as well as time and state fixed effects. Controls include an indicator variable for each geographical macro-area, an indicator for Democratic winner D, percentage of Black population, percentage of Hispanic population, unemployment rate, and percentage of agencies reporting non-missing data (Law Enforcement Officers Killed and Assaulted, LEOKA). Standard errors are clustered at the district level. MSE-optimal bandwidths and a triangular kernel are used, and robust p-values are reported. *Average Outcome* is the mean of the outcome variables and is computed across all districts with mixed-gender races.

Leave-One-Out Estimates



Note: For each outcome, the sample was restricted N times, where N equals the number of districts (D) plus the number of years (Y). In each iteration, one district or one year was excluded, equation (1) was re-estimated, and the resulting coefficient was stored. The figure displays the density of these estimates for each outcome. The dotted red line corresponds to the full sample estimate.

FIGURE D2 Density of leave-one-out estimates

Appendix E | OTHER OFFICES: JUDGES AND MORE

As mentioned in Section 5, to assess whether concurrent elections of particular importance in the justice and law enforcement systems could confound the estimated effect of female political representation, we examine four offices: state trial court judges³⁴, U.S. attorneys, district attorneys³⁵, and sheriffs. We focus, in particular, on three features of these offices, namely (i) the selection process, e.g., appointment or election, (ii) their geographical jurisdiction, and (iii) the length of their terms. The discussion below illustrates these features, and explains how these reduce the likelihood that our results are driven by overlapping elections to crime-related offices. In short, compared to the office of U.S. Representative, we find substantial differences, especially with respect to the time in office and area served, which limits these concerns.

Consider state trial court judges, henceforth *judges*, which represent the first level in a state's court system, below the appellate courts and a state's supreme court. With respect to their selection, judges join or remain on the bench in different ways, and methods vary widely across states (Brennan Ctr. for Justice, 2025; Lim et al., 2015). Looking at how judges are selected for their first term, only roughly half of the states (28 states) elect judges to their first full term – either in partisan or nonpartisan elections – while around 40 percent (20 states) rely on gubernatorial or legislative appointments. In a few cases, there is also variation *within* the same state.³⁶ This heterogeneity implies that concurrent elections for Congress and the judiciary are possible in only a subset of states. Where judges are appointed, instead, the decisions typically reflect statewide considerations rather than “local-district” politics, reducing concerns that the same underlying factors may influence both political and judicial representation.

Whatever the method of selection, it is important to note that judicial jurisdictions are generally broader than congressional districts. Lim et al. (2015) note that some small states in New England have just one judicial district covering the whole state, while in Southern and Midwestern states judicial districts tend to cover several counties (three or four); and in some Pacific and Mid-Atlantic states, just one or two counties. The alignment between judicial districts and congressional districts could occur in those states with smaller jurisdictions – like those in the Pacific and Mid-Atlantic regions – since one or more counties may fall within the boundaries of a given congressional district (see also Appendix B.3). However, such instances are rare, and limited to very few states.³⁷

The length of judicial terms further reduces the likelihood of potential confounding. Judges typically serve for 6 years, and never less than 2 years. Therefore, most congressional elections occur when a judge has already been serving his or her term for some years. In some states, terms are effectively permanent: judges are life-tenured in 3 states, and in 8 others they face retention elections, that is a yes-or-no vote with no other candidates

³⁴State trial courts are also called district courts, circuit courts, or superior courts (Lim et al., 2015).

³⁵Depending on the state, district attorneys are also called state attorneys, prosecuting attorneys, county attorneys, or public prosecutors. We use *district attorneys* to refer to any of these five denominations. See the American Civil Liberties Union (ACLU) and the Legal Information Institute (LII) at Cornell University.

³⁶According to the Brennan Ctr. for Justice (2025), 19 states use non-partisan elections, 9 partisan elections, 18 gubernatorial appointments. The remaining states use either a legislative appointment, that is a vote of the state legislature (2 states), or other a mixed system of selection (3 states, e.g., Arizona, where judges can be selected either by gubernatorial appointment or by non-partisan election, depending on the jurisdiction). Similarly, a judge can be re-appointed to subsequent terms in different ways, including elections, appointments, and retention elections, that is a yes-or-no vote with no other candidates appearing on the ballot. For subsequent terms, 8 states use retention elections, 19 non-partisan elections, 7 partisan elections, 5 gubernatorial appointment, and 3 legislative appointment. The remaining 6 use other methods. Data include the District of Columbia. See also Lim et al. (2015).

³⁷Among the 8 states included in these two regions, only 5 elect judges (Brennan Ctr. for Justice, 2025).

appearing on the ballot.³⁸ In such retention elections, incumbents win more than 99% of the time, making the office of judge basically a lifetime appointment (Lim et al., 2015). These institutional features make turnover in the judiciary far less frequent than in the U.S. House, limiting the likelihood of concurrent changes in representation.

Finally, these institutional features vary substantially across states, resulting in a mosaic of selection methods and geographical jurisdictions. For example, two states may both elect judges for their first term, but differ in how they reappoint them. This heterogeneity minimizes the likelihood that our results are driven by a narrow subset of states or election cycles, for which judicial and congressional elections coincide.

U.S. Attorneys are, instead, appointed by the U.S. President and confirmed by the Senate. They serve 4-year terms, or at the President's discretion (see 28 U.S. Code § 541), implying that the term of a given U.S. Attorney spans approximately two congressional terms. They have jurisdiction over federal judicial districts, which are typically larger than congressional districts. To get a sense, consider that there are 93 federal judicial districts, but 435 congressional district in the U.S. (Koteen et al., 2014). Similarly to the case of judges, these institutional features lead us to exclude the possibility of this office being a potential source of confounding.

District attorneys (DAs) serve 4-year terms, are predominantly elected, and may have jurisdiction over either a judicial district or county. Consequently, congressional elections generally occur while a DA is already in office, making concurrent turnover unlikely. In a limited number of cases, however, the elections for a DA and a Representative may overlap, particularly in states where these prosecutors are elected in and serve a county whose boundaries coincide with (or fall within) a congressional district. As of 2001, for example, DAs were appointed in 3 states and elected in 47, and they had jurisdiction over a county only in 26 (Coppolo, 2003). Yet, even in these states, we argue that systematic confounding is unlikely. First, not all DAs are up for re-election at the same time. For example, the American Civil Liberties Union (ACLU) of Oregon notes that in the state “in any given election, at least a third of the District Attorneys are up for re-election.” Second, these contests are rarely competitive. For example, during the 2018 prosecutorial elections, 80% (836) of candidates ran for office without an opponent. Third, white men represent the vast majority of prosecutors – around 70% of them, as of 2019 (Reflective Democracy Campaign, 2019).³⁹ This indicates that these elections are non-competitive and mostly dominated by men. Even when competition occurs, it is typically among male candidates. Taken together, these characteristics make it improbable that the election of a congresswoman would systematically coincide with, or influence, a shift in the gender composition of prosecutorial offices.

Similar arguments apply to the case of sheriffs, who have jurisdiction over a county and are mostly elected. Sheriffs are elected in 46 of the 48 states in which such an office exists.⁴⁰ The length of their term varies across states, but the vast majority serve 4-year terms, and almost always for longer than 2 years.⁴¹ Therefore, even though the geographical overlap between congressional and sheriff elections may occur, the longer term length

³⁸In 2 of these 8 states, judges run for elections for their first term (Brennan Ctr. for Justice, 2025).

³⁹The definition of *prosecutors* used in Reflective Democracy Campaign (2019) is broader than the one we adopt, in that, it includes not only district attorneys, state attorneys, prosecuting attorneys, county attorneys, and public prosecutors, as well as Solicitor Generals and Attorney Generals. However, our original categories cover around 80% of the sample of all elected prosecutors who were in office in 2019.

⁴⁰Alaska does not have a sheriff's office, and Connecticut abolished the office in the early 2000s. Sheriffs are appointed in Rhode Island and Hawaii.

⁴¹As of 2012, sheriffs were elected to a 2-year term in only two states (Arizona and New Hampshire), to a 3-year term in one (New Jersey), and to a 6-year term in one (Massachusetts) Data from the office of the Sheriffs (archived from the original on 19 October 2014, with Wayback Machine). See also the Wikipedia page

of sheriffs mitigates concerns about concurrent turnover. Moreover, these elections are typically non-competitive, and heavily male-dominated. Recent statistics show that around 60% of sheriffs run unopposed, and are overwhelmingly men: in 35 states, less than 5% of sheriffs are women, of which 16 states have none at all. Overall, white men account for around 90% of sheriffs nationwide ([Reflective Democracy Campaign, 2020](#)).

Taken together, these elements go against the hypothesis that our results may be confounded by a concurrent and broader increase in female representation, not only in the political but also within the judicial and law-enforcement systems. Consequently, we are confident that the effects we identify stem from female *political* representation.