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More Equality for Women Does Mean Less War: Descriptive Representation, Legislative Votes, and International Conflict

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Although the pacifying effect of gender equality is said to be a "near-consensus" (Cohen and Karim 2022, 2), the causal mechanisms remain underspecified, and causal identification is weak. I address those shortcomings by providing the first causal evidence regarding the effects of women's legislative representation on the state's foreign military actions. I argue that women legislators affect foreign policies via legislative votes, and hence that the effect depends on whether military deployment requires legislative approval. I test the hypotheses by exploiting as-if random variation in mixed-gender close races and analyze data from 270,553 candidates in 253 legislative elections across 50 countries. The analysis indicates that women's close victories reduced the state's military actions but only with legislative veto power. The analysis of legislative votes also suggests that women legislators, especially those in government parties, deviate from party lines with other legislators and substantially reduced votes for military deployment.

Keywords: Women's Representation, International Conflict, Legislature
JEL classification: F50, F51, J16

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More Equality for Women Does Mean Less War:

Descriptive Representation, Legislative Votes, and International Conflict

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Abstract

Although the pacifying effect of gender equality is said to be a "near-consensus" (Cohen and Karim 2022, 2), the causal mechanisms remain underspecified, and causal identification is weak. I address those shortcomings by providing the first causal evidence regarding the effects of women's legislative representation on the state's foreign military actions. I argue that women legislators affect foreign policies via legislative votes, and hence that the effect depends on whether military deployment requires legislative approval. I test the hypotheses by exploiting as-if random variation in mixed-gender close races and analyze data from 270,553 candidates in 253 legislative elections across 50 countries. The analysis indicates that women's close victories reduced the state's military actions but only with legislative veto power. The analysis of legislative votes also suggests that women legislators, especially those in government parties, deviate from party lines with other legislators and substantially reduced votes for military deployment.

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The pacifying effect of gender equality is said to be "well-established" and a "near-consensus" (Bakken and Buhaug 2021, 4; Schaftenaar 2017, 764; Cohen and Karim 2022, 2). Many cross-national studies have found that gender equality, measured by composite indexes and women's representation in legislatures and executives, is robustly associated with fewer international and intrastate conflicts.[1] Moreover, legislative studies have provided micro-level evidence that women representatives are less supportive of military actions.[2] The gender-peace thesis appears to be another finding that comes closest to an empirical law, following the democratic-peace thesis.

It turns out, however, that those findings stand on flimsy grounds. The causal mechanisms are underspecified, and the causal identification is weak (Cohen and Karim 2022; McDermott 2015). A few studies have also found that the relationships are heterogeneous and even opposite across different dimensions of gender equality (Caprioli and Boyer 2001; Kattelman and Burns 2022). The legislative studies exclusively focus on a single country, raising a question of generalizability to other countries (Bäck and Debus 2019; Itzkovitch-Malka and Friedberg 2018; Lippmann 2022).

I address those shortcomings by providing the first causal evidence regarding the effects of women's legislative representation on international conflict and analyzing the institutional channels through which women legislators affect state's military actions (e.g., war and other

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[1] Best, Shair-Rosenfield, and Wood (2019), Bjarnegård and Melander (2011), Caprioli (2000, 2003, 2005), Caprioli and Boyer (2001), Dahlum and Wing (2020), Koch and Fulton (2011), Melander (2005a), Regan and Paskeviciute (2003), Schaftenaar (2017), Shair-Rosenfield and Wood (2017).

[2] Atkinson and Windett (2019), Bendix and Jeong (2020), Swers (2007), Volden, Wiseman, and Wittmer (2013).

military operations against foreign states).[3] Women's legislative representation is the most widely studied among many dimensions of gender equality and conflict. Borrowing insights from studies about parliaments and war, [4] I argue that women legislators affect military actions through legislative veto power—the legislature's authority of approving or disapproving a government policy via legislative votes.[5] With the legislative veto power, women legislators can vote against military deployment, persuade other legislators, and thus prevent military conflicts.

I substantiate those arguments by exploiting as-if random variation in mixed-gender close races and analyze data from 270,553 candidates in 253 national legislative elections across 50 countries. I code the candidate genders at scale and calculate the proportion of close races won by women. I also address the empirical problems discussed by Marshall (2023). The analysis indicates that closely electing women reduced the government's initiations of military actions, but only when the legislature had veto power over military actions. I also explore the causal mechanisms by analyzing legislative votes over military deployment at a party level, and find that women's close victories substantially decreased votes for military deployment.

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[3] Strictly speaking, I analyze legislators' sex. However, I follow convention and use the term "gender," while acknowledging the conceptual problems (Cohen and Karim 2022).

[4] Choi (2010), Dieterich, Hummel, and Marschall (2015), Mello and Peters (2018), Peters and Wagner (2014), Raunio and Wagner (2017), Wagner (2018).

[5] My definition is narrower than those in previous studies, which include legislature's authority over budget, executive appointment, and deliberative processes (Choi 2010; Dieterich, Hummel, and Marschall 2010; Wagner 2018), or seat shares, voting rules, and the difference in preferences between a government and legislature (Henisz 2000).

These findings elucidate the crucial role of legislatures in gender and peace. This study highlights legislative veto power as one of the critical mechanisms through which women legislators deter governments' military actions. Unlike previous studies (Best, Shair-Rosenfield, and Wood 2019; Koch and Fulton 2011), I combine cross-national and party-level analyses to substantiate the claim and investigate causal mechanisms. Indeed, my empirical findings suggest that women legislators, especially those in government parties, deviate from the party line *with other legislators* and substantially reduced votes for military deployment. This spillover effect can explain why closely elected women, who comprise only a small fraction of legislators, can affect the governments' military actions.

Moreover, I provide a new research design and data for identifying the causal effects of women's representation on a wide variety of outcomes in the social sciences. Women's legislative representation is shown to correlate with various outcomes at a country level, including international trade and agreements,[6] corruption,[7] foreign aid,[8] human rights (Mechkova, Dahlum, and Petrarca 2023; Melander 2005b), welfare policies (Bolzendahl and Brooks 2007; Kittilson 2008; Patton and Fording 2020), environmental protection (Atchison and Down 2019; Mavisakalyan and Tarverdi 2019), and even capital punishment (Moreland and Watson 2016). However, causal identification remains an open question. Several studies use instrumental

[6] Betz, Fortunato, and O'brien (2021; 2023), Imamverdiyeva and Shea (2022), Park and Shin (2023).

[7] Debski et al. (2018), Dollar, Fisman, and Gatti (2001), Esarey and Schwindt-Bayer (2018, 2019), Jha and Sarangi (2018).

[8] Breuning (2001), Hicks, Hicks, and Maldonado (2016), Tusalem (2022), Yoon and Moon (2019).

variables (IVs), but they often acknowledge possible violations of exclusion restriction and include them in robustness checks. [9] Other studies use mixed-gender close races and other natural/randomized experiments at a subnational level within a single country.[10] However, not only are these studies limited in external validity, but also they do not allow me to analyze a central government's decisions (e.g., foreign policies) or the roles of national-level institutions (e.g., legislative veto power). With the extensive gender classification across many countries, the design and data can be used with nearly all outcome variables found in previous studies.

This is not to say that my approach is superior in every aspect. Compared to extant cross-national studies, my approach can be used only in 50 majoritarian democracies in contemporary periods. Moreover, my estimand is limited to the effects of closely electing women under the first-past-the-post (FPTP) system, and is unlikely to be extended to other aspects of gender equality or electoral systems, such as proportional representation (PR) systems and gender quotas.[11] Although this certainly limits the external validity, it also allows me to develop and test a theory in a more

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[9] Dahlum and Wig (2020), Esarey and Schwindt-Bayer (2018, 2019), Jha and Sarangi (2018). For Esarey and Schwindt-Bayer (2019), see the later *Design* section and footnote 25.

[10] Baskaran and Hessami (2018), Bauhr and Charron (2021), Bhalotra, Clots-Figueras, and Iyer (2018), Blair and Schwartz (2023), Brollo and Troiano (2016), Chattopadhyay and Duflo (2004), Clots-Figueras (2011, 2012), Ferreira and Gyourko (2014), Pereira and Fernandez-Vazquez (2023).

[11] For quotas, see Bush (2011), Clayton (2021), and Clayton and Zetterberg (2018).

focused manner. I believe this is beneficial, especially given the difference between democracies and autocracies (Fallon, Swiss, and Viterna 2012) and among electoral systems.[12]

Theory: Women Legislators as Veto Players

While several studies have examined the effects of gender equality by using aggregated indicators (Caprioli 2003; Dahlum and Wig 2020), it turns out that the effects are different and even opposite across different aspects of gender equality, suggesting the need for disaggregating the concept (Cohen and Karim 2022). Indeed, women's representation in leadership and legislative positions are shown to have opposite effects; while women leaders have higher propensities for aggressive foreign policies,[13] women's legislative representation correlates with peace.[14] These contrastive findings are not surprising given the institutional differences; while women leaders are responsible for foreign policies and incentivized to cultivate a hawkish reputation and obtain better foreign policy outcomes (Blair and Schwartz 2023; Reiter and Wolford 2022; Schwartz and Blair 2020), women legislators have no or only veto power over foreign policies. Given the limited institutional power, foreign policy outcomes are unlikely to be attributed to individual legislators, and hence, legislators are more concerned about their own or voters' preferences.[15]

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[12] Aldrich (2020), Luechinger, Schelker, and Schmid (2023), Profeta and Woodhouse (2022), Roberts, Seawright, and Cyr (2013), Salmond (2006), Skorge (2023), Thames (2017).

[13] Caprioli and Boyer (2001), Dube and Harish (2020), Imamverdiyeva and Shea (2022), Post and Sen (2020), Powell and Mukazhanova-Powell (2019), Schramm and Stark (2020).

[14] See footnote 1.

[15] Angevine (2017), Atkinson, Mousavi, and Windett (2023) Bendix and Jeong (2020), Itzkovitch-Malka and Friedberg (2018), Lagassé and Mello (2018).

Although I am agnostic about where women legislators' preferences come from, the literature (and later empirical analysis) consistently shows that women legislators are, on average, more left-leaning and less supportive of military actions than men legislators.[16] This might be due to biological differences (Fukuyama 1998) and socialization (e.g., education; Angevine 2017; Caprioli 2000; Stauffer et al. 2022), but more likely due to the gendered nature of elections (Paxton, Kunovich, and Hughes 2007). Women legislators tend to rely on women voters,[17] who are on average less supportive of military actions.[18] Moreover, voters and party leaders tend to select legislators whose behaviors are congruent with their gender stereotypes.[19] As women are stereotyped to be more cooperative, liberal, and dovish (Butler, Tavits, and Hadzic 2023; Dolan 2010; Lawless 2004), women legislators are incentivized to follow the stereotype for obtaining nominations and votes. Thus, although determining the origins of women legislators' preferences is beyond the scope of this paper, I can infer that women legislators are, on average, less supportive of military actions than men legislators (later, I empirically assess this assumption).

[16] This does not mean that *all* women legislators oppose war, or that they are less supportive of war than *ordinary women*.

[17] Bendix and Jeong (2020), Lloren (2015), MacDonald and O'Brien (2011).

[18] Brooks and Valentino (2011), Eichenberg (2003), (2016), Eichenberg and Stoll (2012), Lizotte (2019), Nincic and Nincic (2002).

[19] Atkinson and Windett (2019), Bäck and Debus (2019), Blair and Schwartz (2023), Cassese and Holman (2018), Dolan (2010).

Hypotheses: Institutional Channels

However, this does not mean that women legislators can always influence the state's military actions (Best, Shair-Rosenfield, and Wood 2019). I argue that the effect depends on whether a military action requires legislative votes (*legislative veto power*). With legislative veto power, women legislators can vote against military actions.[20] Moreover, they can alter the voting behaviors of other legislators. Even when women legislators comprise only a small fraction of legislators (and the fraction of closely elected women is even smaller), they can persuade and negotiate with other legislators to build coalitions. This spillover effect is important as women legislators are, on average, more competent than men legislators; because women are electorally disadvantaged, only competent women can defeat men in (close) elections (Ashworth, Berry, and Bueno de Mesquita 2023). This implies that they can potentially be better at coalition-building than men legislators (Volden, Wiseman, and Wittmer 2013).

Women legislators might also affect military actions via other institutional channels, such as their influence over the military budget and executive appointment (Dieterich, Hummel, and Marschall 2010). However, I consider that the effects through those channels are limited. The military budget is usually decided in defense committees, in which women are under-represented (Angevine 2017; Heath, Schwindt-Bayer, and Taylor-Robinson 2005). As a plenary session usually calls votes for an entire budget, many women legislators cannot oppose military actions without voting against the entire budget. Similarly, it is often difficult and disproportionate to

[20] Although legislative veto is endogenous to political and historical backgrounds of countries, it rarely changes in the contemporary era; only India experienced a change in my sample. This implies the weak exogeneity of the institutional variable. See footnote 5 about the definition.

dismiss executives solely on the fact that they plan to initiate (not necessarily lose) military conflicts; even when women legislators oppose military actions, they may not prefer executive turnover. They may support the incumbent in other policy areas, and the votes for nonconfidence can be more heavily punished than policy-specific defections. Thus, without denying the potential roles of the alternative channels, I consider the legislative veto power as the primary channel.[21]

With the legislative veto power, women legislators can prevent the government's military actions either by having the legislature reject the bill or scrapping the bill even before legislative votes. An optimistic government may bring the matter to a legislature, which, in turn, might reject the proposal. More realistically,[22] a government can foresee such rejection and thus refrain from submitting or implementing the military plan. Agenda setters (e.g., party leaders) can also dismiss bills that are unlikely to be passed. In all cases, women's representation reduces the government's military actions. Hence, I posit the following:

H1. Women's legislative representation reduces the government's military actions against foreign states. However, this effect exists only when a legislature has veto power over military actions.

Moreover, I expect that the effect differs for women's representation in government and opposition parties.[23] As military deployment is usually issued by a government, government

[21] Later, I empirically assess these alternative mechanisms.

[22] Indeed, 96.94% of military deployments are approved in my sample of legislative votes.

[23] I consider the government-opposition division as a key difference, while not denying the potential roles of left-right ideologies. Due to data limitation, I cannot analyze the roles of ideological positions in the cross-national analysis. In the party-vote analysis, however, I conduct

parties tend to support it, while opposition parties tend to oppose it (Wagner et al. 2018). Thus, even though women legislators are more party-disciplined (Clayton and Zetterberg 2021), women legislators in government parties have incentives to deviate from the party line and vote against military action. By contrast, opposition legislators, regardless of their gender, are less likely to vote for military deployment. If opposition parties would decide to support a government's military action, women legislators could deviate from the party line. However, such a case is less common. Thus, I expect that H1 is driven by the defection of women legislators in government parties. Consequently, I posit the following:

> H2. Women's legislative representation in government parties has a larger effect on the government's military actions than their representation in opposition parties.

The null hypothesis is that women's legislative representation does not affect military actions regardless of legislative veto or parties, possibly due to party discipline (Clayton and Zetterberg 2021) or the limited importance of foreign policies in elections. Another alternative hypothesis is that women's legislative representation increases military actions with legislative veto power. Women legislators might approve military actions for signaling their hawkish stances to their constituencies and colleagues, thus overcoming gender stereotypes (Swers 2007).

Design: Mixed-gender Close Races

However, testing the hypotheses raises challenges; women's representation is endogenous to confounders and military actions themselves. Not only is women's representation affected by

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an analysis and do not find clear differences due to party ideologies (see a later section about additional analyses).

observable factors, but it also depends on hardly observable time-varying factors, such as electoral prospects (Weeks et al. 2023), party centralization (Aldrich 2020), gender ideology (Paxton and Kunovich 2003), and international gender norms (Paxton, Hughes, and Green 2006), all of which can also affect the state's use of military forces. Even worse, military actions or even the *anticipation* of military actions affect gendered mobilization and women's representation, suggesting reverse causality.[24] The endogeneity casts doubt on the validity of commonly used approaches, such as difference-in-differences, fixed or random effects, and lagged outcomes and predictors.

Neither do the instrumental variables nor single-country studies provide solutions. Authors often acknowledge that the exclusion restriction—a core assumption of IV designs—is questionable. It is difficult to claim that the historical plow use (Dahlum and Wig 2020), years from suffrage (Jha and Sarangi 2018), gender ratios of secondary school enrollment and labor force (Esarey and Schwindt-Bayer 2019) are randomly assigned, free from time-varying confounders, and affect the military actions only through their effects on women's legislative representation. For example, the instrumental variables can affect women's representation in other positions (e.g., executives and bureaucracy), which in turn can affect military actions and other outcomes.[25]

[24] Agerberg and Kreft (2020), Bakken and Buhaug (2021), Hadzic and Tavits (2023), Kang and Kim (2020), Schroeder (2017), Webster, Chen, and Beardsley (2019).

[25] Although Esarey and Schwindt-Bayer (2019) claim that the design is valid as far as "[w]omen's representation in parliament may be considered as a measure of female leadership participation overall" (p.1734), the design is not valid unless women's representation in legislatures and other positions has the same average effects. Otherwise, women's legislative representation is a bad

Single-country studies with mixed-gender close races are promising,[26] but it is not immediately clear how to apply the designs to cross-national analysis. Although previous studies use close electoral races between women and men candidates and resulting as-if random variation in women's representation, they only examine subnational elections in a single country; none have provided cross-national data about candidates' genders. However, without knowing the candidates' genders, I cannot define mixed-gender close races or use the natural experiment.

Classification of Candidate Gender at Scale

I address those problems by classifying candidate gender at scale and thus extending the natural experiment to cross-national studies. The main data source is the Constituency-Level Elections Archive (CLEA; Kollman et al. 2019), which contains constituency-level information on elections worldwide and, importantly, full names of candidates. Consistent with the literature,[27] I retrieved the data of lower or unicameral chambers. For simplicity and to also ensure comparability, I subset

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proxy for women's overall representation. In my case, this assumption does not hold (Caprioli and Boyer 2001). Moreover, even though Esarey and Schwindt-Bayer (2019) admit the endogeneity of the IVs and use fixed effects (p.1720), the IVs become too weak, and the results do not hold with the fixed effects (p.1728). Finally, the overidentification tests cannot "support" (p.1725) or "accept" (p.1729) the validity of the IVs (Greene 2011), or even reject it unless the treatment effect is constant across units (Angrist and Pischke 2009).

[26] See footnote 10.

[27] See footnote 1.

the data to independent democracies with FPTP systems or mixed systems involving FPTP.[28] FPTP systems are the most candidate-oriented with less party discipline and thus constitute the "most likely case" for testing the theory. Finally, a few elections were also dropped due to missing data.[29] This resulted in 270,553 unique candidates from 253 elections across 50 countries.

I then used the candidates' names to classify their gender. Because it is difficult to manually code 270,553 names, I used gender detection tools—Gender API and Namsor.[30] Gender API is based on fuzzy string match (Perl 2023), and it matches names to the gender information of over 6 million names in 190 countries, providing the proportions of women and men in the matched records. Gender API is shown to be the most accurate among gender detection tools; the accuracy is 98.53% and 97.89% in two multi-cultural samples (Sebo 2021). A potential drawback is that Gender API does not classify unconventional names, resulting in a relatively high number of non-classifications: 0.34% and 0.66% in Sebo (2021). Therefore, I supplemented it with Namsor—a statistical learning method based on over 7 million names globally (2023). While Namsor has slightly lower accuracy (97.98% and 96.95% in Sebo 2021), it performs well even with

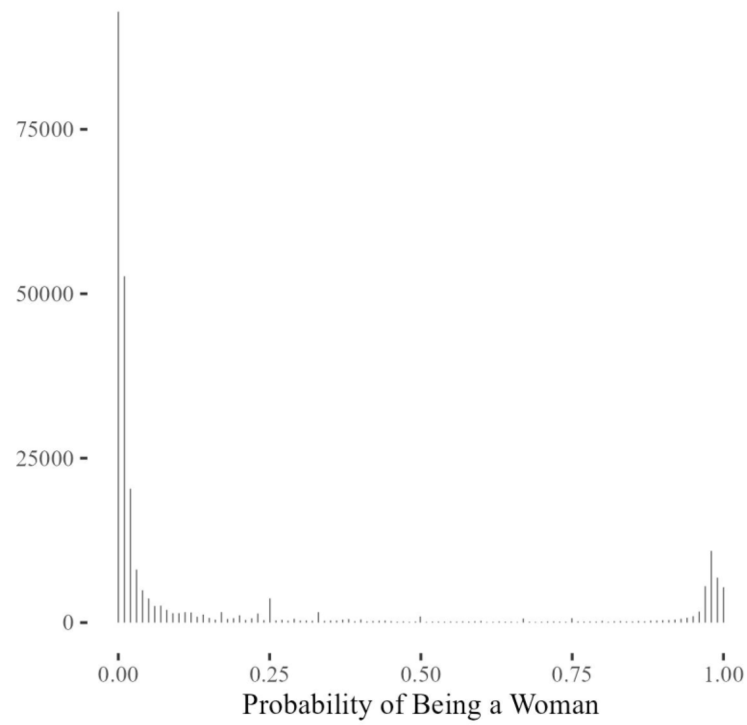
[28] The data about independence, democracy, and electoral systems are derived from Bormann and Golder (2013). In principle, it is possible to apply similar designs to multi-member districts and PR systems (Luechinger, Schelker, and Schmid 2023). However, the substantive meanings and measurements of "close victories" under those systems differ from those under FPTP system. For mixed systems, I only use the constituencies under FPTP system.

[29] See Appendix A1. Uncontested and suspected races are also dropped. Japanese names are translated to roman characters with a Python module, cutlet (McCann 2023).

[30] Chatbots such as ChatGPT are not good at this type of context-specific coding exercises.

unconventional names. Thus, I first applied Gender API and then used Namsor for non-classified names. Figure 1 shows the predicted probabilities of being women for all candidates in the sample. The concentration at 0 and 1 suggests that the classifiers confidently assigned gender.[31]

Figure 1. Histogram of Predicted Probabilities of Candidate Gender



NOTE: The histogram shows the predicted probabilities of candidate gender.

I validated the classification by randomly sampling and manually coding 100 candidates in mixed-gender close races. I was able to code the genders of 92 candidates, among which the

[31] In Appendix A2, I show the confidence of the classifications by country. Later, I also check the robustness by excluding countries of less confident gender classifications.

genders of 83 candidates (90.22%) were identical to the predicted genders.[32] The accuracy was somewhat lower than those in Sebo (2021), due to imprecise names in the CLEA.[33] With the caveat, however, the classification is sufficiently accurate for statistical analysis.[34]

I then calculated the victory margins of women candidates in 30,191 constituencies of mixed-gender races (50.01%).[35] Based on automatic bandwidth selection, I chose the optimal bandwidth and define a race as close if the victory margin is less than 2.60 percentage points (Cattaneo, Jansson, and Ma 2020).[36] There were 1,159 constituencies within the optimal bandwidth. Finally, because the outcome variable—military actions—can be defined only at a country level, I aggregated the constituency-level data to each election.[37] I calculated the proportion of women's close victories R_i for each election i (the number of women's close victories divided by the number of mixed-gender close races).[38] As far as the results of close races

[32] Among the 92 candidates, 37 and 55 are women and men, respectively. The accuracy is $36/37 = 97.30\%$ and $47/55 = 85.45\%$ for women and men, respectively. The F1 score is 0.89 and 0.91 for women and men, respectively.

[33] Several names are only available in non-Latin characters (e.g., Thais and Mongolians).

[34] As a baseline, the accuracies of text analyses ranged from 60 to 90% (Terechshenko et al. 2020).

[35] The vote share of a top woman candidate minus the vote share of a top men candidate.

[36] See Appendix A3 for density tests. I later conduct robustness checks with different bandwidth. I used the density-based automatic bandwidth selection as the outcome variable was available only at a national-level and thus the conventional bandwidth selection could not be used.

[37] Nellis, Weaver, and Rosenzweig (2016) used similar aggregation in the case of India.

[38] I assigned a missing value if there was no close race. See the later discussion about weights.

are randomly assigned, the values of R_i are also randomly assigned, allowing me to identify the causality. For example, parties may have been informed of which constituencies were likely to be closely fought and strategically assigned women candidates. However, this does not bias the estimates as far as the *results* of the close races are randomly assigned.

Characterizing the Sample, Treatment, and Design

Importantly, the estimand of this study differs from the effect of electing women in non-close races or the effect of legislators' gender. First, as widely acknowledged, my design—an aggregated version of the regression discontinuity design (RDD)—provides the estimate that is local to close races (Angrist and Pischke 2009). I believe that the local effect is important, because the close races are the most "variable" component of women's legislative representation. It is easier to change the results of close races than those of decisive races; moving 2.6 percentage points of vote shares is sufficient for flipping the results. Close races are also relevant to my theory, which emphasizes the electoral competition and incentives of legislators.

Table I compares the characteristics of women candidates in close and non-close races (*sample characteristics*). By linking candidates' parties to Herrmann and Döring (2023) and V-Party (Lindberg et al. 2022) datasets, I measured the ideological positions (left-right, gender equality, and anti-violence) and electoral strength (local platform, affiliated organizations, internal cohesion, and funding resources) of the candidates' parties.[39] Unfortunately, the data were available only for 26.16% to 34.21% of the candidates.[40]

[39] The datasets are linked via Party Facts (Döring and Regel 2019).

[40] Independent candidates and minor parties are not available in Party Facts.

Table I indicates that closely-elected women are less supportive of violence and electorally stronger than other women candidates. This suggests that I cannot generalize the later findings to non-close races, and that closely-elected women constitute the "most likely" case; they are more likely to oppose military actions than non-closely elected women. Having said that, however, the results in Table I are driven by women losers who received only negligible votes. When I made a similar comparison for women candidates within the winning margins of ±30 percentage points, I did not find significant differences. I also did not find significant differences among women winners. These results imply that my findings can potentially be generalized to women who have reasonable prospects of victory.

Table I. Sample Characteristics: Women in Close and Non-close Races

		Political Position				Electoral Strength			
	Gov. party	General left-right	Economic left-right	Gender equality	Anti-violence	Local platform	Affilliate orgs.	Internal cohesion	Funding resource
Diff-in-means	0.03 (0.04)	0.07 (0.04)	0.21 (0.10)	-0.06 (0.05)	0.11* (0.03)	0.16* (0.04)	0.15* (0.05)	-0.06 (0.05)	0.01* (0.00)
N	11,594	18,422	11,594	11,594	11,594	11,594	11,594	11,594	11,594

NOTE: The table shows the differences in means. The robust standard errors are in parentheses. The p-values are adjusted for multiple hypothesis testing. $^{\dagger}\, p < 0.1$; $^{*}\, p < 0.05$.

Second, as discussed by Marshall (2023) and other studies (Bucchianeri 2018; Hall 2015), the mixed-gender RDD identifies the effects of closely electing women, not the effects of legislators' genders.[41] Closely elected women and men are different not only in their gender but also in their ideological positions, electoral strength (Anzia and Berry 2011; Ashworth, Berry, and Bueno de Mesquita 2023; Fulton 2012), and other characteristics. In the main analyses, I estimated

[41] I do not estimate theoretical parameters like voters' discrimination (Ashworth, Berry, and Bueno de Mesquita 2023). My estimand also differs from the effect of ordinary women's representation; women legislators differ from ordinary women.

the "bundled" effects of closely electing women, without isolating the effects of gender from those of other characteristics (Sen and Wasow 2016). Indeed, I consider that women legislators oppose military actions partly due to their ideological preferences. Women legislators can also sway legislative votes because they are selected to be competent. Isolating these relevant characteristics would detach the treatment from reality and render it less meaningful.

Table II compares the characteristics of women and men in mixed-gender close races (*treatment characteristics*). Table II indicates that closely-elected women are more left-oriented, put higher priorities on gender equality and peace, and are somewhat stronger in elections. These are consistent with my arguments and previous findings that women legislators are less supportive of military actions and more competent than men legislators (Ashworth, Berry, and Bueno de Mesquita 2023; Marshall 2023).

Table II. Treatment Characteristics: Closely Winning Women and Men

		Political Position				Electoral Strength			
	Gov. party	General left-right	Economic left-right	Gender equality	Anti-violence	Local platform	Affilliate orgs.	Internal cohesion	Funding resource
Diff-in-means	-0.09 (0.04)	-0.23^{*} (0.04)	-0.50^{*} (0.09)	0.24^{*} (0.05)	0.11^{*} (0.03)	0.04 (0.04)	0.17^{*} (0.05)	0.04 (0.05)	0.01 (0.00)
N	551	897	551	551	551	551	551	551	551

NOTE: The table shows the differences in means. The robust standard errors are in parentheses. The p-values are adjusted for multiple hypothesis testing. $^{\dagger}\, p < 0.1$; $^{*}\, p < 0.05$.

Third, one may even doubt the core assumption that the results of close races are randomly assigned. For example, women in government parties might be stronger in close races as they have access to state resources. To assess this possibility, I compare the characteristics of closely winning and losing women in Table III (*balance checks*) and find no statistically significant difference (later, I also check the balance with aggregated covariates).

Table III. Balance Checks: Closely Winning and Losing Women

		Political Position				Electoral Strength			
	Gov. party	General left-right	Economic left-right	Gender equality	Anti-violence	Local platform	Affilliate orgs.	Internal cohesion	Funding resource
Diff-in-means	0.08 (0.04)	0.09 (0.04)	0.20 (0.10)	0.02 (0.09)	0.00 (0.08)	0.19 (0.09)	-0.02 (0.07)	0.00 (0.08)	0.00 (0.00)
N	737	980	737	737	737	737	737	737	737

NOTE: The table shows the differences in means. The robust standard errors are in parentheses. The *p*-values are adjusted for multiple hypothesis testing. $^{\dagger} p < 0.1$; $^{*} p < 0.05$.

Cross-national Analysis: Specification

By using the treatment variable R_i (the proportion of women's close victories across mixed-gender close races), I estimated its effect on military actions. The unit of analysis was each month (t) after election i. I included all months until the next election (except for election months). The data on military actions were obtained from the incident-level records of the Correlates of War (CoW) project (Sarkees and Schafer 2000). The dataset covers the period from 1993 to 2014. I use 4,492 observations of 125 elections in 36 countries available both in the CLEA and CoW.[42]

The outcome variable Y_{it} is the count of militarized incidents initiated by a state t months after a legislative election i. The militarized incidents include the overt and actual use of military forces against other states, such as the occupation of territory, seizure of material or personnel, attack, and war.[43] As I am interested in the actual use of military forces, I used the incident-level data instead of dispute-level data. Although the data are based on media reports, "as long as the

[42] See Appendix A4 & A5 for summary statistics and geographical distribution of countries.

[43] I do not include mere threat or display of military forces (see placebo tests). The CoW discards incidents if they occur within three days of another incident of similar actors and types. Military interactions during war are not counted as separate incidents.

measurement error is uncorrelated with the independent variables, measurement error in the dependent variable is not particularly problematic" (Weidmann 2016, 208).

The moderator D_i is an indicator that takes 1 if a war declaration (V-Dem; Coppedge et al. 2021) or use of military force (IAEP; Wig, Hegre, and Regan 2015) requires approval of a lower or unicameral chamber.[44] Both of the datasets are based on expert coding. While the V-Dem focuses on war and de jure rules with more objective coding, the IAEP is more inclusive and captures de facto practices with more subjective coding. I used both of the datasets to supplement the weakness of the other.[45]

With these variables, I used Poisson regressions with the maximum likelihoods (ML);[46]

$$Y_{it} \sim \text{Pois}(\lambda)$$

$$\lambda = \exp\left(\alpha_{country_i} + \delta_{veto} R_i \times I(D_i = 1) + \delta_{\neg veto} R_i \times I(D_i = 0) + \beta D_i\right).$$

The quantities of interest are δ_{veto} and $\delta_{\neg veto}$, which represent the effects of women's close victories with ($D_i = 1$) and without ($D_i = 0$) the legislative veto power, respectively. I is an indicator function. The coefficient of the constituent term, β, represents the descriptive difference between $D_i = 1$ and $D_i = 0$ when no women win close races (it is not the causal effect of the

44 I manually filled the values of minor island countries. I used the data at the timing of election i so that the treatment did not affect the moderator.

45 I did not use Henisz (2000)'s data, which are not specific to military actions. See footnote 5.

46 I also estimated linear and negative binomial models in robustness checks. Poisson regressions have good finite-sample properties with fixed effects (Greene 2011).

legislative veto power).[47] The model includes country fixed effects $\alpha_{country_i}$ so that I compare the elections within a country.[48]

I weighted the observations by the number of mixed-gender close races in election i (denominator of R_i). When there are only a few close races, the variability of R_i is limited (e.g., if there is only one close race, R_i is either 0 or 1). As the number of close races grows, R_i becomes continuous and more informative.[49] Weighting accounts for these differences (Greene 2011). Due to zero weights, 748 observations were dropped, resulting in 3,744 observations. Finally, because the treatment variable R_i is constant for a given i, the standard errors were clustered by election.

Cross-national Analysis: Results

Table IV shows the main findings. The first column shows the average effect of women's close victories. Although the coefficient is negative, the estimate is indistinguishable from zero. However, as seen in the second column, the average effect masks heterogeneous effects. With the legislative veto power, women's close victories significantly reduce military actions, while the coefficient is even positive and significant without the legislative veto power. Importantly, the latter positive estimate is driven by a single country; the estimate ceases to be significant when I drop India (see Figure A8-2). A potential explanation for the positive effect is that a government

[47] The constituent term R_i perfectly correlates with the interaction terms and thus was omitted.

[48] I did not use R_i as an instrumental variable for the total seat share of women. The IV approach imposes the structural assumption that the effects of closely and non-closely electing women are the same, which is less plausible given the results in Table I.

[49] Without any close races, the weight was zero and hence those elections were dropped. I also checked the robustness with an alternative specification. See a later robustness check.

uses women's representation to legitimize military actions, even though women legislators have little influence over military actions (Clayton, O'Brien, and Piscopo 2019; Mechkova, Dahlum, and Petrarca 2023).

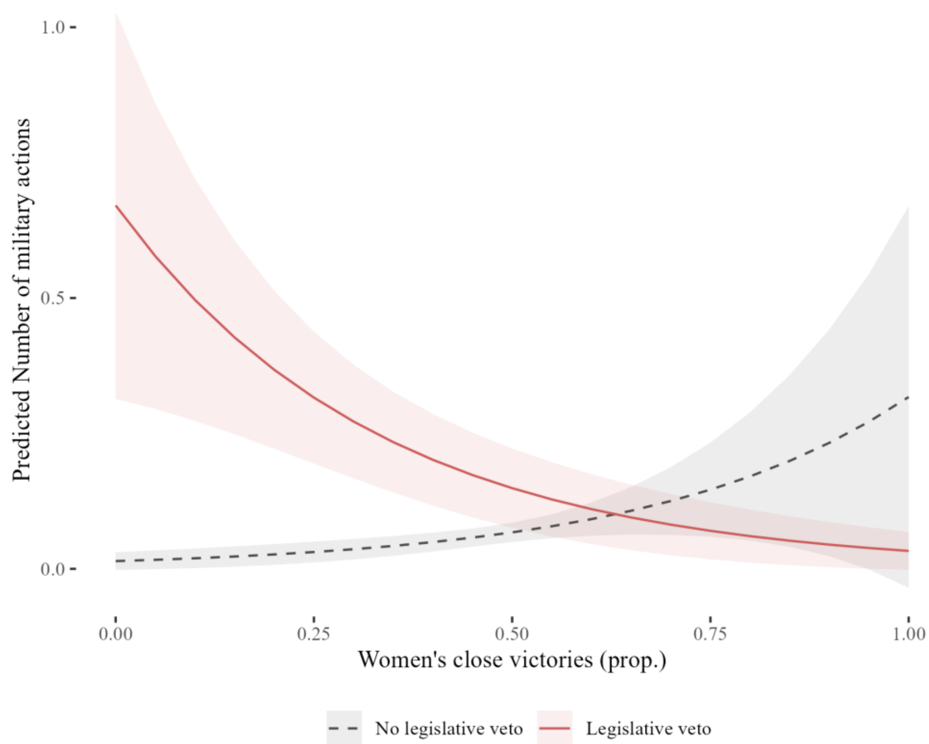
Table IV. Results of Cross-national Analysis

	1	2
Women's close victories (prop.)	-0.86 (1.08)	
Legislative veto × Women's close victories		-3.01* (0.69)
No legislative veto × Women's close victories		3.10* (1.11)
Legislative veto		3.85* (0.75)
N	3,744	3,744

NOTE: The table shows the ML estimates of the coefficients in the Poisson models. The models include country fixed effects. The standard errors are clustered by election. $^{\dagger}p < 0.1$; $^{*}p < 0.05$.

Figure 2 plots the predicted number of military actions over the values of the treatment variable R_i. If women would win all close races, it could reduce the number of military actions by 0.67 with legislative veto power (red solid line). This corresponds to a decrease by more than one standard deviation of the outcome variable. By contrast, without a legislative veto power (grey dashed line), a similar change increases military actions by 0.32. Thus, even though the effect without legislative veto power is positive and significant in Table IV, the substantive effect is somewhat limited.

Figure 2. Effect Sizes of Women's Close Victories



NOTE: The figure shows the predicted number of military actions over the proportion of women's close victories. The shaded areas are the 95% confidence intervals.

In Table V, I test H2 by breaking down the treatment variable R_i to the proportions of women's close victories in government and opposition parties.[50] While the first and third columns show the unconditional effects, the second and fourth columns show the effects conditional on the legislative veto power. Consistent with H2, the main results are driven by women's close victories in government parties (second column of Table V).

[50] For examples, (N of women's close victories in government parties) / (N of mixed-gender close races involving government parties).

Table V. Heterogeneous Effects between Government and Opposition Parties

	1	2	3	4
Women's close victories in gov. parties	-0.81 (0.68)			
Women's close victories in opp. parties			-0.25 (0.78)	
Legislative veto × Women's close victories in gov. parties		-1.72* (0.83)		
No legislative veto × Women's close victories in gov. parties		0.58 (0.76)		
Legislative veto × Women's close victories in opp. parties				-0.51 (0.87)
No legislative veto × Women's close victories in opp. parties				2.12 (1.79)
Legislative veto		0.94 (0.75)		2.55* (1.19)
N	3,270	3,270	3,411	3,411

NOTE: The table shows the ML estimates of the coefficients in the Poisson models. The observations are dropped if there is no government or opposition party. The models include country fixed effects. The standard errors are clustered by election. $^{\dagger} p < 0.1$; $^{*} p < 0.05$.

Additional Analyses

I checked the validity of the design, heterogeneous effects, and robustness, which are summarized in Table VI and detailed in Appendix A6-A8. The treatment variable did not correlate with most covariates, providing further credence to the identification assumption. The treatment variable also did not affect the past outcome variable or mere threat and display of military forces, which usually do not require legislative approval. Moreover, I also found that the treatment effects depend on the legislative veto over treaty ratification—a placebo conditioning variable. I also checked heterogeneous effects over time, based on the seat share of women legislators and the proportion of mixed-gender close races. The results are also robust to sample configuration, different bandwidths, control variables, fixed effects, regression specification, and standard error calculation. Overall, the analyses provide robust support for the pacifying effects of women's legislative representation.

Table VI. Additional Analyses (Cross-national Analysis)

Validity checks		
Balance check with aggregated indicators	✓[1]	Table A6-I
Placebo tests with past military actions	✓	Table A6-II
Placebo tests with threat and display of forces	✓	Table A6-III
Placebo tests with legislative veto over treaty ratification	✓	Table A6-IV
Heterogeneous effects		
Proportion of mixed-gender close races	✓	Figure A7-1
Overall women's seat share	✓	Figure A7-2
Time between elections	✓	Figure A7-3
Robustness checks		
Omission of countries with less confident gender classification	$-^{*}$	Table A8-I
Aggregation to elections	$-^{*}$	Table A8-II
Different bandwidths	$-^{*2}$	Figure A8-1
Control for covariates	$-^{*}$	Table A8-III
Year fixed effects	$-^{*}$	Table A8-IV
Log linear model	$-^{*}$	Table A8-V
Negative binomial model	$-^{*}$	Table A8-VI
Linear probability model with the dichotomized outcome	$-^{*}$	Table A8-VII
Logit model with the dichotomized outcome	$-^{*}$	Table A8-VIII
Control for the number of close races without weights	$-^{*}$	Table A8-IX
Standard errors clustered by elections and year	$-^{*}$	Table A8-X
Leave-one-country-out tests	$-^{*}$	Figure A8-2
Alternative channels		
Military budget	null	Table A9-I
Military capability	null	Table A9-II
Appointment of a leader	null	Table A9-III
Appointment of ministers/secretaries	null	Table A9-IV

NOTE: $^{\dagger} p < 0.1$; $^{*} p < 0.05$. Note 1: 1 out of 8 predictors is significant at a 5% level. Note 2: Null in 5 out of 10 specifications.

As seen at the bottom of Table VI and in Appendix A9, I did not find evidence for alternative institutional channels, such as military budget and capability, and appointment of leaders and ministers. Thus, although I do not intend to eliminate all alternative channels or argue that they are mutually exclusive, I examine the remaining key mechanism—legislative veto power—in the following analysis.

Party-vote Analysis: Specification

I analyzed party-level legislative votes over foreign military deployment. The main dataset was the Parliamentary Deployment Votes Data (PDVD), the database of legislative votes over military deployment in 21 countries between 1990 and 2019 (Ostermann and Wagner 2023). The database contains all legislative votes over the foreign deployment of military forces. Among those records, 239 legislative votes by 49 parties in eight countries between 1991 and 2019 are matched with the CLEA.[51] As the PDVD is available for each party (unfortunately, not for each legislator), I used a pair of party j and legislative vote h as a unit of analysis.

For each party and legislative vote, I calculated the treatment variable r_{jh}, which is the proportion of close victories of women candidates belonging to party j in the latest legislative election before legislative vote h. The outcome variable was the composition of positive, negative, and absent votes for military deployment in party j in legislative vote h.[52] Following Tomz et al. (2002) regarding compositional data, I used the logarithmic ratios of vote shares as outcomes: $\log\left(\frac{\text{Yes votes}+1}{\text{Abstention}+1}\right)$, $\log\left(\frac{\text{Yes votes}+1}{\text{No votes}+1}\right)$, and $\log\left(\frac{\text{No votes}+1}{\text{Abstention}+1}\right)$. I also examined the effects on vote concentration within each party, which was measured as the Herfindahl-Hirschman index

[51] The datasets are linked via Party Facts (Döring and Regel 2019). The countries include Canada, France, Germany, Italy, Japan, Lithuania, UK, and USA. The main results of the cross-national analysis (i.e., Table IV) hold in a sample of those countries.

[52] For votes for military withdrawal and anti-interventions, yes (no) votes are considered negative (positive) votes for military deployment (Ostermann and Wagner 2023).

(Yes votes^2 + No votes^2 + Abstention^2) and standardized to a 0-1 scale. The index takes 1 if all members cast the same votes, and 0 if their votes are equally split.[53]

I used a linear model for each outcome y_{jh} and estimated the coefficients by ordinary least squares (OLS);[54]

$$y_{jh} = a_j + u_{election_h} + \rho r_{jh} + \gamma g_{jh} + \epsilon_{jh}.$$

The parameter of interest is ρ, which represents the causal effects of closely electing women. The model includes party and election fixed effects, a_j and $u_{election_h}$. This means that I compared parties after an election to the same parties after different elections, and also to other parties after the same election. I also controlled for an indicator g_{jh} that takes 1 for government parties and 0 for opposition parties. As I conducted additional analyses by interacting r_{jh} and g_{jh} for testing H2, I included g_{jh} in the main specification for comparability. The coefficient γ represents the baseline difference between government and opposition parties (it is not the causal effect of government parties).

Similar to the cross-national analysis, the observations were weighted by the number of mixed-gender close races involving women candidates of party j in the last election. Of 1,111 observations, 368 were dropped due to zero weights. Finally, because r_{jh} is constant for a given party after an election, I clustered the standard errors by party-election pair.

[53] See Appendix A10 for summary statistics.

[54] I did not use the seemingly unrelated regressions as the estimates are numerically identical to OLS when the right-hand side variables are the same across equations (Greene 2011).

Party-vote Analysis: Results

Table VII shows the results of the party-vote analysis. The odd-numbered columns show the average effects, whereas the even-numbered columns show the effects for government and opposition parties. The average effects weakly support H1 (odd-numbered columns); women's close victories reduce positive votes for military deployment and make parties less cohesive, while not changing the ratio of negative votes and abstention.

As seen in the even-numbered columns, I also found larger effects for government parties; the coefficients for government parties are approximately twice as large as those for opposition parties, providing support for H2. Moreover, in contrast to the null findings in the event data analysis (Table V), the coefficients are negative and significant even for opposition women (Table VII). This implies that opposition women can decrease votes for military actions, but they do not alter the government's decisions. This probably reflects the fact that opposition legislators have less influence over government policies.

Table VII. Results of Party-votes Analysis

	Log(Yes votes / Abstention)		Log(Yes votes / No votes)		Log(No votes / Abstention)		Vote concentration	
	1	2	3	4	5	6	7	8
Women's close victories (prop.)	$-0.13^{\dagger}$ (0.07)		-0.16 (0.11)		0.03 (0.06)		-0.09^{*} (0.04)	
Gov. party × Women's close victories		-0.32^{*} (0.13)		$-0.40^{\dagger}$ (0.20)		0.08 (0.18)		$-0.23^{\dagger}$ (0.12)
Opp. party × Women's close victories		-0.17^{*} (0.06)		-0.21^{*} (0.10)		0.04 (0.08)		-0.12^{*} (0.05)
Gov. party	0.15^{*} (0.02)	0.24^{*} (0.05)	0.26^{*} (0.03)	0.37^{*} (0.08)	-0.11^{*} (0.02)	$-0.13^{\dagger}$ (0.08)	0.06^{*} (0.01)	0.12^{*} (0.05)
N	743	743	743	743	743	743	743	743

NOTE: The table shows the OLS estimates of the coefficients. The models include party and election fixed effects. The standard errors are clustered by pair of a party and election. $^{\dagger}p < 0.1$; $^{*}p < 0.05$.

To better understand the effect sizes, Table VIII shows the effects on the share of each legislative vote.[55] As seen in Table VIII, if women would win all close races, it could reduce the share of positive votes by 36 percentage points in government parties. This amounts to nullifying the baseline difference between government and opposition parties. As mixed-gender close races comprise only a small fraction of all races (8.73%), the large effect suggests the changes in other legislators (men and non-closely elected women); an 8.75 percentage point increase in women's seat share leads to a 36 percentage point decrease in the share of positive votes. Although caution should be exercised regarding ecological inferences, the most plausible explanation is that closely elected women, especially those in government parties, deviated from the party line *with other legislators* including men and non-closely elected women.[56] This spillover effect can explain why women's close victories influence states' military actions, despite their rarity.

[55] The estimates in Table VIII are less efficient than those in Table VII as they do not account for the compositional nature of the outcome variables (Tomz, Tucker, and Wittenberg 2002).

[56] An alternative possibility is that women's close victories only affect non-closely elected legislators. However, this seems less plausible in the context of this study.

Table VIII. Effect Sizes: The Effects on Vote Shares

	Yes votes (prop.)		No votes (prop.)		Abstention (prop.)	
	1	2	3	4	5	6
Women's close victories (prop.)	-0.14 (0.08)		0.09 (0.08)		0.05 (0.03)	
Gov. party × Women's close victories		-0.36* (0.14)		0.24 (0.17)		0.12 (0.11)
Opp. party × Women's close victories		-0.18* (0.07)		0.12 (0.08)		0.06 (0.04)
Gov. party	0.20* (0.02)	0.30* (0.06)	-0.18* (0.02)	-0.25* (0.07)	-0.02* (0.01)	-0.05 (0.05)
N	743	743	743	743	743	743

NOTE: The table shows the OLS estimates of the coefficients. The models include party and election fixed effects. The standard errors are clustered by pair of a party and election. $^{\dagger} p < 0.1$; $^{*} p < 0.05$.

Additional Analyses

I also checked covariate balances, heterogeneous effects, and robustness, which are summarized in Table IX and detailed in Appendix A11-A13. Only one among twelve covariates correlates with the treatment r_{jh} with a marginal statistical significance. I did not find evidence for heterogeneous effects over time or parties' ideological positions. The main results (the effect of closely electing women in government parties) hold for most robustness checks. Overall, the analysis provides robust evidence for the legislative veto mechanism.

Table IX. Additional Analyses (Party-vote Analysis)

	log(Yes votes / Assentation)	log(Yes votes / No votes)	log(No votes / Assentation)	Vote concentration	
Validity checks					
Balance check with aggregated indicators		✓[1]			Table A11-I
Heterogeneous effects					
Party ideologies		✓			Figure A12-1
Time between elections		✓			Figure A12-2
Robustness checks					
Different bandwidths	$-^{*2}$	null^{3}	null	$-^{*4}$	Figure A13-1
Control for covariates	$-^{\dagger}$	null	null	$-^{*}$	Table A13-I
Legislative vote fixed effects	$-^{\dagger}$	$-^{\dagger}$	null	$-^{\dagger}$	Table A13-II
Standard errors clustered by party	$-^{*}$	$-^{*}$	null	$-^{\dagger}$	Table A13-III

NOTE: $^{\dagger}p < 0.1$; $^{*}p < 0.05$. Note 1: 1 out of 12 predictors is significant at a 10% level. Note 2: Significant at a 10% level in 2 out of 10 specifications, and null in 4 out of 10 specifications. Note 3: Significant at a 10% level in 2 out of 10 specifications, and null in 6 out of 10 specifications. Note 4: Significant at a 10% level in 1 out of 10 specifications, and null in 5 out of 10 specifications.

Case Study

The party-vote analysis examined an observable implication of my theory; women legislators are less likely to vote for military deployment. However, as I discussed in the theory section (p.8), the sample is inevitably truncated; when women legislators can effectively persuade other legislators and reject a bill on the floor, a government can anticipate it and thus abstain from submitting the bill. Although I believe that the sample selection makes my estimates conservative, it is still useful to examine a case involving an unrealized legislative vote.

To this end, I briefly discuss the roles of Congresswoman Marcia Fudge in the failed authorization of US intervention in Syria in 2013. The case of the 2013 resolution is illustrative, as the Obama administration failed to secure a majority of votes and abandoned the intervention even before a floor vote. Marcia Fudge swayed the voting behavior of the Congressional Black

Caucus (CBC), whose support was necessary for passing the resolution (Bacon 2013). Thus, the case neatly illustrates how a congresswoman sways other legislators and thwarts a military plan.[57]

The possibility of the Syrian intervention emerged on August 21st, 2013 when the US government reported the Syrian government's use of chemical weapons. This prompted Barack Obama, who had previously referred to the use of chemical weapons as a “red line,” to seriously consider a military intervention. Likely due to the criticism of the 2011 intervention in Libya and the resulting controversy over the War Power Resolution, Obama did not immediately order air strikes. Instead, he decided to bring the matter to Congress on August 31st. The Senate committee passed the resolution on September 4th with a few additional restrictions.

However, opinions on the floor were divided. In the Senate, 24 were in favor of the military intervention, 29 were against it, and 47 were undecided (as of September 5th; The New York Times 2013). In the House, 32 were supportive, while a majority are against (181) or undecided (213).[58] This made it necessary, if not sufficient, for Obama to secure 43 votes from the CBC (Bacon 2013). Although the CBC had historically been less supportive of military actions, they were the core supporters of the Obama administration. In fact, a majority of CBC members voted in favor of the 2011 intervention in Libya.[59] Nonetheless, few CBC members expressed support in

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[57] Fudge won an uncontested election in 2012. However, as discussed in p.16, our findings can potentially be extended to women legislators who has reasonable prospects of victory.

[58] Opinions of seven legislators were unknown.

[59] 24 members voted for authorizing and funding the Libyan intervention, 6 voted against both, 8 voted for funding but against authorization, and 2 abstained.

2013. Only 5 members supported the intervention, while 6 opposed it and 30 were undecided (The New York Times 2013).[60]

While the lack of support can be attributed to various factors, such as skeptical attitudes among black voters, one factor was Marcia Fudge. As the chair of the CBC, she asked CBC members on September 3rd to “limit any public comment on the subject of Syria until after the meeting [with the President]” (Korte 2013). Although Fudge’s true intention was less clear, its impact was evident. While the message did not change the minds of the most liberal legislators, such as Barbara Lee, it silenced mainstream Democrats who had supported the Libyan intervention, as well as newly elected members. Among the 24 legislators who had supported the Libyan intervention, only 5 expressed support for the Syrian intervention. All newly elected members remained undecided. On September 9th, the President and Susan Rice held a meeting with the members of the CBC for nearly two hours, but they were unable to persuade them. On the following day, Obama abandoned the intervention and struck a deal with Russia. The resolution never received a floor vote.

Officially, her message on September 3rd aimed to encourage CBC members to thoroughly assess all available information (Korte 2013). Nevertheless, I speculate that the message was influenced by several other factors. Ideologically, Fudge positioned herself somewhere between liberal and mainstream Democrats, with a policy focus on welfare and agricultural policies.[61] Unlike her colleague Barbara Lee, she did not consistently oppose military interventions. In 2011, for instance, she cast a vote in favor of the Libyan intervention. However, the situation in 2013

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[60] Opinions of two members were unknown.

[61] She was more liberal than 75% of Democrats (Lewis 2023).

was different. The former chairs of the CBC, Barbara Lee and Emanual Cleaver, did not hesitate to oppose the military intervention. Both black and women voters held skepticism toward the military intervention (Dimock and Doherty 2013). On the other hand, she needed to maintain a relationship with Obama. In September, Republicans threatened to defund Obamacare, a cause for which Fudge had put in much effort. If the CBC's votes would be divided as they were in 2011, it might weaken the unity of the CBC and thus the Obama administration. The message on September 3rd seemed to rest on a delicate balance within those political contexts.

The case of Marcia Fudge illustrates how a congresswoman can leverage her political resources, such as the position of the CBC chair, to influence the voting behaviors of other legislators. The actions of other congresswomen, such as Barbara Lee, who used her well-established reputation to pressure Obama (2013), and Michele Bachmann, who used the Tea Party platform to unite the opposition (Henry 2013), are also illustrative. Therefore, although political resources possessed by women legislators vary across cases, they can influence a state's military actions. Certainly, the case study does not rigorously or comprehensively reveal the causal mechanism or how her experience as a black woman shaped her ideologies and actions; these aspects require a separate paper. The purpose of this case study is to complement the quantitative analyses by demonstrating that women legislators can influence other legislators and thus alter the course of military actions.

Discussion

Despite the well-established correlation between women's legislative representation and peace, the causal mechanisms are underdeveloped, and the causal evidence is weak (Cohen and Karim 2022; McDermott 2015). I have addressed those shortcomings by focusing on legislative veto power as a key mechanism and using a natural experiment. The cross-national analysis showed that women's close victories causally reduced the states' military actions when legislatures have veto power. Similarly, the analysis of legislative votes indicated that women's close victories decreased votes for military deployment, especially those among government parties.

These findings constitute the first causal evidence supporting the gender-peace thesis. As Cohen and Karim (2022) argue, scholars need to move from correlational analyses to analysis of causality and mechanisms. To this end, I have provided new data and a novel design. Importantly, my approach can be used for various outcomes, including welfare policies, international trade and agreements, foreign aid, corruption, and human rights, all of which lack rigorous causal identification in cross-national analyses.

I have also addressed Cohen and Karim (2022)'s call for a greater focus on causal mechanisms at micro-levels. Although I did not intend to exhaustively test all mechanisms and have focused on institutional channels, the party-vote analysis suggests that women legislators, especially those in government parties, deviate from the party line with other legislators. I have also examined alternative channels, such as women legislators' influence over budget and executive appointment, but found no evidence supporting them (Appendix A9). I believe these are important steps for bridging the macro and micro studies of gender and peace.

Nevertheless, it should be acknowledged that this study has not addressed all problems. Future studies should expand the data and design to other electoral systems (e.g., PR systems and quotas) and non-democracies, in which the effects can differ due to distinct institutional incentives.[62] It is equally important to extend the time frame to earlier periods and thus explore the roles of international norms and institutions (Dahlum and Wig 2020; Dube and Harish 2020).

[62] See footnote 11 and 12.

Furthermore, while I have focused on institutional mechanisms, women legislators can affect extra-institutional (e.g., anti-war protests) and deliberative (e.g., committee discussion, agenda setting, and framing) processes. These mechanisms are important for understanding how women legislators alter other legislators' behaviors (i.e., spillover effect). Finally, the unit of analysis should be disaggregated to individual legislators. Compiling the individual-level data of legislative votes across many countries would provide a real micro-foundation for cross-national studies.

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