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Wartime Violence and Post-War Women's Representation

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Abstract

How does wartime violence shape post-war women's representation? Does past violence make women more or less likely to run for office? And if they do run, are they getting elected? This article argues that violence influences women's representation in contrasting ways at these two stages. In wartime, women have more opportunities to gain leadership skills, which likely increases the number of women running for office after the war. However, past violence also increases threat perceptions among voters. This, combined with gender stereotypes about male and female politicians, likely reduces voter support for female candidates. Using pre- and post-war electoral and wartime violence data at the municipal level from Bosnia, the authors present evidence that is consistent with their argument. The results hold across a number of robustness tests, including accounting for post-war demographic gender balance and women's party list placement.

Keywords: gender; representation; wartime violence; post-conflict; Eastern Europe

How does wartime violence shape post-war women's representation? Does exposure to violence motivate women to engage more fully in political life, or does it make them withdraw from this traditionally male-dominated realm? And if women do engage, are they likely to succeed in political contests? We know from prior literature that women enter more public roles in wartime (Fuest 2008; Goldin 1991; Wood 2008). However, it is not clear whether women's empowerment extends beyond the war and into the political realm (Buvinic et al. 2013; Byrne, Marcus and Powers-Stevens 1996). Similarly, recent micro-level research suggests that exposure to violence increases civic and political activism *in general* (Bellows and Miguel 2009; Blattman 2009; Voors et al. 2012), but how it influences *women's* political activism and voter response to female candidates is still an open question. Various studies have demonstrated how the presence of women in government and legislatures has important effects on the content of policy output (Schwindt-Bayer and Mishler 2005; Wängnerud 2009), the external political efficacy of women (Atkeson and Carrillo 2007; Barnes and Burchard 2013) and the duration of post-conflict peace (Shair-Rosenfield and Wood 2017). This only underscores the importance of better understanding the relationship between wartime violence and post-war women's representation.

In this article, we examine whether and how the extent of municipal-level wartime violence is associated with women's post-war representation in that municipality. We explore the relationship between violence and (a) the supply of female candidates as well as (b) the electoral success of these candidates. We expect violence to influence women's representation in contrasting ways at these two stages. On the one hand, we argue that exposure to violence motivates capable and qualified women to seek political office. Women often organize and mobilize politically in wartime and gain valuable political experience. Such wartime activism should be more common in locales that experience a great deal of violence, and therefore, increases the post-war supply of

capable and motivated women ready to run for political office. On the other hand, we expect that voters who reside in high-exposure locales experience higher perceptions of threat and prioritize security and safety over other concerns after the conflict ends. This, combined with traditional gender stereotypes about the competencies and personal qualities of male and female politicians, reduces the electoral success of female candidates.

Our study relies on data from Bosnia after its major civil war in 1992–1995. We use geographic variance in the level of wartime violence and pre- and post-war electoral data to estimate the association between violence and post-war women's representation. We find that municipalities that experienced more wartime violence witnessed a *greater number of women running* for local political office but *fewer of them being elected* than did comparable, low-violence municipalities. The results hold across numerous robustness tests, including direct-effect analyses (Acharya, Blackwell and Sen 2016) that account for (a) the gendered demographic changes that occur in wartime and (b) post-war women's list placement.

This article contributes to the literature on the social and political legacies of wartime violence by extending its scope to women's representation. We present evidence that exposure to wartime violence may promote women's representation at some stages of the electoral process while hindering it at others. The second finding, in particular, may seem to challenge some of the recent work on post-conflict women's representation. For example, prior studies have argued that conflict can induce institutional changes (such as gender quotas) that increase women's presence in legislatures (Anderson and Swiss 2014; Bush 2011). However, changing institutions in response to violence is just one way to affect women's representation and is not the focus of our study. Rather, by using fine-grained subnational data, we hold the institutional changes constant and focus instead on an alternative channel through which violence can influence women's representation, that is, by shaping voter preferences.

In a recent book, Tripp (2015) suggests that the institutional and attitudinal effects may run in opposite directions: she observes that while women's legislative representation is significantly higher in post-conflict African countries than in those that never experienced conflict, *approval* of women's leadership is actually lower in the former. The author notes that these two findings are not contradictory 'because female legislative representation is largely shaped by an institutional innovation, namely the introduction of quotas, which are adopted precisely to sidestep cultural and religious objections to women's leadership' (Tripp 2015, 215). Our study is consistent with this conclusion: while institutional innovation in post-conflict societies can increase women's representation, the violence committed during conflict may also reduce women's presence in government through its effects on voter support for women's leadership. We elaborate on these contributions in the concluding section.

Violence and Women's Political Representation

We argue that communal experience with wartime violence is associated with a higher supply of female candidates post-war, but a lower likelihood of women winning political office. We detail each part of this argument in turn.

The Supply of Female Candidates

We borrow from several strands of related literature to build our argument. First, prior studies on gender and conflict have shown that civil conflict can provide a unique opportunity to expand women's representation. In wartime, women assume new roles and participate in activities that were previously male dominated. With men serving as soldiers and potentially dying in combat, women often become the new heads of households and enter the workforce in unprecedented numbers in order to support themselves and their families (Acemoglu, Autor and Lyle 2004; Goldin 1991). They may even be recruited into military and paramilitary organizations, or

lead activist groups committed to bringing an end to hostilities (Grayzel 1999; Hughes 2009). Exercising authority and agency that was unavailable in the pre-war period, women active in war-time become politicized and gain valuable experience in the process. As a consequence of having acquired this experience in traditionally male-dominated realms, women in the post-war period tend to be more familiar with the responsibilities of a political candidate.

We argue that locations that experienced more violence also witnessed a greater expansion of qualified and motivated women willing to seek office post-war, since women were most likely to assume the new roles described above in these locations. The demographic consequences of war are most severe in high-violence communities, thereby compelling more women to become heads of households and workforce participants. Additionally, places that experience higher levels of violence require more combatants, making women's participation in military organizations more likely. High-violence locations are also fertile ground for peace and community activism in which women can engage during wartime. In other words, high-violence areas are more likely to provide women with experience and skills that motivate them to become candidates for political office.¹

Secondly, previous micro-level studies have shown that experience with wartime violence is often associated with increased prosocial behavior and community participation (Bellows and Miguel 2009; Blattman 2009; Voors et al. 2012). People who are exposed to more violence during war are more likely to take up leadership positions in their communities and to be more active in civic and social groups – an effect that persists at both the individual and communal levels (Gilligan, Pasquale and Samii 2014). The mechanism behind this finding is not clear, but several studies propose that this results from psychological coping with wartime adversities labelled 'post-traumatic growth' (Blattman 2009; see also Powell et al. 2003). Note that women in high-violence locales are more likely to have been personally exposed to violence *and* to have opportunities for political engagement. Therefore, our analysis is unable to sort out which of these mechanisms – women's empowerment during war or post-traumatic growth – are responsible for the hypothesized positive association between communal violence and women's political activism. This remains a task for future work.² The discussion above generates our first hypothesis.

HYPOTHESIS 1: (Candidate Supply): Communal exposure to wartime violence is positively associated with the number of women running for political office post-war.

The Success of Female Candidates

While women's political activism is likely to increase as a result of wartime violence, such violence may simultaneously decrease support for female candidates. We derive this argument from findings in the literature on gender stereotypes as well as terrorism and voting. The argument proceeds in three stages.

First, prior research argues that people associate men and women with specific stereotypes. Women are associated with communal roles, such as being a homemaker and caregiver, and attributes such as being affectionate, nurturing, kind and sympathetic (Bauer 2015; Eagly and Karau

¹Some studies have argued that wartime gains made by women are short-lived, and that traditional gender roles are reintroduced in the post-war period (Buvinic et al. 2013; Byrne, Marcus and Powers-Stevens 1996). While this might be the case, for our argument, it is the experience and skills women gain in wartime that are relevant. Even if their status deteriorates after the conflict, women who were politically and socially active during the conflict nevertheless acquired skills that make them formidable political candidates.

²We also recognize that party elites may play an important role in recruiting candidates (Cheng and Tavits 2011; see also Kittilson 2006). However, even if elites were to recruit women more aggressively in high-violence locales, women nevertheless need to exhibit the motivation to contest elections as candidates. Therefore, if we observe that more women are *running* for office in high-violence areas, this association is unlikely to emerge simply because of party recruitment efforts; it should be at least partly due to women's willingness to participate.

2002). In contrast, men are associated with agentic roles, such as being a leader and a breadwinner, and attributes such as being decisive, dominant and aggressive (Bauer 2015; Eagly and Karau 2002). The literature on gender and politics confirms these findings specifically for voters (Koch 2002). While voters generally see men as more decisive and competent, as stronger leaders, and better able to handle crises (Dolan 2014), they perceive women as less fit to deal with issues such as national security and law and order (Huddy and Terkildsen 1993; see also Bauer 2015 and Holman, Merolla and Zechmeister 2016 for a review).

Secondly, threat perceptions heighten the demand for leadership defined by attributes stereotypically associated with men (Holman, Merolla and Zechmeister 2011). For instance, research shows that (a) individuals prefer male faces over female ones at times of war (Little et al. 2007), (b) to cope with the threat of terror, individuals look for hawkish and decisive leadership (Merolla, Ramos and Zechmeister 2007) and (c) women are less preferred as leaders during security threats (Barnes and O'Brien 2017; Berinsky 2009; Holman, Merolla and Zechmeister 2011; Lawless 2004). These general patterns in response to security and safety threats are also reflected in voter behavior: when voters are subjected to the threat of violence, they often turn to more hawkish candidates (Berrebi and Klor 2008; Gadarian 2010; Getmansky and Zeitzoff 2014; Hadzic, Carlson and Tavits 2017; Merolla and Zechmeister 2009), which benefits men, who are traditionally viewed in this way (Falk and Kenski 2006; Holman, Merolla and Zechmeister 2016; Lawless 2004; Schroeder 2017; Swers 2007).

Finally, we contend that beyond active threats such as ongoing wars or terrorism, violence perpetrated during a civil war also introduces important variation in the salience of safety issues post-war. Fear of violence is likely to be more prevalent among residents of locales that experienced higher levels of wartime violence. Even after open hostilities cease, they should attach greater importance to continued safety. We expect their preferences to be colored by anxiety and fear generated by past violence, which in turn is likely to affect their political opinions and choices.

To summarize, we argue that safety concerns in high-violence communities provide a backdrop to individuals' preferences regarding who is best able to represent their communities. Anxiety and fear over safety elevate the importance of qualities that are traditionally associated with men (hawkishness, pragmatism, decisiveness). Women, however, will be perceived as less decisive, weaker and having less sound judgment when it comes to ensuring the community's well-being in locales traumatized by past violence. Therefore, individuals in high-violence locales are likely to trust men's judgement more and to prefer men in public roles more generally. This dynamic is then reflected in the electoral success of female candidates: we expect voters in high-violence locations to favor male candidates, making it harder for women to get elected. This generates our second hypothesis.

HYPOTHESIS 2: (Electoral Success): Communal exposure to wartime violence is negatively associated with the number of women elected to political office post-war.

The Case of Bosnia

Bosnia held its first competitive, multiparty elections in November 1990. National and local elections were held concurrently; this study focuses on the latter. After the elections, Bosniak (Bosnian Muslim) and Croat members of the government organized an independence referendum in February 1992. While voters overwhelmingly approved independence, the country's Serb population boycotted the referendum. Bosnian Serbs instead favored remaining part of Yugoslavia, which by this time was coming increasingly under the control of Serbia's Slobodan Milošević. Soon after the referendum, a civil war broke out in April 1992 and continued until December 1995, concluding with the internationally sponsored Dayton Agreement.

Bosnia is an appropriate test case for our study for three reasons. First, it is not an unusual case with respect to (a) the nature of the violence that occurred or (b) the state of citizens' attitudes toward women's representation. For instance, most modern civil wars are similarly inter-ethnic in nature (Sambanis 2001; Sarkees and Wayman 2010). This makes the Bosnian War a common conflict type. Also, popular attitudes toward women's political leadership in post-war Bosnia were quite similar to global attitudes. According to the World Values Survey (1995–1998), about 53 per cent of respondents in Bosnia and 47 per cent of respondents in all other countries agreed that 'on the whole, men make better political leaders than women do'. Furthermore, similar to many other post-conflict societies, Bosnia experienced a significant increase in women's representation following the conflict: the share of female local councilors increased from 5 per cent in 1990 to 18 per cent in 2000. This increase is likely due to the adoption of gender quotas in 1998 (Antić and Lokar 2006), which stipulated that for most party lists, around one-third of all candidates must be women, including one in the top three, two in the top six, and three in the top nine (Provisional Election Commission 2000).³

Secondly, Bosnia represents a difficult case for testing our argument. Due to the quota system, there is fairly little room for women's representation to vary widely across locations. Therefore, if we find a significant association between violence and women's representation, we can be more confident that this association is likely to be generalizable and even stronger in contexts with less aggressive, countrywide measures to increase women's representation. Another reason why Bosnia is a difficult case is that the conflict ended with outside intervention and the international community has actively promoted women's representation post-war. This likely further reduces potential variation in women's representation outcomes, thereby making it more difficult to find significant results.⁴

Finally, Bosnia is a suitable test case because fine-grained, subnational data on violence severity and detailed information on election results and candidate traits are often not available in post-war countries. Detailed pre-war data are even harder to find because these get lost or damaged during the war. Thus examining how wartime violence shapes representation is challenging and often impossible. The pre- and post-war data that we assembled on Bosnia offer a rare opportunity to study the relationship between violence and women's representation. We discuss the history of women's representation in Bosnia in more detail in the Appendix.

Data and Methods

We now test the hypotheses that violence has (1) a positive association with the number of women running for office and (2) a negative association with the number of women elected. To do so, we assembled data on electoral performance, violence severity and a number of control variables. All measures are at the pre-war municipal level ($n = 109$), the smallest electoral unit for which data are available.⁵ The number of municipalities in the analyses is 87 due to missing data. Descriptive statistics for the variables are presented in Appendix 2.⁶

We focus on the 2000 local elections. While the first post-war local elections were held in 1997, detailed data for those elections are not available. There is no reason to believe that anything occurred between 1997 and 2000 that may bias our analysis and make it more likely for us to detect significant results. If anything, by focusing on elections that were held after gender quotas

³The average party list contained thirteen candidates, four of whom were female.

⁴Bosnia is one case among many in which international actors have actively promoted women's representation (Bose 2002; Bush 2011; Hughes 2009; Hughes and Tripp 2015; Krook, O'Brien and Swip 2010; Tajali 2013).

⁵New municipalities were created after the war, but in most cases they were carved out of pre-war ones. This enabled us to simply pool the data at the pre-war level.

⁶In Appendix 3 we show that the municipalities that are excluded from our analysis appear to be quite similar to those that are included.

were adopted (in 1998), it may be more difficult to uncover significant relationships if the quota reduced variation in women's representation across localities.

We opted to use local rather than national elections because the latter are contested at the district level, and there are too few districts to conduct an empirical analysis. Moreover, conducting the analysis at the local level is arguably a harder test of the Electoral Success Hypothesis. Most studies that have examined the consequences of threat perceptions have focused on the national level, where the association between security and politics/policy is clearest. While local governments are also responsible for policy areas that concern security and safety (policing and crime prevention, street lighting, criminal justice, etc.), the connection between threat perceptions and voter preferences is less obvious. Therefore, we believe that uncovering a significant association between violence and women's representation (through voters' preferences) is more difficult when focusing on local elections.⁷

Dependent Variables

For the Candidate Supply Hypothesis, the dependent variable, *Candidates*, measures the percentage of candidates who were women in the 2000 local elections. We obtained the raw list of candidates from the Central Election Commission of Bosnia and Herzegovina and hand-coded the gender of each candidate. This variable ranges from 28.57 per cent to 38.13 per cent and has a mean of 32.41 per cent. The average number of total and female candidates was around 181 and 59, respectively.⁸ Unfortunately, data for *Candidates* were not available for pre-war elections, which is why we rely on cross-sectional analysis, controlling for other covariates that may influence pre-war women's engagement and representation, as described below.

The dependent variable for the Electoral Success Hypothesis is *Councilors*, which measures the percentage of elected councilors who were women in the 1990 and 2000 local elections. We obtained these data from pre-war statistical bulletins for 1990⁹ and from the electoral commission for 2000. To test the Electoral Success Hypothesis, then, we essentially have municipal-level panel data with information on *Councilors* pre- and post-war; the 1990 data provide a baseline measure of women's representation, which allows us to account for pre-war levels of support for women's political representation in our analysis.¹⁰ Across 1990 and 2000, the range for *Councilors* is

⁷The 2000 local elections were held on Saturday, 8 April 2000, and polls were open from 7:00am to 7:00pm. Like all Bosnian elections from 1996 to 2000, they were overseen by the OSCE. The Provisional Election Commission was responsible for administering the elections in co-operation with municipal election commissions and polling station committees. In casting their ballots, voters were allowed to support one party list and the distribution of votes for the lists determined how many seats were allocated to each party. Additionally, voters could indicate their preference for any number of candidates that appeared on the party list they supported (from none to all the candidates on the list). These preference votes determined the final order of the candidates on the list. We describe Bosnia's electoral system circa 2000 in more detail in Appendix 4.

⁸As noted earlier, the quota rule required around one-third of all candidates to be women, including one of the top three, two of the top six, and three of the top nine. It was common for parties to only meet that minimal requirement and simply place a female candidate in every third position on their list (that is, in the third spot, sixth, ninth, etc.). This practice explains why there is so little variation in *Candidates* (28.57 per cent to 38.13 per cent). However, this lack of variation should only make it more difficult for us to find support for the Candidate Supply Hypothesis. Additionally, the quota could not be fully applied to parties that fielded only a few candidates and to independent candidates. Due to this and other parties only meeting the minimal requirement, the minimum for *Candidates* is 28.57 per cent. In order to determine whether our results are driven by the presence of small parties or independents, we conducted an additional analysis that accounts for the average length of the lists in the municipality. These results, reported in Appendix Table 9.24, show that accounting for list length does not alter any of the conclusions.

⁹Republički Zavod za Statistiku - Socijalistička Republika Bosna i Hercegovina. 1991. *Rezultati Izbora u SR BiH 1990 - Statistički Bilten* 223.

¹⁰Unfortunately, we were not able to use vote shares as an additional dependent variable. While the electoral commission does provide data on the number of votes individual candidates received, it does so only for parties that had at least one candidate elected to the municipal council.

0–35.48 per cent (mean = 10.93 per cent). For 1990, the range is 0–17 per cent (mean = 4.56 per cent), while for 2000 it is 0–35.48 per cent (mean = 17.30 per cent). In raw terms, these averages correspond to around three and five female councilors in 1990 and 2000, respectively.¹¹

Independent Variables

We use publicly available data from the *Bosnian Book of the Dead* (BBD), released in 2007 by the Research and Documentation Center in Sarajevo, to construct our measures of local violence severity. The BBD contains data on the number of confirmed dead and missing during the Bosnian War at the pre-war municipal level. Using these data, we calculated the total number of confirmed dead and missing as a percentage of the pre-war population. We refer to this measure of violence severity as *Casualty*. The variable ranges from 0.12 to 10.33 and has a mean of 2.23. As a robustness test, we also calculated the total number of confirmed dead (not including missing) as a percentage of the pre-war population and refer to this measure as *Confirmed Dead*. This variable ranges from 0.11 to 6.77 and has a mean of 1.82.¹² Due to severe right-skewness in both measures, we log *Casualty* and *Confirmed Dead* in the analyses that follow (Gates et al. 2012; Lancina 2006).

Control Variables

We control for a number of potential confounders that may bias any potential results for the Candidate Supply Hypothesis. Of course, like with any study that uses observational data, we can never be completely confident that we are accounting for all the relevant factors. Therefore, we want to reiterate that this study ultimately presents evidence of associations rather than causal effects.¹³

First, we create the variable *1990 Councilors* to control for the percentage of councilors elected in the 1990 local elections who were women. Recall that for the Electoral Success Hypothesis, this information is already incorporated into the panel structure (that is, the dependent variable is measured at two time points). Because pre-war data on the gender distribution of the candidates are not available, *1990 Councilors* is included as a control in all models that correspond to the Candidate Supply Hypothesis.

Our models also account for additional potential confounders, that is, pre-war factors that may explain variation in both the independent (*Casualty* or *Confirmed Dead*) and dependent variables (*Candidates*) (Watt and van den Berg 2002). These include municipal-level indicators for urbanization level, income per capita, ethnic polarization and the percentage of the population composed of women. Our data source for these controls is the 1992 *Statistical Almanac of the Republic of Bosnia and Herzegovina*, which contains the 1991 Bosnian census (the last census of the pre-war period) and other relevant information. Due to space constraints, we include a more detailed discussion of these controls in Appendix 7.

¹¹The average district magnitude was fifty-six and twenty-nine seats for 1990 and 2000, respectively. While the average district magnitude decreased post-war, this should only be a potential concern if violence is correlated with district magnitude or changes in magnitude. In Appendix 5, we show that these correlations are all statistically insignificant.

¹²Rather than using the raw number of casualties, we instead employ the number as a *percentage of the population* because we believe this is a better indicator of violence severity. For instance, the average *number* of casualties across our sample is 775. This would correspond to almost 19 per cent of the population in the smallest municipality but only 0.4 per cent in the largest. By accounting for population size, our violence measures capture how a comparable number of casualties can nevertheless correspond to very different experiences among a municipality's residents.

¹³Following the advice of Lenz and Sahn (2018), we present minimal specifications in Appendix 6. These results show that we detect the expected associations between violence and women's participation and representation in the absence of any control variables.

Method of Analysis

To estimate the association between violence and the number of women *running* for office (Candidate Supply Hypothesis), we use ordinary least squares regression and the following model specification:

$$\text{Candidates}_i = \beta_0 + \beta_1 \text{Violence}_i + \beta_{2,6} \text{Controls}_i + \epsilon_i,$$

where i indexes the municipality, β_1 represents the estimate for the relevant violence measure, and ϵ_i is the stochastic error term.

We use a difference-in-differences regression to estimate the association between violence severity and the number of women *elected* to office (Electoral Success Hypothesis). This approach allows us to compare how *Councilors* differs between pre-war (1990) and post-war (2000) elections across municipalities that experienced varying levels of violence. Note that this is not simply a model of how the percentage of female councilors changed over time, but instead a technique that estimates the change in the trends. In other words, the share of female councilors could be on the rise/decline in both high- and low-violence municipalities, but if the change is significantly different in high- than in low-violence municipalities, then this difference-in-differences is attributed to the level of violence experienced in those municipalities. However, this approach relies on the admittedly strong assumption that the trends in women's representation in the municipalities were (and would have continued to be) parallel absent the violence. In order to address this specific problem, we account for municipal heterogeneity by including municipality fixed effects in the estimation. More specifically, we use the following model specification:

$$\text{Councilors}_{it} = \gamma_i + \beta_1 (\text{Violence}_i \times \text{Post-War}_t) + \beta_2 \text{Post-War}_t + \epsilon_{it},$$

where i indexes the municipality, γ_i represents the municipality fixed effects, Post-War_t is an indicator for the election year (2000 election = 1, 1990 election = 0), and β_1 is the violence estimate. ϵ_{it} is the stochastic error term.¹⁴

We also use robust standard errors, clustered on municipality and election year, to account for correlation in observed outcomes within a particular municipality across elections and between municipalities during a particular election. Finally, to further address concerns about a lack of randomness in our independent variable (*Violence*), we also report various balance tests in Appendix 8. There are no large pre-war imbalances between high- and low-violence municipalities across most theoretically relevant variables, including pre-war women's representation, urbanization levels, income per capita and women as a share of the pre-war population. The only exception is ethnic polarization: high-violence municipalities are somewhat more ethnically polarized than low-violence ones. This all ultimately suggests that our modeling strategy is unlikely to overestimate the association between wartime violence and women's representation. Nevertheless, for the results we are about to report, we continue to exercise caution in attributing any causal effects to violence. Because both hypotheses are directional in nature and the sample size is small (87 municipalities), we opted to use one-tailed t -tests for our main analyses.

Results I: Candidates

We begin by testing the Candidate Supply Hypothesis, that violence exposure is positively associated with the number of women running for office. The results, presented in Table 1, are consistent with expectations: the more severe the violence, the higher the percentage of candidates who are women.

¹⁴Because the difference-in-differences method requires measurements of the dependent variable in both the pre- and post-treatment periods, we are unable to use this technique to test the Candidate Supply Hypothesis. As noted earlier, data on the number of women *running* for office are only available for the 2000 elections.

Table 1. Wartime violence and female candidates

| Variables | Model 1: Candidates | Model 2: Candidates |
|-----------------------|------------------------|------------------------|
| log(Casualty) | 0.560* (0.253) | |
| log(Confirmed Dead) | | 0.651* (0.275) |
| Urban Share | 0.051* (0.012) | 0.049* (0.012) |
| Income per Capita | -0.065 (0.089) | -0.067 (0.088) |
| Ethnic Polarization | -0.927 (0.913) | -1.008 (0.909) |
| Share of Women | -0.099 (0.243) | -0.066 (0.243) |
| 1990 Councilors | 0.111* (0.051) | 0.111* (0.051) |
| Constant | 35.870* (12.123) | 34.405* (12.099) |
| Municipalities | 87 | 87 |
| <i>N</i> | 87 | 87 |
| <i>R</i> ² | 0.411 | 0.415 |

Note: cell entries represent unstandardized coefficient estimates with standard errors in parentheses. The dependent variable is *Candidates* (on 0–100 scale). One-tailed *t*-tests were used. **p* < 0.05

In substantive terms, increasing $\log(\text{Casualty})$ by a value equal to the difference between its minimum (−2.15) and maximum (2.34) values is associated with an approximately 2.51-point increase in the percentage of candidates who are women. Given that in the average municipality 181 candidates appeared on the ballot, this increase corresponds to approximately 4.5 more women running for office. Performing the same exercise with $\log(\text{Confirmed Dead})$ yields similar results (2.68-percentage-point increase corresponding to roughly 4.8 additional female candidates).

While the magnitude of these estimates may seem modest, it is important to remember that the gender quota rule applied to the 2000 local elections. As noted earlier, the quota required that at least around one-third of the candidates on most lists were women. The fact that we continue to detect significant associations even in the presence of aggressive reforms designed to increase representation nationwide is quite reassuring. We suspect that in the absence of such reforms, the associations we present would be even more pronounced.

These results remain robust to the inclusion of a number of controls, including the level of pre-war women's representation. Also, the robustness tests that we detail below and in Appendix 9 show that the results still hold when employing a different modeling strategy, using additional controls that account for pre-war gender inequality and after accounting for war-time demographic change.

Results II: Councilors

We now test the Electoral Success Hypothesis, that exposure to violence is negatively associated with the number of women elected to office. The results, presented in Table 2, confirm that exposure to violence is negatively correlated with the electoral success of female candidates. Specifically, the difference-in-differences estimates are statistically significant and equal −1.947 and −2.217 when employing $\log(\text{Casualty})$ and $\log(\text{Confirmed Dead})$, respectively.

Substantively, these estimates are fairly large in magnitude. For instance, increasing $\log(\text{Casualty})$ by a value equal to the difference between its minimum and maximum values is associated with an 8.73-point decrease in the percentage of councilors who are women. The average post-war municipal council is composed of roughly twenty-nine members, so this decrease corresponds to around 2.5 fewer women being elected. Repeating this exercise with $\log(\text{Confirmed Dead})$ yields a 9.12-percentage-point decrease, which corresponds to around 2.6 fewer female councilors.

These results are especially striking when we consider how exposure to violence is associated with more women running for office. Yet, despite the larger supply of female candidates, fewer are

Table 2. Wartime violence and female councilors

| Variables | Model 1: Councilors | Model 2: Councilors |
|--------------------------------|------------------------|------------------------|
| log(Casualty) × Post-War | -1.947* (1.039) | |
| log(Confirmed Dead) × Post-War | | -2.217* (1.153) |
| Post-War Municipalities | 13.715 (1.196) | 13.546 (1.139) |
| <i>N</i> | 87 | 87 |
| <i>R</i> ² | 174 | 174 |
| | 0.915 | 0.915 |

Note: cell entries represent unstandardized coefficient estimates with clustered (on municipality and election year) robust standard errors in parentheses. The dependent variable is *Councilors* (on 0–100 scale). One-tailed *t*-tests were used. The intercept term is dropped to allow for municipality fixed effects (not shown). **p* < 0.05

actually elected. Our results also suggest that while gender quotas are effective at increasing the number of women seeking office, they may be much less successful at mitigating factors that reduce the number elected (Jones 2005; Paxton, Hughes and Painter 2010). Even in the presence of aggressive reforms intended to increase representation, we find that violence has a negative and significant association with women's representation. This is consistent with our argument that wartime violence shapes voter biases in favor of male candidates.

Robustness Tests

We performed several robustness tests to further substantiate our findings. First, because we rely on cross-sectional (rather than panel) data to test the Candidate Supply Hypothesis, we had to use controls instead of municipality fixed effects to account for differences between municipalities in the original analysis. For that reason, we conducted additional analyses that use alternative specifications of the models presented in Table 1. Appendix Tables 9.1 and 9.2 show that the conclusions do not change when controlling for population density or size instead of urbanization level. We further show in Appendix Tables 9.3–9.6 that the results remain the same when using alternative controls for ethnic diversity. We also re-estimated the models controlling for pre-war economic and social gender inequalities: *Employment Share* measures the percentage of pre-war public sector workers that were women¹⁵ and *Illiteracy Share* captures the percentage of the pre-war illiterate population that was female. Appendix Table 9.7 shows that controlling for these gender inequalities does not change any conclusions.

Next, we estimated a series of regressions that accommodate the bounded nature of the dependent variables (neither *Candidates* nor *Councilors* can be lower than 0 or exceed 100). The results, presented in Appendix Tables 9.8–9.10, show that violence exposure continues to be associated with (a) more women running for office and (b) fewer women getting elected.

Because *Candidates* measures the *percentage* of candidates who are women, we also wanted to address concerns that our first set of findings is driven by fewer men (rather than more women) running for office. We do so by estimating count models in which the dependent variable is the *number* of men or women running and present the results in Appendix Table 9.11. The pattern that emerges is that violence exposure is associated with both more men and women running, but the violence estimates are somewhat larger for women than they are for men.

Turning to the second set of results, we explored whether the negative association between violence and the electoral success of female candidates is due to voters preferring combat veterans, who are mostly male and also more common in high-violence locales. Without data on the veteran status of the candidates, this is obviously a difficult issue to address. However, we tried to do so by calculating various measures of how 'militarized' the violence in a municipality was. We

¹⁵Data disaggregated by gender are not available for the pre-war private economy.

calculated (a) the share of casualties that were military as opposed to civilian and (b) the number of military casualties as a percentage of the population *minus* the number of civilian casualties as a percentage of the population. We use these measures to proxy for how involved military personnel were in the violence that occurred (at a given level of overall violence). We then replicated Table 2 employing these measures as independent variables. The results, presented in Appendix Table 9.12, show that the degree of militarization is not associated with the electoral success of women.

Finally, we considered whether we are committing an ecological fallacy by attributing the lack of women's electoral success in high-violence locales to reduced voter support for women's leadership. More specifically, even if the share of votes female candidates cumulatively receive is held constant, how concentrated these votes are can still affect the number of women who are elected.¹⁶ However, this should only be a concern if violence is associated with how concentrated votes are for female (or male) candidates, and we have no theoretical reason to suspect that they are.

Nevertheless, in order to address this potential issue, we used the partial vote shares data discussed earlier (footnote 10) to create a measure of how concentrated the votes received by women were. For each municipality, we calculated a fractionalization index that captures the probability that two randomly selected votes that were cast for women were cast for *different* women. Higher (lower) values indicate that the votes received by women were more dispersed (concentrated). We did the same for votes received by men. We then used these indices as dependent variables in models presented in Appendix Tables 9.13 and 9.14. As these results show, violence is not associated with the level of vote concentration for women or men. We should state that these are only suggestive analyses, given that the data are partial and do not include parties that failed to have any councilors elected. Nonetheless, despite this limitation, the fact that we do not detect an association between violence and vote concentration (for men or women) is still reassuring.

Demographic Change

We also addressed two additional concerns in more detail. First, we considered a potential alternative channel through which violence can shape post-war women's representation. Because wartime casualties tend to be disproportionately male (Buvinic et al. 2013), demographic changes occur in wartime that create opportunities for women to enter previously male-dominated realms to an extent not possible pre-war. Therefore, more women entering politics as candidates post-war may simply be a consequence of gender imbalance in the population rather than the channels we propose in our study. To explore this possibility, we conducted an additional analysis to capture the association between violence and women's participation and representation, independent of the demographic changes induced by war.

First, in order to construct a measure of post-war gender balance, we use data from the 2013 Bosnian census (the only one conducted in the post-war period) to calculate the share of each municipality's post-war population that is female. We then conducted a direct-effect analysis as described in Acharya, Blackwell and Sen (2016) to examine whether violence exposure continues to be associated with a larger supply of female candidates after accounting for the post-war gender balance. In doing so, we estimate the direct effect of our independent variable (*Casualty* or *Confirmed Dead*) on the dependent variable (*Candidates*) that is not mediated by a post-treatment variable (gender balance). Unlike regression analysis that controls for a post-treatment variable, this approach is designed to produce unbiased estimates. The results of the direct-effect

¹⁶For example, imagine two municipalities (A and B) that both have a district magnitude of four. In municipality A, one woman receives 90 per cent of the vote while the remaining 10 per cent is distributed among men. In municipality B, two women each receive 45 per cent of the vote and the remaining 10 per cent is again distributed among men. Despite women receiving the same share of the vote in the two municipalities, *Councilors* would equal 25 per cent in A (one woman, three men) and 50 per cent in B (two women, two men).

analysis are presented in Appendix Table 9.15 and show violence exposure continues to be associated with a larger supply of female candidates.

In Appendix Table 9.16, we present the results of the direct-effect analysis that accounts for gender balance while examining the relationship between violence and women's electoral success. For this analysis, the mediator (gender balance) assumes the pre-war values (from the 1991 census) for observations that correspond to the 1990 elections and post-war values (from the 2013 census) for observations that correspond to the 2000 elections. Once again, accounting for the demographic gender balance does not change the results. We continue to find that violence exposure is associated with fewer women getting elected. In sum, these analyses reassure us that the findings are not an artifact of the demographic changes induced by war.

Party List Placement

We further scrutinize our finding regarding women's electoral success by examining whether the association is due to women's list placement. As noted earlier, the fact that violence is positively associated with the number of women running should make it more difficult to detect a negative association for the number who are elected. However, what if, despite the larger supply of female candidates, women are placed in less desirable list positions? As prior work shows, parties play a crucial role in how engagement translates into representation (Lühiste 2015; Norris and Lovenduski 1995; Rahat 2007). Even as more women run in locales that experienced significant violence, it is possible that parties (rather than voters) discriminate against female candidates by placing them in 'fighting' or 'ornamental' list positions (Skard and Haavio-Mannila 1985). Although open lists allow voters to alter candidate order through preference votes, where candidates originally appear on the list can still affect who is elected if enough voters do not have strong candidate preferences.

To address this concern, we created multiple measures of women's list placement and examined their relationship to violence. For the first measure, *Relative Placement*, we calculate a Borda count score for every candidate in a particular municipality. Then, we sum the scores across the municipality for all male candidates, do the same for female candidates, and subtract the latter from the former. We then finally divide this value by the total number of candidates. As *Relative Placement* increases, men's list placement improves relative to women's placement.¹⁷

Using *Relative Placement* as the dependent variable, we ran a series of models that explore the relationship between violence and women's list placement. As the results from Appendix Table 9.17 show, the estimates for our violence measures are negative and statistically significant. This holds regardless of whether *Candidates* is included in the model as a mediator or not, suggesting that violence may actually improve women's list placement.

We also constructed an alternative measure of women's list placement, *Top 10*, which captures the percentage of candidates who were placed in a top 10 position on a list who are women. Using *Top 10* as the dependent variable but otherwise keeping all modeling choices the same, the results are presented in Appendix Table 9.18. Once again, the estimate for violence severity is always positive and significant, indicating that violence is associated with more women being placed in desirable list positions.¹⁸

¹⁷For example, imagine all candidates in a municipality appear on one of two lists, List A or List B. List A consists of six candidates and men are in the first, second and fifth positions. List B consists of five candidates and men are in the first, third and fourth positions. For male candidates, the cumulative Borda count for List A is 13 (6 + 5 + 2) and 10 (5 + 3 + 2) for List B. Therefore, the score for male candidates is 23. For female candidates, the cumulative Borda count is 8 (4 + 3 + 1) and 5 (4 + 1) for Lists A and B, respectively. The score for female candidates is therefore 13. Next, we subtract the female score from the male one (23 - 10) and divide this value by the total number of candidates across both lists (13/11). This process yields a value of roughly 1.18, indicating men are placed better than women in that municipality.

¹⁸We also calculated measures of women's list placement for the top 5, 4, 3, 2 and 1 candidates. The results from Appendix Tables 9.19–9.23 show that violence either has a null effect on placement or is associated with women being better placed.

In sum, these additional analyses indicate that exposure to wartime violence does not lead parties to discriminate against female candidates. This, together with our finding that violence is associated with more women running for office, suggests that neither the supply of candidates nor party gatekeepers holds back women's political representation post-war. Voter-level biases in favor of male candidates, possibly stemming from lingering security and safety threats as we theorized, remain the most plausible explanation for the negative association between violence and the electoral success of female candidates.

Conclusion

We argued that exposure to wartime violence has important implications for post-war women's representation. More specifically, we expected the association between violence and representation to differ across stages. Whereas violence provides women with opportunities to acquire experience and skills that make them formidable political candidates, which is reflected in a higher number of women running for office, we also argued that violence hinders women's electoral success by reducing voter support for women's leadership. We proposed that this reduced voter support is a consequence of higher threat perceptions among residents of high-violence communities combined with gender stereotypes about male and female politicians. Using pre- and post-war electoral as well as wartime violence data from Bosnia, we presented evidence that is consistent with both parts of our argument.

Our findings make four main contributions to a number of literatures. First, previous studies on the determinants of women's representation have tended to address institutional and attitudinal factors such as the electoral system, gender quotas and prevailing views about gender roles (Iversen and Rosenbluth 2010; Paxton, Hughes and Painter 2010). We instead focus on the role of violence in a post-conflict context.

Secondly, prior studies about the gendered effects of conflict have mostly concerned themselves with the sociodemographic and economic consequences of violence, including health, labor market dynamics and educational attainment (Acemoglu, Autor and Lyle 2004; Annan et al. 2011; Goldin 1991). We contribute by addressing important political outcomes.

Thirdly, research that does address the gendered *political* legacies of conflict has tended to focus on the institutional changes, and particularly the adoption of gender quotas, brought about by conflict. By using subnational electoral data, we are able to hold institutional features constant and therefore come closer to disentangling the institutional and attitudinal factors that shape representation. Our focus on attitudes moves the study of the gendered political legacies of conflict forward and adds much-needed nuance to our understanding of how violence may shape women's representation differently at various stages of the electoral process.

Fourthly, our article makes an important contribution to the literature on stereotypes and vote choice. Numerous studies have found that in the US context, voters do not systematically discriminate against female candidates (Burrell 1994; Darcy, Welch and Clark 1994; Fox 2000). In fact, because men and women tend to be associated with particular issue competencies (Kahn 1996), the electoral context can mediate voter bias in ways that favor female candidates (Burrell 1994; Sanbonmatsu 2002). In this article, we do not claim that gender stereotypes in and of themselves disadvantage women. Rather, in line with other work, we argue that stereotypes *in combination with* the electoral context can have important consequences for women's representation (Barnes, Branton and Cassese 2017; Fulton 2012; Lawless 2004; Sanbonmatsu 2002). In this instance, we contend that wartime violence, by increasing the salience of safety and demand for 'male' leadership, creates an electoral context that is inhospitable to female candidates. Future studies should likewise consider how context interacts with stereotypes in ways that influence important representational outcomes.

Finally, we recognize that our study has limitations that future research can address and build upon. First, we have been careful throughout the article to stress how we are ultimately presenting

evidence of associations rather than causal effects. Secondly, while we believe and argued that Bosnia is a good test case for a number of reasons, without conducting similar research in alternative settings we cannot be completely confident that our findings apply more broadly across post-conflict societies. Thirdly, we did not present direct evidence of the mechanisms proposed in this article (that is, war preparing future candidates, violence reducing voters' support for women's leadership). Further research should therefore identify cases or events that provide more causal leverage, address similar questions in different settings and probe the various potential mechanisms.

Supplementary material. Data replication sets can be found in Harvard Dataverse at: <https://doi.org/10.7910/DVN/XERE2A> and online appendices at: <https://doi.org/10.1017/S0007123419000619>.

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