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Absent laws and missing women: Can domestic violence legislation reduce female mortality?

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Abstract

This study explores the relationship between the presence of domestic violence legislation and the adult mortality of women relative to men. Using a panel of 159 economies between 1990 and 2014, domestic violence legislation is found to be associated with a lower women-to-men adult mortality ratio. The most conservative estimate suggests a 2.3% decline relative to the mean sample value. These findings are extended to show that domestic violence legislation is also negatively correlated with physical violence by intimate partners for a cross-section of economies. Sensitivity analyses reveal that the findings are robust to different estimation methods and empirical specifications.

KEYWORDS

domestic violence, gender, laws, mortality, women

JEL CLASSIFICATION

J1; J16; K32

1 | INTRODUCTION

In an influential article, Amartya Sen noted that over 100 million women were missing.[1] Despite biological advantages women have over men, in many places the ratio of women to men skews toward men. A large literature has attempted to explain the drivers of adult mortality and explain why there may be excess female mortality (Anderson & Ray, 2010; Cutler et al., 2006). This study focuses on one particular candidate from the set of potential predictors—domestic violence legislation. Fears of domestic violence have particularly risen since the COVID-19 pandemic began. Studies have noted a sharp increase in domestic violence as people shelter at home (Aguero, 2021; Leslie & Wilson, 2020). A strand of the literature has also attempted to quantify the deaths due to domestic

violence, acknowledging hurdles in obtaining clean measures. Anderson and Ray (2010) report that excess female deaths from "injuries"—in part resulting from domestic violence—were extremely high in India in the year 2000, equaling 225,000. This is far greater than maternal mortality estimates. The findings are likely to be on the conservative side given substantial under-reporting of domestic violence cases (Palermo et al., 2013).

Systematic and direct evidence on the impact of domestic violence legislation on female mortality or well-being in general is limited. Beleche (2019) finds that across provinces in Mexico, legislation criminalizing domestic violence is associated with a significant reduction in suicide rates among women. Other studies have shown that the one-child law in China and sex-selective abortion laws have significantly affected women-to-men mortality among newborns (Ebenstein, 2010). Factors such as education, employment, and political empowerment have been found to be important in affecting the status of women, and thereby mortality rates. However, no study as of this writing provides any direct evidence on the effect of domestic violence legislation on adult women mortality.

The number of economies implementing domestic violence legislation has increased dramatically since 1990. Only 4 of the 159 countries in this study had implemented domestic violence legislation in 1990. By 2014 this number rose to 89 economies. Little is known about the factors that contribute to the adoption of domestic violence legislation. Suggestive evidence[2] indicates a number of factors: pressure exerted by those favoring a patriarchal structure, the presence of a wide and strong coalition in favor of the legislation, and commitments to international conventions such as Committee to Eliminate All Forms of Discrimination against Women (CEDAW) and the Council of Europe Convention on preventing and combating violence against women (the Istanbul Convention). The literature on gender disparity in the broader set of laws suggests that factors such as the momentum created by international conventions like the CEDAW, strong rule of law, women's political empowerment, mobilization of women's networks, and increasing labor force participation may promote the adoption of domestic violence legislation (see, e.g., Hallward-Driemeier et al., 2013a; World Bank, 2015).

This study contributes to the literature by estimating a reduced form relationship between domestic violence legislation and women-to-men adult mortality ratio. Using panel data for 159 economies between 1990 and 2014, this study finds that the presence of domestic violence legislation is associated with significantly lower women-to-men adult mortality ratio. Much attention is afforded to endogeneity concerns, and various methods are used to address them. Despite these robustness checks, due caution is necessary in interpreting the results as causal. The finding is robust to a host of controls including overall economic development, health and disease prevalence, political empowerment, education, employment, institutional environment, and gender-specific laws. We complement these robustness checks with a falsification test where we replace the outcome variable with labor force participation. We also use an instrumental variables (IV) approach. We use the human rights conventions that specifically target violence against women in generating legislations to address domestic violence to instrument for the adoption of domestic violence legislation, while accounting for general conventions that promote gender equality overall. Our main findings are confirmed with the IV approach. Our conservative baseline estimate suggests that domestic violence legislation lowers women-to-men adult mortality ratio by about 2.3% of its mean value.

2 | CONCEPTUAL FRAMEWORK

Economists have generally relied on the dependency theory and intra-household bargaining model of domestic violence to motivate their empirical analysis. The dependency theory, originally attributed to Gelles (1976), posits that women with more resources tend to have better options outside of abusive

partnerships and are therefore more likely to leave violent partners. Building on this insight, the intra-household bargaining model argues that women with better outside options relative to men have higher threat points that enable them to credibly threaten to leave partners and therefore essentially bargain for less violence. This prediction is general and not dependent on the specifics of the bargaining model (see, e.g., Papageorge et al., 2016). The bargaining outcome and therefore the equilibrium level of domestic violence are also affected by factors that determine the utility of violence to men and disutility to women. We follow the tradition of using the bargaining framework to motivate the reduced form empirical analysis.

We posit that domestic violence legislation improves women's threat point thereby lowering domestic violence and women's mortality relative to men. However, there are studies that run counter to the finding that greater autonomy leads to lower domestic violence. For instance, Eswaran and Malhotra (2011) find some evidence for the evolutionary theory where greater female autonomy leads to paternity uncertainty that triggers spousal insecurity and jealousy and thereby violence as a response. By raising the cost of inflicting violence, legislation against domestic violence could still curtail violence against women arising from paternity uncertainty.

The literature on gender-based violence highlights several factors that could affect the bargaining outcome by altering either the outside options available to women or the associated utility or disutility of violence (see, e.g., Heise, 1998; Larsen, 2016; Rodriguez-Menes & Safranoff, 2012 for literature review). Using an integrated, ecological approach for understanding the drivers of gender-based violence, Heise (1998) classifies the drivers into four groups. The first group includes ontogenic or personal factors that an individual brings to his or her behavior or relationships. For example, men who witnessed marital violence as a child were subject to child abuse, or had a rejecting father could have a higher proclivity to inflict violence on women (see, e.g., Bowlus & Seitz, 2006; Pollak, 2004). Other personal factors may include education and health. For instance, Papageorge et al. (2016) argue that better health improves longevity, which incentivizes women to invest in their future. Such investment improves women's outside options thereby lowering the possibility of being trapped in partnerships prone to domestic violence. This is empirically validated for the USA. The enforcement of domestic violence legislation allows women to leave hostile circumstances, protects them from further abuse, and may provide for monetary compensation. Within the bargaining framework, these effects raise women's threat point and thereby likely to reduce domestic violence arising from personal factors.

The second group of factors as pointed out by Heise (1998) stems from the microsystem, which is defined as the individual's interaction with his or her immediate surroundings. Perhaps the most important factor in the microsystem is the family. Studies have shown that men raised in patriarchal families are much more likely to inflict violence on women than others. While violence against women could be an end in itself, in some cases it is a means for men to get greater control over the household resources (see, e.g., Atkinson et al., 2005; Rodriguez-Menes & Safranoff, 2012). Thus, domestic violence can increase with an improvement in women's outside options as men try to regain control lost due to women's improving outside options. However, recent studies show that such male backlash occurs only when men hold patriarchal views (Atkinson et al., 2005; Yilmaz, 2018) or when social institutions protecting women are weak (Luke & Munshi, 2011). Thus, by strengthening the institutional environment protecting women, domestic violence legislation may reduce domestic violence. Other elements of the microsystem discussed in the literature include culture of male control over family resources, marital conflict, use of alcohol, and dissatisfaction with marriage (see, e.g., Bloch & Rao, 2002; Larsen, 2016). For these factors, domestic violence legislation would fall under the set of equalizing institutions that lower domestic violence.

The third group of factors belongs to the exosystem or the institutions and social structures both formal and informal that impinge on the immediate settings in which a person is found and thereby

influence, delimit, or determine what goes on there (Belsky, 1980). As pointed out by Heise (1998), these factors often result from changes taking place in the larger social milieu. Some important elements of the exosystem found to impact the level of domestic violence include social isolation of women, peer group behavior and attitudes, poverty and economic stress, socioeconomic status of men both in absolute and relative terms, and quality of institutions like courts, police, and laws that protect women (see, e.g., Aizer, 2010; Anderberg et al., 2016; Beleche, 2019; Fox et al., 2002; Larsen, 2016; Stevenson & Wolfers, 2006). In terms of legal reforms, the study by Stevenson and Wolfers (2006) is illustrative. The authors analyze the impact of unilateral divorce reform in the USA during the 1970s and 1980s on domestic violence. The key mechanism is that by allowing women to seek divorce without the consent of their husband, the reform provided better outside options to women thereby lowering domestic violence. Comparing states that implemented the reform versus. those that did not, they find strong evidence in favor of their mechanism. We expect similar channels to be at play for the effect of domestic violence legislation on mortality.

The last group of factors belongs to the macrosystem that refers to the broad set of cultural values and beliefs that permeate and inform the other three groups of factors mentioned earlier. Some important macrosystem factors include the notion of masculinity based on toughness, dominance, and male honor; rigid gender roles along traditional lines; the sense of ownership and entitlement that men have over women; and cultural ethos that condones violence to settle interpersonal disputes. The importance of macrosystem in influencing ontogenic, microsystem, and exosystem factors has been discussed in the literature. For instance, Heise (1998) notes that male supremacy, a macrosystem factor, would likely influence the organization of power in community institutions as well as distribution of decision-making authority in intimate relationships. Larsen (2016) and Rodriguez-Menes and Safranoff (2012) also note that male domination observed in patriarchal societies is in part exerted through the prevailing social and cultural norms. Agarwal (1997) goes beyond and argues that social norms impact not just individual behaviors but also other important elements of the bargaining model including what can be bargained and the threat points of the individuals. Therefore, better laws such as domestic violence legislation would protect women from violence resulting from social norms and cultural values and beliefs.

Since women are invariably the victims of domestic violence, men constitute a reasonable comparison group.[3] As part of our empirical strategy, we use the adult female to male mortality rate as our dependent variable, which would be unaffected by factors common to both male and female mortality. Thus, the omitted variable bias problem issue is largely due to factors correlated with domestic violence legislation that disproportionately affect the mortality rate for women compared to men. These factors may affect the women-to-men mortality ratio either directly or through their impact on domestic violence as discussed earlier. We identify these factors by drawing on the existing strands of literature on bargaining models of domestic violence, factors that drive countries to adopt legislation against domestic violence, and the broader literature on female mortality.

The empirical strategy must account for several factors at the octogenic, microsystem, exosystem, and macrosystem levels to avoid omitted variable bias, as these factors may vary systematically between countries with and without domestic violence legislation. Indirect factors that affect the women-to-men mortality ratio either through their impact on domestic violence or other channels have also to be accounted for. For instance, the fertility rate may directly affect the women-to-men mortality ratio. It may also indirectly alter the condition and status of women. Failure to control for fertility rate could cause omitted variable bias problem if it varies systematically between countries that have implemented domestic violence legislation and those that have not. Similarly, overall economic development and social conditions are likely to exert a substantial effect on women's mortality and outside options, perhaps more so than for men (Pritchett & Summers, 1996).[4] Better institutional

environment in terms of democratic accountability, law and order, and so forth may constrain the unequal distribution of power within households that favors men over women (as described by dependency theory of domestic violence). Improving the institutional environment may lead to a better bargaining outcome for women through several channels such as better health outcomes and therefore lower women-to-men mortality. It is also possible that better-functioning institutions and a more responsive government may generate better services such as healthcare that is likely to benefit the relatively underprivileged, which happen to be women in many economies. The political empowerment of women may lead to more favorable laws for women and women legislators becoming role models for other women to improve their conditions (Chatopadhyay & Duflo, 2004; Ghani et al., 2013; Hallward-Driemeier et al., 2013b; Macmillan et al., 2018).

Our panel estimation provides the first defense against various sources of omitted variable bias problem. The panel estimations control for country-specific and time invariant factors through country dummy variables (country fixed effects). Thus, differences across countries in aggregate characteristics of the population, culture, quality of institutions (that protect women), social norms, and so forth that are relatively stable over time are accounted for in the country fixed effects. The panel estimation also controls for year fixed effects, eliminating global shocks that may alter the mortality rate of women relative to men. Country and year fixed effects do not control for factors that change over time in a country or vary across countries in a year. Thus, we account for several factors that impact the women-to-men mortality ratio and could be correlated with the adoption of domestic violence legislation. These include education (proxy for wages) and labor force participation rate among women relative to men; gender-specific measures of health (prevalence of HIV among women relative to men, fertility rate) and general measures of healthcare availability (health expenditure per capita and immunization rates among children); level of urbanization[5]; level and growth rate of GDP per capita; socioeconomic conditions based on poverty rate, unemployment and consumer confidence in the economy; presence of women relative to men in the lower house of the parliament as a proxy for women's political empowerment; general attitudes toward equality, as reflected by ratification and conventions such as the CEDAW and other gender-specific provisions in the constitution; and the broader institutional environment in terms of the democratic accountability, corruption, bureaucracy quality, internal conflicts, and economic and political risks in the country.

3 | DATA AND MAIN VARIABLES

Data for domestic violence legislation are obtained from the World Bank's Women Business and the Law database (henceforth, WBL). Data on mortality rates and other control variables are obtained from the World Development Indicators (WDI) as well as other sources. Our main specification and the IV specification take advantage of the panel nature of the data by including both country and year fixed effects. The sample used is an unbalanced panel of up to 159 countries for which data are available. Table A1 in the Online Appendix provides the list of countries included in the sample. The time covered is 1990–2014. We estimate the following reduced form equation:

$$Y_{it} = \alpha + \beta X_{it-1} + \mu Z_{it} + w_t + v_i + u_{it}\ldots\ldots \quad (1)$$

Subscripts i and t denote the country and year, respectively. Y is the dependent variable, and X is the presence of domestic violence legislation (lagged). Z is the vector of various controls that vary across countries and years. w_t and v_i denote the year fixed effects and country fixed effects, respectively; u_{it} is the error term. In the Online Appendix, Table A2 contains the summary statistics of the main variables

used in the regressions, correlations between explanatory variables are provided in Table A3, and a formal definition of the variables used is provided in Table A4.

Ideally, the dependent variable would be adult mortality rate due to domestic violence. However, data for this variable are scarce and unreliable due to gross under-reporting. Thus, we use overall adult mortality rates that bias our results downward especially if domestic violence has low prevalence.[6] Our main dependent variable equals adult mortality rate for women (per 1,000 adult women population) divided by adult mortality rate for men (per 1,000 adult men population) obtained from the WDI. Using the ratio of women-to-men adult mortality rate (henceforth, women-to-men mortality ratio) has the advantage as it eliminates factors common to both men and women mortality from spuriously affecting our results. The mean value of the dependent variable equals 0.66, implying that the incidence of mortality is lower among women, equaling on average 66% of the same for men. The lower mortality rate among women versus. men holds for most of our country-year sample points reflecting biological factors. One disadvantage of using women-to-men mortality ratio is that it says little about the impact on the absolute mortality rate of women. Thus, we also report results for absolute adult mortality rate for women as the dependent variable.

Our main explanatory variable is a binary variable equal to 1 if the country has domestic violence legislation in place in the previous year and 0 otherwise. The variable is obtained from the WBL. A country is considered to have domestic violence legislation if there is specific legislation addressing violence between spouses, within the family or members of the same household, or in interpersonal relationships, including intimate partner violence that is subject to criminal sanctions or provides for protection orders for domestic violence. Following the UN, WBL defines domestic violence to include physical violence, emotional or psychological violence, sexual violence, or financial or economic violence (World Bank, 2015). Legislation that does not provide for sanctions or orders of protection against domestic violence is not considered to satisfy the criteria of having domestic violence legislation. The mean value of the domestic violence legislation dummy variable equals 0.35, implying that on average in any given year about 35% of the countries have domestic violence legislation in place. At the beginning of the sample time period (1990), only 4 countries had domestic violence legislation compared with 89 at the end of the sample period, 2014 (see Tables A5 and A6 in the Online Appendix for details).

We pay due attention to endogeneity concerns. Regarding reverse causality, we are unaware of any formal work on how a change in women-to-men mortality ratio may prompt countries to implement domestic violence legislation. One possibility is that an increase in the mortality ratio due to higher domestic violence may force politicians to address the problem by implementing legislation against domestic violence. If this were true, it would imply a positive relationship between women-to-men mortality ratio and the adoption of legislation against domestic violence. Note that this implies that our findings are conservative, given that the negative relationship that we find between the two is attenuated. We caution that a positive feedback effect from women-to-men mortality ratio to the adoption of legislation against domestic violence discussed here is a mere possibility and not a given. It requires further research to ascertain or reject it. Furthermore, the domestic violence legislation variable in the estimations is lagged by 1 year. Thus, any contemporaneous or future effects of women-to-men mortality ratio on the decision to adopt legislation against domestic violence do not impact our estimation results.

A relatively more serious concern is the omitted variable bias. We address the problem in several ways. These include using various controls and estimation methods including country fixed effects, IV estimation, propensity score matching (PSM) estimation, and a falsification test. Nevertheless, due caution is needed in interpreting our results as necessarily causal. The motivation for the controls is provided in Section 2, and their detailed definition is available in Table A4 in the Online Appendix.

We briefly list the controls here for reference. In the baseline model, the controls include country dummies (country fixed effects) and year dummies (year fixed effects); (log of) GDP per capita (at constant US$2005); annual real growth rate of GDP per capita; ratio of women to men labor force participation rate; ratio of number of years of education among adult (aged 25 years or more) women and men; proportion of population living in urban areas; a dummy variable equal to 1 if the country has ratified the optional protocol for CEDAW and 0 otherwise to capture other policy efforts toward gender equality; percentage of children in the age group of 12–23 months who are immunized against measles and the same against DPT as proxy measures of health; and fertility rate. Data source for education variable is Gakidou et al. (2010) and World Bank (2015) for the CEDAW ratification dummy. The remaining variables are taken from the WDI.

We complement the baseline controls with several others in the robustness section. However, this comes at the cost of a sharp decline in sample size due to missing data. These additional controls fall in the areas of health, political empowerment of women, institutions and governance, gender disparities in the laws, and socioeconomic conditions. For health, we use the percentage of females in the age group of 15–24 who have HIV relative to males; and a proxy measure of the quality of public health services which equals the (log of) total health expenditure per capita in the country (PPP adjusted and in constant 2011 international dollars). Data source is WDI. For political empowerment of women, we use the percentage of seats in the lower house of the parliament that are held by women. Data source is Inter-Parliamentary Union (IPU). For gender disparities in the laws, we control for seven dummy variables for gender-specific laws in the country. These variables capture whether the constitution guarantees equality of all citizens; whether constitution has non-discrimination clause covering gender; whether married women are allowed by law to be the head of household; whether married women are required by law to take permission from their husbands to pursue profession, to open bank account, to sign contract, and to initiate legal proceedings. Data source for all these variables is WBL. For the institutional environment, we use several variables from International Country Risk Guide (ICRG). These include the measures for democratic accountability, prevalence of internal conflict, quality of bureaucracy, law and order situation, economic stability and risks (economic risk rating), and political stability and risks (political risk rating). Last, we control for the socioeconomic conditions in the country using a measure from ICRG based on the poverty rate, unemployment rate, and the level of consumer confidence in the economy. For more details on the control variables discussed here, see Table A4 in the Online Appendix.

Figure 1 provides some suggestive evidence on the long-run unconditional relationship between domestic violence legislation and the adult women-to-men mortality ratio. For country-year pairs with no domestic violence legislation in place, the average women-to-men mortality ratio equals 0.72 (median value). The corresponding figure for the country-year pairs with domestic violence legislation in place is much lower at 0.57. Figure 2 shows the short-run impact. That is, in the year just after the implementation of the domestic violence legislation, the annual change in women-to-men mortality ratio equaled −0.0025 (median change over all countries) compared to a much smaller figure (in absolute value) of −0.0015 in the years following no change in the domestic violence legislation.

4 | BASE REGRESSION RESULTS

Table 1 contains the results for our baseline specification. These are panel estimation results that include country and year fixed effects. With only country and year fixed effects, the estimated coefficient value of domestic violence legislation dummy variable is negative equaling −0.015, and statistically significant at the 1% level (column 1). In columns 2–5, we add the baseline controls sequentially
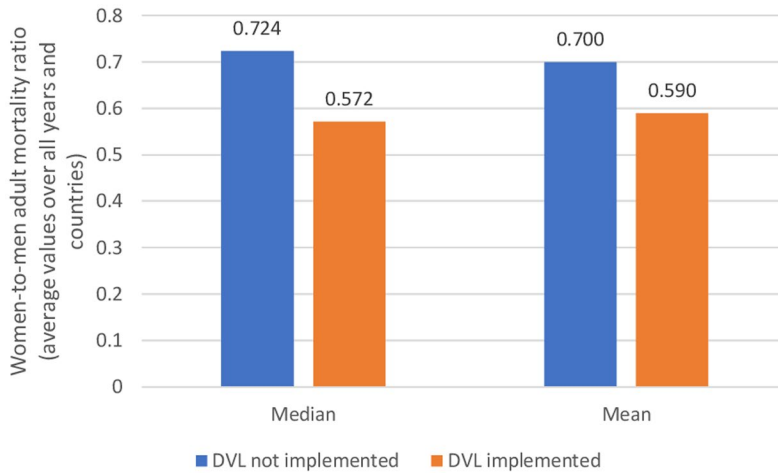
FIGURE 1 Women-to-men adult mortality ratio before and after the implementation of the domestic violence legislation. Source: Author's own calculations. Sample size: 159 countries and 1990–2014 period
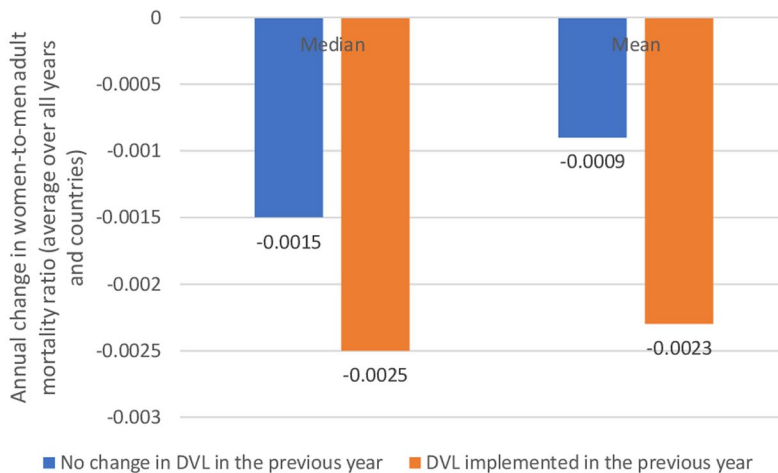

FIGURE 2 Change in women-to-men adult mortality ratio in the year just after domestic violence legislation is implemented versus. the average annual change in the remaining years. Source: Author's own calculations. Sample size: 159 countries and 1990–2014 period

to the previous specification. Doing so, the estimated coefficient value of the domestic violence legislation dummy remains negative, large, and almost unchanged in value, and significant at the 1% level. This is encouraging in that it suggests that failure to account for other determinants of women-to-men mortality ratio, if any, is less likely to have any significant impact on our main result. According to our most conservative baseline estimate (columns 1–4), domestic violence legislation is associated with a decrease in women-to-men adult mortality ratio by about 2.27% of its mean value. Expressed in standard deviation units, the stated decline equals 0.08 standard deviation units of women-to-men adult mortality ratio. A graphical illustration of the relationship between women-to-men mortality ratio and domestic violence legislation discussed here is provided in Figure A1 (without controls) and Figure A2 (with controls) in the Online Appendix.

TABLE 1 Base regression results (OLS)

Dependent variable: Women-to-men adult mortality ratio	(1)	(2)	(3)	(4)	(5)
Domestic violence legislation	−0.015***	−0.015***	−0.015***	−0.015***	−0.016***
	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Log of real GDP per capita		−0.010	−0.009	−0.009	−0.012
		(0.011)	(0.010)	(0.010)	(0.010)
GDP per capita growth (annual %) based on constant LCU		−0.017	−0.021	−0.021	−0.018
		(0.015)	(0.015)	(0.015)	(0.015)
Labor force participation rate, female over male			−0.037	−0.032	−0.030
			(0.063)	(0.064)	(0.064)
Years of education, female over male			−0.833***	−0.804***	−0.693**
			(0.294)	(0.305)	(0.311)
Urban population (% of total)				−0.071	−0.054
				(0.113)	(0.114)
CEDAW optional protocol ratified					0.006
					(0.006)
Immunization, measles (% of children aged 12–23 months)					−0.021
					(0.024)
Immunization, DPT (% of children aged 12–23 months)					0.011
					(0.021)
Fertility rate					0.008
					(0.007)
Country dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Constant	0.668***	0.749***	1.383***	1.391***	1.296***
	(0.006)	(0.083)	(0.239)	(0.242)	(0.263)
Number of observations	3,696	3,696	3,696	3,696	3,696
Adjusted R^2	0.062	0.065	0.119	0.120	0.128

Notes: Standard errors in brackets. All standard errors are Huber-White robust and clustered on the country. Significance is denoted by *** (1%) and ** (5%).

For the various controls, the results are broadly in the expected direction, although the coefficients are not statistically significant in most cases. That is, as expected, women-to-men adult mortality ratio is lower in countries that have higher GDP per capita, higher GDP per capita growth rate, higher labor force participation and education among women relative to men, higher urbanization, higher immunization rate against measles among children, and lower fertility rate. However, only the education variable is significantly correlated (at 5% level) with the dependent variable. One reason for the lack of significance for most controls could be that they are correlated with each other. We find some evidence of this. That is, we regress the dependent variable on the domestic violence legislation dummy and one control at a time. We do so with and without the country dummies. The exercise reveals that in the model without country dummies, seven of the nine control variables are significantly correlated (at the 10% level or less) with the dependent variable and in the expected direction. With the country dummies included, four of the nine controls are significantly correlated with the dependent variable

and in the expected direction. Another reason why most of the controls in our baseline estimation are not statistically significant could be that there is a nonlinear relationship between the controls and the dependent variable. We find some evidence of this.[7]

5 | IV AND PSM

5.1 | IVs estimation

According to the World Bank (2015), over the past 25 years, there has been a precipitous rise in the number of economies adopting domestic violence legislation. The report suggests that the increase has largely been driven by international and regional human rights conventions. Thus, we instrument domestic violence legislation using (1-year lagged values of) two instruments: the ratification of the convention of Bélem do Pará and the ratification of the Istanbul Convention of the Council of Europe. The Bélem do Pará convention also known as the Inter-American Convention on the Prevention, Punishment, and Eradication of Violence against Women is a regional convention that was adopted in 1994 within the Organization of American States. Both these conventions, unlike CEDAW, are directly on issues of gender violence. Two points to note here. First, we define two dummy variables, one for each protocol, which takes the value of 1 for the years the relevant protocol (Bélem do Pará and the Istanbul Convention of the Council of Europe) is ratified and 0 otherwise. We use 1-year lagged values of these dummy variables as instruments. Second, there is a possibility of violating the exclusion restriction condition of instruments if these conventions generate laws that go beyond just domestic violence and into areas related to the labor market. However, this is unlikely to pose much problem for us since we already control for labor force participation ratio (of women and men) and it does not show much correlation with the dependent variable. More importantly, there is evidence to suggest that the protocols have limited direct effects (i.e., other than through domestic violence legislation) on gender-specific outcomes including gender-based violence. Furthermore, there are other campaigns that may have been influential in changing laws such as the UN Secretary-General campaign "UNITE to End Violence against Women" in 2008; however, these are typically one-time events, and thus they get absorbed in the year fixed effects in our regressions.

The IV regression results are contained in Table 2. These results use the same controls (including country and year fixed effects) as in the baseline model discussed earlier. In Table 2, the first-stage IV results (Panel A) confirm a positive and significant (at 1% level) relationship between domestic violence legislation dummy and each of the two instruments. The F statistic for the excluded instruments is significant at the 1% level and well above the minimum recommended level of 10. The over-identification test based on Hansen's J statistic implies that we cannot reject the null hypothesis that the instruments are exogenous. The second-stage IV results (Panel B) confirm that domestic violence legislation is strongly negatively correlated with women-to-men mortality ratio. The estimated coefficient value of (instrumented values of) domestic violence legislation ranges narrowly between −0.068 (column 1) and −0.072 (columns 3 and 4) across the different specifications, significant at the 5% level in some specifications and at the 1% level in others. Note that the IV estimates of domestic violence legislation are much larger (more negative) than what we found in our baseline regressions (Table 1). This suggests that our baseline results may be on the conservative side, being biased toward zero due to measurement errors and/or failure to account for other omitted variables.

TABLE 2 IV regression results

	(1)	(2)	(3)	(4)	(5)
Panel B: Second stage IV results					
Dependent variable: Women-to-men adult mortality ratio					
Domestic violence legislation (instrumented values)	−0.068**	−0.069**	−0.072***	−0.072***	−0.071***
	(0.029)	(0.028)	(0.026)	(0.026)	(0.025)
Log of real GDP per capita		0.003	0.004	0.004	−0.002
		(0.010)	(0.009)	(0.009)	(0.008)
GDP per capita growth (annual %) based on constant LCU		−0.028*	−0.032**	−0.032**	−0.031**
		(0.015)	(0.015)	(0.015)	(0.015)
Labor force participation rate, female over male			−0.050	−0.048	−0.045
			(0.062)	(0.064)	(0.062)
Years of education, female over male			−0.771***	−0.760***	−0.588**
			(0.280)	(0.285)	(0.290)
Urban population (% of total)				−0.021	−0.007
				(0.115)	(0.115)
CEDAW optional protocol ratified					0.015*
					(0.008)
Immunization, measles (% of children ages 12–23 months)					0.018
					(0.025)
Immunization, DPT (% of children aged 12–23 months)					−0.011
					(0.025)
Fertility rate					0.010
					(0.007)
Country and year dummies	Yes	Yes	Yes	Yes	Yes
Number of observations	3,447	3,447	3,447	3,447	3,447
Panel A: First stage IV results					
Dependent variable: Domestic violence legislation (1-year lag)					
Belem do Para convention ratified (lagged values)	0.299***	0.309***	0.342***	0.342***	0.343***
	(0.067)	(0.068)	(0.071)	(0.069)	(0.067)
Council of Europe—Istanbul Convention ratified (lagged values)	0.224***	0.223***	0.220***	0.205***	0.153***
	(0.033)	(0.032)	(0.035)	(0.036)	(0.043)
Other controls (as above)	Yes	Yes	Yes	Yes	Yes
F-statistic (from first stage IV)	28.91***	29.88***	27.34***	23.54***	16.55***
Hansen *J* statistic (over identification test)	0.975	0.976	0.996	1.000	0.992

Note: Standard errors in brackets. All standard errors are Huber-White robust and clustered on the country. Significance is denoted by *** (1%), ** (5%), and * (10%).

5.2 | Propensity score matching

For our next robustness check, we use the PSM estimation method (Rosenbaum & Rubin, 1983, 1984; Rubin, 1997). PSM uses observational data to create groups of treated and control units that have similar covariate values so that subsequent comparisons, made within these matched groups, are not confounded by differences in covariate distributions. There is little discussion in the literature on how to exploit the panel nature of the data for the PSM analysis. Thus, we follow two different estimation techniques. First, we disregard the panel nature of the data. The treatment group here includes all country-year cases with the domestic violence legislation in place in the last year, and the control group includes all country-year cases with no domestic violence legislation in place in the last year. The outcome variable is the level of women-to-men mortality ratio. Matching between treatment and control group is conditioned on all the variables that are included in our baseline regression (Table 1). This is the levels model. Second, we take first differences (annual changes) in all the variables and repeat the matching exercise. The treatment group here includes country-year cases that implemented domestic violence legislation during the previous year and the control group includes all country-year cases with no change in domestic violence legislation during the past year. Similarly, the outcome variable is the annual change in women-to-men mortality ratio. Likewise, the conditioning variables used for matching cases between the treatment and control group include annual change in the baseline control variables. This is the annual change model.

Figure 3a and b shows the estimated propensity scores for the treatment and control groups for the levels model and the annual change model, respectively. As required, our propensity scores are strictly bounded away from 0 and 1; moreover, the overlap in propensity scores between units in treatment and controls groups is reasonable, although not perfect. Table 3 contains the PSM results based on the following specification: logit model is used for estimating the probability of receiving the treatment, matching is done using the nearest neighborhood method and with replacement, each observation unit in the treatment group is matched to at most one observation in the control group, and there is no restriction on the size of neighborhood for finding a match. Panel A contains results for the average treatment effect (ATE) when using annual values (levels model), while Panel B contains results for the ATE when using annual changes. Both these results confirm our earlier finding of a large, negative, and statistically significant relationship between domestic violence legislation and women-to-men adult mortality ratio. Qualitatively similar results obtain under alternative specifications such as one to many mappings and limited neighborhood matching.[8]
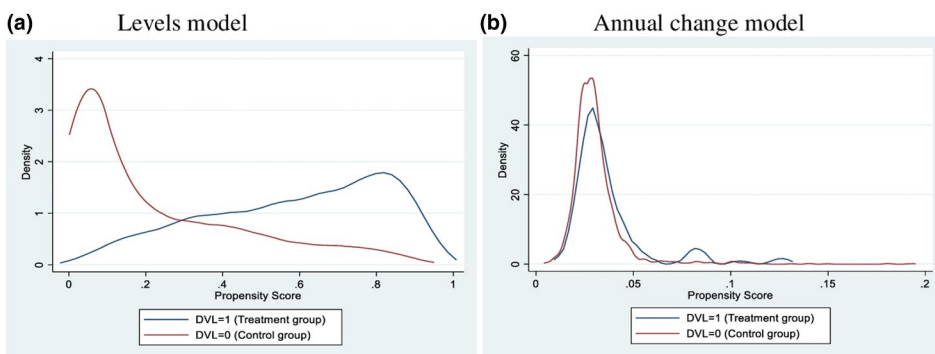

FIGURE 3 Propensity scores. (a) Levels model. (b) Annual change model. DVL is Domestic violence legislation Y:1 N:0 (one year lagged values) as defined in the main text

TABLE 3 Propensity score matching (PSM) results

Panel A: Using levels of variables	
Dependent variable: Women-to-men adult mortality ratio	
Average treatment effect (ATE) in population[*]	−0.034***
	(0.011)
Number of observations	3,696
Panel B: Using annual changes in variables	
Dependent variable: Annual change in Women-to-men adult mortality ratio[**]	
ATE in population[**]	−0.002**
(*Educated* = 1 versus. *Educated* = 0)	(0.001)
Number of observations	3,696

Notes: (1) Robust standard errors in brackets; (2) Specification used is discussed in the text above. Significance is denoted by *** (1%), ** (5%), and * (10%).

*Domestic violence legislation dummy (1-year lag) = 1 versus. domestic violence legislation dummy (1-year lag) = 0;

**Change in domestic violence legislation dummy (1-year lag) from last year = 1 versus. change in domestic violence legislation dummy (1-year lag) = 0.

6 | ADDITIONAL ROBUSTNESS CHECKS

6.1 | Additional controls

As discussed in Sections 2 and 3, we exclude additional controls from our baseline regression results as these controls entail a significant drop in sample size due to unavailable data. There is also the issue of multicollinearity if we included all these variables into a single regression. Thus, regression results with the additional controls are reported separately from the baseline regression results. Specifically, using the final baseline specification (column 5, Table 1), we add in separate regressions additional controls for political empowerment, health, gender-specific laws, institutions, and socioeconomic condition. Regression results are provided in Table 4. For brevity, only the estimates for the domestic violence legislation dummy are shown. The negative and statistically significant relationship between women-to-men mortality ratio and domestic violence legislation survives the additional controls.

6.2 | Lagged effects of domestic violence legislation

One concern with the baseline results discussed earlier is if the relationship between women-to-men mortality ratio and domestic violence legislation is permanent or likely to reverse in future years. Another concern is if there are lagged effects of domestic violence legislation on women-to-men mortality ratio. If there are lagged effects, it would imply these results are an underestimation of the true effect of domestic violence legislation. We check for these possibilities by repeating our baseline regressions with 2 and 3 years lagged values of the domestic violence legislation dummy included in the specification in addition to the 1 year lagged values used in the baseline model. Regression results are provided in columns 1 and 2 of Table A8 in the Online Appendix. They show only a quantitatively small and insignificant decline in women-to-men adult mortality ratio at 2- and 3-year lags, with the decline at 1-year lag maintained as in the baseline model. Thus, there is no evidence of either any lagged effect or future reversal in the relationship between domestic violence legislation and women-to-men mortality ratio.

TABLE 4 Robustness for additional controls

Dependent variable: Women-to-men adult mortality ratio	(1)	(2)	(3)
Model/specification	Domestic violence legislation (1-year lag)	Adjusted R^2	Observations
Panel A: Base estimate	−0.016***	0.128	3,696
	(0.005)		
Panel B: Institutions and governance	−0.013**	0.181	2,966
	(0.006)		
Democratic Accountability (ICRG), Internal Conflict (ICRG)			
Bureaucracy Quality (ICRG)			
Corruption (ICRG)			
Law & Order (ICRG)			
Economic Risk Rating (ICRG)			
Political Risk Rating (ICRG)			
Panel C: Political empowerment			
Representation of women in political office	−0.010*	0.153	2,655
	(0.005)		
Panel D: Health			
Prevalence of HIV, female over male (% ages 15–24)	−0.014**	0.213	1,895
Health expenditure, public (% of GDP)	(0.006)		
Panel E: Women legal restrictions	−0.015**	0.277	1,998
	(0.007)		
Married women allowed to be head of household by law (dummy)			
Married women not required by law to take permission from husband to pursue profession			
Married women not required by law to take permission from husband to open bank account			
Married women not required by law to take permission from husband to sign contract			
Married women not required by law to take permission from husband to initiate legal proceedings			
Constitution guarantees equality of all citizens			
Constitution has non-discrimination clause covering gender			
Panel F: Socioeconomic	−0.015**	0.151	2,966
	(0.006)		
Socioeconomic condition (unemployment, poverty, consumer confidence; ICRG)			

Notes: All regressions use the specification in column 5 of Table 1 with additional controls as indicated above. Sample size varies because of missing data.

***$p < .01$; **$p < .05$; *$p < .1$. Huber-White robust standard errors clustered on the country in brackets.

6.3 | Developing countries

While the implementation of domestic violence legislation is a problem in both rich and developing countries, it is likely to be a bigger problem in the developing countries where enforcement mechanisms are relatively weak. Besides, understanding if our results hold for developing countries is important from the development economics point of view because developing countries constitute most of our sample (113 of the 159 countries). Baseline regression results for the sample of developing countries (low-income, upper middle-income, and lower middle-income countries as defined by the World Bank) are provided in columns 3 and 4 of Table A8 in the Online Appendix. These results confirm a large, negative, and statistically significant (at the 1% level) relationship between women-to-men mortality ratio and domestic violence legislation in the developing countries.

6.4 | A falsification test

As a falsification test, we regress the female-to-male labor force participation ratio on the domestic violence legislation variable. The motivation is that the labor force participation ratio is an important proxy for overall well-being of women relative to men; thus, it should be positively correlated with domestic violence legislation in case the domestic violence legislation variable is a proxy for the overall well-being of women relative to men. However, regression results provided in columns 5 and 6 in Table A8 in the Online Appendix show no significant relationship between domestic violence legislation and the labor force participation ratio.

6.5 | Absolute women's mortality rate

We repeat the baseline regressions (as in Table 1) but replace the dependent variable with the absolute adult female mortality rate. Regression results provided in Table A9 in the Online Appendix confirm a negative and significant (at 5% level or less) relationship between absolute adult female mortality rate and the domestic violence legislation variable.

6.6 | Prevalence of physical violence by intimate partners

One drawback with the results so far is that the dependent variable is not a direct measure of deaths due to domestic violence. To somewhat account for this, we provide regression results using WHO's prevalence of physical violence by intimate partners as the dependent variable. This dependent variable equals the proportion of ever-partnered women aged 15–49 years experiencing intimate partner physical violence at least once in the past 12 months. The WHO data are available for 73 countries, and for a single year for a country varying between 2000 and 2014. Thus, all the results in this section are based on variation across a cross-section of economies. We experiment with several transformations of the domestic violence legislation variable. The first measure is the proportion of years for which the country has had the domestic violence legislation in place 10 years before the year for which the WHO data are available. We complement this with a binary version, which is a dummy variable equal to 1 if the country has domestic violence legislation in place for a majority of years (i.e., 5 or more years) during the 10 years before the year for which the WHO data are available in the country and 0 otherwise. This binary variable equals 1 for 43 countries and 0 for the remaining 31

countries in our sample. The same control variables in the baseline model (as shown in Table 1) are employed, averaged over the last 3 years before the year for which the WHO data are available in the country.[9] The use of average values ensures that the results are not unduly affected by annual fluctuation in one or more of the control variables.

Regression results using the WHO data are provided in Table 5. For brevity, only the results for the parsimonious specification without any control variables and the specification with the full set of controls (final specification) are presented. Columns 1 and 2 provide the results using proportion of years for which the country has had the domestic violence legislation in place 10 years before the year for which the WHO data are available. The corresponding results using the binary version of the domestic violence legislation variable are provided in columns 3 and 4. These results confirm our main finding that domestic violence legislation is associated with lower violence against women by

TABLE 5 Prevalence of violence by intimate partners (WHO) results

Dependent variable: Prevalence of physical violence by intimate partners (WHO data)	(1)	(2)	(3)	(4)
Domestic violence legislation dummy (average over last 10 years)	−11.269***	−4.101**		
	(2.701)	(2.048)		
Domestic violence legislation in place in 5 or more out of the last 10 years Y:1 *N*:0			−9.408***	−3.924**
			(2.414)	(1.886)
Log of real GDP per capita		−1.689		−1.816
		(1.264)		(1.221)
GDP per capita growth (annual %) based on constant LCU		17.855		19.703
		(25.228)		(26.149)
Labor force participation rate, female over male		−7.567		−6.944
		(6.173)		(6.160)
Years of education, female over male		0.720		0.648
		(7.675)		(7.545)
Urban population (% of total)		2.903		3.188
		(6.804)		(6.567)
CEDAW optional protocol ratified		2.024		2.275
		(2.643)		(2.694)
Immunization, measles (% of children aged 12–23 months)		−26.766		−25.339
		(24.420)		(24.403)
Immunization, DPT (% of children aged 12–23 months)		−2.210		−3.601
		(27.153)		(27.290)
Fertility rate		1.979		1.920
		(1.275)		(1.249)
Constant	20.043***	51.608***	19.657***	52.216***
	(2.201)	(18.118)	(2.177)	(17.941)
Number of observations	73	73	73	73
Adjusted R^2	0.211	0.561	0.193	0.567

Notes: Huber-White robust standard errors in brackets. All the control variables are averages over 3 years before the year for which data on the dependent variable are available in the country.

***$p < .01$; **$p < .05$.

intimate partners. This negative relationship is significant at the 1% level in the specification with no other controls (columns 1 and 3) and at the 5% level in the specifications with all the controls included (columns 2 and 4).

7 | CONCLUSION

Women make up about half of the world's population. Laws and institutions that improve their well-being warrant attention and priority. In this study, we uncovered a significant beneficial relationship between the presence of domestic violence legislation and the women-to-men mortality ratio. Our findings indicate that the presence of domestic violence legislation may have resulted in saving many lives of women, a worthy achievement that is also likely to be accompanied with economic benefits. Of course, domestic violence legislation is not the complete story and should be employed as one among many efforts to protect women. However, as our results indicate, it is an important part of the overall strategy to save lives.

While this study is a first step toward understanding how laws and institutions protect women, several promising questions remain to be answered. For example, it is unclear why the introduction of legislation protecting women from domestic violence is such a relatively recent phenomenon, with only a handful of countries having such legal protections in place as recently as the early 1990s. Furthermore, what are the drivers of domestic violence legislation, and what accounts for the uneven geographic distribution of such legislation? Are there factors that complement or substitute domestic violence legislation as far as the impact on women's mortality rates is concerned? We hope the present study motivates more research in the area.

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ENDNOTES

[1] http://www.nybooks.com/articles/archives/1990/dec/20/more-than-100-million-women-are-missing/.

[2] See, for example, Allah-Mensah and Osei-Afful (2017), Burnet and Kanakuze (2018), Sifaki (2017), and World Bank (2015).

[3] Ruhm (1998) uses a similar approach for assessing the impact of parental leave on women's employment.

[4] For example, studies have shown that mortality rates and overall well-being of women and men are differently affected by factors such as parental education, rainfall shocks, household incomes, and prices of goods (Foster 1994; Rose 1999).

5 Urbanization is found to account for important factors related to mortality including density of health services and disease transmission. Initially, urbanization was detrimental for mortality rates due to the easy spread of diseases. More recently, urbanization goes hand in hand with better access to healthcare.

6 However, by lowering domestic violence, domestic violence legislation may affect women's mortality indirectly through lower suicides (Beleche 2019), lower physical, mental, and psychological distress and substance use (Carbone-López et al., 2006), and more time to exploit economic opportunities (WHO, 2004). This gives credence to the use of the overall mortality rates.

7 We check for possible nonlinear effects of some of the controls by repeating the baseline regressions of Table 1 and adding squared terms for all the continuous (non-dummy) variables. Regression results are provided in Table A7 in the Online Appendix. They reveal two things. First, our main result for domestic violence legislation continues to hold. Second, female-to-male ratio of labor force participation and the fertility rate exhibit a significant nonlinear relationship with the dependent variable.

8 These results are available on request from the authors.

9 Our main result for the relationship between domestic violence legislation and violence by intimate partners is qualitatively unchanged if we use averages values of the controls over the 10 years (instead of 3 years) before the year for which the WHO data are available in the country. Table A10 in the Online Appendix provides the results using 10-year averages for the control variables.

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SUPPORTING INFORMATION

Additional Supporting Information may be found online in the Supporting Information section.