HHS Public Access

Author manuscript

Criminology. Author manuscript; available in PMC 2019 October 07.

Published in final edited form as:

Criminology. 2018 August ; 56(3): 574–607. doi:10.1111/1745-9125.12181.

Does Increasing Women's Education Reduce Their Risk of Intimate Partner Violence? Evidence from an Education Policy Reform

Abigail Weitzman

University of Texas – Austin, aweitzman@utexas.edu

Abstract

Although scholars have employed rigorous causal methods to examine the relationship between education and crime, few studies have taken a causal approach to the study of education and intimate partner violence (IPV) specifically. From a social causation perspective, improving women's education should protect them from violence, yet from a social selection perspective, education could proxy for unobserved factors that explain negative associations between education and IPV. This study adjudicates between the two possibilities using an exogenous source of variation in education—a 1990s compulsory schooling reform in Peru. Specifically, the author conducts an instrumented regression discontinuity that implicitly controls for women's unobserved endowments by comparing women who were aged just above (N=8,195) and below (N=6,645) the school-age cutoff at the time of the reform. Consistent with the social causation perspective, increasing women's schooling reduced both their recent and longer-term probabilities of psychological, physical, and sexual IPV, and their recent and longer-term probabilities of experiencing any IPV and poly-victimization. Supplemental mediation analyses provide support for three interrelated causal pathways—improvements in women's personal resources, delayed family formation, and changes in partner selection. These findings confirm the protective effects of women's education and further illuminate mechanistic processes by which this occurs.

The effects of educational expansion on crime in the United States and Europe are well documented—numerous studies have found that increasing formal schooling reduces rates of property crime, violent crime perpetrated against strangers, and white-collar crime (Hjalmarsson, Holmlund, & Lindquist, 2015; Lochner, 2004, 2007, 2011; Machin, Marie, & Vujić, 2011, 2012; Meghir, Palme, & Schnabel, 2012; Moretti, 2005), as well as corresponding conviction and incarceration rates (Hjalmarsson et al., 2015; Machin et al., 2011, 2012). Nevertheless, few studies have investigated the consequences of educational expansion outside the United States or Europe or considered the link between educational expansion and intimate partner violence (IPV) specifically. Though some scholars assert that IPV and other types of crime share a similar etiology (Fagan & Wexler, 1987; Felson, 2006; Felson & Lane, 2010), other scholars disagree (R. E. Dobash, Dobash, Cavanagh, & Lewis, 2004; Moffitt, Krueger, Caspi, & Fagan, 2000; Thomas, Dichter, & Matejkowski, 2011). Whether educational expansion has an analogous effect on IPV, particularly with respect to IPV victimization, and if so, why this occurs, thus remain open questions. In this study, I draw on a 1990s compulsory schooling reform in Peru to investigate the individual-level effects of increasing women's schooling on their risk of IPV victimization in the Latin American context.

Many scholars view IPV as a form of gender-based violence in which men, more than women, use violence to maintain power within their relationships (Atkinson, Greenstein, & Lang, 2005; R. E. Dobash et al., 2004; R. P. Dobash, Dobash, Wilson, & Daly, 1992; Weitzman, 2014). However, some scholars assert that IPV is more appropriately conceptualized as a violent form of dyadic conflict in which men and women often both partake (Bartholomew & Cobb, 2010; Dutton, 2012; Felson, 2006; Felson, Savolainen, Hughes, & Ellonen, 2015; Moffitt, Robins, & Caspi, 2001). Still, others argue that both conceptualizations may be accurate given that IPV can occur for different reasons in different relationships (Johnson, 1995; Kelly & Johnson, 2008).

Irrespective of how IPV is conceptualized, many studies suggest that women's education is protective against it (Ackerson, Kawachi, Barbeau, & Subramanian, 2008; Flake, 2005; Friedemann-Sánchez & Lovatón, 2012; Jewkes, 2002; Perales et al., 2009), a notion I refer to as the social causation perspective (Figure 1a). Yet there is also reason to believe that certain circumstances that lead women to achieve more schooling also affect their risk of IPV (whether gender-based or dyadic). For instance, having a father who holds a traditional gender ideology may simultaneously make a woman more likely to leave school early and more likely to enter or stay in an abusive or unhappy relationship than if she had a father who possessed a comparatively more egalitarian ideology. In other words, it remains possible that observed negative relationships between women's education and IPV are explained by omitted characteristics of women and their circumstances, giving rise to the possibility of social selection (Figure 1b).[1]

Indeed several studies find no association between women's education and IPV (Hindin & Adair, 2002; Panda & Agarwal, 2005; Schuler, Hashemi, Riley, & Akhter, 1996), or that the association disappears once other characteristics of women and their households are controlled for (Bangdiwala et al., 2004; Bhattacharyya, Bedi, & Chhachhi, 2011) (see Vyas and Watts (2009) for a thorough review of the debate). These studies suggest one of two possibilities. Either the effects of women's education are mediated by subsequent outcomes in women's lives, such as employment and family formation, or, the commonly observed negative relationship between women's education and IPV is a function of unobserved characteristics, such as features of their childhood households, that simultaneously contribute to their educational attainment (Cueto, Guerrero, Leon, Zapata, & Freire, 2014; Dercon & Krishnan, 2009; Gertler & Glewwe, 1992) and later risk of IPV (Figure 1b) (Bangdiwala et al., 2004; Hoffman, Demo, & Edwards, 1994).

This study serves as one of the first explicit tests of whether women's education has a causal effect on intimate partner violence and further adjudicates between potential mechanisms by which this effect could occur. To evade the problem of selection bias (where education and

[1]"Social causation" and "social selection" have been used to characterize similar debates about the effects of education on other outcomes such as mental health (Halpern-Manners, Schnabel, Hernandez, Silberg, and Eaves 2016).

IPV are both a function of preexisting, unobserved factors), I exploit an exogenous source of variation in education—a compulsory schooling reform in Peru that extended the mandated years of schooling by five years, but only for children still enrolled in primary school at the time. Specifically, I use a strategy known as an instrumented or "fuzzy" regression discontinuity that compares IPV rates among women who were just below the primary school-age completion threshold in 1993, when the reform was enacted, to those just above it. This strategy isolates the effects of education while implicitly controlling for all potential confounders (Behrman, 2015a, 2015b; Grant, 2015; Ozier, 2016; Weitzman, 2017). To discern between various mechanisms, I conduct a supplemental mediation analysis. Taken together, these analyses help to reconcile discrepant findings within past research, bolster theoretical conceptualizations of the relationship between women's education and IPV, and broaden our understanding of the effects of educational expansion on crime in a developing country context.

THEORETICAL PERSPECTIVES ON WOMEN'S EDUCATION AND INTIMATE PARTNER VIOLENCE

SOCIAL CAUSATION PERSPECTIVE

Current literature points to several causal mechanisms by which women's education may alleviate their risk of IPV (Figure 1a). Because these mechanisms are interrelated they may co-occur. First, increasing women's education should expand their personal resource bases, including their cognitive skills, employment opportunities, and occupational status (Psacharopoulos & Patrinos, 2004; Smith-Greenaway, 2013). In turn, these personal resources should reduce women's economic dependence on partners, and as dependency theory posits, increase their ability to leave abusive or unhappy relationships (Kalmuss & Straus, 1982). Moreover, resources such as cognitive abilities may also affect dyadic IPV by improving individuals' interpersonal skills, social cognition, and skills for coping with negative emotionality in a non-violent way (Krueger, Moffitt, Caspi, Bleske, & Silva, 1998; Moffitt et al., 2001).

Second, the timing of secondary schooling may compete with early marriage and fertility (Duflo, Dupas, Kremer, & Sinei, 2007; Flórez & Núñez, 2003). If delayed entrance into marriage allows women more time to accumulate their own material resources, then increasing education should implicitly provide them with more leverage to bargain with their partners and thus make them better able to minimize the risk of abuse or violent conflict (Kalmuss & Straus, 1982). Similarly, delaying fertility should reduce women's vulnerability to IPV by reducing the economic and physical dependence that stems from children (Kalmuss & Straus, 1982). Moreover, delaying marriage and fertility may afford women more opportunities to find personal fulfillment outside the domestic realm. Such extra-household fulfillment could reduce pressures on marital relationships and correspondingly reduce the risk of marital conflict and dyadic IPV.

Third, resource theory and family stress theory argue that the wealth or poverty of a woman's partner may affect her risk of IPV. The former, resource theory, asserts that men sometimes use violence as a way to influence relationship outcomes when they lack other means of negotiation (Felson & Messner, 2000; Fox, Benson, DeMaris, & Van Wyk, 2002; Goode, 1971). The latter, family stress theory, argues that a couples' joint resource base affects IPV by influencing the household's level of financial stress and conflict (Fox et al. 2002). Evidence that IPV occurs more frequently in poorer households has indeed been found in a number of countries, including the United States, Thailand, India, and Colombia (Allen & Straus, 1979; Friedemann-Sánchez & Lovatón, 2012; Hoffman et al., 1994; Weitzman, 2014). If a woman's education affects the characteristics of the potential partners she attracts, including their education, then having more education may reduce her likelihood of household poverty and associated financial conflict via her ability to attract partners with greater human capital.

Fourth, schools are often important sites of public health campaigns, information dissemination, and socialization (Merakou, Costopoulos, Marcopoulou, & Kourea-Kremastinou, 2002). If women are exposed to anti-violence messaging while in school, then this may influence their attitudes towards intimate partner violence and in turn their tolerance and/or use of violence in their own lives (Boyle, Georgiades, Cullen, & Racine, 2009; Gage & Hutchinson, 2006).

SOCIAL SELECTION PERSPECTIVE

Although there are many reasons to believe that women's education should reduce IPV (both gender-based and dyadic), the results of previous research have been mixed (Vyas and Watts 2009). Null effects of education could result from controlling for potential mediators—characteristics of women that stem from their education and subsequently affect their risk of IPV. Alternatively, they may stem from the fact that most studies investigating the link between education and IPV have relied on conventional, naïve methods (Ackerson et al., 2008; Boyle et al., 2009; Flake, 2005; Friedemann-Sánchez & Lovatón, 2012; Perales et al., 2009). Although conventional studies often control for a wide range of environmental, demographic, and other background characteristics, they are unable to control for *all* potential confounders. This poses a problem when unobserved characteristics simultaneously contribute to women's educational attainment and their risk of IPV (Figure 1b).

To evade the issue of endogeneity, scholars have developed methodological techniques that rely on the randomization of education. These include randomized control trials of school incentive programs (Baird, Chirwa, McIntosh, & Ozler, 2010) and quasi-experimental studies that exploit random variation in formal education via the elimination of school fees and uniforms (Behrman, 2015b; Kadzamira & Rose, 2003), compulsory schooling laws (Güneş, 2015), and the construction of new schools (Duflo, 2001). In both randomized control trials and quasi-experimental studies, women who receive treatment (additional years of schooling) should be no different from women who do not receive treatment except with regard to their amount of education. The high degree of similarity across treatment and control groups should allow for an unbiased estimation of the effects of education.

CURRENT STUDY

This study employs a particular type of quasi-experimental study design—the instrumented or "fuzzy" regression discontinuity. Regression discontinuities prove valuable when trying to isolate the effects of an endogenous variable using observational data (Hahn, Todd, & Van der Klaauw, 2001; Moscoe, Bor, & Bärnighausen, 2015) and certain assumptions are met (Imbens & Lemieux, 2008). To eliminate bias, the instrumented regression discontinuity technique relies on an exogenously determined threshold that defines whether a respondent was or was not induced to change her behavior (Hahn et al., 2001). As described below, I compare two highly similar groups of women who were aged just above and below primary-school completion at the time a compulsory schooling reform was implemented.

Regression discontinuities have become a popular tool for examining the causal effects of education on a wide range of outcomes related to IPV, including human capital accumulation, desired and realized fertility, sexual behavior and health, and maternal health (Behrman, 2015a, 2015b; Grant, 2015; Ozier, 2016; Weitzman, 2017). However, only once has the technique been used to investigate IPV itself. Specifically, Peterman, Behrman, & Palermo (2015) used the implementation of Universal Primary Education in Uganda and Malawi to estimate the effects of women's schooling on the likelihood of physical and/or sexual IPV (combining the two forms of violence into a composite measure). The authors found that increasing women's schooling reduced recent IPV in Uganda, and similarly reduced recent IPV for women who completed primary school in Malawi, but exacerbated the risk of IPV among women who did not complete primary schooling in Malawi. In other words, the authors found a non-linear relationship between education and IPV in the Malawian case (but not in the Ugandan case).

The present study expounds upon the work of Peterman, Behrman, & Palermo (2015) in four key ways. First, it examines the effects of women's education on psychological, physical, and sexual IPV separately, as well as together. In so doing, it illuminates the extent to which the effects of women's education are consistent across different forms of violence. Second, it provides an additional analysis of causal pathways. This mediation analysis unearths important information about *how* education affects IPV. Third, whereas Peterman, Behrman, & Palermo (2015) focus on an educational increase occurring at primary school age, this study focuses on an educational increase occurring at secondary school age. This is an important distinction given that secondary education should equip women with more human capital than primary alone and that the timing of secondary school coincides with a period of the life course during which many women enter their first relationships. Finally, this study relies on data from Latin America rather than East Africa. As such, it diversifies our understanding of the relationship between education and IPV across distinct institutional and cultural settings.

THE PERUVIAN CONTEXT

Peru has one of the highest IPV rates in the world (Garcia-Moreno et al. 2006; WHO 2013) and the second highest IPV rate in the Western hemisphere (Bott et al. 2012; WHO 2013). Thirty-two percent of Peruvian women report experiencing psychological IPV in their lifetimes; 39% report experiencing physical IPV; and 9% report experiencing sexual forms of IPV (Bott et al. 2012; WHO 2013).

Primary school has been free and available to the public in Peru since 1828 (World Bank 2007), and has been compulsory since 1905 (Freeburger & Hauch, 1964). From 1905 to 1992, compulsory schooling meant that children were mandated to complete six years of primary schooling beginning at age 6 and ending at age 11. In 1993, however, the Peruvian government amended the constitution to require an additional five years of schooling, making a total of eleven years compulsory. However, this change in policy only applied to children who had not yet completed six or more years by 1993 (UNESCO 2001).

The change in compulsory schooling law led to a dramatic increase in the Gross Enrollment Ratio in secondary schools,[2] from 67 percent in 1990 to 91 percent in 2010 (World Bank 2014). Female children, indigenous children, and children who were poor or living in rural areas at the time of implementation benefited most from this policy shift, as these children had historically faced the greatest educational disadvantages (UNECOSOC 2011; UNESCO 2014). For instance, in 1994, the illiteracy rate among rural women in Peru was four times greater than it was among rural men (UNESCO 2002). Extending the years of compulsory schooling was intended to bridge such gender gaps in basic education and skills (UNESCO 2002).

DATA AND METHODS

DATA

This analysis relies on a nationally representative, stratified random sample of women of reproductive age. These data were collected continuously through standardized Demographic and Health Surveys (DHS) conducted in Peru between 2004 and 2012 with the support of the Peruvian Ministry of Health and the United States Agency for International Development (USAID). The DHS are collected in over 80 lower and middle-income countries and are commonly used to monitor and evaluate demographic patterns and change (for more information see www.dhsprogram.com).

All women aged 15 to 49 in sampled households were interviewed. For safety reasons, and to ensure data quality, however, the DHS only administered the IPV module to one random woman per household and only if that woman’s privacy could be secured. Questions about IPV were not asked to men. Ninety-nine percent of selected women completed the IPV module.

ANALYTIC SAMPLE

To isolate the effects of education, I compare respondents who were within three years above the exposure age for CS11 in 1993 (N= 8,195) to women who were at or within three years below the exposure age for CS11 in 1993 (N= 6,645), as illustrated in Figure 2. To ensure that schooling was completed by the time of survey, I restrict the analysis to

[2]This is calculated by dividing the number of school-age children enrolled by the total number of school-age children in the population.

respondents aged 23 and older. Thus, all respondents in the final analytic sample were between 23 and 33 years of age at the time of interview. To contextualize the outcomes of interest, however, I present means from the broader sample of DHS respondents aged 15 to 49 years old (Table 1) that describe IPV and theoretically related mechanisms for the average Peruvian woman of reproductive age.

MEASURES

EDUCATION—Education is a continuous variable measured with respect to the highest year of completed schooling. The lowest value (0) indicates that a respondent had no schooling. The highest value (17) indicates that a respondent completed 17 years of schooling, which in Peru, means that she likely completed college. In the DHS, education is truncated at 17 years, which may slightly attenuate effect estimates.

INTIMATE PARTNER VIOLENCE—The DHS intimate partner violence module included questions about psychological, physical, and sexual forms of intimate partner violence, based on a modified version of the Conflict Tactics Scale (CTS) (Murray A. Straus, Hamby, Boney-McCoy, & Sugarman, 1996). The CTS has been criticized on several fronts. One resounding concern is the scale's reliability (R. P. Dobash et al., 1992). However, in the Peruvian DHS data, the Cronbach's alpha for all IPV measures is .85, suggesting a high degree of internal consistency across IPV items. Another concern is that the CTS measures specific actions rather than resultant harm (R. P. Dobash et al., 1992; Walby & Towers, 2017). In other words, estimates of IPV do not necessarily translate into analogous estimates of injury. The CTS's lack of information on injury is especially problematic to assessments of gender symmetry in dyadic violence (R. P. Dobash et al., 1992; Murray A Straus, 2012; Walby & Towers, 2017). However, given that the DHS did not include equivalent questions about respondents' treatment of partners, I focus exclusively on women's experiences of IPV victimization. In sum, the measures used in this study, based on the CTS, allow me to assess the effects of women's education on their self-reported experiences of IPV but do not allow for inferences about women's corresponding use of violence or resulting health outcomes.

Respondents selected to participate in the module were first asked three questions about psychological violence. These included, "Has your spouse ever…" "Humiliated you?" "Threatened you with harm?" And "Threatened to leave you and/or take your children?" Respondents who answered "yes" were asked a follow-up question about whether this had occurred "sometimes, often, or not at all within the past twelve months." Relying on information about the occurrence and timing of psychological violence, I create two measures—*psychological violence in the last year* and *psychological violence ever*. For both measures, I code respondents who reported experiencing one or more types of violence (1), and respondents who reported experiencing none (0). Eight respondents (0.05% of the analytic sample) were missing information on all psychological measures and are excluded from the analysis. Thirty-three percent of women in the DHS reported ever experiencing at least one form of psychological violence in their lifetimes; seventeen percent reported experiencing at least one form within the last 12 months (Table 1).

Seven questions about physical forms of IPV were asked in the Peruvian DHS. In their original form, these included “Does your husband ever slap you?” “Twist your arm or pull your hair?” “Push you, shake you, or throw something at you?” “Punch you with a fist or something that could hurt you?” Kick you, drag you, or beat you up?” and “Threaten/ attack you with a knife, gun, or any other weapon?” As with questions about psychological violence, respondents who answered “yes” were subsequently asked whether this form of violence had occurred within the last 12 months. Based on answers to these questions, I create two measures of physical IPV—*physical violence in the last year* and *physical violence ever*—where respondents who reported experiencing one or more types of violence are coded (1), and respondents who reported experiencing none are coded (0). Five respondents (<0.05%) were missing information on all physical measures and are excluded from the analysis. Thirty-nine percent of women in the DHS reported ever having experienced physical violence from their partner; fourteen percent reported experiencing physical violence within the last year (Table 1).

The DHS also asked two questions pertaining to sexual IPV. These included whether a respondent’s partner had ever “forced [her] to have sex,” and whether her partner had ever forced her to “perform other sexual acts.” Again respondents who answered “yes” were asked whether this had happened within the last 12 months. Like psychological and physical violence, I create measures of *sexual violence in the last year* and *sexual violence ever,* in which respondents are coded (1) when they reported being forced to have sex or to perform other sexual acts and (0) when they reported neither. Two respondents (0.01%) provided no information about sexual IPV and are excluded from corresponding analyses. Ten percent of women reported experiencing sexual IPV previously in their lifetime; four percent reported experiencing sexual IPV within the last year (Table 1).

Among women reporting any form of IPV, overlap between psychological, physical, and sexual forms was common. For instance, among women who experienced any IPV in the year before survey, 35% reported experiencing two forms of violence (e.g. psychological *and* physical), while 11% reported experiencing all three forms. Likewise, among women who experienced any form of IPV in their lifetimes, 39% reported experiencing two forms and 17% reported experiencing all three (analysis not shown). Given the extent of overlap, I create two composite measures of IPV: *any form of violence in the last year* and *any form of violence ever*. For these measures, respondents are coded (1) when they reported at least one type of psychological, physical, or sexual IPV and (0) when they reported none. Twenty-two percent of respondents experienced at least one form of IPV (psychological, physical, or sexual) in the year before survey; 48% experienced at least one form within their lifetimes (Table 1).

I also create two measures of recent and lifetime poly-victimization. *Two or more forms of IPV is* defined (1) when respondents reported experiencing a combination of psychological, physical, and sexual IPV and (0) when respondents reported experiencing either no IPV or one form only (psychological, physical, or sexual). Ten percent of DHS respondents experienced two or more forms of IPV in the last year; 27% experienced two or more forms within their lifetimes (Table 1).

POTENTIAL MECHANISMS—In addition to intimate partner violence, I investigate four theoretical mechanisms by which women's education may affect the probability of experiencing IPV. Descriptive statistics for each mechanism are presented in Table 1.

Mechanism 1: Women's resources.: Women's resources are conceptualized as literacy (a cognitive skill), employment, and occupational status. To assess literacy, DHS investigators showed survey participants a card with one sentence written on it. Investigators then noted whether respondents could not read at all, could read only parts of the sentence, or could read the entire sentence. I code those who could read the entire sentence (1) literate, and those who could not (0) illiterate. Eight blind participants (0.04% of the sample) are excluded from this measure.

Respondents' employment status (1/0) is derived from the question "Have you done any (remunerated) work in the last twelve months?" To capture both formal and informal employment, DHS interviewers reminded respondents that work included jobs that were compensated in-kind or with cash, and that work included owning a small business, selling things informally, or working on the family farm or in the family business. Seventy-seven percent of women in the DHS had been employed in the previous year (Table 1).

Employed respondents in the DHS were also asked about their occupation. Using Treiman's (1976) Occupational Prestige Scale as a guide, I assign respondents to a prestige level, ranging from (1) to (8), with (8) equaling the greatest occupational prestige. I exclude non-employed respondents from this measure because non-employment is already captured in the dichotomous employment measure.[3]

Mechanism 2: Family formation.: I test two measures of early family formation: teen marriage and teen parenthood. Both are measured dichotomously and pertain to whether the respondent entered her first marriage or had her first child before the age of 20. Because all respondents included in this study are age 23 or older these measures do not need to be censored. Fifty-four percent of Peruvian women reported marrying as a teen; forty-six percent had their first birth during their teen years (Table 1).[4]

Mechanism 3: Partner selection.: I measure partners' resources with their *years of schooling* (0–17) and their *occupational prestige* (1–8), the latter of which is based on Treiman's (1976) Occupational Prestige Scale. These measures can only be assessed among currently partnered respondents.

Mechanism 4: Attitudes.: Starting in 2005, the Peruvian DHS included five questions regarding women's attitudes towards intimate partner violence. Specifically, these questions were whether "Wife beating is justified if a woman…" "goes out without telling her spouse," "neglects her children," "argues with her spouse," "refuses to have sex," or "burns

[3]When including non-employed women (n=3,930, 26%) in the occupational prestige scale and assigning them a value of (0), the estimated effect of education retains its statistical significance but is slightly attenuated (available upon request).

[4]Relying on measures of earlier family formation, at ages 15 and 18, leads to substantively similar conclusions. However, the estimated effect of education is smaller in magnitude when assessing its relationship to family formation at these earlier ages, and the estimated effect of women's education on the likelihood of first birth occurring before age 15 fails to reach statistical significance at conventional levels (results available upon request).

the food." Based on responses to these questions, I code respondents as believing *IPV is OK* (1) if they responded "yes" or "don't know" to any question and (0) if they responded "no" to all questions.[5] Only six percent of women in the DHS reported "wife beating" is ever justified (Table 1). No attitudinal questions about psychological or sexual IPV were asked in the Peruvian DHS.

CONTROL VARIABLES—To improve the precision of estimates I adjust for respondents' ethnolinguistic group (Spanish, Quechua, Aymara, or other indigenous group); childhood location (state capital, city, town, or rural); number of siblings (0 to 20);[6] family history of IPV;[7]and state of residence in all models (Table 2). While the DHS does not provide an indicator of childhood poverty, ethnolinguistic group and childhood location together should capture much of the variance related to respondents' childhood socioeconomic status. Models do not adjust for current marital status or other post-treatment variables (except for state of residence) because these variables may be endogenous to the outcome (Gelman & Hill, 2007). For example, a respondents' education may affect her marital status by improving her income. These effects of education on income may provide respondents with means of leaving an abusive relationship, thereby simultaneously affecting marital status and probability of experiencing IPV before survey.

ANALYTIC STRATEGY

INSTRUMENTED REGRESSION DISCONTINUITY—The first set of analyses is intended to make causal inferences about the effects of women's education by using a technique known as an instrumented or "fuzzy" regression discontinuity (IRD). This strategy improves upon conventional, naïve models by overcoming the issue of endogeneity. For instance, in OLS models, unobserved characteristics that simultaneously affect years of education and IPV, such as parents' gender ideology, will bias estimates if they contribute to both the independent and dependent variable (Figure 1b). IRDs avert this bias by comparing two groups that fall immediately above and below an exogenously determined threshold and that therefore do not systematically differ. The threshold in this study is age 11 in 1993: women who were ages 11 and younger in 1993 should have been enrolled in primary school and thus exposed to CS11; women who were ages 12 and older in 1993 should have already completed primary and not been exposed (Figure 2).

Given that women who were age 11 in 1993 should be indistinguishable from women who were age 12 in 1993 (Table 2), comparing these two groups should inherently control for all potential confounders. Nevertheless, grade repetition and delayed school entry are common in Peru (Pal, 2004), meaning that at least some women who were age 12 or older in 1993 could have been enrolled in primary school at the time of CS11. Given that it is impossible to determine whether each individual respondent was exposed, I exploit discontinuities in the *probability* of CS11 exposure using two-stage least squares regression (2SLS) that

[5]Excluding respondents who "don't know" (n=91, <1%) from the measure yields identical results (available upon request).
[6]Number of siblings is only adjusted for in the first stage, however, because it should not theoretically affect IPV.
[7]Four percent of the analytic sample was missing information on history of family violence. These respondents are excluded from all analyses. The estimated effects of education on IPV remain similar when including these respondents and not controlling for history of family violence (available upon request).

instruments years of education with age in 1993. The first stage generates predicted values of years of education, $\widehat{D}$, conditional on being age 11 or younger in 1993, Z, and respondents' background characteristics, X (ethnolinguistic background, childhood location, number of siblings, history of family violence, and state of residence) (Eq. 1):

$$\widehat{D}_i = \alpha_0 + \alpha_1 Z_i + \ldots \alpha_k X_k \quad \text{(Eq. 1)}$$

The second stage uses respondents' estimated years of education, $\widehat{D}$, to predict a given outcome, Y (Equation 2):

$$Y_i = \beta_0 + \beta_1 \widehat{D}_i + \ldots \beta_k X_k + \varepsilon_i \quad \text{(Eq. 2)}$$

In other words, age in 1993 (a proxy for CS11 exposure) is first used to predict years of education. The resulting estimate is then used to predict IPV. Because a second aim of this study is to investigate theoretical mechanisms, and because a variable must directly be affected by women's education to be a plausible mechanism, I repeat this process using estimated years of education, $\widehat{D}$, to predict each mechanism indicator separately. To account for the DHS clustered sampling design, the standard errors in these models are clustered by survey cluster.

In order for IRD analyses to yield unbiased estimates, they must meet several criteria. First, any educational increase associated with CS11 exposure should capture a true discontinuity, as opposed to a secular time trend. Second, in order to isolate the effects of education, the instrument must affect the outcome only via the treatment. The main threat to this criterion is an effect of CS11 on partners' education. Third, the instrument is assumed to be exogenous (Angrist & Imbens, 1995). Although unlikely, this could be violated if parents timed their births or urged their children to complete primary school early in anticipation of CS11. Finally, although not specific to the IRD approach, one challenge this study faces is that the primary outcome of interest—intimate partner violence—is experienced within relationships. If one pathway by which education reduces IPV is delayed relationship entry, then excluding never-partnered women from this analysis should attenuate effect estimates. I test and discuss each of the criteria in thorough detail after presenting the main results.

MEDIATION ANALYSIS—Instrumented regression discontinuities, although they facilitate causal inferences about the effects of education, do not allow for a formal assessment of mediation. Therefore, in a second set of analyses, I more explicitly test the mechanisms by which education affects IPV (if at all). To do so, I estimate a series of naïve, nested models. The first model uses reported years of education to predict *any form of IPV in the last year* without controlling for any mechanism indicators. Subsequent models control for one indicator at a time, revealing the extent to which an individual mechanism mediates the relationship between education and IPV.

This second set of analyses helps to disentangle the relationship between education and IPV in two ways. First, naïve estimates of the effect of education will be downwardly biased if an unobserved characteristic like economic hardship leads women to exit school early and to enter or stay in violent relationships and will be upwardly biased if an unobserved characteristic like female headship of women's childhood home leads them to stay in school longer and to enter or stay in violent relationships. Comparing the results of the first naïve model to the results of the IRD analyses therefore illuminates the magnitude and direction of such bias. Second, controlling for any pathway by which education affects IPV should reduce the observed magnitude and significance of the effect of IPV in naïve models. Adding potential mediators into the model one by one therefore helps to adjudicate between them. However, when adding potential mediators to the models, the sample size and composition may change in ways that could bias the estimated effect of education. For example, when examining whether partners' education mediates the effects of respondents' education, the sample is restricted to partnered women. Thus, to further verify the extent to which the relationship between education and IPV is reduced when each mechanism is considered, I implement Sobel-Goodman tests. Because Sobel-Goodman tests can only be conducted using a linear specification without clustering standard errors, these analyses are estimated using non-clustered linear probability models.[8]

In light of the possibility that distinct mechanisms may mediate the effects of education on different forms of IPV (psychological, physical, and sexual), I repeat all mediation analyses estimating each form of IPV separately. The resulting pattern of effects across models suggests that, overall, those factors mediating the effect of a woman's education on her likelihood of experiencing any form of IPV are similar to the factors mediating the effect of a woman's education on her likelihood of experiencing psychological, physical, and sexual IPV specifically.[9]

RESULTS

RESULTS OF INSTRUMENTED REGRESSION DISCONTINUITY

The first component of this analysis is intended to discern whether women's education has a causal, negative effect on their likelihood of IPV victimization. In order for instrumented regression discontinuities to be valid with one endogenous regressor, the t-value on the exposure variable, in this case CS11 exposure, must be larger than 3.2; the corresponding p-value, must be below 0.0016; and the *F* statistic for the excluded instrument must be greater than 10 (Stock, Wright, & Yogo, 2002). I therefore begin by assessing the relationship between CS11 exposure and respondents' education. The results of this first stage suggest that being exposed to CS11 increased a woman's years of schooling by 0.21 years ($p<0.001$, Table 3). Figure 3 graphically depicts this jump. Importantly, the strength of this relationship ($p<.001$) and the F statistic for the excluded instrument ($F = 280$; $p<0.001$) indicate that CS11 exposure serves as a valid instrument for years of schooling.

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[8]As a sensitivity test, I rerun all models using logistic regression with standard errors clustered by survey cluster (but without Sobel-Goodman tests). The naïve regression results remain substantively unchanged (available upon request).

[9]The strength of mediation effects, however, varied slightly across the different IPV forms (available upon request).

In the second stage, I regress IPV on the estimated years of education generated in the first stage. This reveals that increasing women's schooling in Peru diminished their probability of reporting all three forms of intimate partner violence—psychological, physical, and sexual (Table 4). More specifically, increasing a woman's years of schooling by one year reduced her recent and longer-term probabilities of psychological violence by 1 and 2 percentage points, respectively. These differences each constitute 6% reductions from the mean among Peruvian women (shown in Table 1). In terms of physical violence, a one-year increase in schooling decreased recent and longer-term probabilities by 1 and 3 percentage points—reductions of 7% and 8%, compared to the average Peruvian woman. Increasing schooling by an additional year similarly decreased recent and longer-term probabilities of sexual violence by 1 percentage point each. In relative terms, these effects constitute 25% and 10% reductions from the mean.

Increasing women's schooling also reduced their probability of experiencing any form of IPV at all and their probability of experiencing two or more forms of IPV relative to none or one form only. That is, a one-year increase in schooling constituted 2 and 4 percentage point reductions in the probability of experiencing any form of IPV within the last year and in a woman's lifetime, respectively (Table 4). These effects equate 9% and 8% reductions in the probability of reporting any form of IPV compared to the average Peruvian woman of reproductive age. Likewise, increasing a woman's schooling by one year decreased her probability of recent poly-victimization by 1 percentage point, or 10% compared to the mean among Peruvian women; and her probability of lifetime poly-victimization by 2 percentage points—a 7% decrease relative to the mean.

Given that for each IPV measure, increasing women's schooling decreased the prevalence of both lifetime and recent IPV, these findings suggest that increasing women's education reduced the onset of IPV, rather than only reducing the recurrence of violence after initial onset.

Any proposed mechanism by which education reduces IPV must stem at least in part from women's schooling. To test this, I employ the same 2SLS instrumented regression discontinuity design, using women's estimated years of education to predict each of the theoretical mechanisms. First I evaluate the effect of women's education on their individual resources (Mechanism 1). This suggests that increasing a woman's education by one year increased her probability of being literate by 1 percentage point but had no effect on her probability of employment compared to if she had not received an additional year of education (Table 5). However, among employed women, a one-year increase in education improved occupational status by 0.38 points (5% of the 8-point scale).

Next I examine the effect of education on early family formation and partner selection (Mechanisms 2 and 3), finding that a one-year increase in women's education decreased their probabilities of teen marriage and teen parenthood by 5 percentage points each (Table 5)—reductions of approximately 9–11% from the mean among Peruvian women. A one-year increase in respondents' education is also associated with 0.34 more years of schooling and with 0.30 higher occupational prestige scores among partners (Table 5). Finally, I assess the extent to which improving women's education affected their attitudes toward intimate partner violence (Mechanism 4). The findings suggest that increasing women's education by one year decreased their probability of believing IPV is ever acceptable by 1 percentage point (Table 5)—a 17 percent reduction compared to the average Peruvian woman.

Thus, the results of instrumented regression discontinuities offer two clear takeaways. First, women's education has an independent, negative effect on their risk of IPV victimization. This effect is consistent across different types of IPV and with respect to both recent and lifetime prevalence. Second, increasing women's education also affects four domains of their lives that may explain why education has this negative effect—through changes in women's personal resources, family formation, partner selection, and attitudes. In the following section I formally test the extent to which each of these domains acts as a mediator.

RESULTS OF FORMAL MEDIATION ANALYSIS

Having demonstrated that women's education has a causal effect on IPV, the second analytic component of this study investigates the explanatory power of different potential mechanisms. As a first step, I estimate the effect of education on the composite measure of any form of IPV in the last year (psychological, physical, or sexual) using a naïve linear specification without controlling for any mediators. The results indicate that increasing women's education by one year decreased their likelihood of experiencing any form of IPV within the last year by 0.3 percentage points (Table 6). This estimate serves as a basis for comparison when adding potential mediators into subsequent models. It is also worth noting that this estimate is only one-sixth the size of the point estimate of the IRD model presented in Table 4, highlighting how the issue of endogeneity, if unaddressed, leads the estimated effect of women's education to be downwardly biased.

Despite the issue of endogeneity, nested naïve models help to reveal the specific pathways by which education affects IPV. In addition, they illuminate how discrepancies could have arisen between past studies, conditional on which aspects of women's lives they controlled for. In other words, if a variable mediates the effects of education, then the coefficient on education should become attenuated and statistically weaker when that variable is included in the model than when it is not. Comparing the results of the first model in Table 6 to subsequent models suggests that the negative effect of women's education on IPV is primarily mediated by their occupational prestige, teen parenthood, and their partners' education. The results of Sobel Goodman tests further confirm that these indicators explain between one-fifth and slightly less than one-half of the effect of women's education on IPV.

Although the Sobel-Goodman tests in Table 6 are consistent with the IRD analyses in Table 5 in that they suggest women's education affects their literacy and attitudes toward IPV, Sobel-Goodman tests indicate that women's education retains its strong significance ($p<.01$) even when these two variables are included in the model. The Sobel-Goodman tests in Table 6 also suggest that education affects the occupational prestige of the partners women select. However, they additionally indicate that partners' occupational prestige is not correlated with IPV, meaning that partners' occupational prestige is unlikely to be a mediator.

ROBUSTNESS TESTS

The first portion of this study's analysis—the instrumented regression discontinuity—is intended to yield unbiased estimates of the effects of education by implicitly holding constant all potential confounders. However, the validity of IRD analyses hinges on several assumptions, each of which I describe and test below.

ASSUMPTION 1: RESULTS ARE NOT DRIVEN BY A SECULAR TREND

One concern that arises in regression discontinuity analyses is that the jump in the explanatory variable—in this case the jump in education—does not represent a "true" discontinuity, but instead, captures a secular trend over time. If so, then the estimated effects of education would be confounded by broader cross-cohort variation occurring independently of CS11.

In light of this possibility, I conduct a placebo test of the first stage in which I compare the adjusted years of schooling across two adjacent groups of cohorts, *neither of which was exposed to CS11* (one group was aged 12 to 14 in 1993; the other was aged 15 to 17 in 1993). Since neither group was exposed to CS11, there should be no significant difference in their schooling. As expected, the placebo test yields null results (available upon request). Nevertheless, as a second sensitivity test, I rerun all IPV models controlling for birth cohort and survey year in the first and second stage. The results of these models, presented in Appendix A, remain substantively unchanged from those presented in Table 4. Furthermore, post-estimation Hansen *J* tests largely indicate that the identifying instruments are uncorrelated with the error term in these sensitivity tests. If the effects of education were attributable to a secular time trend, then the Hansen *J* test statistic should indicate that the error term in the second stage is correlated with the time trend in the first stage.

ASSUMPTION 2: RESULTS ARE NOT DRIVEN BY AN ANALAGOUS EFFECT OF CS11 ON PARTNERS' EDUCATION

Another challenge this study faces is that both men and women who were enrolled in primary school in 1993 were exposed to CS11. This means that CS11 exposure could have affected at least some women's partner's education. If so, then it would be impossible to entirely isolate the effects of *women's* education from the effects of their partners' education.

An examination of age heterogamy between respondents and their partners reveals that the average age difference is 4.5 years, with male partners tending to be older. Because the youngest cohort of respondents analyzed was born in 1984, the average respondent's partner was born in 1980 or earlier. As a consequence, most respondents' partners were age 13 or older in 1993 and therefore should not have been exposed to CS11. Nevertheless, to gain a better understanding of the extent to which partners' simultaneous exposure to CS11 biases the estimates presented above, I rerun all models limiting the sample to respondents whose partners were born in 1981 or before (the majority of whom should not have been exposed to CS11). The results, presented in Appendix B, are nearly identical to those in Table 4. Thus, although not conclusive, this supplementary analysis lends confidence in the assumption that the main IRD results are not driven by partners' CS11 exposure.

ASSUMPTION 3: WOMEN'S EXPOSURE TO CS11 WAS EXOGENOUS

A third threat to validity is the possibility that women who were and were not exposed to CS11 are systematically different (Angrist & Imbens, 1995). If this were to occur, then similarly to naïve analyses, the estimated effects from IRD models would be biased. This possibility is the primary reason I limit the analytic sample to women aged within three years above and below primary school completion—to ensure that the groups do not differ substantially in terms of experiences associated with age or cohort while still leaving a sample size large enough to generate statistical power and minimize Type II error. Using this bandwidth, there is still a five-year age difference between the youngest (1984) and oldest (1979) birth cohorts in the analytic sample, which may be large enough to render the exposed/unexposed groups meaningfully different. I therefore run more conservative models where the sample is restricted to women aged two years above and below the cutoff (between the ages of 10 and 13 in 1993). These alternative models (Appendix C) generate results comparable in direction and significance to the primary analyses presented in Table 4 above.

Another way in which the exposed/unexposed groups could be systematically different is if parents anticipated the policy reform and correspondingly manipulated the timing of respondents' birth. This is highly unlikely given that respondents were born 9 to 14 years prior to CS11 implementation. Moreover, bivariate analyses comparing means and distributions of background characteristics across the two groups provides little evidence of selection into CS11 exposure (Table 2). The only significant difference between them is their average number of siblings and this difference is trivial (0.20).

A third possibility related to non-random sorting is that, after the education reform became public knowledge, some parents urged respondents to complete primary school early to avoid having to keep them enrolled longer. This would be detectable in the data—the unexposed group would be denser, especially at ages closest to the threshold. To test this I use a manipulation test based on local polynomial density estimation (McCrary, 2008). The results of this test are null (p=.4), which suggests that the density of observations on both sides of the threshold are equivalent and thus that any ex post facto sorting was negligible.

ASSUMPTION 4: EFFECTS ARE LARGER WHEN ESTIMATED AMONG BOTH EVER- AND NEVER-PARTNERED WOMEN

The DHS intimate partner violence module only asked questions about IPV to women who had ever been in a relationship, but delayed relationship formation may be one pathway by which education reduces IPV. Intuitively, then, the estimated effects of education on IPV should be even greater when women who had never been in a relationship are included in the model.

To assess this possibility I rerun the first stage model including respondents who were selected to participate in the module but who had never been in a relationship, and then rerun the second stage models including these same respondents, coding them as (0) for all IPV outcomes. Indeed, when including these women in the first stage, I find that being exposed to CS11 implementation increased women's years of schooling by 0.28 years—an effect that is notably larger than in the main analysis (Appendix D.1). This likely reflects that for women for whom CS11 induced a large increase in education, schooling competed with relationship formation. Also as anticipated, in the second stage, the effects of women's education are significant, negative, and slightly larger in magnitude than the effects found in the main analysis (Appendix D.2).

DISCUSSION AND CONCLUSION

Although rigorous studies employing causal methods firmly establish that educational expansion has had a negative effect on a wide variety of crimes in the United States and Europe, few studies have applied such methods to the study of intimate partner violence specifically or to the study of educational expansion outside these two contexts. In light of this paucity, I offered one of the first causal analyses examining the individual-level effects of women's education on the risk of IPV victimization using a compulsory schooling reform in Peru. Despite that a negative educational gradient in IPV has been observed in diverse contexts around the globe, some research has found that women's education has no effect on IPV net of other personal and household characteristics. Such discrepancies arise from a longstanding, tacit assumption that education has a causal effect. By taking this assumption for granted, scholars have failed to consider an equally plausible, alternative explanation—that women's endowments may simultaneously affect education and IPV, thus explaining the observed relationship. To adjudicate between the two possibilities, I exploited an exogenous source of variation in women's schooling and used an instrumented regression discontinuity that compared IPV rates across two highly similar groups of women who differed only with respect to their schooling. The results provided robust, compelling evidence in support of the social causation perspective. That is, increasing women's education was found to decrease their recent and longer-term probabilities of three different forms of IPV—psychological, physical, and sexual—and to reduce their recent and longer-term probabilities of experiencing any form of IPV as well as their probability of experiencing poly-victimization (more than one form of IPV). Thus, as education continues to expand in Peru and the average woman achieves greater levels of schooling, the prevalence of IPV is likely to continue to decline.

Why then have some studies found women's education to have no effect on IPV? One issue may be a lack of statistical power. However, another likely explanation is that scholars have controlled for various different facets of women's lives, some of which explain the relationship between education and IPV and thus render the "effect" of education null when controlled for. In consideration of this possibility, I drew on diverse theoretical perspectives to propose and test four different potential mechanisms.

First, increasing women's education is likely to augment their personal resource bases, such as their cognitive skills and employment prospects. As dependency theory stipulates, improvements to women's personal resources should decrease their economic dependence on partners in ways that make it more feasible for them to leave abusive or unhappy relationships in which violence takes place (Kalmuss & Straus, 1982). Moreover, if improved cognitive skills result in improved interpersonal skills, then education's impact on cognition could result in healthier, more communicative relationship dynamics that prevent dyadic IPV. The results of IRD analyses and mediation analyses, including Sobel-Goodman tests, suggested that if education affects IPV via changes in women's resources, then it is primarily through their occupational prestige rather than through their employment or literacy.

Second is the possibility that education reduces IPV by leading women to delay family formation. This could occur if, as dependency theory also suggests, the physical and economic demands imposed by children exacerbate women's vulnerability to IPV. It could also occur if by delaying family formation women are able to achieve higher occupational prestige and/or greater earnings or if delaying family formation leads to greater relationship satisfaction. Nested models with Sobel-Goodman tests offered partial support for this explanation. Specifically, they indicated that women's education substantially reduces their probability of teen marriage and teen parenthood, and that teen parenthood in particular significantly reduces their probability of recent IPV.

Resource and family stress theory offer a third theoretical pathway from increased education to decreased IPV—via partner selection and associated financial stress and conflict (Fox et al. 2002). For instance, reductions in financial stress may not only stem from improvements in women's personal resources, but also from selecting partners with greater human capital. To some extent, the results of mediation analyses also affirmed this possibility. However, Sobel-Goodman tests indicated that partners' education more than their occupational prestige explain the educational gradient in IPV. A fourth possibility is that women's education protects against IPV by deterring attitudinal acceptance of it. Though an IRD analysis suggested that increasing women's education does negatively affect attitudes toward it, mediation analyses suggested that attitudinal differences explain little of education's effect on IPV.

These findings have important implications for how scholars conceptualize the effects of women's education on IPV. Not only do they confirm that the widely observed negative relationship between education and IPV is causal, but they also illuminate the superior explanatory power of some mechanisms over others. For instance, although education is often thought of as a "resource", the only resource pathway by which education was found to mediate IPV was through occupational status, not literacy or employment. Moreover, delayed childbearing had a large mediation effect, suggesting that the timing of family formation may be of equal or greater importance than improving women's economic resource bases. Future scholarship concerned with the determinants of IPV may therefore benefit from placing greater emphasis on the role of childbearing and family formation. Furthermore, future scholarship may benefit from better understanding whether women's education has similar effects on gender-based IPV (enacted primarily by men) and on reciprocal, dyadic IPV (enacted by both partners). In this study, I was unable to distinguish between the two because the DHS did not ask comparable questions about women's treatment of partners.

This study faces several other limitations. In particular, IPV was self-reported in the DHS. If education not only affects IPV but also the reporting of it, then the estimated effect of education could be overstated. Relatedly, the DHS did not collect a comprehensive history of IPV experiences. Without this information I was unable to identify women whose first experience of IPV occurred while still in school. These women's experiences may downwardly bias estimates of IPV ever occurring within a respondents' lifetime, especially if they contributed to school dropout. Furthermore, because the DHS did not ask about respondents' sexual orientation, I could not distinguish between relationships that were hetero and non-heterosexual. A final limitation is that the DHS did not collect comprehensive measures of non-partner violence victimization. This disallowed me from determining whether the observed effect of women's education on IPV reflects a broader decline in violent crime owing to educational expansion in Peru.

Over the last two decades, numerous Latin American countries besides Peru have adopted or amended compulsory schooling policies. For example, Mexico extended its number of compulsory schooling years by three years in 1992. Brazil made eight years of formal education compulsory in 1996; Belize and Chile made nine years compulsory in 2000 and 2003 respectively; and most recently, Colombia made five years compulsory in 2010 (Los Angeles Times 2010; Reimers 2006; World Bank 2006). These changes in compulsory schooling highlight the possibility for future scholarship to continue to investigate the effects of educational expansion on IPV, and on crime more generally, in Latin America. Such studies would benefit from employing study designs similar to the one here and would further benefit from investigating potential heterogeneity in the effects of women's education across institutional and cultural contexts. Expanding existing evidence of the effects of education on IPV is critical to better understanding socioeconomic disparities in family violence and related health outcomes.

Acknowledgements:

This research was conducted while a postdoctoral fellow at the University of Michigan, with support from a training grant from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (T32 AG000221).

Biography

Bio: Dr. Abigail Weitzman is an assistant professor in the Sociology Department and a research affiliate of the Population Research Center and the Teresa Lozano Long Institute of Latin American Studies at the University of Texas – Austin.

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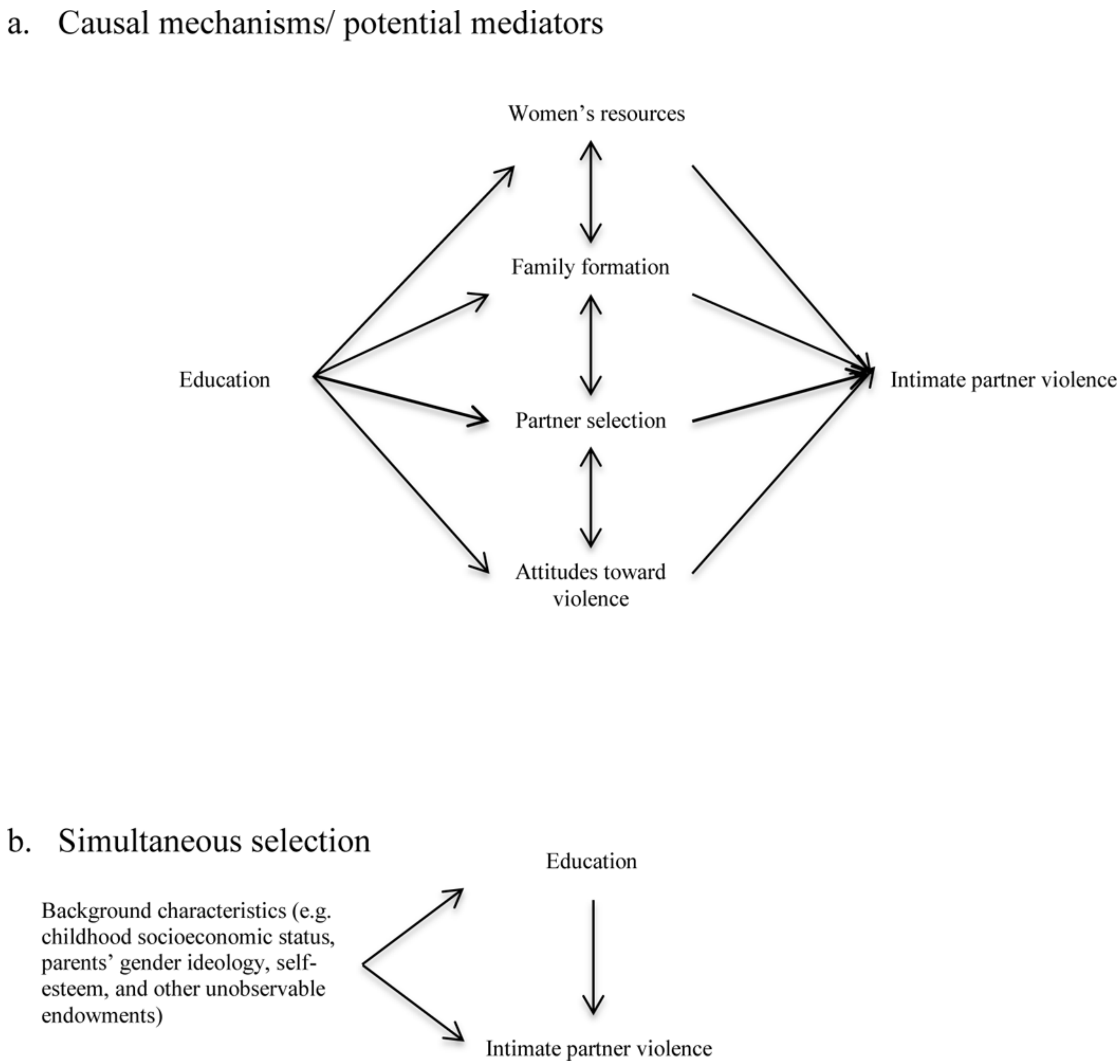

Figure 1.
Theoretical Relationships between Women's Education and Intimate Partner Violence

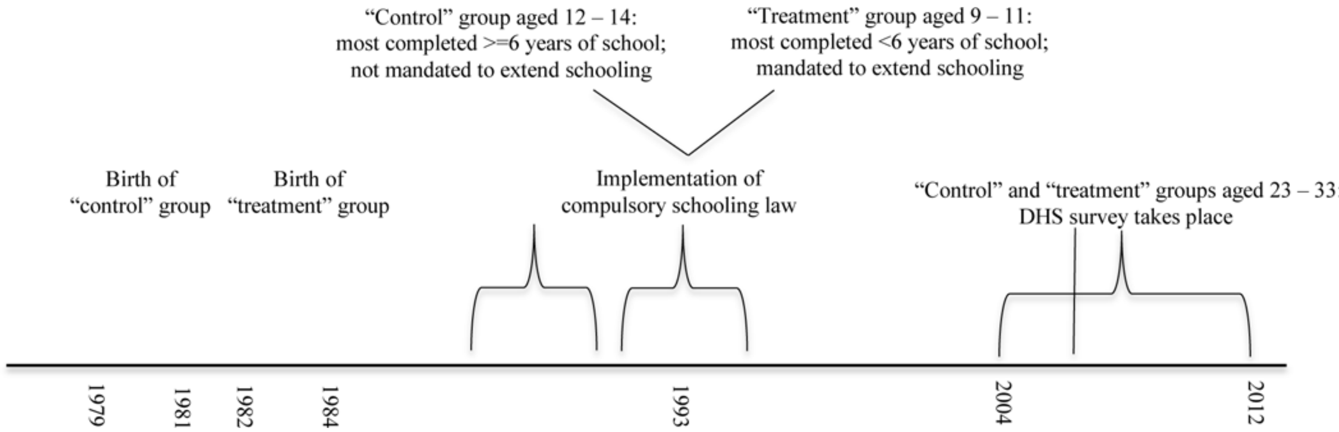

Figure 2.
Timeline of Events

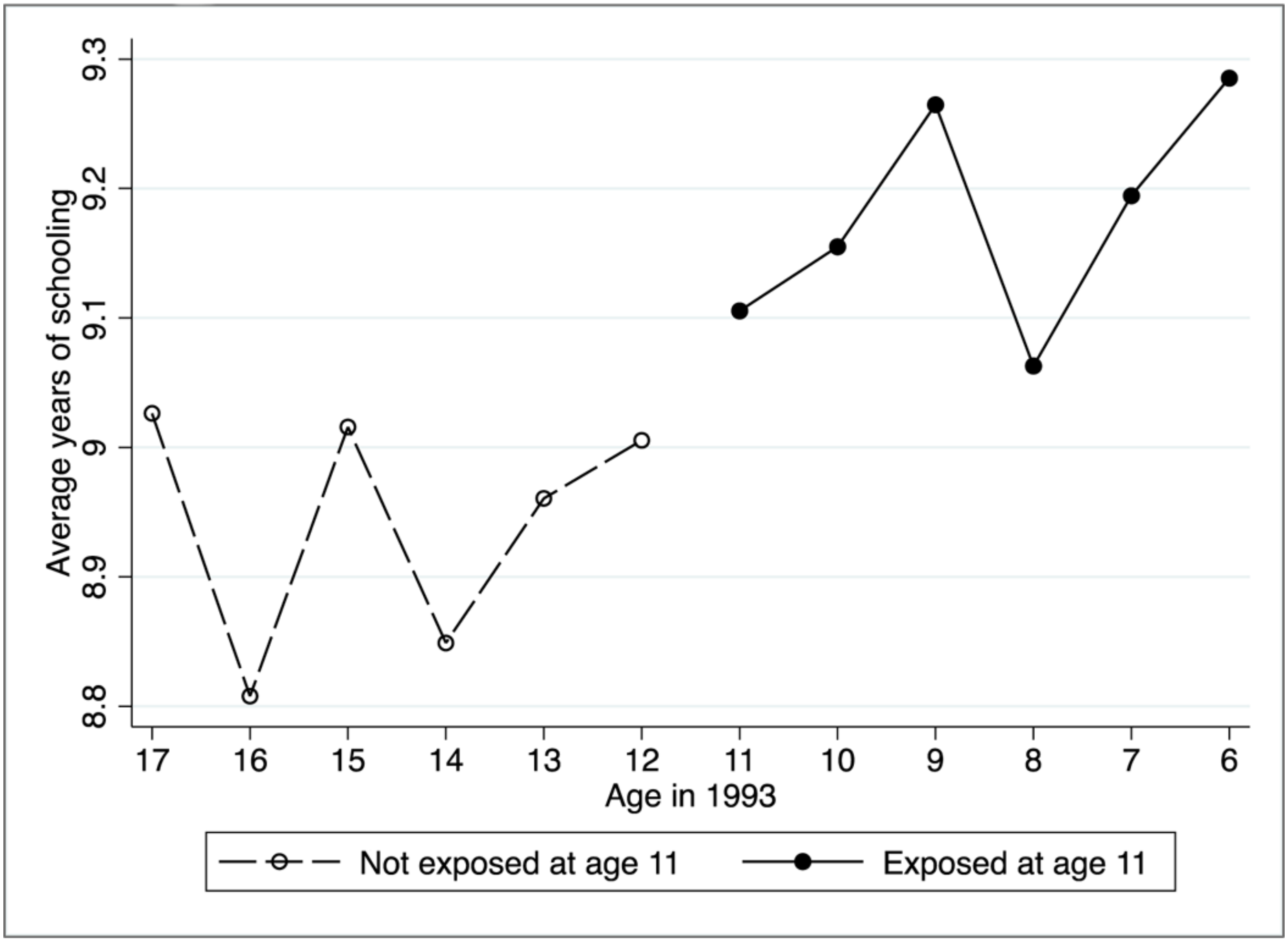

Figure 3.

Average years of schooling among respondents in the DHS, by birth cohort

Source: 2004 −2012 Peruvian Demographic and Health Survey Data Calculations by author

Table 1.

Intimate Partner Violence and Theoretical Mechanisms among Peruvian Women Aged 15–49 (N=75,203)

	Mean	SD
Intimate partner violence		
Psychological violence within last year	.17	
Psychological violence ever	.33	
Physical violence within last year	.14	
Physical violence ever	.39	
Sexual violence within last year	.04	
Sexual violence ever	.10	
Any form of IPV within last year	.22	
Any form of IPV ever	.48	
Two or more forms of IPV within last year	.10	
Two or more forms of IPV ever	.27	
Mechanism 1: Women's resources		
Literate	.85	
Employed	.77	
Occupational prestige (1–8)[a]	2.81	2.85
Mechanism 2: Family formation		
Married as teen	.54	
First birth as teen	.46	
Mechanism 3: Partner selection		
Partner's years of schooling (0–17)	9.14	3.80
Partner's occupational prestige (1–8)[b]	3.70	2.51
Mechanism 4: Attitudes		
Attitudes: IPV is OK[c]	.06	

Source: Peruvian Demographic and Health Surveys 2004–2012

[a] *Occupational prestige* is calculated among employed respondents only.

[b] *Partner's occupational prestige* is calculated among ever-married and currently partnered respondents only. Observations in 2012 and between August and December 2010 are not included in this measure because information on partners' occupation was not collected at these times.

[c] Attitudinal questions were first asked in 2005. Only observations from 2005 onward are included in this measure.

Table 2.

Unexposed and Exposed Subsamples' Background Characteristics and Results of Bivariate Analyses

	Not exposed to 11-year compulsory schooling at age 11 (n=8,195)		Exposed to 11-year compulsory schooling at age 11 (n=6,645)		
	Mean	**SD**	**Mean**	**SD**	**Sig.**
Ethnolinguistic group: Spanish	.88		.89		
Ethnolinguistic group: Quechua	.09		.09		
Ethnolinguistic group: Aymara	.01		.01		
Ethnolinguistic group: other indigenous	.02		.02		
Grew up in: state capital	.25		.24		
Grew up in: city	.14		.15		
Grew up in: town	.21		.22		
Grew up in: rural area	.40		.40		
Number of siblings	5.76	2.85	5.56	2.84	*
History of family violence	.49		.48		

Note: Results of bivariate analyses noted in "Sig." column.

***p<0.001,

**p<0.01,

*p<0.05,

†p<0.1

Table 3.

First Stage Results Predicting Women's Years of Education

	Years of education	
Exposure at age 11	.21	***
	(.06)	
Constant	11.32	***
	(.20)	
Observations	14,840	
R-squared	.35	
F statistic	280.00	***

Note: All models adjust for ethnolinguistic background, childhood location, number of siblings, history of family violence, and state of residence.

Standard errors in parentheses, clustered by survey cluster

*** $p<0.001$,

** $p<0.01$,

* $p<0.05$,

† $p<0.1$

Table 4.

Second Stage Results Predicting Women's Probability of Reporting Intimate Partner Violence

	Psychological		Physical		Sexual		Any form of IPV		Two or more forms of IPV	
	Within last year	Ever	Within last year	Ever	Within last year	Ever	Within last year	Ever	Within last year	Ever
Estimated years of education	−.01 **	−.02 ***	−.01 *	−.03 ***	−.01 ***	−.01 ***	−.02 ***	−.04 ***	−.01 *	−.02 ***
	(.00)	(.01)	(.00)	(.01)	(.00)	(.00)	(.01)	(.01)	(.00)	(.01)
Constant	.32 ***	.49 ***	.23 ***	.64 ***	.13 ***	.21 ***	.39 ***	.76 ***	.21 ***	.39 ***
	(.05)	(.06)	(.05)	(.07)	(.03)	(.04)	(.06)	(.07)	(.04)	(.06)
N	14,836	14,836	14,837	14,837	14,840	14,840	14,840	14,840	14,840	14,840

Note: All models adjust for ethnolinguistic background, childhood location, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster

*** p<0.001,

** p<0.01,

* p<0.05,

† p<0.1

Table 5.

Second Stage Results Predicting Theoretical Mechanisms

	Mechanism 1: Women's resources			Mechanism 2: Family formation		Mechanism 3: Partner's resources		Mechanism 4: Attitudes
	Literate	Employed	Occupational prestige[a]	Married as teen	1st birth as teen	Years of schooling	Occupational prestige[b]	IPV is OK[d]
Estimated years of education	.01 ***	.01	.38 ***	−.05 ***	−.05 ***	.34 ***	.30 ***	−.01 **
	(.00) ***	(.01)	(.04)	(.01)	(.01)	(.04)	(.03)	(.00)
Constant	.83	0.71 ***	.48	1.03 ***	.99 ***	6.33 ***	1.04 **	.17 ***
	(.04)	(0.06)	(.43)	(.06)	(.06)	(.38)	(.34)	(.03)
N	14,825	14,835	7,586	14,836	14,836	14,743	10,336	14,592

Note: All models adjust for ethnolinguistic background, childhood location; number of siblings, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster

*** p<0.001,

** p<0.01,

* p<0.05,

† p<0.1

[a] *Occupational prestige* is calculated among employed respondents only.

[b] *Partner's occupational prestige* is calculated among ever-married and currently partnered respondents only. Observations in 2012 and between August and December 2010 are not included in this measure (information was not collected at these times).

[c] *Difference in occupational prestige* is calculated among ever-married and currently partnered respondents where both the respondent and her partner were listed as having an occupation and where information on each of their occupations was available.

[d] Attitudinal questions toward IPV were only asked in Peru starting in 2005. Only observations from 2005 onward are included in this measure.

Table 6.

Mediation Analysis Examining the Pathways by Which Education Affects Intimate Partner Violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Years of education	−.003 ***	−.005 ***	−.004 ***	−.003	−.003 **	−.002 †	−.002	−.004 **	−.003 **
	(.001)	(.001)	(.001)	(.002)	(.001)	(.001)	(.001)	(.001)	(.001)
Mechanism 1: Women's resources									
Literate		.040 **							
		(.014)							
Employed			.040 ***						
			(.008)						
Occupational prestige				−.002					
				(.002)					
Mechanism 2:Family formation									
Married as teen					.010				
					(.008)				
1st birth as teen						.020 **			
						(.008)			
Mechanism 3: Partner's resources									
Years of schooling							−.004 **		
							(.001)		
Occupational prestige								.002	
								(.002)	
Mechanism 4: Attitudes									
IPV is OK									.070 ***
									(.015)
Constant	.222 ***	.198 ***	.190 ***	.236 ***	.212 *	.198 ***	.243 ***	.229 ***	.219 ***
	(.023)	(.024)	(.024)	(.033)	(.024)	(.024)	(.024)	(.028)	(.023)
N	14,840	14,829	14,839	7,589	14,840	14,840	14,747	10,340	14,596
		Results of Sobel-Goodman Mediation Tests							
Education's effect on mediator		.039 ***	.004 ***	.266 ***	−.050 ***	−.052 ***	.406 ***	.227 ***	−.004 ***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mediator’s effect on IPV		.044 **	.043 ***	−.002	.010	.024 **	−.004 **	.002	.066 ***
Indirect effect of education		.002 **	.0002 **	−.001	−.001	−.001 **	−.002 **	.000	−.000 ***
Direct effect of education		−.005 ***	−.004 ***	−.003	−.003 **	−.002 †	−.002	−.004 **	−.003 **
Proportion of total effect that is mediated		−.501	−.056	.187	.143	.366	.447	−.106	.081

Note: All models adjust for ethnolinguistic background, childhood location, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster,

*** p<0.001,

** p<0.01,

* p<0.05,

† p<0.1

Appendix A.

Second Stage Results Predicting Intimate Partner Violence in Restricted Sample (Born Between 1980 and 1983)

	Psychological		Physical		Sexual		Any form of IPV		Two or more forms of IPV	
	Within last year	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**
Estimated years of education	−.01 **	−.02 ***	−.01 *	−.03 ***	−.01 ***	−.01 ***	−.02 ***	−.04 ***	−.01 *	−.02 ***
	(.00)	(.01)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)	(.00)	(.01)
Constant	.30 ***	.48 ***	.27 ***	.66 ***	.12 ***	.20 ***	.39 ***	.75 ***	.24 ***	.42 ***
	(.06)	(.07)	(.06)	(.08)	(.03)	(.04)	(.07)	(.08)	(.05)	(.07)
N	10,138	10,138	10,136	10,136	10,138	10,138	10,138	10,138	10,138	10,138

Note: All models adjust for ethnolinguistic background, childhood location, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster

*** p<0.001,

** p<0.01,

* p<0.05,

† p<0.1

Appendix B.

Second Stage Results Predicting Intimate Partner Violence Adjusting for Time Trends (Birth Cohort and Survey Year)

	Psychological		Physical		Sexual		Any form of IPV		Two or more forms of IPV	
	Within last year	Ever	Within last year	Ever	Within last year	Ever	Within last year	Ever	Within last year	Ever
Estimated years of education	−.02 **	−.02 ***	−.01 *	−.03 ***	−.01 ***	−.01 ***	−.02 ***	−.04 ***	−.01 *	−.02 **
	(.00)	(.01)	(.00)	(.01)	(.00)	(.00)	(.01)	(.01)	(.00)	(.01)
Constant	−9.44 *	3.97	2.21	−2.51	4.51 †	2.96	−6.94	.96	.94	1.76
	(4.66)	(5.63)	(4.52)	(6.09)	(2.46)	(3.32)	(5.15)	(6.34)	(4.03)	(5.28)
N	14,836	14,836	14,837	14,837	14,840	14,840	14,840	14,840	14,840	14,840

Note: All models adjust for ethnolinguistic background, childhood location, history of family violence, state of residence, birth cohort, and survey year.

Robust standard errors in parentheses, clustered by survey cluster

*** $p<0.001$,

** $p<0.01$,

* $p<0.05$,

† $p<0.1$

Appendix C.

Second Stage Results Predicting Intimate Partner Violence Among Respondents whose Partners Should Not Have Been Exposed (Born 1981 or Earlier)

	Psychological		Physical		Sexual		Any form of IPV		Two or more forms of IPV	
	Within last year	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**
Estimated years of education	−.02 **	−.02 ***	−.01 *	−.04 ***	−.01 ***	−.01 ***	−.02 ***	−.04 ***	−.01 *	−.02 ***
	(.01)	(.01)	(.01)	(.01)	(.00)	(.00)	(.01)	(.01)	(.00)	(.01)
Constant	.34 ***	.49 ***	.27 ***	.65 ***	.14 ***	.19 ***	.43 ***	.78 ***	.24 ***	.38 ***
	(.06)	(.07)	(.06)	(.08)	(.03)	(.04)	(.07)	(.09)	(.05)	(.07)
N	10,660	10,660	10,659	10,659	10,662	10,662	10,662	10,662	10,662	10,662

Note: All models adjust for ethnolinguistic background, childhood location, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster

*** p<0.001,

** p<0.01,

* p<0.05,

† p<0.1

Appendix D.1.

First Stage Results Predicting Women's Years of Education, Including Women Who Have Never Been in a Relationship

	All eligible respondents in IPV module	
Exposure at age 11	.28	***
	(.05)	
Constant	10.14	
	(.23)	
Observations	17,933	
R-squared	.28	

Note: All models adjust for ethnolinguistic background, childhood location, number of siblings, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster

*** $p<0.001$,

** $p<0.01$,

* $p<0.05$,

† $p<0.1$

Appendix D.2.

Second Stage Results Predicting Intimate Partner Violence, Including Respondents Who Have Never Been in a Relationship

	Psychological		Physical		Sexual		Any form of IPV		Two or more forms of IPV	
	Within last year	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**	**Within last year**	**Ever**
Estimated years of education	−.02 ***	−.03 ***	−.01 *	−.05 ***	−.01 ***	−.01 ***	−.02 ***	−.05 ***	−.01 *	−.03 ***
	(.00)	(.00)	(.00)	(.01)	(.00)	(.00)	(.01)	(.00)	(.00)	(.00)
Constant	.34 ***	.54 ***	.25 ***	.70 ***	.12 ***	.20 ***	.39 ***	.82 ***	.23 ***	.43 ***
	(.04)	(.05)	(.04)	(.06)	(.02)	(.03)	(.06)	(.06)	(.04)	(.05)
N	17,928	17,928	17,929	17,929	17,932	17,932	17,932	17,932	17,932	17,932

Note: For all outcomes, respondents who have never been in a relationship are coded as (0) “none.” All models adjust for ethnolinguistic background, childhood location, history of family violence, and state of residence.

Robust standard errors in parentheses, clustered by survey cluster

*** p<0.001,

** p<0.01,

* p<0.05,

† p<0.1