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Women’s Education and Intimate Partner Violence: Evidence from a Natural Experiment

Abstract Education is theorized to have an important protective effect against intimate partner violence (IPV) in women’s lives, yet evidence regarding this possibility has been mixed. One reason for conflicting evidence may be that few studies have accounted for the endogeneity of women’s education. This study exploits a change in compulsory schooling laws in Peru to conduct an instrumented regression discontinuity analysis that assesses the effect of increasing girls’ years of schooling on their subsequent risk of psychological, physical, and sexual IPV as adults. The results indicate that increasing girls’ schooling reduced both recent and longer-term probabilities of all three forms of intimate partner violence. An investigation into the underlying mechanisms suggests that reductions in IPV may be attributable to changes in women’s personal and relative share of household resources, timing of family formation, attitudes, spouses’ resources, and participation in household decision-making.
Introduction

Over the last several decades, intimate partner violence has become of increasing interest to family scholars (Fox et al. 2002; Friedemann-Sánchez and Lovatón 2012; Straus and Smith 1990; Yount and Carrera 2006), as it kills more people around the world annually than all wars and civilian conflicts combined (Fearon and Hoeffler 2014), and additionally undermines the human capital, health and wellbeing, and physical and economic capacities of those hurt by it (Beydoun et al. 2012; Campbell 2002; Crowne et al. 2011; Dunkle et al. 2004). Within the scholarship on intimate partner violence (IPV), evidence on the effects of women’s education has been mixed. Some scholars find no association between women’s educational background and IPV (Hindin and Adair 2002) while others argue that education plays an important role in reducing intimate partner violence in women’s lives (Ackerson et al. 2008; Jewkes 2002).

There are several theoretical explanations for why improving women’s and girl’s education may subsequently reduce the risk of IPV. For instance, increasing a woman’s years of schooling may afford her greater earnings and employment opportunities that in turn allow her to negotiate for less violence within her relationship (Anderson 2007; Kalmuss and Straus 1982). Schooling may also affect women’s self-esteem and gender ideology, thereby decreasing their tolerance of IPV (Boyle et al. 2009). Moreover, extending women’s education, especially through adolescence, may lead women to delay the process of relationship and family formation (Black, Devereux and Salvanes 2008; Skirbekk, Kohler and Prskawetz 2004), and in this way allow women more time to establish their own resource bases and to decide whether and with whom they will eventually partner.
Although there are numerous theoretical reasons to believe that women’s education protects against intimate partner violence, previous attempts to test these possibilities have been undermined by the fact that education is highly correlated with unobservable characteristics such as self-esteem, gender ideology, and socioeconomic status. Such personal attributes may not only affect family conflict, but also whether or not a woman is willing or able to leave an abusive partner (Bangdiwala et al. 2004b; Hoffman, Demo and Edwards 1994).

In recent years, scholars have made important advances toward evading such problems when estimating the effects of education on other familial outcomes such as fertility and desired fertility (Behrman 2015a; Breierova and Duflo 2004), sexual behavior (Alsan and Cutler 2013; Behrman 2015b), and child health (Güneş 2015). These studies have been able to isolate the effects of education by exploiting exogenous sources of variation in women’s educational access, such as educational policy reforms (Behrman 2015a; Güneş 2015), spatial and temporal variation in school proximity (Alsan and Cutler 2013; Breierova and Duflo 2004), and randomized control trials that incentivize school attendance for some students but not others (Baird et al. 2010). To date, only one study has attempted to isolate the causal effect of women’s education on intimate partner violence (Peterman, Behrman and Palermo 2015), finding that in Malawi and Uganda, completing primary school reduces women’s probability of physical and sexual IPV.

This study expands existing knowledge on the relationship between education and intimate partner violence by exploiting a change in Peruvian compulsory schooling laws in the 1990s to estimate the causal effect of women’s years of formal education on IPV. In so doing, this study makes three important contributions to existing literature. First, I
test whether increasing women’s years of schooling (rather than primary school completion) decreases the risk of intimate partner violence. Second, I investigate the effects of education on three different forms of IPV: psychological, physical, and sexual. And third, I consider the causal effects of education in Peru, a country that has one of highest rates of intimate partner violence in the Western hemisphere (Bott et al. 2012; WHO 2013) and in the world (Garcia-Moreno et al. 2006; WHO 2013). Thirty-two percent of women in Peru report ever experiencing psychological IPV in their lifetimes; thirty-nine percent report ever experiencing physical IPV; and 9 percent report ever experiencing sexual forms of IPV (Bott et al. 2012; WHO 2013).

**Women’s Education and Intimate Partner Violence**

Extant literature on IPV points to several mechanisms by which education may protect against intimate partner violence. Importantly, these mechanisms are interrelated and may therefore co-occur. First, dependency theory posits that a woman’s physical or economic dependence on her spouse reduces her ability to leave an abusive relationship, thus increasing her vulnerability to IPV (Kalmuss and Straus 1982). If education improves women’s cognitive skills, such as literacy, and/or improves women’s employment opportunities and occupational status, then education may reduce IPV by reducing women’s economic dependency on spouses.

Second, the timing of secondary schooling may compete with early fertility and marriage, thereby leading women to delay family formation (Duflo et al. 2007; Flórez and Núñez 2003). Because women’s economic and physical dependency on spouses is often exacerbated by the presence of children, the negative effect of education on fertility
may ameliorate IPV by reducing women’s dependency on spouses. Likewise, if delayed entrance into marriage allows women more time to accumulate their own material resources, then increasing education may indirectly provide women with more negotiating power within their relationships.

Thirdly, schools are often important sites of public health campaigns, information dissemination, and socialization (Merakou et al. 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 of intimate partner violence in their own lives (Boyle et al. 2009; Gage and Hutchinson 2006).

Fourth, resource theory and family stress theory argue that the wealth or poverty of a woman’s spouse 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 and Messner 2000; Fox et al. 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 intimate partner violence more frequently occurs in poorer households has indeed been found in a number of countries, including the United States, Thailand, India, and Colombia (Allen and Straus 1979; Friedemann-Sánchez and Lovatón 2012; Hoffman, Demo and Edwards 1994; Weitzman 2014). If a woman’s education affects the characteristics of the potential spouses 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.
Fifth, bargaining theory purports that, to the extent that one’s relative share of household resources provides them with power (Babcock et al. 1993), the size of a woman’s resource base relative to her spouses’ should affect intimate partner violence.¹ Bargaining theory differs from dependency theory in that it argues that increases in women’s resources beyond a certain threshold continue to provide them with implicit negotiating power. For example, a man whose wife earns a substantially greater income than him may feel threatened that his wife would be in the more desirable position on the re-marriage market and this belief may compel him to behave in a way that pleases her (McElroy and Horney 1981). Additionally, when a woman possesses a greater share of a couple’s joint resources, the implicit or explicit threat the she will withhold some or all of her resources within the relationship may induce her partner to behave in a certain way, potentially reducing his use of violence (Lundberg and Pollak 1993).

Finally, the effect of education on women’s relative share of household resources, and the effect of education on women’s non-cognitive skills may impact women’s participation in household decision-making. In Peru and elsewhere in Latin America, greater participation in decision-making is often associated with a lower risk of IPV (Flake and Forste 2006; Friedemann-Sánchez and Lovatón 2012; Gage 2005).² This may mean that an effect of education on women’s decision-making bolsters their perceived power within relationships, or alternatively, that education impacts unobservable power dynamics that simultaneously improve women’s participation in decision-making and reduce their risk of violence.

¹ Others refer to this idea as exchange theory (Bittman et al. 2003) or comparative resource theory (Fox et al. 2002).
² Friedmann-Sánchez and Lovatón (2012) and Gage (2005), however, find that IPV is also lower in egalitarian households than in households where women dominate decision-making.
Measuring the Effect of Education on Intimate Partner Violence

Despite the many ways in which education may theoretically affect intimate partner violence, evidence on the effects of education on IPV has been mixed. On the one hand, several studies have found a negative association between women’s formal education and IPV in countries as diverse as the United States, India, Peru, and South Africa (Ackerson et al. 2008; Flake 2005; Jewkes 2002; Weitzman 2014). On the other hand, other studies have found no association between women’s individual educational background and IPV (Hindin and Adair 2002; Panda and Agarwal 2005), or that the association between women’s education and IPV disappears once other economic characteristics are controlled for (Bangdiwala et al. 2004a; Bhattacharyya, Bedi and Chhachhi 2011) (see Vyas et al. (2009) for a thorough review of the debate).

One reason for discrepant findings may be that a woman’s educational background not only reflects her years of schooling but also her unobservable features, such as her socioeconomic status, gender ideology, or self-esteem. These features may contribute both to a woman’s educational attainment and her risk of intimate partner violence, thereby leading scholars to inaccurately characterize the role of education. For example, women who grow up in a gender inequitable home may simultaneously be discouraged from attending school and tacitly encouraged to accept IPV. OLS estimates would understate the effects of education for these women because education would capture both years of schooling and ideologies that increase the tolerance of IPV independently of schooling. Alternatively, OLS estimates may be overstated when an unobserved characteristic like high self-esteem leads women to pursue greater amounts of
education while also leading them to be intolerant of IPV independently of their education.

In an effort to evade such problems, scholars have recently developed methodological techniques that rely on the randomization of education. These include randomized control trials of school incentive programs (Baird et al. 2010) and quasi-experimental studies that exploit random variation in formal education via the elimination of school fees and uniforms (Behrman 2015b; Kadzamira and 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 education) should be no different from women who do not receive treatment except with regard to their number of years of education. This high degree of similarity across treatment and control groups should allow for an unbiased estimation of the effects of education.

Compulsory Schooling in Peru

I use the amendment of compulsory schooling laws in Peru to assess the effect of years of education on intimate partner violence. In Peru, primary school has been free and available to the public since 1828 (World Bank 2007), and has been compulsory since 1905 (Freeburger and 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 were still enrolled in school in 1993 (UNESCO 2001).

This change in compulsory schooling law led to a dramatic increase in the Gross Enrollment Ratio in secondary schools, 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 the most from this policy shift, as these children have 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 and socioeconomic gaps in basic education and skills (UNESCO 2002).

While the extension of compulsory schooling from six to eleven years (CS11) successfully increased enrollment in secondary school, many argue that overall school quality remains poor in Peru (World Bank 2007). In fact, only 5 percent of Peruvian children test at the same level as the OECD average (World Bank 2007). The quality of the Peruvian education system may have important implications for the magnitude of the effects of education on women’s risk of intimate partner violence, especially if schooling reduces IPV through effects on women’s and girls’ cognitive skills.

Notably, over the last two decades, numerous other Latin American countries have also adopted or amended their compulsory schooling policies. These changes reflect a regional agenda of educational reform, known as “Education for All,” that began with the Jomtien Conference in 1990 (UNESCO 2002). For example, Mexico extended its

3 This is calculated by dividing the number of school-age children enrolled by the total number of school-age children in the population.
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). Although these countries have recently enacted or amended compulsory schooling policies, the recency of their implementation and/or data limitations prevent me from assessing the effects of formal education on IPV in these various other contexts. Nevertheless, changes in compulsory schooling throughout Latin America underscore the importance of understanding the effects of increasing women’s years of education on intimate partner violence and women’s wellbeing more broadly.

Data and Methods

Data

Data for this analysis are derived from the Peruvian Demographic and Health Surveys (DHS) collected continuously from 2004 to 2012. The DHS are standardized, nationally representative household surveys that interview all women ages 15 to 49 in selected households. For safety reasons, however, only one woman per household is randomly selected to participate in the intimate partner violence module. More than 99 percent of respondents asked to participate in the IPV module comply. To ensure that all respondents completed their education before survey, I restrict the sample to women ages 23 and older at the time of survey.4

4 The maximum number of years of schooling in the DHS is 17. If a respondent enters school at age 6, and completes 17 years of school, then her education should be complete at age 22.
To isolate the effects of education, this study compares respondents who were just above the exposure age for CS11 (N= 9,989) to women who were at or just below the exposure age for CS11 in 1993 (N= 8,668). In other words, I compare women who were ages 9 to 11 years in 1993, and who therefore should have completed six years of schooling by the time of CS11 implementation, to women who were ages 12 to 14 in 1993, and who therefore should not have yet completed six years of schooling at the time of implementation. All respondents in the final sample are between 23 and 33 years of age at the time of survey.

**Estimation Strategy: Instrumented Regression Discontinuity Design**

I employ an instrumented regression discontinuity design (RDD) to estimate the effects of women’s education on intimate partner violence. This type of strategy improves upon conventional study designs such as ordinary least squares (OLS) regression (Appendix A) by evading the issue of endogeneity.\(^5\) That is, in OLS models, unobserved characteristics that simultaneously affect years of education and intimate partner violence may bias OLS estimates. RDD analyses avert this dilemma by comparing two groups that differ only in their exposure to an event that led one group, but not the other, to achieve more years of education.

In a clean regression discontinuity design, exposure to CS11 implementation would be a definitive event contingent only on birth cohort (Angrist and Pischke 2009), and would therefore be exogenous to all individual-level characteristics. Because

\(^5\) I present the results of linear probability models to maintain consistency in the interpretation of the effects of education on dichotomous outcomes. Marginal effects calculated from the results of logistic regressions are similar in magnitude and direction to the coefficients resulting from these linear probability models (available upon request).
respondents aged just above and below the age cutoff in 1993 would differ only in their exposure to CS11 implementation, the RDD would implicitly adjust for the endogeneity of education and would simply be modeled by regressing the dependent variable, $Y$, on birth cohort $B$, and treatment status, $D$ (Equation 1).

(Eq. 1) \[ Y_i = \alpha_0 + \alpha_1 B_1 + \alpha_2 D_1 + \ldots + \alpha_k X_k + \nu_i \]

However, regression discontinuities are complicated, or made “fuzzy” (Angrist and Pischke 2009), when individuals do not comply with their treatment assignment. For instance, grade repetition and delayed school entry, both of which are prevalent in Peru (Pal 2004), mean that some women who were ages 12 and older in 1993, and who should not have been exposed to CS11, nonetheless were required to extend their education because they had not yet completed their original six years of schooling at the time of CS11 implementation. Another issue of noncompliance arises when the law is not enforced and women who were exposed to CS11 do not adhere to their mandated years of schooling.

To address problems of noncompliance, instrumented RDD analyses use an instrumental variable to exploit discontinuities in the probability of CS11 exposure, estimating an effect solely for respondents who complied with their treatment assignment. In this study, I instrument CS11 exposure with birth cohort and estimate an effect of years of education for respondents who complied with the Peruvian school enrollment age schedule. These analyses are modeled with a two-stage least squares (2SLS) estimator. In the first stage, I regress $D$, the treatment (interval-level years of schooling), on $Z$, the identifying instrument (dichotomous exposure to CS11 at age 11 or younger) (Equation 2).
\[
D_i = \alpha_0 + \alpha_1 Z_i + \cdots \alpha_k X_k + \nu_i
\]

In the second stage, I regress \( Y \), the outcome (IPV), on the predicted value of \( D \) from the first stage (Equation 3).

\[
Y_i = \beta_0 + \beta_1 D_i + \cdots \beta_k X_k + \epsilon_i
\]

To improve the precision of my estimates, I adjust all models for respondents’ ethnolinguistic group (Spanish, Quechua, Aymara, or other indigenous group); family history of IPV; number of siblings (0 to 20);\(^6\) childhood location (state capital, city, town, or rural); and state of residence. However, I do not adjust for current marital status or for any other post-treatment variable (except for state of residence), because these variables may be endogenous to the outcome (Gelman and Hill 2007). For example, a respondents’ education may affect her marital status by expanding her employment opportunities and improving her income. These effects of education on employment and earnings may provide respondents with means of leaving an abusive relationship, thereby simultaneously affecting respondents’ marital status and probability of experiencing IPV before the time of survey.

RDD analyses rest on several assumptions. First, the instrument (exposure to CS11) must affect the outcome only via the treatment, and must therefore be uncorrelated with the error term in both the first and second stage. Second, the instrument must affect the treatment assignment, in this case, by increasing respondents’ years of schooling. Third, the instrument must be randomized conditional on a characteristic that is independent of the outcome and the treatment (Angrist and Imbens 1995). In this study, CS11 exposure is conceptualized as conditional on birth cohort. The plausibility of birth

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\(^6\) Respondents’ number of siblings should not theoretically predict IPV, and its inclusion in the second stage reduces the efficiency of the model. Number of siblings is therefore only adjusted for in the first stage.
cohort being independent of IPV depends on how many birth cohorts are included in the model. Put differently, one must include enough cohorts to generate a powerful sample size but the band of cohorts included must still be narrow enough to ensure that the treatment and control groups differ only in their exposure to CS11. I therefore restrict my analysis to women who were born between 1979 and 1984, all of whom were within three years of the age cutoff for CS11 exposure in 1993. As a sensitivity test, I run more conservative models where the sample is restricted to women born between 1980 and 1983 (two years above and below the cutoff in 1993). These alternative models generate results comparable in direction and significance to the primary analyses presented below (Appendix B).

**Intimate Partner Violence Outcomes**

The DHS intimate partner violence module includes questions about psychological, physical, and sexual forms of intimate partner violence. Respondents selected to participate in the module are first asked three questions about psychological violence. These include, “Has your spouse ever…” “Humiliated you?” “Threatened you with harm?” And “Threatened to leave you and/or take your children?” Respondents who answer “yes” are asked a follow-up question about whether this has 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 report experiencing one or more types of violence (1), and respondents who report experiencing none (0). Thirty percent of respondents
included in this study’s sample report ever experiencing at least one form of psychological violence; eighteen percent report experiencing at least one form within the last twelve months.

Seven questions about physical forms of intimate partner violence are asked in the Peruvian DHS. In their original form, these include “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 answer “yes” are subsequently asked whether this form of violence has occurred within the last twelve 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 report experiencing one or more types of violence are coded (1), and respondents who report experiencing none are coded (0). Twenty-eight percent of respondents in this study report ever having experienced physical violence from their partner; just under half of these women report experiencing physical violence within the last year.7

The DHS also asks two questions pertaining to sexual IPV. These included whether a respondent’s spouse has ever “forced [her] to have sex,” and whether her spouse has ever forced her to “perform other sexual acts.” Again respondents who answer “yes” are asked whether this has happened within the last twelve months. Like psychological and physical violence, I create two measures of sexual violence in the last year and sexual violence ever, in which I code respondents (1) when they reported being

7 The lifetime prevalence of physical violence is slightly lower in this study’s sample than in the DHS at large, and lower than reported by Bott. et al (2012), because the maximum age of respondents I consider is 33 years (as explained in the following section).
forced to have sex or to perform other sexual acts and (0) when they report neither.\textsuperscript{8} Seven percent of respondents report experiencing sexual IPV previously in their lifetime; approximately 3 percent report experiencing sexual IPV within the last year.

\textbf{Indicators of Theoretical Mechanisms}

In addition to intimate partner violence, I operationalize six theoretical mechanisms by which women’s education may have affected the probability of experiencing IPV. Although these outcomes reveal important information about how education impacted IPV (if at all), my analyses of these mechanisms is limited by the availability of information collected in the DHS and is therefore not exhaustive.

\textit{Mechanism 1: Women’s resources.} I conceptualize women’s economic resources as literacy (a cognitive skill), employment, and occupational status. To assess literacy, DHS investigators show survey participants a card with one sentence written on it. Investigators then note whether respondents cannot read at all, can read only parts of the sentence, or can read the entire sentence. I code those who can read the entire sentence (1) literate, and those who cannot (0) illiterate. Eight blind participants (0.04\% of the sample) are excluded from this model.

I derive respondents’ employment status (1/0) from the question “Have you done any (remunerated) work in the last twelve months?” To capture both formal and informal employment, DHS interviewers remind respondents that work included jobs that were compensated in-kind or with cash, and that work includes owning a small business, selling things informally, or working on the family farm or in the family business.

\textsuperscript{8} In results not shown, we run all analyses observing the two original variables separately, which lead to substantively similar conclusions in terms of direction and magnitude.
Employed respondents in the DHS are 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 their inclusion would attenuate the results (n=6,171, 32%).

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. All respondents included in this study are age 23 or older and thus these measures do not need to be censored.

Mechanism 3: Attitudes. Starting in 2005, the Peruvian DHS included five questions regarding women’s attitudes towards intimate partner violence (α = 0.74). Specifically, these questions are 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 the food.” Based on responses to these questions, I code respondents as believing IPV is OK (1) if they respond “yes” or “don’t know” to any question and (0) if they respond “no” to all questions. Only five percent of the sample reports “wife beating” is ever justified (Table 1). No attitudinal questions about sexual IPV are asked in the Peruvian DHS.

Mechanism 4: Partner’s resources. I measure spouses’ 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 5: Respondent’s relative share of resources. Relative attributes of respondents compared to spouses include their difference in years of schooling and their difference in occupational prestige. Both measures range from (-1) to (1), and are calculated by subtracting the respondent’s years of schooling (or occupational prestige) from her spouse’s, and then dividing this number by the sum of their joint years of schooling (or joint occupational prestige). Multiplying these values by 100 provides the percent difference in spouse’s years of schooling or occupational prestige compared to the respondent. Negative numbers equate a higher share of schooling or prestige among respondents; positive numbers equate a higher share among spouses. These measures can also only be assessed among currently partnered respondents.

Mechanism 6: Decision-making. In all years, the Peruvian DHS included six questions about respondents’ participation in household decision-making. These questions were only asked to currently partnered and ever-married women, and pertained to decisions about the daily needs of the household, what food will be cooked, large household purchases, how spouse’s earnings will be spent, visiting with family and friends, and the respondent’s own healthcare. I first create a series of dichotomous variables (1/0) for participating in a given decision or not, and then combine these dummies into an index ranging from (0) to (6) decisions that respondents participate in (α = 0.73).

Descriptive statistics are provided separately for women exposed to CS11 implementation and for women not exposed to CS11 implementation in Table 1.

[Table 1]

First-Stage Results
To begin, I assess the relationship between CS11 exposure and respondents’ years of schooling. The results of this first stage suggest that being exposed to CS11 implementation increased a woman’s years of schooling by 0.21 years (p<0.001) (Table 2). The modest size of this increase is most likely attributable to the fact that six years of schooling were already compulsory at the time CS11 was implemented. Nevertheless, the strength of the relationship between CS11 exposure and years of schooling is sufficient for CS11 exposure to serve as a relevant instrument. With one endogenous regressor, the validity of the instrument can be determined either by the t-value on CS11 exposure, which must be larger than 3.2; the corresponding p-value, which must be below 0.0016; or the $F$ statistic for the excluded instrument, which must be greater than 10 (Stock, Wright and Yogo 2002). Not only is the p-value on CS11 exposure below 0.0016, but the $F$ statistic for the excluded instrument is well above 10 ($F = 280; p<0.001$). Figure 1 graphically portrays changes in the average years of women’s schooling by birth cohort in the DHS.

[Figure 1]

[Table 2]

**Second-Stage Results of the Effects of Women’s Education on IPV**

In the second stage, I regress IPV on the predictions generated by the first stage. This reveals that increasing women’s schooling in Peru diminished the probability of all three forms of intimate partner violence—psychological, physical, and sexual (Table 3). Specifically, increasing a woman’s years of schooling by one year respectively reduced her recent and longer-term probabilities of psychological violence by 1 and 3 percentage points; her recent and longer-term probabilities of physical violence by 1 and 3
percentage points; and her recent and longer-term probabilities of sexual violence by 1 percentage point each compared to if she had not attended school for an extra year. Given that for each type of violence, 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.

[Table 3]

**Second-Stage Results From Investigations into Underlying Mechanisms**

Supplementing the analysis of IPV, I employ the same instrumented regression discontinuity design to investigate six mechanisms that may explain the negative relationship between women’s education and IPV (Table 4). First I evaluate the impact of women’s education on their individual resources (Mechanism 1). I find 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. 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 (Mechanism 2). I find that increasing women’s education by one year decreased their probabilities of teen parenthood and teen marriage by 5 percentage points each—a reduction of approximately 12 percent from the mean of the unexposed control group for both outcomes (Table 4).

Following, I explore the effects of women’s education on their attitudes toward intimate partner violence (Mechanism 3). This reveals that increasing women’s education
by one year decreased their probability of believing IPV is ever acceptable by 1 percentage point—a 20 percent decrease from the mean of the unexposed control group.

I then consider the effects of women’s education on the resources possessed by their current partners (Mechanism 4). Here I find that, on average, a one-year increase in respondents’ education is associated with 0.34 more years of schooling and with 0.30 higher occupational prestige scores among partners (Table 4).

Building on the investigations into the effects of education on women’s and their partners’ resource bases individually, I next assess the extent to which improving women’s education impacted their relative share of resources between them and their partners (Mechanism 5). The results of this analysis indicate that increasing women’s education helped to reduce the education gap between partners but had no effect on the gap in their occupational prestige (Table 4).

Finally, given the observed effects of women’s education on their occupational status and relative share of education between partners, I test whether increasing women’s education also affected their participation in household decision-making (Mechanism 6). I find that increasing a woman’s education by one year increased her involvement in decision-making by 0.05 points on a 7-point scale compared to if she had not received any additional education. Women’s greater decision-making may have reduced IPV by changing their perceived value in the home, or alternatively, may signal a change in underlying power dynamics that simultaneously affect IPV and women’s household bargaining power.

Results of Sensitivity Analyses
One competing explanation for my findings is that the discontinuity captures a downward trend in IPV that would have occurred even in the absence of educational reform. To test this, I run an alternative specification of all IPV models in which I control for time (birth cohort) in the first stage. The results of these models remain substantively unchanged from the main results (Appendix C), thus suggesting that the main findings are not attributable to a secular trend. Moreover, post-estimation Hansen $J$ tests indicate that the identifying instruments are uncorrelated with the error term even after adjusting for birth cohort, with one exception—the Hansen $J$ test is statistically significant ($p<0.05$) in the model of recent physical IPV. If, overall, my results were attributable to a secular time trend, then in most or all of the supplementary models, 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. The fact that it did not in five out of six models provides further support that the main RDD results cannot be attributed to a secular time trend in the second stage.

Given that the DHS intimate partner violence module only asks questions about IPV to women who have ever been in a relationship, and that education may affect the timing of first relationship, 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. If delayed relationship formation is one pathway by which education reduced IPV, then intuitively, the negative effects of education on IPV should be even greater when women who have never been in a relationship are included. 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

This study addressed an ongoing debate over whether increasing women’s education reduces their risk of intimate partner violence. Previous research pertaining to this question has been mixed, with some studies suggesting that education has a protective effect (Anderson 2007; Kalmuss and Straus 1982) and other studies suggesting that education has a negligible effect (Hindin and Adair 2002). These contradictions can be attributed, in part, to the fact that few studies of IPV have accounted for the endogeneity of education. To overcome this methodological problem I conducted a regression discontinuity analysis that compared the rates of psychological, physical, and sexual intimate partner violence across two comparable groups of women that differed only with regard to their exposure to a change in compulsory schooling law. Importantly, exposure to this policy change was conditional only on birth cohort and on no other individual characteristic. The results of this RDD analysis revealed that increasing girls’ education by one year decreased their risk of recent and longer-term probabilities of all three forms of violence as adults. Moreover, the point estimates of RDD models were two to three times the magnitude of the point estimates of naïve OLS models (Appendix
A), thus illustrating how the problem of endogeneity, if unaddressed, can lead scholars to mischaracterize the effects of education.

To enrich our understanding of how education alleviates IPV, I explored six plausible mechanisms. The first of these analyses, considering the effects of women’s education on women’s material and non-material resources, revealed that education had a positive effect on literacy and on employed women’s occupational prestige. Thus, even amidst concerns about poor school quality in Peru, I found suggestive evidence that education may reduce IPV by improving women’s cognitive skills and by reducing their economic dependency on spouses. The second mechanism I considered was the timing of family formation. The results of these analyses indicated that increasing women’s years of education reduced their rates of teen pregnancy and teen marriage. Preventing teen pregnancy may diminish IPV by decreasing women’s physical and economic dependency on spouses, while preventing teen marriage may diminish IPV by providing women more time to decide whom they partner with. A third mechanism I tested was women’s attitudes toward IPV, which may influence the tolerance of IPV in their everyday lives. Indeed, I found that increasing women’s education decreased their attitudinal acceptance of IPV.

I also found that increased education was associated with a fourth mechanism—greater resources (education and occupational prestige) among partners. According to family stress theory (Fox et al. 2002), improvements in the human capital of spouses may reduce IPV by boosting overall household income and lowering financial stress and conflict between spouses. And, according to resource theory (Goode 1971), higher
income among male partners should reduce IPV by providing potentially abusive partners with alternative means of persuasion within their relationships.

A fifth mechanism I examined was the status difference between respondents and their partners, as measured by the difference in their education and occupational prestige. Such differences may contribute to IPV if they influence power dynamics within a household. This analysis revealed that increasing women’s education decreased the educational difference between spouses but had no effect on their occupational differences. Finally, the last mechanism I considered was women’s participation in household decision-making, which may either signal or influence their perceived power in the home. As hypothesized, I found that increased schooling among women had a positive effect on women’s involvement in household decision-making.

Taken together, the findings of this study indicate that the expansion of schooling in Peru has had important effects on women’s wellbeing. In particular, increasing girls’ access to education reduced their exposure to intimate partner violence as adults, and more broadly, affected their economic and family life in ways that afforded them more autonomy. These findings beget future research investigating whether the effects of women’s education are larger in countries where overall school quality is believed to be stronger than in Peru, and whether more recent changes in compulsory schooling laws in other Latin American countries will have similar effects on intimate partner violence in the future.
Figure 1: 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: Descriptive Statistics

<table>
<thead>
<tr>
<th></th>
<th>Not exposed to 11-year compulsory schooling at age 11</th>
<th>Exposed to 11-year compulsory schooling at age 11</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
</tr>
<tr>
<td><strong>Education (treatment)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Years of education (0-17)</td>
<td>9.84</td>
<td>4.26</td>
</tr>
<tr>
<td><strong>Intimate partner violence</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Psychological violence within last year</td>
<td>0.17</td>
<td>0.19</td>
</tr>
<tr>
<td>Psychological violence ever</td>
<td>0.30</td>
<td>0.30</td>
</tr>
<tr>
<td>Physical violence within last year</td>
<td>0.13</td>
<td>0.12</td>
</tr>
<tr>
<td>Physical violence ever</td>
<td>0.30</td>
<td>0.26</td>
</tr>
<tr>
<td>Sexual violence within last year</td>
<td>0.03</td>
<td>0.02</td>
</tr>
<tr>
<td>Sexual violence ever</td>
<td>0.06</td>
<td>0.05</td>
</tr>
<tr>
<td><strong>Pathway Indicators</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Literate</td>
<td>0.91</td>
<td>0.92</td>
</tr>
<tr>
<td>Employed</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Occupational prestige (1-8)(^a)</td>
<td>4.55</td>
<td>2.63</td>
</tr>
<tr>
<td>Married as teen</td>
<td>0.42</td>
<td>0.40</td>
</tr>
<tr>
<td>First birth as teen</td>
<td>0.37</td>
<td>0.35</td>
</tr>
<tr>
<td>Attitudes: IPV is OK(^b)</td>
<td>0.05</td>
<td>0.05</td>
</tr>
<tr>
<td>Spouse’s years of schooling (0-17)</td>
<td>9.59</td>
<td>3.48</td>
</tr>
<tr>
<td>Spouse’s occupational prestige (1-8)(^c)</td>
<td>3.98</td>
<td>2.43</td>
</tr>
<tr>
<td>Relative difference in years of schooling (-1 – 1)</td>
<td>0.04</td>
<td>0.27</td>
</tr>
<tr>
<td>Relative difference in occupational prestige (-1 – 1)(^c)</td>
<td>-0.01</td>
<td>0.29</td>
</tr>
<tr>
<td>Involvement in decision-making (0-6)(^d)</td>
<td>5.07</td>
<td>1.27</td>
</tr>
<tr>
<td><strong>Controls</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ethnolinguistic group: Spanish</td>
<td>0.90</td>
<td>0.90</td>
</tr>
<tr>
<td>Ethnolinguistic group: Quechua</td>
<td>0.08</td>
<td>0.08</td>
</tr>
<tr>
<td>Ethnolinguistic group: Aymara</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td>Ethnolinguistic group: other indigenous</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td>Grew up in: state capital</td>
<td>0.35</td>
<td>0.36</td>
</tr>
<tr>
<td>Grew up in: city</td>
<td>0.15</td>
<td>0.15</td>
</tr>
<tr>
<td>Grew up in: town</td>
<td>0.19</td>
<td>0.18</td>
</tr>
<tr>
<td>Grew up in: rural area</td>
<td>0.31</td>
<td>0.31</td>
</tr>
<tr>
<td>Number of siblings</td>
<td>5.50</td>
<td>(2.89)</td>
</tr>
<tr>
<td>History of family violence</td>
<td>0.48</td>
<td>0.48</td>
</tr>
</tbody>
</table>

\(^a\) *Occupational prestige* is calculated among employed respondents only.

\(^b\) Attitudinal questions were first asked in 2005. Only observations from 2005 onward are included in this measure.

\(^c\) *Spouse’s occupational prestige* and *relative difference in occupational prestige* are calculated among ever-married and currently partnered respondents only. 3,086 respondents are excluded because the question was not asked in 2012. 1,490 respondents are excluded because the question was not asked between August and December 2010.

\(^d\) *Involvement in decision-making* is calculated among ever-married and currently partnered respondents only.
<table>
<thead>
<tr>
<th>Variables</th>
<th>Coefficient</th>
<th>Std. Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Exposure at age 11</td>
<td>0.21***</td>
<td>(0.06)</td>
</tr>
<tr>
<td>Constant</td>
<td>11.32***</td>
<td>(0.20)</td>
</tr>
</tbody>
</table>

Observations: 14,836  
R-squared: 0.35

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<0.10
Table 3: IV Regression Second-Stage Results Predicting Women’s Probability of Reporting Psychological, Physical, and Sexual Intimate Partner Violence

<table>
<thead>
<tr>
<th></th>
<th>Psychological</th>
<th>Physical</th>
<th>Sexual</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
</tr>
<tr>
<td>Years of education</td>
<td>-0.01**</td>
<td>-0.02***</td>
<td>-0.01*</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.01)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.32***</td>
<td>0.49***</td>
<td>0.23***</td>
</tr>
<tr>
<td></td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.05)</td>
</tr>
<tr>
<td>N</td>
<td>14,836</td>
<td>14,836</td>
<td>14,837</td>
</tr>
</tbody>
</table>

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<0.10
## Table 4: IV Regression Second-Stage Results Predicting Underlying Mechanisms

<table>
<thead>
<tr>
<th>Mechanism 1: Respondents' resources</th>
<th>Mechanism 2: Family formation</th>
<th>Mechanism 3: Attitudes</th>
<th>Mechanism 4: Partner's resources</th>
<th>Mechanism 5: Respondents' relative share of resources</th>
<th>Mechanism 6: Decision-making</th>
</tr>
</thead>
<tbody>
<tr>
<td>Literate, Employed, Occupational prestige</td>
<td>Married as teen, 1st birth as teen</td>
<td>IPV is OK</td>
<td>Years of schooling, Occupational prestige</td>
<td>Difference in years of schooling, Difference in occupational prestige</td>
<td>Involvement in decision-making</td>
</tr>
<tr>
<td>Years of education</td>
<td>0.01***</td>
<td>-0.05***</td>
<td>-0.05***</td>
<td>-0.01***</td>
<td>0.34***</td>
</tr>
<tr>
<td>(0.003)</td>
<td>(0.01)</td>
<td>(0.04)</td>
<td>(0.01)</td>
<td>(0.00)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.83***</td>
<td>0.71***</td>
<td>0.48</td>
<td>1.03***</td>
<td>0.99***</td>
</tr>
<tr>
<td>(0.04)</td>
<td>(0.06)</td>
<td>(0.43)</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.03)</td>
</tr>
<tr>
<td>N</td>
<td>14,825</td>
<td>14,835</td>
<td>7,585</td>
<td>14,836</td>
<td>14,836</td>
</tr>
</tbody>
</table>

*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<0.10

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

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

*c Spouse's occupational prestige* is calculated among ever-married and currently partnered respondents only. 14 respondents are excluded because their spouse’s occupation is listed as “other” and cannot be ranked. 14 respondents are excluded because their spouse is not currently working. 3,086 respondents are excluded because the question was not asked in 2012. 1,490 respondents are excluded because the question was not asked between August and December 2010.

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

e *Involvement in decision-making and spouse drinks alcohol* are calculated among ever-married and currently partnered respondents only.
### Appendix A: Regression Results Predicting Intimate Partner Violence, Estimated with Linear Probability Models

<table>
<thead>
<tr>
<th></th>
<th>Psychological</th>
<th></th>
<th>Physical</th>
<th></th>
<th>Sexual</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
<td>E</td>
</tr>
<tr>
<td>Years of education</td>
<td>-0.003**</td>
<td>-0.01***</td>
<td>-0.003**</td>
<td>-0.01***</td>
<td>-0.002***</td>
<td>-0.00***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.000)</td>
<td>(0.1)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.19***</td>
<td>0.30***</td>
<td>0.15***</td>
<td>0.34***</td>
<td>0.05***</td>
<td>0.00***</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.03)</td>
<td>(0.02)</td>
<td>(0.03)</td>
<td>(0.01)</td>
<td>(0)</td>
</tr>
<tr>
<td>(N)</td>
<td>14,836</td>
<td>14,836</td>
<td>14,837</td>
<td>14,837</td>
<td>14,840</td>
<td>14,840</td>
</tr>
</tbody>
</table>

*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<0.10
Appendix B: IV Regression Second-Stage Results Predicting Intimate Partner Violence, Estimated with Two-Stage Instrumental Regression Models Among Respondents Born Between 1980 and 1984

<table>
<thead>
<tr>
<th></th>
<th>Psychological</th>
<th></th>
<th>Physical</th>
<th></th>
<th>Sexual</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
<td>Ever</td>
</tr>
<tr>
<td>Years of education</td>
<td>-0.01*</td>
<td>-0.02***</td>
<td>-0.01*</td>
<td>-0.03***</td>
<td>-0.01***</td>
<td>-0.01***</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.00)</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.30***</td>
<td>0.48***</td>
<td>0.27***</td>
<td>0.66***</td>
<td>0.12***</td>
<td>0.20***</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.07)</td>
<td>(0.06)</td>
<td>(0.08)</td>
<td>(0.03)</td>
<td>(0.04)</td>
</tr>
</tbody>
</table>

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<0.10
### Appendix C: IV Regression Second-Stage Results Predicting Intimate Partner Violence, Estimated with Two-Stage Instrumental Regression Models, Adjusting for Birth Cohort

<table>
<thead>
<tr>
<th></th>
<th>Psychological</th>
<th>Physical</th>
<th>Sexual</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
</tr>
<tr>
<td>Years of education</td>
<td>-0.01** (0.00)</td>
<td>-0.02*** (0.01)</td>
<td>-0.01† (0.00)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.31*** (0.05)</td>
<td>0.49*** (0.06)</td>
<td>0.22*** (0.05)</td>
</tr>
<tr>
<td></td>
<td><strong>14,836</strong></td>
<td><strong>14,836</strong></td>
<td>14,837</td>
</tr>
</tbody>
</table>

*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<0.10
Appendix D.1: First-Stage Results Predicting Women’s Years of Education, Including Women Who Have Never Been in a Relationship

<table>
<thead>
<tr>
<th>All eligible respondents in IPV module</th>
</tr>
</thead>
<tbody>
<tr>
<td>Exposure at age 11</td>
</tr>
<tr>
<td>(0.05)</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>(0.23)</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>R-squared</td>
</tr>
</tbody>
</table>

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<0.10

Appendix D.2: IV Regression Second-Stage Results Predicting Intimate Partner Violence, Estimated with Two-Stage Instrumental Regression Models, Including Respondents Who Have Never Been in a Relationship

<table>
<thead>
<tr>
<th>Psychological</th>
<th>Physical</th>
<th>Sexual</th>
</tr>
</thead>
<tbody>
<tr>
<td>Within last year</td>
<td>Ever</td>
<td>Within last year</td>
</tr>
<tr>
<td>Years of education</td>
<td>-0.02***</td>
<td>-0.03***</td>
</tr>
<tr>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.34***</td>
<td>0.54***</td>
</tr>
<tr>
<td>(0.04)</td>
<td>(0.05)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>N</td>
<td>17,928</td>
<td>17,928</td>
</tr>
</tbody>
</table>

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; 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<0.10
References


2011. "Challenges for Education with Equity in Latin America and the Caribbean: Regional Preparatory Meeting." Buenos Aires: UNECOSOC.


