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Impact of Domestic Violence on Children's Education in Colombia: Methodological Challenges[1]

Ragui Assaad, **Greta Friedemann-Sánchez**, and **Deborah Levison**[*]
Humphrey School of Public Affairs, University of Minnesota, 301 19th Ave. S., Minneapolis, MN 55455, USA

Abstract

This paper explores the methodological challenges of estimating the effects of intimate-partner violence (IPV) against the mother on the educational outcomes of her children. We tackle the problem of potential endogeneity and non-random selection of children into situations where they are exposed to IPV using non-parametric matching methods and parametric instrumental variable methods. Using Colombia's 2005 DHS (N= 21,827) we find that IV and non-IV estimators produce qualitatively similar results, but at varying degrees of precision, for some of the educational outcomes but not for others. This suggests that the exogeneity of IPV to various education outcomes cannot be taken for granted and that appropriate methods need to be used to study its causal effects.

Kew Words

Domestic violence; intimate partner violence; education; children; outcomes; Colombia

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Due to its endemic nature, intimate partner violence (IPV) is increasingly being recognized as a human development problem that international development institutions and developing country governments ought to tackle (World Bank 2014, Agarwal and Panda 2007). Although IPV is prevalent in most parts of the world, we know more about its determinants and consequences in industrialized country settings. A recent study comparing 10 countries, all but one developing countries, estimates that between 15% and 71% of ever-partnered women have been physically and/or sexually abused by their partners at some point in their lives (Garcia-Moreno 2006). IPV has cascading negative effects on the economic wellbeing (Renzetti 2009), physical health (Matthew et al. 1996) and mental health (DeJonghe et al. 2008) of individual victims, as well as on the incidence of unintended pregnancy (Pallitto and O'Campo 2004). Furthermore, IPV has negative consequences not only for individuals subjected to violence, but also for the development of their children. IPV is highly predictive of poor child nutrition (Heaton and Forste 2008); it has negative effects on children's intellectual (Huth-Bocks et al. 2001) emotional, behavioral and social development (Evans et al. 2008), as well as on their academic performance (Peek-Asa et al 2007). In this paper

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Corresponding author: Greta Friedemann-Sánchez, gfs@umn.edu, Phone: 612-625-4747, Fax: 612-625-3513.
[*]Authors' names listed alphabetically.

we explore the methodological challenges of estimating the effects of IPV on multiple measures of children's educational outcomes, including current school attendance, grade advancement in the last year (both unconditional and conditional on staying in school), drop-out in the past year, current grade conditional on still being in school, completed years of education, and grades completed per year of exposure to school. Specifically, we compare different methodological approaches to addressing endogeneity issues. Because many unobservable characteristics affect whether IPV takes place and may also directly affect some of its outcomes, studies that do not address this endogeneity are likely to produce misleading results. As a case study we use Colombia, which in 2005 had a national life-time prevalence of physical violence for ever-partnered women of 40% (Friedemann-Sanchez, Lovaton 2012). In our sample, 23% of 6–14 year-olds live in households where IPV was present in the last 12 months.

EDUCATION EFFECTS OF INTIMATE PARTNER VIOLENCE ON CHILDREN AND METHODOLOGICAL ISSUES

Although the study of effects of IPV on children is more extensive for developed nations and specifically for the United States, an area that is understudied in both developed and developing nations is the effect that IPV has on children's educational outcomes. Studies conducted in the United States have found lower reading levels among adolescents who have been exposed to IPV (Thompson and Whimper 2010), lower academic achievement in math and reading for children in elementary and middle school (Kiesel et al. 2011) and lower scores on standardized tests for children ages 6 to 17 – especially for girls and children younger than 12 years old (Peek-Asa et al. 2007). Among the few studies conducted in low-income countries is one for Sri Lanka, which found that children who were directly (by watching, hearing, intervening) or indirectly exposed to IPV at home (by observing maternal injuries, depression) had poor school attendance and lower academic achievement on average, as measured by exam scores (Jayasinghe et al. 2009). A study conducted in Brazil found that children 5 to 12 years old who lived with mothers exposed to psychological, physical and sexual IPV were more likely to be among those dropping out of school or failing a school year (Durand et al. 2011). A small study, also in Brazil, compared the effects of IPV exposure to non-exposure among children 7 to 11 years old and found that exposed children had lower performance in regular academic tasks (i.e. reading, quality of work, math) but found no differences in grades or test performance (Bracalhone et al. 2004). Because the aim of our paper is to explore the methodological difficulties in estimating the effect of IPV exposure on education outcomes, it is notable that all of these studies use conventional multivariate regression methodologies that treat IPV as an exogenous regressor. With fewexceptions (Emery 2009, 2011; Margolin et al. 2010), these studies do not consider the possible endogeneity of IPV or correct for it.

The causal pathways between child exposure to IPV and education outcomes are not well documented; however, studies find associations among various psychosocial and education outcomes. Children who are exposed to IPV have higher levels of internalizing (depression, anxiety), externalizing (physical aggression) behaviors, trauma, and PTSD symptomatology (Evans 2008; Graham-Bermann et al 2012). A seminal meta-analytic study exploring the effects on children who witness inter-parental violence in the United States shows negative psychological, social and academic outcomes (Kitzmann et al 2003).

Some studies have attempted to examine the pathways through which IPV affects child outcomes. A recent study in the United States examining the effects of parent-to-youth aggression, physical IPV, and community violence on child outcomes found that combinations of different types of violence may have more serious psychosocial and academic effects than exposure to just one type (Margolin et al. 2010). Another US-based study found that children exposed to community and IPV violence had negative school achievement outcomes (Thompson and Massat 2005). In this steady, we focus on the methodological challenges of identifying the effects of IPV on child education outcomes, but are unable to explore the various pathways through which this effect is transmitted.

Estimating the existence and magnitude of effects of IPV on children's outcomes poses methodological challenges because of potential endogeneity problems. In fact, these challenges are common to all studies that attempt to study the causal effects of social behaviors on development outcomes. A recent paper on a related topic – the effect of teen childbearing on the mother's educational outcomes in the US – summarizes the challenge well. It concluded "that after forty years of research on educational consequences of teen child-bearing, *the magnitude of the causal relationship* is still unclear" (Kane et al. 2013, p. 2130, *our emphasis*). In particular, studies of the effects of IPV on various outcomes rarely address these methodological challenges and therefore can say little about the *causal impact* of IPV.

A primary objective of our paper is to estimate and compare the effects of exposure to IPV on several education outcomes when using different approaches to addressing endogeneity problems. In doing so, we realize that there are no panaceas and that each method involves a number of potentially untestable assumptions and thus limitations. If methods that make use of different identifying assumptions produce varying results, we can use this information to make inferences about the importance of endogeneity in this particular context.

We are concerned with two potential problems, referred to jointly as *endogeneity*. First, there may be non-random selection into IPV based on unobserved/confounding variables. That is, personal and/or family characteristics and histories – not all of which are known to researchers – can influence the likelihood of both IPV status for women and negative educational outcomes for children. Possible confounding factors that could affect both IPV status and child outcomes include the mental health of the mother's partner, his involvement in crime, his drug and alcohol use, and the stability of the relationship itself, all of which could affect IPV as well as the child's educational outcomes directly. For instance, an alcoholic father may not only beat his wife, but also make the home environment difficult for study. We refer this set of issues as *selection on unobservables*. Second, there is also the potential for *reverse causality*. For example, some women may be subject to abuse because of their children's poor performance in school. Overlooking the potential endogeneity issues associated with IPV – as is standard in this literature – is problematic. If the types of confounding or reverse causality described above exist but are ignored in an analysis, the regression coefficients estimates would likely be biased upward.

Some studies have used longitudinal data to address the potential endogeneity of IPV (Emery 2009, 2011; Margolin 2010). Emery (2011) used panel data from the Project on Human Development in Chicago Neighborhoods to examine the effect of IPV on child behavior. He controlled for selection by estimating child fixed-effects models on the panel data where each child is observed twice over a three year period. He thus disentangled the relationship between IPV and children's behavior from the effects of unobserved child and family background characteristics. He found that after correcting for selection, IPV was still significantly associated with an increase in child internalizing and externalizing behavioral problems and weakly associated with child truancy. Margolin et al (2010) examined how exposure to different interpersonal domains of violence affected academic performance among other child outcomes, controlling for pre-exposure outcome levels. In so doing, they addressed selection on time-invariant unobservables but did not address potential confounders that are time-varying. Because we do not have access to panel data, these approaches are not available to us.

Conventional regression methods and non-parametric matching methods both assume that selection is based on observable characteristics which, once controlled for, provide unbiased estimates of the treatment effect. We explore the use of both parametric instrumental variable (IV) methods as well as non-parametric matching methods as possible ways to address the potential endogeneity problems associated with exposure to IPV, which in keeping with the literature on causal inference we refer to as "treatment." Instrumental variable methods address possible selection on unobservables and endogeneity but make other identifying assumptions related to the properties of the instrumental variable. While both sets of assumptions can be questioned, if the methods produce similar results we can be more confident of the validity of the results. If the results are different, this potentially casts doubt on the validity of methods that do not account for endogeneity.

Methodology

Context: IPV and Education in Colombia

In a 2005 study in Colombia using the nationally representative Demographic and Health Survey (DHS), 40% of women reported having ever experienced any type of physical violence, whereas 22% reported it experiencing it in the last 12 months (Friedemann-Sánchez, Lovatón 2012). The prevalence of a woman having *ever* experienced severe forms of physical violence (threatened or attacked with a knife or a fire arm, strangled or burned, raped) is 16.6%. Sexual assault (11.7%) constitutes the most common severe form of violence. Being pushed or shaken is the most frequently reported among the less severe forms of violence (34%). The life-time and past-year rates of emotional abuse are even higher than the rates of physical violence at 66.4% and 52.3% respectively. The highest probability of experiencing IPV is associated with the maltreatment of the woman's partner when he was a child (ibid). There is robust evidence for developed countries (Whitfield, Anda, Dube, & Felitti 2003), also reported for a few developing regions like India (Martin et al. 2002), that childhood exposure to violence between parents is a risk factor for becoming a victim and/or perpetrator of violence later in life.

According to Colombia's national statistical office (Departamento Administrativo Nacional de Estadísca — DANE), the literacy rate among individuals 15 years old and older was 91.6% (91.3% for men and 91.8% for women) in the 2005 population census. A comparison of literacy rates since 1964 (when 75% of men and 71.1% of women reported they were literate) shows that they have been steadily improving for both men and women. According to the 2005 census, 78% of 5–6 year olds, 92% of 7–11 year olds, and 77% of 12–17 year-old children were enrolled in school; individuals between 15 and 24 years of age had on average 9 years of formal schooling (DANE 2005). A 2010 national study reports that of all children in elementary and secondary school, 77.6% were enrolled in public schools and 5.5% received tuition subsidies; 75.6% of urban children were enrolled (DANE 2011). There is almost equal distribution by gender of current enrollment rates in primary (51% boys, 48% girls) and secondary (49% boys, 51% girls) education (ibid).

Our analysis uses Colombia's 2005 Encuesta Nacional de Demografía y Salud (ENDS), known internationally as the Demographic and Health Survey (DHS); it is a nationally representative sample show similar levels of educational outcomes to those estimated by DANE (See Tables 1 and 3). We find that 93% of the 6–14 year-olds in our sample were attending school at the time of the survey. Over 91% of those who were in school the previous year advanced to the next grade; among those in school in both the previous year and the survey year, over 95% advanced. Only 2% dropped out in the year prior to the survey.

Data and Sample

The sample includes all children ages 6–14 living in households where the mother is present, is under the age of 50, and has responded to the domestic violence module in the 2005 DHS for Colombia. The DHS is a 3-stage stratified cluster sample covering all but two (sparsely populated) provinces of Colombia; it contains 3,935 clusters with an average sample size of 9.5 households per cluster (Ijeda et. al 2006, pp. 411, 413). The DHS 2005 sample includes 31,140 children between the ages of 6 and 14. Of those, 23,253 were living with a mother who was between the ages of 15 and 49 and who was selected for interview for the domestic violence module. The final sample includes 21,827 children because of losses due to mothers who could not be safely interviewed in private or who had never been married or in a de facto union. As shown in Table 1, there is a noticeable and highly significant bivariate negative association between the presence of IPV in a household and children's educational outcomes. For example, children in households with IPV have a 1.8 percentage point lower probability of attending school, a 1.9 percentage point lower probability of advancing from one grade to the next and are behind by more than one-third of a year, on average, in terms of current grade attainment. It remains to be seen whether these differences remain after controlling for observables and correcting for selection. In addition, our chosen instrument, the maltreatment of the male partner when he was a child, is strongly associated in a bivariate sense with the incidence of IPV in the household (Table 2). The descriptive statistics for our dependent and explanatory variables are shown in Table 3.

Estimation Strategy

Our estimation strategy consists of two sets of methods that make different identifying assumptions regarding the endogeneity of exposure to IPV (the "treatment"). The first set of methods, which includes conventional regression models and non-parametric matching methods, controls for a large number of individual, household-level and community-level covariates but assumes that selection into IPV is based only on observable characteristics and is therefore exogenous to child outcomes conditional on including these controls. Non-parametric matching models have two distinct advantages over regression-based models: they do not assume any *a priori* functional form for the relationship between IPV and the child's educational outcome, and they rely on comparing (or "matching") the treatment observations with a closely matched set of control observations rather than using all the untreated observations in the sample as controls, some of which are simply not comparable to those experiencing IPV.

With cross-sectional data, the main way to address endogeneity is with instrumental variable methods. Accordingly, the second set of methods we use involves estimating both linear and non-linear parametric instrumental variable (IV) models. Such an IV strategy crucially depends on the validity of the instruments selected.[2] Our instrument of choice relates to the mother's partner's experience of violence when he was a child (that is, whether or not he was regularly beaten as a child). Previous studies (Friedemann-Sánchez, Lovatón 2012) have shown that the partner's childhood experience of violence is a powerful predictor of IPV. We argue that, additionally, this variable is a good candidate for a valid instrument. We posit that with the inclusion of appropriate controls for household socioeconomic status and social context, the mother's partner childhood experience of violence is excludable from the child's educational outcome equation.

It is possible that the mother's partner's childhood experience with violence will affect the child's school performance in other ways. For instance, it could increase the likelihood that the child herself is subjected to violence. The co-occurrence of violence directed at an intimate partner and her children is well-documented in the literature (Patel 2011, Casique 2009). Since we cannot differentiate in this paper between violence directed to children or to an intimate partner, we treat both as part and parcel of IPV. It is also possible that the partner's childhood experience with violence could have affected the partner's own development, including unobserved outcomes such as involvement with crime, his mental health and his relationships with women, which may in turn have an impact on the child's educational success, separate from IPV. Although we believe that these other pathways are

[2]For readers new to this methodology: An instrumental variable is a proxy for the endogenous treatment of interest (in this case IPV). A valid instrument is a variable that is correlated with the treatment (IPV) but uncorrelated with any other determinants of the (child's human capital) outcome. That is, it only affects the outcome through its effect on the treatment. This condition is referred to as an exclusion restriction. When causal effects of the treatment are heterogeneous, i.e., when they differ across individuals, two additional assumptions are necessary: (i) that the instrument is exogenous, that is, independent of the treatment and the outcome (the exogeneity assumption), and (ii) that its effect on the treatment is monotonic, that is, while some individuals' treatment status may not be affected by the instrument, all those that are affected are affected in the same way (the monotonicity assumption). Subject to these assumptions, the IV method yields the effect of the treatment for those individuals whose treatment status changes when the instrument changes value (the compliers), what is known as the local average treatment effect (LATE). This could be different from the average treatment effect (ATE), which would also include the effect for individuals whose treatment status is not affected by the instrument (the always treated and the never treated). See Chapter 4 of Angrist and Pischke (2009) for a more detailed discussion of IV estimation.

likely to be relatively unimportant, we acknowledge this to be a potential threat to the exclusion assumption upon which the validity of our instrument depends.

With regard to the exogeneity of the instrument, we argue that because this instrument is determined at a much earlier time, it is independent of both the mother's IPV status and the child's educational outcomes. Finally, we claim that this instrument is very likely to satisfy the monotonicity assumption, in the sense that a man's experience of violence as a child is likely to either not affect his chances of perpetuating violence himself or to increase it, but not to decrease it. While it is possible that a man who experiences violence in his childhood consciously chooses to avoid perpetuating violence as an adult, the literature overwhelmingly shows that the opposite is true (Flake and Forste 2006, Kishor and Johnson 2004, Whitfield et al. 2003, Friedemann-Sánchez, Lovatón 2012).

We acknowledge that the mother's partner's childhood experience of violence is not a perfect instrument because of some remaining concerns about its excludability and monotonicity, but it provides an alternative way to control for selection. If the instrument violates the exclusion restriction as described above, its effect should be to bias upwards the effect of IPV on child educational outcomes. That is, the effect would capture both any effect of IPV itself and also effects operating through pathways relating to the partner's direct impact on the child (e.g., violence against the child, alcoholism, controlling behaviors, etc.). If, however, we find that the use of the instrumental variable reduces the measured effect of IPV on child outcomes, compared to non-IV methods, this would suggest that endogeneity was biasing the non-IV results upwards.

Dependent variables—We use seven measures of child schooling outcomes as dependent variables. No single variable can perfectly capture a child's educational history. In this analysis, we have no information on learning via training outside of formal education, yet this is an important path to the accumulation of skills (Bourdillon et al 2010). Because we expect to learn somewhat different things from alternate educational outcomes, our analysis is repeated for each of the seven dependent variables that we are able to calculate from the data we have. These include whether the child is currently attending school or not, has dropped out in the past year or not, has advanced a grade since the previous year or not – both conditional on not dropping out (i.e., repeating the grade only) and unconditional (either repeating or dropping out) – as well as the child's current grade in school given their age, total number of years of education successfully completed to date, and grades attained per year of exposure to schooling. Because the "years of education" variable is conditional on having entered school, we limit the sample in that part of the analysis to children 10–14 year olds instead of 6–14 to avoid problems related to delayed entry. The "grades per year of exposure to school" variable is calculated by dividing grades attained by age minus age of school entry.[3] Since age of school entry is not known, we assume the age of entry to be six. For this dependent variable we also use a sample of 10–14 year olds, leaving out the younger children because the effect of measuring age of school entry with error is exaggerated at younger ages (since so few grades have been completed). Our sample mean of 0.83 indicates

[3]Thanks to David Lam for suggesting this measure.

that, on average, Colombian children ages 10–14 successfully completed eight-tenths of a year for each year of potential exposure to schooling.

Explanatory Variables—Our main explanatory variable, which we refer to as the treatment, is the potentially endogenous regressor indicating whether or not the child's mother has experienced physical intimate partner violence in the past 12 months. Women were asked about the following experiences: (i) being pushed or shaken, (ii) hit with a hand, (iii) hit with an object, (iv) bitten, (v) kicked or dragged, (vi) threatened with a knife, (vii) attacked with a knife or firearm, (viii) being subject to an attempt at strangulation or burning, and (ix) being raped. The occurrence of any of these at least once in the past 12 months constitutes IPV by our definition. For the purposes of this paper we did not consider the experience of emotional violence, such as controlling behaviors or threats, to be instances of IPV. Note that while a child's mother's partner may be her husband, he may or may not be the child's biological father. We use the term "partner" to avoid confusion.

Additional individual and household-level variables used as controls in both the child outcome equation and in the first stage equation for IPV exposure include child sex, age, age-squared, whether the child is the son or daughter of the household head, the mother's age and age squared when the child was six, the mother's and her partner's years of schooling, marital and cohabitation status of the mother, the household's migrant status, its wealth quintile, and variables indicating the composition of the household in terms of numbers of female and male children and adults of various ages and sexes and the presence of relatives on either the mother's side or that of her partner. While we know whether the mother is currently married or cohabiting with a male partner, we do not know whether non-cohabiting mothers have a non-residential partner or whether the partner is the children's father. Cobb-Clark and Tekin (2013) have found these variables to matter for adolescent outcomes.

Community-level controls include regional dummy variables and municipality averages for a wealth index, years of education of men and women, the child-woman ratio as a proxy for fertility, the percentage of female-headed households, the percentage of the population living abroad, the percentage of women and men in formal employment, the percentage of households with piped water and sewage disposal, and the percentage of households cooking with firewood. All these municipality-specific variables were calculated by averaging over the DHS sample in each municipality. Because the partner's information could be missing, we also include two dummy variables indicating whether the partner's education is missing and whether the information on the partner's childhood experience with violence is missing. These variables could be missing either because there is no partner or because the respondent does not know. Although the indicator of missing information on the partner's childhood experience with violence relates to the instrumental variable, we cannot assume that the fact that the partner might be missing is unrelated to child outcomes. Therefore, we include these missing indicators as regular controls in both the first and second stage equations.[4]

[4]We also experimented with excluding observations with missing partner information. This did not appreciably affect the results.

Estimation methods—In the parametric estimation, we select functional forms that are appropriate to each of the outcome variables. For the four binary outcome variables – in school or not, dropped out or not, and advanced a grade or not, both conditional and unconditional on staying in school – the non-IV parametric model we use is probit and the parametric IV models are IV-probit and IV-regress. For the two count data variables – current grade and completed years of education – the parametric non-IV model is Poisson and the parametric IV models are IV-Poisson and IV-regress. For the continuous outcome – grades per year of exposure to school – the non-IV method is OLS and the IV method is IV-regress.

The nonparametric method we use for all outcome variables is propensity score matching with kernel matching. Propensity score matching methods can theoretically correct for selection into treatment if selection is mainly based on observable characteristics. This is achieved by predicting the probability of selection into treatment, the propensity score, as a function of observables, then matching treatment and control observations on the propensity score. However, the propensity score is usually estimated using a Probit or Logit equation with some degree of arbitrariness as to what covariates to include in the model and what functional form to adopt. Boosted regression is an alternative method for selecting the propensity score equation that can significantly improve predictive accuracy. It is a multivariate non-parametric regression technique that uses an automated, data-adaptive algorithm that can estimate the non-linear relationship between a variable of interest and a large number of covariates (McCaffrey et al. 2004). Boosting produces well-calibrated probability estimates by adding together many simple functions estimated on partitions of the data to obtain a smooth function of a large number of covariates. Boosted models are typically fit iteratively on a portion of the data called the "training data" and then their goodness of fit is tested on the remaining part of the data, referred to as "test data." We present results using a conventional probit approach to estimating the propensity score as well as ones that rely on boosted regression.

We implement boosted regression using the STATA plugin 'boost' (Schonlau 2005). Because such a highly flexible technique runs the risk of over-fitting (that is estimating a model that fits the training data well but that does not generalize to the rest of the data in the sample), there are a number of tuning parameters that must be carefully chosen. The first parameter is the proportion of the data set aside for the training data versus the test data. We use the default in Schonlau's program, which is 80 percent of the sample allocated to the training data. The second parameter is the number of interactions (number of splits in the tree). One split corresponds to a main effect model, two splits to a model with main effects and two-way interactions, and so on. Hastie et al. (2001) suggest that two-way interactions are generally not sufficient, but any number in excess of four does not significantly improve the fit of the model. Accordingly, we use three-way interactions as our base estimate and present sensitivity analyses with two and four-way interactions. The third tuning parameter is the shrinkage parameter. Shrinkage means reducing the impact of each additional tree to avoid over-fitting. The smaller the shrinkage parameter, the less the risk of over-fitting, but the larger the number of iterations must be. We follow the advice of McCaffrey et al. (2004) and use a relatively small shrinkage parameter of 0.0005 to ensure a smooth fit. The fourth parameter is the bagging parameter, which is the fraction of randomly selected observations used for fitting the regression tree at each iteration. We use the program's default value of 0.5. The last parameter to select is the maximum number of iterations. Schonlau (2005) recommends that the product of the maximum number of iterations and the shrinkage parameter be in the range of 10 and 100. We set the maximum number of iterations at the lower end of this range at 20,000 iterations, where the product of the number of iterations and the shrinkage parameter (0.0005) is 10. Since our treatment variable is binary, we select a logistic distribution. Finally, the covariates we use in the model are the same covariates we used in the parametric model and the conventional matching model.

Once the IPV propensity score is estimated for each child using either probit or boosted regression, different matching methods can be used to match treatment and control observations. For our base estimates, we use kernel matching with the standard Epanechnikov kernel function, but undertake sensitivity analysis of our results using other matching methods, such as uniform and normal kernel and five nearest neighbors.[5]

Because the propensity scores are estimates, analytical standard errors are understated. We therefore report bootstrapped standard errors for all of the matching results. We also report bootstrapped standard errors for the IV-Poisson model since that model is estimated using a two-stage technique rather than by means of a full-information maximum likelihood method. Finally, to account for the fact that children in the same households probably share the same mother, all standard errors reported throughout the paper are based on the assumption that observations are clustered at the household level.

Results

As laid out above, we present results on the effect of being exposed to IPV on seven different child education outcomes using both non-parametric methods (boosted matching) and IV and non-IV parametric methods. These results are summarized in Table 4, which shows the marginal effect of IPV exposure on the seven different outcomes for the various methods we consider.[6] The main findings are that the non-IV methods results suggest that IPV has a negative, statistically significant effect on most educational outcomes. However, these significant effects disappear when IV methods are used. The difference is not simply a matter of precision; for some outcomes, the point estimates are quite different, while in others they are similar in magnitude but less precisely estimated. We conclude from these differences that endogeneity is potentially a concern.

To illustrate the differences in the non-IV and IV method results, we begin by discussing the results for the first dependent variable – child's school attendance. The boosted matching and conventional matching results are almost identical; they show that the average treatment

[5]Once the propensity score has been predicted using the "boost" command or a Probit model, we use the user-written STATA ado file PSMATCH2 v4.04 to undertake the matching estimation. See Leuven and Sianesi (2003). Sensitivity analysis for different matching methods is only presented for the boosted regression model. Sensitivity analyses for the conventional matching approach using a Probit first stage is available from the authors upon request.

[6]Table A1 shows the first stage regressions for the IV models and the propensity score equation for the conventional matching model. Appendix B Tables show the full regressions for the parametric models for each of the outcome variables. Appendix B tables are available from the authors [if not online].

effect on the treated (ATT) is a reduction in the probability of currently attending school of 1.2 percentage points, which is significant at conventional levels using bootstrapped standard errors (See Table 5). If we believe these results, they imply that IPV increases the probability of non-attendance from 6.9 percent for a matched control group to 8.2 percent for the treatment group, a relative increase of nearly 19 percent. The Probit results produce larger estimates of the effect of IPV, showing a reduction of 2.7 percentage points for a reference child and are significant at the 5 percent level.[7]

We use two instrumental variable methods: an IV-Probit regression which accounts for the fact that the dependent variable is binary, and an IV-regress model which treats the dependent variable as continuous. The reason we include the IV-regress model is to be able to conduct a number of tests on the validity of the instruments. For the school attendance outcome, the IV models produce much smaller, negative marginal effects that are statistically insignificant.

Table 5 includes the results from a number of conventional test for instrumental variable models. All the tests are conducted for the IV-regress model with clustered standard errors at the household level. The first test is whether the instrument has sufficient explanatory power in the first stage equation. As shown in Table 5, the F-statistic for the instruments in the first stage is 190, which is well above the threshold level of 30 that is usually necessary in two-stage least squares models. The next test is a test of the endogeneity of the instrument. We use a robust regression-based test proposed by Wooldridge (1995), which is appropriate for regressions with robust standard errors in a two-stage least square setting. Although the endogeneity test suggests that the mother's experience with IPV is not endogenous, we should interpret this result with caution given the concerns discussed above about the validity of the instrument. With one instrument it is not possible to conduct an over-identification test of the exclusion restriction.

For the next three outcome variables – grade advancement conditional on staying in school (vs. repeating), recent drop-out, and unconditional grade advancement (vs. repeating or dropping-out) – the point estimates of the effect of IPV are roughly similar across non-IV and IV methods. Again, the IV methods produced much less precise and therefore statistically insignificant estimates. In each case, the test of endogeneity reveals that exogeneity cannot be rejected (see Table 5), so it is appropriate to focus on the results of the Probit and matching models for these three outcome variables.

Comparing the boosted matching, conventional matching and Probit results, we note that they are generally of the same order of magnitude. The two matching methods produce very similar results and the results from the probit estimation are typically larger probably because matching does a better job in selecting the control group. According to these models, mother's IPV has no discernible effect on the likelihood that children dropped out of school in the year prior to the survey. The two grade advancement measures do, however, seem to be affected. IPV significantly reduces grade advancement conditional on continued attendance at school by 1.5 and 1.8 percentage points, respectively, according to the both

[7]The reference child has all his continuous variables set at the mean and all his dummy variables set at zero.

matching estimators and the Probit estimator. Overall, in our sample, only four percent of children do not advance from one grade to the next (conditional on staying in school) (See Table 3). IPV therefore increases non-advancement by a relative rate of 34 to 41 percent.

Based on the same estimators, unconditional grade advancement – a combination of the drop-out and unconditional advancement variables – is reduced by 2.1 to 2.8 percentage points. Again, these estimates are statistically significant at higher than the 1 percent level. In this sample, eight percent of children did not advance, instead dropping out or repeating the grade. IPV thus increases non-advancement by a relative rate of 25 to 33 percent. The fact that results differ for conditional and unconditional advancement may indicate that there is, in fact, increased drop-out due to IPV, but that it is imprecisely estimated. The next two dependent variables, current grade – conditional on still being in school – and completed years of education, are appropriately treated as count data and therefore require different estimation methods. Unlike the previous three outcome variables, the point estimates have different signs for the IV and non-IV methods, although the IV results are, as before, statistically insignificant. The difference in the direction of the results suggests that endogeneity could indeed be a problem in these cases. This is supported by the fact that the endogeneity test in Table 5 actually rejects exogeneity at the 10 percent level for "current grade" and "completed years of education." Results for current grade using a Poisson model[8] and boosted and conventional matching models are negative and statistically significant for IPV, suggesting that 6 to 12 percent of children exposed to IPV are likely to be delayed by one grade. For completed years of education, the non-IV estimates of −0.09 to −0.12 indicate that IPV results in the loss of about one-tenth of a year of education. However, the potential endogeneity of IPV casts doubt upon these results.

Finally, the grades per year variable provides yet another way of understanding children's schooling attainment by measuring the average number of grades completed per year of exposure to school. As discussed above, we use a sample of children ages 10 to 14. The appropriate regression methodology in this case is Ordinary Least Squares (OLS) for the non-IV model and two-stage least squares for the IV model. As in the case of the two previous outcome variables, the IV and non-IV methods produce different results. The OLS and the conventional matching results are both statistically significant at the 5 percent level; they suggest a small decrease of 0.017 to 0.02 grades per year due to IPV. In contrast, the IV-regress results indicate a positive effect of 0.10 that is statistically significant at the 5 percent level. The endogeneity test also suggests that exogeneity is rejected at a p-value of 0.008, casting doubt on the non-IV results.

A Note on Matching Model Methodology

In all but one of the results for IPV discussed above, matching model estimates are statistically significant at the 5 percent level, the exception being recent dropout. We conducted a sensitivity analysis on the choice of matching methodology used to assess the robustness of our results (See Table 6). All the methods test the mean difference in the

[8]We also estimated a negative binomial model. The negative binomial differs from the Poisson model by a parameter alpha, which measures data dispersion. When alpha equals zero, the negative binomial reduces to a Poisson. In our case the estimated alpha was small enough to be effectively zero, so we did do not present results from this model.

outcome between treated observations and matched control observations. We discussed above the difference between the conventional way of estimating the propensity score, which uses a Probit regression, and the boosted regression model, which makes use of a non-parametric data-adaptive algorithm. Once the propensity score is estimated, matching is carried out in the same way in both methods.

If kernel matching methods are used, the control observations that are matched to each treated observation are weighted using a kernel function. The kernel function places higher weight on untreated observations that are closer to the treated observation and lower weight on more distant ones (Heckman et al. 1998). The kernel function we use for our base estimate is Epanechnikov (the default). We also conduct sensitivity analyses using Normal and Uniform kernel functions.[9] Five nearest neighbors matching uses the average outcome for the five untreated observations closest to each treated observation, giving each of them equal weight.

We present, in Table 6, the results from the conventional matching model using an Epanechnikov kernel function as well as ones from boosted matching using Epanechnikov, normal and uniform kernel functions, as well as five-nearest neighbors matching.[10] Since we also needed to make a decision on the level of interactions to include in the boosted regression model, we also conduct sensitivity analysis on this parameter. Our base model uses a three-level interaction model, but we present in Table 7, results for two and four-level interactions for comparison.

We can see from Tables 6 and 7 that the results for “attending school”, “grade advancement or not” and “advance or drop out-repeat” are highly robust to the choice of matching method. The results for “recent drop out”, which were statistically insignificant using the Epanechnikov kernel method in the boosted matching model, become larger and statistically significant at the 10 percent level when using conventional matching or a normal kernel function. The results for grade attainment are somewhat less robust, ranging from −0.09 to −0.19, and their statistical significance similarly ranges from 5 percent to under 0.1 percent. Results for years of education are also less robust, ranging from −0.05 and statistically insignificant (five-nearest neighbors and model with four interactions) to −0.1595 and significant at the 0.1 percent level using the normal kernel function. Similarly results for grade per year vary from −0.0075 and statistically insignificant (5-nearest neighbors) to −0.0184 and significant at the 1 percent level (normal kernel).

After matching, balancing tests for all covariates were conducted. Ideally, means of covariates should be the same for the treated and matched untreated observations. In the conventional matching model, all the covariates are balanced for all outcomes, meaning that difference in means tests between the means of all the covariates in the treated and matched control samples were insignificant. The boosted matching model did not perform as well in

[9]The kernel function K(u) is a function of the distance measure $u = (p_i - p_j/h)$, where P_i is the propensity score of the treated observation and P_j is the propensity score of the control observation and h is a pre-specified bandwidth (set to a default of 0.06 in PSMatch2). For the Epanechnikov kernel $K(u) \propto (1 - u^2)$ if $|u \le 1|$, $K(u) = 0$, otherwise. The Normal Kernel is based on $K(u) \propto \exp(-u^2/2)$ and uses all untreated observations. The uniform kernel uses equal weights for all observations falling within the bandwidth h. See Sianesi (2001).

[10]Similar sensitivity results on the conventional matching model are available from the authors upon request.

balancing the covariates, with the number of unbalanced covariates specified in Table 6 for each method and outcome variable. The fact that the boosted model does not match the covariates as well at the mean value does not mean that it necessarily performs worse than the conventional model. A highly non-linear non-parametric propensity score equation, such as the one estimated by the boosted model, might do a poorer job matching the covariates of the treated and controls at the mean, but a better job matching at the other points of the distribution.

Other Results

With respect to estimated effects of other variables on the educational outcomes, the results were remarkably consistent across the different parametric estimation methods in terms of signs and levels of significance (See Tables B1 to B7, available upon request). A few variables stand out. The effect of the child being female is positive across all dependent variables except drop-out, where it is insignificant. As is typical in such analyses, mother's education always has positive effects on educational outcomes, as does partner's education for attendance and grade attainment. Municipality-level averages of female education had the expected effects in most cases (attendance, drop-out and unconditional attainment). Oddly enough, higher municipality-level education averages for adult males increased drop-out. If the household had migrated, children's educational outcomes were negatively affected. Wealth consistently improved outcomes except in the case of conditional grade advancement, where the effect is insignificant. The household's numbers of 0–5 year-olds had negative effects on all educational outcomes for children 6–14. Even the municipality level child-woman ratio had negative effects on grade advancement (conditional and unconditional) and grade attainment. Clearly, caring for young children is among the responsibilities of 6–14 year-olds in Colombia – except, perhaps, when the household includes multiple women ages 18–64, whose presence has positive effects on children's school attendance and grade attainment.

Conclusions and limitations

Relatively few studies have considered the effects of family violence on children's educational outcomes. The studies that do so almost never take account of the possibility of endogeneity and selection bias, as we do using cross-sectional data and as Emery (2011) and Margolin et al. (2010) have done using panel data. There are two main approaches in the literature to deal with selection bias in observational studies using cross-sectional data, namely non-parametric matching approaches and instrumental variable approaches.[11] Each of these approaches makes fairly strong, but different assumptions, to obtain estimates than can be interpreted as causal. The main assumption needed for non-parametric matching approaches is that selection into the "treatment" – IPV in our case – is solely based on observables. The main assumption for IV approaches is that an exogenous instrument can be found that affects the "treatment" but that is excludable from the outcome equation. It is this exclusion restriction that is often difficult to support. We make a good faith effort to find an

[11]A third approach is the use of sibling difference methods with household fixed effects to control for unobserved heterogeneity at the level of the household (see Coneus et al. 2012 and Kane et al. 2013). This approach is not available to us because all children in the household are exposed to IPV.

instrument that satisfies these conditions but, as usual, it is extremely hard to exclude all possible threats to its validity. It is possible, for example, that unobservables in a partner's family background that are associated with his own exposure to violence could also directly affect the child's educational success; this is a limitation that we cannot overcome using cross-sectional data.

Even if endogeneity is not an issue, selection on observables can still be a major challenge because of the highly intertwined factors that affect family outcomes. We address this challenge by adopting the most flexible non-parametric methods available to match affected children with comparable controls. While these non-parametric matching methods are increasingly used to obtain estimates of causal effects (see for example Kane et al. 2013), they also make strong assumptions that may not be supported; we have discussed these limitations above.

To tackle endogeneity, it is necessary to have a convincing instrument. The limitations of this study are primarily due to potential questions about the validity of the instrument. Despite much evidence in the literature on the relevance of our instrument of choice – the childhood experience of violence of the mother's partner – on the probability of the mother being exposed to IPV, its validity is subject to exclusion and monotonicity restriction assumptions, which although plausible can always be questioned.. Using this instrument, we obtain IV estimates of the effect of IPV on a variety of education outcomes. Although our IV estimates are almost always statistically insignificant, they are qualitatively similar to the non-IV estimates for some outcomes, such as grade advancement, advance vs. dropout/ repeat, and recent dropout, but qualitatively different for others, such as current grade, completed years of education, and grades per year of exposure to school. This suggests that endogeneity is a concern for at least some of the outcome variables. Our statistical tests of endogeneity do in fact reject endogeneity for the outcome variables where the IV and non-IV results are qualitatively similar and fail to reject it in the three instances where the results are qualitatively different (current grade, completed years of school and grades per year).

If we take our results on the four binary dependent variables at their face value, a woman's experience of intimate partner violence has a negative effect on her children's school attendance, decreasing it by 1.2 to 2.7 percentage points, according to the matching and Probit results, respectively. This effect sounds small but is in fact substantial if compared to an average nonattendance rate of 6.9 percent: thus IPV increases non-attendance for the average child by 17 to 39 percent. While mother's IPV does not seem to affect the probability that a child dropped out of school in the previous academic year, the two grade advancement measures are negatively affected by IPV. It reduces grade advancement conditional on staying in school by about 1.5 to 1.8 percentage points and 2.1 to 2.8 percentage points, unconditional on staying in school. Again, these effects are substantial as they constitute from 34 to 41 percent of the average conditional non-advancement rate and from 25 to 33 percent of the average unconditional rate, respectively. Studies conducted in Brazil (Durand et al., 2011) and the United States (Emery, 2011) show similar findings.

Since we obtained contradictory results for the remaining outcome variables, which are either continuous or count variables, we are unable to draw conclusions about how IPV affects them. However, the fact that the results are different underscores the potential importance of addressing endogeneity in studies on the effects of IPV on various household outcomes.

Our study advances understanding of the importance of addressing endogeneity in the study of the effects of intimate partner violence on household outcomes by comparing results across multiple statistical strategies that take different approaches to correcting for endogeneity. While each approach has its own identifying assumptions and therefore limitations, they make *different* assumptions. This allows us to check the robustness of the estimates, something rarely done in this literature. While none of these approaches is a perfect way of tackling the very challenging problem of endogeneity, together they allow us to move forward in addressing a very serious issue in a methodologically rigorous way.

Future studies on this topic would benefit greatly from longitudinal data. Cross-sectional data collection efforts can contribute to more reliable analyses by systematically considering the need for instrumental variables and by collecting information on the multiple realms of violence in respondents' lives (Margolin et al. 2010), both past and present

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Table 1

Various School Outcomes for Children Ages 6–14 by Presence of Intimate Partner Violence in household (weighted)

	Intimate Partner Violence in HH			Sample size*	p-value of t-test of difference in means
	No IPV	IPV	Total		
Attending school or not	93.7	91.9	93.3	21,827	0.0000
Grade advancement or not†	96.0	94.1	95.6	19,683	0.0000
Recent drop out or not	2.18	2.58	2.27	20,433	0.0061
Advance or drop out / repeat	92.3	89.1	91.6	20,429	0.0000
Current grade if still in school	4.31	3.91	4.22	20,370	0.0000
Years of education if ever attended (ages 10–14)	5.03	4.72	4.96	11,689	0.0000
Grades per year of exposure to school (ages 10–14)	0.836	0.789	0.826	11,890	0.0000

*Unless otherwise noted, refers to children 6–14 living with mothers 15–49 who responded to the domestic violence module of the Colombia 2005 DHS.

†Conditional on staying in school

Source: Authors' calculations from Colombia 2005 DHS.

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Table 2

Presence of Intimate Partner Violence in Household (%) by Partner Maltreatment as a Child (weighted)

	Partner Maltreated as a Child			
Intimate Partner Violence in Household	**No**	**Yes**	**Missing**	**Total**
Column Percent				
No IPV	83.3	67.2	71.7	76.9
IPV	16.7	32.8	28.3	23.1
Total	100.0	100.0	100.0	100.0
Row Percent				
No IPV	61.6	27.1	11.3	100.0
IPV	41.0	44.1	14.9	100.0
Total	56.8	31.0	12.2	100.0
Sample size*	12,304	6,692	2,831	21,827

* See note to Table 1. Source: Authors' calculations from Colombia 2005 DHS.

Table 3

Descriptive Statistics, Children Ages 6–14, Colombia, DHS 2005 (Weighted)

Variable	Mean	Standard Deviation
Dependent variables:		
school attendance (or not) †	0.933	0.250
grade advancement (vs. repeat, conditional on staying in school) †	0.956	0.205
recent dropout (or not)	0.023	0.149
grade advancement (vs. drop-out / repeat) †	0.916	0.278
current grade if still in school	4.222	2.477
years of education if ever attended (ages 10–14)	4.962	1.854
grades per year of exposure to school (ages 10–14)	0.826	0.267
Instrumental variables:		
partner beaten when a child†	0.310	0.463
missing: partner beaten as child†	0.122	0.327
mother's parents' violence†	0.335	0.472
missing: mother's parents' violence†	0.039	0.193
Endogenous regressor:		
mother's intimate partner physical violence	0.310	0.463
Child variables:		
child is female†	0.495	0.500
child's age	9.923	2.545
child's age-squared/100	1.050	0.511
child is son/daughter of HH head†	0.845	0.362
Mother variables:		
mother's age when child was age 6	31.779	5.806
mother's age squared/100 (when child was 6)	10.436	3.828
mother's years of education completed	7.036	4.147
mother cohabitating (reference category) †	0.432	0.495
mother is married†	0.364	0.481
mother is widow†	0.030	0.171
mother is divorced etc. †	0.174	0.379
Partner-of-mother variables:		
partner's years of education completed	6.732	4.458
missing: partner's years of education†	0.028	0.165
Household variables:		
HH has migrated†	0.148	0.355
wealth quintile 1 (reference category)†	0.227	0.419
wealth quintile 2†	0.222	0.416
wealth quintile 3†	0.206	0.405

Variable	Mean	Standard Deviation
wealth quintile 4 †	0.187	0.390
wealth quintile 5 (richest) †	0.158	0.364
Household composition variables: §		
mother has relatives in HH †	0.164	0.370
partner has relatives in HH †	0.051	0.221
# children ages 0–5	0.642	0.856
# girls ages 6–11	0.725	0.766
# boys ages 6–11	0.726	0.756
# girls ages 12–14	0.335	0.545
# boys ages 12–14	0.365	0.562
# girls ages 15–17	0.161	0.404
# boys ages 15–17	0.179	0.432
# women ages 18–64	1.360	0.693
# men ages 18–64	1.149	0.762
# women ages 65+	0.086	0.292
# men ages 65+	0.069	0.256
Geographic variables:		
rural (vs. urban) †	0.309	0.462
Central (reference category) †	0.164	0.370
Atlantic region †	0.227	0.419
Oriental region †	0.194	0.396
Pacific region †	0.169	0.374
Bogota region †	0.134	0.341
Territories region †	0.112	0.315
Municipality-level variables:		
average wealth factor score	−0.021	0.061
average years of education, women 25–64	7.139	1.524
average years of education, men 25–64	7.123	1.726
child-woman ratio (0–4 / f 15–49)	0.386	0.114
% of HHs female-headed	29.687	5.844
% of population living abroad §§	1.186	1.361
% of employed women in formal work	23.784	10.333
% of employed men in formal work	20.063	11.411
% HHs with access to piped water	85.092	17.048
% HHs with access to sewer	68.508	26.378
% HHs cooking with firewood etc.	18.836	20.960
Sample size		21,827

Notes:

§ includes the index child

[§§] individuals abroad / individuals present in municipality

[†] binary variables

Table 4

Marginal Effects of Intimate Partner Violence on Children's Educational Outcomes, Colombia, 2005

Dependent Variable	Boosted Matching		Conventional Matching		Probit		IV Probit	IV Regress		N
Attending school or not	−0.0121		−0.0121		−0.0271		−0.0008	−0.0074		21,827
(SE)	(0.0046)	**	(0.0046) **		(0.0112)	*	(0.0859)	(0.0351)		
(BSSE)	*(0.0062)*	+	*(0.0054)*	*						
Grade advancement or not†	−0.0154		−0.0152		−0.0184		−0.0269	−0.0124		19,683
(SE)	(0.0044)	***	(0.0043)	***	(0.0056)	***	(0.0396)	(0.0278)		
(BSSE)	*(0.0049)*	**	*(0.0053)*	**						
Recent drop out or not	0.0041		0.0052		0.0099		−0.0020	0.0045		20,433
(SE)	(0.0028)		(0.0028)	+	(0.0067)		(0.0435)	(0.02)		
(BSSE)	*(0.0033)*		*(0.0032)*							
Advance or drop out / repeat	−0.0223		−0.0206		−0.0284		−0.0338	−0.0227		20,429
(SE)	(0.0054)	***	(0.0053)	***	(0.0078)	***	(0.0547)	(0.0352)		
(BSSE)	*(0.0058)*	***	*(0.0058)*	***						
	Boosted Matching		**Conventional Matching**		**Poisson**		**IV Poisson**	**IV Regress**		**N**
Current Grade	−0.1218		−0.0937		−0.0590		0.1525	0.1794		20,370
(SE)	(0.0452)	**	(0.0437)	*	(0.0176)	***	(0.1162)	(0.1523)		
(BSSE)	*(0.0415)*	**	*(0.0454)*	*			*(0.1131)*			
Completed Years of Education ††	−0.104		−0.1216		−0.0918		0.3296	0.3489		11,689
(SE)	(0.0465)	*	(0.0448)	**	(0.0287)	**	(0.2029)	(0.2478)		
(BSSE)	*(0.0489)*	*	*(0.0458)*	**			*(0.2025)*			
	Boosted Matching		**Conventional Matching**		**OLS**			**IV Regress**		**N**
Grades per year §§	−0.0086		−0.0214		−0.0166			0.1015		11,890
(SE)	(0.0069)		(0.0084)	*	(0.0059)	**		(0.0458)	*	
(BSSE)	*(0.0081)*		*(0.0100)*	*						

Note:

*** p ≤ 0.001,

** p ≤ 0.01,

[*] $p \leq 0.05$,

[+] $p<=0.1$

Analytical standard errors (SE) are in parentheses and Bootstrapped standard errors (BSSE) are in parentheses and italics.

SEs are corrected for clustering of children in the same household. Bootstrapped SEs are based on 100 replications.

[†] Conditional on staying in school.

[§] Regressions limited to children 6 to 14 who are currently enrolled in school

[††] Regressions limited to children ages 10 to 14 who ever attended school

[§§] Regressions limited to children ages 8 to 14.

Table 5

Results of Tests for Goodness of Fit and Endogeneity for IV Methods

	Test Statistic	Attending school or not	Grade advancement or not†	Recent drop out or not	Advance or drop out / repeat	Current Grade	Completed Years of Education	Grades per year
First Stage Goodness of Fit	F(1, C-1)†	190.32	187.45	197.35	197.429	196.1	118.82	117.77
	p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	Adj. R-Sq.	0.0678	0.0663	0.0696	0.0673	0.0678	0.0602	0.0607
Robust Test of Endogeneity	F (1, C-1)	0.0230	0.0132	0.0002	0.0032	2.7548	3.60656	7.0508
(H0: treatment is exogenous)	p-value	0.8795	0.9087	0.9896	0.9549	0.0970	0.0576	0.0079
Number of Observations	N	21,827	19,683	20,433	20,429	20,370	11,689	11,890
Number of Clusters	C	13,182	12,365	12,713	12,710	12,620	8,705	8,799

Notes:

All tests are conducted using the IV regress two-stage least squares model.

All tests are adjusted for clustering at the household level.

†C= number of clusters (households)

Table 6

Analysis of the Sensitivity of Results to Using Conventional vs. Boosted Matching and to Different Matching Methodologies in Boosted Matching. Marginal Effects of Intimate Partner Violence on Children's Educational Outcomes

Dependent Variable	Conventional Matching			Boosted Matching											
	Kernel Epanechnikov			**Kernel Epanechnikov**			**Kernel Normal**			**Kernel Uniform**			**Five Nearest Neighbors**		
Attending school or not	−0.0121	**	§(0)	−0.0121	**	§(9)	−0.0139	**	§(14)	−0.0123	**	§(7)	−0.0139	**	§(13)
(SE)	(0.0046)			(0.0046)			(0.0045)			(0.0046)			(0.0051)		
Grade advancement or not	−0.0152	***	§(0)	−0.0154	***	§(6)	−0.0160	***	§(8)	−0.0155	***	§(2)	−0.0142	**	§(10)
(SE)	(0.0043)			(0.0044)			(0.0043)			(0.0043)			(0.0048)		
Recent drop out or not	0.0052	+	§(0)	0.0041		§(12)	0.0046	+	§(12)	0.0043		§(12)	0.0024		§(17)
(SE)	(0.0028)			(0.0028)			(0.0027)			(0.0028)			(0.0031)		
Advance or drop out / repeat	−0.0208	***	§(0)	−0.0223	***	§(8)	−0.0234	***	§(8)	−0.0224	***	§(6)	−0.0253	***	§(13)
(SE)	(0.0053)			(0.0054)			(0.0053)			(0.0054)			(0.0059)		
Grade Attainment	−0.0937	*	§(0)	−0.1218	**	§(5)	−0.1743	***	§(9)	−0.1324	**	§(3)	−0.1002	*	§(9)
(SE)	(0.0437)			(0.0452)			(0.0433)			(0.0447)			(0.0497)		
Years of Education	−0.1216	**	§(0)	−0.104	*	§(12)	−0.1595	***	§(2)	−0.1141	*	§(9)	−0.0530		§(19)
(SE)	(0.0448)			(0.0465)			(0.0447)			(0.0461)			(0.0530)		
Grades per year	−0.0214	*	§(0)	−0.0086		§(5)	−0.0184	**	§(2)	−0.0105		§(2)	−0.0075		§(12)
(SE)	(0.0084)			(0.0069)			(0.0067)			(0.0069)			(0.0077)		

Notes:

*** $p \leq 0.001$,

** $p \leq 0.01$,

* $p \leq 0.05$,

+ $p<=0.1$

Standard errors (SE) are in parentheses.

All treated and untreated observations are on the common support.

§ Number of covariates that are not balanced are in parentheses.

Table 7

Analysis of the Sensitivity of Boosted Matching Results to Using Different Levels of Interaction in Boosted Regression.

Marginal Effects of Intimate Partner Violence on Children's Educational Outcomes

Dependent Variable	Three Interactions			Two Interactions			Four Interactions		
Attending school or not	−0.0121	**	§(9)	−0.0122	**	§(8)	−0.0120	*	§(8)
(SE)	(0.0046)			(0.0046)			(0.0048)		
Grade advancement or not	−0.0154	***	§(6)	−0.0141	**	§(10)	−0.0160	***	§(12)
(SE)	(0.0044)			(0.0045)			(0.0045)		
Recent drop out or not	0.0041		§(12)	0.0041		§(8)	0.0038		§(13)
(SE)	(0.0028)			(0.0028)			(0.0029)		
Advance or drop out / repeat	−0.0223	***	§(8)	−0.0225	***	§(9)	−0.0210	***	§(10)
(SE)	(0.0054)			(0.0053)			(0.0055)		
Grade Attainment	−0.1218	**	§(5)	−0.1923	***	§(7)	−0.1507	**	§(6)
(SE)	(0.0452)			(0.044)			(0.0463)		
Years of Education	−0.104	*	§(12)	−0.1356	**	§(2)	−0.0523		§(6)
(SE)	(0.0465)			(0.0455)			(0.0485)		
Grades per year	−0.0086		§(5)	−0.0132	+	§(5)	−0.0126	+	§(8)
(SE)	(0.0069)			(0.0068)			(0.0071)		

Notes:

*** $p \leq 0.001$,

** $p \leq 0.01$,

* $p \leq 0.05$,

+ $p<=0.1$

Standard errors (SE) are in parentheses.

All treated and untreated observations are on the common support.

§ Number of covariates that are not balanced are in parentheses.

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Appendix Table A1

Coefficient Estimates from First Stage Regressions or IV-Regress and Conventional Matching Models[†]
Dependent Variable: Whether or not mother experienced physical intimate partner violence in past 12 months

	Linear Probability Model		Conventioal Matching (Probit)	
Instruments				
mother's partner experienced violence as child	0.1282	***	0.4502	***
	(0.0093)		(0.0220)	
Child variables:				
child is female	0.0030		0.0103	
	(0.0050)		(0.0263)	
child's age	−0.0151	+	−0.0551	
	(0.0083)		(0.0345)	
child's age-squared	0.0468		0.1703	
	(0.0413)		(0.1748)	
child is son/daughter of HH head	−0.0309	*	−0.1099	**
	(0.0149)		(0.0370)	
Mother variables:				
mother's age when child was age 6	−0.0049		−0.0034	
	(0.0057)		(0.0172)	
mother's age squared/100 (when child was 6)	−0.0024		−0.0316	
	(0.0085)		(0.0263)	
mother's years of education completed	−0.0032	*	−0.0125	***
	(0.0013)		(0.0034)	
mother is married	−0.0469	***	−0.1983	***
	(0.0090)		(0.0250)	
mother is widow	−0.0030		−0.0124	
	(0.0227)		(0.0617)	
mother is divorced etc.	0.1489	***	0.4725	***
	(0.0140)		(0.0312)	
Partner-of-mother variables:				
partner's years of education completed	−0.0014		−0.0062	+
	(0.0012)		(0.0032)	
missing: partner's years of education	−0.0623	*	−0.2072	***
	(0.0273)		(0.0638)	
missing data on mother's partner childhood experience of violence	0.0872	***	0.3179	***
	(0.0132)		(0.0297)	
Household variables:				
HH has migrated	0.0166		0.0575	*
	(0.0110)		(0.0265)	
wealth quintile 2	0.0101		0.0285	
	(0.0137)		(0.0331)	
wealth quintile 3	0.0107		0.0300	

	Linear Probability Model		Conventioal Matching (Probit)	
	(0.0161)		(0.0398)	
wealth quintile 4	−0.0035		−0.0243	
	(0.0179)		(0.0459)	
wealth quintile 5 (richest)	−0.0151		−0.0851	
	(0.0202)		(0.0542)	
Household composition§ variables:				
mother has relatives in HH	−0.0372	*	−0.1291	***
	(0.0154)		(0.0389)	
partner has relatives in HH	−0.0024		0.0006	
	(0.0190)		(0.0499)	
# children ages 0–5	0.0016		0.0032	
	(0.0052)		(0.0124)	
# girls ages 6–11	0.0098		0.0338	*
	(0.0064)		(0.1538)	
# boys ages 6–11	0.0062		0.0202	*
	(0.0064)		(0.0154)	
# girls ages 12–14	0.0035		0.0160	
	(0.0087)		(0.0208)	
# boys ages 12–14	0.0113		0.0427	*
	(0.0083)		(0.0203)	
# girls ages 15–17	0.0132		0.0476	*
	(0.0099)		(0.0240)	
# boys ages 15–17	0.0088		0.0317	
	(0.0096)		(0.0229)	
# women ages 18–64	−0.0115	+	−0.0409	*
	(0.0066)		(0.0184)	
# men ages 18–64	0.0066		0.0227	
	(0.0060)		(0.0153)	
# women ages 65+	−0.0082		−0.0411	
	(0.0154)		(0.0409)	
# men ages 65+	−0.0227		−0.0854	+
	(0.0158)		(0.0446)	
Geographic variables:				
rural (vs. urban)	−0.0177		−0.0674	*
	(0.0127)		(0.0313)	
Atlantic region	−0.0085		−0.0394	
	(0.0126)		(0.0319)	
Oriental region	0.0182		0.0669	+
	(0.0138)		(0.0346)	
Pacific region	0.0159		0.0533	
	(0.0139)		(0.0346)	
Bogota region	0.0408	*	0.1373	**

	Linear Probability Model		Conventioal Matching (Probit)	
	(0.0196)		(0.0490)	
Territories region	−0.0730	***	−0.2654	***
	(0.0147)		(0.0398)	
Municipality-level variables:				
average wealth factor score	−0.0629	+	−0.1959	*
	(0.0355)		(0.0884)	
average years of education, women 25–64	0.0073		0.0194	
	(0.0103)		(0.0241)	
average years of education, men 25–64	−0.0024		−0.0007	
	(0.0091)		(0.0212)	
child-woman ratio (0–4 / f 15–49)	0.0375		0.1588	
	(0.0627)		(0.1522)	
% of HHs female-headed	0.0032	***	0.0108	***
	(0.0010)		(0.0022)	
% of population living abroad[§§]	-0.0029		-0.0083	
	(0.0038)		(0.0087)	
% of employed women in formal work	0.0008		0.0026	
	(0.0009)		(0.0022)	
% of employed men in formal work	0.0010		0.0031	
	(0.0009)		(0.0023)	
% HHs with access to piped water	0.0007	+	0.0027	**
	(0.0004)		(0.0010)	
% HHs with access to sewer	-0.0003		-0.0010	
	(0.0004)		(0.0009)	
% HHs cooking with firewood etc.	-0.0005		-0.0016	
	(0.0005)		(0.0013)	
Constant	0.2759	*	-0.7672	*
	(0.1321)		(0.3742)	
number of observations	21,827		21,827	

Notes:

[†] This is the first stage for the IV-regress model for the “in-school regression”. The first stage for the IV-Probit and IV Poisson and for other outcome variables is very similar.

[§§] individuals abroad / individuals present in municipality

[***] $p \leq 0.001$,

[**] $p \leq 0.01$,

[*] $p \leq 0.05$,

[+] p<=0.1