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Conditional Cash Transfers for Women and Spousal Violence: Evidence of the Long-Term Relationship from the Oportunidades Program in Rural Mexico

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Relationship from the
Oportunidades Program in Rural
Mexico

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Inter-American Development Bank
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Abstract*

This paper provides evidence of the long-term relationship between male-to-female spousal violence and the Oportunidades conditional cash transfer program. It uses data from three nationally representative surveys that include detailed information on the prevalence of spousal abuse and threats of violence against women. Constructing comparable groups of beneficiary and nonbeneficiary households within each village to minimize potential selection biases, the present study finds that, in contrast to the short-run estimates, physical and emotional abuse rates over the long term do not differ significantly between existing beneficiary and nonbeneficiary couples. The paper examines possible causes for the difference, most importantly, the role that marital selection and the diffusion of norms rejecting intimate partner violence may play in explaining these effects.

JEL codes: D03, J12, J16

Keywords: domestic violence, CCT, Mexico, spousal violence, intimate partner violence, violence, Prospera program, Progresa program, Oportunidades program

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1. Introduction

Violence against women has been condemned internationally as a serious human rights, public health, and women's personal security issue. It is of particular concern that partner violence is still quite prevalent across societies. A question of great relevance for policymakers is whether social insurance programs intended to improve women's economic conditions help to effect a sustained reduction in the incidence and severity of intimate partner violence (IPV). Several countries have introduced conditional cash transfer (CCT) programs—poverty alleviation programs that provide funds to adult women in households—based on a growing consensus that targeting resources to females promotes the empowerment of women within the household.¹ However, a possible unintended consequence of this gender-based targeting may be an increased incidence of violence: male partners may resort to the use of violence to gain or regain control over household resources or decision-making. Existing studies of the short-term consequences of the CCT program “Oportunidades” in Mexico (recently named “Prospera”) are partially consistent with this view.²

This paper provides evidence of the longer-term relationship between the Oportunidades program and male-to-female spousal violence. It uses data from three nationally representative surveys, the National Survey on Relationships within the Household (Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares, or ENDIREH) of 2003, 2006, and 2011. The surveys include detailed information on the prevalence of male-to-female spousal abuse and threats of violence against women. Using this data, a pseudo-panel of comparable groups of beneficiary and nonbeneficiary couples within each village is constructed to minimize potential selection biases and allow for estimates of longer-term relationships—9 to 13 years following the program's implementation—and to compare these to patterns observed in the short term.

¹ See the seminal work by Thomas (1990) and a survey of this literature by Duflo (2012). Extensive research has examined this in the context of the Progreso/Oportunidades program; see for instance Attanasio and Lechène (2002), Bobonis (2009), Rubalcava, Teruel, and Thomas (2009).

² See Angelucci (2008) and Bobonis, González-Brenes, and Castro (2013).

The study provides two stylized facts. First, it shows that the incidence of abuse among women is initially high among these couples (in 2003), trending downward over time (in 2006, 2011). Second, among couples in 2006 and 2011, women in beneficiary households are as likely to experience physical or nonphysical forms of abuse as are women in nonbeneficiary couples. These findings stand in stark contrast to the short-term relationship established in observational and experimental studies—women in beneficiary households are significantly less likely to be victims of physical abuse than nonbeneficiary women (e.g., Angelucci 2008; Bobonis, González-Brenes, and Castro 2013; Haushofer and Shapiro 2013; Hidrobo and Fernald 2013; Hidrobo, Peterman, and Heise 2015; Perova 2010).

What explains both the decreased incidence of abuse and the perceived reduction in the capacity of this CCT program to protect adult women against spousal violence? The new evidence presented by this study is consistent with two forces, related and not mutually exclusive, that can impinge on the incidence of violence. First is the possibility of marital selection: couples with aggressive partners may be more likely to dissolve (Bowlus and Seitz 2006). The dynamic selection of couples remaining in union based on male partners' potential for aggression is consistent with both the decrease in the incidence of violence among couples remaining in union as well as with a reduction in the need of the CCT program to help protect women remaining in union against IPV. A second relevant factor is the increasing rejection of IPV by women in Mexico over the past decade. Specifically, recent research has documented a rapid global diffusion of attitudes and norms regarding the unacceptability of IPV during the first decade of the 21st century (Pierotti 2013). The spread of these global attitudes across Mexican society could also help explain these results.

Additional evidence from this study supports both views. Couples eligible for the program experienced a modest increase in marital dissolution rates. Second, we show that an important predictor of spousal abuse among current partners—whether the male partner was exposed to spousal abuse between his parents during childhood—decreases substantially

among couples remaining in union across survey waves. Third, we show evidence suggesting that levels of violence among couples formed after the start of the program are lower than among couples formed before the start of the program, which is consistent with a lower incidence of abuse among new couples. Consistent with the attitudinal shift hypothesis, the study shows that over time women are more likely to reject the justification of IPV, consistent with the cross-national evidence of a shift in attitudes toward IPV. Finally, we show evidence inconsistent with a number of other potential explanations, such as localized spillover effects among nonbeneficiary couples and generalized social violence mediating effects on spousal abuse.

The study has important implications for policy. The program may, in the short term, increase the likelihood of violent threats, which may in turn compromise women's emotional health their wellbeing. Nonetheless, we can state with some confidence that the program has no longer-term negative consequences for women in the form of higher levels of spousal abuse.

2. Overview of the Oportunidades Program

In 1997 the Mexican government initiated a CCT program named “Progresa”—renamed “Oportunidades” in 2001 under the Fox Administration and recently renamed “Prospera” under the Peña Nieto administration—aimed at alleviating poverty and improving the human development of children in rural Mexico. The program targeted the poor in marginal communities, where 40 percent of the children from poor households drop out of school after the primary level. The program has expanded considerably since its inception and has become an integral component of Mexico's social development and poverty reduction efforts. As of 2013, Oportunidades provides cash transfers to 6.5 million families, conditional on children's school attendance, health checks, and participation in health clinics.

The program promotes children's human development in education, nutrition, and health. Table 1 presents a summary of benefits for the years 2003, 2006, and 2011, the periods

for which we have survey data on interactions among intimate partners. The education component of Oportunidades consists of subsidies typically provided to mothers, contingent on their children's regular attendance at school.³ Although the original program, Progresa, initially targeted only children in primary and middle school, Oportunidades was expanded to cover children in secondary school. In 1998, these transfers ranged from 70 to 255 pesos per month per child (approximately 7 to US\$25), depending on the child's gender and grade level, with a maximum of 625 pesos (US\$62.5) per month per family. Scholarship amounts have gradually increased: in 2011 these ranged from 150 pesos per month (approximately US\$12), up to 960 pesos (US\$77).⁴ Families also receive 200400 pesos US\$16\$32) per year for the purchase of school supplies. A further expansion of the program in 2009now offers a cash transfer of approximately 4,200 pesos to youth graduating from high school before age 22 (*Jóvenes con Oportunidades*).

The health and nutrition components consist of both cash transfers and nutritional supplements. Supplements are targeted at infants six months to 23 months old, pregnant and breast-feeding women, and children aged two to five years who exhibit signs of malnutrition. Monthly cash transfers for beneficiary families expanded throughout 19972011: by 2011 these benefits included: nutritional support (*Alimentario*), 225 pesos (18 USD), originally part of Progresa; energy support (*Energético*), 60 pesos, established in 2007 to help families pay for energy costs (electricity, gas, firewood, etc.); compensated nutritional support (*Alimentario Vivir Mejor*), 120 pesos, established in 2008 to compensate families for rising food prices; child support (*Infantil Vivir Mejor*), 105 pesos for every child aged between 0 and 9, established in

³ Receipt of the education-specific benefits is contingent on children attending school, which is verified by school personnel. For primary and secondary school, the child becomes ineligible for support if he or she misses school four times in a month without justification, or 12 times during the school year. High school students become ineligible if they are not certified as active during the school semester, defined according to the regulations of the institution they attend.

⁴ This nominal average value of transfers has gradually increased since the start of the program, and its purchasing power has varied (depending on price levels in these areas and relative price changes with respect to foreign currencies (i.e., U.S. dollars) throughout the 1997–2011 period. Given these fluctuations, we opt to report the figure valid at the date of the most recent ENDIREH survey, 2011.

2010; elderly support (*Adultos Mayores*), 315 pesos for every adult aged 70 or over, established in 2006. These benefits are contingent on mothers' participation in monthly health talks with the local health care provider, the vaccination of family members, health checks of all children under five years old; and biannual health checks of all household members. Overall, the program transfers are important, representing approximately 10 percent of the average expenditures of beneficiary families (Skoufias, 2001). Maximum benefit levels have increased by approximately 20 percent over time for families with children only in elementary or middle school; but they have almost doubled for those with children in secondary school (Figure 1).

Program targeting was conducted at two levels. First, eligible localities were identified on the basis of a locality-level eligibility rule. Program officials used locality-level characteristics from the Mexican 1995 Mini-Census of Population to construct a marginality index for each locality that reflected its degree of marginalization and was correlated with the community's incidence of poverty.⁵ Second, program enumerators conducted household surveys within eligible localities to identify households that would be classified as poor. Based on asset holdings used as proxy variables for poverty, the program administrators generated a proxy means test.⁶ Within each eligible community, only households below a threshold became program beneficiaries. The list of potential beneficiaries was then discussed in a community meeting and suggested revisions sent to the central Oportunidades office. In practice, very few changes were made to the list of targeted households (Skoufias, Davis, and de la Vega 2001). This targeting and program eligibility information is important in the construction of the sample of eligible women for this study (see Section 4).

⁵ The variables used to construct this marginality index were: (i) the locality's population, (ii) the number of dwellings in the village, (iii) the proportion of the adult population that was illiterate, (iv) the proportion of adults working in the agricultural sector (in 1990), the proportion of households (v) without potable water, (vi) without drainage, (vii) without electricity, (viii) with a dirt floor (in 1990), and (ix) the average number of persons per room in each household (in 1990).

⁶ Within a subsample of communities, a poverty indicator was constructed using household income data collected from baseline surveys. A discriminant analysis was then separately applied in each region in order to identify the household characteristics that maximized the correct classification of as poor and non-poor (minimizing Type I and Type II targeting errors). Eligible households were identified on the basis of this welfare index (see Skoufias, Davis, and de la Vega 2001 for a more detailed description of the targeting process).

Initially, a locality was eligible for Oportunidades if it was classified as “poor” (marginality grade 4) or “very poor” (marginality grade 5) on a 15 scale based on the locality-level marginality index; if it had access to a primary school, a secondary school, and a health center; and if it was classified as rural (defined as inhabited by fewer than 2,500 people), but had at least 50 inhabitants (Skoufias, Davis, and de la Vega 2001). The last criterion was relaxed early on to incorporate semi-urban localities (localities with between 2,500 and 14,999 inhabitants). The health center criterion was relaxed in 1998 when mobile health clinics were introduced. Since then, the inclusion of less marginal localities into the program has been gradually extended. Between 2000 and 2011, the program's coverage expanded from about 53,000 localities and 2.5 million families, to 97,000 localities and 5.8 million families. In urban areas the program was phased in through a different targeting design starting in 2001. Since this targeting mechanism is very complex and substantially different from the one implemented in rural and semi-urban areas, and in order to maintain a sample comparable to that of the short-run study, we focus our analysis on rural households.

3. Data, Measurement, and Summary Statistics

The study uses data from the ENDIREH of 2003, 2006, and 2011. These are three cross-sectional, nationally representative household surveys measuring the prevalence and intensity of intimate partner violence in Mexico, among other intra-household interactions. They contain data on household demographics, socioeconomic characteristics, (limited) marital histories, household decision-making, marital conflict, and a module designed to measure the prevalence and severity of spousal violence. The 2003 survey was administered to 54,230 women 15 years or older living with a husband or partner, whereas the 2006 and 2011 surveys were administered respectively to 113,561 and 152,636 women in the same age range but

independently of marital status. In the following paragraphs, we provide a detailed description of the various measures of violence used in the analysis.⁷

The measures of incidence of violence constructed for this study consist of dichotomous variables indicating whether the female partner had suffered physical, sexual, emotional, or economic abuse from her spouse or partner in the past 12 months. In the case of both physical and sexual violence, a single incident reported within the past year is classified as violence. Physical violence includes pushing, kicking, throwing objects, hitting with hands or objects, choking, attacking with a knife or blade, and shooting. Sexual violence includes demanding sex against woman's will, forced sexual acts, and forced sexual relations. Constructing an incidence measure of emotional violence is a challenging task because this form of violence comprises a complex set of behaviors (Follingstad and DeHart 2000; Strauss and Gelles 1990). Following the authors' earlier work, two measures are constructed: one of incidence of emotional abuse and another of incidence of threats of violence, and assess how results may be sensitive to these definitions.⁸

Data on program participation come from the ENDIREH surveys, and is self-reported by women. The measure of program participation available in the ENDIREH 2003 is whether the woman receives benefits from any government support program. Although Oportunidades is the largest and most generous cash transfer program, there are other small government programs that provide noncash benefits. As a result, this measure may over-report the receipt of Oportunidades benefits. Nonetheless, although there is some noise in the data—because only 10 households per village are randomly selected to participate in the survey—the correlation of the proportion of beneficiary households using the ENDIREH survey data with administrative data on the number of recipient households at the locality level in 2003 is 0.84 (not reported in

⁷ This follows closely the description provided in the documentation and results of the survey in Castro et al (2006).

⁸ See the Data Appendix (Appendix A) for details on the construction of these variables and slight modifications to the structure of questions across the three survey rounds.

the tables), which suggests that the information from the household survey closely represents receipt of Oportunidades benefits.

The ENDIREH 2006 and 2011 surveys ask women specifically whether they receive benefits from Oportunidades, and separately whether they are beneficiaries of other government support programs. In order for the analysis to be comparable to that using data from the ENDIREH 2003 survey, the measure we use is the analogous measure of being a beneficiary from any government support program (i.e., Oportunidades or other). In the sample of women in the ENDIREH 2006 and 2011 selected for analysis, only 3.0 percent and 2.6 percent of those who report being beneficiaries of any government support program are not beneficiaries of Oportunidades. These reliability checks suggest that the information from the household survey closely represents receipt of Oportunidades benefits.⁹

To minimize potential selection biases that may result from the targeting and endogenous take-up of the program, the analysis of the short-run relationship in 2003 had been restricted to a particular subset of households. The 2003 sample includes couples with women 25 years or older, with children younger than 11 years old, who have been in a relationship before the start of the Oportunidades program (that is, since 1997 or earlier). These restrictions result in a sample of 2,867 couples. For this study, we construct a pseudo-panel of comparable households from the 2006 and 2011 surveys. That is, the 2006 (2011) survey sample was restricted to couples with women 28 (33) years or older with children between the ages of three and 13 (eight and 18) years. The resulting overall sample sizes for the longer-term analyses are 4,705 couples in the 2006 survey and 5,800 in the 2011 survey. These sample restrictions minimize confounding that may result from endogenous take-up of the program based on household socioeconomic characteristics and preferences for human capital investments (see Section 4).

⁹ We also estimate analogous models using the ENDIREH 2006 and 2011 data with the Oportunidades beneficiary indicator as the explanatory/treatment variable of interest. The results do not differ in any significant way from those reported in the tables. These are available from the authors upon request.

The summary statistics indicate that spousal violence remains pervasive in rural Mexico but has decreased considerably throughout the period. Whereas 16 percent of women in the sample reported experiencing some form of physical or sexual spousal abuse in the year 2003, the incidence decreased to 13.7 percent by the year 2006 and to 10.2 percent by 2011 (Table 2). The incidence rates of physical, sexual, and emotional violence have all decreased when compared to 2003. In 2011, approximately 7.4 percent of women reported having experienced some form of physical violence (down from 10.8 percent in 2003; significant at the 10 percent confidence level); 4.2 percent reported some act of sexual violence (down from 9.0 percent in 2003; significant at 1 percent confidence); and 6.0 percent reported evidence of emotional abuse in the previous year (down from 11.3 percent in 2003; significant at 1 percent confidence).

Households in the sample are of relatively low socioeconomic status. More important, some stark differences are observable in a number of dimensions of socioeconomic status when the pseudo-panel of couples is compared across survey years. A significant share of women report speaking an indigenous language (14 percent in 2003, 16 percent in 2006, and 20 percent in 2011); this ethnic identity is highly correlated with low socioeconomic status in Mexico (Table 3, Panel A). In addition, approximately 8 percent of women in 2003 have no schooling, and this figure increases to 14 percent among the couples selected in 2011, though 65 percent have completed primary school in 2003, and 56 percent in 2011. The average age of women in the sample is 34.9 years in 2003, increasing to 37.4 years in 2006 and to 42.4 years in 2011. This trend in age is primarily explained by the age restrictions imposed on the samples.

The proportion of women who report having been exposed to spousal abuse between their parents during childhood is quite high, at approximately 10 percent in 2003, 11 percent in 2006, and 13 percent in 2011 (Panel A). Existing evidence regarding the intergenerational transmission of violent behavior suggests that women in this context may be at a particularly

high risk of experiencing spousal violence, which may help explain the prevalence of abuse reported above.

Male partners belong to the same age group (the average partner age is 37.7 years in 2003, 45.8 years in 2011), have attained similar levels of schooling, and are as likely to have an indigenous background (Table 3, Panel B). The proportion of women reporting that their male partners were exposed to spousal abuse between their parents during childhood is significant, at approximately 18 percent in 2003, but decreasing to 12 percent in 2006 and 2011. These are important predictors of spousal abuse among current partners (see, for example, Bowlus and Seitz 2006; Casique 2006). Finally, households are relatively large, with about 5.7 members on average, a statistic usually correlated with low socioeconomic status in Mexico.

4. Empirical Methodology

4.1. Estimation

To obtain robust estimates of the relationship between the incidence of spousal abuse and Oportunidades beneficiary status, we estimate ordinary least squares (OLS) models conditioning on a large set of predetermined individual and household socioeconomic characteristics as well as village fixed effects, so as to capture any village-specific unobserved heterogeneity influencing spousal abuse patterns (for example, access to health clinics, community groups, village-level conditions affecting partners' socioeconomic conditions and economic opportunities). The regression equation for outcome Y_{iv} is the following:

$$(1) Y_{iv} = \theta T_{iv} + X_{iv}\beta + \alpha_v + \varepsilon_{iv},$$

where the treatment indicator T_{iv} equals one for beneficiary household i in village v and is zero otherwise; X_{iv} are the predetermined covariates that are possibly significantly correlated with T_{iv} and Y_{iv} ; α_v are village fixed effects, and ε_{iv} are unobserved determinants of domestic violence. We cluster standard errors at the village level.

4.2. Dealing with Endogenous Selection into the Treatment

As discussed in more detail in Bobonis, González-Brenes, and Castro (2013), various potential reasons for endogenous program take-up—and thus within-village household-level unobserved heterogeneity—may be: (i) the targeting mechanism, which tries to ensure that low socioeconomic status households are the actual program beneficiaries (Skoufias, Davis, and de la Vega 2001); (ii) the possibility that program take-up decisions may be endogenous, based on the extent of women’s decision-making power within the household; (iii) that beneficiary couples may be more likely to dissolve (e.g., divorce) because of the potentially greater likelihood of conflict and the improvement in women’s socioeconomic conditions outside of current relationships, leading to a sample of households remaining in union; and finally, that (iv) the program may lead to changes in marital matching and sorting patterns owing to the expected changes in household resources and intra-household dynamics (especially for young individuals). As a result of these potential selection and endogeneity problems, simple means comparisons of spousal abuse outcomes between beneficiary and nonbeneficiary households would violate the assumptions of unconditional independence necessary for identification of the program average treatment effect (Rubin 1974).

The replication analysis uses various strategies to try to minimize the extent of bias in the estimates. First, as mentioned in Section 3, the study uses a subsample of households with children ages between 0 and 10 in 2003 (ages between three and 13 in 2006, and ages between eight and 18 in 2011), and households whose demographic compositions make them likely to—at least initially—fully take up the program if eligible, thus minimizing concerns of endogenous program take-up. Second, the analysis is conditioned on a set of predetermined individual and household socioeconomic characteristics that are strongly correlated with determinants of program eligibility and likely to capture a large component of the variation determining program take-up. Finally, the sample is restricted to women ages 25 and older in 2003 (28 and older in 2006, 33 and older in 2011).

The sample restrictions are insufficient to construct comparable groups of beneficiary and nonbeneficiary households for the empirical analysis. The comparison of individual and household predetermined covariates documents this potential selection bias: beneficiary women are more likely to be with an indigenous partner and to be indigenous themselves; both they and their partners have significantly lower school attainment levels than non-beneficiaries (Table 4, columns 3, 7, and 11). These patterns tend to hold in the samples across the three survey waves. This is not surprising: Oportunidades is targeted at poor households in marginalized communities. To address the targeting of the program to these poor communities, we make comparisons of beneficiary and nonbeneficiary households within villages so as to remove all selection based on the village-level targeting of the program. This within-village comparison dramatically reduces the observed selection into the program. A within-village means comparison of the same predetermined characteristics among these groups of households shows drastic reduction in—although not a complete elimination of—the observed predetermined observable differences in characteristics (Table 4, columns 4, 8, and 12). We additionally employ statistical methods to reduce the extent of confoundedness of the correlation between the spousal violence outcomes and households' beneficiary status, and to ensure comparability with the original study.

To address possible concerns about unobserved heterogeneity in the within-village household comparison, the study pursues a set of tests and sensitivity analyses inspired by the work on diagnostics of selection on observable and unobservable variables (Altonji, Elder, and Taber 2005; Imbens 2003; 2004). Essentially, the study identifies the observable characteristics (X_{iv}) correlated with treatment assignment (T_{iv})—the woman's age, partner's age, partner's schooling, family size, and years in union—are also significant predictors of spousal abuse outcomes, as these may plausibly be the covariates most correlated with the unobservable characteristics that jointly determine program eligibility/take-up and abuse outcomes. For those identified variables, the study evaluates the robustness of the results to flexible specifications

that allow for high-order and interaction terms between these variables and also include interactions with the woman's level of education. The results obtained from this sensitivity analysis are qualitatively and quantitatively similar across specifications.

Finally, estimates are presented from empirical models that additionally condition on households' asset-holding patterns and access to infrastructure. There is a trade-off in doing so: on one hand, these controls may reduce concerns of unobserved heterogeneity due to households' varying wealth levels which may influence, for instance, the opportunity costs of partners to engage in spousal abuse or the likelihood of separation. On the other hand, because these variables are measured at the time of the survey, they are potentially endogenous regressors; householders may use program-related cash benefits to make home improvements or purchase assets (Gertler, Martinez, and Rubio-Codina 2012). The results are robust to the inclusion and exclusion of these additional control variables.

5. Results

5.1. Graphical Evidence

The discussion begins with a graphical analysis of the patterns in the data. Figure 2 shows the trends in physical violence among couples in the sample across the three survey years. The incidence of physical abuse among women in nonbeneficiary households is quite high at 12.6 percent in 2003, and shows a downward trend over time to 9.9 percent in 2006 and to 7.9 percent in 2011 (significant at the 90 percent confidence level). In comparison, the incidence among beneficiary couples is 8.9 percent, lower than that among couples in nonbeneficiary households in 2003 (3.9 percentage points, not significant). However, the incidence among beneficiary couples hovers around 10.0 percent in 2006 and 7.9 percent in 2011, such that physical violence rates between these two groups of households converge in the longer term. A similar though less stark pattern of short-term differences (in 2003) and later convergence (in

2006, 2011) is observed in the incidence of sexual abuse, emotional abuse, and threats of violence.¹⁰

5.2. Overall Relationship between Oportunidades Beneficiary Status and Physical, Sexual, and Emotional Violence

Estimates of the overall five-year (2003), nine-year (2006), and 13-year (2013) relationship between program beneficiary status and spousal violence and threats outcomes are displayed in Table 5. Odd-numbered columns report the estimates based on a specification in which control variables enter linearly. Even-numbered columns report a specification that includes (i) interaction terms between the female's educational attainment and the partner's age, the partner's schooling attainment, household size, and the couple's years in union; (ii) polynomial terms for each partner's age, the partner's education, household size, and years in union; and (iii) the additional interaction of the higher-order terms. Since the latter model is more likely to reduce or eliminate potential biases, it is the preferred specification for the present study.

For purposes of comparison, the discussion starts with the preferred estimates of the short-run relationship. As documented in Bobonis, González-Brenes, and Castro (2013), domestic violence incidence rates, physical and sexual abuse in particular are lower among beneficiary couples than among nonbeneficiary ones in the short run (columns 12). The estimated difference in the incidence of physical or sexual abuse is 9.6 percentage points (53 percent) in the more parsimonious specification and 8.2 percentage points (45 percent) in the preferred specification. We also find independent reductions in the incidence of physical and of sexual violence (generally significant at 90 percent confidence). The estimates on both physical and sexual violence are larger in absolute terms than the comparison of raw differences (see Figure 2) and the cross-village OLS estimates (not reported). In contrast, domestic violence

¹⁰ Not shown in the figures for the sake of conciseness. Appendix Table A2 reports these means and tests of differences by survey round, overall and by household beneficiary status.

incidence rates do not vary significantly across beneficiary and nonbeneficiary households in 2006 and 2011 (columns 34) (the reduction in the magnitude of the relationship is significant at 95 percent confidence). Our preferred estimates for 2011 show a statistically insignificant difference in the incidence of physical violence of 0.4 percentage points and no relationship with sexual violence (column 6). We also find no evidence of a significant difference in the incidence of violent threats or acts of emotional violence among beneficiary women (Table 5, rows 45).

5.3. Relationship between Program Beneficiary Status and Substitution between Emotional and Physical/Sexual Violence

This subsection investigates whether there is a *sustained* degree of substitution between threats of violence or emotional violence on one hand, and physical/sexual abuse on the other. This follows our argument that an increase in women's socioeconomic opportunities can generate a greater incentive for a male partner to use emotional violence or threats of physical violence to extract rents or (re)gain control over his female partner's resources or decision-making (see Bobonis, González-Brenes, and Castro 2013 for details). To implement these tests we use two sets of additional violence measures: (i) indicator variables for the incidence of threats of spousal abuse and no incidence of physical (or sexual) violence; and (ii) indicator variables for the incidence of emotional violence and no incidence of physical (or sexual) violence.

The discussion begins, once more, with a graphical analysis. Figure 3 presents the trend in the incidence of threats of violence with no associated physical abuse among couples in the sample across the three survey waves. The incidence of threats of abuse with no associated physical abuse among beneficiary households is moderate, at 3.9 percent, in 2003, and shows a downward trend over time to 1.5 percent in 2006 and to 0.7 percent in 2011. In comparison, the incidence of this form of abuse among nonbeneficiary couples is only 2.7 percent, lower than that among beneficiary couples in 2003 (1.2 percentage points, not significant). The incidence among nonbeneficiary couples decreases to 1.5 percent in 2006 and 0.5 percent in

2011. Substitution between these forms of abuse between these two groups of households converges in the longer term. Similar patterns of short-term differences and longer-term convergence are observed when alternate measures of substitution (not shown in the figures) are used.

The estimates from parametric regressions confirm the graphical evidence (Table 6). In contrast to the significant short-run estimates (column 1, joint significance p-value = 0.025), we observe a decreasing magnitude of a longer-term relationship in the incidence of violent threats or acts of emotional violence where acts of physical or sexual abuse are absent. The individually estimated coefficient estimates on threats of violence and emotional abuse are positive but imprecisely estimated in 2006, though these are jointly significant at the 5 percent level (p-value = 0.039). Moreover, emotional violence conditional on no physical violence is higher for beneficiary households by 2.4 percentage points (70 percent) in 2006 (column 4, row 4). By 2011, the estimates of the relationship are substantially smaller in magnitude and indistinguishable from zero (columns 56) (joint significance p-value = 0.773).

In conclusion, the estimates on the longer-term relationships provide evidence of a reduction in the correlation between the Oportunidades program and the incidence of physical and sexual abuse among beneficiary women, as well as a *decreasing* degree of substitution between the incidence of violent threats or acts of emotional violence and acts of physical or sexual abuse. These are in significant contrast with the estimates of the short-term relationship.

6. Discussion of Main Results, Possible Explanations, and Robustness Tests

6.1. Repeated Interactions and Marital Selection

The stark differences in the longitudinal pattern of the relationship suggest that the models of violence and household bargaining, in which male partners may use violence as instruments of coercion (Anderson and Genicot 2014, Bloch and Rao 2002, Bobonis, González-Brenes, and Castro 2013) may correctly capture interactions within the household in the short term, but may

poorly capture those over the longer term. As mentioned earlier, in this class of models male partners are heterogeneous in their willingness to engage in violence and have private information regarding the 'gains to marriage,' such as their own private income or their status within the household based on traditional gender roles. This private information enables violent partners to use threats of abuse to coerce transfers from their wives, and to strategically use physical violence if their wives do not comply. However, once male partners reveal their willingness to use violence such couples may dissolve, effectively increasing the proportion of nonviolent types. Abuse rates may thus tend to decrease among couples remaining in union over time, and the relationship between abuse rates and program receipt status may tend to weaken.

The study provides evidence partially consistent with this interpretation. First, it is documented that both beneficiary and nonbeneficiary couples experienced a modest increase in marital dissolution rates during this time period. Retrospective data on marital histories from the 2006 and 2011 surveys are used to construct marital dissolution rates for couples in union as of 1998, the start of the program (see Figure 4). As of 2003, 3.2 percent of couples had separated or divorced, and the divorce rate rose to 5.0 percent by the year 2006, an increase of 1.8 percentage points. That said, there is no evidence of differential divorce rates among beneficiary women couples than among nonbeneficiary ones, at least in the 2003-2006 period.¹¹ A similar pattern emerges when using the retrospective data for survey year 2011. In this case, 4.5 percent of couples dissolved by 2006 and this rate increased to 7.9 percent as of 2011; again there is no evidence of a differential response among couples in beneficiary households. If women in relationships with a violent partner were disproportionately likely to dissolve (as documented in the Canadian case by Bowlus and Seitz 2006), this increase in divorce-based

¹¹ The short-term effects (for 1999) are consistent with the evidence shown by Bobonis (2011) using the short-term experimental evaluation of the Progresa program. He also finds that the household marital dissolution effects are concentrated among young and relatively educated women.

selection could help explain the drop in the incidence of violence among these cohorts of women.

Second, a comparison of socioeconomic and demographic characteristics of households across the three survey waves suggests that there are important changes in their distributions (see Tables 2 and 3). Women are more likely to report speaking an indigenous language across survey waves, which is correlated with low socioeconomic status in Mexico; they tend to have lower educational attainment levels. They also report a higher prevalence of violence in their households during childhood. Moreover, their male partners are more likely to be indigenous themselves. However, they report a lower prevalence of violence in their male partners' households during childhood, decreasing from 18 percent in 2003 to 12 percent in 2006 and 2011. Given the strong correlation in the intergenerational transmission of violent behavior, the decrease in this statistic may be informative of the substantial drop in the incidence of violence observed in the sample across survey waves.¹² In summary, in spite of our construction of a pseudo-panel based on the woman's age and the age of her children, important changes in the distribution of predetermined characteristics remain.¹³

Third, among couples formed following the start of the program, levels of violence tend to be lower than among couples formed before the start of the program, consistent with the view that abuse in new couples may be lower (Table 7). To evaluate this, we divide the samples in two groups: couples who have been in union since 1997 or earlier—who made their current marital choices before the start of the program—and those in union since 1998 or later.

¹² A number of housing characteristics (for example, housing with firm floor, drainage, number of rooms) improve across survey years. In addition to the average differences we observe between the samples, we also see relevant changes in the distribution of these characteristics across program beneficiaries and non-beneficiaries. Comparing the differences in means between the samples (Table 4, columns 3, 7, 11), we can see that the gap in woman's age between beneficiaries and non-beneficiaries increased from 0.27 to 1.5 years in 2006, and down to 0.73 years in 2011, the gap in the proportion of indigenous women increased by 4 percentage points (from 13 to 17 percent), and the difference in the proportion of women who completed secondary schooling increased from 4 percent to 9 percent; that is, with beneficiary women in the sample being disproportionately less educated than nonbeneficiary ones in 2006 and 2011. Additionally, the gap in the proportion of women with indigenous partners also increased, from 11 percent to 16 percent; the gap in partner's schooling increased from 1.35 to 2.42 years; and finally, the gap in the proportion of partners who witnessed spousal violence narrowed from 6 percent to 2 percent.

¹³ See Casique and Castro (2014) for a rich analysis of changes in household socioeconomic characteristics and patterns of intimate partner violence across the three survey waves.

Although the sample is not sufficiently large to make definitive statements regarding these differences, a tendency for physical and sexual violence levels to be lower among more recently formed couples (rows 13) is observed. Moreover, there is also a tendency across survey rounds for threats of violence to decrease among more recently formed couples relative to older ones, although this pattern does not hold for the measure of emotional violence (rows 45).

To the extent that marital selection dynamics may drive changes in the composition of beneficiary households, these sample selection and treatment effect heterogeneity patterns could help explain the time path of spousal violence among beneficiary couples. However, our explanation is not the only potential source of selection of households in our pseudo-panel. For instance, selective out-migration from rural areas—consistent with evidence suggesting that migration toward urban areas among program beneficiaries has been more common for those households with higher educational attainment (Azuara 2009)—could also help explain the changes in the sample distribution of these couples.

Consideration is given to the possibility that these contrasting results reflect observable differences in sample composition that are not necessarily strongly correlated with male partners' willingness to engage in violent behavior against their spouses. To account for these observable sample differences across survey waves, the reweighting method introduced in the decomposition literature by DiNardo, Fortin, and Lemieux (1996) is used. This method enables examination of the extent to which the estimated relationship is muted by the difference in the samples' composition.

First, the 2006 and 2011 samples are reweighted to resemble the 2003 sample distribution in terms of observable characteristics. The predicted probabilities, or propensity scores, are estimated separately for each of the 2006 and 2011 survey waves and by

beneficiary status, via a flexible logit regression on the observable (X_{iv}) variables.¹⁴ Second, reweighting factors are estimated:

$$(2) \psi(X_{iv}) = [\Pr(S_{iv}\{2003\}=1 | X_{iv}) / \Pr(S_{iv}\{2003\}=1)] / [\Pr(S_{iv}\{2003\}=0 | X_{iv}) / \Pr(S_{iv}\{2003\}=0)]$$

where $S_{iv}\{2003\}$ indicates whether the observation belongs to the 2003 sample, and X_{iv} is the vector of covariates. The reweighting factor incorporates the sample proportions to adjust for the fact that the number of observations is different across samples. In the final step, the counterfactual relationship on the appropriately reweighted sample is estimated.¹⁵ Bootstrapped standard errors are generated via a bootstrap of the whole estimation procedure (both the estimation of the logit model to construct the weights and the computation of the conditional mean differences).

The distribution of the estimated propensity scores across the three samples reveals that these are, overall, balanced in household observable characteristics (Appendix Figure A1). As a summary statistic, the mean of the estimated propensity scores in the 2003 and 2006 samples are 0.41 and 0.36 respectively, and 0.37 and 0.31 for the estimates of the 2003 and 2011 samples. Moreover, the reweighting methodology achieves almost perfect balance in the covariates. Appendix Table A3 shows the means of the control variables in 2003 and those of the reweighted 2006 and 2011 samples. So that the pseudo-panel would be as close as possible to that studying 2003, time-dependent variables (age, partner's age, and years in union) were normalized so as to be measures according to the 2003 survey year when estimating the propensity scores. After the adjustment of the sampling weights, these are the only control variables that significantly differ between the original and subsequent samples. In

¹⁴ The DFL reweighting method is analogous to the propensity score reweighting method commonly used in the program evaluation literature (see Hirano, Imbens, and Ridder, 2003), except that the reweighting is done over observations in the same treatment group across time periods.

¹⁵ The procedure has advantages and disadvantages. In addition to its simplicity, the procedure is more robust than standard Oaxaca-Blinder decomposition methods based on a linear regression model when the underlying conditional expectation of Y given X and T is non-linear. Its main undesirable property is that reweighting estimators can perform poorly in small samples when there is a problem of common support. See Fortin, Lemieux, and Firpo (2010) for a detailed discussion of the procedure and its properties.

summary, this weighing scheme helps to account for the fact that some of the couples in the 2003 sample are underrepresented in the subsequent pseudo-panels; it also helps in judging whether the effect of Oportunidades is still present but only in the subset of women that are most like those in union in 2003, or whether the effect appears to dissipate over time for all women.

The main results from the reweighted regression models are reported in Table 8. Although most of the results remain statistically insignificant as in the benchmark models, larger coefficients are seen for the 2006 sample, where beneficiary status is related to an increased incidence of physical or sexual violence. In 2011 the results do not differ significantly from the unweighted analysis, again indicating that observable differences in sample composition do not drive the results.

This analysis confirms the main findings that beneficiary women are as likely to be victims of abuse as nonbeneficiary ones in the longer term, once observable differences in sample composition are taken into account. It also suggests that if marital selection patterns can help explain the results, these are due to sample selection and treatment effect heterogeneity based on characteristics that are directly unobservable to the researcher (such as the male partner's willingness to engage in violent behavior against his spouse). Suggestive of this is that the incidence of physical abuse is significantly higher among nonbeneficiary couples in which the partner was exposed to violence during childhood (15.5 percentage points in 2006, 3.7 percentage points in 2011; not reported in the tables). Given this strong correlation in the intergenerational transmission of violent behavior, the decrease in this statistic may be informative of the substantial drop in the incidence of violence observed in the sample across survey waves.

6.2. Increasing Rejection of IPV

Recent research has documented a rapid global diffusion of norms regarding the unacceptability of spousal violence across a broad set of countries. Specifically, Pierotti (2013) uses nationally representative, repeated cross-sectional data from Demographic and Health Surveys (DHS) across a broad set of low- and middle-income countries to document that women of reproductive age have increasingly rejected the justification of violence from intimate partners. She argues that new global cultural scripts rejecting violence against women—via international and national policies and discussions starting in the mid/late 1990s—may then be reflected in modifications of individual attitudes toward IPV across a large spectrum of societies. These new global scripts and norms may have also diffused across Mexican society so as to decrease women's tolerance for IPV.¹⁶

To evaluate this hypothesis, the study uses additional information available in the ENDIREH data. Following the analysis in Pierotti (2013), an indicator variable is constructed to measures whether the woman believes a man is justified in hitting or beating his female partner when she does not meet her responsibilities.¹⁷ These measures are imperfectly comparable to those from existing DHS data.¹⁸ Figure 5 shows the trend in this measure among couples in the sample across the three survey years, by beneficiary status. Consistent with the cross-country evidence, the proportion of women reporting some justification of IPV shows a sharp reduction

¹⁶ Suggestive of this phenomenon in the Mexican context is the passage of laws promoting gender equality and establishing the right of women to live free of violence in 2006 and 2007, respectively. Reports in the 2011 ENDIREH survey that 73 percent of women are knowledgeable of the gender equality legislation and 82 percent of women report being knowledgeable of the freedom from violence legislation are consistent with a strong dissemination of these scripts as embodied in national policy.

¹⁷ The 2003 and 2006 survey rounds ask the same question: “*En su opinión, cuando la mujer no cumple con sus obligaciones, ¿el marido tiene el derecho de pegarle?*” [In your opinion, when a woman does not meet her responsibilities, does the partner have the right to hit her?]. In contrast, the question in the 2011 survey round is modified: “*¿El hombre tiene el derecho de pegarle a su esposa?*” [“Does a man have the right to hit his partner?”] Therefore, the responses in the 2011 survey round are not strictly comparable to those in earlier rounds. These are reported in order to show a more complete picture, subject to this caveat.

¹⁸ Specifically, Pierotti (2013) constructs outcome variables derived from questions that asked respondents whether it is okay for a man to hit or beat his wife under certain circumstances. Specifically, the most common form of the question asked, “Sometimes a husband is annoyed or angered by things which his wife does. In your opinion, is a husband justified in hitting or beating his wife in the following situations?” The five scenarios presented to respondents were (1) if she goes out without telling him, (2) if she neglects the children, (3) if she argues with him, (4) if she refuses to have sex with him, and (5) if she burns the food (Pierotti, 2013; p. 248).

over this time period: from 20 percent in the year 2003 to 9.3 percent in 2006 and to 3.3 percent in the year 2011 (not reported in the figure). This stark change in the justification of IPV occurs among women in both beneficiary and nonbeneficiary households. The study estimates a decrease of 12.4 percentage points (54 percent; significant at 95 percent confidence)—from 22.8 percent in 2003 to 10.4 percent in 2006—among women in beneficiary households, and a similar change of 9.9 percentage points (58 percent; significant at the 95 percent confidence level) among those in nonbeneficiary households. Further proportional decreases of proportional size are estimated between the 2006 and 2011 survey rounds. These patterns also hold for a more comprehensive sample of the rural population in Mexico, which suggests that the forces driving these patterns are present across Mexican society.

The study also documents that the incidence of IPV sharply decreases over time among couples in nonbeneficiary households, and that this pattern is independent of the sample selection criteria used (see Table 9, columns 2 and 5). In the pseudo-panel, the incidence of physical violence decreases from approximately 13 percent in 2003, to 10.2 percent in 2006 and 8.3 percent in 2011 (row 4). A similar picture emerges using less restrictive sample restrictions: all women currently in a relationship in rural households (row 1); among this group, those in a relationship since 2003 (row 2); and among the second group, those ages 25 and older in 2003 (row 3). The incidence of physical violence for these households lies in the 7.99.0 percent range in 2006 and in the 6.47.0 percent range in 2011, a significant drop from the incidence documented in the earlier period. Second, the null relationship between the program beneficiary status and the incidence of physical violence is stable across the same sample specifications (columns 1 and 4).¹⁹ That these patterns hold for a more comprehensive sample of the rural population in Mexico suggests that the forces driving our findings are not just a function of the pseudo-panel structure of the preferred sample, but are persistent across Mexican society.

¹⁹ We find analogous patterns for all other indicators of abuse. These are omitted for the sake of brevity.

6.3. Alternate Explanations

This subsection evaluates other explanations that may be consistent with the evidence: localized spillover effects among nonbeneficiary couples, changes in the de facto conditionality of the cash transfer program, and generalized social violence mediating effects on spousal abuse, among others.

6.3.1 Localized Spillover Effects

A first alternate explanation is that the program empowers women in the community and provides them with the means to prevent spousal abuse, directly via interactions with beneficiary women with higher levels of empowerment in the community; indirectly via improved socioeconomic conditions and options outside of current relationships; or changes in the norms of intolerance of abuse, among others.²⁰ Therefore, to the extent that these spillover effects reduce the incidence of abuse among nonbeneficiary women and increase female partners' intolerance of abuse, they can help explain the stylized facts shown earlier.

We evaluate this alternate explanation empirically by estimating empirical models that capture spillover effects at the level of the village. Specifically, we estimate a variant of our main empirical model (1). The regression equation incorporating these effects is the following:

$$(3) Y_{ivm} = \theta_1 T_{ivm} + \beta_1 E[T_{-i,v,m}] + X_{ivm} \beta_2 + \alpha_m + \varepsilon_{ivm},$$

where the treatment indicator T_{ivm} equals 1 for beneficiary household i in village v , municipality m and is 0 otherwise; $E[T_{-i,v,m}]$ represents the proportion of beneficiary households in the sample in village v (excluding household i). This specification incorporates the possibility that local spillovers are a (linear) function of the proportion of beneficiary households in the village (e.g.,

²⁰ As shown by Angelucci and De Giorgi (2009) and Avitabile (2012), the program had spillover effects on the consumption levels and health behaviors (e.g., cervical cancer checks) of nonbeneficiary households. Bobonis and Finan (2009), Lalive and Cattaneo (2009) show evidence of spillover effects on middle-school participation among children in nonbeneficiary households.

Miguel and Kremer 2004). These potential effects are captured by the β_1 term. We also estimate additional specifications that allow for heterogeneous spillover effects between beneficiary and nonbeneficiary households by including an interaction term between T_{ivm} and $E[T_{-i,v,m}]$. Because the $E[T_{-i,v,m}]$ term is highly collinear with village fixed effects, we substitute these for municipality fixed effects in these specifications. We cluster standard errors at the village level.

We report estimates of these models for physical abuse (Table 10, columns 12); Panel A reports estimates for the 2006 data, whereas Panel B reports analogous ones for the 2011 data. Estimates for other outcome variables are comparable to these (not reported for the sake of brevity). The estimates imply that a 10 percentage point increase in the proportion of beneficiary women leads to a statistically insignificant 0.21 percentage point increase in the incidence of physical abuse in 2006, and an 0.01 percentage point decrease in its incidence in 2011 (column 1). In the specification allowing for heterogeneous spillover effects by beneficiary status, the point estimates imply a statistically insignificant 0.80 percentage point increase in the incidence of physical abuse among nonbeneficiary couples in 2006, and an 0.12 percentage point decrease in its incidence in 2011 (column 2).

Spillover effects for beneficiary couples are more precisely estimated zeros: the point estimates imply a 0.24 percentage point reduction in physical violence in 2006, and a 0.07 percentage point increase in 2011 (all statistically insignificant). Finally, it is worth noting that issues of unobserved heterogeneity generally cause upward bias (in absolute magnitude) in the estimates of the spillover effects; such that these can be considered overestimates of the true spillover or social interaction effects. We conclude that this alternative mechanism cannot explain the results.

6.3.2. Conditionality of Cash Transfers

A second alternate explanation is changes in the de facto conditionality of the cash transfers. In Bobonis, González-Brenes, and Castro (2013), the sample was restricted to intact households

with children ages 11 years and younger at baseline, that is, children who were not old enough to attend secondary school. The reasoning was that because school participation rates are close to 100 percent for children in primary schools among the population of interest (and there are no program impacts in primary school participation) (Behrman, Sengupta, and Todd 2005; Schultz 2004), we assumed that conditionality constraints are not likely to be binding for households with primary school children and that the take-up of the program is complete for these households. We argue that this should have reduced concerns of endogenous program take-up based on women's decision-making power and tolerance for violence. For purposes of the analysis of long-term relationship, the conditionality of the transfers becomes a binding constraint for a large proportion of households as children progress through grades into middle and secondary school. Estimates of the relationship would be incomparable across survey waves if there were endogenous take-up of the program among households in the pseudo-panel in the 2006 and 2011 waves.²¹

This possible concern is addressed by estimating the relationships for households with the same characteristics in subsequent samples as in the baseline sample—women 25 years or older with children 11 years or younger for both the 2006 and 2011 surveys—as opposed to the pseudo-panels for whom evidence has been shown above. Although the pseudo-panel approach tries to maximize the overlap of women across the samples, this alternative approach will include many new women and will exclude others that no longer meet the selection criteria. Again, only estimates of these models for physical abuse are reported, for the sake of brevity (Table 10, column 3). The analysis of this sample of households yields similar results. No significant differences are found in the incidence of physical abuse between beneficiary and nonbeneficiary households. This analysis makes unlikely the claim that conditionality and aging of the couples could significantly explain the results.

²¹ In addition, program impacts on spousal violence could be stronger among younger couples. Since partners in the pseudo-panel age across survey waves, this could help explain the results.

6.3.3. Generalized Social Violence

Another potential alternate explanation considered is changes in the incidence of abuse or in its reporting due to the marked increase in social violence. As is well known, Mexico has experienced a surge in homicide rates since 2007, concentrated in particular regions of the country. Many analysts attribute this drastic change in the level of violence to consequences of the federal government's anti-crime policies meant to combat drug cartels (Astorga and Shirk 2010; Dell 2015).

The study examines the possible effect of this surge in generalized social violence across municipalities and or states on trends in spousal abuse and the relationship with program beneficiary status. To the extent that the surge in homicides can impinge on partners' stress levels or, more broadly, on their emotional health, it could lead to greater conflict-related abuse. However, if this generalized conflict discourages women from reporting events of abuse, this would be consistent with the significant drop in reported abuse rates in 2011, although not in 2006. These are two potential mechanisms for a mediating effect, among others.

Empirical models that capture these mediating factors are estimated at the level of the municipality or state. The regression equation incorporating these factors is the following:

$$(4) Y_{ivm} = \theta_1 T_{ivm} + \theta_2 T_{ivm} H_{m(s)} + \beta_1 H_{m(s)} + X_{ivm} \beta_2 + \varepsilon_{ivm}.$$

The $H_{m(s)}$ variable measures the homicide rate per hundred thousand individuals in municipality m (or alternatively, state s); the other variables are defined as above.²² The homicides measures are included for the calendar year preceding the household survey (2005 and 2010, respectively), because the surveys are conducted over a long time period and we aim to ensure

²² The homicide data are available from Mexico's National Statistics and Geography Institute (INEGI). We follow the standard specifications in the literature and estimate the relationship between violence and individual/household outcomes with measures of violence at the municipality level (Camacho 2008; León 2012). Empirical models of this sort find strong relationships with adult labor force participation (BenYishay and Pearlman 2013) and student achievement (Michaelsen and Salardi 2015) in Mexico.

that the timing of measured homicides predates that of abuse outcomes.²³ The β_1 coefficient captures the partial correlation between homicides and spousal abuse rates among nonbeneficiary couples, whereas the θ_2 term captures the differential correlation among beneficiary couples. In the main specification, because the homicide rate is measured at the municipality level, village fixed effects are not included. In a second specification with village fixed effects, the differential mediating effect for beneficiary couples can be identified.

Estimates of these models for physical abuse (Table 10, columns 4-6) are reported, for the sake of brevity. The estimate for 2006 implies that a one standard deviation increase in the municipality-level homicide rate (10.16 deaths per 100,000 individuals) increases spousal physical abuse among nonbeneficiary couples by 0.56 percentage points and decreases its incidence by 0.66 percentage points among beneficiary ones (Panel A, column 4). Neither of these estimates is statistically distinguishable from zero. Analogous estimates for survey year 2011 are an order of magnitude smaller: a reduction of 0.04 percentage points and an increase of 0.05 percentage points, respectively (Panel B). Estimates of the differential effect for beneficiary couples in the within-village specification imply a 0.27 percentage point increase in 2006, and a 1.06 percentage point increase in 2011; neither is statistically significant (column 5). Finally, analogous estimates using the state-level homicide rate measure imply differential effects for beneficiary couples of -0.13 percentage points in 2006, and 3.2 percentage points in 2011 (column 6). The point estimate for 2011 implies that recent violence can explain a substantial closure in the protective relationship of Oportunidades for intra-household violence; however, all estimates are statistically indistinguishable from zero. Therefore, although some evidence for the latter period is suggestive of this mechanism, the analysis does not generally support the idea that social violence has led to a decrease in intra-household violence.

²³ The results are robust to using contemporaneous year measures (2006 and 2011, respectively). These are also robust to the use of gender-specific homicide rates, in spite of the different trends in the gender of the victims, shown in Valdivia and Castro (2013). Estimates are available upon request.

6.3.4. Improvement in Women's Labor Market Opportunities

An extensive literature documents that increases in a woman's relative wage, by increasing her bargaining power within the household as a result of an improvement in her outside options, can lead to lower levels of violence (Aizer 2010; Bowlus and Seitz 2006). An improvement in women's relative income-generating opportunities in Mexico over the past decade may help explain the strong decline in the incidence of violence.

To evaluate this hypothesis, models analogous to that of Aizer (2010) are estimated to capture the mediating effects of the gender wage gap at the state level. The regression equation incorporating these effects is the following:

$$(5) Y_{ivs} = \theta_1 T_{ivs} + \theta_2 T_{ivs} W_s + \beta_1 W_s + X_{ivs} \beta_2 + \varepsilon_{ivs}.$$

The W_s variable measures the female/male wage ratio in state s relative to the average gender wage gap for the sample of women and men in our study; the other variables are defined as above. The β_1 coefficient captures the partial correlation between homicides the female/male wage ratio among nonbeneficiary couples, whereas the θ_2 term captures the differential correlation among beneficiary ones. The state-level rural wage gap measure is used because the surveys are representative at the state level; thus the lowest level of aggregation at which these measures can be consistently estimated is at this level.²⁴ Moreover, because the female/male wage ratio rate is measured at the state level, village or state fixed effects are not included in this specification. In a second specification with state fixed effects, the differential mediating effect for beneficiary couples can be identified.

Estimates of these models for physical abuse (Table 10, columns 78) are reported, for the sake of brevity.²⁵ The estimate for 2006 implies that a one standard deviation increase in the

²⁴ Using a state-level female wage gap measure may be somewhat restrictive for purposes of the analysis, because it may not appropriately capture the relative labor market opportunities women face across distinct municipalities and villages within the state. However, it should capture broad differences at the state level in these relative labor market opportunities.

²⁵ We find similar qualitative results for emotional violence and threats of abuse, although the estimates are less precisely estimated; these are available upon request.

female/male wage ratio ($= 0.122$) decreases spousal physical abuse by 1.40 percentage points (significant at the 10 percent level; Panel A, column 7). This suggests that a closure of the gender wage gap can explain a significant portion of the decrease in physical violence as of 2006. However, the relationship for 2011 implies that an analogous increase in the later period ($= 0.101$) has no relationship with the rates of physical abuse (Panel B, column 7). Moreover, estimates from a model that allows for a heterogeneous response by couples' beneficiary status implies that the reduction in physical abuse is strictly concentrated among beneficiary households. The estimate implies a 2.3 percentage point reduction in physical abuse per standard deviation increase in the gender wage ratio among beneficiary households, and no effect among nonbeneficiary households (Panel A, column 7). These heterogeneous effects would imply a greater reduction in physical abuse rates among beneficiary households in a context where the gender wage gap was narrowing.

7. Conclusions

The main objective of this paper is to provide evidence of the longer-term relationship between the Oportunidades CCT program and the prevalence of male-to-female spousal violence in rural Mexico. It addresses a concern raised in recent work challenging the consensus that targeting resources to women in the forms of CCTs may help promote the empowerment of women within the household (Angelucci 2008; Bobonis, González-Brenes, and Castro 2013; Rivera, Hernández, and Castro 2006).

The evidence suggests that, in the longer term, women in beneficiary households are as likely as nonbeneficiary women to experience physical or nonphysical abuse. Specifically, the present study finds that a decade after the start of the program, physical and emotional abuse rates do not vary significantly among existing beneficiary and nonbeneficiary couples. These findings stand in stark contrast to the short-run relationship established in observational and experimental studies—that women in beneficiary households are significantly less likely to be

victims of physical abuse than are nonbeneficiary women (Angelucci 2008; Bobonis, González-Brenes, and Castro 2013; Haushofer and Shapiro 2013; Hidrobo and Fernald 2013; Hidrobo, Peterman, and Heise 2015; Perova 2010).

To try to understand the mechanisms underlying these diverging relationships, the study evaluates whether marital selection—the types of couples remaining in a marital relationship as a result of the program—can play an important role. In particular, evidence suggests that reported levels of emotional violence among beneficiary couples formed after the start of the program are lower than among nonbeneficiary couples, consistent with the argument that those couples more likely to suffer emotional abuse may dissolve, and abuse may be lower in new couples. Finally, the study evaluates whether the increasing rejection of IPV by women in Mexico over the past decade can also help explain these trends. That these patterns hold for a more comprehensive sample of the rural population in Mexico indicates that the forces driving these findings are persistent across Mexican society, consistent with the diffusion of norms regarding the unacceptability of spousal violence. Moreover, because the Prospera program is an instrumental component of discourse and policy changes at the federal government level regarding the empowerment of women, it is possible that the program may have contributed to the decline in observed levels of IPV. However, the authors' ability to test this hypothesis is limited and is outside the scope of this paper.

The present study may have important implications for policy: it provides a mixed view of CCT programs' effectiveness in improving women's empowerment within the household. The program may, in the short term, increase the likelihood of violent threats, which may in turn compromise women's emotional health and other aspects of their wellbeing. In contrast, we can state with some confidence that the program has no longer-term negative consequences in the livelihoods of women, at least in the form of higher levels of spousal abuse. Evaluating the robustness of this finding using experimental methodologies and exploring these relationships in other contexts would be extremely valuable future research.

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Appendix A: Data – Incidence of Violence Measures

The ENDIREH surveys ask respondents regarding thirty (30) potentially violent experiences in their interactions with their partners. We use seventeen (17) of these questions to define our variables of interest (listed in Table A1 below). Incidences of violence measures consist of dichotomous variables indicating whether the woman suffered physical, sexual, or emotional intimate partner violence from her spouse or partner in the past 12 months.

- (a) Physical violence includes pushing, kicking, throwing objects, hitting with hands or objects, choking, attacking with a knife or blade, and shooting.
- (b) Sexual violence includes demanding sex, forced sexual acts, and forced sexual relations.

A single incident in these categories is classified as a positive case of violence.

Emotional violence constitutes a complex set of behaviors (Strauss and Gelles 1990; Follingstad and DeHart 2000). Therefore, constructing an incidence measure of emotional violence measures is a challenging task. On one hand, we would like to use a comprehensive measure that encompasses abusive behaviors not usually captured in household surveys. On the other hand, the construction involves making value judgments as to what constitutes psychological but not physical violence. In this study:

- (c) Emotional violence includes whether the partner destroys or hides things that belong to the woman or to the household; the partner locks her up and prohibits her from leaving the house or from having visitors; has threatened her with a knife, blade, gun, or rifle; or has threatened to kill himself, her, or the children (see Table A1). We categorize the survey questions as “low” or “high” severity. The violence indicator is equal to one if (i) a woman reports experiencing at least two of the “low” severity situations, or (ii) a woman reports experiencing only one “low” severity situation, but states it happened more than once in the past year, or (iii) a woman reports at least one incident of the “high” severity situation.

Finally, although the format of the questions varies slightly across surveys waves, we aim to maintain consistent definitions throughout. In 2003, respondents were asked for each question whether they had experienced the situation within the past twelve months. If the answer was affirmative, there was a follow-up question that asked how often it had occurred. In subsequent surveys, respondents were asked how many times the situation had occurred, with a “No times” or “Did not occur” option. If the woman reported the situation occurring at least once, there was a follow-up question that asked how often it occurred in the previous year.

Appendix Table A1: Intimate Partner Violence Questions Across Survey Rounds (2003, 2006, 2011)

	2003	2006	2011
In the past twelve months, has your spouse/partner...	x		
How many times has your spouse/partner...		x	x
In the last year, this occurred...		x	
Since October of 2010, this occurred... ^a			x
Frequency responses available			
Yes/No	x		
No times / Didn't occur		x	x
One time	x	x	x
A few times	x		x
Many times	x		x
Multiple times		x	
Physical Violence			
Pushed you or pulled your hair?	x	x	x
Tied you up?	x	x	x
Kicked you?	x	x	x
Thrown an object at you?	x	x	x
Hit you with his hands or with an object?	x	x	x
Tried to choke or strangle you?	x	x	x
Attacked you with a knife or blade?	x	x	x
Shot you with a firearm?	x	x	x
Sexual Violence			
Demanded that you have sex with him, even if you don't want to?	x	x	x
Forced you to do sexual acts that you don't agree with?	x		
When you have sex, forced you to do things you don't like?		x	x
Used physical force to have sexual relations?	x	x	x
Emotional Violence			
Destroyed, thrown away, or hidden things that belong to you or to your household? ^b	x	x	x
Locked you in, forbidden you from going out or being visited? ^b	x	x	x
Threatened to leave you, hurt you, take your children away or kick you out? ^{b,c}	x	x	x
Threatened you with a deadly weapon (knife, switchblade, gun or rifle)? ^c	x	x	x
Threatened to kill you, kill himself, or kill the children? ^c	x	x	x

Notes:

a. The ENDIREH 2011 survey took place between October 3rd and November 11th, 2011.

b. These questions are categorized as "low severity."

c. These questions are used to define the "Threat of violence" variable.

Table A2: Outcome Variables - Male-to-Female Spousal Abuse and Threats of Violence, Overall and by Beneficiary Status

Variable Name	Sample Means			Differences in Sample Means	
	2003 (1)	2006 (2)	2011 (3)	2006 – 2003 (4)	2011 – 2003 (5)
Panel A: All Women					
Physical or sexual violence	0.159	0.137	0.102	-0.021 (0.019)	- 0.056*** (0.018)
Physical violence	0.107	0.099	0.074	-0.008 (0.018)	-0.033* (0.017)
Sexual violence	0.089	0.069	0.042	-0.020 (0.013)	- 0.047*** (0.013)
Threat of physical violence	0.079	0.042	0.022	- 0.037** (0.016)	- 0.057*** (0.015)
Emotional violence	0.113	0.071	0.060	- 0.042** (0.016)	- 0.053*** (0.016)
Panel B: Beneficiaries					
Physical or sexual violence	0.134	0.143	0.102	0.009 (0.021)	-0.032 (0.021)
Physical violence	0.088	0.100	0.071	0.012 (0.019)	-0.016 (0.018)
Sexual violence	0.081	0.071	0.044	-0.009 (0.014)	- 0.037*** (0.013)
Threat of physical violence	0.072	0.043	0.022	- 0.030** (0.015)	- 0.051*** (0.014)

Emotional violence	0.105	0.067	0.056	- 0.038** (0.016)	- 0.049*** (0.016)
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Panel B: Non-Beneficiaries

Physical or sexual violence	0.183	0.128	0.102	-0.055* (0.032)	- 0.080*** (0.031)
Physical violence	0.126	0.099	0.079	-0.028 (0.030)	-0.048* (0.028)
Sexual violence	0.098	0.064	0.040	-0.033 (0.024)	- 0.058*** (0.024)
Threat of physical violence	0.086	0.041	0.022	- 0.044** (0.022)	- 0.063*** (0.022)
Emotional violence	0.121	0.078	0.066	-0.042* (0.024)	- 0.054*** (0.023)

Notes: Sample means and their differences weighted by inverse sampling weights; significant differences between beneficiary and nonbeneficiary households at (*) 10 percent, (**) 5 percent, and (***) 1 percent levels. The standard errors of mean differences are clustered at the village level. The sample includes couples with women ages 25 /28 /33 and older, and with children aged 0/10 /313 /818, respectively for the 2003 /2006 /2011 surveys. N = 2867 /4705 /5800.

Appendix B: Methodology – Sample Reweighting Procedure

Figure A1: Distribution of Propensity Score Estimates, 2003/2006 and 2003/2011 Survey Waves



Notes: Distribution of propensity scores estimated on observable characteristics, including variables for woman and partner's age, woman and partner's indigenous status, women's schooling-level indicators, the partner's schooling attainment level, household size, cohabiting couple indicator, years in union, and variables measuring reported histories of spousal abuse in parental household during childhood. Top panel pools the 2003 and 2006 samples, and the bottom panel pools the 2003 and 2011 samples (see Section 4 for details).

Table A3: Descriptive Statistics – Woman, Partner and Couple Characteristics using DiNardo, Fortin, and Lemieux (1996) Weights

Variable Name	Sample Means		
	2003 (1)	2006 (2)	2011 (3)
Panel A: Female Partner Characteristics			
Woman's age	34.89	37.72	42.59
Indigenous woman	0.14	0.16	0.16
No schooling	0.08	0.09	0.09
Primary school	0.65	0.66	0.66
Middle school	0.18	0.17	0.17
Secondary school	0.04	0.03	0.03
Violence in woman's childhood	0.10	0.09	0.10
Panel B: Partner and Couple Characteristics			
Partner's age	37.73	40.88	45.72
Indigenous partner	0.14	0.16	0.16
Partner's schooling attainment	5.70	5.59	5.53
Violence in partner's childhood	0.18	0.16	0.17
Cohabiting couple	0.19	0.19	0.19
Family size	5.82	5.91	5.98
Years in union	15.17	18.19	23.09
Observations	2867	4705	5800

Notes: Sample means weighted using DiNardo, Fortin, and Lemieux (1996) weights and propensity scores based on 2003 sample (see Section 4 for details). The sample includes couples in rural villages with women ages 25 /28 /33 and older, and with children aged 0-10 /3-13 /8-18, respectively for the 2003 /2006 /2011 surveys.