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

Gustavo J. Bobonis † Roberto Castro ‡ Juan S. Morales §

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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. We use 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 non-beneficiary households within each village to minimize potential selection biases, we find that – in contrast to the short-run estimates – physical and emotional abuse rates do not vary significantly among existing beneficiary and non-beneficiary couples. We examine possible mechanisms for the discrepancy in the short and long-term impacts of the program; most importantly, the role that marital selection and the diffusion of norms rejecting intimate partner violence may play in explaining these effects.

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† Associate Professor, Department of Economics, University of Toronto, BREAD, and CIFAR. Address: 150 St. George St., Toronto, Ontario, M5S 3G7, Canada. Tel: 416-946-5299. Fax: 416-978-6713. E-mail: gustavo.bobonis@utoronto.ca.
‡ Professor, Regional Center for Multidisciplinary Research, National Autonomous University of Mexico, Apartado postal 4-106, Cuernavaca, Morelos, 62431, México. Tel: (52-777)-329-1853. E-mail: rcastro@servidor.unam.mx.
§ Ph.D. Candidate, Department of Economics, University of Toronto. Address: 150 St. George St., Toronto, Ontario, M5S 3G7, Canada. E-mail: juan.morales@utoronto.ca.

I. Introduction

Violence against women has been condemned internationally as a serious human rights, public health, and women's personal security concern. It is of particular concern that partner violence is still quite prevalent across societies. A question of great relevance for policy is whether social insurance programs intended to improve women's economic conditions help generate a *sustained* reduction in the incidence and severity of intimate partner violence. A number of 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 potential unintended consequence of this gender-based targeting may be an increased incidence of violence, as this may increase male partners' use of violence to gain or regain control over household resources or decision-making. Existing studies of the *short-run* consequences of the Mexican Oportunidades CCT Program are partially consistent with this view.[2]

This paper provides evidence of the longer-term relationship between the Mexican Oportunidades conditional cash transfer program and male-to-female spousal violence. To accomplish this, we use data from three nationally representative surveys, the National Survey on Relationships within the Household (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 survey data, we construct a pseudo-panel of comparable groups of beneficiary and non-beneficiary couples within each village to minimize potential selection biases, which allows us to estimate longer-term relationships – nine to thirteen years following the program's implementation – and to compare these to patterns observed in the short-run.

We provide two stylized facts. First, we show that the incidence of abuse among women is initially high among these couples (in 2003) and shows a downward trend over time (in 2006, 2011). Second, among couples in 2006 and 2011, women in beneficiary households are as likely to experience abuse of physical or non-physical forms as women in non-beneficiary couples. These findings stand in stark contrast to the short-run relationship established in observational and experimental studies – women in beneficiary households are significantly less likely to be

[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 PROGRESA/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).

victims of physical abuse than non-beneficiary 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? Our new evidence is consistent with two related and non-mutually exclusive forces that can impinge on the incidence of violence. First is the possibility of marital selection: in a context of repeated interactions – a natural consideration in the case of marital relationships – 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 intimate partner violence by women in the Mexican context 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). If these global attitudes have diffused across Mexican society in such ways as to decrease women's tolerance for these forms of violence, this could also help explain our results.

We provide additional evidence that 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 suggestive evidence that levels of violence among couples formed following the start of the program are lower than those formed preceding the start of the program, consistent with the view that the incidence of abuse may be lower among new couples. Consistent with the attitudinal shift hypothesis, we show that across time women are more likely to reject the justification of intimate partner violence, consistent with the cross-national evidence. Finally, we show evidence inconsistent with a number of other potential explanations, such as localized spillover effects among non-beneficiary couples and generalized social violence mediating effects on spousal abuse.

The study has important implications for policy. The program may, in the short-run, 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-run negative consequences in the livelihoods of women in the form of experiencing higher levels of spousal abuse.

The paper is structured as follows. In Section II we provide a concise description of the Oportunidades program and its implementation over time. We present the data and descriptive statistics in Section III, and our empirical methodology in Section IV. The main estimates are reported in Section V. A discussion of the main results, possible explanations, and robustness tests follows in Section VI, and Section VII concludes.

II. Overview of the Oportunidades Program

The Mexican government initiated in 1997 a conditional cash transfer 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 targets 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 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 PROGRESA initially targeted only children in primary and middle school, Oportunidades was expanded to cover children in secondary school. In 1998, these ranged from 70 to 255 pesos per month (approximately 7 to 25 USD), depending on the gender and grade level the child is attending, with a maximum of 625 pesos (62.5 USD) per month per family. Scholarship amounts have gradually increased, and in 2011 these ranged from 150 pesos per month (approximately

[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 4 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.

12 USD), up to 960 pesos (77 USD).[4] Families also receive yearly benefits for the purchase of school supplies of between 200 and 400 pesos (16 and 32 USD). In a further expansion of the program in 2009, it now 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 6-months to 23-months old, pregnant and breast-feeding women, and children aged 2-5 years who exhibit signs of malnutrition. Monthly cash transfers for beneficiary families expanded throughout 1997-2011, 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 0-9, established in 2010; elderly support (Adultos Mayores), 315 pesos for every adult aged 70 or over, established in 2006. These benefits are contingent on participation by mothers in monthly health talks with the local health care provider, the vaccination of family members, health checks of all children under 5 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 in only elementary or middle school, but have almost doubled for those with children in secondary school (see Figure 1).

The targeting of the program was done 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

[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., USDs)) 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.

[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 who 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).

proxy variables for poverty, the program administrators generated a proxy-means test.[6] Therefore, 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 et al. 2001). This targeting and program eligibility information is important in the construction of our sample of eligible women (see Sections IIII and IV.B).

Initially, a locality was eligible for Oportunidades if it was classified as "poor" (marginality grade 4) or "very poor" (marginality grade 5) out of a 1-5 scale based on the locality-level marginality index, and if it had access to a primary school, a secondary school, a health center, and was classified as rural (defined as inhabited by fewer than 2,500 people), but had at least 50 inhabitants (Skoufias et al. 2001). The last criterion was relaxed early on to incorporate some 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 around 53,000 localities and 2.5 million families, to 97,000 localities and 5.8 million families. The program was phased-in through a different targeting design in urban areas starting in 2001. Since this targeting mechanism is very complex and substantially different to 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.

III. Data, Measurement, and Summary Statistics

We use data from Mexico's National Surveys on Relationships within the Household ("Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares", or ENDIREH) of 2003, 2006 and 2011. These are three cross-sectional, nationally representative household surveys measuring the prevalence and intensity of intimate partner violence, among other intra-household interactions. It contains data on household demographics, socio-economic 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

[6] Within a sub-sample 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 et al. 2001 for a more detailed description of the targeting process).

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

We construct measures of incidence of violence that 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 constitutes a complex set of behaviors (Strauss and Gelles 1990; Follingstad and DeHart 2000). Following our earlier work, we construct two measures: 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 comes 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 non-cash benefits. As a result, this measure may over-report the receipt of Oportunidades benefits. Nonetheless, although there is some noise in the data – since only ten 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 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

[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 sample of women in the ENDIREH 2006 and 2011 selected for our analysis, only 3.0 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]

In order to minimize potential selection biases as a result of the targeting and endogenous take-up of the program, we had restricted the analysis of the short-term relationship in 2003 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 preceding 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, we restrict the 2006 (2011) survey sample to couples with women 28 (33) years or older with children between the ages of 3 and 13 (8 and 18) years. The resulting overall sample sizes for the longer-term analyses are 4,705 in the 2006 survey and 5,800 couples in the 2011 survey. As we will discuss below, these sample restrictions minimize potential confounding due to endogenous take-up of the program based on household socio-economic characteristics and preferences for human capital investments (see Section IV).

The summary statistics indicate that spousal violence remains a pervasive phenomenon in rural Mexico, but one that 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 has 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 socio-economic status. More importantly, we observe some stark differences in a number of dimensions of socio-economic status as we compare the pseudo panel of couples

[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.

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 socio-economic 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, although 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). Given the existing concerns and evidence regarding the intergenerational transmission of violent behavior, this suggests that women in this context may be at a particularly high risk of experiencing spousal violence, helping 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 similar schooling attainment levels, 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 (e.g., Bowlus and Seitz 2006; Casique 2006). Finally, households are relatively large, with around 5.7 members on average, a statistic usually correlated with low socioeconomic status in the Mexican context.

IV. Empirical Methodology

A. Estimation

To obtain robust estimates of the relationship between the incidence of spousal abuse and Oportunidades beneficiary status, we estimate ordinary least squares models conditioning on a large set of pre-determined individual and household socio-economic characteristics as well as village fixed effects, in order to capture any village-specific unobserved heterogeneity influencing spousal abuse patterns (e.g., access to health clinics, community groups,

village-level conditions affecting partners' socio-economic conditions and economic opportunities). The regression equation for outcome Y_{iv} is the following:

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

where the treatment indicator T_{iv} equals one for beneficiary household i in village v and is zero otherwise; X_{iv} are the pre-determined 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.

B. Dealing with Endogenous Selection into the Treatment

As discussed in more detail in our piece on the short-run relationship, 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 socio-economic 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) due to the potentially greater extent of conflict and the improvement in women's socio-economic conditions outside of current relationships – leading to a selected sample of households remaining in union; and finally, that (iv) the program may lead to changes in marital matching and sorting patterns due 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 among beneficiary and non-beneficiary households would violate the assumptions of unconditional independence necessary for identification of the program average treatment effect (Rubin 1974).

We conduct the replication analysis using various strategies to try to minimize the extent of bias in our estimates. First, as mentioned in Section III, we use a sub-sample of households with children ages between 0 and 10 in 2003 (ages between 3 and 13 in 2006, and ages 8 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, we condition on a set of pre-determined individual and household socio-economic characteristics which are strongly correlated with determinants of program eligibility and likely capture a

large component of the variation determining program take-up. Finally, we restrict the sample 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 non-beneficiary households for the empirical analysis. Comparing individual and household pre-determined covariates documents this potential selection bias: beneficiary women are more likely to be with an indigenous partner and 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 since 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 non-beneficiary households within villages in order 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 pre-determined characteristics among these groups of households shows drastic reduction in – although not a complete elimination of – the observed pre-determined observable characteristics differences (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.

In order to address the potential concerns of unobserved heterogeneity in the within-village household comparison, we pursue a set of tests and sensitivity analyses inspired by the work on diagnostics of selection on observable and unobservable variables (i.e., Imbens 2003; 2004; Altonji, Elder and Taber 2005). Essentially, we identify which observable characteristics (X_{iv}) that are 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, we evaluate 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 levels of education. The results obtained from this sensitivity analysis are qualitatively and quantitatively similar across specifications.

Finally, we also present estimates 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 since households may choose to make household improvements or purchase assets from the program-related cash benefits (Gertler, Martinez, and Rubio-Codina 2012). The results are robust to the inclusion and exclusion of these additional control variables.

V. Results

A. Graphical Evidence

We start the discussion 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 non-beneficiary 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). In comparison, the incidence among beneficiary couples is 8.9 percent, lower than that among couples in non-beneficiary ones 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 among these two groups of households converge in the longer-run. We observe a similar although less stark pattern of short-run differences (in 2003) and later convergence (in 2006, 2011) in the incidence of sexual abuse, emotional abuse, and threats of violence.[10]

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

Estimates of the overall five-year (2003), nine-year (2006), and thirteen-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

[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.

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, we consider this our preferred specification.

For purposes of comparison, we start the discussion with our preferred estimates of the short-run relationship. As documented in our earlier piece, domestic violence incidence rates in the short-run are lower among beneficiary couples than among non-beneficiary ones, physical and sexual abuse in particular (columns 1-2). 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 our preferred specification. We also find independent reductions in the incidence of physical and of sexual violence (generally significant at 90 percent confidence). It is worth highlighting that the estimates on both physical and sexual violence are of larger magnitude in absolute terms than the comparison of raw differences (see Figure 2) and the cross-village OLS estimates (not reported). In contrast, domestic violence incidence rates do not vary significantly across beneficiary and non-beneficiary households in 2006 and 2011 (columns 3-4) (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 4-5).

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

In this sub-section, we investigate 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 male partners to use emotional violence or threats of physical violence to extract rents or (re)gain control over their female's partners resources or decision-making (see Bobonis, González-Brenes, and Castro 2013 for details). In order to implement these tests we construct measures that capture this phenomenon. 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.

We once more start the discussion 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 non-beneficiary couples is only 2.7 percent, lower than that among beneficiary couples in 2003 (1.2 percentage points, not significant). The incidence among non-beneficiary couples decreases to 1.5 percent in 2006 and 0.5 percent in 2011. Substitution between these forms of abuse among these two groups of households converge in the longer-run. We also observe similar patterns of short-run differences and convergence using alternate measures of substitution (not shown in the figures).

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 in the absence of acts of physical or sexual abuse. The individually estimated coefficient estimates on threats of violence and emotional abuse are positive but imprecisely estimated in 2006, although 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 5-6) (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.

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

A. Repeated Interactions and Marital Selection

The stark differences in the longitudinal pattern of the relationship suggests that the models of violence and household bargaining, in which male partners may use violence as instruments of coercion (see Bloch and Rao 2002, Bobonis, González-Brenes, and Castro 2013, Anderson and Genicot 2014), may correctly capture short-run interactions within the household, but may poorly capture those in the longer-run. 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 types to use threats of abuse to coercively demand transfers from their wives, and strategically use physical violence (as a punishment) depending on whether their wife complies with the act of coercion. However, once this information has been revealed through partners' actions, couples with violent types may dissolve such that couples in future periods are disproportionately composed of non-violent types. This can lead to both a tendency for abuse rates to decrease among couples remaining in union over time, and for their relationship with program receipt status to be dampened.

We provide evidence partially consistent with this interpretation. First, we document that both beneficiary and non-beneficiary couples experienced a modest increase in marital dissolution rates over this time period. We use retrospective data on marital histories from the 2006 and 2011 surveys and construct marital dissolution rates for couples in union as of 1998, at the start of the program (see Figure 4). As of 2003, 3.2 percent of couples had separated or divorced, and the divorce rate rises 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 non-beneficiary ones, at least in the 2003-2006 period.[11] A similar pattern emerges when examining the data using the retrospective history for survey year 2011. In this case, 4.5 percent of couples have dissolved by 2006 and this rate increases to 7.9 percent as of 2011, and 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 selection could help explain the drop in the incidence of violence among these cohorts of women.

Second, a comparison of socio-economic 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

[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.

likely to report speaking an indigenous language across survey waves, which is correlated with low socio-economic status in Mexico; they tend to have lower educational attainment levels; and 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 pre-determined characteristics remain.[13]

Second, levels of violence among couples formed following the start of the program tend to be lower than those formed preceding 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 preceding the start of the program – and those in union since 1998 or later. Although the sample is not sufficiently large to make definitive statements regarding these differences, we observe a tendency for physical and sexual violence levels to be lower among more recently formed couples (rows 1-3). Moreover, we observe 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 4-5).

To the extent that changes in the composition of beneficiary households is driven by these marital selection dynamics, 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

[12] A number of housing characteristics (e.g., 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 to 9 percent, that is, with beneficiary women in the sample being disproportionately less educated than non-beneficiary ones in 2006 and 2011. Additionally, the gap in the proportion of women with indigenous partners also increased, from 11 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 partner's who witnessed spousal violence narrowed from 6 to 2 percent.

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

suggesting that migration towards 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.

We further consider the possibility that we observe these contrasting results due to these observable differences in sample composition that are not necessarily strongly correlated with male partners' willingness to engage in violent behavior against their spouse. To account for these observable sample differences across survey waves, we use the reweighting method introduced in the decomposition literature by DiNardo, Fortin, and Lemieux (1996). This methodology allows us to examine the extent to which the estimated relationship is muted due to the difference in the samples' composition.

First, we reweight the 2006 and 2011 samples so they 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_{it}) variables.[14] Second, we estimate reweighting factors:

$$(2) \qquad \psi(X_{it}) = [\Pr(S_{it}\{2003\}=1 \mid X_{it}) / \Pr(S_{it}\{2003\}=1)] \times [\Pr(S_{it}\{2003\}=0 \mid X_{it}) / \Pr(S_{it}\{2003\}=0)]$$

where $S_{it}\{2003\}$ indicates whether the observation belongs to the 2003 sample, and X_{it} is our 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, we estimate the counterfactual relationship on the appropriately reweighted sample.[15] We construct bootstrapped standard errors 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 reveal 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 respectively 0.40 and 0.37 respectively, and 0.43 and

[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 rime 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.

0.40 for the estimates of the 2003 and 2011 sample. 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. Given that we wanted our pseudo panel to be as close as possible to that of the short-run study, we normalize time dependent variables (age, partner's age, and years in union) 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 summary, this weighing scheme allows us to account for the fact that some of the couples in the 2003 sample are underrepresented in the subsequent pseudo-panels and to judge 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 it seems to be the case that the effect dissipates 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 our benchmark models, we see relatively larger coefficients for the 2006 sample, where beneficiary status is related to an increase the incidence of physical or sexual violence. In 2011 the results do not differ significantly from the un-weighted analysis, again indicating that observable differences in sample composition do not drive our results.

This analysis confirms our main findings that beneficiary women are as likely to be victims of abuse as non-beneficiary ones in the longer-run, once we take into account observable differences in composition of the sample. 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 male partners' willingness to engage in violent behavior against their spouse). Suggestive of this is that the incidence of physical abuse is significantly higher among non-beneficiary 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.

B. Increasing Rejection of Intimate Partner Violence

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 towards IPV across a large spectrum of societies. These new global scripts and norms may have also diffused across Mexican society in such ways as to decrease women's tolerance for IPV.[16]

To evaluate this hypothesis, we use additional information available in the ENDIREH data. Following the analysis in Pierotti (2013), we construct an indicator variable that measures whether the woman believes an intimate partner 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 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 non-beneficiary households. We estimate 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 non-beneficiary households. We estimate further decreases of proportional size between the 2006 and 2011 survey

[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 women does not meet her responsibilities, the partner has 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. We report these 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).

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.

We also document that the incidence of intimate partner violence starkly decreases over time among couples in non-beneficiary 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 7.9-9.0 percent range in 2006 and in the 6.4-7.0 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 our preferred sample, but that this is persistent across Mexican society.

C. Alternate Explanations

In this subsection, we evaluate other explanations potentially consistent with the evidence: localized spillover effects among non-beneficiary couples, changes in the de facto conditionality of the cash transfer program, generalized social violence mediating effects on spousal abuse, among others.

Localized Spillover Effects

A first alternate explanation is that the program empowers women in the community and provides them with the instruments to prevent spousal abuse, directly via interactions with beneficiary women with higher levels of empowerment in the community, or indirectly via improved socio-economic conditions and options outside of current relationships or changes in the norms of intolerance of abuse, among other mechanisms.[20] Therefore, to the

[19] We find analogous patterns for all other indicators of abuse. These are omitted for the sake of brevity.
[20] As shown by Angelucci and De Giorgi (2008) and Avitabile (2012), the program had spillover effects on the consumption levels and health behaviors (i.e., cervical cancer checks) of non-beneficiary households. Bobonis and Finan (2009), Lalive and Cattaneo (2009) show evidence of spillovers effects on middle school participation among children in non-beneficiary households.

extent that these spillover effects reduce the incidence of abuse among non-beneficiary women and increase female partners' intolerance of abuse, this 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:

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

where the treatment indicator T_{ivm} equals one for beneficiary household i in village v, municipality m and is zero 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., 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 among beneficiary and non-beneficiary households by including an interaction term between T_{ivm} and $E[T_{-i,v,m}]$. Since 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 1-2); 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 non-beneficiary couples in 2006, and an 0.12 percentage point decrease in its incidence in 2011 (column 2).

Spillover effects estimates 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.

Conditionality of Cash Transfers

A second alternate explanation is changes in the de facto conditionality of the cash transfers. In our study on the short-run relationship, we restricted the sample to intact households with children ages 11 years and younger at baseline; children who were not old enough to attend secondary school. The reasoning for this was that because school participation rates are close to one hundred percent for children in primary schools among the population of interest (and there are no program impacts in primary school participation) (Schultz 2004; Behrman, Sengupta, and Todd 2005), 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]

We address this potential concern by estimating the relationships for households with the same characteristics in our subsequent samples as in our 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 we have shown evidence above. Note that while 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, we only report estimates of these models for physical abuse for the sake of brevity (Table 10, columns 3). The analysis of this sample of households yields similar results. We find no significant differences in the incidence of physical abuse between beneficiary and non-beneficiary households. This analysis makes unlikely the claim that conditionality and aging of the couples could significantly explain the results.

Generalized Social Violence

[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.

Another potential alternate explanation we consider 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 seen an explosive increase in homicide rates since 2007, a surge that has been 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 (e.g., Astorga and Shirk 2010; Dell 2011).

We consider the potential for this surge in generalized social violence across municipalities and or states to mediate the trends in spousal abuse and the relationship with program beneficiary status. On one hand, to the extent that the surge in homicides can impinge on partners' stress levels or their levels of emotional health more broadly, this could lead to greater conflict-related abuse. On the other hand, if this generalized conflict negatively impinges on women's willingness to report events of abuse, this would be consistent with the significant drop in reported abuse rates in 2011, although not so in the year 2006. These are two potential mechanisms for a mediating effect, among others.

We evaluate this empirically by estimating empirical models that capture these mediating factors at the level of the municipality or state. The regression equation incorporating these factors is the following:

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

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), since the surveys are conducted over a long time period and we aim to ensure 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 non-beneficiary couples, whereas the θ_2 term captures the differential correlation among beneficiary ones. In our main specification, because the homicide rate is measured at the municipality level, we do not include village fixed effects in this specification. In a second specification with village fixed effects, we can identify the differential mediating effect for beneficiary couples.

[22] 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 (e.g., 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.

[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 among the victim's gender, shown in Valdivia and Castro (2013). Estimates are available upon request.

We report estimates of these models for physical abuse (Table 10, columns 4-6) 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 non-beneficiary couples by 0.56 percentage points and decreases its incidence by 0.66 percentage points among beneficiary ones (see 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 (see 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 implies 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 violence as of recent can explain a substantial closure in the protective relationship of Oportunidades for intra-household violence. However, all estimates are statistically indistinguishable from zero. We conclude that, 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.

Improvement in Women's Labor Market Opportunities

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

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

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

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 non-beneficiary couples, whereas the θ_2 term captures the differential correlation among beneficiary ones. We use the state-level rural wage gap measure

because the surveys are representative at the state level and 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, we do not include village or state fixed effects in this specification. In a second specification with state fixed effects, we can identify the differential mediating effect for beneficiary couples.

We report estimates of these models for physical abuse (Table 10, columns 7-8) for the sake of brevity.[25] The estimate for 2006 implies that a one standard deviation increase in the 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, in the period 2006, if we estimate 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 non-beneficiary households (Panel A, column 7). These heterogeneous effects would imply a greater reduction in physical abuse rates among beneficiary households in a context of decrease in the gender wage gap.

VII. Conclusions

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

The evidence suggests that, in the longer-run, women in beneficiary households are as likely to experience abuse of physical or non-physical forms as non-beneficiary women. Specifically, we find that a decade following the

[24] Using a state-level female wage gap measure may be somewhat restrictive for purposes of the analysis, as 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; available upon request.

start of the program, physical and emotional abuse rates do not vary significantly among existing beneficiary and non-beneficiary couples. These findings stand in stark contrast to the short-run relationship established in observational and experimental studies – women in beneficiary households are significantly less likely to be victims of physical abuse than non-beneficiary 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).

To try to understand the mechanisms underlying these diverging relationships, we evaluate 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, we find very suggestive evidence that reported levels of emotional violence among beneficiary couples formed following the start of the program are lower than those of non-beneficiary couples, consistent with the argument that those couples more likely to suffer emotional abuse may dissolve and abuse in new couples may decrease. Finally, we evaluate whether the increasing rejection of intimate partner violence by women in the Mexican context 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 our findings are persistent across Mexican society, consistent with the diffusion of norms regarding the unacceptability of spousal violence. Moreover, since Oportunidades (now Prospera) is an instrumental component of discourse and policy changes at the federal government level regarding the empowerment of women, we conjecture that the program may have contributed to the decline in observed levels of intimate partner violence. However, our ability to test this hypothesis is limited and is outside the scope of this paper.

The article may have important implications for policy, since it provides a mixed view of conditional cash transfer programs' effectiveness in improving women's empowerment within the household. The program may, in the short-run, 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-run negative consequences in the livelihoods of women, at least in the forms of experiencing 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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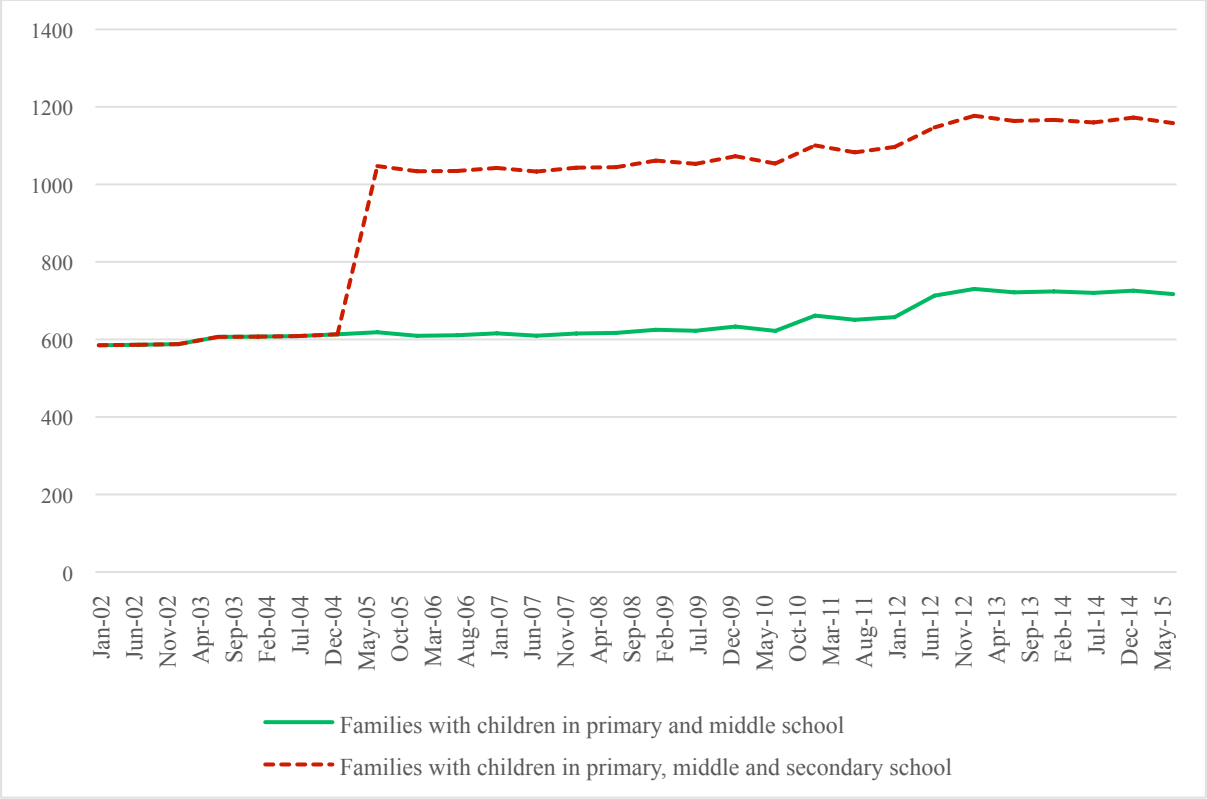
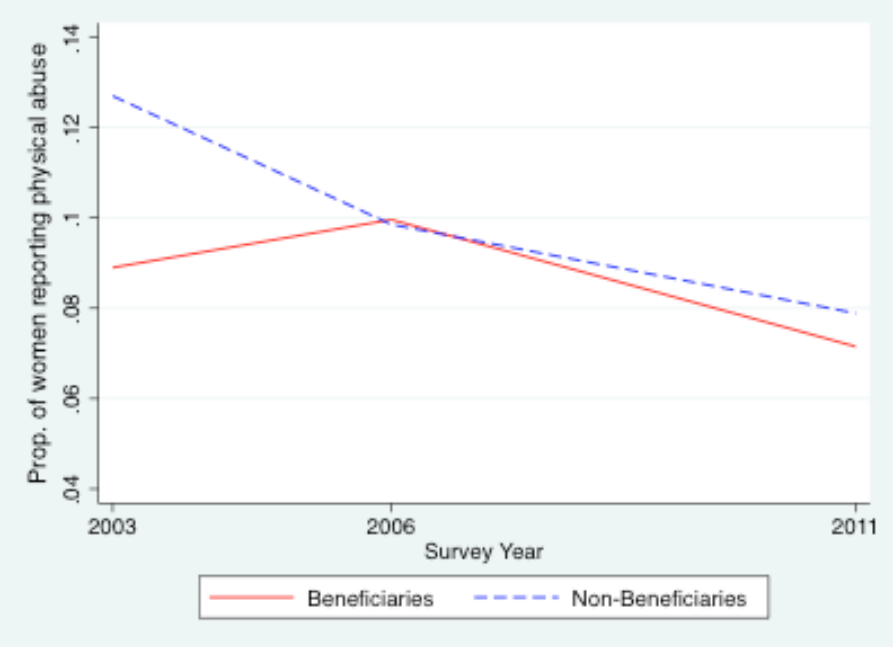
Figure 1: Oportunidades Program Maximum Monthly Benefits
(in Real 1998 Mexican Pesos)



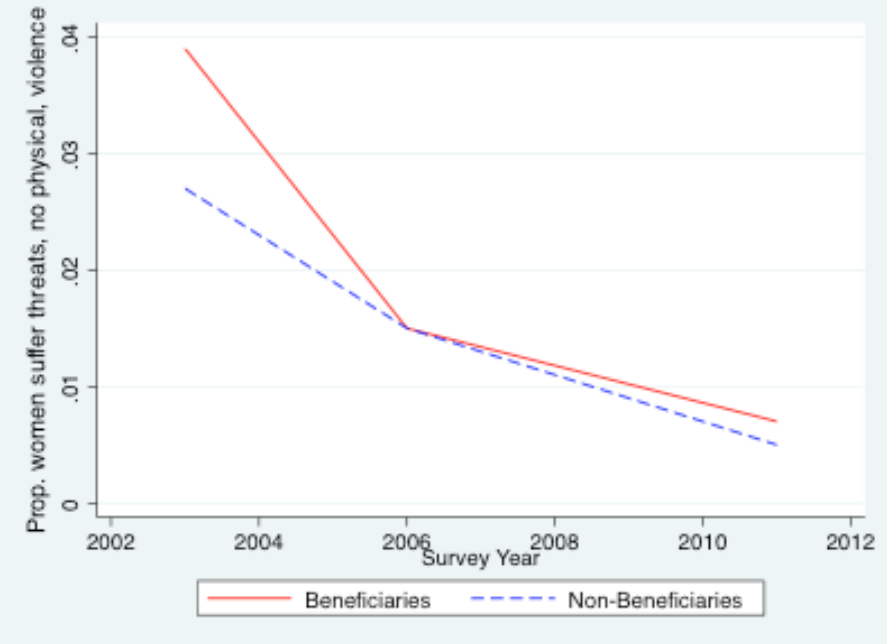
Source: Authors' calculations based on data from Secretariat of Social Development (SEDESOL) Mexico and the Bank of Mexico.

Figure 2: Incidence of Physical Violence by Beneficiary Status, Survey Wave



Notes: Shown are sample proportions of women reporting being victims of physical violence during the previous year, separately for beneficiaries and non-beneficiaries of government social programs. Sample proportions weighted by inverse sampling weights. The sample includes couples in rural villages with women ages 25 /28 /33 and older, with children aged 0-10 /3-13 /8-18, respectively for the 2003 /2006 /2011 surveys.

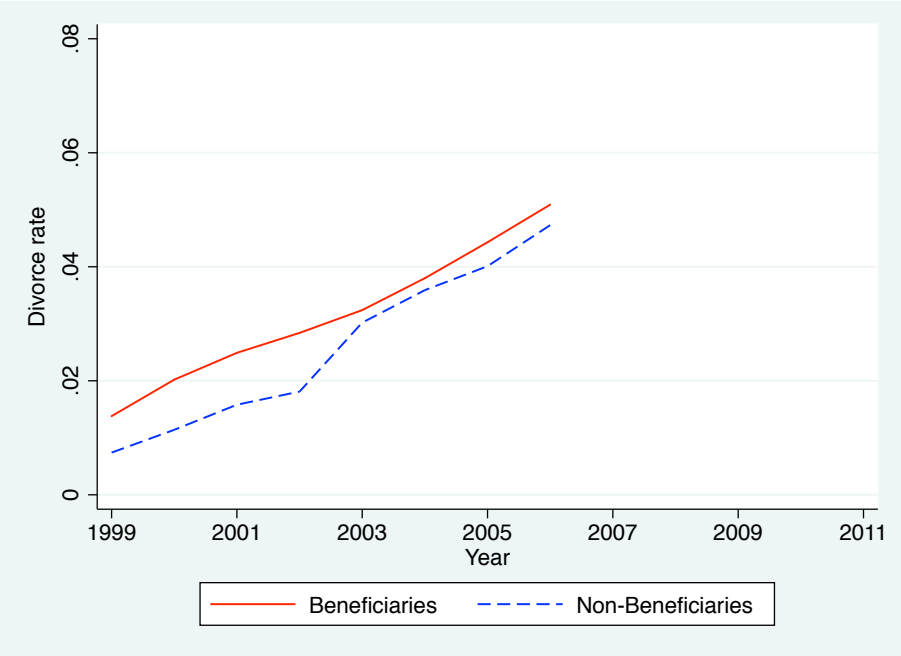
Figure 3: Substitution between Threats of and Physical Violence, by Survey Wave



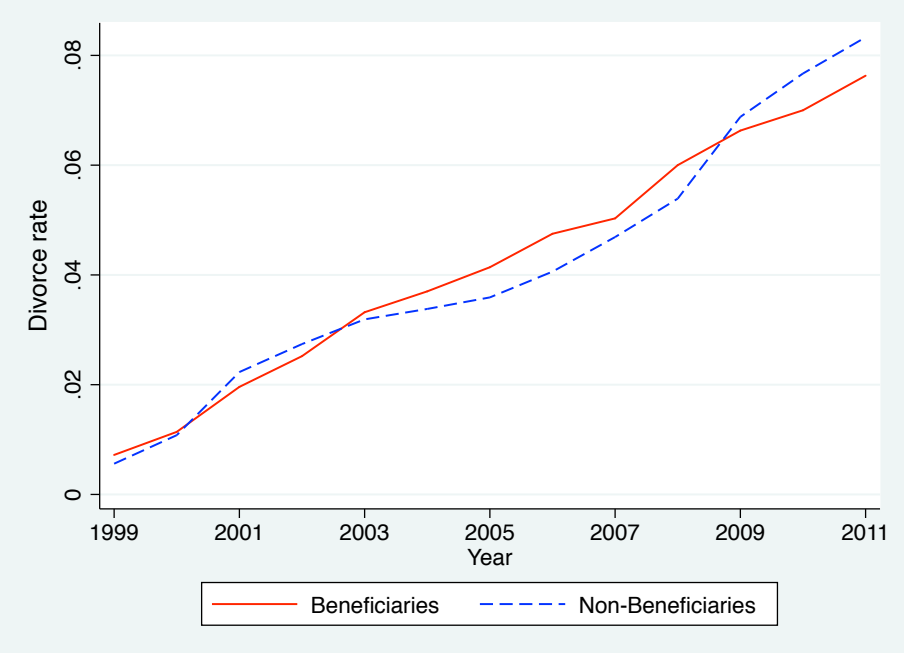
Notes: Shown are sample proportions of women reporting being victims of threats, conditional on them reporting no incidence of physical violence during the previous year, separately for beneficiaries and non-beneficiaries of government social programs. Sample proportions weighted by inverse sampling weights. The sample includes couples in rural villages with women ages 25 /28 /33 and older, with children aged 0-10 /3-13 /8-18, respectively for the 2003 /2006 /2011 surveys.

Figure 4: Divorce Rates by Beneficiary Status

Panel A: Survey Year 2006

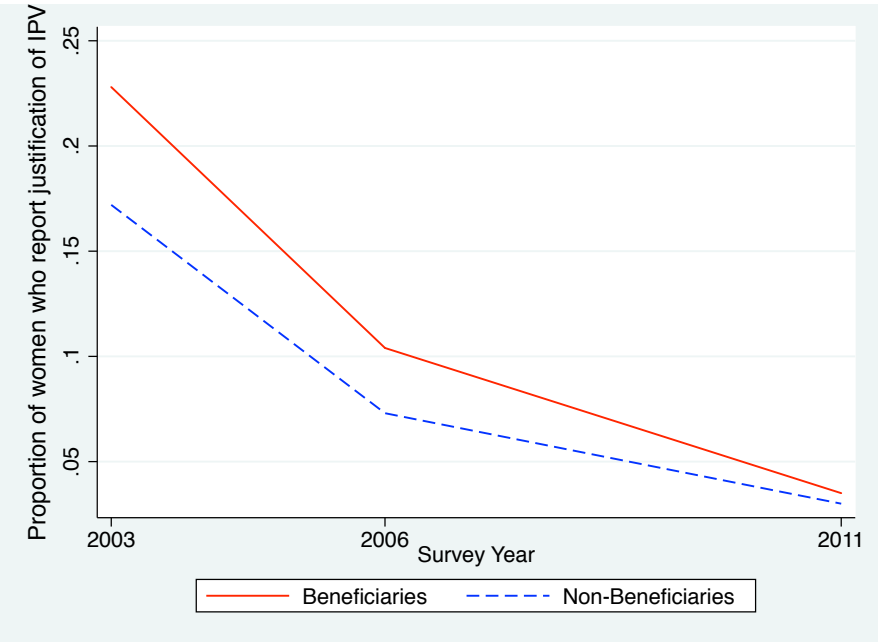


Panel B: Survey Year 2011



Notes: Sample means weighted by inverse sampling weights. The sample includes couples in rural villages with women ages 28 /33, and with children aged 3-13 /8-18, respectively for the 2006 /2011 surveys.

Figure 5: Reports of Justification of Intimate Partner Violence, by Survey Wave



Notes: Shown are sample proportions of women reporting agreeing with the statement that an intimate partner is justified in hitting or beating his female partner when she does not meet her responsibilities, separately for beneficiaries and non-beneficiaries of government social programs. Sample proportions weighted by inverse sampling weights. The sample includes couples in rural villages with women ages 25 /28 /33 and older, with children aged 0-10 /3-13 /8-18, respectively for the 2003 /2006 /2011 surveys.

Table 1: Summary of Oportunidades Benefits for Sample Years
(First Semester – Years 2003, 2006, 2011)

Benefit	2003	2006	2011
Nutritional support	155	180	225
Elderly support	-	250	315
Energy support	-	-	60
Compensated nutritional support	-	-	120
Child support	-	-	105
Educational component (by grade)			
Primary			
First	-	-	-
Second	-	-	-
Third	105	120	150
Fourth	120	140	175
Fifth	155	180	225
Sixth	205	240	300
Middle			
First			
Male	300	350	440
Female	315	370	465
Second			
Male	315	370	465
Female	350	410	515
Third			
Male	335	390	490
Female	385	450	565
EMS (Secondary)			
First			
Male	505	585	740
Female	580	675	850
Second			
Male	545	630	795
Female	620	715	905
Third			
Male	575	665	840
Female	655	760	960
Additional educational support			
Basic education	790	915	1,155
EMS (Secondary)	655	760	960
Monthly Maximum			
Families with children in primary and middle school	945	1,095	1,560
Families with children in primary, middle and EMS (secondary) school	1,600	1,855	2,520
Youth with Oportunidades			
(Traditional Model)	0	0	4,192

Notes: The benefits reported are nominal values in Mexican pesos for the January-June period of the sample year.

Source: Secretariat of Social Development (SEDESOL), Government of Mexico.

Table 2: Description of Outcome Variables - Male-to-Female Spousal Abuse and Threats of Violence

Variable Name	Description	Sample Means		
		2003 (1)	**2006** (2)	**2011** (3)
Physical or sexual violence	Indicator for any occurrence of physical or sexual abuse	0.160	0.137	0.102
Physical violence	Indicator for any occurrence of physical abuse (e.g., push, beating, attack with blade)	0.108	0.099	0.074
Sexual violence	Indicator for any occurrence of sexual abuse (e.g., use of force to have sexual relations)	0.090	0.069	0.042
Threat of physical violence	Indicator for any occurrence of physical abuse threat (e.g., threat of leaving, threat w/ deadly weapon, threat to kill)	0.079	0.042	0.022
Emotional violence	Indicator for any occurrence of psychological abuse, excluding perceptions questions (e.g., locked you in, threatened to leave you)	0.113	0.071	0.060

Notes: Sample means weighted by inverse sampling weights. The sample includes couples 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. N = 2867 /4705 /5800.

Table 3: Descriptive Statistics – Woman, Partner, Couple, and Household Characteristics

Variable Name	Sample Means		
	2003	2006	2011
	(1)	(2)	(3)
Panel A: Female Partner Characteristics			
Woman's age	34.89	37.39	42.38
Indigenous woman	0.14	0.16	0.20
No schooling	0.08	0.16	0.14
Primary school	0.65	0.57	0.56
Middle school	0.18	0.20	0.22
Secondary school	0.04	0.04	0.05
Violence in woman's childhood	0.10	0.11	0.13
Panel B: Partner and Couple Characteristics			
Partner's age	37.73	40.92	45.85
Indigenous partner	0.14	0.16	0.20
Partner's schooling attainment	5.70	5.24	5.64
Violence in partner's childhood	0.18	0.12	0.12
Cohabiting couple	0.19	0.22	0.20
Family size	5.82	5.92	5.51
Years in union	15.17	17.50	22.83
Panel C: Other Household Characteristics			
Dirt floor	0.29	0.25	0.12
Firm floor	0.64	0.65	0.77
Access to water - private tap	0.69	0.64	0.72
Access to water - public tap	0.04	0.06	0.04
Electricity	0.95	0.97	0.98
Computer	n/a	0.04	0.11
Telephone (Landline)	0.15	0.21	0.18
Radio	0.77	0.77	0.66
Drainage	0.69	0.75	0.85
Television	0.89	0.83	0.88
Num. of bedrooms	1.76	1.94	2.14
Num. of rooms	2.96	3.24	3.53
Observations	2867	4705	5800

Notes: Sample means weighted by inverse sampling weights. 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.

Table 4: Descriptive Statistics – Woman, Partner, Couple, and Household Characteristics (cont'd)

Variable	2003				2006				2011			
	Means		Difference in Means		Means		Difference in Means		Means		Difference in Means	
	Ben.	Non-B.	Overall	Vill. FE	Ben.	Non-B.	Overall	Vill. FE	Ben.	Non-B.	Overall	Vill. FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Woman's Characteristics												
Woman's age	35.03	34.76	0.27	1.24	37.94	36.45	1.50 ***	1.14 ***	42.65	41.92	0.73 **	0.15
Indigenous woman	0.20	0.08	0.13 ***	0.00	0.22	0.06	0.16 ***	0.01	0.27	0.09	0.17 ***	0.01
No schooling	0.12	0.05	0.07 ***	0.02	0.20	0.10	0.10 ***	0.02	0.20	0.09	0.12 ***	0.01
Primary school	0.71	0.60	0.11 ***	0.10 *	0.61	0.49	0.12 ***	0.08 **	0.61	0.49	0.12 ***	0.07 ***
Middle school	0.14	0.22	-0.08 ***	-0.05	0.16	0.25	-0.08 ***	-0.03	0.19	0.26	-0.07 ***	0.01
Secondary school	0.02	0.06	-0.04 ***	-0.04 *	0.02	0.09	-0.07 ***	-0.03 *	0.02	0.11	-0.09 ***	-0.06 ***
Violence in woman's childhood	0.10	0.10	0.00	0.01	0.12	0.10	0.02	0.02	0.13	0.11	0.02 *	0.03 *
Panel B: Partner/Couple Charac.'s												
Partner's age	38.35	37.11	1.24 *	3.04 ***	41.70	39.58	2.13 ***	1.81 ***	46.13	45.34	0.80 **	0.50
Indigenous partner	0.20	0.09	0.11 ***	-0.01	0.22	0.06	0.16 ***	0.01 *	0.26	0.10	0.16 ***	-0.01
Partner's schooling attainment	5.02	6.36	-1.35 ***	-1.35 ***	4.37	6.74	-2.38 ***	-1.34 ***	4.76	7.19	-2.42 ***	-1.40 ***
Violence in partner's childhood	0.15	0.21	-0.06	-0.06	0.12	0.13	-0.01	0.01	0.11	0.13	-0.02	0.01
Cohabiting couple	0.19	0.20	-0.01	-0.06	0.23	0.22	0.01	0.01	0.22	0.16	0.06 ***	0.04 *
Family size	6.29	5.35	0.94 ***	0.66 ***	6.19	5.46	0.72 ***	0.50 ***	5.74	5.11	0.63 ***	0.50 ***
Years in union	16.01	14.35	1.67 ***	2.30 *	18.82	15.23	3.59 ***	2.51 ***	23.62	21.45	2.18 ***	0.89 **
Panel C: Other Characteristics												
Dirt floor	0.36	0.21	0.16 ***	0.03	0.32	0.13	0.19 ***	0.07 **	0.15	0.07	0.07 ***	0.01
Firm floor	0.61	0.66	-0.05	0.02	0.64	0.67	-0.03	0.02	0.81	0.71	0.10 ***	0.07 ***
Access to water - private tap	0.60	0.78	-0.18	0.00	0.60	0.72	-0.13 ***	-0.01	0.68	0.79	-0.11 ***	-0.01
Access to water - public tap	0.05	0.03	0.02 *	0.02	0.06	0.05	0.00	0.00	0.04	0.05	-0.01	-0.01
Electricity	0.92	0.97	-0.05 ***	0.01	0.95	0.98	-0.03 ***	0.01	0.97	0.99	-0.02 ***	0.00
Computer	-	-	-	-	0.01	0.09	-0.08 ***	-0.05 ***	0.05	0.21	-0.16 ***	-0.09 ***
Telephone (Landline)	0.11	0.20	-0.09 ***	-0.08 *	0.14	0.31	-0.17 ***	-0.08 ***	0.14	0.26	-0.12 ***	-0.06 ***
Radio	0.75	0.79	-0.04	0.06	0.75	0.82	-0.08 ***	0.00	0.61	0.75	-0.14 ***	-0.07 ***
Drainage	0.60	0.78	-0.18 ***	0.00	0.69	0.84	-0.15 ***	-0.01	0.82	0.91	-0.09 ***	-0.04 ***
Television	0.89	0.89	0.00	-0.02	0.79	0.89	-0.10 ***	0.03	0.84	0.94	-0.10 ***	-0.01
Num. of bedrooms	1.69	1.83	-0.14 *	0.02	1.88	2.04	-0.15 ***	-0.08	2.06	2.27	-0.22 ***	-0.11 **
Num. of rooms	2.77	3.14	-0.38 ***	-0.16	3.03	3.60	-0.57 ***	-0.35 ***	3.28	3.95	-0.66 ***	-0.43 ***
Observations	1477	1391			2788	1917			3408	2392		

Notes: Sample means weighted by inverse sampling weights. Significant differences between beneficiary and non-beneficiary households at (*) 10 percent, (**) 5 percent, and (***) 1 percent levels. The standard errors of mean differences are clustered at the village level for columns (4), (8), (12). The sample includes couples in rural villages with women ages 25 /28 /33, and with children aged 0-10 /3-13 /8-18, respectively for the 2003 /2006 /2011 surveys.

Table 5: Estimates of the Relationship between Oportunidades Beneficiary Status and Spousal Violence

	Coefficient Estimate on Beneficiary Status [1/0] (s.e.)					
Dependent variables	**Survey Year 2003**		**Survey Year 2006**		**Survey Year 2011**	
	(1)	(2)	(3)	(4)	(5)	(6)
Physical or sexual violence	-0.096**	-0.082*	0.024	0.025	0.002	-0.001
	(0.047)	(0.044)	(0.026)	(0.025)	(0.018)	(0.017)
Physical violence	-0.062*	-0.055*	0.011	0.012	-0.001	-0.004
	(0.032)	(0.031)	(0.024)	(0.024)	(0.015)	(0.015)
Sexual violence	-0.066*	-0.050	0.017	0.013	0.000	0.000
	(0.039)	(0.035)	(0.020)	(0.019)	(0.012)	(0.012)
Threat of violence	0.025	0.018	0.012	0.011	0.004	0.004
	(0.037)	(0.023)	(0.018)	(0.018)	(0.010)	(0.010)
Emotional violence	0.041	0.027	0.011	0.008	0.005	0.003
	(0.040)	(0.032)	(0.020)	(0.018)	(0.015)	(0.015)
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual & household controls (linear)	Yes	Yes	Yes	Yes	Yes	Yes
Individual control interaction terms	No	Yes	No	Yes	No	Yes
Individual control polynomial terms	No	Yes	No	Yes	No	Yes
Asset Controls	No	No	No	No	No	No
Observations	2867	2867	4705	4705	5800	5800

Notes: Robust standard errors in parentheses, clustered at the village level; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from OLS regressions weighted by survey sampling weights. Controls include indicator 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. For details on polynomial and interaction terms, see Section IV.

Table 6: Estimates of the Relationship between Oportunidades Beneficiary Status and the Substitution between Emotional Violence and Physical/Sexual Violence

Dependent variable	Coefficient Estimate on Beneficiary Status [1/0]		Coefficient Estimate on Beneficiary Status [1/0]		Coefficient Estimate on Beneficiary Status [1/0]				
	Survey Year 2003		Survey Year 2006		Survey Year 2011		2003	2006	2011
	Overall Sample	Conditional on no physical and/or sexual violence	Overall Sample	Conditional on no physical and/or sexual violence	Overall Sample	Conditional on no physical and/or sexual violence	Mean of dependent variable, overall/conditional		
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7)	(8)	(9)
Threat of violence & no physical violence	0.031** (0.014)	0.033* (0.018)	0.012 (0.008)	0.014 (0.010)	0.004 (0.004)	0.004 (0.004)	0.027 / 0.031	0.015 / 0.016	0.006 / 0.007
Threat of violence & no physical/sexual violence	0.029** (0.013)	0.024 (0.017)	0.003 (0.006)	0.004 (0.008)	0.003 (0.003)	0.003 (0.004)	0.024 / 0.029	0.010 / 0.011	0.004 / 0.005
Emotional violence & no physical violence	0.044** (0.021)	0.036 (0.028)	0.016 (0.011)	0.024** (0.012)	0.003 (0.011)	0.003 (0.012)	0.048 / 0.055	0.030 / 0.034	0.035 / 0.038
Emotional violence & no physical/sexual violence	0.052*** (0.018)	0.040* (0.023)	0.000 (0.009)	0.009 (0.011)	0.005 (0.010)	0.001 (0.011)	0.035 / 0.043	0.021 / 0.024	0.028 / 0.032
Joint significance test p-value	[0.025]	-	[0.039]	-	[0.773]	-			
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes			
Individual & HH controls (linear)	Yes	Yes	Yes	Yes	Yes	Yes			
Individual control interaction terms	Yes	Yes	Yes	Yes	Yes	Yes			
Individual control polynomial terms	Yes	Yes	Yes	Yes	Yes	Yes			
Asset Controls	No	No	No	No	No	No			
Observations	2867	2611/2479	4705	4248/4074	5800	5410/5283			

Notes: Robust standard errors in parentheses, clustered at the village level; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects and/or OLS regressions weighted by survey sampling weights. Controls include indicator 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. For details on polynomial and interaction terms, see Section IV. The sample sizes in specifications conditioning on no episodes of physical violence or no episode of physical or sexual violence are reported at the bottom of columns (2), (4) and (6).

Table 7: Male-to-Female Spousal Abuse and Threats of Violence, by Union Formation Preceding/Following the Start of the Oportunidades Program

Survey Year	2003				2006				2011			
Couples in Union since	1997 or Earlier	1998 or Later	Difference	Difference Within Villages	1997 or Earlier	1998 or Later	Difference	Difference Within Villages	1997 or Earlier	1998 or Later	Difference	Difference Within Villages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Variable Name												
Physical or sexual violence	0.164	0.100	-0.064 **	-0.100 **	0.140	0.116	-0.024	-0.043	0.104	0.083	-0.022	-0.034
			(0.031)	(0.042)			(0.022)	(0.029)			(0.018)	(0.023)
Physical violence	0.109	0.085	-0.025	-0.027	0.100	0.094	-0.005	-0.018	0.076	0.057	-0.019	-0.024
			(0.028)	(0.030)			(0.020)	(0.024)			(0.014)	(0.018)
Sexual violence	0.092	0.056	-0.037	-0.090 **	0.072	0.041	-0.034 ***	-0.046 **	0.044	0.023	-0.016	-0.020
			(0.024)	(0.038)			(0.013)	(0.019)			(0.012)	(0.014)
Emotional violence	0.112	0.121	0.009	-0.037	0.070	0.078	0.008	-0.031	0.059	0.070	0.012	0.003
			(0.064)	(0.065)			(0.017)	(0.026)			(0.017)	(0.020)
Threat of violence	0.076	0.112	0.036	-0.013	0.041	0.051	0.009	0.004	0.023	0.011	-0.012 ***	-0.014
			(0.063)	(0.062)			(0.013)	(0.015)			(0.006)	(0.009)
Threat of violence & no physical violence	0.029	0.077	0.047	0.006	0.015	0.016	0.001	0.003	0.007	0.000	-0.007 ***	-0.008 ***
			(0.063)	(0.053)			(0.007)	(0.009)			(0.001)	(0.003)
Threat of violence & no phys./sexual viol.	0.024	0.075	0.051	0.009	0.010	0.009	-0.001	0.001	0.005	0.000	-0.004 ***	-0.006 ***
			(0.063)	(0.052)			(0.005)	(0.008)			(0.001)	(0.002)
Emotional violence & no phys. violence	0.054	0.083	0.029	-0.014	0.030	0.036	0.006	-0.018	0.033	0.053	0.020	0.013
			(0.063)	(0.056)			(0.012)	(0.016)			(0.016)	(0.017)
Emotional violence & no phys./sexual viol.	0.040	0.081	0.041	0.008	0.021	0.021	0.000	-0.015	0.026	0.051	0.025	0.017
			(0.063)	(0.053)			(0.008)	(0.013)			(0.016)	(0.017)
Observations	2628	254			4240	465			5208	592		

Notes: Sample means weighted by inverse sampling weights. Significant differences between couples formed preceding and following the start of the program at (*) 10 percent, (**) 5 percent, and (***) 1 percent levels. The sample includes couples in rural villages with women ages 25 /28 /33, and with children aged 0-10 /3-13 /8-18, respectively for the 2003 /2006 /2011 surveys.

Table 8: Estimates of the Relationship between Oportunidades Beneficiary Status and Spousal Violence in 2006 & 2011, Samples Weighted to Resemble Distribution of Observable Characteristics in 2003

	Coefficient Estimate on Beneficiary Status [1/0] (s.e.)			
Dependent variables	**Survey Year 2006**		**Survey Year 2011**	
	(1)	(2)	(3)	(4)
Physical or sexual violence	0.051*	0.052**	-0.002	-0.003
	(0.026)	(0.026)	(0.021)	(0.021)
Physical violence	0.033	0.035	0.000	-0.002
	(0.024)	(0.024)	(0.017)	(0.017)
Sexual violence	0.024	0.020	-0.002	0.000
	(0.020)	(0.020)	(0.015)	(0.015)
Threat of violence	0.018	0.017	0.004	0.004
	(0.017)	(0.017)	(0.010)	(0.010)
Emotional violence	0.015	0.014	0.011	0.008
	(0.018)	(0.018)	(0.017)	(0.017)
Village Fixed Effects	Yes	Yes	Yes	Yes
Individual & household controls (linear)	Yes	Yes	Yes	Yes
Individual control polynomial & interaction terms	No	Yes	No	Yes
Observations	4705	4705	5800	5800

Notes: Robust standard errors in parentheses, clustered at the village level; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects and/or OLS regressions using DiNardo, Fortin, and Lemieux (1996) weights and propensity scores based on 2003 sample (see Section VI for details). Controls include indicator 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. For details on polynomial and interaction terms, see Section IV.

Table 9: Estimates of the Relationship between Oportunidades Beneficiary Status and Physical Violence in 2006 & 2011, Varying Sample Definitions

	Coefficient Estimate on Beneficiary Status [1/0] (s.e.)	Mean [s.e.], Non-Beneficiary Women	N	Coefficient Estimate on Beneficiary Status [1/0] (s.e.)	Mean [s.e.], Non-Beneficiary Women	N
Dependent variable: Physical violence [1/0]	**Survey Year 2006**			**Survey Year 2011**		
Sample	(1)	(2)	(3)	(4)	(5)	(6)
Women in rural areas, currently in a relationship	0.006	0.090	10,745	0.001	0.064	17,281
	(0.010)	[0.006]		(0.006)	[0.004]	
Women in rural areas, in relationship since 2003	0.003	0.088	10,057	-0.004	0.070	12,622
	(0.011)	[0.006]		(0.008)	[0.005]	
Women in rural areas, in relationship since 2003, age ≥ 25 in 2003	0.011	0.079	8,592	-0.003	0.070	10,539
	(0.012)	[0.007]		(0.009)	[0.006]	
Women in rural areas, in relationship since 2003, age ≥ 25 in 2003, children ages 0-11 in 2003	0.011	0.102	4,705	-0.001	0.083	5,800
	(0.024)	[0.013]		(0.015)	[0.009]	
Village Fixed Effects	Yes			Yes		
Individual & household controls (linear)	Yes			Yes		
Individual control polynomial & interaction terms	No			No		

Notes: Robust standard errors in parentheses, clustered at the village level; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects regressions. Controls include indicator 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.

Table 10: Evaluation of Alternate Explanations

Dependent variable:	**Physical violence [1/0]**							
Sample:	Villages with 2+ Couples in Sample		Couples with Children Ages 0-11	All Couples				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
***Panel A*: Survey Year 2006**								
Beneficiary indicator	0.021	0.082**	0.016	0.014	0.015	0.012	0.0011	-0.0004
	(0.018)	(0.040)	(0.017)	(0.019)	(0.031)	(0.037)	(0.0124)	(0.0111)
Proportion of beneficiary	0.021	0.080						
households in the village	(0.048)	(0.060)						
Beneficiary × Prop. of beneficiary		-0.104*						
households in village		(0.060)						
Homicides in municipality				0.0005				
or state (per 100,000)				(0.0010)				
Beneficiary × Homicides in				-0.0012	-0.0003	0.0000		
municipality or state (per 100k)				(0.0011)	(0.0017)	(0.0003)		
Female / male wage ratio							-0.1112*	-0.0014
							(0.0597)	(0.0622)
Beneficiary ×								-0.1863**
Female / male wage ratio								(0.0910)
***Panel B*: Survey Year 2011**								
Beneficiary indicator	-0.007	-0.019	0.000	-0.016	-0.012	-0.005	-0.0143**	-0.0140**
	(0.012)	(0.025)	(0.014)	(0.014)	(0.019)	(0.024)	(0.0058)	(0.0054)
Proportion of beneficiary	-0.001	-0.012						
households in the village	(0.029)	(0.033)						
Beneficiary × Prop. Of beneficiary		0.019						
households in village		(0.037)						
Homicides in municipality				0.0000				
or state (per 100,000)				(0.0004)				
Beneficiary × Homicides in				0.0000	0.0005	0.0012		
municipality or state (per 100k)				(0.0005)	(0.0006)	(0.0047)		
Female / male wage ratio							0.0126	-0.0561
							(0.0371)	(0.0575)
Beneficiary ×								0.1057***
Female / male wage ratio								(0.0507)
S.E. Clustering and Fixed Effects	Municipality	Municipality	Village	State	Village	Village	State	State
Individual & household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (Panel A / Panel B)	4475/5565	4475/5565	6617/6555	4705/5800	4705/5800	4705/5800	4705/5800	4705/5800

Notes: Robust standard errors in parentheses, clustered at the village or higher geographic level; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from OLS regressions weighted by survey sampling weights. For details on controls and specifications, see Section IV.