

The Role of Conditional Cash Transfers in Reducing Spousal Abuse in Mexico: Short-Term vs. Long-Term Effects^{*}

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Current version: March 2010

First version: November 2009

Abstract: This paper provides evidence of the longer-term effects of the Oportunidades conditional cash transfer program on male-to-female spousal violence. We use data from two 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 may play in explaining these differential effects.

^{*} We thank Juan Morales for excellent research assistance, Dwayne Benjamin, Aloysius Siow, Emmanuel Skoufias, and participants at the Latin America and the Caribbean Regional Gender Study Workshop at the World Bank for helpful comments and suggestions, and Caridad Araujo, Tania Barham, Sonia M. Frías, Marta Rubio-Codina, Iliana Yaschine and the staff at Oportunidades and INEGI for providing data and their general support throughout. We are responsible for any errors.

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I. Introduction

Acts of spousal violence are commonly considered coercive instruments at the disposition of individuals for the control of household resources or partners' behaviors (Tauchen, Witte, and Long 1991; Bloch and Rao 2002; Felson and Messner 2000). Given the concerns that violence and coercion represent limitations on the freedom of an individual (Sen 1999), it is of particular concern that partner violence is quite prevalent across societies.¹ For instance, in the context of our study – rural Mexico – seven percent of women in a marital union report having been victims of physical abuse inflicted by their male partners at some point during the previous twelve months (Castro, Riquer, and Medina 2006; Castro and Casique 2008). In other countries such as Haiti and Zambia, the annual incidence of physical spousal abuse against adult females rises to approximately a quarter of the population (Kishor and Johnson 2004). This high prevalence across less developed countries has contributed to the recognition of spousal abuse as an important and global public policy concern.

Could social insurance policies intended to improve women's economic conditions and opportunities help reduce the incidence and severity of spousal abuse? A growing number of developing countries have introduced conditional cash transfer (CCT) programs – poverty alleviation programs that provide funds to *adult women* in households in exchange for certain actions such as children's school attendance, school performance, and preventive health care visits (Rawlings and Rubio 2003; Maluccio and Flores 2004). The basis for this gender-specific targeting is a growing consensus that targeting resources to women may help promote the empowerment of women within the household and in the community.² That said, a potential unintended consequence of the gender-based targeting of these programs may be an increased incidence of domestic violence, as unexpected changes in women's economic opportunities may increase the incentives for male partners to use violence or threats of violence to (re)gain control over household resources or decision-making (McCloskey 1996; Kimerling and Baumrind 2004). Partially consistent with this alternative view is evidence from the well-known and influential PROGRESA/Oportunidades CCT Program. Using data from the two-year experimental evaluation of short-run program impacts, Angelucci (2008) finds that women receiving

¹ Violence against women has been condemned at the highest international world population and women conferences (i.e., Cairo 1994 and Beijing 1995) as a serious human rights, public health, and women's personal security concern.

² See a survey of this literature by Duflo (2005) and the seminal papers by Thomas (1990) and Schultz (1990). The first paper to examine this in the context of the PROGRESA/Oportunidades program is Attanasio and Lechène (2002).

small transfer amounts are significantly less likely to suffer from spousal physical abuse, while a subset of women receiving large transfer amounts are more likely to suffer from abuse. Examining program impacts two to six years following its implementation, Bobonis, Castro, and González-Brenes (2006) find that although women in beneficiary households were significantly less likely to be victims of physical abuse than women in comparable non-beneficiary households, they were *more* likely to be victims of emotional violence with no associated physical abuse.

The mixed evidence regarding the short-run policy impacts of the PROGRESA/Oportunidades CCT Program shows a need to understand the longer-term consequences of this innovative policy experiment on the livelihoods of poor women. This paper contributes towards filling this gap by providing the first evidence of the longer-term effects of the program on male-to-female spousal violence. To accomplish this, we use data from two recently available nationally-representative surveys, the National Survey on Relationships within the Household of 2003 and 2006 (ENDIREH 2003, 2006), which include detailed information on the prevalence of male-to-female spousal abuse and threats of violence against women. Using this comparable survey data allows us to estimate longer-term program impacts – five to nine years following its implementation – and compare these to short-run estimates from our previous study (Bobonis, Castro, and González-Brenes 2006).

Constructing comparable groups of beneficiary and non-beneficiary households within each village to minimize potential selection biases, we find that, five to nine years following the 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 estimates – where women in beneficiary households were significantly less likely to be victims of physical abuse but substantially more likely to suffer emotional abuse (with no associated physical abuse) than non-beneficiary women. Together, these findings provide some evidence against traditional economic theories of the family, which generally claim that these transfer programs, by improving women’s bargaining position in the household, may decrease the incidence of both physical and emotional abuse.

We also examine possible mechanisms for the discrepancy in the shorter and longer-term impacts of the program. A first candidate explanation is the possibility that marital selection – the types of couples remaining in a marital relationship as a result of the program – may play an important role in explicating these

differential effects. As shown by Bobonis (2009) in the short-term experimental evaluation of the PROGRESA program, couples eligible for the program experienced a modest increase in marital dissolution rates, with most of the effect concentrated among young and relatively educated women households.³ To the extent that changes in the composition of beneficiary households is partly driven by these marital selection dynamics, and that the effects among the remaining intact couples are weaker due to women's lower socio-economic status, these selection and treatment effect heterogeneity patterns could help explain the time path of spousal violence among beneficiary couples. We provide evidence consistent with this interpretation, although we are not able to rule out alternatives, such as rural/urban and international migration or slight modifications in the survey questions (Azuara 2009).⁴

The paper is structured as follows. In Section II we provide a concise description of the Oportunidades program and its implementation. We present the data used in the analysis in Section III. In Section IV, we then describe our identification strategy and discuss how it avoids the identification pitfalls. The main estimates are reported in Section V. A brief discussion of the results 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 – 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. It provides cash transfers to the mothers of over 2.6 million children conditional on school attendance, health checks, and participation in health clinics. The education component of Oportunidades consists of subsidies provided to mothers, contingent on their children's regular attendance at school.⁵ These cash transfers are available for each child attending school in grades three to nine of primary and lower secondary school, and range from 70 to 255 pesos per month (approximately 7 to 25 USD), depending on the gender and grade level the child is

³ See Bowlus and Seitz (2006) for evidence on spousal abuse, divorce, and marital selection patterns in the Canadian context.

⁴ We also cannot rule out general equilibrium effects on women's level of power within the household as a result of changes in marital patterns that may attenuate any possible short-run effects, as proposed by Chiappori, Iyigun and Weiss (2009).

⁵ Receipt of the education-specific benefits is contingent on children attending school at least 85 percent of the time, which is verified by school personnel.

attending, with a maximum of 625 pesos (62.5 USD) per month per family in 1998.⁶ The health and nutrition components consist of cash transfers of approximately 12 pesos per month and nutritional supplements targeted at 4-months to 2-years, pregnant and breast-feeding women, and children aged 2-5 years who exhibit signs of malnutrition (Gómez de León and Parker 2000). 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 8 percent of the average expenditures of beneficiary families (Skoufias 2001).

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.⁷ Second, program enumerators conducted household surveys within eligible localities to identify households that would be classified as poor. Based on asset holdings used as proxy variables for poverty, the program administrators generated a proxy-means test.⁸ 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. 1999). This targeting and program eligibility information is important in the construction of our sample of eligible women (see Sections III.B and IV).

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

⁶ 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 1998-2006 period. Given these fluctuations, we opt to report the figure valid at the start of the program, in 1998.

⁷ 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).

⁸ 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).

inhabited by fewer than 2,500 people), but had at least 50 inhabitants (Skoufias et al. 1999). 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. The inclusion of less marginal localities into the program was gradually extended throughout the 1997-2006 period. By the year 2003, localities within the marginality grade 3 (average marginality) had been incorporated into the program. The program was phased-in through a different targeting design in urban areas starting in 2001. Since this targeting mechanism is very complex and very different to the one implemented in rural and semi-urban areas, we focus our analysis on rural households.

III. Data, Sample, and Summary Statistics

A. Description of the Data

We use data from Mexico's National Surveys on Relationships within the Household of 2003 and 2006 (ENDIREH 2003 and 2006), two 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 years or older living with a husband or partner, whereas the 2006 survey was administered to 113,561 women in this age range independent of marital status. Only one eligible woman per household was interviewed. In the following paragraphs, we provide a detailed description of the various measures of violence used in the analysis.⁹

We construct measures of incidence of violence which 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,

⁹ This follows closely the description provided in the documentation and results of the survey in Castro et al (2006).

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.¹⁰

Emotional violence constitutes a complex set of behaviors (Strauss and Gelles 1990; Follingstad and DeHart 2000). Therefore, constructing an incidence measure of emotional violence is a challenging task. On one hand, we would like to use measures of emotional abuse as comprehensive as possible to encompass abusive behaviors usually not captured in surveys. On the other hand, the construction involves making value judgments as to what constitutes violence of a psychological but non-physical form. Therefore, we constructed a measure of incidence of emotional abuse, and a measure of incidence of threats of violence, and assess how results may be sensitive to these definitions. For emotional abuse, survey questions are categorized as "low" or "high" severity. "Low" severity emotional abuse includes: a partner who stops speaking to a woman, humiliates her, destroys or hides things that belong to her or the household, accuses her of cheating, locks her up and prohibits her from leaving the house or having visitors, has made her feel fear, or has turned her relatives against her. There are only two remaining questions, which are categorized as "high" severity emotional violence: a partner who has threatened a woman with a knife, blade, gun, or rifle, or a partner who has threatened to kill himself, her, or the children. For each of these (as well as the physical and sexual violence) questions, women were asked how often it had occurred ("one time", "multiple times", never"). The emotional violence indicator is equal to one if (i) a woman answers "yes" to at least two of the "low" severity emotional abuse questions, or (ii) a woman answers "yes" to only one "low" severity emotional abuse question, but states it happened multiple times in the past year, or (iii) a woman answers "yes" to one of the "high" severity emotional violence questions.

To construct this measure, we restrict the questions to those which are least likely to involve the woman's perceptions: whether the partner destroys or hides things that belong to her or to the household, locks her up and prohibits her from leaving the house or from having visitors, and the threats of various forms.¹¹

¹⁰ The 2003 survey presented the questions in a slightly different form. Women were first asked whether it had occurred in the past twelve months, and for those who answered affirmatively, how often it had occurred ("one time", "a few times", many times"). See Appendix Table A1 [IN PROGRESS] for a comparison of the questions in the 2003 and 2006 ENDIREH surveys used to construct these measures.

¹¹ The measure needs to take into account and reduce the possibility that it may involve perceptions of abuse, and may involve a substantial degree of bias – especially if the reporting to these questions is related to the woman's observed and unobserved determinants of program take-up.

Finally, we construct the threat of violence indicator, which strictly includes the responses to the threat questions mentioned above.¹²

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 data aggregated by percentiles of the marginality index distribution are incredibly similar to administrative data grouped in an analogous format.¹³

Although the ENDIREH 2006 survey asks women specifically whether they receive benefits from Oportunidades, and separately whether they are beneficiaries of other government support programs, in order for the analysis to be comparable to that using data from the ENDIREH 2003 survey, the measure we use is the analogous measure of being a beneficiary from any government support program (i.e., Oportunidades or other). In the sample of women in the ENDIREH 2006 selected for our analysis, only 3 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.¹⁴

B. Selection of Sample for Analysis

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 short-term effects 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,

¹² We attempted to construct measures of the severity of violence. These indices were constructed based on the responses to the questions described above (and presented in detail in the data appendix). However, we believe that the quality of the severity and intensity data is quite limited, and chose not to use these in the main analysis.

¹³ 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.

¹⁴ We also estimate analogous models using the ENDIREH 2006 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.

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. To maintain a comparable sample of households in the follow-up study, we restrict the 2006 survey sample to couples with women 28 years or older with children between the ages of 3 and 13. In the 2006 sample we include couples who have been in a relationship preceding the start of the program ($N = 4,240$) as well as those who started the relationship in 1998 or later ($N = 520$). The resulting overall sample size for the longer-term analysis is 4,760 couples. 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. The latter relationship duration restriction helps us address or rule out issues of marital sorting based on the tolerance for spousal abuse, and other potential determinants of the incidence of violence, such as the gains to marriage (see Section IV).

C. Descriptive Statistics

The summary statistics based on the survey responses indicate that spousal violence remains a pervasive phenomenon in rural Mexico, but has decreased considerably since the 2003 survey. In 2006, thirty-one (31) percent of women in the sample reported having experienced some form of spousal violence within the previous year, be it physical, sexual, emotional or economic, compared to forty (40) percent in 2003 (Table 1). The incidence rates of physical, sexual, and emotional violence are of similar magnitude in this population, and all decrease when compared to 2003, with roughly 10 percent reporting having experienced some form of physical violence (down from 11 percent in 2003), 7 percent reporting some act of sexual violence (down from 9 percent), and 7 percent reporting evidence of emotional abuse in the previous year (down from 11 percent). These stark measures of abuse compare favorably to reported incidence of abuse in East African country contexts, but are high relative to the incidence rates reported in developed countries.

Comparing violence incidence rates in 2006 between couples in a relationship preceding the start of the program (since 1997 or earlier) and those who formed a relationship in 1998 or later, there seems to be no significant differences in physical violence and threats of abuse (see Table 1). We only observe small (and insignificant) differences in the incidence of threats of violent behavior and physical abuse. This suggests that

the timing of the formation of relationships (preceding or following the program) do not seem to be correlated with the incidence of abuse among this population.

Households in the sample are of relatively low socio-economic status. Also, it is interesting that we observe some significant differences in some of the socio-economic characteristics as we compare the 2003 and 2006 samples. Approximately 16 (8 in 2003) percent of women in the 2006 sample have no schooling, although 57 (65 in 2003) have completed some primary school (Table 2, Panel A). A significant fraction of women are indigenous (around 16 percent in 2006 and 14 percent in 2003, based on the linguistic definition of indigenous background), which is highly correlated with low socio-economic status in Mexico. The women's average age is approximately 37.4 years (34.9 years in 2003), thus relatively young (as expected) since we select women with young children; the difference in age is explained by the different age restrictions imposed on the two samples. The proportion of women who report being exposed to spousal abuse between their parents during childhood is quite high, at approximately 10 percent in 2003 and 17 percent in 2006. Given the existing concerns and evidence regarding the intergenerational transmission of violent behavior, this statistic may provide a sign that women in this context may be at a particularly high risk of experiencing spousal violence, thus explaining the prevalence of abuse reported above.

Most male partners belong to the same age group (the average partner age is approximately 41 years in 2006, 38 in 2003), have similar schooling attainment, and are as likely to have an indigenous background (Table 2, Panel B). The proportion of women reporting that their male partners were exposed to spousal abuse between their parents during childhood is substantial, at approximately 18 percent in 2003 and 21 percent in 2006. These, as will be shown below, are important predictors of spousal abuse among current partners. Finally, note that households are relatively large, with around 5.7 members on average, a statistic usually correlated with low socioeconomic status in the Mexican context.

In addition to the average differences we observe between the two 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 two populations (Table 2, columns 4 and 8), we can see that the gap in woman's age between beneficiaries and non-beneficiaries increased from 0.27 to 1.53 years, the gap in the proportion of indigenous women increased by 3 percentage points (from 13 to 16 percent), and the

difference in the proportion of women who completed secondary schooling increased from 4 to 8 percent, that is, with beneficiary women in the sample being disproportionately less educated than non-beneficiary ones in the year 2006. 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 2.36 years; and finally, the gap in the proportion of partner's who witnessed spousal violence narrowed from 6 to 3 percent, mostly due to a sharp decrease in this incidence for beneficiary couples, from 15 to 20 percent. Note that these differences are *not* driven by the fact that we are comparing couples in union since 1997 or earlier in 2003 to all couples in 2006, since these differences persist by comparing the former to the analogous sample of couples in a union since 1997 in the latter survey (Table 2, Panels C and D).

We offer two possible explanations for these observed differences: marital selection and selective rural/urban or international migration. First, there is evidence that the types of beneficiary couples who remain in a marital union tend to be more negatively selected in terms of their socio-economic status. Bobonis (2009), using the sample from the short-run experimental evaluation of the program in rural areas, documents that separation rates in couples consisting of women with high educational attainment and partners with low educational attainment increased as a result of the program. The second reason – selective migration towards urban areas or internationally – is consistent with evidence suggesting that migration towards urban areas among program beneficiaries has been more common for those households with higher educational attainment (Azuara 2009). To the extent that the effects of the program are heterogeneous by these characteristics (or unobserved characteristics correlated with these measures of socio-economic status), these selection patterns may result in varying treatment effects of the program among the remaining couples in rural areas.

IV. Empirical Methodology

Differences in spousal violence incidence rates between program beneficiaries and non-beneficiaries may in general reflect not only the effects of the Oportunidades program on violent behavior within the household, but also any differences in characteristics across groups that determine their selection into being program recipients and which independently affect spousal abuse patterns. Means comparisons of household baseline covariates document this potential selection: 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; and finally, they report that their partners observed more spousal abuse during childhood, than non-beneficiary households (Table 2). Various potential reasons for this endogenous (self-) selection into the program 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) 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 (ATE) (Rubin 1974).

To deal with these potential threats to validity, and in the absence of random assignment of the program to households in the sample, one can look for situations where treatment is based on observed variables and is otherwise exogenous. Under a conditional unconfoundedness assumption, treatment assignment is assumed independent of the potential outcomes conditional on a set of observed pre-treatment variables (Rubin 1978; Rosenbaum and Rubin 1983). In this paper, we use various strategies to minimize the extent of bias in program ATE estimates. First, as mentioned in Section III.B, we use a sub-sample of households with children ages between 0 and 10 in 2003 (ages between 3 and 13 in 2006), and households whose demographic compositions make them likely to 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 – shown in Table 2 – 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 (i) women ages 28 (25 in 2003) and older and (ii) couples who have been in union since 1997 – who made their marital decisions preceding the start of the program – in order to

minimize the likelihood that they benefited directly from program benefits (which started in some villages in 1997) as children, changing their socio-economic characteristics or possibly affecting their marital matching patterns. In the robustness tests section, we present estimates from empirical models that additionally condition or match on households' asset holding patterns, which should improve the common support of the beneficiary and non-beneficiary household samples, and address potential sample selection biases due to divorce.

Using this specific sub-sample, we first present ordinary least squares estimates of the ATE, conditioning on this 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). The regression equation for outcome Y_{iv} is the following:

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

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 can alternatively estimate the ATE non-parametrically using a matching estimator weighted by the sampling probabilities (Abadie and Imbens 2006). This weighted matching estimator is:

$$(2) \quad \hat{\theta}_M = \sum_i \rho_i \cdot (T_{iv} \hat{Y}_{1iv} + (1 - T_{iv}) \hat{Y}_{0iv})$$

where \hat{Y}_{0iv} equals Y_{0iv} if $T_{iv} = 0$, and equals a weighted average of the closest matches if $T_{iv} = 1$; and, likewise, \hat{Y}_{1iv} equals Y_{1iv} if $T_{iv} = 1$, and equals a weighted average of the closest matches if $T_{iv} = 0$.

Our case-control matching methodology pairs couples within each village to eliminate potential sources of unobserved heterogeneity due to village-level characteristics that may jointly influence program eligibility, take-up, and familial violence outcomes. However, this approach may induce bias due to the fact that number of couples sampled in each village may be small and the matching procedure may thus not minimize the unobserved heterogeneity due to unobserved determinants of familial violence across couples

within each village. Therefore, we also assess whether our results are robust to our research methodology by carrying out case-control matching within and across villages, an approach that should theoretically reduce the bias due to household-level unobserved heterogeneity.

V. Results

The overall five to nine-year treatment effects of the program on the spousal violence and threats outcomes are displayed in Table 3. We present OLS estimates in Column 1, and estimates from the various non-parametric case-control matching estimators in Columns 3, 5, 7, and 9. The proportional magnitudes of the estimated effects relative to the non-beneficiary household couples' incidence rates are reported in the columns following each estimate. Since the within village matching-estimator estimates are more likely to reduce or eliminate potential biases, we consider these our preferred estimates. We then report average treatment effects (ATE) estimates for the subset of couples in a relationship preceding the start of the program and compare these to the shorter-term ATE estimates from the 2003 sample. Finally, we report estimates of program impacts for the couples formed in 1998 or later, following the phase-in of the program, to see whether program effects differ significantly for couples who may have responded in terms of marital selection as a result of the potential eligibility to the program.

A. Overall Program Impacts on Physical and Emotional Violence

Domestic violence incidence rates do not vary significantly across beneficiary and non-beneficiary households five to nine years following eligibility for the Oportunidades program. The estimated reduction in the incidence of any type of violence varies among specifications, ranging between 0.6 and 4.7 percentage points (2-15 percent), but in many cases is not statistically different from zero (Table 3, row 1). There are no significant reductions in the incidence of physical abuse (row 2). Our preferred within-village matching estimate (with bias adjustment) shows an increase of 2.0 percentage points, or 21 percent (significant at 90 percent confidence), but the estimates are not robust to alternative specifications. Moreover, the estimates of program impacts on sexual and emotional violence show no evidence of significant reductions or increases in

the incidence of abuse (rows 3 and 4). These results stand in contrast to shorter-term estimates based on the 2003 survey, which indicate substantial reductions in physical abuse among beneficiary households.

We also find no evidence of significant increases in the incidence of violent threats or acts of emotional violence without associated acts of physical or sexual abuse based on the 2006 survey sample (Table 3, Panel B). Reports of the (unconditional) incidence of threats of violent behavior did not increase among beneficiaries relative to non-beneficiaries (row 1), analogous to the evidence from the 2003 survey. However, threats of violent behavior conditional on no physical abuse suggest small increases of approximately 4-6 percent in our preferred specifications, but the estimates are not significantly distinguishable from zero (row 2). The results expanding the definition of physical abuse to include sexual violence show imprecise estimates of reductions in threats of approximately 37-48 percent (row 3). Moreover, we find no evidence of an increase in the incidence of emotional violence conditioning on a lack of physical violence (row 4). The estimates from our preferred specifications indicate reductions 5-9 percent in its incidence, insignificant at conventional confidence levels. These results are also robust to conditioning on a lack of prevalence of physical or sexual abuse (row 5). Again, these results stand in contrast to shorter-term estimates based on the 2003 survey, which indicate substantial reductions in physical abuse, but significant increases in the incidence of verbal non-physical abuse in the household.

In conclusion, the overall results presented in this sub-section show no evidence of reductions in physical abuse or increases in male partners' use of emotional abuse (associated with no physical or sexual abuse) among couples as a result of the program. These are in significant contrast with the short-term estimates reported in Bobonis, Castro, and González-Brenes (2006) – rationalized as the result of an increase in women's socioeconomic opportunities generating a greater incentive for male partners' to use emotional violence or threats of physical violence to extract rents from the wife's greater endowment. However, because we are estimating impacts for couples in 2006, irrespective of the timing of union formation, this can lead to lack of comparability with the 2003 estimates. In the following sub-section, we examine whether the estimated effects are different among the subset of couples in a relationship since 1997 or earlier, to assess whether these sample composition differences drive the divergence in results.

B. Program Impacts among Couples in Union Preceding the Onset of the Oportunidades Program

Our shorter-term (two to six years program impact) estimates for year 2003 show that the estimated reduction in the incidence of different types of violence varies among specifications, but in general, the results indicate that being a beneficiary of Oportunidades (based on our measure of being a beneficiary of any social program) led to reductions on physical spousal violence (Table 4). Most notably, we find large and statistically significant reductions in the incidence of physical abuse, where our preferred matching estimate (with bias adjustment) shows an impact of 3.6 percentage points, or 33 percent (significant at 95 percent confidence; row 2). The remaining estimates are also negative, with the exception of the program impact on emotional violence which suggests a moderate increase of 2.6 percentage points, or 23 percent (row 4).

In contrast, we find no evidence of five to nine year longer-term program impact estimates on physical violence (column 6). The comparable matching point estimate for physical violence suggests an increase of 1.1 percentage points, or 11 percent, in incidence. The estimated impacts on emotional violence differ as well; the point estimate implies a reduction of 1.4 percentage points, or 21 percent, in the incidence of this type of abuse. Both estimates for 2003 and 2006, however, are insignificantly different from zero.

In order to be more precise in comparing the estimated duration-specific program impacts, we compare the 90 and 95 percent confidence intervals for the period-specific estimates of physical and emotional violence effects (Figure 1, Panels A and B). We can reject the equality of the period-specific program impacts on physical violence at the 90 percent confidence level (Panel A).¹⁵ In contrast, we observe that the 90 and 95 percent confidence intervals for the shorter-term and longer-term effect emotional violence impact estimates overlap, implying that we cannot reject the hypothesis of these effects being the same in the two samples (Panel B).¹⁶

The shorter-term program impacts estimates on the incidence of violent threats or acts of emotional violence indicated that the Oportunidades program led to substantial increases in these outcome measures, in some cases almost doubling the incidence of these acts (Table 5, columns 1 and 3). There is evidence of a substantial increase in the incidence of emotional violence conditioning on a lack of physical violence (row 4).

¹⁵ The 90 percent confidence intervals for the physical violence 2003 estimates is [-0.064,-0.008], and that for the 2006 estimate is [-0.008,0.030].

¹⁶ The 2006 estimates are robust to case-control matching within and across villages – see estimates in Appendix Table A1, Panel A.

The point estimate for the bias corrected matching estimator indicated an increase of 3.6 percentage points (79 percent), significant at a 99% confidence level (row 4). The remaining estimates were all positive as well, ranging from 0.1 to 1.8 percentage points (5 to 81 percent), although none of these were significantly different from zero. In Bobonis, Castro and González-Brenes (2006), we also document how these conditional relationships are consistent with models of household decision-makers' interactions with asymmetric information in the male partners' gains to marriage, as these predict that increases in female partners' socioeconomic opportunities can lead to an increase in husbands' use of violent threats associated with no physical abuse – consistent with the empirical evidence.

In 2006 however, the analogous longer-term estimates generally result in negative effect point estimates, although mostly small and insignificantly different from zero. The point estimates from our matching estimators range from reductions of 0.02 to 1.0 percentage points (1 to 54 percent) (column 8). In particular, our estimated emotional violence impact conditional on the woman not suffering physical violence suggests a reduction of 0.2 percentage points, or 7 percent (row 4). Again, in order to be more precise in comparing the estimated duration-specific program impacts, we compare the 90 and 95 percent confidence intervals for the period-specific estimates of emotional violence effects, conditional on no physical violence (Figure 1, Panel C). We can again reject the equality of the period-specific program impacts on physical violence at the 90 percent confidence level.^{17 18}

In conclusion, the evidence is quite suggestive of important differences in duration-specific program impacts on spousal abuse. While in 2003 Oportunidades seemed to have had opposing effects in the incidence of physical violence and emotional violence, these have been attenuated completely by 2006. We observe no longer-term effects of Oportunidades on spousal physical abuse rates or emotional violence associated with no physical violence.

C. Heterogeneous Responses based on Female Partner's Decision-Making Power

¹⁷ The 90 percent confidence intervals for the (conditional) emotional violence 2003 estimates is [0.015, 0.057], and that for the 2006 estimate is [-0.018, 0.013].

¹⁸ Again, the 2006 estimates are robust to case-control matching across villages – see estimates in Appendix Table A1, Panel B.

Our theory of instrumental violence in household decision-making also predicts that the effects should be concentrated among the subset of couples in which partners tend to behave according to traditional gender roles – in cases that male partners have greater decision-making power in the household, for instance. We test this hypothesis by estimating subgroup average program impacts for the main variables of interest for the couples in which female partners are categorized as having low or high decision-making power, respectively.

We use an index measure created by Casique (2006) and reported in Castro, Riquer, and Medina (2006) that aggregates the female partner's decision-making power based on responses to thirteen questions regarding which partner(s) contribute to the household's decisions in areas such as: (i) the female spouse's labor force participation decision; (ii) the household's expenditure decisions; (iii) decisions regarding when to have sexual relations; (iv) the use of contraceptives; and (v) fertility decisions.¹⁹ This index measure is then categorized as female partners having low or high decision-making power. See Casique (2006) for further details on the construction of this measure.

The results are reported in Table 6. We can clearly observe a distinction between the shorter-term and longer-term estimates – as the short-term relationships are concentrated among low female partner decision-making power households, whereas the longer-term effects do not differ for this group. Physical violence incidence rates in 2003 are lower among beneficiary households than among non-beneficiary households, and these effects are somewhat concentrated in reductions among females with low decision-making power within the household (Table 6, Panel A, row 1). The estimated reduction in the incidence of physical violence for this subgroup varies among specifications, ranging between 12.9 percentage points (138 percent; significant at 99 percent confidence) in the OLS specification, and 3.5-3.9 percentage points (38-42 percent; significant at 85 or 90 percent confidence) in the matching models specifications (row 1, columns 1-3). In contrast, the estimated reductions in physical violence for the subgroup of female partners with high decision-making power lie in the 1.0-4.0 percentage point (9-34 percent) range and are generally imprecisely estimated (row 1, columns 5-7).

We also find evidence that the 2003 program impacts on emotional violence are substantially greater among the sample of couples with low female partner decision-making power (Panel B). Among women with

¹⁹ The responses to these questions are coded in the following manner: decisions made solely by the male partner (= 0), jointly by both partners (= 1) or by the female partner (= 2). These are grouped based on a principal components analysis and aggregated into an index measure which lies in the [0,1] range using a weighted average; the weights are the proportion of the total variance explained by each of the components.

low decision- making power, the OLS point estimate indicates that the incidence of emotional violence increases by 5.4 percentage points (49 percent; not significantly different from zero) as a result of the program, and the matching estimates indicate an increase in the 5.1-7.7 percentage points (47-70 percent) range (significant at least at 90 percent confidence) (row 2, columns 1-3). Again, the estimated effects among women with high decision- making power are smaller, lying in the 0.7-3.4 percentage points (6-30 percent) range and are statistically indistinguishable from zero at conventional confidence levels (row 2, columns 5-7).

We also find a substantial and statistically significant short-term increase in the incidence of emotional violence conditioning on a lack of physical (or physical and sexual) violence concentrated among women with low decision-making power (Panel B). Our preferred matching estimate (with bias adjustment) shows an impact of 9.7 percentage points (significant at 99 percent confidence) among the former group and an impact of 2.3 percentage points among the latter group. The empirical evidence thus confirms this short-run substitution between physical and non-physical forms of spousal abuse, and in particular among the subsample of households where women report having low decision-making power.

In contrast, we find no evidence of any longer-term effects concentrated among couples in which women have low decision-making power (Table 6, Panels C and D). The comparable matching estimates for physical violence suggest increases of 0.9 to 1.4 percentage points, or 9-15 percent, in incidence (Panel C, row 1). The estimated impacts on emotional violence differ as well; the preferred estimate implies a reduction of 1.2 percentage points (18 percent), in the incidence of this type of abuse, and this is not precisely estimated. The analogous estimates for the incidence of emotional violence associated with no physical or sexual abuse are very small or in the opposite direction as shown in the short-term results. Also, all of these estimates are insignificantly different from zero (Panel D).²⁰

VI. Discussion of Main Results

Since the evidence is indicative of differences in duration-specific impacts among couples in union preceding the onset of the program, a second objective of the study is to explain the nature of this discrepancy. Given that we observe that across survey rounds beneficiary couples seem more likely to be composed of

²⁰ The estimates are robust to case-control matching within and across villages – see estimates in Appendix Table A2.

individuals with lower levels of socio-economic status (i.e., schooling, indigenous classification, family size), a first possibility is that marital selection – the types of couples remaining in a marital relationship as a result of the program – may play an important role in explicating these differential effects. As mentioned above, Bobonis (2009) finds in the short-term experimental evaluation of the PROGRESA/Oportunidades program, that couples eligible for the program experienced a modest increase in marital dissolution rates, with most of the effect concentrated among young and relatively educated women households.²¹ To the extent that changes in the composition of beneficiary households is partly driven by this process of marital selection, and that the effects among the remaining intact couples are weaker, these selection and treatment effect heterogeneity patterns could help explain the time path of spousal violence among beneficiary couples.

Moreover, these stark differences in the longitudinal pattern of program impacts suggests that the model of violence and household bargaining in Bobonis, Castro, and González-Brenes (2006) may correctly capture short-run interactions within the household, but may poorly capture longer-run interactions. Specifically, our model does not allow for endogenous divorce decisions of individual partners and subsequent future-period interactions, which we believe play an important role in explaining the results. Incorporating marital selection dynamics and possible general equilibrium impacts of improvements in female partners' improved socio-economics status may provide important insights and informative predictions about the sustainability of these social programs on spousal abuse patterns.

A second potential explanation – selective out-migration from rural areas – is consistent with evidence suggesting that migration towards urban areas among program beneficiaries has been more common for those households with higher educational attainment (Azuara 2009). Again, to the extent that the effects of the program are heterogeneous by these characteristics (or unobserved characteristics correlated with these measures of socio-economic status) selection patterns may result in varying treatment effects of the program among the remaining couples in rural areas.

VII. Evaluation of Explanations and Robustness Tests

A. Program Impacts Among Couples Forming Unions Following the Onset of Oportunidades

²¹ See Bowlus and Seitz (2006) for evidence on spousal abuse, divorce, and marital selection patterns in the Canadian context.

To address the potential marital selection argument, we informally claim that it is likely that marital dissolution patterns are concentrated among couples in which emotional violence incidence increases as a result of the program. If this were the case, these women may be able to attract or may be more willing to start new relationships with men with lower tendencies towards the use of emotional and/or physical abuse. These selective new relationship formation patterns among beneficiary women may lead to lower reported levels of violence among these couples.

In order to test this hypothesis, we study the union formation patterns among couples following the onset of Oportunidades; that is, those who enter a relationship since 1998 or later. Unfortunately, we do not have sufficiently detailed marital histories to identify which individuals are entering a new relationship following a previous dissolution, forcing us to pool together all relationships formed post-1997. Interestingly though, there seem to be clear differences in terms of the socio-economic characteristics of these more recently-formed couples: these are less likely to report that either individual speaks an indigenous language, and tend to be more educated (Table 2, Panels C and D). These patterns are consistent with some selection argument for these more recently formed couples.

We also find very suggestive evidence that reported levels of emotional violence among these couples are lower than those of non-beneficiary couples, although our sample sizes do not allow us to state any result with sufficient confidence (Table 7). While our preferred matching estimates suggest a slight increase in the incidence of physical abuse (insignificant at conventional confidence levels, row 1), the program impacts estimates on emotional violence – both unconditional and associated with no physical or sexual violence – suggest reasonable reductions in abuse (Panel A, row 3 and Panel B, rows 2-3).

B. Robustness to Unobserved Differences in Socio-Economic Status

Interpreting the variation in the assignment of beneficiaries and non-beneficiaries within villages as exogenous relies on certain assumptions, and violations of these could lead to mistaken conclusions regarding the tests presented above. We need to assume that the individual and household-level unobserved determinants of threats of and actual violence are uncorrelated with the assignment to being a program beneficiary, conditioning on those observed and common village factors. In this sub-section, we discuss an important

potential bias – unobserved differences in socio-economic status that influence patterns of familial violence – and present a test to assess the robustness of our estimates to more robust identifying assumptions.

Although we ultimately cannot rule out some degree of selection and omitted variable biases in our estimates, we take comfort in the fact that the theoretical predictions and our short-term set of results suggest a clear symmetry between the incidence of physical abuse on one hand and the incidence of emotional abuse and threats of violence on the other – a symmetry that is difficult to reconcile with monotonic selection bias arguments. For instance, the joint-cost hypothesis in non-cooperative bargaining models, which predicts that increasing the size of the surplus increases the opportunity cost for the aggressor to use violence, thus reducing its incidence (e.g., Kennan 1980) could lead to a downward bias (in absolute terms) on the estimated program impacts on physical abuse, but an upward bias on the emotional abuse effects. This would be the case, for instance, if beneficiary households had lower levels of asset-holdings, income, and/or gains to marriage than non-beneficiary households. Many other unobserved heterogeneity arguments would also not predict biases to go in opposite directions in the physical and emotional abuse empirical models.

We also report estimates from empirical models that condition or match across beneficiary and non-beneficiary households using observable contemporaneous asset-holdings and dwelling characteristics (Table 8). These measures – indicators for whether the household has a dirt floor, potable water, access to potable water in the village, electricity, telephone, radio, drainage, kitchen, as well as the number of rooms and the number of bedrooms in the dwelling – should capture the socio-economic gradient across households in these poor villages. We have somewhat less confidence in these estimates to the extent that these characteristics may be endogenous to the receipt of the program, as the program transfers may affect households' asset accumulation capabilities (Gertler, Martinez, and Rubio-Codina 2007). That said, the point estimates and precision of the matching estimates is not sensitive to this robustness check.

VII. Conclusions

The main objective of this paper is to provide evidence of the longer-term effects of the Oportunidades conditional cash transfer program on 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 (Bobonis, Castro and González-Brenes 2006; Rivera, Hernández and Castro 2006; Angelucci 2008).

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 five to nine years following the 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 estimates – where women in beneficiary households were significantly less likely to be victims of physical abuse but substantially more likely to suffer emotional abuse (with no associated physical abuse) than non-beneficiary women. 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.

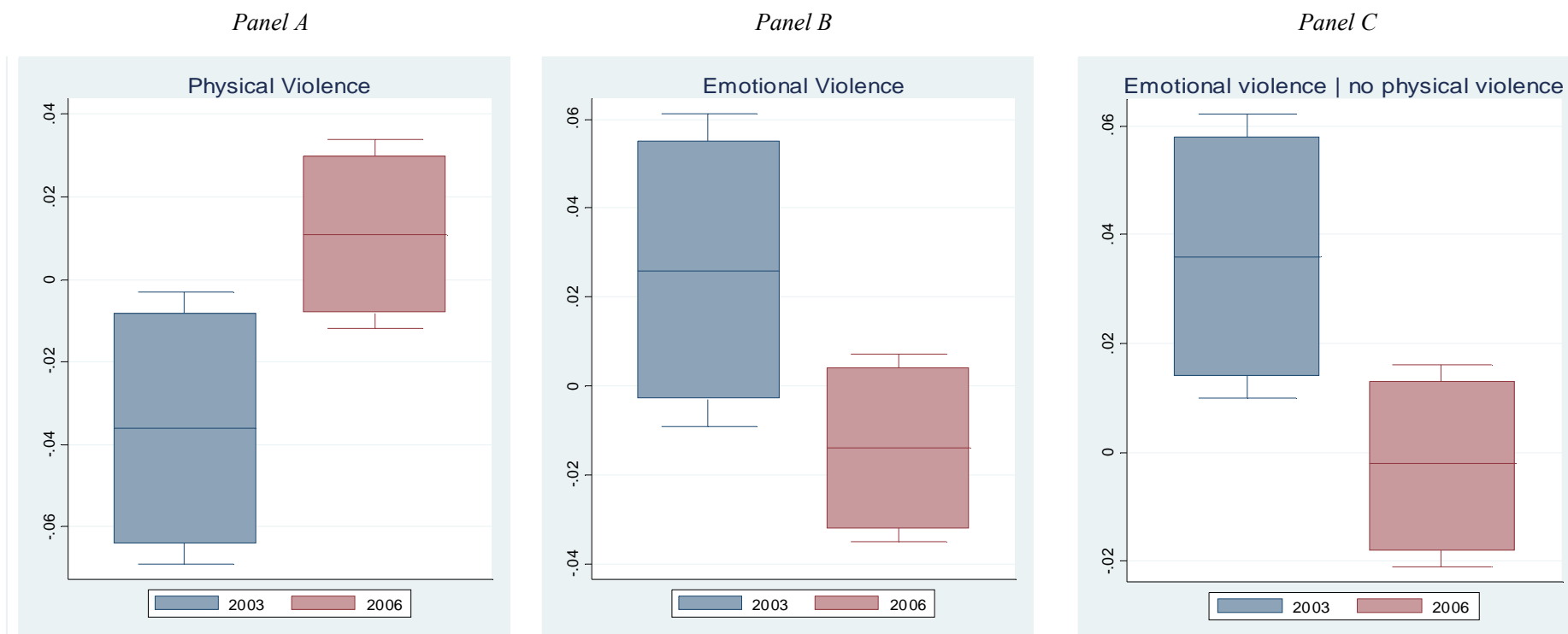
The article may have important implications for policy, since it provide a mixed view of conditional cash transfer programs' effectiveness in improving women's empowerment within the household. The program may, in the short-run, increase the likelihood of violent threats which may in turn compromise 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.

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Figure 1: Graphical Representation of Average Treatment Effects Estimates of Any Social Program on Physical and Emotional Violence



Notes: Each reported coefficient and 90 (box) / 95 (lines) percent confidence interval is from a different estimator. Reported are point estimates and confidence intervals of the social programs' average treatment effects on physical violence (Panel A), emotional violence (Panel B), and emotional violence conditional on the woman reporting no incidence of physical violence (Panel C). Estimates from non-parametric matching estimators weighted by survey sampling weights. Controls for matching estimators include indicator variables for woman and partner's age, indigenous status, household size, women's schooling-level indicators, cohabiting couple indicator (2003 only), and variables measuring reported histories of spousal abuse in parental household during childhood. The sample includes women ages 25 (28) and older in rural villages with children ages 10 (13) and younger in 2003 (2006).

Table 1: Description of Outcome Variables - Male-to-Female Spousal Abuse and Threats of Violence

Variable Name	Description	Sample Means			
		Survey Year 2003	Survey Year 2006		
		Couples In Union Since 1997 or Earlier	All Couples	Couples In Union Since 1997 or Earlier	Couples In Union Since 1998 or Later
Any violence	Indicator for any occurrence of physical, sexual, emotional, or economic abuse	0.40	0.31	0.32	0.34
Physical violence	Indicator for any occurrence of physical abuse (e.g., push, beating, attack with blade)	0.11	0.10	0.10	0.09
Sexual violence	Indicator for any occurrence of sexual abuse (e.g., use of force to have sexual relations)	0.09	0.07	0.07	0.04
Emotional violence	Indicator for any occurrence of psychological abuse, excluding perceptions questions (e.g., locked you in)	0.11	0.07	0.07	0.07
Threat of physical violence	Indicator for any occurrence of physical abuse threat (e.g., threat of leaving, threat w/ deadly weapon, threat to kill)	0.08	0.04	0.04	0.05
Observations		2867	4760	4240	520

Notes: Sample means weighted by inverse sampling weights. The sample includes couples with women ages 25 and older in rural villages with children ages 10 and younger in 2003, and couples with women ages 28 and older in rural villages with children ages 3-13 years in 2006.

Table 2: Comparison of Observable Characteristics, Beneficiary and Non-Beneficiary Households

Pre-treatment Covariate	Survey Year 2003				Survey Year 2006			
	Couples in Union Since 1997 or Earlier				All Couples			
	Mean		Difference		Mean		Difference	
	All	Beneficiaries of Any Program	All Non-Beneficiaries	Ben. - Non-Ben.	All	Beneficiaries of Any Program	All Non-Beneficiaries	Ben. - Non-Ben.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Panel A: Female Partner Characteristics								
Woman's age	34.9	35.0	34.8	0.27	37.4	37.9	36.4	1.53 ***
Indigenous woman	0.14	0.20	0.08	0.13 ***	0.16	0.22	0.06	0.16 ***
No schooling	0.08	0.12	0.05	0.07 ***	0.16	0.20	0.10	0.10 ***
Primary school	0.65	0.71	0.60	0.11 ***	0.57	0.61	0.49	0.12 ***
Middle school	0.18	0.14	0.22	-0.08 ***	0.20	0.16	0.25	-0.08 ***
Secondary school	0.04	0.02	0.06	-0.04 ***	0.04	0.02	0.09	-0.08 ***
Spousal violence in woman's childhood	0.10	0.10	0.10	0.00	0.17	0.17	0.18	-0.02
Panel B: Other Household Characteristics								
Partner's age	37.7	38.4	37.1	1.24 *	40.9	41.7	39.6	2.13 ***
Indigenous partner	0.14	0.20	0.09	0.11 ***	0.16	0.22	0.06	0.16 ***
Partner's schooling	5.7	5.0	6.4	-1.35 ***	5.2	4.4	6.7	-2.36 ***
Spousal violence in partner's childhood	0.18	0.15	0.21	-0.06 *	0.21	0.20	0.23	-0.03
Cohabiting couple	0.19	0.19	0.20	-0.01	n/a	n/a	n/a	n/a
Family size	5.8	6.3	5.4	0.94 ***	5.9	6.2	5.5	0.71 ***
Years in union	15.2	16.0	14.3	1.67 **	17.4	18.7	15.1	3.67 ***
Observations	2867				4760			

Notes: Sample means weighted by inverse sampling weights. Significant differences between beneficiary and non-beneficiary households at (*) 10%; (**) 5 percent, and (***) 1 percent levels, respectively. The sample includes couples with women ages 25 and older in rural villages with children ages 10 and younger in 2003, and couples with women ages 28 and older in rural villages with children ages 3-13 years in 2006.

Table 2: Comparison of Observable Characteristics, Beneficiary and Non-Beneficiary Households (cont.)

Pre-treatment Covariate	Survey Year 2006 Couples in Union Since 1997 or Earlier				Survey Year 2006 Couples in Union Since 1998 or Later			
	Mean		Difference		Mean		Difference	
	Beneficiaries of Any Program		All Non- Beneficiaries	Ben. - Non-Ben.	Beneficiaries of Any Program		All Non- Beneficiaries	Ben. - Non-Ben.
	All (1)	(2)	(3)	(4)	All (5)	(6)	(7)	(8)
<u>Panel C: Female Partner Characteristics</u>								
Woman's age	38.0	38.3	37.4	0.92	32.1	32.1	32.1	0.03
Indigenous woman	0.17	0.23	0.06	0.16 ***	0.06	0.11	0.03	0.08 ***
No schooling	0.17	0.20	0.12	0.09 ***	0.06	0.10	0.04	0.06 *
Primary school	0.58	0.62	0.51	0.11 ***	0.44	0.52	0.40	0.13 *
Middle school	0.18	0.15	0.24	-0.08 ***	0.31	0.33	0.30	0.03
Secondary school	0.03	0.01	0.07	-0.06 ***	0.12	0.03	0.17	-0.13 ***
Spousal violence in woman's childhood	0.16	0.16	0.18	-0.02	0.22	0.29	0.19	0.10
<u>Panel D: Other Household Characteristics</u>								
Partner's age	41.7	42.2	40.8	1.46 ***	34.2	33.9	34.4	-0.51
Indigenous partner	0.18	0.23	0.07	0.17 ***	0.06	0.09	0.04	0.04
Partner's schooling	5.0	4.3	6.4	-2.14 ***	7.2	5.6	8.0	-2.42 ***
Spousal violence in partner's childhood	0.21	0.20	0.23	-0.03	0.23	0.22	0.23	-0.01
Cohabiting couple	n/a	n/a	n/a	n/a	n/a	n/a	n/a	n/a
Family size	6.0	6.2	5.5	0.68 ***	5.3	5.6	5.1	0.42
Years in union	18.8	19.6	17.2	2.36 ***	5.7	5.7	5.7	0.02
Observations	4240				520			

Notes: Sample means weighted by inverse sampling weights. Significant differences between beneficiary and non-beneficiary households at (*) 10%; (**) 5 percent, and (***) 1 percent levels, respectively. The sample includes couples with women ages 28 and older in rural villages with children ages 3-13 years in 2006.

Table 3: Estimates of the Average Treatment Effect (ATE) of Any Social Program on Spousal Violence, All Couples

Dependent Variables	ATE Estimate (s.e.)										
	Survey Year 2006										
	OLS estimate		Matching est.		Matching est.		Matching est.		Matching est.		Mean of dep. variable
ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Violent actions											
Any violence	0.029 (0.027)	9%	-0.021 (0.018)	-7%	-0.006 (0.018)	-2%	-0.047** (0.019)	-15%	-0.038** (0.019)	-12%	0.315
Physical violence	0.008 (0.022)	8%	0.015 (0.011)	15%	0.020* (0.011)	21%	-0.013 (0.012)	-13%	-0.006 (0.012)	-6%	0.099
Sexual violence	0.009 (0.018)	13%	-0.004 (0.008)	-6%	0.009 (0.008)	13%	-0.010 (0.011)	-14%	-0.002 (0.011)	-2%	0.068
Emotional violence	0.007 (0.019)	9%	-0.014 (0.010)	-20%	-0.011 (0.010)	-16%	-0.017* (0.010)	-24%	-0.012 (0.010)	-18%	0.070
Village Fixed Effects (or Matching within Village)	Yes		Yes		Yes		No		No		
Additional asset controls	No		No		No		No		No		
Bias Adjustment	-		Yes		No		Yes		No		
Observations	4760		4760		4760		4760		4760		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects OLS regressions and non-parametric matching estimators weighted by survey sampling weights. Controls for OLS regression and matching estimators include indicator variables for woman and partner's age, indigenous status, household size, women's schooling-level indicators, cohabiting couple indicator (2003 only), and variables measuring reported histories of spousal abuse in parental household during childhood. The sample includes women ages 25 (28) and older in rural villages with children ages 10 (13) and younger in 2003 (2006).

Table 3: Estimates of the Average Treatment Effect (ATE) of Any Social Program on Spousal Violence, All Couples (cont.)

Dependent Variables	ATE Estimate (s.e.)										
	Survey Year 2006										
	OLS estimate		Matching est.		Matching est.		Matching est.		Matching est.		Mean of dep. variable
ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel B: Threats of violence											
Threat of violence	0.008 (0.017)	20%	0.007 (0.008)	16%	0.008 (0.008)	20%	0.007 (0.007)	17%	0.009 (0.007)	22%	0.043
Threat no physical violence	0.009 (0.010)	54%	0.001 (0.007)	4%	0.001 (0.007)	6%	0.002 (0.005)	15%	0.002 (0.005)	14%	0.017
Threat no physical or sexual violence	0.001 (0.009)	7%	-0.006 (0.006)	-48%	-0.004 (0.006)	-37%	-0.001 (0.004)	-10%	-0.001 (0.004)	-11%	0.012
Emotional violence no physical violence	0.022* (0.012)	67%	-0.002 (0.009)	-5%	-0.003 (0.009)	-9%	-0.002 (0.008)	-5%	-0.002 (0.008)	-5%	0.033
Emotional violence no physical or sexual violence	0.008 (0.011)	34%	-0.011 (0.009)	-45%	-0.011 (0.009)	-46%	-0.009 (0.008)	-39%	-0.009 (0.008)	-37%	0.024
Village Fixed Effects (or Matching within Village)	Yes		Yes		Yes		No		No		
Additional asset controls	No		No		No		No		No		
Bias Adjustment	-		Yes		No		Yes		No		
Observations	4760 / 4299 / 4123		4760 / 4299 / 4123		4760 / 4299 / 4123		4760 / 4299 / 4123		4760 / 4299 / 4123		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects OLS regressions and non-parametric matching estimators weighted by survey sampling weights. Controls for OLS regression and matching estimators include indicator variables for woman and partner's age, indigenous status, household size, women's schooling-level indicators, cohabiting couple indicator (2003 only), and variables measuring reported histories of spousal abuse in parental household during childhood. The sample includes women ages 25 (28) and older in rural villages with children ages 10 (13) and younger in 2003 (2006).

Table 4: Estimates of the ATE of Any Social Program on Spousal Violence, Couples in Union Since 1997 or Earlier

Dependent Variables	ATE Estimate (s.e.)									
	Survey Year 2003					Survey Year 2006				
	OLS estimate		Matching est.		Mean of dep. var.	OLS estimate		Matching est.		Mean of dep. var.
	ATE	%Δ	ATE	%Δ		ATE	%Δ	ATE	%Δ	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Any violence	-0.029 (0.053)	-7%	-0.045 ⁺ (0.031)	-11%	0.395	0.030 (0.030)	9%	-0.031 ⁺ (0.019)	-10%	0.320
Physical violence	-0.060* (0.035)	-55%	-0.036** (0.017)	-33%	0.108	0.009 (0.025)	9%	0.011 (0.012)	11%	0.096
Sexual violence	-0.066** (0.029)	-73%	-0.022 (0.016)	-24%	0.090	0.009 (0.021)	13%	-0.001 (0.009)	-1%	0.068
Emotional violence	0.041 (0.036)	36%	0.026 (0.018)	-23%	0.113	0.004 (0.021)	7%	-0.014 (0.011)	-21%	0.068
Village Fixed Effects (or Matching within Village)	Yes		Yes			Yes		Yes		
Bias Adjustment	-		Yes			-		Yes		
Observations	2867		2867			4240		4240		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects OLS regressions and non-parametric matching estimators weighted by survey sampling weights. Controls for OLS regression and matching estimators include indicator variables for woman and partner's age, indigenous status, household size, women's schooling-level indicators, cohabiting couple indicator (2003 only), and variables measuring reported histories of spousal abuse in parental household during childhood. The sample includes women ages 25 (28) and older in rural villages with children ages 10 (13) and younger in 2003 (2006), in the current relationship since 1997 or earlier.

Table 5: Estimates of the ATE of Any Social Program on Threats of and Emotional Violence, Couples in Union Since 1997 or Earlier

Dependent Variables	ATE Estimate (s.e.)									
	Survey Year 2003					Survey Year 2006				
	OLS estimate		Matching est.		Mean of dep. var.	OLS estimate		Matching est.		Mean of dep. var.
ATE	%Δ	ATE	%Δ	ATE		%Δ	ATE	%Δ		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Threat of violence	0.004 (0.036)	6%	0.010 (0.021)	5%	0.079	0.007 (0.020)	18%	0.004 (0.009)	11%	0.038
Threat no physical violence	0.024 (0.020)	64%	0.016 (0.016)	81%	0.037	0.010 (0.010)	61%	0.000 (0.007)	-1%	0.016
Threat no physical or sexual violence	0.021 (0.019)	62%	0.009 (0.014)	74%	0.034	0.001 (0.008)	11%	-0.006 (0.007)	-54%	0.010
Emotional violence no physical violence	0.040* (0.022)	63%	0.036*** (0.013)	79%	0.063	0.016 (0.012)	51%	-0.002 (0.009)	-7%	0.032
Emotional violence no physical or sexual violence	0.034+ (0.023)	67%	0.018 (0.015)	78%	0.051	0.008 (0.012)	38%	-0.010 (0.009)	-46%	0.022
Village Fixed Effects (or Matching within Village)	Yes		Yes			Yes		Yes		
Bias Adjustment	-		Yes			-		Yes		
Observations	2611 / 2479		2611 / 2479			3834 / 3670		3834 / 3670		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. Coefficient estimates from village fixed effects OLS regressions and non-parametric matching estimators weighted by survey sampling weights. Controls for OLS regression and matching estimators include indicator variables for woman and partner's age, indigenous status, household size, women's schooling-level indicators, cohabiting couple indicator (2003 only), and variables measuring reported histories of spousal abuse in parental household during childhood. The sample includes women ages 25 (28) and older in rural villages with children ages 10 (13) and younger in 2003 (2006), in the current relationship since 1997 or earlier.

Table 6: Estimates of Program Impacts by Category of Female Partner’s Decision-Making Power, Couples In Unions Since 1997 or Earlier

Dependent Variables	Sub-Group ATE Estimates (s.e.)							
	Survey Year 2003							
	Female Partners with Low Decision-Making Power				Female Partners with High Decision-Making Power			
	OLS	Matching Estimates		Mean of dep. var.	OLS	Matching Estimates		Mean of dep. var.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
<u>Panel A: Violent actions</u>								
Physical violence	-0.129*** (0.050) -138%	-0.035+ (0.024) -38%	-0.039* (0.024) -42%	0.093	-0.028 (0.037) -24%	-0.040+ (0.025) -34%	-0.010 (0.025) -9%	0.116
Emotional violence	0.054 (0.064) 49%	0.077*** (0.028) 70%	0.051* (0.028) 47%	0.109	0.034 (0.041) 30%	0.027 (0.031) 23%	0.007 (0.031) 6%	0.115
<u>Panel B: Threats of violence</u>								
Emotional violence no physical violence	0.095* (0.052) 165%	0.097*** (0.022) 168%	0.059*** (0.022) 102%	0.058	0.030 (0.039) 45%	0.023 (0.024) 36%	-0.001 (0.024) -2%	0.066
Emotional violence no physical or sexual viol.	0.074 (0.054) 141%	0.042** (0.020) 79%	0.035* (0.020) 67%	0.052	0.043 (0.032) 86%	0.022 (0.022) 44%	0.000 (0.022) -1%	0.050
Village Fixed Effects (or Matching within Village)	Yes	Yes	Yes		Yes	Yes	Yes	
Bias Adjustment	-	Yes	No		-	Yes	No	
Observations	1110 / 1003 / 939				1757 / 1608 / 1540			

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (+) 85 percent, (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. See notes to Tables 4 and 5 for details on the .specifications, the set of controls, and the samples.

Table 6: Estimates of Program Impacts by Category of Female Partner’s Decision-Making Power, Couples in Union Since 1997 or Earlier (cont.)

Dependent Variables	Sub-Group ATE Estimates (s.e.)							
	Survey Year 2006							
	Female Partners with Low Decision-Making Power				Female Partners with High Decision-Making Power			
	OLS	Matching Estimates		Mean of dep. var.	OLS	Matching Estimates		Mean of dep. var.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
<u>Panel C:</u> Violent actions								
Physical violence	0.002 (0.033) 2%	0.009 (0.015) 9%	0.014 (0.015) 15%	0.095	0.045 (0.059) 41%	0.035* (0.020) 32%	0.041** (0.020) 37%	0.111
Emotional violence	-0.021 (0.028) -32%	-0.012 (0.015) -18%	-0.007 (0.015) -10%	0.067	0.061 (0.045) 79%	0.018 (0.016) 22%	0.015 (0.016) 19%	0.078
<u>Panel D:</u> Threats of violence								
Emotional violence no physical violence	0.008 (0.019) 23%	0.001 (0.011) 3%	0.000 (0.011) 0%	0.035	0.024 (0.027) 81%	0.013 (0.011) 44%	0.005 (0.011) 17%	0.030
Emotional violence no physical or sexual viol.	0.004 (0.019) 17%	-0.012 (0.011) -49%	-0.011 (0.011) -46%	0.025	0.012 (0.022) 56%	0.009 (0.011) 43%	0.003 (0.011) 16%	0.022
Village Fixed Effects (or Matching within Village)	Yes	Yes	Yes		Yes	Yes	Yes	
Bias Adjustment	-	Yes	No		-	Yes	No	
Observations	2713 / 2444 / 2318				1527 / 1390 / 1352			

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (+) 85 percent, (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. See notes to Tables 4 and 5 for details on the .specifications, the set of controls, and the samples.

Table 7: Estimates of Program Impacts by Category of Female Partner’s Decision-Making Power, Couples In Unions Since 1998 or Later

Dependent Variables	ATE Estimate (s.e.)										
	Survey Year 2006						Survey Year 2006				
	OLS estimate		Matching est.		Matching est.		Matching est.		Matching est.		Mean of dep. variable
	ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	
Panel A: Violent actions											
Physical violence	-0.121 (0.163)	-132%	0.020 (0.056)	22%	0.054 (0.056)	59%	0.037 (0.030)	41%	0.025 (0.030)	28%	0.092
Sexual violence	0.017 (0.113)	46%	-0.004 (0.023)	-10%	0.003 (0.023)	8%	0.006 (0.015)	15%	-0.001 (0.015)	-4%	0.036
Emotional violence	-0.111 (0.132)	-150%	-0.042 (0.026)	-56%	-0.039 (0.026)	-53%	-0.030 (0.022)	-40%	-0.047** (0.022)	-64%	0.074
Panel B: Threats of violence											
Threat of violence	-0.050 (0.173)	-92%	-0.013 (0.033)	-24%	0.043 (0.033)	79%	0.022 (0.023)	40%	0.001 (0.023)	1%	0.054
Emotional violence no physical violence	0.064 (0.107)	176%	-0.010 (0.012)	-27%	-0.013 (0.012)	-36%	-0.023 (0.015)	-65%	-0.028* (0.015)	-77%	0.036
Emotional violence no physical or sexual violence	0.013 (0.053)	61%	-0.013 (0.009)	-62%	-0.014 (0.009)	-65%	-0.019 (0.012)	-88%	-0.025** (0.012)	-117%	0.022
Village Fixed Effects (or Matching within Village)	Yes		Yes		Yes		No		No		
Additional asset controls	No		No		No		No		No		
Bias Adjustment	-		Yes		No		Yes		No		
Observations	520 / 465 / 453		520 / 465 / 453		520 / 465 / 453		520 / 465 / 453		520 / 465 / 453		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (+) 85 percent, (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. See notes to Tables 4 and 5 for details on the .specifications and the set of controls. The sample includes women ages 25 (28) and older in rural villages with children ages 10 (13) and younger in 2003 (2006), in the current relationship since 1998 or later.

Table 8: Robustness Checks of Program Impacts, Asset Characteristics Controls

Dependent Variables	ATE Estimate (s.e.)									
	Survey Year 2003					Survey Year 2006				
	OLS estimate		Matching est.		Mean of dep. variable	OLS estimate		Matching est.		Mean of dep. variable
	ATE	%Δ	ATE	%Δ		ATE	%Δ	ATE	%Δ	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Panel A: Violent actions										
Physical violence	-0.061*	-56%	-0.058***	-54%	0.108	0.006	6%	0.007	8%	0.096
	(0.035)		(0.020)			(0.025)		(0.012)		
Sexual violence	-0.061**	-68%	-0.017	-19%	0.090	0.007	11%	-0.013 ⁺	-20%	0.068
	(0.029)		(0.014)			(0.021)		(0.009)		
Emotional violence	0.031	27%	-0.010	-9%	0.113	0.006	9%	-0.012	-17%	0.068
	(0.033)		(0.021)			(0.021)		(0.009)		
Panel B: Threats of violence										
Emotional violence no physical violence	0.044 ⁺	70%	0.025*	40%	0.063	0.018	56%	0.000	0%	0.032
	(0.031)		(0.014)			(0.013)		(0.006)		
Emotional violence no physical or sexual violence	0.050*	98%	0.026*	51%	0.051	0.010	46%	-0.010*	-48%	0.022
	(0.027)		(0.014)			(0.012)		(0.006)		
Village Fixed Effects (or Matching within Village)	Yes		Yes			Yes		Yes		
Additional asset controls	Yes		Yes			Yes		Yes		
Bias Adjustment	-		Yes			-		Yes		
Observations	2867		2867			4240		4240		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (+) 85 percent, (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. See notes to Tables 4 and 5 for details on the .specifications, the set of controls, and the samples. Additional asset controls are a dirt floor dwelling indicator, access to telephone and electricity indicators, radio ownership, and number of bedrooms.

Appendix Table 1: Estimates of the ATE of Any Social Program on Spousal Violence, Couples in Union Since 1997 or Earlier, Comparison of Matching Estimates

Dependent Variables	ATE Estimate (s.e.)								
	Survey Year 2006								
	Matching est.		Matching est.		Matching est.		Matching est.		Mean of dep. var.
ATE	%Δ	ATE	%Δ	ATE	%Δ	ATE	%Δ		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Violent actions									
Any violence	-0.019 (0.019)	-6%	-0.031 (0.019)	-10%	-0.041** (0.021)	-13%	-0.043** (0.021)	-14%	0.320
Physical violence	0.020* (0.012)	21%	0.011 (0.012)	11%	-0.004 (0.013)	-5%	-0.010 (0.013)	-10%	0.096
Sexual violence	0.011 (0.009)	16%	-0.001 (0.009)	-1%	-0.003 (0.012)	-4%	-0.011 (0.012)	-16%	0.068
Emotional violence	-0.009 (0.011)	-13%	-0.014 (0.011)	-21%	-0.007 (0.010)	-11%	-0.012 (0.010)	-17%	0.068
Panel B: Threats of violence									
Threat of violence	0.004 (0.009)	11%	0.004 (0.009)	11%	0.013 (0.007)	34%	0.010 (0.007)	27%	0.038
Threat no physical violence	0.000 (0.007)	-1%	0.000 (0.007)	-1%	0.004 (0.005)	23%	0.004 (0.005)	23%	0.016
Threat no physical or sexual violence	-0.006 (0.007)	-54%	-0.006 (0.007)	-54%	0.000 (0.005)	-2%	0.000 (0.005)	-4%	0.010
Emotional violence no physical violence	-0.002 (0.009)	-7%	-0.002 (0.009)	-7%	-0.002 (0.009)	-5%	-0.001 (0.009)	-3%	0.032
Emotional violence no physical/sexual violence	-0.010 (0.009)	-46%	-0.010 (0.009)	-46%	-0.012 (0.009)	-55%	-0.012 (0.009)	-57%	0.022
Village Fixed Effects (or Matching within Village)	Yes		Yes		No		No		
Bias Adjustment	No		Yes		No		Yes		
Observations	4240		4240		4240		4240		

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. See notes to Tables 4 and 5 for details on the .specifications, the set of controls, and the samples.

Appendix Table 2: Estimates of the ATE of Any Social Program by Woman’s Decision-Making Power, Couples in Union Since 1997 or Earlier, Comparison of Matching Estimates

Dependent Variables	Sub-Group ATE Estimates (s.e.)							
	Survey Year 2006							
	Female Partners with Low Decision-Making Power				Female Partners with High Decision-Making Power			
	OLS	Matching Estimates		Mean of dep. var.	OLS	Matching Estimates		Mean of dep. var.
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
<u>Panel A:</u> Violent actions								
Physical violence	0.002 (0.033) 2%	-0.008 (0.016) -9%	-0.007 (0.016) -8%	0.095	0.045 (0.059) 41%	0.006 (0.020) 32%	0.018 (0.020) 37%	0.111
Emotional violence	-0.021 (0.028) -32%	-0.026* (0.014) -39%	-0.017 (0.014) -26%	0.067	0.061 (0.045) 79%	0.009 (0.016) 12%	0.016 (0.016) 20%	0.078
<u>Panel B:</u> Threats of violence								
Emotional violence no physical violence	0.008 (0.019) 23%	-0.009 (0.012) -25%	-0.007 (0.012) -21%	0.035	0.024 (0.027) 81%	0.018* (0.011) 61%	0.014 (0.011) 48%	0.030
Emotional violence no physical or sexual viol.	0.004 (0.019) 17%	-0.020 (0.013) -80%	-0.018 (0.013) -71%	0.025	0.012 (0.022) 56%	0.003 (0.011) 16%	0.003 (0.011) 14%	0.022
Village Fixed Effects (or Matching within Village)	Yes	No	No		Yes	No	No	
Additional asset controls	No	No	No		No	No	No	
Bias Adjustment	-	Yes	No		-	Yes	No	
Observations	2713 / 2444 / 2318				1527 / 1390 / 1352			

Notes: Each reported coefficient is from a different estimator. Robust standard errors in parentheses; significant at (*) 90 percent, (**) 95 percent, (***) 99 percent confidence levels. See notes to Tables 4 and 5 for details on the .specifications, the set of controls, and the samples.