The Effects of Migration on Intimate Partner Violence: Evidence for Exposure Reduction Theory in Bangladesh¹

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Abstract

This paper studies the effects of seasonal migration incentives on intimate partner violence. There are at least three reasons why migration may have consequences for violence among the ultra-poor. First, migration induces a positive income shock that lessens poverty-stressors and, with it, violence. Second, the extra-income associated with the migration of the male spouse increases his bargaining power and lessens woman's say in the household decisions. Third, when the man migrates, the woman spends less time with her potential perpetrator and faces a lower risk of victimization. To address the net effect of migration on violence, we collected data on intimate partner violence among the female respondents of the No Lean Season intervention 2017 program-year endline survey. Our results indicate that 77% of these women have experienced violence at the hands of an intimate partner during their lifetime, and almost one in every two has been physically or sexually abused by her partner in the last six months. Our analysis suggests that migration reduces violence. As such, women of households receiving interest-free migration loans are less likely to experience physical or sexual violence. Violence, however, is higher in villages that did not receive the migration incentive but are surrounded by villages that did receive it. We hypothesize this increase is explained by the fact that, when the male spouse is less likely to migrate, the woman spends more time with the perpetrator of violence. The evidence we provide is in line with exposure reduction theory and suggests that seasonal migration has the potential to improve well-being by providing women with periods of reduced violence throughout the year.

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1. Introduction

This paper studies the effects of seasonal migration on intimate partner violence among the ultra-poor in rural Bangladesh. This is a region of the world where three out of every four women are abused by a romantic partner at some point of their life, and half were abused in the last year (Bangladesh Bureau of Statistics, 2013). Physical and/or sexual violence from hands of an intimate partner are rampant throughout the entire country with 54% of the women experiencing either form of violence during their lifetime, and 27% in the last year (Bangladesh Bureau of Statistics, 2013).² In Bangladesh, violence against the women is exacerbated by traditional gender norms that prescribe the man's role as the breadwinner, limiting the woman's economic independence (Koenig, Ahmed, Hossain, & Mozumber, 2003; Heath, 2014; Schuler & Nazneen, 2018). Child marriage (Yount, et al., 2016), female seclusion (Amin, 1997), and high acceptance of wife beating (Bangladesh Demographic and Health Survey 2007; Biswas, Rahman, Kabir, & Raihan, 2017; Fattah & Camelli, 2017) represent just some of the prevalent norms that dampen Bangladeshi women's wellbeing.

Although intimate partner violence is a worldwide phenomenon (Devries, et al., 2013), it is particularly harmful among the ultra-poor. In the case of rural Bangladesh, seasonal income insecurity and poverty are key factors which make victims and survivors of violence highly vulnerable. Every year, between the planting and the harvest season of rice, the demand for agricultural labor falls and the price of food increases leading to hunger (Khandker, 2012). As a coping strategy in face of this *lean season*, some families send their members to other areas of the country for work.³ Yet not all the households that could benefit from seasonal migration do so. The migration experiment of Bryan, Chowdhury, & Mobarak (2014) suggests that households that are close to their subsistence level view migration as a risky endeavour as, if costs outweigh benefits, the household could go below their subsistence level. A small migration grant covering the round-trip bus ticket can induce 22% of households to engage in seasonal migration resulting in increases in household food and non-food expenditures by 30% and increased per-person daily caloric intake by 500-700 kcal (Bryan, Chowdhury, & Mobarak,

² Similar figures are found in the Violence Against the Women Survey of 2011 (57.8 prevalence of physical or sexual violence ever, and 37% prevalence in the last twelve months), the Demographic Health Surveys of 2007 (57.8 prevalence of physical and/or sexual violence ever, and 37% prevalence in the last twelve months) (53.3 prevalence of physical and/or sexual violence ever, and 23.9% prevalence in the last twelve months), and the World Health Organization Multi-Country Study of 2006 (61.7 prevalence of physical or sexual violence ever in Bangladesh provinces, and 37% prevalence in the last twelve months) (NIPORT, Mitra and Associates, and ICF International., 2009; Bangladesh Bureau of Statistics, 2013; Garcia-Moreno, Jansen, Ellsberg, Heise, & Watts, 2006)

³ Khandker, Khaleque, & Samad (2011)'s survey suggests that 34% to 38% of households in northern rural Bangladesh use seasonal migration as a copying strategy during the lean season.

2014). Migration incentives also generate spillover effects, both among the beneficiary and the non-beneficiary households. By varying the share of households in the village offered the migration grant, a second round of the experiment documents that, the larger the number of neighbours receiving the grant, the higher the likelihood a family sends a seasonal migrant; and a 10% increase in emigration leads to a 1.5% to 2.2% increase in agricultural wages (Akram, Chowdhury, & Mobarak, 2018). Moreover, for every subsequent 10% increase in emigration agricultural wages increase by 1.5-2.2%. Notwithstanding, migration does not always come with large welfare gains as it can generate non-monetary disutility for the migrant himself (Lagakos, Mobarak, & Waugh, 2017).

This paper delves into the welfare effects of incentives for seasonal migration and their effects on the wellbeing of those who do not migrate, in particular on women. Specifically, we are interested in understanding how seasonal migration affects the prevalence of intimate partner violence among the ultra-poor. There are at least three reasons why migration can affect violence among this demographic. First, *absolute resource theory* posits that an income shock increasing the overall resources available to the family reduces poverty-stressors and with it, violence (Fox, Benson, DeMaris, & Van Wyk, 2004; Cools & Kotsadam, 2017). In contrast, *bargaining theory* would predict that households that send the male spouse as a seasonal migrant confer on him a higher bargaining power which could potentially increase violence. Third, *exposure reduction theory* suggests that women in households with a seasonal migrant are less likely to be abused by the mere fact that they spend less time with the potential perpetrator (Dugan, Nagin, & Rosenfeld, 1999, 2003). Since the predictions of the three mechanisms point at different directions, the net effect of migration on violence remains an empirical issue.

To quantify the effect of seasonal migration on intimate partner violence (IPV), we rely on the experimental variation induced by the *No Lean Season* (NLS) intervention. Based on the original migration experiment of Bryan, Chowdhury, & Mobarak (2014) and the subsequent research of migration subsidies, the NLS intervention offers poor families in rural Bangladesh interest free migration loans.⁶ In 2017, the NLS program offered migration loans

⁴ This is especially true in contexts with traditional gender roles such as Bangladesh, where the man's inability to provide the household basic needs threatens the notion of masculinity.

⁵ Because income shocks affects both the outside marriage utility and how resources are allocated within the household (Manser & Brown, 1980; McElroy & Horney, 1981; Lundberg & Pollak, 1996); if the male spouse derives direct utility from violence, the fact that he earns more improves his ability to purchase violence either by offering her a larger transfer in compensation (Farmer & Tiefenthaler, 1997) or by increasing his say in the overall household utility (Ramos, 2018).

⁶ By 2021, the program plans to disburse more than 200,000 migration incentives. See http://yrise.yale.edu/featured-projects/ for further details and projections.

of 20 dollars to roughly 160,000 landless households located in 700 villages in Northern Rangpur. These villages were randomly selected to receive the migration incentive and the eligible households in the designated villages were offered the loan. To study spillover effects onto nearby villages, the intervention randomly varied the share of neighboring villages receiving the migration incentive. Given the large scale of the program, only one-third of the villages were interviewed (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018).

We collected data on IPV among the women that responded to the NLS endline survey. Given the endorsement of violence against the women and the culture of silence in the Bangladeshi context (Naved, Talukder, Khan, & Farhad, 2013), we purposely designed the data collection to minimize potential risks both for the respondents and the fieldworkers. With our strategy, we attained an 84% response rate for a sample of 2,848 respondents. Similar to national figures, we document a lifetime prevalence of IPV of 76% and a 35% prevalence of physical and/or sexual violence in the six months preceding the interview. These figures reinforce the rampant nature of the IPV in rural Bangladesh and the urgent need for understanding how different policies could affect it.

Overall, our results indicate that migration reduces violence. Relative to households located in villages not receiving migration loans and not surrounded by other villages receiving the incentive – the control group –, women in beneficiary households are 3.5 percentage points less likely to experience physical or sexual violence in the last six months. Violence, however, is higher at non-beneficiary households whose neighboring villages received the migration incentive – spillover villages. Among these women, physical or sexual violence increases by 5 to 7 percentage points. We explain these experimental effects through exposure reduction theory and hypothesize that the lower migration rate of spillover villages forces women to spend more time with the perpetrators, leading to an increase in violence. To support our hypothesis, we estimate the effect of migration on violence using the random assignment to the NLS intervention as an instrument. In the six months preceding the IPV survey, 16% of the families in the control group sent their male head of household as a seasonal migrant. While in villages receiving the incentive the NLS intervention induced the migration of 4.7 percent of the families; in spillover villages migration dropped by 4.4 percentage points. Once we account for induced in and out-migration, we conclude that migration eliminates physical and/or sexual violence almost mechanically.

We believe these findings provide causal evidence supporting exposure reduction theory. The results complement the existing evidence from the criminology literature on female homicides and legally binding exposure reduction measures (Dugan, Nagin, & Rosenfeld, 1999, 2003; Reckdenwald & Parker, 2012), showing that even temporary and voluntary measures such as migration reduce violence. Within economics literature, we contribute to the understanding of how employment interacts with violence in the household. While this strand of the literature emphasizes the role of female employment (Aizer, 2010; Heath, 2014; Anderberg, Rainer, Wadsworth, & Wilson, 2016; Alonso-Borrego & Carrasco, 2017), we add to the debate by quantifying an alternative mitigation strategy: male employment reduces female's exposure to the potential perpetrator. Our work is closest to Chin (2012) who acknowledges the triple effect of female employment. First, labour income reduces the woman's economic dependence on her partner and increases her bargaining power. Second, her employment outside the household lessens the time she spends with the potential perpetrator. Third, female insertion in the labour force challenges the traditional gender roles and can induce male backlash (Macmillan & Gartner, 1999). Although Chin (2012) finds that, for the Indian context, the exposure reduction effect dominates; quantifying the extent to which each of the three underlying forces contributes to the observed prevalence of violence remains a challenge.

More broadly, this paper contributes to the empirical literature identifying what works for the prevention and reduction of intimate partner violence (see, for instance, Ellsberg, et al., (2015); Michau, Horn, Bank, Dutt, & Zimmerman (2015); or Bourey, Williams, Bernstein, & Stephenson (2015) for systematic reviews). A handful of the evidence for the developing world comes from interventions giving social assistance to women upon certain conditionalities (e.g. Buller, Hidrobo, Peterman, & Heise (2016); Angelucci (2008); Bobonis, González-Brenes, & Castro (2013); Hidrobo & Fernald (2013); Hidrobo, Hoddinott, Peterman, Margolies, & Moreira (2014); Hidrobo, Peterman, & Heise (2016); Roy, Hidrobo, Hoddinott, & Ahmed (2018)). Because these intervention are explicitly targeted to women,⁷ they can challenge men's traditional gender role as breadwinner and their net effect should be considered as a lower bound of the overall effect of the transfer. In this paper, we think about interest-free migration loans as an in-kind transfer to the household—rather than a transfer to an individual—that the family can freely assign within its members. In this regard, the magnitude of the effects presented here are directly interpretable as, at least in the short term, they are not shifting the prevailing gender norms.

⁷ An exception is Haushofer, Shapiro, Ring, & Wang (2018).

All in all, the evidence suggests that seasonal migration has the potential to improve women's wellbeing by providing them with periods of reduced violence throughout the year. Our results carry important implications for policy as the improvements occur through a rather mechanical effect of reduced exposure to the perpetrator. Whether or not violence returns to its original levels once the migration episode ends still needs to be addressed in future rounds of data collection and the continued scale-up of the intervention.

The rest of the paper is organized as follow. Section 2 presents a panorama of IPV and gender norms in the Bangladeshi context. In sections 3 and 4 we provide further details on the source of variation induced by the NLS intervention and the protocols for intimate partner violence data collection. Section 5 describes the sample. Section 6 characterizes the patterns of violence in the sample. Section 7 presents the results. Section **Error! Reference source not found.** concludes.

2. Context

We conducted our research in the district of Rangpur in Northern Bangladesh. As in the rest of the country, intimate partner violence is rampant in Rangpur. According to the latest Violence Against Women survey, 60% of women in the district have been physically or sexually abused by an intimate partner during their lifetime and 32% have been abused in the last 12 months (Bangladesh Bureau of Statistics, 2016). The lifetime prevalence of sexual violence is strikingly high at 34%, making it the highest across Bangladesh.⁸ In spite of these high prevalence rates of violence, a cultural norm of silence prevails. For every ten women physically and/or sexually abused by their intimate partners, seven never disclosed their experiences; mainly because they did not consider necessary to report, because they did not want to bring shame to the family, or because they were afraid of their partner's response (Bangladesh Bureau of Statistics, 2016). The fact is that at least one quarter of married women in Rangpur consider wife is beating an acceptable practice, either as a coping strategy or as a social norm (Bangladesh Demographic Health Survey 2014; Biswas, Rahman, Kabir, & Raihan, 2017; Yount, Halim, Schuler, & Head, 2013). Not surprisingly, 74% of men in the country report being violent against their partners at some point of their life and consider such practice is justifiable upon certain circumstances (Johnson & Das, 2009; Akhter & Wilson, 2016).

⁸ In Rajshahi and Rangpur, the lifetime prevalence of sexual IPV is 34.2 and 34.1% (Bangladesh Bureau of Statistics, 2016).

Besides gender-based violence, other patriarchal gender norms prevail in the rural areas of the country. Bangladesh has the highest rate of early marriage in South Asia and the second highest in the world with almost 80% of the women being married before age 18 and around 40% before age 15 (Solotaroff & Pande, 2014; Yount, et al., 2016). In the rural context, marriage is generally perceived as a transaction between the father and the bridegroom's family meant to release the parental household from a burden (Chowdhury, 2004). Dowry agreements are also a common form of extracting resources from the bride's family, notably affecting disadvantaged families (Bates, Schuler, Islan, & Islan, 2004). Women's mobility, participation in the labor market, and access to economic resources is also limited by the practice of female seclusion or *Purdha* (Amin, 1997), and even when women have access to economic resources, they retain little control over them (Roy, Hidrobo, Hoddinott, & Ahmed, 2018; Goetz & Gupta, 1996). Married at an early age, low educated, and with limited access to economic resources, women in rural Bangladesh enter marital relationships with low bargaining power and their participation in activities that could enhance their economic empowerment can deepen their situation of poverty through violence (Heath, 2014).

3. Experimental Variation

The *No Lean Season* (NLS) program tackles seasonal poverty by offering interest-free migration loans to poor households. In consultation with the Mushfiq Mobarak's research team, the international NGO Evidence Action (EA) developed the program with the goal of bringing the migration experiment of Bryan, Chowdhury, & Mobarak (2014) to a sustainable scale. After a piloting phase in 2016 reaching 16,268 households in 82 villages; the NLS 2017 program-year increased its size by almost a factor of ten, reaching 158,155 households in 699 villages and disbursing 40,574 migration loans (Levy & Sri Raman, 2018). By 2021, the program plans to reach more than 500,000 households and disburse more than 250,000 loans (No Lean Season Budget Projections, 2018). These migration loans are meant to cover a round-trip bus ticket, and are offered and disbursed before and during the lean season, after which they are repaid (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018).

The implementation of the program is carried out by the local NGO RDRS. First, eligible households are invited to village meetings to learn more about the program. Migration Organizers (MOs) follow-up with these households recording their interest in the offer. Among the households not attending the village meetings, the MO conduct door-to-door offers. When a household accepts the loan offer, they can fill out an application with their village MO or visit the closest RDRS branch office. Eligible households only need to present an official

identification for the application and disbursement, and the loan is immediately disbursed. Households in remote villages follow a similar procedure, but in pre-specified village-based disbursements days. As part of the soft conditionality of the loans, MOs remind households about the need to migrate. Once the MOs learn about the migrant's return, they proceed to debrief the households on their migration experience and collect the repayment. Whenever the household is unable to pay, they are given the opportunity to describe the situation. Evidence Action, in coordination with RDRS management, reviews household cases that are considered for a loan repayment waiver and final decisions are communicated to the household by the MOs (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018).

For the 2017 program-year the migration loans were of about 20 dollars. The eligibility conditions were two-fold: either the household had less than 0.5 acres of land, or any member of the household had skipped meals in the two week prior to the targeting survey (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018). This second condition differs from the previous rounds of the experiment, in which the household must have skipped meals in the previous lean season to be eligible.

The NLS program was implemented as a stratified randomized controlled trial, with two levels of randomization: at the branch office and at the village level (see Figure 1). First, RDRS branch offices were randomly assigned into treatment and control group. All villages within a one-hour bike ride from a branch office were assigned to the catchment area of the particular branch so that no village was assigned to more than one branch. Then, villages in the treated branches were randomly assigned to three treatment arms: T) *incentivized*, C1) *spillover*, and C2) *spillover-control*. Only eligible households in the *incentivized* villages were offered migration loans. Households in *spillover* villages did not receive the migration incentive, but where surrounded by villages that received the loans. Households in *spillover-control* villages were in the catchment area of a treated branch, did not receive the loans, and were surrounded by villages that did not receive the loans either. None of the villages in the control branches received the migration loans. We refer to them as *pure controls*.

The assignment process is as follows. First, all of the villages within a treated branch are projected into a circle around the centroid of the branch catchment area, and one village is randomly selected to be the *spillover* village. One third of the circle surrounding this *spillover* village receives the program becoming the *incentivized* villages. The rest of the villages in a treated branch are considered as *spillover-controls*. The villages in non-treated branches are the *pure-controls*.

The experimental design generates treatment spillovers. While pure control villages are sufficiently far away from incentivized villages to experience no spillovers effects; a large out-migration from incentivized villages can induce in-migration into the spillover village. The variation is such that households in incentivized villages experience higher induced migration and higher income — via wages at the destination; while households in spillover villages experience lower migration and higher income — via labor market effects at the origin (Bryan, Chowdhury, & Mobarak, 2018). We make use of this exogenous variation for identification. Given the large scale of the program, only a subset of villages were surveyed for evaluation purposes: the spillover village, a randomly selected incentivized village, a randomly selected spillover-control village, and a randomly selected pure-control village (see Figure 2). In incentivized villages, 40 eligible household were randomly selected for the interviews. In non-incentivized villages (i.e. spillover, spillover-control, and pure-control) 20 eligible households were randomly selected.

The NLS data collection took place at three points in time: baseline, midline and endline. The baseline data collection acted as a targeting survey and was conducted between June and September of 2017. The midline survey took place between January and February of 2018 and gathered data on migration, consumption, wages and income. The endline survey took place between April and May of 2018. The primary purpose of this last survey was to collect data on migration, consumption, and income during the lean season. Other secondary outcomes such as household living conditions, health, gender and social norms, economic activities, savings and risk were also collected during at this time.

The final sample of the NLS intervention 2017 program-year endline survey consisted of 4,548 households in 190 villages across 110 branches: 70 control branches and 40 treatment branches. One village per control branch, for 70 pure-control villages and 1,398 pure-control households. One spillover-control village per treated branch, for 40 spillover-control villages and 792 spillover-control households. One spillover village per treated branch, for 40 spillover villages and 798 spillover-control households. One incentivized village per treated branch, for 40 treated villages and 1,560 incentivized households.

Further details of the intervention, the experimental design, the randomization strategy and the data collection can be found in the No Lean Season 2017 Pre-Analysis Plan in the AER RCT Registry, No. AEARCTR-0002685 (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018).

4. IPV Data Collection and Measures of Violence

We partner with Evidence Action (EA) and Innovations for Poverty Actions (IPA) Bangladesh to collect data on intimate partner violence among the participants of the No Lean Season intervention. A couple of days after the NLS endline survey, we deployed our survey team in the field. Using the NLS listing, we identified the households with a female respondent of the NLS survey and invited them to participate in our study. Since the IPV study was not directly related with the NLS intervention, we requested a separate consent form. To preserve the confidentiality and security of the respondents, we only interviewed one woman per household and the interviews were conducted by female enumerators only. In the event of a sudden interruption, our team was instructed to change immediately to a set of family planning questions which are considered routine questions for women in Bangladesh (Ellsberg & Heise, 2005, p. 122). Whenever we could not guarantee complete privacy during the interview, we refrained from collecting data. Regardless of the disclosure of violence, all the respondents were given referral information about the services available for victims, including local organizations and the national help line 109.

The cultural support for the use of violence and the low proactiveness of bystanders supposed a challenge for our research. To overcome this hurdle, we included men in our survey team. Their role was to escort female enumerators when necessary and to prevent family members from interrupting the interview. Both female and male members of the team were trained in the definitions of violence and recognition of their own biases around IPV, as well as in crisis intervention and stress management. Given the high prevalence of violence in the country, we also lead a self-reflection session about own experiences of violence among the female enumerators.

Both for security and budget purposes, we designed the IPV interviews to last for about 30 minutes. After obtaining informed consent from the respondent, we proceeded to a set of introductory question on marital history and family planning (the latter served as dummy questions in case of sudden interruptions). Next, we inquired about the experiences of violence from the current partner using the standardized IPV module from the World Health Organization Violence Against Women instrument (Ellsberg & Heise, 2005). Rather than a revival of violence, these sets of questions are designed as a behavioural characterization of the episodes and offer the respondent multiple opportunities for disclosure. We focus on the

⁹ Following the World Health Organization guidelines (Ellsberg & Heise, 2005), we presented the research as a women's health study and rely on IPA's field presence and experience to negotiate access with the community.

¹⁰ The respondents were forewarned about this strategy at the beginning of the of the interview.

¹¹ None of this information was provided in written form as it increased the risk of revictimization for the respondents.

lifetime and past six months experiences of physical, emotional, and sexual violence by hands of the current partner. The physical violence subsection, for instance, includes six questions on whether the women has been slapped, pulled by the hair, hit with the fist, kicked, choked, or threaten with a weapon. Whenever the respondent answer *yes* to any of questions, we inquired whether the event happened in the last six months and about its frequency (once, few times, many times). Our questionnaire, available in Appendix I, also included questions on controlling behaviours, injuries, coping strategies, acceptability of violence within the community, and the respondent's gender norms.

The main outcome variables for our analysis are a set of binary indicators of physical, sexual and emotional violence in six months preceding the IPV interview. For comparability purposes with global measures of IPV, we also build an indicator of any form of violence and another one of physical and/or sexual violence. The lifetime prevalence of violence by the current partner is our secondary outcome. We complemented the IPV data with the household-roster of the NLS baseline survey and the migration and income data of the NLS endline survey. While in the NLS survey inquired about any migration episodes taken place between the 2017-2018 lean season and the date of the interview, we build a separate migration variable accounting for the migration episodes taking place within the same period of time that our primary measure of violence (six months before the IPV interview). As for the income variables, we restrain our analysis to household level data.

5. IPV Sample

Of the 4,548 households in the NLS endline survey, 74% had a female respondent. Among these 3,383 households, 84% responded the IPV survey. The intimate partner violence study final sample consist of 2,848 women located in 190 villages: 928 respondents in incentivized villages, 525 in spillover villages, 486 in spillover-control villages, and 909 in pure control villages. In general, the response rate was not statistically different across treatment arms. Although it is slightly lower for incentivized villages (80.7%), the difference is only statistically significant at the 10% level (see Table A 1).

The average respondent of the IPV questionnaire is a 36 years old woman married with a 43 years old man (see Table 1). The couple lives in a 5-member household formed by the respondent, her partner, a child under 9 and two other family members. Almost all of these families are married and have the male spouse as the head of household. Relative to the entire set of respondents of the NLS endline survey, the households in the IPV sample are more likely to have a man as head of household and to be married (see Table A 2). We are not surprised by

this feature as, by construction, the IPV sample presumes a female respondent of the NLS survey and the NLS survey was only administered to the head of the household or spouse. Other dimensions in which our set of households differ from the universe of NLS respondents are age and literacy of the female spouse. While 50% of the women in the IPV sample can read and write, only 35% of the women in households not included in our sample can do so. Similarly, the women that participated in the IPV study are 3 years younger than those who did not.

Table 1 presents the above household characteristics by treatment arm, and a pairwise comparison households in incentivized (T), spillover (C1) and spillover-control villages (C2) with households in pure control villages (C). We do not document persistent differences between the households in treated branches and in the control branches, and whenever the difference is statistically significant, the magnitude is small relative to the control group. Similar patterns are observed when performing the analogous exercise in the overall NLS sample (see Table A 3). This gives us reassurance of the balance of the randomization within the IPV sample and allows us to claim that systematic differences across treatment arms are not due to sample selection.

6. Prevalence of Intimate Partner Violence

We begin our analysis by documenting the prevalence of violence in the sample. Despite the existence of several measures of violence in the country, ¹² comprehensive measures of IPV among the ultra-poor are scarce as this population tends to be difficult to reach and the limited infrastructure of the households makes it difficult to guarantee security conditions required for the data collection. Thereby, the mere fact of providing the up-to-date figures displayed in Table 2 is a contribution to the debate.

Similar to the findings of the 2015 survey on Violence Against Women (VAW) (Bangladesh Bureau of Statistics, 2016), 77% of the women in our sample have experienced a form of abuse at the hands of an intimate partner during their lifetime. Physical violence is the most prevalent form of violence at 64%, followed by emotional violence at 56%, and sexual violence at 31%. For these women, violence is far from a once in a lifetime event. Not only 45% of the respondents report a form of abuse in the last six months, but in almost 90% of the cases the abuse occurred more than once. In the six months preceding the IPV interview, 21%

¹² The WHO multi-country study on women's health and domestic violence (Garcia-Moreno, Jansen, Ellsberg, Heise, & Watts, 2006), the Bangladesh Demographic and Health Survey 2007 (2009) and 2014 (2016), as well as the Report on Violence Against Women (VAW) Survey 2011 (2013) and 2011 (2016).

of the women report a form of physical violence and 23% report a form of sexual violence. We highlight the fact that, even if the lifetime prevalence of sexual violence is lower than other forms of violence, the intensity of sexual violence in the six months preceding the IPV interview is high. While in three of every four of the cases of physical violence the events occurred more than once; almost all of the cases of sexual violence (94%) correspond to multiple episodes of abuse. Furthermore, even if there is some cooccurrence of violence, the 35% prevalence rate of physical and/or sexual violence in the last six months suggests that, physical and sexual violence do not necessarily respond to the same shocks, at least in the short-run.

Aside violence, our respondents are also limited by other patriarchal gender norms. Eight out of every ten of our respondents were married before age 18 and almost one out of every two were married before age 15 (Panel A, Table 5). The overwhelming majority of women consider that a good wife must obey her husband even if she disagrees with him (99.3%) and that it is important for the husband to show his wife who is the boss (84.1%). In the eyes of 30% of the respondents, the fact that a woman earns more than her partner is problematic. Almost 60% considers that it is a wife's obligation to have sex with her husband's even if she does not feel like it. When we inquire about whether or not they consider others outside the family should act when a man mistreats his wife, 68.7% think that bystanders should not intervene (Panel B, Table 5). Four of out every ten women believes that, in their community, a husband is justified in beating his wife if she goes out without telling him (21.1%), neglects the house or the children (21.7%), argues with him (22.5%), refuses to have sex (14.1%), does not cook food properly (10.1%), shows disrespect for her in-laws (28.6%) or if he suspects that she is being unfaithful (14.7%). Hence, beyond their own regressive gender norms, these women are aware of the fact that their communities justify wife beating.

Given our interest on the effects of migration on IPV, we next document violence and gender norms differ by the migration status of the household. To do so, first we analyze the migration patterns. As shown in the first column of Table 3, 26% of the households in our sample sent a migrant during the 2017-2018 lean season and in 81% of the cases, the migrant was the head of the household. Conditional on migration, the average migration episode lasts for about 7 weeks.¹³ To minimize measurement error, in our causal analysis we focus on the

¹³ The differences in the household characteristics described in section 5, suggests that the migration rates in the IPV sample might differ from the ones in the full NLS sample. Table A 4 confirms this intuition: while 26% on the households in the IPV sample send a member as a seasonal migrant; 23% of the households in full NLS sample did so. When looking at the household in control branches exclusively, a similar difference arises. We observed that 21% of the household in the IPV sample send the head of the household a migrant and the analogous figure for the full NLS sample is 18%. Since the households of the IPV sample constitute 62% of the NLS sample, this implies that the 1,700 households excluded from our study must have had even lower migration rates. In the results section we deep into this comparison.

migration of the head of the household in the six months preceding the IPV interview. During these period, 16% of the households send in the control group send the head of the household as a migrant for an average duration of 3 weeks.

In columns three to six of Table 4 and Table 5, we perform a naïve comparison on the prevalence of violence and gender norms between households with and without a migrant. Although there are no persistent differences in the levels of violence nor in early marriage, women in households with a seasonal migrant are slightly less likely to consider it is a wife's obligation to have sex with her husband. By the same token, these women also believe their community justifies wife beating to a less extent. The above comparison, however, captures mere correlations and cannot be interpreted as a causal effect.

7. Results

Our analytical strategy estimates the causal impact of migration incentives on violence by comparing the prevalence of violence across treatment arms. Our preferred specification performs a pairwise comparison between the households in each treatment arm of the treated branches with households in control branches:

$$Y_{ivj} = \beta_0 + \beta Treatment_{vj} + \phi_j + \epsilon_{ivj}, \qquad (1)$$

where Y_{ivj} is the level of violence that the female respondent of household i located in village v of subdistrict j experienced in the last six months and ϕ_j is a battery of subdistrict fixed effects. $Treatment_{vj}$ could be any of three dummies indicating whether the village v of subdistrict j was selected to received migration loans offers ($Incentivized_{vj}$), as spillover village ($Spillover_{vj}$), or as spillover control ($Spillover Control_{vj}$). In all the specification, we adopt a conservative approach and cluster the errors at the branch level. The β s are the experimental coefficients of interest capturing the intention-to-treat of the program.

In Table 6 we present the results of estimating Equation (1) for three different pairwise comparisons: incentivized versus pure-control villages in the top panel, spillover versus pure-control in the middle panel and spillover-control versus pure-control in the bottom panel. Relative to the control group, women of households receiving the migration loans are 3.5 percentage points less likely to experience physical or sexual violence in the last six months and 1.9 percentage points less likely to be sexually abused. Although the point estimates are not statistically significant at the conventional levels, the magnitude of the coefficient is suggestive, and indicates a 10% and 8% reduction. The opposite trend is observed when looking at emotional violence, which increases by 12%. When looking at the spillover villages

versus the control villages, we document an increase of all forms of violence. Women in villages that did not receive the migration loans but that are surrounded by villages that did are 4.2 percentage points more likely to experience physical violence, 7.4 percentage points more likely to experience emotional violence sexual violence and 6 percentage points more likely to experience emotional violence. These large and statistically significant coefficients suppose a 17% percent increase of physical or sexual violence and a 31% increase in sexual violence alone. The point estimates suggest virtually no effect of spillover effects of the program in the spillover-control villages relative to the pure-controls.

Why violence decreases in incentivized villages and increases in spillover villages? To explain this behavior, we rely on exposure reduction theory. Following on the intuition of Table 3, we hypothesize that, while men in incentivized villages are more likely to migrate than the men in pure-control villages; men in spillover villages are less likely to do so. If the labor market effects are such that men in spillover villages have higher wages and/or face a higher demand for labor, then they are less likely to migrate. With less migration, women in the spillover villages spend more time with potential preparators so that violence increases. In contrast, the induced migration in the incentivized villages reduces the time women spend with the perpetrator, and as such, violence is reduced. Given that households could self-select into migration and the unobserved drivers into migration can be correlated with violence, we estimate the effect of migration on violence using the random assignment to the NLS intervention as an instrument through the following specification:

$$Y_{ivj} = \beta_0 + \beta_1 Migration_{ivj} + \phi_j + \varepsilon_{ivj}$$

$$M_{ivj} = \alpha_0 + \alpha_1 Incentivized_{vj} + \alpha_2 Spillover_{vj} + \alpha_3 SpilloverControl_{vj} + \phi_j + e_{ivj},$$
where M_{ivj} is a binary indicator of migration of household i in village v of subdistrict j .

In Table 7 we present the results of the first stage regression of migration against all treatment arms. We also present specifications comparing each treatment arm in the treated branches with the pure-controls. While in the six months preceding the IPV survey, 16% of the families in the control group send their male head of household as a seasonal migrant; the NLS intervention induced a 4.7 percentage points increase in migration in villages receiving the

¹⁴ In Table A 5 we present the results of a linear regression with the three treatment arm dummies simultaneously and a battery subdistrict fixed effects: $Y_{ivj} = \beta_0 + \beta_1 Incentivized_{vj} + \beta_2 Spillover_{vj} + \beta_3 Spillover Control_{vj} + \phi_j + \epsilon_{ivj}$. This is not our prefer specification as the sub-district fixed effects can be capturing part of the spillover effects of the design. Even so, the results point in the same direction: the NLS program reduces violence in incentivized villages and increase violence in spillover violence, in particular, sexual violence.

incentive and reduced migration in spillover villages by 4.5 percentage points.¹⁵ In Table 8 we rely on this experimental variation to predict migration and estimate the causal effects of migration on IPV.¹⁶ As hypothesized by exposure reduction theory, the IV estimates suggest that the migration of the head of the household in the six months preceding the IPV interview reduces physical or sexual violence by 72 percentage points and sexual violence by almost 69 percentage points. Such large effects are consistent with the high prevalence of violence, and indicate that induced migration eliminates physical or sexual violence almost mechanically. Interestingly, the effects are mostly guided by changes in sexual violence.

Table A 9 presents the IV estimates of the effect that the migration of the head of the household at any point of the 2017-2018 lean season has on the level of violence the women experienced in the 6 months before the IPV interview. Once we break the one-to-one mapping between the period of migration and the period of violence, the point coefficients reduce almost by half. A similar change occurs when we estimate the effect of the migration of any member of the household on violence (Table A 10). Overall, once we widen the definition of migration, the estimates of the effect of migration on violence decrease in magnitude providing us with a reassurance that the specification in Table 8 is capturing changes in the exposure to the perpetrator that necessarily occur during the migration episode and not to other changes in the household dynamics.

As a final test of exposure reduction theory we test the competing hypothesis of absolute resource and bargaining in our intention-to-treat specification. If our results were guided by the increase in the resources in incentivized household reducing violence and the decrease of the resources in spillover villages leading to violence, the experimental coefficients of estimating equation (1) with a control for household income should get closer to zero. In Table 9 we show that this is not the case. Even if household income is negatively correlated with violence, the β coefficients remain virtually unchanged from those in Table 6. Hence, even if household income reduces conflicts in the household, the observed changes in violence are not due the overall availability of resources. To test for bargaining theory, in Table 10 we perform the analogous exercise as in Table 9, controlling for migration income. If our findings were guided

¹⁵ Similar figures are found when running the same specification with migration of the head of the household (Table A 6) and migration of any member of the household (Table A 7) as main outcomes. It is also worth noting that both the migration rates of the control group and the induced migration are lower than what documented in previous rounds of the intervention. A detailed discussion of this issue can be found in Bryan, Mobarak, Naguib, Reimao, & Shenoy (2018) (2019) and Levy & Raman (2018).

¹⁶ In Table A 8 we present the analogous exercise using pairwise comparisons of each treatment arm against the pure-control as instrument for migration.

by bargaining theory, once we control for migration income the intention-to-treat coefficients of the specification contrasting incentivized households with pure control households should vanish. Yet we observe that the coefficients in the top panel of Table 10 are relatively stable to those in the top panel of Table 6.¹⁷

Finally, in Table 11 we address whether the increase in migration in spillover villages is led by changes on the intensive margin or on the extensive margin of violence. Specifically, we test whether the rise in violence displayed in the middle panel of Table 6 is guided by women that have previous experiences of abuse, or by women only experiencing abuse after the implementation of the program. To do so, we estimate equation (1) using the lifetime prevalence of violence as the outcome variable. If the results were just due to temporary changes in violence among women who have a history of abuse, the lifetime prevalence should not capture the effect. In contrast, if the results were due to an onset of violence, the lifetime prevalence should jump as well. In the middle panel of Table 11 we document that women in villages in the spillover treatment arm not only experience a higher prevalence of sexual violence in the last six months but also face a higher incidence of sexual violence. Moreover, in Table A 11 we show that induced migration cannot account for this effect.

8. Discussion

Overall, the evidence suggests that seasonal migration has the potential for improving women's wellbeing by providing them with periods of reduced violence throughout the year. We believe that our results carry important implications for policy as the improvements occur through a rather mechanical effect of reduced exposure to the perpetrator. Whether or not violence returns to its original levels once the migration episode ends still needs to be investigated. If this is the case, permanent reductions in intimate partner violence would require more comprehensive strategies that target gender norms and attitudes towards violence, and reduced exposure to perpetrators can be used as a first step to catalyze such transformation.

¹⁷ Clearly, this is only suggestive evidence and a definitive test for bargaining would involve controlling for husbands' say in the household decision. Even if the share of the household income contributed by the male is a potential proxy for the husband's bargaining power, in a context with strong traditional gender roles where men are prescribed as the breadwinner and women's access to economic opportunities is limited by the *Purdha*, we anticipate little variation in this variable.

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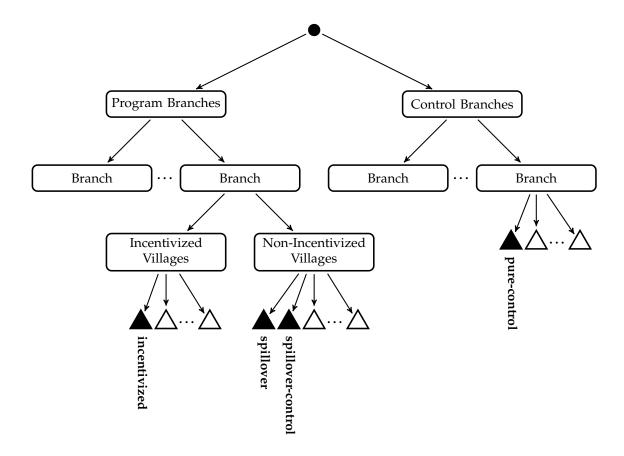
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Figures

Figure 1. No Lean Season Intervention Assignment and Recording Mechanism



Notes: The solid triangles represent villages that were elected for the data collection. Source: No Lean Season 2017 Pre-Analysis Plan in the AER RCT Registry, No. AEARCTR-0002685 (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018).



Figure 2. No Lean Season Intervention Final Assignment within a Treated Branch

Notes: The black filled pints represent villages that were elected for the data collection. *Source:* No Lean Season 2017 Pre-Analysis Plan in the AER RCT Registry, No. AEARCTR-0002685 (Bryan, Mobarak, Naguib, Reimao, & Shenoy, 2018).

Tables

Table 1. Descriptive Statistics and Balance of the Intimate Partner Violence Sample

	All	Incentivized	Spillover	Spillover control	Pure control	T-	С	C	1-C	C2	2-C
		T	C1	C2	C	Diff	p-val	Diff	p-val	Diff	p-val
Household size	4.572	4.418	4.690	4.447	4.728	-0.310	0.000	-0.039	0.690	-0.282	0.004
	(0.027)	(0.045)	(0.064)	(0.063)	(0.051)	(0.081)		(0.097)		(0.094)	
Male head	0.983	0.985	0.971	0.979	0.990	-0.005	0.351	-0.019	0.011	-0.011	0.138
	(0.002)	(0.004)	(0.007)	(0.006)	(0.003)	(0.006)		(0.007)		(0.007)	
Marital status of head	0.983	0.986	0.966	0.977	0.992	-0.006	0.214	-0.027	0.004	-0.015	0.053
	(0.002)	(0.004)	(0.008)	(0.007)	(0.003)	(0.005)		(0.009)		(0.008)	
No of children under 9	0.795	0.812	0.771	0.778	0.800	0.013	0.771	-0.028	0.620	-0.022	0.660
	(0.015)	(0.026)	(0.034)	(0.035)	(0.026)	(0.044)		(0.057)		(0.050)	
Age of head	43.453	42.432	44.510	43.027	44.117	-1.684	0.019	0.393	0.653	-1.090	0.195
-	(0.223)	(0.386)	(0.530)	(0.536)	(0.395)	(0.709)		(0.873)		(0.836)	
Literacy of head	0.427	0.450	0.411	0.399	0.426	0.025	0.408	-0.014	0.713	-0.027	0.486
•	(0.009)	(0.016)	(0.021)	(0.022)	(0.016)	(0.030)		(0.039)		(0.038)	
Age of spouse	35.870	34.699	36.884	35.459	36.711	-2.011	0.002	0.173	0.817	-1.251	0.069
	(0.192)	(0.333)	(0.443)	(0.452)	(0.344)	(0.618)		(0.747)		(0.680)	
Literacy of spouse	0.502	0.561	0.467	0.541	0.440	0.121	0.000	0.027	0.468	0.101	0.004
•	(0.009)	(0.016)	(0.022)	(0.023)	(0.016)	(0.033)		(0.037)		(0.034)	

Notes: Robust standard errors clustered at the branch level.

Table 2. Prevalence of Intimate Partner Violence

	Lifetime	Last 6 months	More than once (last 6 months)
any violence	0.770	0.449	0.397
	(0.008)	(0.009)	(0.009)
physical or sexual violence	0.696	0.355	0.307
	(0.009)	(0.009)	(0.009)
physical violence	0.639	0.209	0.157
	(0.009)	(0.008)	(0.007)
sexual violence	0.315	0.231	0.217
	(0.009)	(0.008)	(0.008)
emotional violence	0.560	0.303	0.257
	(0.009)	(0.009)	(0.008)

Notes: Standard deviations in parenthesis.

Table 3. Migration

	All	Incentivized	Spillover	Spillover	Pure	T-	С	C1	-C	C2-	-C
				control	control						
		T	C1	C2	C	Diff	p-val	Diff	p-val	Diff	p-val
Household with a migrant	0.262	0.298	0.211	0.243	0.263	0.036	0.320	-0.051	0.158	-0.020	0.593
_	(0.008)	(0.015)	(0.018)	(0.019)	(0.015)	(0.036)		(0.036)		(0.038)	
Migration of HH	0.212	0.259	0.154	0.189	0.210	0.048	0.132	-0.056	0.068	-0.021	0.514
	(0.008)	(0.014)	(0.016)	(0.018)	(0.014)	(0.032)		(0.030)		(0.032)	
Weeks of migration HH	6.692	6.554	7.136	7.065	6.497	0.057	0.920	0.638	0.460	0.568	0.440
	(0.199)	(0.332)	(0.657)	(0.503)	(0.298)	(0.565)		(0.860)		(0.733)	
Migration episodes HH	1.005	1.004	1.000	1.000	1.010	-0.006	0.440	-0.010	0.142	-0.010	0.142
	(0.003)	(0.004)	(0.000)	(0.000)	(0.007)	(0.008)		(0.007)		(0.007)	
Migration head of HH (6 months)	0.162	0.190	0.114	0.158	0.162	0.028	0.341	-0.047	0.078	-0.003	0.913
	(0.007)	(0.013)	(0.014)	(0.017)	(0.012)	(0.029)		(0.027)		(0.030)	
Weeks of migration HH (6 months)	4.157	4.369	5.750	3.831	3.422	0.948	0.130	2.328	0.011	0.409	0.505
_ ,	(0.230)	(0.376)	(0.823)	(0.488)	(0.359)	(0.620)		(0.893)		(0.611)	

Notes: Migration episodes and weeks of migration conditional on migration. Robust standard errors clustered at the branch level.

Table 4. Prevalence of Intimate Partner Violence, by Migration Status

	, , , , , , , , , , , , , , , , , , , ,	Households with a	Households without a		
	All	Migrant	Migrant	Diff	p-val
P		me prevalence of Intima			1
any violence	0.770	0.756	0.775	-0.019	0.361
	(0.008)	(0.016)	(0.009)	(0.021)	
physical or sexual violence	0.697	0.681	$0.702^{'}$	-0.022	0.363
	(0.009)	(0.017)	(0.010)	(0.024)	
physical violence	0.640	0.619	0.647	-0.028	0.234
	(0.009)	(0.018)	(0.010)	(0.024)	
sexual violence	0.315	0.303	0.319	-0.015	0.425
	(0.009)	(0.017)	(0.010)	(0.019)	
emotional violence	0.561	0.545	0.566	-0.021	0.356
	(0.009)	(0.018)	(0.011)	(0.023)	
Panel B	B. Prevalence	of Intimate Partner Vic	olence in the last 6 months	5	
any violence	0.449	0.466	0.443	0.022	0.323
•	(0.009)	(0.018)	(0.011)	(0.023)	
physical or sexual violence	0.355	0.364	0.352	0.012	0.596
	(0.009)	(0.018)	(0.010)	(0.022)	
physical violence	0.209	0.236	0.200	0.036	0.050
	(0.008)	(0.016)	(0.009)	(0.018)	
sexual violence	0.231	0.223	0.234	-0.011	0.588
	(0.008)	(0.015)	(0.009)	(0.020)	
emotional violence	0.303	0.321	0.297	0.024	0.266
	(0.009)	(0.017)	(0.010)	(0.021)	
Panel C. Prevale	ence of Intim	ate Partner Violence in	the last 6 months (more the	han once)	
any violence	0.397	0.412	0.392	0.020	0.348
	(0.009)	(0.018)	(0.011)	(0.021)	
physical or sexual violence	0.307	0.309	0.307	0.002	0.924
	(0.009)	(0.017)	(0.010)	(0.021)	
physical violence	0.157	0.172	0.152	0.020	0.198
	(0.007)	(0.014)	(0.008)	(0.015)	
sexual violence	0.217	0.209	0.220	-0.011	0.576
	(0.008)	(0.015)	(0.009)	(0.019)	
emotional violence	0.257	0.270	0.252	0.018	0.365
	(0.008)	(0.016)	(0.009)	(0.020)	

Notes: Robust standard errors clustered at the branch level.

Table 5. Prevalent Gender Norms

	10010 011	Households with	Households		
	All	a Migrant	without a Migrant	Diff	p-val
		el A. Early Marriage			
Married before age 18	0.825	0.832	0.823	0.010	0.555
8	(0.007)	(0.014)	(0.008)	(0.016)	
Married before age 15	0.450	0.466	0.445	0.021	0.384
8	(0.009)	(0.018)	(0.011)	(0.024)	
		C. Own Gender Norn		/	
A good wife obeys her husband	0.993	0.995	0.992	0.003	0.397
,	(0.002)	(0.003)	(0.002)	(0.003)	
Show wife who is the boss	0.841	0.868	0.831	0.037	0.047
	(0.007)	(0.012)	(0.008)	(0.019)	
Woman earning more	0.304	0.292	0.309	-0.016	0.406
č	(0.009)	(0.017)	(0.010)	(0.019)	
Wife obligation to have sex	0.583	0.521	0.605	-0.084	0.000
C	(0.009)	(0.018)	(0.011)	(0.020)	
No family intervention	0.687	0.700	0.682	0.018	0.495
	(0.009)	(0.017)	(0.010)	(0.026)	
Panel C.	Community.	Attitudes, Justificatio	n of Wife Beating	,	
Any justification	0.393	0.356	0.406	-0.050	0.042
	(0.009)	(0.018)	(0.011)	(0.024)	
Go out without telling him	0.211	0.195	0.216	-0.021	0.262
	(0.008)	(0.015)	(0.009)	(0.018)	
Neglect house or children	0.217	0.183	0.229	-0.046	0.008
	(0.008)	(0.014)	(0.009)	(0.017)	
Argue with him	0.225	0.205	0.233	-0.028	0.149
_	(0.008)	(0.015)	(0.009)	(0.019)	
Refuse to have sex	0.141	0.118	0.149	-0.030	0.088
	(0.007)	(0.012)	(0.008)	(0.018)	
Do not cook properly	0.101	0.094	0.104	-0.010	0.470
	(0.006)	(0.011)	(0.007)	(0.013)	
Suspected of being unfaithful	0.147	0.121	0.156	-0.034	0.015
	(0.007)	(0.012)	(0.008)	(0.014)	
Show disrespect for in-laws	0.286	0.240	0.303	-0.062	0.005
	(0.008)	(0.016)	(0.010)	(0.022)	

Notes: Robust standard errors clustered at the branch level.

Table 6. Effect of Migration Incentives on Intimate Partner Violence Treatment Arm vs. Pure Controls

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Incentivized	-0.0055	-0.0336	-0.0046	-0.0192	0.0349
	(0.0300)	(0.0251)	(0.0198)	(0.0214)	(0.0281)
Observations	1836	1837	1837	1837	1837
R-squared	0.059	0.069	0.039	0.092	0.058
Adjusted R-squared	0.033	0.044	0.014	0.068	0.033
Spillover	0.0739**	0.0628*	0.0422*	0.0736***	0.0590**
	(0.0295)	(0.0324)	(0.0245)	(0.0252)	(0.0283)
Observations	1434	1434	1434	1434	1434
R-squared	0.089	0.091	0.057	0.126	0.073
Adjusted R-squared	0.057	0.059	0.024	0.095	0.041
Spillover-control	0.0060	0.0078	0.0000	0.0282	0.0300
	(0.0334)	(0.0303)	(0.0303)	(0.0299)	(0.0326)
Observations	1395	1395	1395	1395	1395
R-squared	0.087	0.076	0.041	0.094	0.077
Adjusted R-squared	0.053	0.043	0.007	0.061	0.044
Mean of Control	0.451	0.361	0.210	0.233	0.288

Notes: Specification of the specified treatment arm vs. the pure control. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

Table 7. Effect of Migration Incentives on Migration of the Head of Household Migration in the last 6 months, First Stage

	All	Incentivized vs. Pure	Spillover vs. Pure	Spillover-control vs.
		Control	Control	Pure Control
Incentivized	0.0455*	0.0472*		
	(0.0251)	(0.0260)		
Spillover	-0.0267		-0.0448*	
	(0.0238)		(0.0244)	
Spillover-control	0.0140			0.0177
	(0.0274)			(0.0278)
Observations	2848	1837	1434	1395
F-test	4.918	3.300	3.371	0.406
P-value	0.003	0.072	0.069	0.525
R-squared	0.005	0.003	0.003	0.000

Notes: Migration of the head of the household in the 6 months prior to the intimate partner violence interview. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

Table 8. Causal Effect of Migration on Intimate Partner Violence

<u> </u>								
	Any violence	Physical or Sexual	Physical	Sexual	Emotional			
Migration head of HH (6 months)	-0.3310	-0.7245*	-0.0818	-0.6950*	0.1590			
	(0.3868)	(0.3810)	(0.2470)	(0.3611)	(0.3078)			
Observations	2848	2848	2848	2848	2848			
F-test	4.918	4.918	4.918	4.918	4.918			
P-value	0.003	0.003	0.003	0.003	0.003			
R-squared	0.005	0.005	0.005	0.005	0.005			

Notes: Prevalence of violence in the last 6 months. Migration of the head of the household in the 6 months prior to the intimate partner violence interview. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; *** p<0.05; *** p<0.01

Table 9. Effect of Migration on Intimate Partner Violence Controlling for Household Income Treatment Arm vs. Pure Controls

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Incentivized	-0.0061	-0.0351	-0.0042	-0.0206	0.0363
	(0.0301)	(0.0255)	(0.0199)	(0.0214)	(0.0279)
Household income	-0.0001	-0.0004*	-0.0004*	-0.0002	0.0000
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Observations	1834	1835	1835	1835	1835
R-squared	0.059	0.070	0.041	0.092	0.058
Adjusted R-squared	0.033	0.045	0.015	0.067	0.032
Spillover	0.0715**	0.0589*	0.0382	0.0731***	0.0563**
	(0.0297)	(0.0327)	(0.0246)	(0.0255)	(0.0282)
Household income	-0.0002	-0.0006***	-0.0006***	-0.0003	-0.0002
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Observations	1433	1433	1433	1433	1433
R-squared	0.090	0.094	0.061	0.127	0.074
Adjusted R-squared	0.057	0.062	0.027	0.096	0.041
Spillover-control	0.0020	0.0018	-0.0037	0.0248	0.0268
	(0.0337)	(0.0311)	(0.0307)	(0.0305)	(0.0327)
Household income	-0.0006*	-0.0009***	-0.0006***	-0.0005***	-0.0005*
	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Observations	1395	1395	1395	1395	1395
R-squared	0.090	0.083	0.045	0.097	0.080
Adjusted R-squared	0.056	0.050	0.010	0.064	0.046

Mean of Control	0.451	0.361	0.210	0.233	0.288
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Notes: Household income in 1,000 Takas. Prevalence of violence in the last 6 months. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects.

* p<0.1; ** p<0.05; *** p<0.01

Table 10. Effect of Migration on Intimate Partner Violence, Controlling for Migration income Treatment Arm vs. Pure Controls

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Incentivized	-0.0047	-0.0338	-0.0027	-0.0203	0.0365
	(0.0302)	(0.0252)	(0.0198)	(0.0213)	(0.0280)
Migration income	0.0002	0.0007	0.0009	-0.0000	0.0004
	(0.0009)	(0.0009)	(0.0008)	(0.0008)	(0.0008)
Observations	1835	1835	1835	1835	1835
R-squared	0.059	0.069	0.040	0.092	0.058
Adjusted R-squared	0.033	0.044	0.014	0.067	0.032
Spillover	0.0724**	0.0614*	0.0402	0.0747***	0.0566**
	(0.0293)	(0.0324)	(0.0243)	(0.0256)	(0.0279)
Migration income	-0.0004	0.0001	-0.0001	0.0004	-0.0003
	(0.0009)	(0.0009)	(0.0008)	(0.0008)	(0.0008)
Observations	1433	1433	1433	1433	1433
R-squared	0.089	0.091	0.056	0.126	0.074
Adjusted R-squared	0.056	0.058	0.023	0.095	0.041
Spillover-control	0.0056	0.0079	0.0003	0.0286	0.0291
	(0.0334)	(0.0304)	(0.0304)	(0.0302)	(0.0325)
Migration income	-0.0008	0.0000	0.0002	0.0003	-0.0006
	(0.0009)	(0.0009)	(0.0007)	(0.0008)	(0.0008)
Observations	1395	1395	1395	1395	1395
R-squared	0.086	0.076	0.041	0.094	0.078
Adjusted R-squared	0.053	0.042	0.006	0.061	0.044
Mean of Control	0.451	0.361	0.210	0.233	0.288

Notes: Migration income in 1,000 Takas. Prevalence of violence in the last 6 months. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects.

Table 11. Effect of Migration Incentives on Lifetime Prevalence of Intimate Partner Violence

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Incentivized	0.0144	0.0017	0.0173	0.0005	0.0557*
	(0.0222)	(0.0226)	(0.0238)	(0.0250)	(0.0294)
Observations	1836	1837	1837	1837	1837
R-squared	0.087	0.098	0.095	0.079	0.105
Adjusted R-squared	0.062	0.073	0.071	0.055	0.081
Spillover	0.0364	0.0466	0.0282	0.0681**	0.0693**
	(0.0329)	(0.0300)	(0.0332)	(0.0328)	(0.0317)
Observations	1434	1434	1434	1434	1434
R-squared	0.112	0.118	0.106	0.107	0.138
Adjusted R-squared	0.080	0.088	0.075	0.076	0.108
Spillover-control	-0.0006	0.0385	0.0348	0.0375	0.0562
	(0.0285)	(0.0266)	(0.0272)	(0.0336)	(0.0340)
Observations	1395	1395	1395	1395	1395
R-squared	0.125	0.125	0.122	0.079	0.134
Adjusted R-squared	0.093	0.094	0.090	0.046	0.103
Mean of Control	0.774	0.696	0.630	0.233	0.538

Notes: Lifetime prevalence of intimate partner violence. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

^{*} p<0.1; ** p<0.05; *** p<0.01

Appendix I

Definitions of Violence

	World Health Organization Characterization of Violence						
Physical	slapped; pushed or shoved; hit with fist; kicked, dragged, or beaten up; choked						
	or burnt on purpose; threatened to use or actually used a gun, knife or other						
	weapon						
Sexual	Physically forced unwanted sexual intercourse; non-approved sex acts; unwanted sex on the basis of fear						
Emotional	insulted; belittled or humiliated; scare or intimidate; threatened to hurt						

Source: Ellsberg & Heise (2005).

Intimate Partner Violence Questionnaire

ection 1. Marital history would like to ask you some questions about your marital history.	
Are you married at the moment?	Yes/No
Are you living with your husband?	Yes/No
How old were you when you got married?	
Does your husband has any relationship with any other women?	Yes/No
	Don't know
	No answer
Previously, were you married?	Yes/No
ection 2. Reproductive health and family planning	
I would like to ask you about all of the pregnancies that you have had during your life time. Have you ever been pregnant? How many times - include the	Number of pregnancies
pregnancies that did not end in a live birth?	
Have you ever used anything, or tried in a way to delay or avoid getting pregnant?	Yes/No
Are you currently doing something, or using any methods, to avoid getting pregnant?	Yes/No
What (main) method are you currently using?	Oral pill injectable copper t/ iud diaphragm/jelly calendar female sterilization condom male sterilization withdrawal others:
Does your husband know that you are using a method to avoid getting pregnant?	Yes/No

Section 3. Respondent and partner

When two people marry, they usually share both good and bad moments. I would now like to ask you some questions about your relationship and how your husband treats you. If anyone interrupts us I will change the topic of conversation and go back to the reproductive health questions. I would again like to assure you that your answers will be kept secret, and that you do not have to answer any questions that you do not want to. May I continue?

				m · 1 · 1 · .
		about some situations that	are true for many women.	Thinking about your
	husband, would you say it	is generally true that he:		
	Yes/No/Don't Know			
	Tries to keep you from sec			
	Tries to restrict contact wi			
	Insists on knowing where			
	Ignores you and treats you			
	Gets angry if you speak w			
	Is often suspicious that yo			
		mission before seeking hea		
		things that happen to many	women, and that your curre	ent husband, or any other
pa	rtner may have done to you			
	Has your current	Has this happened in	In the past 6 months	Before the past 6 months
	husband ever	the past 6 months?	would you say that this	would you say that this
	Yes/No	Yes/No	has happened once, a	has happened once, a few
			few times or many	times or many times?
			times?	Yes/No
	T 1. 1	6 11 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1	Once/Few/Many	
	Insulted you or made you			
	Belittled or humiliated you		D 1 1 1 1 1 1	1 141
	Done things to scare or intermediately smashing things)?	timidate you on purpose (e.g	g. By the way he looked at	you, by yelling and
	Threatened to hurt you or	someone you care about?		
	Has your current	Has this happened in the	In the past 6 months	Before the past 6
	husband ever	past 6 months?	would you say that this	months would you say
	Yes/No	Yes/No	has happened once, a	that this has happened
			few times or many	once, a few times or
			times?	many times?
			Once/Few/Many	Yes/No
	Slapped you or thrown sor	mething at you that could hu		1 2 22 2 7 2
	Pushed you or shoved you			
		th something else that could	hurt you?	
	Kicked you, dragged you			
	Choked or burnt you on pr			
		ally used a gun, knife or other	er weapon against you?	
	Has your current	Has this happened in the	In the past 6 months	Before the past 6
	husband ever	past 6 months?	would you say that this	months would you say
	Yes/No	Yes/No	has happened once, a	that this has happened
	103/110	105/110	few times or many	once, a few times or
			times?	many times?
			Once/Few/Many	Yes/No
	Did your current husband	ever physically force you to		
		intercourse you did not wan		
	might do?	moreourse you are not wan	. to occurse you were arran	a or what your husband
		red you to do something se	xual that you found degrad	ing or humiliating?
S.	Did your husband ever for	rced you to do something se	xual that you found degrad	ing or humiliating?
	Did your husband ever for ection 4. Injuries		<u> </u>	•
Ιv	Did your husband ever for ection 4. Injuries would now like to learn more	reed you to do something server about the injuries that you	<u> </u>	•
I v	Did your husband ever for ection 4. Injuries would now like to learn morked about.	re about the injuries that you	n experienced from your hu	sband's acts that we have
I v tal By	Did your husband ever for ection 4. Injuries would now like to learn more liked about. y injury, I mean any form o		n experienced from your hu	sband's acts that we have
I v tal By	Did your husband ever for ection 4. Injuries would now like to learn more ked about. y injury, I mean any form of her things like this.	re about the injuries that you	experienced from your hu	sband's acts that we have
I v tal By	Did your husband ever for cetion 4. Injuries vould now like to learn more liked about. v injury, I mean any form of the things like this. Have you ever been injured.	re about the injuries that you f physical harm, including c	uts, sprains, burns, broken (any of) your husband/	sband's acts that we have bones or broken teeth, or
I v tal By	Did your husband ever for section 4. Injuries would now like to learn more liked about. y injury, I mean any form of the things like this. Have you ever been injure partner(s). Please think of	re about the injuries that you f physical harm, including ced as a result of these acts by the acts that we talked abou	uts, sprains, burns, broken (any of) your husband/	sband's acts that we have bones or broken teeth, or
I v tal By	Did your husband ever for ection 4. Injuries would now like to learn more liked about. y injury, I mean any form of the things like this. Have you ever been injure partner(s). Please think of In your life, how many time	re about the injuries that you f physical harm, including ced as a result of these acts by the acts that we talked about nes were you injured by (any	uts, sprains, burns, broken (any of) your husband/ t before. y of) your	sband's acts that we have bones or broken teeth, or Yes/No/Don't Know
I v tal By	Did your husband ever for ection 4. Injuries would now like to learn more liked about. y injury, I mean any form of the things like this. Have you ever been injure partner(s). Please think of In your life, how many time	re about the injuries that you f physical harm, including ced as a result of these acts by the acts that we talked abou	uts, sprains, burns, broken (any of) your husband/ t before. y of) your	sband's acts that we have bones or broken teeth, or Yes/No/Don't Know
I v tal By	Did your husband ever for ection 4. Injuries would now like to learn more ked about. y injury, I mean any form of her things like this. Have you ever been injure partner(s). Please think of In your life, how many tin husband/partner(s)? Woul times?	re about the injuries that you f physical harm, including ced as a result of these acts by the acts that we talked about ness were you injured by (any d you say once or twice, sev	uts, sprains, burns, broken (any of) your husband/ t before. y of) your	sband's acts that we have bones or broken teeth, or Yes/No/Don't Know
I w tal By otl	Did your husband ever for ection 4. Injuries would now like to learn more liked about. Injury, I mean any form of her things like this. Have you ever been injure partner(s). Please think of In your life, how many tin husband/partner(s)? Woul times? Has this happened in the partner in the	re about the injuries that you f physical harm, including ced as a result of these acts by the acts that we talked about nes were you injured by (any d you say once or twice, severast 6 months?	uts, sprains, burns, broken (any of) your husband/ t before. y of) your	sband's acts that we have bones or broken teeth, or Yes/No/Don't Know Yes/No/Don't Know
I w tal By otl	Did your husband ever for ection 4. Injuries would now like to learn more liked about. y injury, I mean any form of the things like this. Have you ever been injure partner(s). Please think of In your life, how many tin husband/partner(s)? Would times? Has this happened in the partner of the	re about the injuries that you f physical harm, including ced as a result of these acts by the acts that we talked about nes were you injured by (any d you say once or twice, severast 6 months?	uts, sprains, burns, broken (any of) your husband/ t before. y of) your yeral times or many	sband's acts that we have bones or broken teeth, or Yes/No/Don't Know Yes/No/Don't Know

	During the times that you were hit, did you ever fight back physically or to defend yourself?	Never Once or twice
	If yes: how often? Would you say once or twice, several times or most of the	Several times
	time?	Many times
		Don't know
		No answer
	Have you ever hit or physically mistreated your husband/partner when he was	Never
	not hitting or physically mistreating you?	Once or twice
	If yes: how often? Would you say once or twice, several times or many times?	Several times Many times
		Don't know
		No answer
S	ection 6. Gender norms	110 WILDWOL
	In this community and elsewhere, people have different ideas about families and	what is acceptable
	behaviour for men and women in the home. I am going to read you a list of state	
	to tell me whether you generally agree or disagree with the statement. There are	
	Agree/ Disagree/ Don't know/No answer	
	A good wife obeys her husband even if she disagrees.	
	Family problems should only be discussed with people in the family.	
	It is important for a man to show his wife/partner who is the boss.	
	If a woman earns more money than her husband, it's almost certain to cause prol	
	It's a wife's obligation to have sex with her husband even if she doesn't feel like	e it.
	If a man mistreats his wife, others outside of the family should intervene	
	I would now like to ask you some questions about your community, not about you	
	community, a husband is justified in hitting or beating his wife in the following	situations:
	Yes/No/Don't Know	
	If she goes out without telling him?	
	If she neglects the house or the children?	
	If she argues with him?	
	If she refuses to have sex with him?	
	If she doesn't cook food properly? If he suspects her of being unfaithful?	
	If she shows disrespect for in-laws?	
т.		
	would now like to give you a card. On this card are two pictures. No other information for the first picture is of a sad face, the second is of a happy face.	ation is written on the card.
	o matter what you have already told me, i would like you to put a mark below the	sad nicture if someone has
	ver touched you sexually, or made you do something sexual that you didn't want to	
	d. Please put a mark below the happy face if this has never happened to you. Once	
	ease fold it over and put it in this envelope. This will ensure that I do not know yo	
_	We have now finished the interview. Do you have any comments, or is there anyth	
	Id?	mg size you would into to
	have asked you about many difficult things. How has talking about these things	Good/Bad /Same
	ade you feel	S S S G B G G F G G F G G F G G G G G G G G G
	•	

Appendix II

Table A 1. Differential Attrition

	All	Incentivized	Spillover	Spillover-control	Pure-control	Т	-C	C	1-C	C2	
		T	C1	C2	C	Diff	p-val	Diff	p-val	Diff	p-val
Response rate	0.842	0.807	0.869	0.866	0.851	-0.044	0.049	0.018	0.425	0.015	0.489
	(0.006)	(0.012)	(0.014)	(0.014)	(0.011)	(0.022)		(0.023)		(0.022)	

Notes: Robust standard errors clustered at the branch level.

Table A 2. No Lean Season Sample vs Intimate Partner Violence Sample

	NLS Sample	IPV Sample	Not in IPV Sample	Diff	p-val
Household size	4.526	4.572	4.449	0.123	0.037
	(0.023)	(0.027)	(0.043)	(0.058)	
Male head	0.944	0.983	0.878	0.105	0.000
	(0.003)	(0.002)	(0.008)	(0.009)	
Marital status of HH head	0.935	0.982	0.855	0.128	0.000
	(0.004)	(0.002)	(0.009)	(0.011)	
No of children under 9	0.739	0.795	0.645	0.151	0.000
	(0.011)	(0.015)	(0.018)	(0.026)	
Age of head	44.869	43.444	47.263	-3.819	0.000
	(0.191)	(0.223)	(0.342)	(0.450)	
Literacy of head	0.417	0.426	0.402	0.024	0.191
	(0.007)	(0.009)	(0.012)	(0.018)	
Age of spouse	36.980	35.877	39.113	-3.236	0.000
	(0.169)	(0.192)	(0.321)	(0.425)	
Literacy of spouse	0.445	0.502	0.349	0.153	0.000
	(0.007)	(0.009)	(0.012)	(0.018)	

Notes: Robust standard errors clustered at the branch level.

Table A 3 Descriptive Statistics and Balance of the No Lean Season Sample

	All	Incentivized	Spillover	Spillover control	Pure control			С	1-C	C	2-C
		T	C1	C2	C	Diff	p-val	Diff	p-val	Diff	p-val
Household size	4.526	4.324	4.617	4.497	4.716	-0.392	0.000	-0.099	0.285	-0.219	0.014
	(0.023)	(0.039)	(0.055)	(0.054)	(0.043)	(0.080)		(0.093)		(0.088)	
Male head	0.944	0.925	0.937	0.957	0.961	-0.036	0.000	-0.023	0.049	-0.004	0.682
	(0.003)	(0.007)	(0.009)	(0.007)	(0.005)	(0.009)		(0.012)		(0.009)	
Marital status of head	0.935	0.915	0.930	0.948	0.952	-0.037	0.000	-0.022	0.083	-0.004	0.727
	(0.004)	(0.007)	(0.009)	(0.008)	(0.006)	(0.009)		(0.013)		(0.011)	
No of children under 9	0.739	0.719	0.741	0.747	0.755	-0.036	0.338	-0.015	0.730	-0.008	0.854
	(0.011)	(0.019)	(0.028)	(0.028)	(0.021)	(0.038)		(0.043)		(0.043)	
Age of head	44.869	44.482	45.459	44.434	45.210	-0.728	0.247	0.250	0.732	-0.776	0.314
_	(0.191)	(0.337)	(0.449)	(0.440)	(0.343)	(0.626)		(0.726)		(0.766)	
Literacy of head	0.417	0.421	0.415	0.389	0.431	-0.011	0.692	-0.017	0.621	-0.042	0.174
-	(0.007)	(0.013)	(0.017)	(0.017)	(0.013)	(0.027)		(0.033)		(0.031)	
Age of spouse	36.980	36.104	37.673	36.676	37.707	-1.603	0.003	-0.035	0.956	-1.031	0.107
	(0.169)	(0.301)	(0.394)	(0.377)	(0.303)	(0.520)		(0.631)		(0.635)	
Literacy of spouse	0.445	0.476	0.419	0.463	0.415	0.061	0.023	0.004	0.902	0.049	0.096
	(0.007)	(0.013)	(0.017)	(0.018)	(0.013)	(0.026)		(0.030)		(0.029)	

Notes: Robust standard errors clustered at the branch level.

Table A 4. Difference in Migration Between the No Lean Season Sample and the Intimate Partner Violence Sample

	All	Incentivized	Spillover	Spillover control	Pure control	T-	·C	C1	-C	C2	2-C
		T	C1	C2	C	Diff	p-val	Diff	p-val	Diff	p-val
			Panel A. In	timate Partner	Violence Sar	mple	<u></u>		<u></u>		
Household with a migrant	0.262	0.298	0.211	0.243	0.263	0.036	0.320	-0.051	0.158	-0.020	0.593
_	(0.008)	(0.015)	(0.018)	(0.019)	(0.015)	(0.036)		(0.036)		(0.038)	
Migration head of HH	0.212	0.259	0.154	0.189	0.210	0.048	0.132	-0.056	0.068	-0.021	0.514
_	(0.008)	(0.014)	(0.016)	(0.018)	(0.014)	(0.032)		(0.030)		(0.032)	
Weeks of migration HH	6.692	6.554	7.136	7.065	6.497	0.057	0.920	0.638	0.460	0.568	0.440
	(0.199)	(0.332)	(0.657)	(0.503)	(0.298)	(0.565)		(0.860)		(0.733)	
Migration episodes HH	1.005	1.004	1.000	1.000	1.010	-0.006	0.440	-0.010	0.142	-0.010	0.142
	(0.003)	(0.004)	(0.000)	(0.000)	(0.007)	(0.008)		(0.007)		(0.007)	
			Panel	B. No Lean Sec	ason Sample						
Household with a migrant	0.233	0.251	0.199	0.217	0.240	0.010	0.737	-0.041	0.189	-0.023	0.473
	(0.006)	(0.011)	(0.014)	(0.015)	(0.011)	(0.031)		(0.031)		(0.032)	
Migration head of HH	0.182	0.207	0.148	0.165	0.182	0.025	0.310	-0.034	0.182	-0.016	0.524
	(0.006)	(0.010)	(0.013)	(0.013)	(0.010)	(0.025)		(0.025)		(0.025)	
Weeks of migration HH	6.587	6.272	6.975	7.252	6.465	-0.192	0.698	0.510	0.448	0.787	0.194
	(0.164)	(0.269)	(0.507)	(0.431)	(0.246)	(0.494)		(0.669)		(0.602)	
Migration episodes HH	1.006	1.009	1.000	1.000	1.008	0.001	0.848	-0.008	0.147	-0.008	0.147
	(0.003)	(0.005)	(0.000)	(0.000)	(0.006)	(0.007)		(0.005)		(0.005)	

Notes: Migration episodes and weeks of migration conditional on migration. Robust standard errors clustered at the branch level.

Table A 5. Effect of Migration Incentives on Intimate Partner Violence

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Incentivized	0.0132	-0.0170	0.0079	-0.0064	0.0429
	(0.0296)	(0.0257)	(0.0198)	(0.0223)	(0.0267)
Spillover	0.0464	0.0406	0.0177	0.0522**	0.0433
	(0.0291)	(0.0298)	(0.0241)	(0.0256)	(0.0278)
Spillover-control	-0.0068	-0.0031	-0.0024	0.0140	0.0138
_	(0.0320)	(0.0298)	(0.0271)	(0.0285)	(0.0295)
Observations	2848	2848	2848	2848	2848
R-squared	0.071	0.067	0.034	0.092	0.067
Adjusted R-squared	0.055	0.050	0.016	0.076	0.050

Notes: Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

Table A 6. Effect of Migration Incentives on Migration of the Head of the Household

	All	Incentivized vs. Pure Control	Spillover vs. Pure Control	Spillover-control vs. Pure Control
Incentivized	0.0790***	0.0818***		
	(0.0270)	(0.0281)		
Spillover	-0.0224		-0.0372	
	(0.0273)		(0.0272)	
Spillover-control	0.0099			0.0092
	(0.0284)			(0.0277)
Observations	2848	1837	1434	1395
R-squared	0.077	0.092	0.104	0.102
Adjusted R-squared	0.061	0.068	0.073	0.070

Notes: Migration of the head of the household. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; *** p<0.05; *** p<0.01

Table A 7. Effect of Migration Incentives on Migration of Any Member of the Household

			G '11 B	G '11 1
	All	Incentivized vs. Pure	Spillover vs. Pure	Spillover-control vs.
	All	Control	Control	Pure Control
Incentivized	0.0639**	0.0681**		
	(0.0280)	(0.0292)		
Spillover	-0.0212		-0.0414	
	(0.0304)		(0.0319)	
Spillover-control	0.0072			0.0105
	(0.0303)			(0.0297)
Observations	2848	1837	1434	1395
R-squared	0.092	0.106	0.120	0.126
Adjusted R-squared	0.076	0.082	0.090	0.095

Notes: Migration of any member of the household. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

Table A 8. Causal Effect of Migration Incentives on Intimate Partner Violence
Treatment Arm vs. Pure Controls

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
	Panel A. Ince	entivized vs. Pure co	ntrol		
Migration head of HH (6 months)	-0.0943	-0.7122	-0.0984	-0.4074	0.7408
,	(0.6308)	(0.6097)	(0.4099)	(0.4838)	(0.6453)
Observations	1837	1837	1837	1837	1837
F-test	3.300	3.300	3.300	3.300	3.300
P-value	0.072	0.072	0.072	0.072	0.072
Partial R-squared	0.003	0.003	0.003	0.003	0.003
-	Panel B. Sp	oillover vs. Pure cont	trol		
Migration head of HH (6 months)	-1.6601**	-1.4015*	-0.9412	-1.6423**	-1.3174
,	(0.8456)	(0.8133)	(0.6440)	(0.8255)	(0.8719)
Observations	1434	1434	1434	1434	1434
F-test	3.371	3.371	3.371	3.371	3.371
P-value	0.069	0.069	0.069	0.069	0.069
Partial R-squared	0.003	0.003	0.003	0.003	0.003
·	Panel C. Spillo	ver-control vs. Pure	control		
Migration head of HH (6 months)	0.3728	0.4435	0.0028	1.5964	1.6949
,	(2.0783)	(1.9121)	(1.6761)	(2.9082)	(3.5090)
Observations	1395	1395	1395	1395	1395
F-test	0.406	0.406	0.406	0.406	0.406
P-value	0.525	0.525	0.525	0.525	0.525
Partial R-squared	0.000	0.000	0.000	0.000	0.000

Notes: Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

Table A 9. Causal Effect of Migration on Intimate Partner Violence,
Migration of the Head of the Household

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	Any violence	Physical or Sexual Physical		Sexual	Emotional			
Migration of the HH	-0.1217	-0.4307*	-0.0143	-0.4152*	0.1900			
_	(0.2546)	(0.2503)	(0.1561)	(0.2301)	(0.2106)			
Observations	2848	2848	2848	2848	2848			
First-Stage:								
F-test	6.791	6.791	6.791	6.791	6.791			
P-value	0.000	0.000	0.000	0.000	0.000			
Partial R-squared	0.009	0.009	0.009	0.009	0.009			

Notes: Prevalence of violence in the last 6 months. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. * p<0.1; ** p<0.05; *** p<0.01

Table A 10. Causal Effect of Migration on Intimate Partner Violence,
Migration of Any Member of the Household

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Household with a migrant	-0.1582	-0.5268*	-0.0218	-0.5104*	0.2169
	(0.3048)	(0.2972)	(0.1888)	(0.2693)	(0.2535)
Observations	2848	2848	2848	2848	2848
First-Stage:					
F-test	4.693	4.693	4.693	4.693	4.693
P-value	0.004	0.004	0.004	0.004	0.004
Partial R-squared	0.006	0.006	0.006	0.006	0.006

Notes: Prevalence of violence in the last 6 months. Robust standard errors clustered

Table A 11. Causal Effect of Migration on Lifetime Prevalence of Intimate Partner Violence

	Any violence	Physical or Sexual	Physical	Sexual	Emotional
Migration head of HH	0.2680	-0.0696	0.1521	-0.2268	0.3623
_	(0.2319)	(0.2232)	(0.2173)	(0.2264)	(0.2971)
Observations	2848	2848	2848	2848	2848
First-Stage:					
F-test	6.791	6.791	6.791	6.791	6.791
P-value	0.000	0.000	0.000	0.000	0.000
Partial R-squared	0.009	0.009	0.009	0.009	0.009

Notes: Lifetime prevalence of intimate partner violence. Migration of the head of the household. Robust standard errors clustered at the branch level. All specifications include sub-district fixed effects. *p<0.1; **p<0.05; ***p<0.01