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**Transfers, Behavior Change Communication, and
Intimate Partner Violence**

Postprogram Evidence from Rural Bangladesh

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INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

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**Transfers, Behavior Change Communication, and Intimate Partner Violence:
Postprogram Evidence from Rural Bangladesh[†]**

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Abstract

Transfer programs have been shown to reduce intimate partner violence (IPV), but little evidence exists on how activities linked to transfers affect IPV or what happens when programs end. We assess postprogram impacts on IPV of randomly assigning women in Bangladesh to receive cash or food, with or without nutrition behavior change communication (BCC). Six to 10 months postprogram, IPV did not differ between women receiving transfers and a control group; however, women receiving transfers with BCC experienced 26 percent less physical violence. Evidence on mechanisms suggests sustained effects of BCC on women's threat points, men's social costs of violence, and household well-being.

Keywords: Transfers, behavior change communication, intimate partner violence, social protection, gender, Bangladesh

JEL codes: D1, O12

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1. Introduction

Intimate partner violence (IPV) is a major global public health problem with economic costs ranging from 1 to 4 percent of gross domestic product (García-Moreno et al. 2015; Ribero and Sánchez 2005).¹ IPV has multiple malign consequences for women's physical and mental health (Ellsberg et al. 2008; Kapiga et al. 2017) and is the leading cause of women's death by homicide (Devries et al. 2013). Adverse effects are transmitted intergenerationally, with IPV linked to poorer child development, nutrition, and health outcomes, as well as a greater likelihood of children also entering into abusive relationships (Aizer 2010; Hasselmann and Reichenheim 2006; Karamagi et al. 2007; Koenen et al. 2003; Pollak 2004). Using data from 141 studies from 81 countries, Devries et al. (2013) estimate that 30 percent of all adult women have experienced some form of IPV. There is considerable regional variation in this prevalence, with South Asia (41 percent)—the region of our study—among the highest in the world.

Recent interest has grown in the potential of social transfer programs, and in particular cash transfers, to reduce IPV. Transfer programs are a promising platform, in that they are widely used as antipoverty tools worldwide and are potentially scalable. A growing body of work—drawing largely from Latin America—finds that transfers targeted to women, often conditioned on trainings or other activities, reduce the prevalence of physical violence against women by 5 to 11 percentage points (Angelucci 2008 and Bobonis, González-Brenes, and Castro 2013 in Mexico; Hidrobo, Peterman, and Heise 2016 in Ecuador; Perova and Vakis 2013 in Peru; Haushofer and Shapiro 2016 in Kenya). While this literature finds some subgroups to be at

¹ Economic costs estimated for Brazil, Chile, South Africa, Tanzania, and Columbia.

greater risk of violence, no evidence has been found of transfer programs leading to increased IPV on average.²

The existing literature primarily discusses two mechanisms as driving the reductions in IPV from transfer programs. First, transfers made to women may improve their bargaining position within the household. Early theoretical models of the economic behavior underlying IPV (Farmer and Tiefenthaler 1997; Tauchen, Witte, and Long 1991)—as well as later variants—view IPV as part of noncooperative Nash bargaining between partners. In these models, a male’s utility increases in the violence he inflicts on his partner—for example, because he derives esteem from doing so or because it releases frustration. Constraining his behavior is the female partner’s “threat point”: her ability to leave the relationship. Her capacity to generate income, either from working or from public or private transfers, is a primary factor shaping this ability. In this context, a social transfer targeted to a woman that remains in her control increases her threat point. While this effect could be reversed if males use IPV to extract resources from their partners (Bloch and Rao 2002; Bobonis, González-Brenes, and Castro 2013) or if males increase violence as backlash against the change in power dynamics (Chin 2012), the existing literature has found limited evidence of these playing out (as noted above). A second mechanism is that transfers increase total household income and reduce poverty-related stress; this in turn

² Angelucci (2008) notes that physical violence increases in Mexico where the transfer payment is large relative to household income and the husband has low levels of schooling, and Hidrobo and Fernald (2013) find in Ecuador that a cash transfer program significantly increases emotional violence in households where a woman with primary school education or less has greater or equal education relative to her partner.

may reduce the conflict within the household that precipitates violence (Ellsberg et al. 2015; Fox et al. 2002).³

Although the literature is encouraging regarding the potential for transfer programs to reduce IPV, it leaves knowledge gaps on several important policy questions. First, what happens to IPV after transfer programs end? Existing studies provide evidence of impacts on IPV while programs are ongoing, but none has postprogram quantitative data on IPV. To the extent that qualitative evidence exists, it points to preexisting levels of IPV reverting as soon as the program ends (Buller et al. 2016). As most programs do not provide transfers indefinitely, this issue is central to whether transfer programs can be a sustainable solution to reducing IPV.

Second, what role do the complementary activities in these transfer programs play, and do these shape postprogram effects on IPV? When considering mechanisms, existing studies have focused on the receipt of transfer income itself. However, most of the transfer programs studied also involve other activities or conditionalities accompanying the transfer. In some cases, the literature has acknowledged that complementary activities might play a role,⁴ but this mechanism is usually not emphasized; moreover, none of the existing studies can empirically distinguish the effects of transfers from other activities, as these features are bundled in the interventions they study. Meanwhile these complementary activities could independently affect IPV. For example, group-based trainings or other activities could strengthen a woman's threat

³ For example, Buller et al. (2016), in the context of a transfer program targeted to women and linked to nutrition trainings in Ecuador, use quantitative and qualitative analysis to assess mechanisms and find support for improvements in women's threat points as well as reductions in household poverty-related stress and conflict.

⁴ For example, Buller et al. (2016) note that improved nutrition knowledge from the nutrition training in Ecuador also increased women's status in their households.

point by improving her self-efficacy and self-esteem, increasing her perceived ability to change her situation (Brody and Vojtkova 2016). Group-based activities could also build social capital (Brody and Vojtkova 2016). Frequent social interaction has been shown to have economic returns even after programs end (Feigenberg, Field, and Pande 2013). Public health, sociology, and criminology literatures also suggest that social ties could reduce IPV victimization by improving women's threat point (providing outlets for victims to escape violent relationships [Stets 1991]) and by increasing the social cost of men's violent behavior (increasing the chances that violence is recognized by others and increasing "social control" in the form of others' disapproval [Stets 1991; Van Wyk et al. 2003]).⁵ Effects on self-efficacy, self-esteem, and social capital could also persist after the program ends. For policy decisions, it is critical to understand which features of transfer programs are needed to reduce IPV and whether adding certain features to transfers leads to more sustainable reductions.

Third, do existing findings generalize globally? The literature on transfers and IPV is dominated by studies from Latin America, with fewer studies from other parts of the developing world (for example, Haushofer and Shapiro 2016 in Kenya) where gender dynamics may differ. In rural Bangladesh, a context where patriarchal norms and female seclusion are prevalent, existing work on violence finds an ambiguous relationship between IPV and women's autonomy as well as other factors typically seen as improving women's threat point. For example, Schuler et al. (2013) find that increased women's empowerment can reduce IPV in Bangladesh, while Fakir et al. (2016) suggest that it may lead to increases. Evidence from Bangladesh is also inconclusive regarding effects on IPV of participating in microcredit groups (for example, Hadi

⁵ Reviews such as Ellsberg et al. (2015) also emphasize the importance of nonmonetary interventions, including those supporting discussions about gender relationships and shared decision making, for reducing IPV.

2000 and Bates et al. 2004 find reductions, but Bhuiya, Sharmin, and Hanifi 2003 find increases) and of women's employment (Hadi 2005 finds that it reduces the risk of IPV; Bates et al. 2004 find that it increases the risk). The relationship between women's economic empowerment and IPV in Bangladesh has also been found to depend on community characteristics, with evidence suggesting that as women's autonomy becomes more accepted at the community level it may no longer trigger violence (Koenig et al. 2003; Das 2008). Consistent with this evidence, a review by Brody and Vojtkova (2016) document evidence from South Asia that IPV can increase if women's economic empowerment is not complemented with household- and community-level interventions that mitigate these potential adverse consequences. If the relationship between IPV and its determinants is shaped by different contextual factors across regions, then this could alter how transfer programs affect IPV as well, highlighting the importance of evidence from diverse regions.

This study contributes to these knowledge gaps: we investigate what happens to IPV after social transfers end; we disentangle the effects of a transfer from the effects of other accompanying features on IPV; and we situate our study in Bangladesh, a South Asian country where IPV is high and social norms are conservative. Our analysis uses data from the Transfer Modality Research Initiative, a pilot safety net program in rural Bangladesh in which women in very poor households were randomly assigned to receive cash or food, with or without intensive nutrition behavior change communication (BCC). Although baseline, midline, and endline surveys did not collect IPV data, a "postendline" round of data was collected approximately six to 10 months after the program ended and included an IPV module. Drawing on the randomized design, we assess impacts on IPV at postendline, separately estimating the effect of receiving only transfers from the effect of receiving transfers linked to nutrition BCC. We find that, six to

10 months after the program, women receiving only transfers experienced no significant difference in any dimension of IPV relative to the control group; however, women that had received transfers with BCC experienced significantly less physical violence than either the transfer-only group or the control group. We show that this result is robust to a number of alternate specifications. Suggestive evidence on mechanisms indicates that the postprogram effect of transfers linked to BCC may occur through sustained increases in women's threat points, greater social costs to men of inflicting violence, or long-term improvements in household well-being. Although we cannot give conclusive evidence on whether there were differences in effects on IPV *during* the program between transfers with or without BCC, we discuss descriptive evidence suggesting that transfers alone did reduce IPV during the program but this reverted once the program ended.

The paper is organized as follows. We begin by outlining our study context and the intervention that we assess in Section 2. We then describe the data available to us in Section 3 and our estimation strategy in Section 4. Section 5 presents our main results, while in Section 6 we explore plausible mechanisms that underlie these. Section 7 discusses our findings and concludes.

2. Study Context and Study Design

Gender Context

Reports of IPV are high in Bangladesh. A Violence Against Women survey conducted by the Bangladesh Bureau of Statistics in 2015 found that 72.6 percent of currently married women reported having ever experienced any type of violence by their current husbands, 49.6 percent reported ever experiencing physical violence from their current husbands, and 20.8 percent

reported physical violence in the past 12 months (Bangladesh Bureau of Statistics 2016). In a module administered to men in the Bangladesh Demographic Health Survey 2004, 74 percent of men reported ever being violent against their wives (Johnson and Das 2009). A survey between 2000 and 2003 using the same World Health Organization (WHO) Violence Against Women instrument used in this study found that 41.7 percent of Bangladeshi women in the rural province of Matlab reported ever experiencing physical violence from an intimate partner in their lifetimes (García-Moreno et al. 2006).

Many studies link IPV in rural Bangladesh to gender norms (for example, Koenig et al. 2003). Although gender equality in Bangladesh has improved in some dimensions over the past four decades, patriarchal gender norms persist in much of rural Bangladesh (Das 2008). *Purdah*—the practice of female seclusion (Amin 1997)—is common, and restrictions remain on women’s movements outside the home without accompaniment by a male family member. While women in very poor households tend to work outside the home out of economic necessity, typically as domestic workers or agricultural wage workers, they often face harassment and social stigma (Das 2013). Group membership among women is low (Alkire et al. 2013), which combined with seclusion norms limits social contact. Women’s control over resources and asset ownership is also low (Alkire et al. 2013; Roy et al. 2015). Women have limited direct control over money, as men typically visit markets to purchase food and other household items. Women’s role in household decision making also tends to be limited. Traditionally, men dominate most major household decisions including those related to expenses (Roy et al. 2015), and mothers-in-law have considerable influence over decisions related to food purchases, daily menus, and childcare practices.

An implication is that many poor rural Bangladeshi married women—including participants in our study, prior to intervention—may have low threat points within their marriages. In light of their limited potential to function in society without the protection of their husbands, as well as a lack of resources under their control, the cost of losing family support is high.⁶ Their outside options may be limited further by a perceived lack of agency, given their limited voice within the home, as well as a lack of social support outside the home.

The Transfer Modality Research Initiative

Study Design

The Transfer Modality Research Initiative (TMRI) was a pilot safety net program based on two cluster randomized control trials in rural Bangladesh: one in the northwest region (the “North”) and one in the coastal southern region (the “South”). In the North, study villages were randomly assigned to a control group or to one of four treatment arms in which beneficiaries received a cash transfer (“Cash”), a food ration (“Food”), a half cash transfer and half food ration (“Cash&Food”), or a cash transfer along with nutrition BCC (“Cash+BCC”). In the South, study villages were also randomly assigned to a control group or to one of four treatment arms; the first three treatment groups were the same as in the North. In the fourth treatment group in the South, instead of a cash transfer along with nutrition BCC, beneficiaries received a food ration along with nutrition BCC (“Food+BCC”).

All beneficiaries were poor households (defined as having consumption below the lower poverty line in Bangladesh) with a child zero to 24 months of age in March 2012. The mother of

⁶ Brulé (2012) describes a similar tradeoff in the context of rural India, where land inheritance laws did not increase women’s inheritance because women chose to forgo claiming legal rights in favor of retaining family safety nets.

the zero-to-24-month-old child was the designated beneficiary—both the cardholder for receiving transfers and the target participant in BCC activities. Transfer payments and BCC were undertaken for 24 months, from May 2012 to April 2014.

The program was designed and evaluated by the International Food Policy Research Institute (IFPRI) and implemented by the United Nations' World Food Programme (WFP). WFP managed the procurement and delivery of transfers, as well as the nutrition BCC training, and routinely monitored the program. A nongovernmental organization contracted by WFP, the Eco-Social Development Organization (ESDO), was responsible for the field implementation of project activities, including distributing the monthly food and cash transfers, and delivering the nutrition BCC.⁷

Randomization and Sample Design

To implement TMRI's cluster randomized control trial design, analogous sampling processes were followed in the North and in the South. In each region, five subdistricts (*upazilas*) were selected from a list of *upazilas* where, according to the 2010 Bangladesh Poverty Map prepared by the Bangladesh Bureau of Statistics, the proportion of households living below the lower poverty line in Bangladesh was 25 percent or more. All villages within these five *upazilas* were listed. Villages with fewer than 125 households or villages that were considered peri-urban were dropped. In each region, simple random sampling was used to assign 50 villages from this list to each of the four treatment groups and to the control group, and to assign 25 villages as reserve. In the 250 selected villages in each region, a village census was carried out, which collected

⁷ The study is registered with ClinicalTrials.gov (ID: NCT02237144), received ethical approval from IFPRI's Institutional Review Board, and was reviewed by the Ministry of Food and Disaster Management in Bangladesh.

information on household demographics, poverty indicators, and whether households were participating in social safety net and other targeted interventions.

From these data, a list of households was constructed that were considered poor (that is, estimated to have consumption below the lower poverty line in Bangladesh); had a child zero to 24 months old in March 2012; and were not receiving benefits from any other social safety net interventions. These were the eligible households for participation in the pilot study. From each village, 10 households meeting these three conditions were randomly selected using simple random sampling, giving a total sample size of 5,000 targeted households.

Transfers

Beneficiaries in the Cash arms received a monthly payment of 1,500 taka (approximately 19 USD) per household. Beneficiaries in the Food arms received a monthly food ration of 30 kilograms of rice, 2 kilograms of *mosoor* pulse (a lentil), and 2 liters of micronutrient-fortified cooking oil. These quantities were chosen so that the initial value of the food ration was equal to the value of the cash transfer of the beneficiaries in the Cash treatment arms. Beneficiaries in the Cash&Food treatment arms received half of each of the two types of transfers: 750 taka, 15 kilograms of rice, 1 kilogram of *mosoor* pulse, and 1 liter of micronutrient-fortified cooking oil.

Cash and food transfers were delivered to women during the second week of every month. Distribution points were no more than 2 kilometers from participants' homes. Cash was delivered using a mobile phone cash transfer system.⁸ For each TMRI cash beneficiary, a bank account was established with Dutch-Bangla Bank Limited (DBBL). When the money was

⁸ Since this method used a mobile phone handset and SIM card, to preserve the design of the experiment, these were provided to all women in the study (in all treatment and control arms).

deposited to her account, the woman received an SMS notifying her to collect the cash on the set distribution date from the DBBL cash-point agent at the cash distribution site. After verifying the woman's identity and authorization through a series of SMS messages, the DBBL agent initiated the transaction and handed the cash to the woman.⁹ Food transfers were packaged individually to facilitate carrying and handed to beneficiaries at designated food distribution points; monitoring reports indicated that participants tended to share rickshaw vans to facilitate transport of the food transfers to their homes.

Behavior Change Communication

The beneficiaries of the Cash+BCC arm in the North and of the Food+BCC arm in the South received the same transfer as in the Cash-only and Food-only treatment groups, respectively, as well as a suite of intensive nutrition BCC interventions focused on education and behavior change at the household and community level (Ahmed et al. 2016). The BCC strategy involved three different activities: (1) weekly group BCC trainings—some with beneficiaries only (that is, the target woman in the Food+BCC or Cash+BCC groups) and some that invited other family members to attend along with beneficiaries; (2) bimonthly visits to the beneficiaries' homes, in addition to more as needed for individual counseling; and (3) monthly group meetings with influential community leaders.

The group BCC trainings only for beneficiaries occurred on the day of the transfer distribution, once a month.¹⁰ For the remaining approximately three group BCC trainings per

⁹ See Ahmed et al. (2016) for more information.

¹⁰ BCC participants were scheduled to receive their transfer (that is, food or cash) a few hours after non-BCC participants, to minimize spillover effects of women sharing BCC messages with non-BCC participants.

month, other household members—particularly mothers-in-law and husbands, as well as other pregnant or lactating women—were invited to attend along with beneficiaries, with the intention of creating a supportive household atmosphere and behavior change at the household level. These combined sessions served to facilitate women’s ability to participate in the BCC, as household members could see what women were participating in and reduce restrictions on attendance, and to increase uptake of BCC messages as husbands and mothers-in-law are also key decision makers on food purchases, infant and young child feeding (IYCF), and child-rearing in the household.

About nine to 15 beneficiaries were part of each group. The group trainings took place no further than 2 kilometers from beneficiaries’ homes and lasted approximately one hour, on average. Monitoring data showed that beneficiaries assigned to a BCC intervention attended on average 48 of the scheduled 52 sessions per year in the North and 49 of the scheduled 52 sessions per year in the South. Trainings covered the following topics: basic nutrition, control and prevention of micronutrient deficiencies, IYCF practices, healthcare, maternal nutrition, and hygiene.¹¹ The BCC training was led by community nutrition workers (CNWs), engaged by ESDO. CNWs were all women from the same villages as TMRI beneficiaries. They were trained by WFP and ESDO to impart the BCC content using a variety of methods including question and

¹¹ Specifically, there were six modules delivered over seven sessions: (1) overall importance of nutrition and diet diversity for health; (2) hand-washing/hygiene for improving nutrition and health; (3) micronutrients: diversifying diets, Vitamin A; (4) micronutrients: diversifying diets, iron, iodine, and zinc; (5) feeding young children: breastfeeding (6) feeding young children: complementary feeding; and (7) maternal nutrition. After the first full cycle of modules was completed, the training cycle was repeated, but incorporated new methods of delivery to keep the women interested. Subsequently, sessions moved toward a more interactive approach, and participants were encouraged to lead the sessions. CNWs identified successful mothers and involved them in facilitating the group meetings. See Ahmed et al., 2016 for further details on the BCC implementation.

answer, flashcards, real-life examples, discussions, practical demonstrations, and interactive exercises such as role-playing and songs during sessions.¹²

CNWs also conducted the twice-per-month home visits to observe household-level practice and encourage the adoption of positive behaviors, as well as followed up with home visits for individual counseling to beneficiaries on an as-needed basis. Attendance at the group BCC sessions was a soft condition of receipt of the transfers; when a mother missed a session, a CNW would follow up with a home visit to uncover what the reason was for missing the session, and no beneficiaries were dropped from the study for failing to attend sessions. Monitoring data showed that 83 percent of respondents reported that, if a session was missed, the CNW followed up with a home visit.

The monthly group meetings with influential community members (such as village heads, religious leaders, school teachers, community elected persons, and local health and family planning staff) were conducted by CNWs and EDSO staff, without the beneficiaries present, to explain the purpose of the BCC and to provide them with the information being conveyed to study participants. The aim of these meetings was, similar to inviting other household members to group BCC trainings, to facilitate women's participation and to increase uptake of messages through a supportive community environment.

Of note, there was no explicit focus on violence or gender issues in any of the BCC components. However, negotiating conflict within the household regarding the purchase and

¹² The BCC component was designed specifically for TMRI by WFP in consultations with IFPRI and local technical experts. Session materials were derived in part from material developed for Alive & Thrive in Bangladesh, a large-scale program aimed at improving breastfeeding and complementary feeding practices.

consumption of foods, particularly those not typically consumed by these poor households, did form part of the interactive exercises that were part of the BCC.

3. Data

Data Collection

Quantitative data collection for TMRI included four rounds of longitudinal surveys: a baseline survey in March–April 2012 prior to the start of intervention in May 2012, a midline survey in June 2013, an endline survey in April 2014 just before the end of intervention, and a postendline survey from October 2014 to February 2015, six to 10 months after the intervention ended. From October to December 2012, a qualitative and quantitative process evaluation was conducted, collecting information on the implementation of the interventions and beneficiaries’ experience with the program.

The baseline, midline, and endline surveys attempted to interview all 5,000 households that were included in TMRI treatment or control groups in the North or South. Surveys were multitopic, including extensive modules on household demographic and socioeconomic characteristics, knowledge and practices regarding child nutrition and hygiene, and women’s status. In the baseline survey, the youngest child in the household zero to 24 months of age in March 2012 was identified as an “index” child. Modules were designated as to be answered by either a male (usually the household head), who was interviewed by a male enumerator, or a female (the index child’s primary female caregiver, almost always the index child’s mother and referred to hereafter as the “mother”), who was interviewed by a female enumerator. The women’s status module was part of the female questionnaire administered to the index child’s mother. IPV questions were not the focus of the women’s status module in these rounds, although some descriptive questions about violence were asked of transfer recipients.

The postendline round had not been part of the original evaluation design but, with supplemental funding, was designed to center around two objectives: assessing postprogram impacts on IPV, as well as measuring early childhood development (ECD) of the index child. Due to budget constraints, the postendline survey included only a subset of the TMRI intervention arms: in the North, (1) Cash, (2) Cash+BCC, and (3) Control; and in the South, (1) Food, (2) Food+BCC, and (3) Control. Because the specific ECD test used was for children 30 months or older, the postendline sample was restricted to children who would be at least 30 months by October 2014 and their mothers. This sample consisted of 2,830 pairs of children and mothers. Of these, 2,749 pairs were successfully interviewed.¹³

Mothers were asked to bring the index child for ECD testing to a village center (usually a school or community club). This location, chosen primarily due to the need for tables and chairs in the ECD testing, had the additional advantage of bringing mothers outside the home so that they could be interviewed privately, away from other household members. Prior to the start of ECD testing, mothers were told what would be covered in the interview, including the IPV questions, and asked for their consent. After the ECD testing was complete, interviewers administered a short instrument to mothers that covered several topics such as the child's home environment and activities, as well as the mother's experience with IPV.

Violence Measures

The violence questions were drawn from the internationally validated standardized IPV modules in the WHO Violence Against Women instrument (Ellsberg and Heise 2005) and were administered following the WHO protocol on ethical guidelines for conducting research on

¹³ Reasons for incomplete interviews were respondents not being home (1 percent), migrating (1 percent), or refusing (< 1 percent).

women's experience with IPV (World Health Organization 2001).¹⁴ These modules ask multiple behaviorally specific questions on a range of abusive acts, a technique shown to maximize disclosure (Ellsberg et al. 2001). We focused on two types of violence: emotional (four questions) and physical (six questions). For each act of violence, women were first asked if their current husband had ever done this. If they reported "yes," they were asked if it had occurred in the past six months; if they responded that it had, they were asked whether in the past six months it had occurred once, a few times, or many times. The reference period of six months was chosen to capture women's experiences after the TMRI interventions had ended.

The primary outcome measures we construct from these questions are indicators of (1) any emotional violence experienced in the past six months, (2) any physical violence experienced in the past six months, and (3) any emotional or physical violence experienced in the past six months. Each is coded as 1 if the woman responded that she had experienced any of the acts categorized as the respective type of violence (see Appendix A for the questions and categorizations) and 0 otherwise.

Estimation Sample

Our sample for estimating the postprogram impacts of TMRI on IPV draws on the subset of women that participated in the postendline survey ($N = 2,749$). This means women who were the mothers of an index child at least 30 months of age as of October 2014 and who were drawn from the Cash, Cash+BCC, or Control arms in the North or the Food, Food+BCC, or Control arms in the South. Because we wish to have information on baseline characteristics of these

¹⁴ This included ensuring adequate training of interviewers, guaranteeing privacy during interviews, ensuring informed consent and confidentiality of responses, and interviewing only one woman per household so that other household members were not aware that survey questions involved IPV. Referral services could not be arranged.

respondent women and their husbands, we further restrict the sample to those who were already part of their household and married at baseline and who had nonmissing information for their husbands at baseline. We also restrict the sample to those who were the respondents for the women's status modules at midline and endline, in order to relate our primary outcomes at postendline to measures of women's status in previous rounds.¹⁵ Our final estimation sample consists of 2,231 women across North and South. In this estimation sample, there are no cases of nonresponse to any of the IPV questions. Appendix B shows that attrition between the 2,830 women in the sample design and the 2,231 women in the estimation sample does not significantly differ across intervention arms.

4. Estimation Strategy

To estimate the impact of transfers on IPV, we take advantage of the randomized experimental design and conduct an intent-to-treat (ITT) analysis using single-difference estimation with postendline data. The randomized assignment and balance in baseline characteristics minimize concerns of bias in the single-difference treatment estimates. We pool the randomized control trials in the North and South to increase the statistical power of the study (Bourey et al. 2015) and create three intervention arms: a “transfer only” treatment (cash in the North or food in the South), a “Transfer+BCC” treatment (Cash+BCC in the North or Food+BCC in the South), and a pooled control group (control in the North or the South).¹⁶

¹⁵ We do not restrict to those who were also the baseline respondents, as several of the women's status questions we assess were not in any case asked at baseline.

¹⁶ Hidrobo, Peterman, and Heise (2016) compare impacts on IPV from equal-value transfers of food, cash, and vouchers in Ecuador and find no significant differences by transfer modality. A pooled transfer arm is also motivated by similar impacts from the TMRI food only and cash only treatments on outcomes such as food

In our base estimation, we take into account the study design and control for the level of stratification. Given that the main IPV indicators of interest are binary, we estimate the following probit model:

$$Prob(Y_{iv} = 1) = \Phi(\alpha + \beta_1 Transfer_v + \beta_2 TransferBCC_v + \delta R_{iv}), \quad (1)$$

where Φ is the cumulative distribution function of the standard normal distribution. Y_{iv} is the IPV outcome of interest for woman i from village v at postendline, and R_{iv} is an indicator for the study region that is the level of stratification. $Transfer_v$ is an indicator that equals one if village v is assigned to the food or cash treatment arms, and β_1 represents the ITT estimator, or the effect of being assigned to a transfer arm relative to the control group. $TransferBCC_v$ is an indicator that equals one if village v is assigned to the Food+BCC or Cash+BCC treatment arms, and β_2 represents the ITT estimator, or the effect of being assigned to a Transfer+BCC arm relative to the control group.

We then estimate a specification with extended baseline control variables, adding baseline socioeconomic characteristics to increase the precision of the estimates and control for any minor differences between treatment and control arms at baseline. These extended baseline control variables include the female respondent's characteristics (whether she is the spouse of the household head, whether she can read and write, her years of education, her number of children zero to five years old, her number of children six to 15 years old, and her age); her male partner's

consumption expenditures and total consumption expenditures (Ahmed et al. 2016). These findings are consistent with economic models that predict food and cash transfers will have similar income effects if the food transfer is inframarginal. The bulk of the TMRI food transfer—the rice—was inframarginal, while the pulses and cooking oil were extramarginal.

characteristics (whether he can read or write, his years of education, and his age); and household characteristics (household size). The estimation extends equation (1) to the following probit model:

$$Prob(Y_{iv} = 1) = \Phi(\alpha + \beta_1 Transfer_v + \beta_2 TransferBCC_v + X_{iv}'\gamma + \delta R_{iv}), \quad (2)$$

where X_{iv} is a vector of control variables.

To test whether β_1 and β_2 are statistically different from each other, we conduct Wald tests of equality and report the p-values. Coefficients from probit models are converted to marginal effects evaluated at the mean of the independent variable. In all regressions, we adjust standard errors for clustering at the village level that was the level of randomization.

5. Results

Baseline Characteristics of Estimation Sample

Before turning to impact estimates, we first present descriptive statistics on our sample and assess balance in these characteristics at baseline. Table 5.1 presents baseline characteristics of the estimation sample, pooled over North and South, disaggregated into “transfer only,” “Transfer+BCC,” and control arms. Female respondents in our sample are on average 27 years old, while their husbands are on average 34 years old. About 90 percent of female respondents are wives of the household head. In nearly all of the remaining 10 percent of cases, they are the daughters-in-law of the head. More than 10 percent, however, live with their in-laws (around 25 percent live with their mothers-in-law, and about 15 percent live with their fathers-in-law),¹⁷ reflecting the common arrangement of a married male head’s parents living with him in old age.

¹⁷ The discrepancy is likely due to mothers-in-law who are widows.

Female respondents have just over three years of education on average; slightly over 50 percent can read and write. This compares to just over two years of education on average for their husbands, with fewer than 40 percent able to read and write.¹⁸ Female respondents have on average 1.3 children ages zero to five years at baseline (by construction of the sample, all have at least one child zero to 24 months) and about 0.9 children six to 15 years old.

In terms of all of these characteristics, differences in means between the three intervention arms are not statistically significant at the 5 percent level. The baseline balance in these characteristics gives confidence to proceed with single-difference impact estimation and interpret those estimates as causal.

Main Results

Table 5.2 presents the main impact estimates of TMRI on IPV six to 10 months after the program ended. The first three columns present base estimates (equation 1), controlling only for the level of stratification, and the last three columns present estimates with extended controls (equation 2). Table 5.2 reveals that transfers alone have no significant impact on emotional or physical IPV six to 10 months after the program ended. However, Transfer+BCC causes a statistically significant reduction in physical violence, by 6 to 7 percentage points. This impact represents a 26 percent decrease in physical violence relative to the mean of the control group. The difference in coefficients between Transfer and Transfer+BCC is significant for physical violence, suggesting that linking the transfers to BCC is required for postprogram impacts. In all cases, the inclusion of control variables has very little impact on the size or significance of coefficients; hereafter, we

¹⁸ A pro-female educational gender gap is consistent with recent evidence on Bangladesh (Chowdhury, Nath, and Choudhury 2002).

present estimates only for specifications with extended controls.¹⁹

A potential concern with Table 5.2 may be the robustness of the isolated statistically significant estimate for Transfer+BCC on physical violence. We explore the robustness of our results in several ways in Tables 5.3 to 5.5. Table 5.3 examines postprogram impacts on the *frequency* of violence, using an additive scale and a maximum scale.²⁰ Results reveal that impacts on this intensive margin are similar to those on the extensive margin; six to 10 months after the program ends, transfers have no impact on the frequency of violence using either scale. However, Transfer+BCC has a large and negative impact on the frequency of physical violence according to both scales.

Table 5.4 reveals postprogram impacts on the individual acts of violence used to construct the aggregate indicators in Table 5.2. While false positives are a concern when testing multiple hypotheses, disaggregating impacts by individual acts allows us to better understand which acts of violence are being affected and to assess whether aggregation masks a more nuanced pattern. The first four rows in Table 5.4 correspond to indicators of emotional violence, and the last six rows correspond to indicators of physical violence. Results are remarkably consistent with the aggregate impacts in Table 5.2. Six to 10 months after the program ended, transfers have no impact on any of the 10 emotional or physical violence indicators.

Transfer+BCC has no significant impact on any of the four emotional violence indicators, but it

¹⁹ The base specifications and extended controls produce similar results on all subsequent estimates as well; the extended controls improve precision.

²⁰ The additive scale sums up the frequency reported for each individual act of physical or emotional violence, respectively. The maximum scale considers the maximum frequency reported over all acts of physical or emotional violence, respectively.

does have a significant (or weakly significant) negative impact on *each* of the six physical violence indicators.

Table 5.5 disaggregates impacts by North and South, allowing us to see if the pooled impacts in Table 5.2 mask differential patterns by region or modality. Results reveal similar impacts across the North and the South. In particular, six to 10 months after the program ends, neither cash alone in the North nor food alone in the South has an impact on emotional or physical IPV. However, Cash+BCC in the North and Food+BCC in the South have a weakly significant negative impact on physical violence of 6 or 7 percentage points; neither has any significant impact on emotional violence.²¹ The last two rows in Table 5.5 reveal that, based on p-values from nonlinear Wald tests on cross-model hypotheses, the impacts on IPV of cash in the North are not significantly different from those of food in the South, and the impacts of Cash+BCC do not significantly differ from those of Food+BCC.

A final concern may relate to social desirability bias driving these impacts. Social desirability bias refers to respondents answering questions in such a way that they believe will be perceived more favorably by the interviewer (Saunders 1991). Since our estimates are based on self-reported IPV, this issue could affect our results if the BCC caused women to perceive reporting physical violence as less socially desirable. We cannot rule out this possibility. However, we believe it is unlikely to drive our results, for two reasons. The first is that the BCC did not discuss emotional or physical IPV; in general, it touched very little on gender or spousal dynamics (apart from discussions about negotiating the purchase and consumption of nontraditional foods for preschool children) and would not be expected to change women's perceptions of social desirability related to these issues. The second is that, if in fact the BCC

²¹ Lower statistical significance of coefficients relative to pooled estimates is expected, given smaller sample sizes.

were to have changed women's perceptions of the social desirability of reporting IPV, one might have expected this to occur for both emotional IPV and physical IPV; however, we see effects only on physical IPV and not on emotional IPV. Bearing in mind this caveat, our results indicate that TMRI's postprogram reduction in physical violence from transfers with BCC—but not from transfers alone—is a robust finding.

6. Mechanisms

We explore three possible mechanisms to explain why Transfer+BCC plausibly led to decreases in IPV six to 10 months after the program ended, while the transfer alone did not. The three mechanisms we explore are related to the different theories posited in the introduction for why transfers and BCC may reduce IPV. The first mechanism that we explore, which supports household economic bargaining models, is that Transfer+BCC (more so than transfers alone) led to improvements in a women's threat point that were sustained even after the program ended; this increased her bargaining power within the household and made her less willing to accept violent behavior through postendline. The second mechanism that we explore, which supports social control theories, is that Transfer+BCC (more so than transfers alone) led to increased interactions with community members that were sustained even after the program ended; this increased the probability of detection and social cost of men of inflicting violence through postendline. The last mechanism we explore, which supports the poverty-related stress theory, is that Transfer+BCC (more so than transfers alone) led to decreases in poverty that were sustained even after the program ended; this reduced stress and conflict within the household. The three mechanisms are complementary, and we cannot empirically disentangle them. Nevertheless, our suggestive evidence provides insight on factors that have changed as a result of the

Transfer+BCC (relative to the transfers alone) and that may have contributed to the decrease in IPV six to 10 months after the program ended.

Improvements in a Woman's Threat Point

To explore whether Transfer+BCC improved a woman's threat point during and after the program ended, we analyze the impact of the program on three empowerment domains (Kabeer 2001): a woman's economic resources, her agency, and her social resources. All three domains would improve her perception of out-of-marriage options and make it more feasible for her to leave the relationship or settle on a noncooperative equilibrium if she is unhappy. For economic resources we have quantitative data across all rounds of the survey, while for agency we have only endline data; for social resources, we rely on qualitative evidence, descriptive evidence, and supportive evidence from related work.

Table 6.1 shows the impact of the program across midline, endline, and postendline on a woman's economic resources: specifically, her perceived control over resources (panel A) and whether she does any work that brings in cash, increases food available, or builds assets for the household (panel B).²² Both outcomes reveal that transfers alone have no impact across any round on a women's economic resources, while Transfer+BCC leads to significant improvements across all three rounds. Statistically significant differences between Transfer and Transfer+BCC begin to emerge at endline for the probability that a woman works and at postendline for a woman's control over resources. These results suggest that in Bangladesh, targeting transfers to women may not be enough for women to increase their control over money

²² We note that these measures could be outcomes of intrahousehold bargaining, in addition to determinants. For example, how much money out of the household's resources the woman "controls" or whether she works might be negotiated between the couple. Noting this caveat, we present these as suggestive evidence.

or economic resources. The finding is consistent with other evidence from Bangladesh, including that asset transfers targeted to women did not increase women's overall resource control (Roy et al. 2015) and that loans targeted to and taken out by women were often controlled by their husbands (Goetz and Gupta 1996; Hashemi, Schuler, and Riley 1996). However, the addition of BCC changes this, possibly through strengthening her claim to the transfer or increasing her options for work opportunities.

Table 6.2 presents results on the impact of the program on a woman's agency or self-efficacy. Stemming from Sen's concept of agency, agency represents a woman's ability to make and act on her choices (Kabeer 2005; Sen 2001). We analyze four different indicators, collected at endline only, that represent a woman's internal locus of control and her perception of her ability to change her life. These may affect her perceived ability to change her marital situation and leave a violent relationship, shaping her threat point. Transfer and Transfer+BCC lead to significant improvements in a woman's ranking of herself compared to others on having rights/power and the ability to change her life. However, only Transfer+BCC leads to significant improvements in a woman's internal locus of control as measured through a 12-question instrument adapted from Levenson (1974).

Lastly, we provide qualitative and descriptive evidence from the process evaluation regarding the effects of Transfer and Transfer+BCC on women's social capital. As suggested by the quotes below, while the program was ongoing, transfers alone were able to improve women's social capital, by providing them with the resources that social customs required for interacting with others in their community and gaining respect. However, given the reliance on resources, it is plausible that these effects faded once the program ended and the woman no longer received transfer income.

“People respect me now. In the past, when I tried to socialize with them, they were not too friendly. They acted as if they were worried I might ask them for a loan.”

“You must at least take some biscuits, if nothing else, for the family you are going to visit, but we could not even afford to do that. That’s why we would not visit anyone.”

—Cash recipient

“It is embarrassing to visit someone empty-handed. But now I can go to someone’s house when invited.”

—Food recipient

The BCC component was likely to increase social capital in several additional ways. By nature of its design, the BCC led to frequent (weekly) interaction of women with others in their community. Consistent with the literature on self-help groups (Brody and Vojtkova 2016), this is likely to have increased social ties and social capital. Moreover, the quote below reveals that the BCC led women to experience greater interaction and public respect in the community by increasing their knowledge of nutrition. A companion paper (Hoddinott et al. 2017b) finds that nonparticipant neighbors of BCC participants also improved their nutrition knowledge and practices, reflecting spillover effects and suggesting increased interaction between BCC participants and their neighbors. Another companion paper (Hoddinott et al. 2017a) finds that increased nutrition knowledge persisted six to 10 months after the program ended, suggesting the increases in interaction and respect may have also persisted after the program ended.

“Dipali said the BCC training boosted their family status within the community. The neighbors regularly come over to hear what the family

learned in the latest training session; it has facilitated a position for them within the Hindu community.”

—Food+BCC recipient

Social Cost of Physical Violence

While increased social ties due to BCC (both the group meetings and the home visits) can lead to improvements in a woman’s threat point and bargaining power, they can also increase the social cost to men of inflicting violence. The increased interaction of women with others in their community makes physical violence more visible, thereby increasing the probability that men are caught. Moreover, a woman’s improved social status in the community may make it more likely that community members will frown upon violence inflicted on her, representing “social control.” Ahmed (2005) describes a similar dynamic in the context of a group-based microcredit intervention in Bangladesh, where “greater visibility of women in the public domain relating to participation in BRAC activities, and changing familial and societal attitude vis-à-vis their activities, may make it less possible for husbands to get away with violence without incurring social scorn.” Brody and Vojtkova (2016) document eight qualitative studies from South Asia (including in Bangladesh) in which women report that self-help group members put social pressure on men to stop beating their wives and showed up in groups to support women who had been beaten. Such factors would make it more costly for men to inflict physical violence (but possibly not emotional violence, which is less visible and perhaps more accepted in communities).

Poverty-Related Stress

Lastly, Table 6.3 reveals that both the Transfer and the Transfer+BCC arms lead to significant increases in household wealth at endline as measured by per capita consumption and assets.

Improvements in wealth may lead to decreases in IPV if they lead to decreases in poverty-related stress and disputes. Qualitative evidence from the process evaluation supports this hypothesis, with disputes over food and money reported to decrease as a result of the transfer (Ahmed, Sraboni, and Shaba 2014). Both the Transfer and the Transfer+BCC arms experience significant increases in household wealth relative to a control group by endline; however, the endline improvements are significantly larger for the Transfer+BCC arm compared with the Transfer arm. Driving this difference is greater use of transfer resources for investment and income generation in the Transfer+BCC arm (see a companion paper, Ahmed, Hoddinott, and Roy 2017). Ahmed, Hoddinott, and Roy (2017) suggest that the larger increases in income resulting from participation in the Transfer+BCC arm are likely to persist after the intervention ends. Thus the postprogram reductions in IPV from Transfer+BCC could plausibly be due to the Transfer+BCC group continuing to experience significantly less poverty-related stress than the Transfer group at postendline.

7. Discussion and Conclusions

Our results show that, six to 10 months after a transfer program ended, women that had received transfers with BCC experienced significantly less physical violence than either the transfer-only group or the control group. Meanwhile, women receiving only transfers experienced no significant difference in any dimension of IPV relative to the control group. This result is robust to alternate specifications. We present suggestive evidence that the postprogram effect of transfers linked to BCC may have occurred through sustained increases in women's threat points, greater social costs to men of inflicting violence, or long-term improvements in household well-being.

Our findings have important policy relevance. Cash transfers are widely used policy tools in the developing world, and there is growing interest in their potential to reduce IPV. However, most programs do not provide transfers indefinitely and thus cannot be a sustainable solution to reducing IPV if their effects dissipate as soon as the program ends. Moreover, many transfer programs include other complementary activities, and thus it is important to understand whether those complementary activities shape postprogram effects on IPV. The results here show that, in the context of rural Bangladesh, adding intensive nutrition BCC to the transfer led to an impact on IPV six to 10 months after the program ended, but the transfer alone did not. Nutrition BCC is often included in transfer programs that aim to improve household food security and child nutrition; thus, even if project objectives focus on households and children rather than women specifically, nutrition-sensitive social protection programming could have the “unintended” benefit of postprogram reductions in IPV.

To our knowledge, our results are the first rigorous evidence showing impacts of a transfer program on IPV after the program has ended. One may wonder how such postprogram impacts on IPV compare with impacts during the program. Is it the case that, in the setting of rural Bangladesh, transfers alone had no impact on IPV even while the transfers were being provided—implying that the BCC was essential for any reduction in IPV? Or is it that transfers alone reduced IPV while provided, but those reductions were not sustained afterward except with the addition of BCC? As noted in Section 1, we cannot rigorously distinguish these.²³ However, several pieces of evidence—detailed in Appendix C—suggest it was likely the latter. First, we

²³ We also cannot show rigorously that Transfer+BCC had impacts on IPV during the program rather than only after it ended. However, given the mechanisms proposed, it seems likely that the reductions in IPV emerged during the program and were sustained (at least partially) rather than newly emerging only afterward.

find that questions at endline on changes in physical abuse since the start of transfers reflect decreases in the frequency of physical abuse in both the Transfer and the Transfer+BCC arms (24 percent and 17 percent report decreases, respectively), and fewer than 1 percent report increases in either arm. Second, women in both the Transfer and the Transfer+BCC arms report improvements in relationships with husbands compared to the control group at endline and postendline; although the proportion reporting improvements is significantly higher in the Transfer+BCC arm at postendline, there is no significant difference in these proportions between the two arms at endline. Finally, in the process evaluation we find qualitative support from an interview of a beneficiary woman receiving transfers only (Ahmed, Sraboni, and Shaba 2014): “Laisu feels that their improved economic status has led to better relations between her and Shahidul. Previously, if she asked Shahidul to buy some food when there was none in the house, he would become angry and hit her. Now, she says, he is generally quite pleasant and does not fight with her anymore.” The mechanism described supports the findings in Tables 6.1 and 6.3, which show no evidence of transfers increasing economic resources in women’s control but suggest that reductions in IPV in the Transfer arm during the program could have been driven by reductions in poverty-related stress. With significant caveats on these observations, further noted in Appendix C, our evidence suggests that both Transfer and Transfer+BCC decreased violence during the program, but only Transfer+BCC had sustained impacts after the program ended.

Thus, an implication of our findings is that, although transfers by themselves may cause a contemporaneous reduction in IPV, sustained reductions in IPV beyond the end of transfers may require additional program activities that lead to sustained improvements in women’s status in the household and community. What will sustainably achieve this may differ by context. In rural Bangladesh, we find that having recently been the target beneficiary of food or cash transfers is

not sufficient. However, having recently been the target beneficiary of transfers as well as an intensive nutrition BCC appears to improve a woman's status. In light of mixed evidence from rural Bangladesh on the relationship between women's economic empowerment and IPV, we provide evidence that providing women with transfers while also engaging them and their household and community members through BCC decreases IPV beyond the end of the program.

Our findings prompt several questions. For policy decisions, it would be important to understand whether our results generalize to a postprogram period of more than six to 10 months after the end of transfers; to a program exposure shorter than two years; to a different type of BCC (for example, one that is less intensive, involves different activities or topics, or is differently targeted to other members of the household and community); to a different type of transfer program (for example, one that challenges gender norms by not just targeting transfers *to* women, but also targeting them *for* women instead of for the household and child); to a different sociocultural or geographic context; or to a different target group among women. These questions remain for future research.

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Tables

Table 5.1 Baseline summary statistics by intervention arm, pooled North and South

| | <i>N</i> | Transfer | Means Transfer+ BCC | Control | Transfer - Control | P-value of diff. | |
|---|----------|----------|---------------------------|---------|--------------------------|-------------------------------|----------------------------|
| | | | | | | Transfer+ BCC - Control | Transfer - Transfer+BCC |
| Respondent's age | 2,231 | 27.41 | 27.11 | 27.33 | 0.84 | 0.57 | 0.46 |
| Respondent is household head | 2,231 | 0.00 | 0.00 | 0.00 | 0.31 | 0.32 | 1.00 |
| Respondent is spouse of head | 2,231 | 0.91 | 0.89 | 0.92 | 0.63 | 0.22 | 0.44 |
| Respondent is daughter-in-law | 2,231 | 0.08 | 0.09 | 0.08 | 0.96 | 0.56 | 0.61 |
| Respondent can read and write | 2,231 | 0.53 | 0.57 | 0.56 | 0.36 | 0.81 | 0.26 |
| Respondent's education (years) | 2,231 | 3.08 | 3.23 | 3.39 | 0.14 | 0.50 | 0.47 |
| Respondent lives with mother-in-law | 2,231 | 0.26 | 0.27 | 0.27 | 0.62 | 0.92 | 0.55 |
| Respondent lives with father-in-law | 2,231 | 0.15 | 0.17 | 0.17 | 0.24 | 0.93 | 0.32 |
| Number of children 0–5 years of respondent | 2,231 | 1.37 | 1.34 | 1.37 | 0.99 | 0.38 | 0.40 |
| Number of children 6–15 years of respondent | 2,231 | 0.97 | 0.90 | 0.94 | 0.62 | 0.58 | 0.32 |
| Husband's age | 2,231 | 34.21 | 33.62 | 33.98 | 0.63 | 0.48 | 0.25 |
| Husband can read and write | 2,231 | 0.37 | 0.39 | 0.37 | 0.98 | 0.61 | 0.63 |
| Husband's education (years) | 2,231 | 2.00 | 2.26 | 2.09 | 0.63 | 0.45 | 0.21 |

Source: Authors' calculations.

Note: P-values are reported from Wald tests on the equality of means of control and each treatment for each variable. Standard errors are clustered at the village level.

Table 5.2 Impact of treatment arms on IPV, postendline, pooled North and South

| | Any emotiona l or physical | Emotiona l | Physical | Any emotiona l or physical | Emotiona l | Physical |
|---|-------------------------------------|-----------------|-----------------------|-------------------------------------|-----------------|-----------------------|
| Transfer | 0.01 (0.04) | 0.03 (0.04) | 0.00 (0.03) | 0.02 (0.04) | 0.03 (0.04) | 0.00 (0.02) |
| Transfer+BCC | -0.04 (0.04) | -0.02 (0.04) | -0.06 (0.03)* * | -0.04 (0.04) | -0.02 (0.04) | -0.07 (0.03)* * |
| <i>N</i> | 2,231 | 2,231 | 2,231 | 2,231 | 2,231 | 2,231 |
| Mean of control | 0.67 | 0.63 | 0.27 | 0.67 | 0.63 | 0.27 |
| Strata indicator | X | X | X | X | X | X |
| Extended controls | | | | X | X | X |
| P-value: Transfer = Transfer+BC C | 0.12 | 0.19 | 0.01 | 0.10 | 0.16 | 0.01 |

Source: Authors' calculations.

Note: Marginal effects of probit models reported. Extended controls include baseline characteristics of woman and her husband.

Standard errors clustered at the village level.* $p < 0.1$ ** $p < 0.05$; *** $p < 0.01$.

Table 5.3 Impact of treatment arms on frequency of violence, postendline, pooled North and South

| | Frequenc y of any emotiona l or physical (0–30) | Frequenc y of emotiona l (0–12) | Frequenc y of physical (0–18) | Frequenc y of any emotiona l or physical (0–3) | Frequenc y of any emotiona l (0–3) | Frequenc y of any physical (0–3) |
|---|--|--|--|---|---|---|
| Transfer | 0.11 (0.36) | 0.18 (0.27) | -0.07 (0.15) | 0.06 (0.10) | 0.08 (0.10) | -0.01 (0.05) |
| Transfer+BCC | -0.47 (0.33) | -0.05 (0.25) | -0.42 (0.13)*** | -0.09 (0.10) | -0.06 (0.10) | -0.14 (0.05)*** |
| R^2 | 0.02 | 0.01 | 0.03 | 0.01 | 0.01 | 0.03 |
| <i>N</i> | 2,231 | 2,231 | 2,231 | 2,231 | 2,231 | 2,231 |
| Mean of control | 3.78 | 2.68 | 1.10 | 1.45 | 1.37 | 0.45 |
| Strata indicator | X | X | X | X | X | X |
| Extended controls | X | X | X | X | X | X |
| P-value: Transfer = Transfer+B CC | 0.10 | 0.40 | 0.01 | 0.13 | 0.18 | 0.01 |

Source: Authors' calculations.

Note: Extended controls include baseline characteristics of woman and her husband.

Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

Table 5.4 Impact of treatment arms on IPV indicators, postendline, pooled North and South

| | Mean of control | Coefficient of Transfer | Coefficient of Transfer +BCC | P-value: Transfer=Transfer+BCC |
|--|-----------------|-------------------------|------------------------------|--------------------------------|
| Insulted you or made you feel bad about yourself | 0.36 | -0.01 | -0.03 | 0.64 |
| | | (0.04) | (0.04) | |
| Belittled or humiliated you in front of other people | 0.23 | 0.02 | 0.02 | 0.98 |
| | | (0.04) | (0.04) | |
| Done things to scare or intimidate you on purpose | 0.56 | 0.01 | -0.02 | 0.37 |
| | | (0.04) | (0.04) | |
| Threatened to hurt you or someone you care about | 0.14 | 0.02 | -0.01 | 0.28 |
| | | (0.03) | (0.03) | |
| Slapped you or thrown something at you that could hurt you | 0.26 | -0.00 | -0.06 | 0.02 |
| | | (0.02) | (0.03)** | |
| Pushed you or shoved you or pulled your hair | 0.13 | -0.01 | -0.03 | 0.20 |
| | | (0.02) | (0.02)* | |
| Hit you with his fist or with something else that could hurt you | 0.12 | -0.02 | -0.05 | 0.05 |
| | | (0.02) | (0.02)*** | |
| Kicked you, dragged you, or beat you up | 0.10 | -0.01 | -0.04 | 0.04 |
| | | (0.01) | (0.01)*** | |
| Choked or burnt you on purpose | 0.03 | -0.01 | -0.02 | 0.11 |
| | | (0.01) | (0.01)** | |
| Threatened to use or actually used a gun, knife, or other weapon against you | 0.02 | -0.01 | -0.02 | 0.12 |
| | | (0.01) | (0.01)** | |

Source: Authors' calculations.

Note: Marginal effects of probit models reported. Extended controls include baseline characteristics of woman and her husband.

Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

Table 5.5 Impact of treatment arms on IPV measures, postendline, North vs. South

| | | Any emotional or physical | Emotional | Physical |
|-----------------|----------------------------|---------------------------------|-----------------|------------------|
| North | Cash | -0.01 (0.05) | 0.01 (0.05) | 0.00 (0.03) |
| | Cash+BCC | -0.02 (0.05) | 0.01 (0.05) | -0.07 (0.04)* |
| | Mean of control | 0.67 | 0.63 | 0.30 |
| South | Food | 0.04 (0.06) | 0.06 (0.06) | 0.01 (0.03) |
| | Food+BCC | -0.06 (0.05) | -0.05 (0.06) | -0.06 (0.03)* |
| | Mean of control | 0.68 | 0.63 | 0.23 |
| North vs. South | P-value: Cash=Food | 0.48 | 0.45 | 0.91 |
| | P-value: Cash+BCC=Food+BCC | 0.52 | 0.44 | 0.88 |

Source: Authors' calculations.

Note: Extended controls include baseline characteristics of woman and her husband. Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

Table 6.1 Impact of treatment arms on a woman's economic resources, across rounds, pooled North and South

| | Midline | Endline | Postendline |
|--|------------------|-------------------|-------------------|
| <i>Panel A: Control over money</i> | | | |
| Transfer | 0.02 (0.03) | 0.03 (0.03) | 0.04 (0.03) |
| Transfer+BCC | 0.06 (0.03)* | 0.08 (0.03)*** | 0.10 (0.03)*** |
| <i>N</i> | 2,231 | 2,231 | 2,231 |
| Mean of control | 0.71 | 0.71 | 0.38 |
| P-value: Transfer=Transfer+BCC | 0.16 | 0.14 | 0.06 |
| <i>Panel B: Probability that a woman works</i> | | | |
| Transfer | 0.02 (0.03) | 0.02 (0.02) | 0.00 (0.02) |
| Transfer+BCC | 0.06 (0.03)** | 0.09 (0.02)*** | 0.05 (0.02)** |
| <i>N</i> | 2,231 | 2,231 | 2,231 |
| Mean of control | 0.75 | 0.82 | 0.81 |
| P-value: Transfer=Transfer+BCC | 0.27 | 0.00 | 0.03 |

Source: Authors' calculations.

Note: Marginal effects of probit models reported. Extended controls include baseline characteristics of woman and her husband. Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$. Control over money is defined as controlling money needed to buy food, clothes, medicine, and toiletries. Probability that a woman works is defined as working or doing business that brings in cash, food, or

assets, across survey rounds.

Table 6.2 Impact of treatment arms on agency, endline, pooled North and South

| | Mean of control | Coefficient of Transfer | Coefficient of Transfer +BCC | P-value: Transfer=Transfer+BCC |
|--|-----------------|-------------------------|------------------------------|--------------------------------|
| Internal locus of control (first factor) | -0.06 | 0.04 (0.07) | 0.19 (0.07)*** | 0.02 |
| Self-ranking on nine-step ladder of having rights | 2.52 | 0.30 (0.11)*** | 0.38 (0.11)*** | 0.45 |
| Self-ranking on nine-step ladder of ability to change life | 2.78 | 0.27 (0.12)** | 0.27 (0.12)** | 1.00 |
| Perceive success/failure as own responsibility vs. destiny | 0.37 | -0.05 (0.03) | -0.01 (0.03) | 0.27 |

Source: Authors' calculations.

Note: Extended controls include baseline characteristics of woman and her husband. Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

Table 6.3 Impact of treatment arms on household resources, endline, pooled North and South

| | Mean of control | Coefficient of Transfer | Coefficient of Transfer +BCC | P-value: Transfer=Transfer+BCC |
|--|-----------------|--------------------------|------------------------------|--------------------------------|
| Monthly total expenditure per capita (nominal) | 1,669.04 | 217.64 (47.51)*** | 420.77 (45.09)*** | 0.00 |
| Total assets and cash in hand (nominal) | 23,716.79 | 3,674.57 (1,665.09)** | 9,058.11 (2,031.20)*** | 0.01 |

Source: Authors' calculations.

Note: Extended controls include baseline characteristics of woman and her husband. Standard errors clustered at the village level.* $p < 0.1$ ** $p < 0.05$. *** $p < 0.01$.

Appendices

Appendix A. IPV Module in TMRI Postendline (English Translation)

Emotional:

| W2_1 | The next questions are about things that happen to many women, and that your current partner, or any other partner, may have done to you. Has your current husband ever: | A 1 Yes, 2 No>>Next Question | B Has this happened in the last 6 months? 1 Yes, 2 No>>D | C In the past 6 months would you say that this has happened once, a few times, or many times? Once.....1 A few times.....2 Many times 3 | D Before the last 6 months would you say that this has happened once, a few times, or many times? One 1 A few times.....2 Many times 3 Never happened before last 6 months 9 |
|-------|---|---------------------------------------|--|--|--|
| W2_1a | Insulted you or made you feel bad about yourself? | | | | |
| W2_1b | Belittled or humiliated you in front of other people? | | | | |
| W2_1c | Done things to scare or intimidate you on purpose (e.g., by the way he looked at you, by yelling and smashing things)? | | | | |
| W2_1d | Threatened to hurt you or someone you care about? | | | | |

Physical:

| W3_1 | Has he ever: | A 1 Yes, 2 No>>Next Question | B Has this happened in the last 6 months? 1 Yes, 2 No>>D | C In the past 6 months would you say that this has happened once, a few times, or many times? Once1 A few times2 Many times..... 3 | D Before the last 6 months would you say that this has happened once, a few times, or many times One 1 A few times.....2 Many times 3 Never happened before last 6 months..9 |
|-------|---|---------------------------------------|---|--|--|
| W3_1a | Slapped you or thrown something at you that could hurt you? | | | | |
| W3_1b | Pushed you or shoved you or pulled your hair? | | | | |
| W3_1c | Hit you with his fist or with something else that could hurt you? | | | | |
| W3_1d | Kicked you, dragged you, or beat you up? | | | | |
| W3_1e | Choked or burnt you on purpose? | | | | |
| W3_1f | Threatened to use or actually used a gun, knife, or other weapon against you? | | | | |

Appendix B. Attrition Analysis

As described in Section 3, of the 2,830 pairs of mothers and children included in our postendline sample design, 2,749 pairs were successfully interviewed. After we imposed the sample restrictions described in Section 3—keeping only the mothers who were already part of their households at baseline, who were married at baseline, who had nonmissing information for their husbands at baseline, and who were respondents for the midline and endline women’s status modules—2,231 women remained in our estimation sample.

To assess whether attrition from our intended sample of 2,830 to our final estimation sample of 2,231 was systematically different across intervention arms, we use ordinary least squares to regress an indicator for whether a woman stayed in the estimation sample on the intervention arms:

$$Stay_{iv} = \alpha + \beta_1 Transfer_v + \beta_2 TransferBCC_v + \delta \varepsilon_{iv} . \quad (1)$$

Table A.1 shows that there was no significant difference in the probability of staying in the estimation sample across intervention arms, in the full sample or disaggregated by region.

Appendix C. Descriptive Evidence on IPV Impacts during Program Exposure

Given that we did not collect the requisite IPV data at midline or endline, we cannot rigorously assess whether transfers alone had an impact on IPV while transfers were being provided. However, several pieces of evidence in our data suggest it was likely that transfers alone reduced IPV while provided, although those reductions were not sustained afterward. One is the set of descriptive questions on physical violence asked of transfer recipients at endline (Table A.2): “Has your husband, another family member, or household resident (ever) physically abused you?” and “Has the frequency of this abuse increased/decreased/remained the same ever since you started receiving the transfer?” Ninety-five percent of the Transfer arm and 96 percent of the Transfer+BCC arm report that they either have never experienced physical abuse as of endline or that the frequency has decreased since starting to receive transfers (with 24 percent of Transfer and 17 percent of Transfer+BCC reporting a decrease); 4 percent of each arm report the same frequency as before receiving transfers; 1 percent and 0 percent of the Transfer and Transfer+BCC arms, respectively, report an increase in frequency. None of these differences between Transfer and Transfer+BCC is statistically significant. Although we interpret these cautiously given that the questions may be leading and we do not know what the control group would have reported, they indicate that both Transfer and Transfer+BCC may have experienced reduced physical violence during the program.

A second set of suggestive results (Table A.3) comes from endline and postendline questions to all female respondents, asking “Did you notice any change in relationship/attitude (good or bad) towards you of your husband?” and “If, yes what has changed? Less conflict/more respect now, or more conflict/less respect now?” At endline, relative to the control groups, Transfer and Transfer+BCC cause significant 41 and 39 percentage point increases, respectively, in the share reporting an improvement in relationship, with no significant difference between the two. At postendline Transfer and Transfer+BCC cause significant 21 and 41 percentage point increases, respectively, in the share reporting an improvement in relationship, now with a statistically significant difference between the two. Although

these questions do not pertain directly to physical violence and again may be leading, they point to comparable improvements in spousal relations during the program from Transfer and Transfer+BCC, but significantly greater improvements at six to 10 months postprogram from Transfer+BCC.

Appendix Tables

Table A.1 Attrition by intervention arm, postendline

| | Full sample | North | South |
|--------------------------------|-------------------|-------------------|-------------------|
| Transfer | 0.01 (0.02) | 0.02 (0.03) | 0.00 (0.04) |
| Transfer+BCC | 0.03 (0.02) | 0.03 (0.02) | 0.02 (0.04) |
| Constant | 0.74 (0.02)*** | 0.81 (0.02)*** | 0.74 (0.03)*** |
| R^2 | 0.01 | 0.00 | 0.00 |
| N | 2,830 | 1,406 | 1,424 |
| P-value: Transfer=Transfer+BCC | 0.57 | 0.78 | 0.61 |

Source: Authors' calculations.

Note: * $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

Table A.2 Reported change in frequency of physical abuse since May 2012, endline

| | Means | | P-value of diff. (Wald test) |
|--|----------|--------------|---------------------------------|
| | Transfer | Transfer+BCC | |
| Never experienced or decreased frequency of physical abuse | 0.95 | 0.96 | 0.37 |
| Never experienced physical abuse | 0.71 | 0.79 | 0.01 |
| Decreased frequency of physical abuse | 0.24 | 0.17 | 0.01 |
| Increased frequency of physical abuse | 0.01 | 0.00 | 0.17 |
| Same frequency of physical abuse | 0.04 | 0.04 | 0.74 |

Source: Authors' calculations.

Note: Extended controls include baseline characteristics of woman and her husband. Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

Table A.3 Impact of treatment arms on improvements in relationship with spouse across survey rounds

| | Endline (improvements since 2012) | Postendline (improvements since 2014) |
|--------------------------------|---|---|
| Transfer | 0.40 (0.08)*** | 0.20 (0.09)** |
| Transfer+BCC | 0.38 (0.08)*** | 0.41 (0.09)*** |
| <i>N</i> | 2,065 | 2,115 |
| Mean of control | 0.15 | 0.13 |
| Strata indicator | X | X |
| Extended controls | X | X |
| P-value: Transfer=Transfer+BCC | 0.78 | 0.04 |

Source: Authors' calculations.

Note: Estimated using ordered probit. Extended controls include baseline characteristics of woman and her husband. Standard errors clustered at the village level.* $p < 0.1$. ** $p < 0.05$. *** $p < 0.01$.

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