

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 ten months postprogram, IPV did not differ between women receiving transfers and a control group; however, women receiving transfers with BCC experienced 26% 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.

I. Introduction

INTIMATE partner violence (IPV) is a major global public health problem with economic costs ranging from 1% to 4% of GDP (García-Moreno et al., 2015; Ribero & Sánchez, 2005). 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; Fulu et al., 2017; Hasselmann & Reichenheim, 2006; Karamagi et al., 2007; Koenen et al., 2003; Pollak, 2004; Yount, DiGirolama, & Ramakrishnan, 2011). Using data from 141 studies from 81 countries, Devries et al. (2013) estimate that 30% of all adult women have experienced physical or sexual IPV. There is considerable regional variation in this prevalence, with South Asia (41%), the region of our study, among the highest in the world.

Received for publication June 28, 2017. Revision accepted for publication July 9, 2018. Editor: Brigitte C. Madrian.

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This work was undertaken as part of the CGIAR Research Program on Policies, Institutions, and Markets led by the International Food Policy Research Institute. Funding support for this study was provided by the Sexual Violence Research Initiative, the World Bank, and the CGIAR Research Program on Policies, Institutions, and Markets. The study builds on research funded by the German Ministry for Economic Cooperation and Development, the U.K. Department for International Development, the Swiss Agency for Development and Cooperation, the United Nations Development Programme, and the U.S. Agency for International Development. We thank DATA for careful data collection, Wahid Quabili for excellent research assistance, Amber Peterman for helpful discussions, and seminar participants at IFPRI, Colgate University, SVRI Forum 2017, and NEUDC 2017 for useful comments. All errors are our own. The study is registered with ClinicalTrials.gov (ID: NCT02237144), received ethical approval from the Institutional Review Board of the International Food Policy Research Institute, Washington, DC, and was reviewed by the Ministry of Food and Disaster Management in Bangladesh.

A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/rest_a_00791.

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, largely from Latin America, finds that transfers targeted to women, often conditioned on training or other activities, reduce the prevalence of physical violence against women by 5 to 11 percentage points (Angelucci, 2008, and Bobonis et al., 2013, in Mexico; Hidrobo, Peterman, & Heise, 2016, in Ecuador; Perova & Vakis, 2013, in Peru; Haushofer et al., 2019, in Kenya). While this literature finds some subgroups to be at greater risk of violence (see Angelucci, 2008, and Hidrobo & Fernald, 2013), little quantitative evidence has been found of transfer programs leading to increased IPV on average.

The literature focuses on 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 & Tiefenthaler, 1997; Tauchen, White, & 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 or settle on a noncooperative equilibrium if unhappy. A social transfer targeted to a woman that remains in her control increases her threat point. While this effect could be reversed if men use IPV to either extract resources from their partners (Bloch & Rao, 2002; Bobonis et al., 2013) or ensure that resource allocation is aligned with their preferences (Eswaran & Malhotra, 2011), or if men increase violence as backlash against the change in power dynamics (Chin, 2012), the literature has found limited evidence of increases in IPV playing out as a result of transfers. A second mechanism is that transfers increase total household income and reduce poverty-related stress, which may reduce the conflict within the household that precipitates violence (Buller et al., 2018; Ellsberg et al., 2015; Fox et al., 2002).

Although the literature is encouraging regarding the potential for transfer programs to reduce IPV, it leaves several knowledge gaps. First, what happens to IPV after transfer programs end? 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, 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. While some of the literature acknowledges that complementary activities might play a role (see Buller et al., 2016), this mechanism is usually viewed as secondary to the transfer itself. Moreover, none of these studies can empirically distinguish the effects of transfers from other activities, as they are bundled. Meanwhile, these complementary activities could independently affect IPV. For example, group-based training or other activities could strengthen a woman's threat point by improving her self-efficacy and self-esteem, increasing her perceived ability to change her situation (Brody et al., 2017). Group-based activities could also build social capital (Brody et al., 2017) that may persist after the intervention ends (Feigenberg, Field, & Pande, 2013). Social ties could reduce IPV victimization by improving women's threat point as well, providing outlets for victims to escape violent relationships (Stets, 1991). Social capital could also increase the social cost of men's violent behavior by increasing the chances that others recognize this violence and increasing social control in the form of others' disapproval (Stets, 1991; Van Wyk et al., 2003). For policy, 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 empirical literature on transfers and IPV is dominated by studies from Latin America, with fewer studies from other parts of the developing world where gender dynamics may differ. In rural Bangladesh, where patriarchal norms and female seclusion are prevalent, studies have found an ambiguous relationship between IPV and women's autonomy, as well as other factors typically seen as improving women's threat point (Fakir et al., 2016; Schuler et al., 2013). For example, evidence from Bangladesh is inconclusive regarding effects on IPV of participating in microcredit groups (Bates et al., 2004; Bhuiya, Sharmin, & Islam, 2003; Hadi, 2000) and of women's employment (Bates et al., 2004; Hadi, 2005). 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; World Bank, 2008). If the relationship between IPV and its determinants is shaped by different contextual factors across regions, then context could alter how transfer programs affect IPV as well.

This study contributes to filling these knowledge gaps. We investigate what happens to IPV after social transfers

end, disentangle the effects of a transfer from the effects of other accompanying features on IPV, and 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 (TMRI), 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 end-line surveys did not collect IPV data, a "post-end-line" round of data was collected approximately six to ten months after the program ended and included an IPV module. Drawing on the randomized design, we assess impacts on IPV at post-end line, separately estimating the effect of receiving only transfers from the effect of receiving transfers linked to nutrition BCC.

We find that six to ten months after the program, women who had received only transfers experienced no significant difference in any dimension of IPV relative to the control group; however, women who had received transfers with BCC experienced significantly less physical violence than either the transfer-only group or the control group. 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 impacts on IPV during the program between transfers with or without BCC, we discuss descriptive evidence suggesting that transfers alone reduced IPV during the program, but this reverted once the program ended.

Section II of this paper outlines our study context and the intervention that we assess. We describe the data available to us in section III and our estimation strategy in section IV. Section V presents our main results, and in section VI, we explore plausible mechanisms that underlie these. Section VII discusses our findings and concludes. Additional material is available in online appendices.

II. Study Context and Study Design

A. Gender Context

Reports of IPV are high in Bangladesh. A Violence against Women survey in 2015 found that 72.6% of currently married women reported having experienced any type of violence by their current husbands, 49.6% reported experiencing physical violence from their current husbands, and 20.8% reported physical violence in the past twelve months (Bangladesh Bureau of Statistics, 2016). In a module administered to men in the Bangladesh Demographic Health Survey 2004, 74% of men reported being violent against their wives (Johnson & Das, 2009). A survey between 2000 and 2003 using the same WHO Violence against Women instrument used in this

study found that 41.7% of Bangladeshi women in the rural province of Matlab reported experiencing physical violence from an intimate partner (Garcia-Moreno et al., 2006).

Many studies link IPV in rural Bangladesh to gender norms (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 (World Bank, 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, they often face harassment and social stigma (Roy et al., 2015). Group membership among women is low (Alkire et al., 2013), which, combined with seclusion norms, limits social contact. Women's asset ownership is also low (Alkire et al., 2013; Roy et al., 2015), and women have limited direct control over money, as men typically visit markets to purchase food and other household items. Mothers-in-law have considerable influence over decisions on food purchases and child care.

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. Given little resource control, limited voice in the home, societal challenges without male protection, and scarce social ties, there are significant costs to losing husbands' support.

B. *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 (RCTs) 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").¹

All beneficiaries were poor households with a child aged 0 to 24 months in March 2012. The mother of the 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 Program (WFP). WFP managed the procurement and delivery of transfers, as well as the nutrition BCC training, and it routinely monitored the program. An NGO 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.

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% or more. All villages within these five upazilas were listed. Villages with fewer than 125 households or villages that were considered periurban 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 collected 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 identified those considered poor (estimated to have consumption below the lower poverty line in Bangladesh), had a child aged 0 to 24 months 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, ten 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 \$19.00) 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. Cash was delivered using

¹A pure BCC arm was not included, as a large RCT focusing solely on the impacts of BCC on child and maternal nutrition, Alive & Thrive (see Menon et al., 2016) was concurrently underway in Bangladesh.

a mobile phone cash transfer system, in which women collected cash from designated distribution sites using mobile verification of identity.² Food transfers were handed to beneficiaries at designated food distribution points. Cash and food distribution points were no more than 2 kilometers from participants' homes (Ahmed et al., 2016).

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 levels (Ahmed et al., 2016). The BCC intervention involved three complementary activities: (a) weekly group BCC trainings—some with beneficiaries only (i.e., the target women in the Food+BCC or Cash+BCC groups) and some that invited other family members to attend along with beneficiaries; (b) twice-a-month visits to the beneficiaries' homes; and (c) monthly group meetings between program staff and influential community leaders.

The group BCC training only for beneficiaries occurred on the day of the transfer distribution, once a month. For the remaining group BCC training each month, other household members—particularly mothers-in-law, husbands, and 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) practices and child rearing in the household.

About nine to fifteen beneficiaries were part of each group. The group training took place no farther 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. Training covered the following topics: basic nutrition, control and prevention of micronutrient deficiencies, IYCF practices, health care, maternal nutrition, and hygiene. The BCC training was led by community nutrition workers (CNWs) engaged by ESDO. CNWs were all women from the same villages as the TMRI beneficiaries. They were trained by WFP and ESDO to impart the BCC content using a variety of methods: question and answer, flash cards, real-life examples, discussions, practical demonstrations, role playing, and songs.³ Anecdotally, the

interactive nature of the sessions led to a lively atmosphere and solidarity among participants. On field visits, researchers observed women arriving early at sessions and leaving late in order to talk with other members.

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 as needed. 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 the reason for missing the session and to convey the missed information. No beneficiaries were dropped from the study for failing to attend sessions.

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 ESDO staff, without the beneficiaries present, to explain the purpose and importance of the BCC and 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 increase the 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. Interactive exercises included negotiating the purchase and consumption of non-traditional foods for preschool children, but the emphasis was on how to acquire nutritious foods for the child rather than how to resolve conflict more generally.

III. Data

A. Data Collection

Quantitative data collection for TMRI included four rounds of longitudinal surveys: a baseline survey in March and April 2012 prior to the start of intervention in May 2012, a midline survey in June 2013, an end-line survey in April 2014 just before the end of intervention, and a post-end-line survey from October 2014 to February 2015, six to ten 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 end-line 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

²Since 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).

³The BCC component was designed specifically for TMRI by WFP in consultation with IFPRI and local technical experts. Session materials were

derived in part from material developed for Alive & Thrive (A&T) in Bangladesh.

in the household, aged 0 to 24 months in March 2012, was identified as an index child. Modules were designated to be answered by either a man (usually the household head), who was interviewed by a male enumerator, or a woman (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 post-end-line 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 the early childhood development (ECD) of the index child. Due to budget constraints, the post-end-line survey included only a subset of the TMRI intervention arms: in the North, (a) Cash, (b) Cash+BCC, and (c) Control; and in the South, (a) Food, (b) Food+BCC, and (c) Control. Because the specific ECD test used was for children 30 months or older, the post-end-line 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.⁴

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 ECD testing, mothers were told what would be covered in the interview, including the IPV questions, and asked for consent. After testing, interviewers administered a short instrument to mothers on several topics, including the child's home environment and the mother's experience with IPV.

B. Violence Measures

The violence questions were drawn from the internationally validated standardized IPV modules in the WHO Violence against Women instrument (Ellsberg & Heise, 2005) and were administered following the WHO protocol on ethical guidelines for conducting research on women's experience with IPV (World Health Organization, 2001).⁵ 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 (a) any emotional violence experienced in the past six months, (b) any physical violence experienced in the past six months, and (c) 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 online 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 who participated in the post-end-line survey. This means women who were the mothers of an index child aged at least 30 months as of October 2014 and 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 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 end line in order to relate our primary outcomes at post-end line to measures of women's status in previous rounds.⁶ 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 differ significantly across intervention arms. Appendix C shows that baseline characteristics of the respondent women in the estimation sample, their husbands, and their households also do not differ significantly across arms.

IV. Estimation Strategy

We take advantage of the randomized experimental design and conduct an intent-to-treat (ITT) analysis using single-difference estimation with post-end-line data. The randomized assignment and balance in baseline characteristics minimize concerns of bias in the single-difference treatment estimates. We pool the RCTs in the North and South to increase the statistical power of the study (Bourey et al., 2015)

⁶We do not restrict to baseline respondents, as these measures were not collected at baseline.

⁴Reasons for incomplete interviews were respondents not being home (1%), migrating (1%), or refusing (less than 1%).

⁵This 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 the survey questions involved IPV. Referral services could not be arranged.

TABLE 1.—IMPACT OF TREATMENT ARMS ON PREVALENCE OF IPV IN PAST SIX MONTHS, POST-END LINE, POOLED NORTH AND SOUTH

	Emotional or Physical	Emotional	Physical	Emotional or Physical	Emotional	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
<i>p</i> -value: Transfer = Transfer+BCC	0.12	0.19	0.01	0.10	0.16	0.01

Marginal effects of probit models. Extended controls include baseline characteristics of woman and husband. Standard errors clustered at village level. * $p < 0.1$; ** $p < 0.05$; and *** $p < 0.01$.

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).

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:

$$\text{Prob}(Y_{iv} = 1) = \Phi(\alpha + \beta_1 \text{Transfer}_v + \beta_2 \text{TransferBCC}_v + \delta R_{iv}), \quad (1)$$

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 post-end line, and R_{iv} is an indicator for the study region, which is the level of stratification. Transfer_v is an indicator that equals 1 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 1 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 0 to 5 years old, her number of children 6 to 15 years old, and her age); her husband's characteristics (whether he can read and write, his years of education, and his age); and household characteristics (household size). Equation (1) is extended to the following probit model:

$$\text{Prob}(Y_{iv} = 1) = \Phi(\alpha + \beta_1 \text{Transfer}_v + \beta_2 \text{TransferBCC}_v + X'_{iv} \gamma + \delta R_{iv}), \quad (2)$$

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, which was the level of randomization.

V. Results

Table 1 presents the main impact estimates of TMRI on IPV six to ten months after the program ended. The first three columns present base estimates (equation [1]), and the last three columns present estimates with extended controls (equation [2]), Table 1 reveals that transfers alone have no significant impact on emotional or physical IPV six to ten months after the program ended. However, transfers+BCC cause a statistically significant reduction in physical violence, by 6 to 7 percentage points. This impact represents a 26% decrease in physical violence relative to the mean of the control group. The difference in coefficients between transfers and transfers+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 present estimates only for specifications with extended controls.⁷ Results are also robust to relaxing the sample restrictions to using all women who responded to the IPV module at post-end line, using linear probability models, and adjusting for multiple testing (see appendix D).

We explore the robustness of our results in several additional ways in tables 2 to 4. Table 2 examines postprogram impacts on the frequency of violence, using an additive scale and a maximum scale.⁸ Results reveal that impacts on this intensive margin are similar to those on the extensive margin; six to ten months after the program ends, transfers have no impact on the frequency of violence using either scale.

⁷The base specifications and extended controls produce similar results on all subsequent estimates as well; the extended controls improve precision.

⁸The 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.

TABLE 2.—IMPACT OF TREATMENT ARMS ON FREQUENCY OF IPV IN PAST SIX MONTHS, POST-END LINE, POOLED NORTH AND SOUTH

	Emotional or Physical (0–30)	Emotional (0–12)	Physical (0–18)	Any Emotional or Physical (0–3)	Any Emotional (0–3)	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
N	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+BCC	0.10	0.40	0.01	0.13	0.18	0.01

Extended controls include baseline characteristics of woman and husband. Score ranges are shown in parentheses in the column headings. Standard errors clustered at village level. * *p* < 0.1; ** *p* < 0.05; and *** *p* < 0.01.

TABLE 3.—IMPACT OF TREATMENT ARMS ON PREVALENCE OF IPV ACTS IN PAST SIX MONTHS, POST-END LINE, 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 (0.04)	–0.03 (0.04)	0.64
Belittled or humiliated you in front of other people	0.23	0.02 (0.04)	0.02 (0.04)	0.98
Done things to scare or intimidate you on purpose	0.56	0.01 (0.04)	–0.02 (0.04)	0.37
Threatened to hurt you or someone you care about	0.14	0.02 (0.03)	–0.01 (0.03)	0.28
Slapped you or thrown something at you that could hurt you	0.26	–0.00 (0.02)	–0.06 (0.03)**	0.02
Pushed you or shoved you or pulled your hair	0.13	–0.01 (0.02)	–0.03 (0.02)*	0.20
Hit you with his fist or with something else that could hurt you	0.12	–0.02 (0.02)	–0.05 (0.02)***	0.05
Kicked you, dragged you, or beat you up	0.10	–0.01 (0.01)	–0.04 (0.01)***	0.04
Choked or burned you on purpose	0.03	–0.01 (0.01)	–0.02 (0.01)**	0.11
Threatened to use or actually used a gun, knife, or other weapon against you	0.02	–0.01 (0.01)	–0.02 (0.01)**	0.12

Marginal effects of probit models. Extended controls include baseline characteristics of woman and husband. Standard errors clustered at village level. * *p* < 0.1; ** *p* < 0.05; and *** *p* < 0.01.

However, transfers+BCC have a large and negative impact on the frequency of physical violence according to both scales.

Table 3 reveals postprogram impacts on the individual acts of violence used to construct the aggregate indicators in table 1. 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 3 correspond to indicators of emotional violence, and the last six rows correspond to indicators of physical violence. Results are consistent with the aggregate impacts in table 1. Six to ten months after the program ended, transfers have no impact on any of the ten emotional or physical violence indicators. Transfers+BCC have no significant impact on any of the four emotional violence indicators but have a significant (or weakly significant) negative impact on each of the six physical violence indicators.

Table 4 disaggregates impacts by the North and South, allowing us to see if the pooled impacts in table 1 mask differential patterns by region or modality. Results reveal similar impacts across the North and South. In particular, six to

TABLE 4.—IMPACT OF TREATMENT ARMS ON PREVALENCE OF IPV IN PAST SIX MONTHS, POST-END LINE, NORTH VS. SOUTH

	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
p-value: Cash = Cash+BCC	0.83	0.92	0.05
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
p-value: Food = Food+BCC	0.05	0.05	0.08
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

Marginal effects of probit models. Extended controls include baseline characteristics of woman and husband. Standard errors clustered at village level. * *p* < 0.1; ** *p* < 0.05; and *** *p* < 0.01.

ten 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 7 and 6 percentage points, respectively; neither has any significant impact on emotional violence.⁹ The last two rows in table 4 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, nor are the impacts of Cash+BCC in the North significantly different from those of Food+BCC in the South.

A final concern may relate to social desirability bias driving these impacts (Saunders, 1991). Since our estimates are based on self-reports, this could affect our results if the BCC caused women to perceive reporting physical violence as less socially desirable. While we cannot rule out this possibility, we believe it is unlikely to drive our results. First, the BCC did not discuss emotional or physical IPV; in general, it touched very little on gender or spousal dynamics and would not be expected to change women's perceptions of social desirability related to these issues. Second, if the BCC 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.¹⁰

VI. Mechanisms

We explore three possible mechanisms to explain why Transfers+BCC led to decreases in IPV six to ten months after the program ended, while Transfers alone did not. All are related to the different theories posited in section I for why transfers and BCC may reduce IPV. The first mechanism, which supports household economic bargaining models, is that Transfers+BCC (more so than Transfers alone) led to improvements in a woman'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 post-end line. The second mechanism, which supports social control theories, is that Transfers+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 to men of inflicting violence through post-end line. The last mechanism is that Transfers+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.

⁹Lower statistical significance of coefficients relative to pooled estimates is expected, given smaller sample sizes.

¹⁰Using List experiments on IPV in Peru, Agüero and Frisnacho (2017) find misreporting bias to be associated with women's completed tertiary education but not with any other measure of empowerment. In our sample, respondents' average education is low and does not significantly differ by intervention arm (appendix table A.2).

TABLE 5.—IMPACT OF TREATMENT ARMS ON A WOMAN'S ECONOMIC RESOURCES, ACROSS ROUNDS, POOLED NORTH AND SOUTH

	Midline	End Line	Post-End Line
<i>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
<i>p</i> -value: Transfer = Transfer+BCC	0.16	0.14	0.06
<i>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
<i>p</i> -value: Transfer = Transfer+BCC	0.27	0.00	0.03

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

The three mechanisms are complementary, and we cannot empirically disentangle them. Moreover, they are not exhaustive of all possible channels but instead reflect the main pathways for which we have evidence.

A. Improvements in a Woman's Threat Point

To explore whether transfers+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, agency, and 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. For economic resources, we have quantitative data across all rounds of the survey, while for agency, we have only end-line data; for social resources, we rely on qualitative evidence, descriptive evidence, and supportive evidence from related work.

Table 5 shows the impact of the program across midline, end line, and post-end line 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).¹¹ Transfers alone have no impact across any round on a woman's economic resources, while Transfers+BCC lead to significant improvements across all three rounds. Statistically significant differences between Transfers and Transfers+BCC begin to emerge at end line for the probability that a

¹¹These measures could be outcomes of intrahousehold bargaining as well as determinants. For example, the share of household resources the woman controls might be negotiated between the couple.

TABLE 6.—IMPACT OF TREATMENT ARMS ON AGENCY, END LINE, 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 or failure as own responsibility versus destiny	0.37	-0.05 (0.03)	-0.01 (0.03)	0.27

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

woman works and at post-end line 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 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 & Gupta, 1996; Hashemi, Schuler, & Riley, 1996). However, the addition of BCC changes this, possibly through strengthening their claim to the transfer or increasing their options for work opportunities.

Table 6 presents results on the impact of the program on a woman's agency or self-efficacy, representing her ability to make and act on her choices (Kabeer, 2005; Sen, 2001). We analyze four different end-line indicators that represent a woman's internal locus of control (Levenson, 1974) and her perception of her ability to change her life. Transfers and Transfers+BCC lead to significant improvements in a woman's ranking of herself compared to others on having rights or power and the ability to change her life. However, only transfers+BCC lead to significant improvements in a woman's internal locus of control.

Finally, we provide qualitative and descriptive evidence from the process evaluation regarding the effects of Transfers and Transfers+BCC on women's social capital. As suggested by the quotes that follow, 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 the transfer.

- “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 other ways. By nature of its design, the BCC led to frequent (weekly) interaction with community members for women who were previously socially isolated. Consistent with the literature on self-help groups (Brody et al., 2017), this is likely to have increased social ties and social capital. The BCC community meetings also made the importance of nutrition knowledge more salient in communities. The process evaluation (Ahmed, Sraboni, & Shaba, 2014) reveals that women's increased knowledge of nutrition led them to experience greater interaction and respect in the community. One Food+BCC recipient reported that the BCC training boosted their family's status, saying that “neighbors regularly come over to hear what the family learned in the latest training session” and that this had facilitated a position for them within the community.

A companion paper (Hoddinott et al., 2017) provides quantitative evidence that BCC increased interaction between BCC participants and their nonparticipant neighbors, showing that these neighbors also improved nutrition knowledge and practices through spillover effects. Another companion paper (Hoddinott et al., 2018) finds that increased nutrition knowledge persisted six to ten months after the program ended, suggesting that increases in interaction and respect may have also persisted after the program ended.

B. Social Cost of Physical Violence

Increased social ties due to BCC can also increase the social cost to men of inflicting violence, particularly in Bangladesh. Ahmed (2005) describes that in the context of a group-based microcredit intervention in Bangladesh, greater visibility of women in the public domain due to participation in program activities made it “less possible for husbands to get away with violence without incurring social scorn.” Brody et al. (2017) document eight qualitative studies from South Asia, including from Bangladesh, in which women

TABLE 7.—IMPACT OF TREATMENT ARMS ON HOUSEHOLD RESOURCES, END LINE, POOLED NORTH AND SOUTH

	Mean of Control	Coefficient of Transfer	Coefficient of Transfer+BCC	<i>p</i> -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

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

report that self-help group members put social pressure on men to stop beating their wives.

Section VIA provides evidence that Transfers+BCC led to sustained increases in women's social interaction (weekly group meetings, twice-a-month home visits, contact with nonparticipant neighbors) and social status (fostered by community meetings). Increased interaction may make physical violence more visible, thereby increasing the probability that men are caught. Women's improved social status may also make it more likely that the community frowns on violence inflicted on them, representing "social control." Such factors increase the cost to men of inflicting physical violence (but possibly not emotional violence, which is less visible and perhaps more accepted in communities). Although we do not have direct evidence on community members' responses to IPV, the existing evidence from Bangladesh suggests that women's sustained increases in visibility, social ties, and social status induced by BCC may have increased men's social costs of violence even after the program ended.

C. Poverty-Related Stress

Table 7 reveals that at end line, both the Transfers and Transfers+BCC arms lead to significant increases in household wealth 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, Hoddinott, & Roy, 2014). However, table 7 also shows that end-line improvements are significantly larger for the Transfers+BCC arm than for the Transfers arm. Ahmed et al. (2017) show that this difference is driven by greater use of transfer resources for investment and income generation in the Transfers+BCC arm, suggesting that larger increases in wealth from Transfers+BCC are likely to persist after the intervention ends. Thus, postprogram reductions in IPV from Transfers+BCC could plausibly be due to this group's continuing to experience significantly less poverty-related stress than the Transfers group at post-end line. Supporting this mechanism, we find at post-end line that Transfers+BCC (but not Transfers only) significantly reduce violence against children, which Bobonis et al. (2013) view as a marker of stress in the household (appendix E).

VII. Discussion and Conclusion

Our results show that six to ten months after a transfer program ended, women who 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.

Several limitations to our findings are worth noting. Because the three components of the BCC were bundled, we are unable to identify which components or combination of them drive our results. We believe that all served complementary roles—group meetings built status and social capital, home visits increased visibility of violence, community meetings affected men's social costs and attitudes toward women's participation in the other components—but these cannot be disentangled. Similarly, because BCC was always provided in combination with transfers, we cannot identify the effect of BCC alone on IPV. We hypothesize that BCC would not have had comparable effects absent the transfers. Conditioning transfers on BCC likely led to the high rates of BCC participation; women's restrictions on mobility and opportunity costs in terms of time would have plausibly limited BCC involvement without the incentive of transfers. The importance of transfers is also signaled by our suggestive evidence that women's increased control over economic resources increased their threat points and that households' increased well-being reduced conflict. Finally, we cannot disentangle the contributions of each of our proposed three mechanisms: women's threat points, men's social costs, and household poverty-related conflict. Our suggestive evidence suggests that all three played complementary roles; however, we cannot empirically distinguish these.

Bearing in mind these caveats, 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 these complementary activities shape postprogram effects on IPV. Nutrition BCC is often included in transfer programs that aim to improve household food security and child nutrition; our results suggest that 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 these postprogram impacts on IPV compare to 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 these reductions were not sustained afterward except with the addition of BCC? As noted in section I, we are unable to rigorously distinguish these. However, evidence detailed in appendix F suggests it was likely the latter. First, we find that questions at end line 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% and 17% report decreases, respectively); fewer than 1% report increases in either arm. Second, women in both the Transfers and Transfers+BCC arms report improvements in relationships with their husbands compared to the control group at end line and post-end line. Although the proportion reporting improvements is significantly higher in the Transfers+BCC arm at post-end line, there is no significant difference in these proportions between the two arms at end line. Finally, in the process evaluation, we find qualitative support from an interview of a beneficiary woman receiving transfers only (Ahmed et al., 2014), who says that she feels that their improved economic status has led to better relations between her and her husband. Previously, if she asked her husband to buy food when there was none in the house, he would become angry and hit her. At the time of the process evaluation, she says he is generally quite pleasant and does not fight with her anymore.

An implication of our findings is that while transfers alone 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 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 offer evidence that providing women with transfers while also engaging them and their household and commu-

nity 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 ten months after the end of transfers; to a program exposure shorter than two years; to a different type of BCC (e.g., one that was less intensive, involved different combinations of components or topics, or differently targeted members of the household and community); to a different type of transfer program (e.g., one that challenged 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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