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Women's Promotions and Intra-household Bargaining: Evidence from Bangladesh

Hannah Uckat*

Department of Economics, University of Oxford

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Abstract

This paper investigates how women's promotions in the workplace affect bargaining in the household. I exploit the design of a promotion programme for women in 27 Bangladeshi garment factories, by comparing women who were quasi-randomly selected for the programme to the shortlisted runners-up. Results using three different estimation approaches (OLS with post-double selection Lasso, regression discontinuity, and matching) show that women's bargaining power increases as a result of the promotion. The effects are largest for the share of income households spend on assignable goods for women (especially clothing and accessories) and remittances. The latter appears to mask expenditures on children, since remittances increase most for women whose children live with other relatives. I find that these direct effects of the promotions are amplified by impacts on women working as subordinates of the new female managers. Using the quasi-random assignment of sewing-line operators to production lines for identification, I observe that women exposed to a female manager have more say in decision-making in the household, especially about their own mobility. Overall, I find suggestive evidence that both the direct and the indirect impacts are driven by women gaining confidence to get involved in bargaining, rather than income effects that ease the budget constraint or changes in the relative wage in the household.

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1 Introduction

Women's leadership in politics and business has received a lot of attention in the last two decades. To improve dismal participation rates, in 2019 more than half of all countries in the world had some type of political quota for women, and 28 countries had introduced a quota on company boards.[1] Despite these efforts, progress has been slow. In Asia, women even now represent only 19.7 % of members of parliament and 12.8 % of corporate board members.[2] Policies that encourage female leadership typically have at least two goals: to improve the participation of a previously under-represented group, and to achieve alleged instrumental benefits on outcomes, such as increased effectiveness of public spending, or higher profits. While the evidence for both goals generally points to more successful interventions in the political compared to the business sphere,[3] one aspect has largely been missing in the discussion: How does female leadership impact on bargaining in the household, both for the pioneering female leaders and the women who are exposed to them?

The literature has largely been mute on this issue, but there is more extensive work analysing the effects of women entering employment for the first time or changes in income. On the theoretical side, standard collective models of intra-household bargaining predict that women's potential earnings outside the marriage, her — potentially endogenously determined — labour supply and favourable changes in the relative wage will increase her bargaining power (Browning et al., 2014; Basu, 2006).[4] The empirical literature mirrors this picture. As reviewed in Heath and Jayachandran (2018) and Duflo (2012), most empirical studies have found positive impacts of women's employment on their bargaining power as well as final household outcomes, such as children's health and education. The only paper focusing on the effects of promotions on the households of the promoted, Folke and Rickne (2020), provides a cautionary tale. As I will further discuss below, the authors find that top-level promotions in Sweden double divorce rates for promoted women.

I analyse the impact of female leadership on intra-household bargaining in the context of the garment sector in Bangladesh. The work the sector provides is similar to many labour-intensive manufacturing industries in low- or middle-income countries. The garment industry is the main source of formal wage employment for Bangladeshi women, in a country where women traditionally did not work outside the home (Asadullah and Wahhaj, 2016). Nevertheless, the positions available to women in the industry continue to be limited to the non-managerial level. About 80 % of production-line workers in the sewing sections are typically female, compared to only 5 % of line supervisors, the lowest-level management position (Macchiavello et al., 2015; Menzel and Woodruff, 2019). The production-line operators each perform one sewing operation repeatedly throughout the work day, before passing the garment on to the next machine operator in the production line. Production lines are usually managed by one or several line supervisors, who communicate with workers and higher-level managers to ensure that the line is meeting its target. This paper studies the impacts on intra-household bargaining if a woman gets promoted from production-line operator to line supervisor.

[1] International Institute for Democracy and Electoral Assistance at https://www.idea.int/data-tools/data/gender-quotas/quotas and Corporate Women Directors International at https://globewomen.org/CWDINet/wp-content/uploads/2019/07/Quota-Chart-June-2019.pdf (both last accessed 11 November 2019)

[2] Inter-Parliamentary Union at http://archive.ipu.org/wmn-e/world.htm and and Corporate Women Directors International at https://globewomen.org/CWDINet/wp-content/uploads/2017/07/1.jpg (both last accessed 11 November 2019). Note that the percentage of corporate board members refers to the Asia-Pacific region.

[3] See Miller (2018) and Pande and Ford (2011) for reviews.

[4] Some theoretical extensions, however, caution that female employment has the potential to lead to conflict and violence in the household (Eswaran and Malhotra, 2011; Ramos, 2018).

The ideal empirical scenario to analyse this research question would be by randomly assigning promotions. However, this is difficult to implement in such a highly competitive and high-pressure environment in the private sector. Effective supervisors are crucial for the smooth functioning of lines, and even small decreases in productivity can potentially lead to missed production targets, delayed shipping dates for orders and significant reductions in revenue and profits. Understandably, profit-maximising firms are therefore reluctant to allow their promotion offers to be determined by chance. Instead, I exploit variation created by a promotion programme for about 200 women in 27 garment factories. In Uckat and Woodruff (2019), we describe the programme in detail and analyse its effect on factory outcomes. As part of the promotion programme, the participating female production-line operators trialled as line supervisors for at least two months, were trained for this role and, if successful, were officially promoted. All participating women therefore gained experience in a management position whose leadership responsibilities were very different from the work they did before.

Drawing intuition from a simple collective household framework following Browning et al. (2014), I posit that a woman's promotion could affect the bargaining process through three potential channels. First, if a woman's promotion is accompanied by an increase in her income, this eases the household's budget constraint, ceteris paribus. I call this a Marshallian income effect. Second, a change in the woman's income can, ceteris paribus, affect the relative wage of the individuals in the household. This relative wage effect could affect the woman's weight in intra-household bargaining irrespective of the effect on total household income (i.e. as a distribution factor). Lastly, the experience in a leadership position without any change in income or in the relative wage could also improve the women's bargaining position in the household as a distribution factor, which I call a position effect. This could, for example, be the case if the new position gives the woman more confidence to get involved in bargaining in the household, or if she can translate the bargaining skills learned in the new position to the household. I will return to this below when discussing results.[5]

To study the impact on the household, I conducted two waves of extensive surveys and economic games with the women involved in the programme and with their husbands, or another adult household member if the woman was unmarried. Because the vast majorities of respondents were married couples, for simplicity I refer to them as "wife" and "husband" in the rest of the paper. The respondents were interviewed simultaneously but separately at their homes. My main outcomes capture different aspects of bargaining power and household decision-making, and consist of the household's income share spent on assignable goods for women, the income share of remittances, recent experiences of domestic violence, the women's involvement in decision-making, as well as economic games inspired by Almås et al. (2018).[6] I also conducted additional high-frequency phone surveys with the wives for statistical power.

I compare outcomes of women who were selected for the programme to the nominated and shortlisted runners-up, exploiting the selection process for the programme as a quasi-experiment. At the start of this process, we determined the number of new supervisors to be selected in each factory together with the respective management. This factory-specific cutoff was set in such a way that the factory would be able to absorb all selected operators as new supervisors within a few months. Factories then provided us with a ranked list of female production-line operators as candidates, who had to

[5]Note that, as I explain Section 2, supervisors only work about 20 minutes per day longer than production-line operators. Therefore, it is unlikely that a promotion affects the household via less time spent at home.

[6]For funding reasons, the home surveys took place after the conclusion of the promotion programme.

fulfill pre-set eligibility criteria. Our team further excluded individuals that failed diagnostic literacy and numeracy tests. These extensive eligibility checks meant that only highly skilled and experienced operators with a supportive family were included in the shortlist for the programme. Out of these eligible and shortlisted operators, only those with a rank that was better than the factory-specific cutoff were selected for the promotion programme. The comparison group consists of the nominated and shortlisted women who passed all diagnostic checks but continued to work as production-line operators.

I argue that the assignment to the selected group is quasi-random because factories have limited ability to predict which women would become successful supervisors among the shortlisted group of women. Because the factories had historically nearly always promoted men, they had large numbers of skilled and experienced female line operators from which to choose new supervisors. The eligibility criteria and diagnostic tests further ensured that the considered line operators were a homogenous sample. While it might be easy for factories to identify operators who would *not* make a good supervisor in their factories, it is much harder to decide the ranking among a sample of women who had *all* been judged to be suitable after an extensive screening. This argument is similar in spirit to the literature on microenterprises, recruitment and venture capitalism, which shows that humans are often inept at predicting which firms, workers or investments will be successful among an already select sample (McKenzie and Sansone, 2017; Hoffman et al., 2018; Nanda, 2016). Since the management was interested in identifying the best supervisors for the factory, it is especially unlikely that they ranked the eligible operators based on their potential *household* outcomes, which are the focus of this paper. I conduct extensive checks comparing the two groups, consisting of t-tests, tests of joint orthogonality and normalised differences following Imbens (2015), for which I use a wide range of variables from the promotion programme baseline. All of these tests show that that two groups do not differ on observable criteria and provide strong support for quasi-random assignment.

I use three estimation approaches: machine-learning techniques to choose predictive control variables for OLS (Belloni et al., 2013, 2014a,b, 2016), a regression discontinuity approach using the operator rankings, and a matching method. They all lead to the same conclusions. I find that the promotion programme was successful in advancing women's careers. All selected women were assigned to work as supervisors for at least two months, and therefore gained experience in a leadership position. Within the first year after the promotion programme start, selected women were 24 percentage points more likely to have been officially promoted to supervisor compared to the shortlisted comparison group, and more were still waiting for an official promotion at the time of the surveys. For the promoted women, personal income increased by about a third compared to the shortlisted group.

When investigating household outcomes, I find that the women who were selected for the promotion programme enjoy significantly higher bargaining power than the comparison group. The effect is largest on the share of income that households spend on assignable goods for women, especially clothing and accessories, as well as the income share of remittances. When using PDS Lasso to analyse effects, the share of household income spent on women's goods increases to about 8 %, compared to 5.3 % in the comparison group. I cannot entirely exclude that part of this effect might be driven by the need to look more professional in the new managerial position, but argue that increasing effect sizes over time make this improbable. This is also unlikely to be the reason for the large effects I find on remittances, which increase from 6 % of household income to about 9.5 %. This large effect appears to result primarily from transfers to family members who take care of the couple's children living outside the household.

In this sample, about half of all children live with relatives outside the household. The large increase in remittances hence seems to mask expenditures on children. These results are confirmed when I implement a regression discontinuity approach relying on the ranking of the shortlisted operators, as well as a matching method that uses variable from the promotion programme baseline. In addition, I check robustness for, amongst others, different specifications, different variable definitions, and different data sources (i.e. the husband's survey and high-frequency phone surveys). I also show that the effects do not primarily result from an effect of the training that was part of the promotion programme.

I then investigate which, if any, of the potential three channels identified in the collective household framework are driving the results. The sample size makes it difficult to make definitive statements, but the different pieces of evidence point me to the same conclusions. I show that a Marshallian income effect is unlikely for two reasons. First, I do not find an effect on unmarried women living on their own. For this group, an income effect would be expected to operate, but a bargaining effect is excluded by definition. Second, I do not see effects on households' expenditures on assignable goods for men and boys, but indications of a positive effect on girls' assignable expenditures. This suggest that a reallocation of expenditures towards female assignable goods has taken place, rather than a Marshallian income effect. I also present evidence that a relative wage effect is improbable, by showing that the impacts are not larger for selected women who started earning more than their husbands. I find most evidence in favour of the leadership position effect driving the results. I see strong positive impacts on women's expenditures and remittances even for women who were not yet officially promoted at the time of the household surveys. These women still had experience in a leadership position during the promotion programme, but did not see their income increase. I observe some evidence that this position effect operates by improving the women's attitudes and beliefs about themselves, which could enable them to get more involved in decisions in the household.

These results suggest that the promotion programme increased the women's bargaining power in the household, if one is willing to assume that women have a higher preference than their partners for female assignable goods expenditures and for supporting family outside the household.

While this is encouraging evidence for policy approaches that aim to increase women's participation in leadership, the direct effects of promotions will only affect the small share of the female population that attains a management position. In the next step, I therefore analyse whether there are any indirect effects of female leadership on women working under a female manager. These effects would potentially be relevant for a much larger number of women. I compare outcomes of female sewing-line operators who were working on lines to which the newly selected female supervisors were assigned for the promotion programme to outcomes of women who were working on other lines with male supervisors. According to industry experts, the assignment of operators to production lines is first and foremost determined by production requirements. It is especially unlikely that workers can sort to lines of new supervisors in their trial period, since factories use these periods to observe the performance of potential new supervisors with the resources that they have been given. Several tests indeed indicate that the two groups I compare are very similar. According to time-invariant observable characteristics, these two groups are balanced on all tested variables, and neither are the observable characteristics jointly predictive of being assigned to a line with a female manager. I again use machine-learning methods to identify control variables for OLS in my estimation approach, but find that the control variables hardly matter.

Having interviewed about 500 exposed and non-exposed wives and their husbands about nine months

after the start of the exposure to female supervisors, I find that the exposed women are considerably more involved in decision-making in the household, especially regarding their own mobility. I see no changes in expenditure outcomes, but some statistically insignificant suggestions of increased conflict in the household. I again evaluate the potential channels identified in the collective household framework, and do not find any suggestions of a Marshallian income or a relative wage effect. There is some tentative evidence that the increased involvement in decision-making is resulting from women being more willing or able to get involved in negotiations in the household. The observed increase of women's involvement in decision-making is again robust to a wide range of checks, though I find a much smaller effect when looking at the husband's data.

Taking the results together, this paper therefore provides evidence that taking a step up on the career ladder can lead to increased bargaining power for promoted women, irrespective of potential income effects or relative wage effects this may bring. Moreover, I see that these direct effects of a leadership position are amplified by impacts on women who work as subordinates of the new female managers, for whom involvement in decision-making in the household increase. Tentative evidence suggests that women's promotions increase women's confidence to get involved in intra-household bargaining, both for female managers as well as the female subordinates working under them. These results underline that there is a potential complementarity between women's power in the workplace and in the household.

With this analysis, I contribute to the vast literature that studies the determinants of and constraints to women's bargaining power and women's empowerment more broadly. While it is beyond the scope of this paper to discuss this literature in depth,[7] I want to highlight my contributions to the two strands of this literature which are most closely related. First, I provide the first evidence about the effect of women's promotions on bargaining in the household. In low- and middle-income countries, the empirical literature investigating how women's work impacts on bargaining power has exclusively focused on the effects of women entering employment for the first time, income increases, or general shocks to female-specific labour demand. Most of these studies have found that better labour market opportunities for women have a positive impact on a range of measures of women's bargaining power, such as domestic violence, expenditures, involvement in decision-making, mobility or women's time use (e.g. Atkin (2009) and Majlesi (2016) for Mexico, Dharmalingam and Morgan (1996), Rahman and Rao (2004), and Luke and Munshi (2011) for India, Getahun and Villanger (2018) for Ethiopia, and Friedemann-Sanchez (2006) for Colombia). However, there are some exceptions. For example, Jensen (2012) finds no impact on mothers' involvement in decision-making or autonomy from increased labour market opportunities for their daughters in India, but he does find that the daughters delayed marriage and childbearing, and had higher labour market participation and more schooling. Also from India, McKelway (2019) reports that employment enabled women to make more independent decisions, but did not increase bargaining power in joint decisions. Blattman and Dercon (2018) find no impact of offering factory work to women in Ethiopia on their autonomy, though this might be due to a lack of statistical power.

Most closely related to this paper is the literature that analyses the relationship between work and women's bargaining power in Bangladesh. Anderson and Eswaran (2009) and Kabeer et al. (2018) both find that work, and particularly formal work outside of the household, is positively related to women's autonomy. Regarding the Bangladeshi garment industry in particular, the Asian Center for

[7] See, for example, Doss (2013) for a review.

Development (2015) reports that "93% of the [surveyed female] workers perceive that this job has given them a voice in family decisions and raised their importance as a family member". These results are supported by Heath and Mobarak (2015), who find that women exposed to the garment sector delay marriage and childbirth. The authors argue that this is due both to increased enrolment of younger girls and a higher likelihood of older girls to be employed. Nevertheless, there is some evidence which indicates that the effects of work on bargaining power may depend on the women's initial circumstances. Heath (2014) shows that women with an initial low bargaining power (proxied by low education and low age at marriage) report higher domestic violence upon entering work, but does not find such an increase for women with higher bargaining power to begin with. This paper adds to this literature in low- and middle-income countries by providing the first evidence about the impacts of a promotion on the household. It thus goes beyond the impact of women taking up work, and instead investigates how the household responds if women climb up the first step of the career ladder. I also provide evidence on potential channels related to a promotion: a leadership position effect, a Marshallian income increase and a change in the relative wage.

In high-income countries, the literature has paid more attention to women's promotions, but the studies have mainly focused on women entering top-level corporate roles and the resulting impact on firm outcomes (See e.g. Bertrand et al. (2019) and Dale-Olsen et al. (2013) analysing Norway's quota for women on company boards, Flabbi et al. (2019) for Italy, Marinova et al. (2016) for Denmark and the Netherlands, Deszö and Ross (2012) for the USA and Sabatier (2015) for France). One exception, and closest to this paper, is Folke and Rickne (2020). The authors analyse the impact of women's and men's promotions to top political and corporate positions on divorce rates in Sweden. They find that a promotion doubles divorce rates for promoted women, but not for men. These effects are particularly pronounced for couples with traditional gender roles in the early phases of the relationship. The authors provide evidence that these higher divorce rates could result from a stressful re-negotiation of household tasks after the women's promotion or from a violation of social norms if the wife becomes the dominant earner. In comparison to Folke and Rickne (2020), this paper focuses on women's promotions to entry-level management positions in a low-income context. And while Folke and Rickne (2020) exclusively discuss divorce, i.e. household breakdown, this paper focuses on a wide range of intra-household bargaining outcomes within continuing marriages.

As a second contribution, this paper presents the first evidence of the impact of female role models in the workplace on intra-household bargaining. The literature so far has primarily focused on female role models in the political sphere, in teaching environments or in the media, and has shown that female role models can be instrumental in changing norms as well as behaviour. For example, Beaman et al. (2009, 2012) exploit the random assignment of women's quotas to Indian village councils to investigate the impact of exposure to female political leaders on perceptions and girls' outcomes. The authors find that villages which were exposed to female politicians in two election cycles had weaker gender stereotypes and perceived women as more effective leaders (Beaman et al., 2009). In addition, the exposure erased the gap in educational attainment for girls in these villages, increased their and their parents' aspirations for them, and decreased the time girls spent on household chores (Beaman et al., 2012). The literature from teaching environments points to similar effects, with many studies finding that female teachers increase female students' performance as well as their career aspirations and choices (e.g. Carrell et al. (2010), Dee (2005), Bettinger and Long (2005), Asgari and Dasgupta (2010), Antecol et al. (2014)).

While none of these analyses from the political and educational environments investigate the impacts on measures of women's bargaining power, the literature on media exposure has demonstrated the potentially large effects that role models can have on these outcomes.[8] In a seminal paper, Jensen and Oster (2009) show that the staggered introduction of cable television across five Indian states had large impacts on outcomes for women living in rural areas. Within the first year of the arrival of television, the authors find a lower acceptability of domestic violence, diminished son preference, and increases in women's autonomy. These changes also translate into a reduction in fertility as well as increases in school enrolment for young children. In a similar vein, Chong and La Ferrara (2009) and La Ferrara et al. (2012) demonstrate that soap operas depicting modern women and relationships reduced fertility and increased rates of separation and divorce in Brazil.

In comparison, this paper investigates the impact of a female role model in the workplace — a female manager on the production line under whom the women work for at least ten hours a day for six days a week in the factory. This exposure to a female supervisor is therefore arguably more direct compared to female political leaders, as well as more direct and more sustained compared to television characters. It most resembles the type of exposure found in teaching environments, but affects the exposed women at a different point in their economic lives. Macchiavello et al. (2015), also working in Bangladesh's garment industry, provide suggestive evidence that having a female line supervisor has positive, though insignificant, effects on female operators' career aspirations. I extend this analysis and investigate whether having a female manager in the workplace changes household bargaining for exposed subordinates.

The paper is structured as follows. I provide an overview of the context in Section 2, and explain the conceptual framework and the study design in Section 3. Section 4 presents the empirical strategy and results for the analysis of the effects on the participants of the promotion programme, which I call the "promotion analysis". Section 5 does the same for the analysis of role model effects, which I call the "exposure analysis". Section 6 concludes.

2 Context: Women in Bangladesh's garment industry

The garment industry in Bangladesh experienced explosive growth from the 1980s onwards. It currently represents more than 80 % of Bangladesh's exports, consists of at least 4,500 factories and employs more than 4 million workers, according to the sector's largest trade association, the Bangladesh Garment Manufacturers and Exporters Association (BGMEA).[9] Figures A.1 and A.2 in the appendix illustrate this impressive development.[10]

The production in garment factories is usually divided into at least three sections—cutting, sewing, and

[8] See DellaVigna and La Ferrara (2015) for an excellent review of the economic and social impacts of media, and La Ferrara (2016) for a review focusing on the potential of the media to further developmental objectives.

[9] According to the BGMEA, its members account account for 100 % of woven garment exports, more than 95 % of sweater exports, and around half of light knitwear exports. (http://www.bgmea.com.bd/home/about, last accessed August 7, 2019)

[10] Note that the number of BGMEA member factories dropped by more than a quarter in 2013-2014. This coincides with the period after the collapse of the Rana Plaza factory in April 2013, during which more than a thousand workers died. In the aftermath of the collapse, the Bangladeshi government, the BGMEA and buyer organisations intensified inspections and closed down some factories, but it is unclear whether this alone explains the large drop. It is also possible that factories deregistered to avoid scrutiny or that non-operating factories were purged from lists following inspections. For more information on the response to the Rana Plaza disaster, see Boudreau (2019).

finishing.[11] After the fabric is cut into the pieces required for the garment in the cutting section (e.g. a front and back piece, two sleeves and neckbinding for a simple T-shirt), the fabric pieces are sewn together to create the garment on the sewing floors, before being passed on to the finishing section for ironing and packaging. The sewing sections employ the majority of workers. They are almost always divided into production lines, each taking the cut fabric pieces as inputs and outputting the complete garment. These sewing lines consist of 20-80 workers, depending on the garment produced, which each perform one sewing operation (e.g. binding the neck for a T-shirt) at a workstation. Lines are typically composed of line operators of different skill levels and helpers doing auxiliary tasks. The production lines are usually managed by at least one line supervisor, several if the line is longer.[12]

The garment industry is the main sector in which Bangladeshi women can find formal wage employment, in a country in which women have traditionally not worked outside the home (Asadullah and Wahhaj, 2016). While statistics from a representative sample of factories are hard to come by, it is estimated that 56-65 % of all workers in the sector are female (Labowitz and Baumann-Pauly, 2015; Asian Center for Development, 2015). However, the opportunities for women in the industry still remain mostly limited to non-managerial positions. Among the line operators and helpers in the sewing section, about 80 % are typically female, but only about 5 % of line supervisors are women (Macchiavello et al., 2015; Menzel and Woodruff, 2019).

The impact of a woman's promotion from line operator to line supervisor on the household of the female supervisor herself, and the female workers she manages, is the focus of this paper. It is therefore important to understand that the work of a line supervisor is very different from that of operators. Whereas line operators sit at their workstations and repeatedly perform their step of the sewing process, line supervisors are tasked with managing workers to ensure that their (part of the) line is running well. They are typically on their feet, and communicate with workers to motivate them to reach the target set for the line, solve problems with machines or inputs, or explain processes to operators. They also communicate any issues, such as worker absenteeism or machine problems that require technicians, to higher-level managers.

Compared to line operators, supervisors work only slightly longer hours, to set up the line for the day and wrap up after production has finished for the day. To illustrate, I compare the hours worked on the last working day reported by line supervisors and by line operators in the part of my sample that is unaffected by the promotion programme.[13] This difference is only about 11 minutes when controlling for factory and month fixed effects, and about 19 minutes without these additional controls. Since line operators in my sample work on average 10 hours per day, line supervisors' work hours are only about 2-3 % longer. Officially promoted line supervisors also earn more than operators. In the sample of factories included in this paper, the highest-skilled operators earn approximately 10,000 Taka per month (about 100 GBP and 125 USD at the time of the surveys).[14] Established supervisors on average

[11] Some factories have additional sections, such as those that spin thread from the raw materials, knit fabric from the thread, dye the fabric, add embroidery to the fabric, or create stonewash in jeans. See Menzel and Woodruff (2019) for a more extensive discussion of the production process, and the differences in grades among sewing operators.

[12] The hierarchy levels above line supervisors in the production department vary with the size of the factories. Larger factories often employ line chiefs responsible for several lines, floor managers responsible for one sewing floor, assistant production managers responsible for one unit, and production managers who report to the general manager. In smaller factories, which might have only few floors and only one unit, there are typically fewer hierarchical levels.

[13] I focus on the part of the sample whose work hours, conditional on working, are likely to be unaffected by the promotion programme. I therefore exclude the nominees and selected women for the promotion programme, as well as the women working with the selected women as supervisors. Note that this is not a representative sample, but serves as an illustration.

[14] These numbers represent net pay, which includes basic pay, overtime pay, and all allowances, minus any deductions,

earn approximately 14,000 Taka per month (about 140 GBP and 175 USD). Note, however, that the income increase for newly promoted supervisors is smaller, as I find below.

It is important to note that women in the garment industry already possess a higher level of bargaining power than one would find for the average Bangladeshi woman. Traditionally, women in Bangladesh were expected to tend to the homestead, and keep out of the public sphere (Asadullah and Wahhaj, 2016). There has been impressive progress towards gender equality, especially in education and health, in the decades coinciding with the growth of the garment industry. While it is beyond the scope of this paper to discuss the relative contribution of the garment sector to gender equality in Bangladesh,[15] it has widely been argued that work in the sector has enabled women to have more say in decision-making in the household and to be more visible in the public sphere (e.g. Asian Center for Development (2015), The World Bank (2008), Kabeer et al. (2018), Heath (2014), and Heath and Mobarak (2015)). For example, in the survey for Asian Center for Development (2015), 93 % of female garment workers reported that their job had "given them a voice in their family decisions and elevated their position in the family".[16] Since this paper is focused exclusively on women working full-time outside the home in the garment industry, it is not immediately obvious whether women's promotions to managerial positions can a priori be expected to have an additional positive effect on these women's bargaining power in the household, considering the high initial bargaining power.

3 Framework and study design

3.1 Intuition from a collective household framework

I illustrate the intuition for the potential effects of a woman's promotion on intra-household bargaining using a simple collective household framework, following Browning et al. (2014). Such a collective approach assumes that a household makes Pareto efficient decisions and that it follows a stable decision-making process. In addition, I assume that the individuals' utility functions are strictly concave. This allows me to write the optimisation programme as follows, for a household with individuals a and b that chooses consumption of private good q of price p and public good Q of price P, with income m depending on wages w:[17]

$$\max_{q_a,q_b,Q} \quad \mu(P, p, \frac{w_a}{w_b}, \textit{promotion}, \boldsymbol{z}) \cdot u_a(q_a, Q) + u_b(q_b, Q) \tag{1}$$

$$s.t. \quad p \cdot (q_a + q_b) + P \cdot Q = m(w_a, w_b) \tag{2}$$

Ignoring the different colours for the moment, the first line of this programme shows that the household maximises a weighted sum of the individuals' utility functions, where the individual utility functions

e.g. for absences.

[15] See e.g. The World Bank (2008) for a review of this progress and the drivers.

[16] However, note that Heath (2014) finds that women in Bangladesh who work for pay and with low baseline bargaining power are at an increased risk for domestic violence. She argues that the violence is used by husbands to counteract the increase in bargaining power women experience from paid work.

[17] Of course, one can extend this simple framework, e.g. by allowing for altruistic preferences, introducing domestic production or labour supply decisions. Nevertheless, this simple framework is instructive to gain intuition for the potential effects of a promotion.

$u_s(q_s, Q)$ for $s = \{a, b\}$ depend on the individual's private consumption of q as well as the level of the public good Q. The public good, for example, could be thought of as expenditures on children. The weight $\mu(.)$ is typically called the bargaining or Pareto weight, and represents the weight given to a's utility in the household programme. The bargaining weight is a function of the economic environment (e.g. prices) as well as potential distribution factors. Distribution factors are variables that can affect the decision process but do not affect the budget constraint or preferences, e.g. the relative wage of the individuals (once total income has been taken into account), or other factors collected in the vector $\mathbf{z}$. The second line of the programme represents a standard budget constraint, where I assume that expenditures on the private and public goods exhaust the household's income.

I posit that a woman's promotion could affect the bargaining process through three potential effects.

1. *Marshallian income effect*: If a woman is promoted with an increase in her wage rate w_a, this eases the household's budget constraint. This would lead to an increase in the set of feasible allocations for the household.

2. *Relative wage effect*: Ceteris paribus, a change in the woman's wage rate w_a affects the relative wage of the individuals. As a potential distribution factor, this could affect the woman's bargaining weight.

3. *Position effect*: Irrespective of any change in total income or relative wages, a promotion to a leadership position could be a distribution factor that affects the woman's Pareto weight. This could take place, for example, if the promotion to a leadership position in the workplace affords the woman a more important role in decision-making in the household, or if she can translate bargaining skills learned in the new position to the household, or if her new position gives her the confidence to get more involved in bargaining in the household.

In Section 4.4, I investigate which of these effects, if any, I observe in my data for the women who were part of the promotion programme.

Similar effects could potentially be at play for the second component of my analysis, where I analyse the effects of women being exposed to the new female managers. Since the subordinates of the female supervisors continue to work in a line operator position, a large change in the wage rate working through a Marshallian income effect or a relative wage effect is possible but arguably less likely.[18] Instead of a position effect, I posit that the third potential effect for line operators exposed to the new female managers would instead read as:

3. *Exposure effects*: The exposure to a female manager can be a distribution factor affecting the women's bargaining weight. This could be the case, for example, if working with a female supervisor as a role model changes the exposed women's perception about a woman's permissible bargaining position in the household, or if the exposed women are able to emulate the management techniques implemented by the female supervisors at their homes.

In Section 5.4, I investigate which of these three effects, if any, I observe in my data for the women who were exposed to the new female managers.

[18] Line operators' wages typically include boni for good attendance and incentives based on production line productivity, which could both potentially be affected by the supervisor. However, these components typically constitute only a small share of line operators' income.

Table 1: Eligibility criteria for promotion programme

Area	Criterion
Panel A: Selection criteria for factories	
Education	$\geq$ 8 years
Experience in garment industry	$\geq$ 2 years
Operator grade	Operator (grade 4) or Senior operator (grade 3)
Interest in supervisor position	Yes
Supportive family	Yes
Panel B: Exclusion criteria using diagnostic tests	
Literacy	Passed basic test
Numeracy	Passed basic test

3.2 Promotion programme

For the analysis, I exploit variation created by a promotion programme for women in the Bangladeshi garment industry, during which selected women worked as production line supervisors for at least two months. The impacts of the training on factory outcomes is analysed in Uckat and Woodruff (2019). The way in which female production line operators were selected for the programme allows me to compare household outcomes of new female supervisors to outcomes of female operators who were short-listed for the programme but did not make the cut because of space constraints.

We only considered skilled and experienced operators for the programme. We worked with 27 large Bangladeshi garment factories. At the beginning of the programme, the factories were asked to provide a ranking of female operators that they wanted to consider for new supervisor positions. Those operators had to fulfil pre-set eligibility criteria in five areas, which are described in Panel A of Table 1. We conducted baseline surveys in the factories with all nominated women. We further implemented diagnostic tests with all candidates and excluded women who did not pass basic literacy and numeracy tests (see Panel B in Table 1). Only women who passed all screening criteria in Table 1 were included in the shortlist. These criteria mean that all short-listed women were skilled production line operators with significant experience in the garment industry and more than primary education. They were also interested in becoming a supervisor and had a family that supported this interest. In total, the 27 factories short-listed 243 eligible women.

In each factory, only the top-ranked candidates in the shortlist were allowed to participate in the promotion programme. We determined a factory-specific cutoff in advance in collaboration with each factory management team. The goal was to only include as many participants in the promotion programme as the factory would be able to absorb as new supervisors in a few months, such that selected operators had a good chance of officially being promoted during the programme duration. Using the final shortlist of operators[19], only the women with a better rank than the factory-specific

[19]We also cross-randomised a two-hour management training on standardised worker evaluations on the factory level. Factories that were randomly selected to participate in this training were able to change their ranking of shortlisted operators once, after they received the diagnostic scores for all tested candidates. Nominees in the other half of the factories undertook the diagnostic tests, but the results were not revealed to the factory, and the original ranking could not be changed. For factories that were assigned to the management training, the final shortlist is the last ranking after the management training, after exclusion of ineligible operators and a potential re-ranking. For factories that did not receive the management training, the final shortlist coincides with the original ranking of operators, after ineligible nominees were excluded. To account for these potential selection effects, I include all diagnostic scores and factory fixed effects in the vector of potential control variables, as discussed in Section 4.1.

cutoff were assigned to the promotion programme. In this paper, these women are the "selected" group, consisting of 199 individuals. The women who passed all eligibility checks but whose rank was worse than the factory cutoff continued working as operators and form the comparison group. I call this group, which consists of 44 individuals, the "nominated" group.

Only the selected women participated in the promotion programme, which entailed trialling as an assistant supervisor with gradually increasing responsibility for at least eight weeks. Factories were encouraged to officially promote the selected women as soon as they felt that their quality warranted a promotion. This is similar to how factories promote new supervisors in the industry. Outside of any intervention, factories typically trial candidates for supervisor positions on the job. They first shadow existing supervisors, and then move to supervising a gradually increasing number of workers. If the factory is satisfied with their performance in managing a line or line section on their own, they receive an official promotion once a supervisor vacancy becomes available.

The selected women were also assigned to different training regimes. These varied in their timing of two different modules, which were designed to train the selected women in soft and technical skills to make them better production line supervisors. The soft-skills module consisted of a four-day training in stress management, assertive communication and leadership skills. The hard-skills module provided an overview of the production process and training on how to plan, manage and solve problems on production lines. Appendix A.1 provides more details. One third of the selected women were assigned to receive both training modules immediately. Another third only received the soft skills component immediately and was trained in the technical skills about six months later. The remaining third received both training modules six months later. Remember, however, that all three groups were trialling as assistant supervisors during a two-months period. Figure A.3 presents the timing in detail.

The impact of these trainings on factory outcomes is analysed in Uckat and Woodruff (2019). For the main analysis of intra-household bargaining, I pool all women assigned to be participants of the promotion programme in the "selected" group, and compare them to the "nominated" group. In Section 4.4, I investigate potential training effects.

3.3 Survey implementation

To collect the outcome variables of interest for this paper, I implemented surveys with women and one of their household members at the respondents' homes. For funding reasons, these took place after the conclusion of the promotion programme discussed in the previous subsection. For the analysis of the effects on the promoted women, which I call the "promotion analysis", the field team targeted all 243 women who were nominated and eligible for the promotion programme by the 27 factories.[20] This sample was interviewed twice in person at their homes. The first wave took place on average four months after the beginning of the promotion programme in the factories, and the second wave on average ten months after the beginning of the programme.[21] For power reasons, I also conducted

[20] In addition, we also surveyed 93 nominated and ineligible operators. Out of these, 52 nominees failed the literacy and numeracy tests, 16 were absent during the evaluation and 25 withdrew before treatment assignment. They are excluded from the analysis since they are arguably not comparable to the nominated and eligible women.

[21] For logistical reasons, the promotion programme was implemented sequentially. This ensured that class size for the training were manageable, and that all trainees were taught by the same trainers. The household surveys followed this pattern. I therefore present the time line in reference to the beginning of the promotion programme in the factories. See Figure A.3 for a detailed time line. The first wave of surveys took place between June and December 2017, the second wave between December 2017 and August 2018.

biweekly phone surveys to collect additional measurements for a subset of outcomes of interest.

The sample for the analysis of role model effects on the women working with female supervisors, which I call the "exposure analysis", was determined during the follow-up surveys for the promotion programme in the factories. On each trial line to which a participant of the promotion programme had been assigned to trial as supervisor, female operators were sampled for the household surveys.[22] These are the so-called "exposed" operators. The trial lines had been chosen by the factories as needing a new supervisor before the start of the promotion programme. In addition, in each factory, the same number of comparison lines was sampled from all other lines not involved in the promotion programme, and from these lines, female operators were again sampled to be interviewed in the household. These latter operators are the "non-exposed" group, who are nearly exclusively supervised by men. Overall, 715 operators were sampled. The interviews for the exposure analysis took place on average nine months after the start of the promotion programme.[23]

The home surveys were implemented in two steps. First, the field team conducted short pre-surveys on the phone, using the mobile phone numbers collected during the factory visits. These pre-surveys were used to collect the respondents' location information and to identify the second household member to be interviewed during the home survey.[24] Crucially, the pre-surveys were also used to agree on an interview time that was convenient for both respondents in the household, and to find an arrangement that would allow the respondents to be interviewed separately from each other in a private room where the interview could not be overheard.[25] This was especially important because the respondents' availability was limited due to the long working hours in the garment industry, and because a majority of respondents only had one room available (when excluding bathrooms and kitchens).

In the second step, a team of two enumerators visited the household at the agreed time and administered the surveys to the respondents in the household. For reasons of respondent and enumerator safety, a team always consisted of one female and one male enumerator. The woman known to us from the factory was always interviewed by the female enumerator, and the other interviewed household member by the male enumerator. The surveys were not conducted if privacy could not be maintained.

The enumerator team was entirely separate from the team that conducted the promotion programme and the surveys in the factory, though they were hired by the same field organisation. At no point during the survey administration was the promotion programme mentioned to the respondents. In the consent form, the surveys were explained to the respondents as aiming to understand "how families of workers in the garment industry live, and how the different members in the family interact with one another".

[22] The sampling of lines and operators was implemented using simple randomisation following a prepared computer script.

[23] These surveys were implemented between August 2017 and March 2018.

[24] If the sampled woman was married and living with their husbands in the same household, the husband was identified as the second respondent to be interviewed. If the sampled woman was living with other adults but not currently married, or married but not living with the spouse, the adult in the household that the respondent identified as the main decision-maker was identified as the second respondent. If the sampled woman lived alone or only with minor children, she alone was interviewed. When interviewing the promotion sample in the second wave, we aimed to interview the same second respondent as in the previous wave. Only if the circumstances of the sampled woman had changed, for example if she married between the surveys or the respondent in the previous wave had left the household, was another respondent interviewed. Throughout the project, the following definition of a household was used. "Someone is a member of your household if they have spent at least 6 months out of the last year living in the same house and eating from the same kitchen. If you share a house with other households or families, we are only interested in the primary household/family."

[25] For example, in a number of cases a neighbours' room was used.

Table 2: Survey response rates

Panel A: Promotion analysis

	Selected	Nominated
Wave 1	93 %	91 %
Wave 2	91 %	77 %
Phone (ever reached)	94 %	95 %
Phone (avg. surveys)	7.28	5.98

Panel B: Exposure analysis

	Exposed	Non-exposed
Wave 1	84 %	83 %

Table 2 shows that the survey response rates for the sampled women were very high, above 90 % in the first wave for the promotion analysis sample and above 80 % for the exposure analysis sample. The response rate for the nominated operators dropped to 77 % in wave 2 for the promotion analysis, compared to 91 % of selected operators. In Section 4.3, I show that this attrition is not driving results. About 70 % of both samples were found to be married and living with their husbands in the pre-surveys, who were then identified as the second respondent to be surveyed. Only for approximately 10 % of respondents, other adult household members were targeted, primarily parents and siblings. For readability, I therefore use the terms "wife" and "husband" in the rest of the paper to refer to the women known to us from the factories and all second household member interviewed, respectively. Close to a fifth of respondents lived without another adult in the household, for example in factory dormitories. Table A.1 shows the breakdown in detail. Since I am interested in bargaining between household members, I exclude these individuals from one-person households from the analysis.

3.4 Main outcome variables of intra-household bargaining

I use the survey data to construct six main outcome variables for intra-household bargaining. These are defined in Table 3. The percentage of the household's income spent on assignable goods for women is the first outcome.[26] Respondents were asked to report their expenditures for women in Taka for 1) cosmetics, 2) clothing and footwear, 3) jewellery and accessories, and 4) health and medical expenditures.[27] These expenditures were winsorised, harmonised on the monthly level, summed up, divided by the household's total monthly income, and multiplied by 100.

The second main outcome variable is the percentage of the household's income spent on remittances to family or friends outside the household in the last calendar month, including monetary and in-kind transfers. These are defined as the transfers to the sampled woman's friends and family plus transfers to the husband's friends and family, if the husband was the second respondent.

The wife's recent experience of physical violence perpetrated by the husband against her is the third

[26] I define assignable goods as private goods whose nature allows the researcher to deduce that they are consumed by a specific type of household member.

[27] Note that, in the survey, the question for cosmetics referred to both women and girls. This was done because it is was thought likely that cosmetics are shared between women and their under-age female household members, such that these expenditures could not be identified separately.

outcome. This is coded as an indicator variable capturing whether the woman reports that she has been slapped or beaten by the husband at least once in the last five months. This recall period was chosen to cover the time period since the start of the promotion programme. This variable was only collected from the wives, whereas all other outcome variables were also collected from the husbands.[28]

Table 3: Main outcome variables

Variable	Definition and construction
Women's assignable expenditures (% in household income)	Percent of household income spent on assignable expenditures for women, consisting of expenditures on 1. Cosmetics for women and girls in the last calendar month, 2. Clothing and footwear for women in the last three months, 3. Jewellery and accessories for women in the last three months, 4. Health and medical expenditures for women in the last three months, Values are winsorised at a fraction of 0.01 in each tail. Expenditures are harmonised on the monthly level, summed up, divided by the household's total monthly income, and multiplied by 100.
Remittances (% in household income)	Percent of household income spent on transfers to family or friends outside the household, including monetary and in-kind transfers, consisting of 1. Transfers to the sampled woman's family and friends 2. Transfers to the husband's family and friends, if the husband was the second respondent. Values are winsorised at a fraction of 0.01 in each tail, summed up, divided by the household's total monthly income, and multiplied by 100.
Violence (5 months, Dummy)	Dummy variable indicating whether woman was beaten or slapped by the other respondent at least once in the past five months.

[28]I followed the guidelines of the World Health Organization for domestic violence research, and under all circumstances prioritised the safety of respondents and the field team (World Health Organization, 2001). Enumerators were specifically trained for the administration of these questions about domestic violence. They were only administered once the enumerator had again confirmed the privacy of the interview, and the other household members were not aware that the survey with the sampled woman included questions about domestic violence. Only two questions about domestic violence were asked to ensure that the woman's survey was not substantially longer than the husband's. Respondents were offered the phone number of Bangladesh's National Helpline for Violence against Women and Children, irrespective of their responses. To increase truthful reporting and privacy, the respondents input their own responses for this section on the tablet, as suggested in Heath (2014), after the questions and response options were read out by the enumerator. This was possible since all respondents in the survey were literate and had experience using mobile devices.

(Table 3 continued.)

Decision-making (Index)	Index of woman's involvement in seven decisions in the household: 1. Decision for her to take up work outside the home for income, 2. Decision for her to accept a promotion[29], 3. Decision to purchase large household appliances, 4. Decision about large house repairs, 5. Decision to purchase clothing and jewellery for herself, 6. Decision for her to take a bus to run an errand, 7. Decision for her to visit a friend in the neighbourhood. For each decision, an ordinal variable was coded such that 4=Woman decides alone without needing permission, 3=Woman decides alone, but needs permission, 2=Woman is involved in joint decision-making, 1=Others decide, but need the woman's permission, 0=Woman is not involved. I create dummy variables for each category and decision, and then compute an index following Anderson (2008) and O'Brien (1984).[30]
Hiding money (Dummy)	Dummy variable indicating whether woman chooses private option in any choice between receiving money privately or publicly in the game.
Controlling money (Taka)	Highest amount in Taka that woman is willing to give up to have money paid to herself instead of her husband (ignoring multiple switching).

The fourth variable captures women's involvement in seven different decisions in the household. These dimensions capture decisions about the woman's daily autonomy, i.e. visiting a friend, taking a bus, and purchasing clothing and jewellery for herself. They also cover decisions which affect the entire household, i.e. regarding large household repairs and purchases of large appliances as well as decisions about the woman's career, i.e. taking up work and accepting a promotion. Respondents were asked to describe who makes these decisions in their household in an open-ended question, which was then coded by enumerators. As a follow-up question, respondents were asked whether the decision-makers required permission from any other household members. For each decision, the two questions were recoded into one ordinal variable where a higher number indicates a more extensive involvement in decision-making. These range from zero, indicating that the woman is not involved in the decision, to four, which indicates that the woman decides alone without needing permission. I then compute a summary index of all seven decisions following Anderson (2008) and O'Brien (1984), taking care not to ascribe cardinal meaning to ordinal differences as described in Footnote 30. Note, however, that

[29]For the selected or nominated women who had been offered a promotion in the wave 2 survey, these questions refer to the decision-making about the actual offer instead of the generic question.

[30] In this paper, an Anderson index is computed as follows. First, if necessary, all variables are recoded such that they indicate the same direction. Second, any ordinal and categorical variables will be recoded into several dummies. Ordinal variables will be recoded such that the lowest value is the excluded base group (e.g. for an ordinal variable with 4 categories from 0=strongly disagree to 4=strongly agree, the dummies will be defined as 0 and above (the excluded base group), 1 and above, 2 and above, 3 and above, 4 and above. All dummies except the base group are included in the index.) For categorical, non-ordinal variables, dummies for each category will be created and all dummies except one base group will be included in the index. Third, using the recoded variables, the index developed by Anderson (2008) and O'Brien (1984) is computed. The index is produced using a self-written Stata programme that follows Anderson's stepwise approach. As a last step, the programme standardises the index by the mean and standard deviation of the sample (separately for the promotion and exposure analysis samples, and within those for husbands and wives separately), to allow for easier comparison of effect sizes.

the results below are robust to different definitions, e.g. coding a binary variable indicating whether the woman is involved versus not involved in a decision, or whether she is the sole decision-maker versus not. The advantage of this ordinal approach is that it captures the entire distribution into one summary index.

The last two main outcomes are derived from two economic games that I implemented as part of the survey, which measure cooperation in the household. These were modelled after Almås et al. (2018). In the first wave of the household survey, respondents played an incentivised game that asked them to choose between receiving different amounts of money privately or publicly.[31] Hiding money is a dummy variable indicating whether the woman ever chose the private option in a series of comparisons. In the second wave, respondents played a different game, which asked them to choose between receiving an amount of money themselves or whether their husband should instead receive a different amount of money.[32] Controlling money is defined as the highest amount the woman is willing to give up to control the money herself in a series of comparisons.[33] Note that, because each game was only implemented in one period, the sample size for these outcomes for the promotion analysis is half that of the other outcomes. Since the sample for the exposure analysis was interviewed only once with the survey instrument for the first wave, the outcome Controlling money is not available for these respondents.

These outcome variables correlate with demographics broadly as expected. This is demonstrated in Table A.2 in the appendix, where I show how the outcome variables relate to a variety of individual and household characteristics for the non-exposed respondents of the exposure analysis. (See Table A.3 in the appendix for variable definitions.) The percentage of income spent on assignable goods for women are positively, though not always significantly, related to the woman's age, her education, the woman's share in income, the number of household members, her age at marriage, and whether the woman brought assets into the marriage. Being married and having a larger number of children is negatively related to women's expenditures, which is not surprising given that these variables capture competing demands on income. The correlates for the household's percentage of income spent on remittances largely look similar to those of women's expenditures, with some notable differences. A higher number of household members correlates negatively with remittances, as does an indicator for whether the woman brought assets into the household. This makes sense since remittances are likely lower in households with fewer low-income relatives living outside the household. Somewhat counterintuitively, however, the woman's income share is also negatively negatively related to the household's remittances.

The quantitatively most important correlate for domestic violence in the last 5 months is the woman's income share, with a higher income share related to less violence. In comparison, the woman's involvement in decision-making is highly positively correlated with the indicator whether she brought assets into the marriage and with the woman being married. Married women and women who brought assets into the marriage are less likely to hide from their spouses in the economic game.

[31] For example, 100 Taka sent via mobile phone credit to the woman tomorrow without the husband knowing vs. 150 Taka sent via mobile phone credit to the woman tomorrow, accompanied by a message to the husband's phone informing him how much the woman receives.

[32] E.g. 100 Taka in phone credit paid to you after the survey vs. 150 Taka in phone credit paid to your husband after the survey.

[33] During the administration of the wave 1 survey, it became clear that there was a strong norm against hiding among respondents. For example, only 12 % of women in the non-exposed group of the exposure analysis ever chose the private option. This had not been apparent during the piloting. Qualitative surveys confirmed this norm against hiding, but also revealed a much weaker norm against directing money to oneself instead of the partner. This motivated the use of a different game in wave 2.

3.5 Descriptive statistics

The women in both samples, for the promotion and the exposure analysis, on average are in their mid-twenties who live with their husbands and one child in nuclear families. Table 4 shows summary statistics for the promotion sample in Panel A, using the data from the factory baseline conducted for the promotion programme. Panel B shows descriptives for the exposure sample in Panel B. Because no baseline for the exposure analysis was conducted, Panel B is derived from the household surveys of the non-exposed comparison group. See Table A.3 in the appendix for variable definitions. Women in both samples are similar on a number of dimensions. They are on average about 26 years old and more than 80 % are married. Nearly two thirds of the women have migrated to Dhaka from their place of birth. On average, they have one child. Interestingly, more than half of respondents with children report that at least one child does not live in the household, but instead lives with relatives.[34] The women are also four to five years younger than their husbands. Despite this large age gap, they only have about a third of a year less education than the husbands. They contribute slightly below half of the total household income.[35] Interestingly, two thirds of all husbands also work in the garment industry.

The samples differ in other aspects, which indicate that the women who were nominated or selected for the promotion programme come from a higher socioeconomic background and play a larger role in their households than women in the exposure analysis sample. For example, 12 % of women in the promotion analysis sample consider themselves the household head, compared with only 1 % in the exposure analysis sample. In addition, the former group has on average about two years of education more than the latter group. The households in the promotion analysis sample also report slightly higher household income, of about 23,500 Taka (roughly 235 GBP and 280 USD at the time of the surveys), compared to about 21,000 Taka (roughly 210 GBP and 260 USD) in the exposure sample.

[34]Note that this is derived from Panel B, since I don't have comparable data from the promotion analysis factory baseline

[35]Note that we only collected individual income data for the wife and husband in the promotion analysis baseline, but total household income during the household surveys. For the promotion analysis baseline, I therefore approximate total household income as the sum of income of both husband and wife. This will only differ from the total household income variable used in Panel B if household members other than the husband and wife also contribute income.

Table 4: Summary statistics

Panel A: Promotion analysis

	N	Mean	SD	Min	Max
Age	198	25.80	3.53	19.00	38.00
Married	198	0.84	0.36	0.00	1.00
Household members	198	3.27	1.57	1.00	10.00
Household head	198	0.12	0.33	0.00	1.00
Migrant	198	0.66	0.47	0.00	1.00
Education years	198	8.33	1.76	0.00	15.00
Nr Children (if ever married)	180	1.04	0.79	0.00	5.00
Combined income of spouses (if married)	167	23630.19	6556.26	9500.00	44760.00
Education difference of spouses (if married)	167	-0.31	3.05	-10.00	9.00
Age gap of spouses (if married)	166	-4.64	2.75	-14.00	4.00
Husband works in garment industry (if married)	167	0.66	0.47	0.00	1.00
Woman's share in spouses' income (if married)	167	0.46	0.14	0.09	1.00

Panel B: Exposure analysis

	N	Mean	SD	Min	Max
Age	238	26.20	4.16	18.00	40.00
Married	238	0.89	0.31	0.00	1.00
Household members	238	3.09	1.42	2.00	8.00
Household head	238	0.01	0.11	0.00	1.00
Migrant	238	0.63	0.48	0.00	1.00
Education years	238	6.08	2.48	0.00	10.00
Nr Children (if ever married)	224	1.16	0.84	0.00	4.00
Child outside HH (if Nr Children ≥ 1)	176	0.57	0.50	0.00	1.00
Total household income	238	21166.26	7325.44	6500.00	43000.00
Education difference of spouses (if married)	208	-0.35	3.74	-10.00	9.00
Age gap of spouses (if married)	208	-5.24	3.25	-23.00	-1.00
Husband works in garment industry (if married)	208	0.66	0.47	0.00	1.00
Woman's share in household income	238	0.48	0.18	0.00	1.00

Panel A uses data from the baseline survey of the promotion programme, conducted in the factory. Because there is no baseline for the exposure analysis, Panel B uses data from the household surveys only from the non-exposed comparison group.

4 The effects of a promotion

4.1 Empirical strategy

The selection process for the promotion programme described in Section 3.2 allows me to compare outcomes of selected women to nominated runners-up who were not included in the programme because the factory did not have enough vacancies available. Recall that, after the factories provided us with a ranked list of candidates fulfilling pre-set criteria, we conducted extensive screening of all nominees and excluded those who failed literacy and numeracy tests. Out of these shortlisted operators, only those with a rank that was better than the factory-specific cutoff were selected. The nominated women who passed all diagnostic checks but ranked below the cutoff continued to work as production-line operators.

I argue that the assignment to the selected and the nominated groups is quasi-random because factories have limited ability to predict which women would be successful as a supervisor among the eligible women. The factories had historically nearly always promoted men, and therefore had large numbers of competent female line operators from which to choose new supervisors. Our extensive eligibility checks meant that only skilled and experienced line operators were considered for the programme. While it might be easy for factories to exclude operators who would *not* make a good supervisor in their factories, it is much harder to decide the ranking among a sample of women who had *all* been judged to be suitable after this extensive screening. This argument is similar in spirit to the literature on microenterprises, recruitment and venture capitalism, which shows that humans are often inept at predicting which firms, workers or investments will be successful among an already select sample (McKenzie and Sansone, 2017; Hoffman et al., 2018; Nanda, 2016).

4.1.1 Balance tests

I find strong support for quasi-random assignment when comparing the selected and the nominated group on observable characteristics.[36] The balance tests in Table 5 demonstrate that the selected and nominated groups are statistically indistinguishable on a wide range of observable characteristics. See Table A.3 in the appendix for variable definitions. Only two tests out of 34 reject equality at the 10 % significance level, using the conventional t-tests in column (5). This is fewer than one would expect to reject by chance if the null hypothesis is true. The selected women have about a year *less* experience in the garment industry, but they score about seven percentage points higher on the diagnostic literacy test than the nominated group.

To take the small sample size into account, I also show p-values using randomisation inference in column (6) and normalised differences between the means of the nominated and selected groups following Imbens (2015) in column (7). The normalised difference is a sample-size free way to investigate balance in covariates, and an absolute value smaller than 0.30 in absolute value can be considered well balanced, according to Imbens (2015).[37] These additional tests strengthen the conclusion that the nominated and

[36] Note that, for all checks in this subsection, I only include women whose data is also used in the analysis, i.e. who were interviewed in at least one household survey wave and had another adult decision-maker in the household. This is to ensure that attrition is not driving an imbalance.

[37] The normalised difference is calculated as the difference in means between the two groups, divided by the square root of the average of the sample variances of the two groups.

Table 5: Balance of nominated and selected groups

	(1) N	(2) Nominated	(3) N	(4) Selected	(5) p-value	(6) p-value (RI)	(7) Norm-diff
Panel A: Variables from baseline survey							
Age	38	26.18	160	25.71	0.45	0.47	-0.13
Married	38	0.89	160	0.83	0.34	0.40	-0.18
Household members	38	3.32	160	3.26	0.85	0.91	-0.03
Household head	38	0.05	160	0.14	0.15	0.30	0.29
Migrant	38	0.68	160	0.66	0.74	0.81	-0.06
Education years	38	8.00	160	8.41	0.19	0.16	0.22
Nr Children (if ever married)	35	1.06	145	1.03	0.88	0.88	-0.03
Combined income of spouses (if married)	34	23408.41	133	23686.89	0.83	0.80	0.04
Education difference of spouses (if married)	34	0.26	133	-0.46	0.22	0.29	-0.22
Age gap of spouses (if married)	34	-4.97	132	-4.55	0.43	0.42	0.15
Women's share in spouses' income (if married)	34	0.45	133	0.47	0.41	0.36	0.18
Decision-making (Index)	38	-0.13	160	-0.18	0.79	0.77	-0.05
Supportive family (Index)	38	0.09	160	0.06	0.89	0.90	-0.02
Experience in garment sector	38	6.97	160	5.91	0.08*	0.12	-0.30
Exposure to female SV	38	0.63	160	0.54	0.33	0.31	-0.18
Internal locus of control (Index)	38	0.20	160	0.08	0.50	0.44	-0.12
Grit (Std index)	38	-0.05	160	0.13	0.28	0.19	0.20
Self-efficacy (Index)	38	-0.06	160	0.06	0.51	0.52	0.11
Emotional competence (Index)	38	0.14	160	0.11	0.86	0.86	-0.03
Multi-factor Leadership (Index)	38	0.08	160	0.08	0.97	0.96	-0.01
Life satisfaction	38	7.55	160	7.60	0.89	0.92	0.03
Self-assessment	38	0.03	160	0.14	0.76	0.71	0.06
Ambition	38	2.16	160	2.20	0.84	0.86	0.04
Panel B: Variables from diagnostic tests before selection							
Literacy score	38	0.51	160	0.58	0.09*	0.04**	0.29
Numeracy score	38	0.45	160	0.47	0.62	0.61	0.08
Processing speed score	38	0.34	160	0.33	0.75	0.72	-0.06
Garment knowledge score	38	0.56	160	0.54	0.28	0.46	-0.21
Family support score	38	0.69	160	0.72	0.39	0.31	0.16
Interest score	38	0.72	160	0.74	0.62	0.59	0.09
Confidence score	38	0.73	160	0.73	0.96	1.00	-0.01
Panel C: Time-invariant variables from household surveys							
Muslim	38	0.97	160	0.97	0.87	1.00	-0.03
Socioeconomic background (Index)	38	-0.10	160	-0.01	0.65	0.73	0.09
Marriage duration (if married)	36	8.72	149	8.13	0.51	0.64	-0.13
Brought assets in marriage (if married)	36	0.31	149	0.34	0.73	0.68	0.06

Column (5) shows the p-value of a conventional t-test of equality of the group means in columns (2) and (4). Column (6) shows the p-value of the t-test implemented using randomisation inference, with 5000 replications of treatment assignment that maintain the share of selected women within factory strata. Column (7) shows the normalised difference of the group means in columns (2) and (4), calculated as the difference in means between the two groups divided by the square root of the average of the sample variances of the two groups, following Imbens (2015). * $p<0.1$, ** $p<0.05$, *** $p<0.01$

selected groups are very similar. I reject only one hypothesis when using the randomisation-inference p-values, and all normalised differences are weakly smaller than 0.30. Lastly, I conduct a test of joint orthogonality by investigating whether these 34 observable characteristics jointly predict selection in a linear regression.[38] The p-value of the F-test is 0.87, which shows that selection is not determined by a wide range of baseline observable characteristics (see Table A.4 in the appendix).

4.1.2 Main specification

I employ three different estimation approaches. I use OLS with control variables chosen by machine learning in my main analysis, and discuss the regression discontinuity design and the matching method I employ in Section 4.3. The following is my main specification for outcome Y of each nominee and selected operator i in factory f, while pooling both household survey waves $w = \{1, 2\}$:

$$Y_{i,f,w} = \alpha + \beta \cdot selected_{i,f} + g(\boldsymbol{X_{i,f}}) + \epsilon_{i,f,w} \tag{3}$$

selected is a dummy variable indicating whether the individual is an operator selected for the promotion programme, which is zero for the nominated group. $\boldsymbol{X}$ is a vector of potential control variables. The vector consists of all 34 variables for which I tested balance on in Table 5 and their squared terms, in addition to factory, enumerator, and month dummy variables.[39] The set of control variables to be included are selected using the Post Double Selection Least Absolute Shrinkage and Selection Operator algorithm (PDS Lasso), using the data-driven penalty loadings for clustered standard errors suggested in Belloni et al. (2013, 2014a,b, 2016). Standard errors are clustered at the individual level.

The PDS Lasso algorithm achieves a parsimonious selection of control variables in two steps. It first implements the Lasso algorithm twice to select two sets of covariates from the $\boldsymbol{X}$ vector: one set which is predictive for the outcome variable Y and one set which is predictive for *selected*, the covariate of interest. In the second step, β is estimated using an ordinary least squares regression of Y on *selected* and the union of the two sets of covariates selected in the first step. As Belloni et al. (2013) show, this method performs well across a variety of methods, enhances efficiency, and leads to valid inference even with mistakes in variable selection.

Since I am interested in intra-household decision-making, I limit the sample to those women who report having another adult decision-maker in the household. As shown in Section 3, this is the husband in the vast majority of cases. I compute sharpened q-values to correct for multiple hypotheses testing for all main outcome variables defined in Table 3 (Benjamini and Hochberg, 1995; Benjamini et al., 2006).

[38] In this paper, to include the variables only defined for married respondents, I follow two steps. First, I set these variables to zero if they are missing and, second, I include dummy variables that indicate whether the variables are missing in the regression. As Table A.4 in the appendix shows, I also do not find that covariates predict selection if I include all covariates but limit the sample to married respondents, or if I only include covariates which are defined for all respondents in the regression. The p-values for the F-statistic of the regression in these cases are 0.96 and 0.92, respectively.

[39] By including all diagnostic scores as well as factory fixed effects, I account for a potential re-ranking on these scores by the factories participating in a cross-randomised management training.

Table 6: Effects on promotions and income

	(1) Supervisor (Dummy)	(2) Personal income (Taka)	(3) Personal income (Taka)
selected	0.24***	887.93*	-270.69
	(0.06)	(509.53)	(544.67)
selected*supervisor			3183.95***
			(711.07)
N	363	363	363
Nominated mean	0.09	9883.31	9883.31
Nominated SD	0.29	3203.56	3203.56

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

4.2 Results

4.2.1 Promotions and income

The promotion programme was successful in advancing women's careers. As column (1) of Table 6 shows, the women selected for the programme were 24 percentage points more likely than the nominated group to have been officially promoted to supervisor after the conclusion of the promotion programme. They also saw an income increase of about 900 Taka, equivalent to about 9 % of the income in the comparison group, as per column (2). However, an official promotion increased the woman's personal income much more. When I interact being selected for the programme with the indicator for being promoted to supervisor in column (3), I find that the personal income of officially promoted women increased by about 32 %, compared to around 9900 Taka (99 GBP, 124 USD) in the nominated group. Recall, however, that all selected women were assigned to trial as supervisors for at least two months, and therefore had experience in a leadership position. A number of them were also still waiting to be officially promoted at the time of the household surveys. Of course, the official promotion is likely an endogenous outcome. I therefore focus on the intent-to-treat effects of being selected for the promotion programme for the main results, but in Section 4.4 unpack the results into the different potential effects identified in Section 3.1.

4.2.2 Main outcomes

Turning to my main outcomes, Table 7 shows the headline results. I find large, positive and significant effects on the share of income that households spend on expenditures on women's assignable expenditures, as well as on remittances. The households of the selected women spend 2.71 percentage points more on women's goods than the households of the nominated women, as shown in column (1). This effect is large at about 51 % of the mean in the nominated group of 5.32 %. Column (2) also shows a large effect on remittances, of 3.48 percentage points. This is 58 % of the comparison group mean. The sharpened q-values in squared brackets support that these results remain significant even after accounting for multiple hypotheses testing.

I do not see significant effects on my other main outcomes, as shown in columns (3) to (6) of Table 7. The effects on domestic violence in the past five months and the woman's decision-making power

Table 7: Effects on main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)	(6) Controlling money (Taka)
selected	2.71**	3.48**	-0.03	0.09	0.03	-5.32
	(1.07)	(1.40)	(0.04)	(0.15)	(0.08)	(17.90)
	[0.04]	[0.04]	[1.00]	[1.00]	[1.00]	[1.00]
N	363	362	363	363	181	182
Nominated mean	5.32	6.00	0.09	-0.16	0.28	146.77
Nominated SD	4.48	8.63	0.29	1.13	0.45	147.72

Standard errors clustered at the individual level in parentheses. Sharpened q-values in squared brackets. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

in the household are small and insignificant. However, their direction supports an increase in bargaining power, with a negative point estimate on domestic violence and a positive estimate on women's involvement in decision-making.[40]

Similarly, the outcomes of the economic games do not appear to be affected. Respondents in the nominated selected group are a little more likely to choose a private payout in the hiding game, as seen in column (5) of Table 7. Their willingness to pay for controlling money themselves is virtually unchanged, however, as shown in column (6). Compared to the mean and standard deviation of the nominated group, both coefficients in columns (5) and (6) are small and insignificant.

To understand these results better, I look at the components of women's assignable expenditure separately in Table 8. The effect is positive for all components of women's assignable expenditures—cosmetics, clothing and footwear, accessories and jewellery, and health expenditures. However, the effects are strongest on expenditures on women's clothing and footwear as well as accessories and jewellery.[41] It is possible that some of the effects on clothing and accessories expenditures are driven by a need for the selected women to look more professional in their new supervisor position. I argue that the size of the effects over time makes this less likely. It is arguably more likely that the women selected for the promotion programme need to invest in a stock of new clothing when starting their new position, which would suggest a larger effect early on. In Table A.5 in the appendix, I look into the effects on women's assignable expenditures for the two waves of the household surveys separately.[42] Even though I lose power because of the smaller sample size when splitting the survey waves, the coefficient in the second wave (3.22 percentage points) is more than a third larger than the effect in the first wave (1.99 percentage points). This shows that the households of the selected women continued to spend a large income share on women's assignable expenditures even eight months after the beginning of the promotion programme, which is less likely to be driven by job requirements.

The increase in remittances is also unlikely to be driven by job requirements. Somewhat surprisingly, I find that the effect on total remittances appears to mainly be driven by transfers to the husband's

[40]Note that I also do not find an impact on whether the women has ever experienced domestic violence, with a point estimate of 0.03 from a mean in the nominated group of 0.30. The mean of 0.30 is in line with the incidence of domestic violence that Heath (2014) finds in Bangladesh. She reports that 37.4 % of married women who work for pay have ever been beaten by their husbands.

[41]When comparing the distributions of these outcomes in the selected and nominated group in Figures A.4 and A.5 in the appendix, I find that the overall effect is concentrated in the upper half of the distribution, though the effect on clothing expenditures is clearly affecting the entire distribution.

[42]I only include respondents interviewed in both waves to ensure that sample composition does not drive potential differences in effect size across waves.

Table 8: Effects on the components of women's assignable goods expenditures

	(1) Cosmetics (% in HH income)	(2) Clothing (% in HH income)	(3) Jewellery (% in HH income)	(4) Health (% in HH income)
selected	0.02	1.06***	0.54**	1.10
	(0.18)	(0.23)	(0.22)	(0.96)
N	363	363	363	363
Nominated mean	0.96	1.60	0.11	2.65
Nominated SD	1.24	1.37	0.67	4.06

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table 9: Effects on remittances (% in HH income)

	All	Married couples with children					
	(1) Total	(2) Total	(3) Wife's family	(4) Husband's family	(5) Total	(6) Wife's family	(7) Husband's family
selected	3.48**	3.53**	0.87	3.03**	-0.61	-0.70	0.64
	(1.40)	(1.69)	(0.96)	(1.46)	(1.67)	(0.94)	(1.51)
selected*Child outside HH					8.18***	3.08***	4.60***
					(1.69)	(1.00)	(1.25)
N	362	237	238	237	237	238	237
Nominated mean	6.00	6.46	2.73	3.73	6.46	2.73	3.73
Nominated SD	8.63	8.31	4.52	6.87	8.31	4.52	6.87

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

family, if I restrict the sample to married couples who have children. Columns (3) and (4) in Table 9 show that the effect on remittances to the husband's family is more than three times as large as the effect on remittances to the wife's family. However, it appears that these remittances are mostly benefitting children who live outside of the household. As mentioned in Section 3.5, in this context it is common for children to not live with their parents and to be cared for by other family members. Of course, the decision whether children live with their parents or not is endogenous, and the sample size becomes challenging when investigating interaction terms. As indicative evidence, however, columns (5) to (7) of Table 9 show that households with selected women report higher remittances if at least one child lives outside the household. This suggest that the higher remittances to the family are masking higher expenditures on children. However, I do not find that the promotion programme led the selected group to send their children to live with relatives, when I investigate the dates that children started living outside the household collected during the surveys (Results not shown for brevity).[43]

[43]I do not find an effect on the way women and husbands spend their time. Across the two household survey waves, nearly all women and husbands report working, 92 % and 94 %, respectively. This means that, at the point of the surveys, some women have left their work in the garment factories since the start of the promotion programme. However, I do not find an impact of being selected for the programme on the extensive margin of working for either women or husbands (Results not shown for brevity). Table A.6 in the appendix shows the effects on women's time use in hours in Panel A and, using the husband's survey data, on his time use in Panel B. The coefficients are small, for both the last work day in columns (1) to (4) and the last non-work day in columns (5) to (7). There is some indication that the selected women work around a quarter of an hour longer and sleep shorter by the same amount on the last work day, but these effects are small in economic terms.

4.3 Robustness checks

In this section, I show that the large positive effects on expenditures on women's assignable goods and remittances are robust to different estimation approaches, different variable definitions, different specifications, different data sources, placebo tests and checks for attrition, though I lose significance in a few instances. I also demonstrate that the effects are not mainly driven by the training that was delivered as part of the promotion programme.

4.3.1 Regression discontinuity design

The positive and significant impacts on women's assignable goods expenditures and remittancesare are robust to implementing a regression discontinuity design (RDD), though I lose statistical power as expected.[44] For the analysis, I use the ranked shortlist of operators provided by each factory after removal of any ineligible candidates, as explained in Section 3.2. Recall that only those operators with a rank better than the factory-specific cutoff were assigned to the programme. Since factories nominated different numbers of operators, I scale the rank of operators by the total number of operators nominated in each factory to make the ranks comparable across factories. I use the distance of this scaled rank variable from the factory-specific cutoff as the running variable. I determine the optimal bandwidth following Imbens and Kalyanaraman (2012)[45] for each outcome variable. Using the observations within this bandwidth, I then allow for a linear function of the running variable separately on either side of the cutoff when estimating the effect of being selected for the promotion programme. Standard errors are clustered at the individual level.

Figure 1 compares the coefficients from the regression discontinuity approach with the estimates from PDS Lasso and the matching approach discussed below. Table A.7 in the appendix gives results for all main outcome variables. As the figure shows, the RDD point estimates for expenditures on women's assignable goods and remittances remittances are larger than those from the PDS Lasso estimation, at 4.94 percentage points for women's expenditures and 6.82 percentage points for remittances. Though they are less precisely estimated, I find significant effects on both these outcomes.[46]

4.3.2 Matching

Estimating the results with a matching method qualitatively also confirms the previous results. I implement the nearest-neighbour matching estimator suggested by Abadie et al. (2004), with one-to-one matching with replacement and using the matrix of the inverse sample standard errors of the matching variables as weights in the matching. I use all balance variables in Table 5, as well as all factory, enumerator and month dummies as matching variables.[47] As Figure 1 and Table A.8 in the appendix show, the matching estimate for the effect on women's expenditures (at 3.98 percentage points) is larger than the estimates obtained using PDS Lasso and significant. The coefficient for

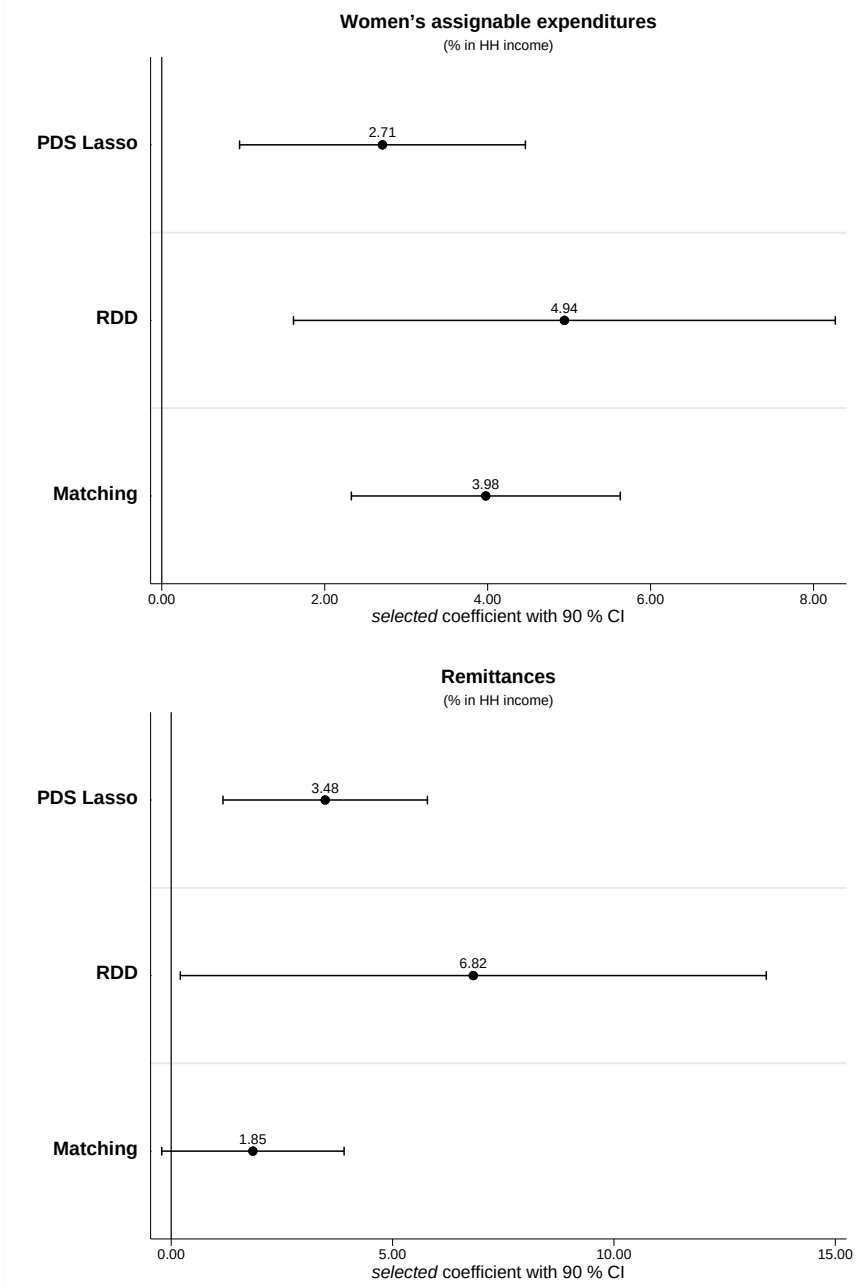
[44] See, for example, Deke and Dragoset (2012) and Schochet (2008) for a discussion of the lower power of regression discontinuity designs.

[45] I use the Stata command rdbwselect to do so.

[46] If I allow for a quadratic function of the running variable on both sides of the cutoff separately, I find coefficients of 9.74 percentage points for women's expenditures (p-value of 0.02), and 3.13 percentage points for remittances (p-value of 0.67). Full results available on request.

[47] To include balance variables which are missing for some observations, I set these to zero if they are missing and also include indicators if they are missing in the matching.

Figure 1: Comparison of estimates using three different estimation approaches



remittances (at 1.85 percentage points), however, is smaller and loses significance with a p-value of 0.14.

4.3.3 Different variable definitions

I next check robustness to different variable definitions for women's assignable expenditures and remittances in Table A.9. I again estimating Equation 3 with PDS Lasso. I repeat the main results from Section 4.2 as a comparison in columns (1) and (4), and vary the definition, first, to take the inverse hyperbolic sine transformation of the percent of total household income and, second, to the monetary value in units of Taka (winsorised, harmonised on the monthly level and then summed up). For both outcomes, the checks confirm large positive effects, though these are not always significant.

The inverse hyperbolic sine transformation for the income share of women's expenditures suggests an effect of about 20 % and of about 30 % for remittances in columns (2) and (5), respectively. However, neither coefficient is significant, with a p-value of 0.15 for women's expenditures and 0.22 for remittances.[48] Since the inverse hyperbolic sine is a transformation which compresses the distribution similar to a natural logarithm, this again suggests that the effect is concentrated in the upper half of the distribution. Note, however, that the impact on the inverse hyperbolic sine of the income share of women's clothing expenditures remains highly positive and significant, with an effect of about 41 % and a p-value of 0.00 (Full results available upon request).

When I vary the definition to look at the sum of women's expenditures and remittances in monetary terms in columns (3) and (6), the coefficients remain highly positive and significant. The point estimate suggests that households of selected women spend about 396 Taka (about 5 USD or 4 GBP) more per month on women's assignable goods, about a third of the comparison mean. Similarly, the estimate for spending on remittances in monetary terms suggests higher expenditures of about 954 Taka (about 12 USD and 9.5 GBP), about 73 % of the comparison mean.

4.3.4 Different specifications

I now return to the preferred definitions of my outcome variables—the percent of household income spent on women's assignable goods or remittances. I investigate robustness of my main results to different OLS specifications and a larger set of potential controls for the PDS Lasso algorithm. This is shown in Table A.10 in the appendix, where the first column repeats the results from my preferred specification in Section 4.2. I then sequentially introduce different sets of control variables, starting from OLS with no controls in column (2). Standard errors are always clustered at the individual level. Column (3) adds factory, enumerator and month fixed effects, and column (4) instead adds linear and squared controls for the imbalanced baseline control variables of experience in the garment industry and the literacy score. Column (5) instead adds linear controls for all baseline balance variables from Table 5, and column (6) adds all changes in columns (2) to (5) together. Column (7) implements OLS with controls chosen based on economic theory, where I include Married, Total household income, Total household income squared, Number of girls, boys, women and men in the household, Age of the respondent, as well as factory, enumerator and month fixed effects. Finally, column (8) implements PDS Lasso but allows for a larger set of potential control variables, including all balance variables from Table 5, factory, enumerator and month dummy variables, all possible interaction terms between these variables, in addition to all squared balance variables.

As seen in Panel A of Table A.10, the point estimates for women's assignable goods expenditures fluctuate around the PDS Lasso estimate in column (1). They remain significant for all specifications, with only one exception. When I include all linear balance variables and all factory, enumerator and month dummy variables as controls in column (6), the coefficient reduces to 1.58 percentage points, compared to 2.71 in column (1). This could possibly due to overcontrolling. When I allow for the larger set of potential controls in PDS Lasso in column (8), with a total of 4,486 potential controls, the coefficient decreases slightly to 2.04 percentage points compared to column (1), but it remains significant at the 5 % level.

[48] See Bellemare and Wichman (2019) for a discussion on the interpretation of regression coefficients with inverse hyperbolic sine transformations.

The effect size for remittances also remains relatively stable across the specifications, and the estimates are always significant. The effect size with all controls in column (6) of Panel B in Table A.10 is about 19 % larger than the PDS result in column (1). Once I allow for the larger set of potential control variables in column (8), the effect size decreases by only around 5 % compared to column (1), to 3.32 percentage points, and it remains significant.

4.3.5 Husband's survey

The effects on women's assignable expenditures and remittances are confirmed when using the data collected during the surveys with the husbands. Table A.11 in the appendix shows the results for the main outcomes, estimating Wquation 3 with the husband's data.[49] The point estimate of the effect on women's expenditures is 2.21 percentage points and significant using the husband's survey, compared to a slightly larger 2.71 percentage points from the wives' data in Table 7. For the budget share of remittances, the estimate from the husband's data is 5.13 percentage points and significant, compared to 3.48 percentage points from the wives' data.

Note that husbands and wives also report similar *levels* of budget shares. The wives of the comparison group of non-selected nominees report that the household spends 5.32 % of household income on women's assignable expenditures and 6 % on remittances on average. This compares to a mean of 5.71 % for women's expenditures and 6.31 % for remittances from the husbands' data. It is a common pattern in this data that both respondents in the household report similar expenditures and income. For example, for total household income before winsorisation, I find a correlation coefficient of 0.85 between the reports of both respondents across both waves. This suggests that information about income and expenditures is widely shared within the household, possibly because the regular income streams in the garment industry are both difficult to hide and make budgeting easier.[50]

4.3.6 High-frequency phone surveys

My main results are confirmed with the data from the wife's high-frequency surveys. Table A.12 in the appendix shows the results. I estimate Equation 3 with the same set of potential control variables, but use survey round fixed effects instead of month fixed effects. I also keep the same estimation sample as in Section 4.2 for comparison. For time reasons, the high-frequency surveys asked about assignable goods expenditures for women and girls combined, and only included questions about remittances to the wife's family (not the husband's). It also did not include questions about income. My outcome measures are therefore, first, the sum of household expenditures on women's and girls' assignable goods in Taka per month (again consisting of cosmetics, clothing, accessories and health), and remittances to the wife's family in Taka per month.[51] I find large and significant effects on women's and girls' assignable expenditures, with a point estimate of 658 Taka (about 8 USD and 6.60 GBP), about 37 % of the mean in the comparison group. My results also show significant and large increases of

[49] Note that I use the same set of potential control variables for the balance variables of the PDS Lasso algorithm as in Section 4.2, i.e. the variables from the wife's data. Also note that some husbands were unexpectedly unavailable at the time of the survey or refused to be surveyed, such that the number of observations is slightly smaller for husbands than for the wives.

[50] This could potentially also explain why I see little effects on the outcomes from the economic games I implemented, which measure respondents willingness to pay for hiding or controlling money.

[51] The monetary values are again winsorised, harmonised on the monthly level and, if applicable, summed up.

remittances to the wife's family, with a coefficient of 1009 Taka (about 13 USD and 10 GBP) that represents about 43 % of the comparison group mean.

4.3.7 Placebo test: Comparing top and bottom half of selected group

I also implement a placebo test by comparing outcomes of the women in the top half of the selected group to outcomes of women in the bottom half of the selected group. This means that I am now only comparing women who were participants of the promotion programme. Finding no effects for this comparison would support the claims that (1) the ranking of eligible women in the shortlist is orthogonal to my outcomes of interest, and that (2) the cutoff which separates the selected from the non-selected is the relevant margin of interest. I create the placebo treatment as follows. I only keep the women who were selected for the promotion programme. For each factory ranking, I then assign the selected women who are in the top half of the ranking to the placebo treatment, and the bottom half of the selected women to the placebo control. For example, imagine a factory had 15 women in the shortlist after excluding ineligible women, and ten of these women were selected for the promotion training, while the other five are the non-selected nominees. I drop the non-selected nominees for this placebo test and assign the five selected women with the top rank in the shortlist to the placebo treatment and the remaining five selected women in the shortlist to the placebo control.

I estimate Equation 3 as described in Section 4.1, but now the *placebo* treatment as created above is my covariate of interest. Table A.13 shows the estimates for the placebo treatment effect for all my main outcomes. Importantly, I do not find effects of the placebo treatment on women's expenditures and remittances. The coefficients for both women's expenditures and remittances are small and insignificant, at 0.31 and 0.60 of a percentage point, respectively. These compare to estimates of 2.71 and 3.48 percentage points for my actual treatment in Section 4.2. The effects of the placebo treatment on the other main outcomes are also small and insignificant, with the exception of the willingness to pay for controlling money in the economic game, for which I find a negative and significant effect.

4.3.8 Attrition

I now show that survey attrition is not driving the results for women's expenditures and remittances, by implementing two checks. First, I restrict the estimation of Equation 3 to a balanced panel in columns (1) and (2) of Table A.14 in the appendix, which means that I only keep respondents who are part of the sample in both waves. Both point estimates, for women's expenditures (at 2.49 percentage points) and remittances (at 2.93 percentage points) are similar to my main results in Section 4.2 and significant despite the smaller sample size.

As a second check, I only include the data from Wave 1 of the household survey when estimating Equation 3, for which the response rates for the selected and the non-selected nominated group were very high and very similar (See Table 2). I show the results in columns (3) and (4) of Table A.14 in the appendix. The effect on women's expenditures in Wave 1 remains significant at 1.92 percentage points, though it is about 30 % smaller than in Table 7. The effect on remittances in Wave 1, at 3.08 percentage points, is only about 12 % smaller than in Table 7, and it remains significant. These two checks suggest that a change in sample composition due to attrition is not explaining the results.

Table 10: Effects on other assignable goods expenditures (% in HH income)

	(1) Men (% of HH income)	(2) Girls (% of HH income)	(3) Boys (% of HH income)
selected	0.32	1.22**	-0.37
	(0.57)	(0.62)	(0.72)
N	363	146	161
Nominated mean	3.92	1.65	2.69
Nominated SD	3.99	2.41	3.65

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies. Columns (2) only includes observations where the respondent reported having at least a daughter, or a girl living in the household. Column (3) only includes observations where the respondent reported having at least a son, or a boy living in the household.

4.3.9 Training effects

I also provide evidence that the results for women's expenditures and remittances shown in Section 4.2 are not primarily driven by the effects of the training provided as part of the promotion programme. To check for a potential effect of training, I investigate the effects for the three different groups discussed in Section 3.2.[52] Remember that all selected women were assigned to work as assistant supervisor for at least two months. In Table A.15, I interact the *selected* variable with variables indicating these training groups. As the first row of the table shows, the effects appear to be primarily driven by being selected for the promotion programme, not the training. The effects remain significant for the group that didn't immediately receive training, at 3.32 percentage points for women's expenditures and 3.29 percentage points for remittances. None of the interaction terms are individually significant. The interaction term which is largest in magnitude is actually negative for the soft only training group, at -2.60 percentage points. Nevertheless, I cannot reject that the interaction terms are jointly equal to zero for both women's expenditures (p-value of 0.13) and remittances (p-value of 0.87). Overall, these results suggest that training is not a large driver behind the observed results.

4.4 Unpacking the results

I now return to the simple collective household framework described in Section 3.1, and evaluate which, if any, of the three potential effects could be driving the large and positive increases in household budget shares spent on women's assignable goods and remittances. The small sample size makes it somewhat difficult to exactly pin down mechanisms, but the different pieces of suggestive evidence point into the same direction.

4.4.1 Marshallian income effect

A Marshallian income effect resulting from the woman's wage increase would ease the household's budget constraint, and increase the set of feasible consumption allocations. This could affect the demand for all goods consumed in the household, though the income elasticity could of course differ

[52]Recall from Section 3.2 that those operators selected for the promotion programme were randomly assigned to one of three groups. One group received training in soft and technical skills immediately, one group only received soft skills training immediately and one group did not receive any training until later. After about six months, the soft skills only group and the group that had initially not been trained also received the missing training.

Table 11: Effects on women in one-person households

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)
selected	0.35	-3.74
	(2.09)	(6.86)
N	49	49
Nominated mean	11.58	19.32
Nominated SD	14.70	20.45

Table only includes unmarried respondents living in one-person households. Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

between goods.[53] However, one would by definition always expect a positive income elasticity for normal goods.

I present two pieces of evidence which suggest that the observed effects are not primarily due to Marshallian income effects. First, I investigate the effects on the percent of household income spent on assignable goods for men, girls and boys in Table 10.[54] Note that I only include the subsample of respondents with a daughter, or a female child in the household when estimating the effects on girls' expenditures (and equivalently for boys' expenditures), which reduces the sample size. As can be seen in column (2), I find a significant positive effect on girls' expenditures of 1.22 percentage points, an increase of 74 % of the comparison mean.[55] This compares to a negative point estimate on boys' expenditures, which is insignificant at -0.37 percentage points. The effect on men's expenditures is positive and insignificant at only 0.32 percentage point, which is much smaller than the effect of 2.71 percentage points for women's expenditures in Table 7. These much larger effects on expenditures on female assignable goods, for both women and girls, compared to male assignable goods, for men and boys, suggest that a re-allocation of the household's budget towards female expenditures is taking place, rather than a Marshallian income effect.

As a second piece of evidence, I investigate the effects on unmarried women who live in one-person households, for example in factory dormitories. For this subsample, intra-household bargaining is excluded by definition, but I should be able to observe possible Marshallian income effects. Since the number of observations is small at only 49, I am mainly interested in the direction and size of effects. Table 11 shows a very small positive effect on women's expenditures of 0.35 percentage points (3 % of the comparison mean), and a negative and effect on remittances of -3.74 percentage points (about 19 % of the comparison mean).[56] Taken together with the evidence from Table 10, this strongly suggests that the large effects on women's assignable goods expenditures and remittances are unlikely to be driven primarily by a Marshallian income increase.

[53]The functional form of the Engel curves will depend on the underlying demand system. See, for example, Banks et al. (1997)'s estimation of quadratic Engel curves based on a Quadratic Almost Ideal Demand System.

[54]Men's assignable goods expenditures consist of tobacco and alcohol, cosmetics, clothing and footwear, jewellery and accessories, and health expenditures. Boys' and girls' assignable expenditures consist of clothing and footwear and medical expenditures each. See Table A.3 for definitions.

[55]Additional analysis shows that the effect is largest on the budget share of education expenditures for female household members (Results available on request).

[56]Note that 47 % of the selected women living in one-person households have been officially promoted with an income increase at the time of the household surveys, so that this lack of effect can not be attributed to a lack of income increase.

Table 12: Relative wage effect

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)
selected	3.48**	2.76*
	(1.41)	(1.52)
selected* Starts earning more	-3.02**	0.97
	(1.51)	(2.25)
N	301	301
Nominated mean	4.99	6.21
Nominated SD	4.36	8.21

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

4.4.2 Relative wage effect

I next investigate whether a relative wage effect could explain the results, that is, whether the increase in the woman's wage rate compared to the husband's affects the bargaining weight as a distribution factor. The evidence suggests that this is unlikely. I look into this by checking whether the effects differ for women who started earning more than their husbands between the factory baseline and the household survey.

We know from Table 5 that the women earn about 46 % of the joint spousal income at factory baseline. In addition, about a third already earn more than their husbands at this point. I then compare this baseline information with the information about the woman's income share from the household surveys, and define a dummy variable *Starts earning more* as equal to one if the women did not earn more than her husband at baseline, but did earn more than the husband at the point of the respective household survey.[57] Across both waves, about 14 % of the women started earning more than their husbands between baseline and household survey.

I interact the new variable, *Starts earning more*, with being selected for the promotion programme in Table 12. The results do not suggest that a relative wage increase in the women's favour is driving the results. In fact, the interaction term is negative and significant for women's expenditures, but positive and insignificant for remittances.[58] Further investigation reveals that the negative interaction term for the budget share of women's expenditures is mainly driven by a decrease in health expenditures, whereas the magnitudes of the interaction terms for the other components of women's expenditures are small.

This evidence suggests that the relative wage increase in the women's favour is not driving the large and positive impacts on women's expenditures and remittances I found before, though the differing results in Table 12 make this conclusion tentative. This is in line with the discussion in Bertrand et al. (2015), which found in the United States that a women who earns more than her husband does not appear to have more favourable intra-household outcomes. Instead, the authors find that these couples

[57] Note that the income information at baseline was collected from the wife. To compute the income ratio at the point of the household survey, I use the reports from the wife about her own income and from the husband about his own income. Since some husbands refused to participate or were unavailable, this reduces the sample size.

[58] Further results show that the interaction term is positive and insignificant for remittances to both the wife's and the husband's family.

Table 13: Effects on women not yet officially promoted

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)
selected	3.59**	3.12*
	(1.42)	(1.69)
N	256	255
Nominated mean	5.49	6.29
Nominated SD	4.65	8.90

Table only includes subsample of women who were not yet officially promoted at the time of the household surveys. Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table 14: Effects on attitudes and beliefs

	(1) Overall self-beliefs (Index)	(2) Confidence (Index)	(3) Negotiating (Index)	(4) Gender attitudes (Index)	(5) Aspirations (Dummy)
selected	0.22*	0.15	0.13	0.07	0.07
	(0.13)	(0.13)	(0.11)	(0.09)	(0.07)
N	363	363	363	363	363
Nominated mean	-0.17	-0.12	-0.10	0.04	0.49
Nominated SD	1.14	1.11	0.95	0.74	0.50

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

are less satisfied with their marriage, more likely to divorce and more likely to have a higher gender gap in non-market work.

4.4.3 Position effect

The previous two subsections suggest that the large effects on women's expenditures and remittances are not driven by a Marshallian income effect or a relative wage effect. I therefore investigate whether the leadership position could affect the woman's bargaining weight irrespective of income changes. To do so, I focus on the subsample of women who had not yet been officially promoted at the time of the household surveys. This means that this subsample has not experienced any wage increase. However, remember that *all* selected women were assigned to trial as supervisor for two months. This subsample therefore also has been selected for a leadership position and has experience in a leadership position. While an official promotion is an endogenous outcome, it is nevertheless instructive to investigate whether the effects observed in Section 4.2 hold for this subgroup.

Table 13 presents the results. The large, positive and significant effects on women's expenditures and remittances are also observed for this subsample of women who were not officially promoted. The point estimates, at 3.59 percentage points for women's expenditures and 3.12 for remittances, remain significant and are close to the coefficients found for the whole sample in Section 4.2. This evidence suggests that even women who participated in the promotion programme but were not officially promoted saw their bargaining weight increase as a result of (at least temporarily) attaining a leadership position in the factory.

I find some evidence that the leadership effect operates by improving the women's attitudes and beliefs

about themselves. Table 14 shows the results for several different measures of attitudes and beliefs (all defined in Table A.3), as well as an overall index in column (1). I find positive point estimates on the women's confidence, her involvement in negotiation, gender attitudes and her aspirations, though none of these are individually significant. If I include all outcomes in columns (2) to (5) into one summary index, the effect on this index is positive and significant at the 10 % level. While this is not conclusive evidence, these results are consistent with the leadership position improving the selected women's self-beliefs and attitudes.

4.5 Summary

The results in this section have shown that quasi-random participation in the promotion programme leads to a higher share of household income spent on female assignable goods (both for women and girls) and on remittances to the family. The latter result appears to mask expenditures on children, since transfers are highest if children live outside the household.

Thinking within a collective household framework, I provided suggestive evidence that the results are mainly driven by the leadership position improving the women's beliefs and attitudes about themselves, rather than Marshallian income effects or an effect working through a change in the relative wage within the household.

Overall, this indicates that the promotion programme increased the women's bargaining power in the household through the leadership position, if one is willing to assume that women have a higher preference than their partners for female assignable goods expenditures and for supporting family outside the household.

5 The effects of exposure to a female manager

In the previous section, I provided evidence that the promotion programme increased bargaining power in the household for the participating women. This is encouraging evidence for the many policy approaches that aim to increase women's participation in leadership positions. However, since a large share of the workforce is made up of non-managerial positions (in the garment industry and otherwise), promotions to a management position will usually only affect a small share of the female population. In this section, I therefore analyse whether there are amplifying effects of female leadership on the non-managerial female workforce that is working under a female manager.

5.1 Empirical strategy

I analyse the potential effects of exposure to the new female supervisors discussed in the previous section on their female subordinates. I compare outcomes of production line operators who have worked with the new female supervisors from the promotion programme to outcomes of production line operators on other, male-supervised lines. I argue that the assignment of workers to production lines is largely determined by production requirements and can be considered quasi-random. As discussed in Section 2, production lines typically consist of 20-80 operators, depending on the type of garment produced. Each operator is responsible for one step in the production process at a work station (e.g.

Table 15: Exposure analysis: Balance of non-exposed and exposed groups

	(1) N	(2) Non-exposed	(3) N	(4) Exposed	(5) p-value	(6) p-value (RI)	(7) Norm-diff
Age	238	26.20	250	26.91	0.10	0.08*	0.15
Married	238	0.89	250	0.92	0.21	0.19	0.11
Household members	238	3.09	250	3.21	0.39	0.38	0.08
Migrant	238	0.63	250	0.58	0.22	0.31	-0.11
Education years	238	6.08	250	5.94	0.56	0.55	-0.05
Nr Children (if ever married)	224	1.16	239	1.24	0.25	0.22	0.11
Total household income	238	21166.26	250	20964.74	0.76	0.74	-0.03
Age gap of spouses	208	-5.24	219	-5.22	0.96	0.96	0.00
Muslim	238	1.00	250	0.98	0.20	0.35	-0.12
Socioeconomic background (Index)	238	0.08	250	-0.06	0.13	0.13	-0.14
Marriage duration (if married)	224	9.50	239	10.02	0.29	0.25	0.10
Brought assets in marriage (if married)	224	0.23	239	0.17	0.08*	0.08*	-0.16
Household head	238	0.01	250	0.03	0.23	0.32	0.11
Education difference (if married)	208	-0.35	219	-0.29	0.86	0.85	0.02
Women's share in household income	238	0.48	250	0.48	0.94	0.94	0.01

Column (5) shows the p-value of a conventional t-test of equality of the group means in columns (2) and (4). Column (6) shows the p-value of the t-test implemented using randomisation inference, with 5000 replications of treatment assignment that maintain the share of exposed women within factory strata. Column (7) shows the normalised difference of the group means in columns (2) and (4), calculated as the difference in means between the two groups divided by the square root of the average of the sample variances of the two groups, following Imbens (2015). * p<0.1, ** p<0.05, *** p<0.01

attaching the sleeve to a T-shirt), which differ in the skills required. According to discussions with an industrial engineer with substantial experience in several garment factories, the assignment of line operators to sewing lines is done by the management of the sewing department, typically line chiefs or production managers. In this process, the most important consideration is to match operators to the work stations that they are skilled at (e.g. overlock machines are only operated by workers skilled in using this machine).

The management of the production floor typically prefers to keep the composition of well-functioning lines unchanged. However, there are some situations when it can be necessary to move operators for production reasons, for example to temporarily cover absences of operators on other lines, or when the style produced changes and with it the production requirements. Operators might also be moved to lines with a lower target if they struggle to meet targets or increase alteration rates. Line operators and supervisors are sometimes able to express their preferences for the assignment of operators to lines, but — barring any serious allegations of misconduct — the production requirements always take precedence in dealing with any potential requests. According to an industry expert, it would be especially unlikely to observe sorting of line operators towards lines of new supervisors in their trial period. During this trial period, supervisors are expected to demonstrate good performance with the team they have been given, and any requests by themselves or operators to move to their lines would be unlikely to be granted.

5.1.1 Balance tests

I provide three pieces of evidence that support this argument.[59] First, I show that the exposed and non-exposed operators are balanced on a number of observable characteristics in Table 15. Since there is no baseline data for the random operators, these characteristics are from the household surveys. Out of the 15 mean-equality tests in Table 15, one test — for assets brought into the marriage — rejects at the 10 % significance level using the conventional p-values in column (5), and one additional test — for age — rejects when I use the randomisation inference p-values in column (6). This is in line with what one would expect to happen by chance. However, the normalised differences in column (7) are small to moderate. None of them are greater than 0.16 in absolute value, much smaller than the cutoff used by Imbens (2015).

Second, I test whether all 15 variables jointly predict exposure to a female supervisor by estimating a linear regression of *exposed* on all 15 variables.[60] The p-value of the overall F test of the regression is 0.41, as seen in Table A.16 in the appendix. I hence conclude that the 15 variables are jointly not predictive for exposure. Third, I test whether the lines of the operators who were exposed to the female supervisors are different from the non-exposed lines on observable characteristics measured before the start of the promotion programme.[61] I use daily line-level production data we collected and harmonised from the factories for four important production outcomes: efficiency, alteration rates, absenteeism and product complexity.[62] As Table 16 shows, before the start of the promotion programme, the lines exposed to the new female supervisors had very similar production statistics as the non-exposed lines. These groups did not differ in their efficiency, their complexity of garments produced and their alteration rates. The exposed lines have only slightly lower absenteeism rates, about 0.34 percentage points lower than the non-exposed mean of 4.23 %.[63]

[59] As in Section 4.1, to take attrition into account, I only include women in these checks whose data is also used in the analysis, i.e. who were interviewed in the household survey and had another adult decision-maker in the household. In the ideal scenario, I would be able to use data of line operator movements to exclude sorting towards lines exposed to new female supervisors based on characteristics that determine the operators' household outcomes of interest. Unfortunately, the factories in this sample do not keep a reliable account of operator movements.

[60] As in Section 4.1, to include the variables only defined for married women, I follow two steps. First, I set these variables to zero if they are missing and, second, I include dummy variables that indicate whether the variables are missing in the regression. As Table A.16 in the appendix shows, I also do not find that covariates predict selection if I include all covariates but limit the sample to married respondents, or if I only include covariates which are defined for all respondents in the regression. The p-values for the F-statistic of the regression in these cases are 0.29 and 0.32, respectively.

[61] Again, I only use lines of operators who are actually included in the analysis, to account for attrition.

[62] Product complexity is measured by the standard minute value (SMV), and is defined as the time that the industrial engineers of each factory estimate a production line with qualified operators working at a standard pace would require to produce one piece of the garment. The efficiency of the lines measures how the daily output of each line in garment units, adjusted for the standard minute value of the garment, compares to the total output possible that day with the workers present on the line and the hours worked. As is standard in the industry, it is calculated for each line and day as

$$efficiency = \frac{(Pieces\,produced \cdot SMV)}{Workers\,on\,the\,line \cdot Hours\,operated \cdot 60}. \tag{4}$$

The alteration rate is the percentage of all garments that needs to be altered out of all produced units, and the absenteeism rate is the percentage of workers absent amongst all workers (present and absent) on the line.

[63] Note that these comparisons are within-day-within-factory comparisons. I include day and factory indicators in the regression to account for seasonality of production and different ways of measurement across factories, respectively. Standard errors are clustered at the line level because of high auto-correlation.

Table 16: Exposure analysis: Balance of exposed and non-exposed line characteristics

	(1) Efficiency	(2) Alteration rate, %	(3) Absenteeism, %	(4) Product complexity
exposed	-1.02	0.02	-0.34**	-0.14
	(0.87)	(0.51)	(0.14)	(0.72)
N	18025	24960	21247	22148
Not exposed mean	48.16	8.52	4.23	17.28

All regressions include factory and day fixed effects, and are limited to the time before project start in each factory. Standard errors are clustered at the line level.

5.1.2 Main specification

My main specification for the exposure analysis is similar to Equation 3 in Section 4.1. For outcome Y of each exposed and non-exposed operator i in factory f,

$$Y_{i,f} = \alpha + \beta \cdot exposed_{i,f} + g(\boldsymbol{X_{i,f}}) + \epsilon_{i,f}. \tag{5}$$

exposed is a dummy variable indicating whether the individual was working on a line to which a selected participant of the promotion programme was assigned, where the individual's line code was collected at the survey follow-up for the promotion programme. *exposed* is equal to zero if the individual was working on a line unrelated to the promotion programme.[64] Note that this comparison is likely to be a conservative estimate, since the female supervisor are likely to have been visible to operators working on non-exposed lines, too. $\boldsymbol{X}$ is a vector of potential control variables, which consists of all 15 variables for which I tested balance on in Table 5 and their squared terms, in addition to factory, enumerator and month dummy variables.[65] The set of control variables to be included are selected using the PDS Lasso algorithm as explained in Section 4.1. Standard errors will be clustered at the production-line level, following Abadie et al. (2017). I again limit the sample to those women who report having another adult decision-maker in the household.

5.2 Results

Table 17 presents the results for the main outcomes when estimating Equation 5. I find that the women working with the new female supervisors report having more say in decision-making processes in the household, especially about their autonomy. Column (4) in Table 17 shows that the effect on the decision-making index is large at 0.36 of the overall standard deviation, and highly significant. It also is robust to multiple hypotheses testing, with a sharpened q-value of 0.00.

I do not find significant effects on the other main outcomes, though there are some suggestions of more conflict in the household. The point estimates for the budget shares spent on women's assignable expenditures in column (1) and remittances in column (2) are negative, but not significant. Their magnitudes in comparison with the non-exposed mean are also smaller than the positive effects I

[64] This means that no selected participant for the promotion programme was assigned to that line and no selected participant was found to be working on that line at the survey follow-up for the promotion programme.

[65] Again, to include variables which are only defined for married respondents, I set these to zero if they are missing and also include variables indicating whether these variables are missing in the vector of potential control variables.

Table 17: Exposure analysis: Main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)
exposed	-0.89	-1.39	0.02	0.36***	0.07
	(0.74)	(1.10)	(0.02)	(0.09)	(0.04)
	[0.22]	[0.22]	[0.22]	[0.00]	[0.22]
N	488	483	482	488	488
Non-exposed mean	6.48	8.49	0.02	-0.26	0.12
Non-exposed SD	6.63	11.81	0.13	1.08	0.33

Standard errors clustered at the production line level in parentheses. Sharpened q-values in squared brackets. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.

Table 18: Exposure analysis: Effects on components of decision-making

	(1) Visit a friend (Index)	(2) Take a bus (Index)	(3) Purchase clothing (Index)	(4) House repairs (Index)	(5) Purchase appliances (Index)	(6) Accept promotion (Index)	(7) Take up work (Index)
exposed	0.13*	0.25**	0.22**	0.17**	0.12	0.11	0.12
	(0.08)	(0.10)	(0.09)	(0.09)	(0.08)	(0.08)	(0.08)
N	488	488	488	488	488	488	488
Non-exposed mean	-0.14	-0.27	-0.17	-0.26	-0.22	-0.12	-0.22
Non-exposed SD	0.85	1.00	1.13	0.87	0.85	1.26	1.18

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.

found for the women who participated in the promotion programme. Recent domestic violence increases insignificantly by 2 percentage points in column (3), and exposed women are also a little more likely to choose a private option in the economic game, as shown in column (5).[66] These indications of conflict could potentially be a response to the wife's higher involvement in decision-making, but I lack data to back this up.

When investigating which of the the seven different decisions are affected, I find that all point estimates are positive, as Table 18 demonstrates.[67] However, I find the largest effects of more than 0.2 SD on the decisions for the woman to take a bus to run an errand, and on purchasing clothing and jewellery for the woman (columns 2 and 3). The effect on the decisions to visit a friend and house repairs are also significantly affected. It hence appears that the women exposed to new female supervisors primarily gain more autonomy in both their mobility and spending decisions, but also in wider decisions in the household.[68]

[66]Note that the incidence of the women ever having been slapped or beaten by their husband is 0.30 in the non-exposed sample. I do not find an effect of exposure on this outcome.

[67]Note that the outcome variables for the separate decisions again are indices, since I dichotomise the four different categories of involvement explained in Table 3 into dummy variables, and then create a standardised Anderson index.

[68]There are also some indications that the exposed women are able to translate some of this decision-maker power into less time spent on domestic work, though this effect is economically small. As column (5) of Panel A in Table A.17 illustrates, the exposed women spend about ten minutes less on domestic work on the last day off work. This is mirrored by the results from the husband's survey, which shows that the husbands' time spent on chores increases by about 12 minutes (see column (5) of Panel B in the same table). Note, however, that these offsetting effects are relative to very different means for wives and husbands. Whereas the wives in the non-exposed group spend on average more than 4.5 hours on chores on the non-working day, the husbands of the non-exposed wives only spend about 2 hours on these tasks. Economically speaking, the magnitude of the effects I find is therefore small. The patterns of time use on the last work day are less clear, as columns (1) to (4) in Table A.17 show. The exposed wives appear to sleep marginally less than the non-exposed wives, which seems to be driven by more time spent on domestic work and leisure. The husbands instead

Table 19: Exposure analysis: Effects on different definitions of the decision-making index

	(1) Ordinal (Index)	(2) Involved (Index)	(3) Sole (Index)	(4) Joint (Index)
exposed	0.36***	0.27***	0.10**	-0.07
	(0.09)	(0.10)	(0.05)	(0.06)
N	488	488	488	488
Non-exposed mean	-0.26	-0.17	-0.20	0.18
Non-exposed SD	1.08	1.29	0.67	0.83

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.

When analysing how the decision-making process changes, I find that the exposed women are both more likely to get involved in the decision-making process at all, and more likely to be the sole decision-maker. The decision-making index presented in Table 17 is constructed using dichotomised ordinal variables and hence captures all potential margins of change, as discussed and defined in Section 3. In Table 19, I present results for three other ways of constructing the index, with column (1) repeating the main result for comparison. In column (2), the index is created using one binary variable for each of the seven decisions, where the binary variable is 1 if the woman is involved in the decision, and 0 if not.[69] In column (3), I also create the index using binary variables for each decision, but this time the variable indicates whether the woman makes the decision alone without needing permission, and is 0 otherwise.[70] Lastly, column (4) creates the index using binary variables indicating a joint decision involving the woman and other household members, which are 0 otherwise.[71]

As column (2) in Table 19 demonstrates, there appears to be a large switch towards women getting involved in decision-making from not being involved at all, of 0.27 of a standard deviation. In addition, there is also a smaller (at 0.1 SD) but significant effect on women being able to make decisions alone, as seen in column (3). The effect on joint decision-making in column (4) is small and insignificant, though the point estimate is negative. These patterns are confirmed when I look at the seven decisions separately in Table A.18 in the appendix. The likelihood of joint decision-making decreases for the decisions about visiting a friend and purchasing clothing for the women, whereas the exposed women are both more involved and more likely to be the sole decision-maker than the non-exposed group for the decisions of taking a bus and purchasing clothing.

Table A.18 also underlines that the women working in the Bangladeshi garment industry are already highly involved in decision-making in their households. In the non-exposed group, for all seven questions about decision-making, at least 85 % of women report being involved in decision-making. Nevertheless, their autonomy is low: Only 10 % of non-exposed women report that they on their own can make decisions about them visiting a friend. 8 % of non-exposed women can decide alone whether they want to take a bus to run an errand, and 17 % decide on their own about buying clothes and jewellery for themselves.

The results in this subsection therefore suggest that the exposure to female supervisors allowed women

report less leisure time, mainly resulting from more time spent on work.

[69] This means that the categorical variable is at least 1=Others decide, but need the woman's permission, or higher.

[70] This means that the categorical variable is 4=Woman decides alone without needing permission.

[71] This means that the categorical variable is 2=Woman is involved in joint decision-making.

to gain more say in intra-household decision-making. However, they are not able to translate this decision-making power into changes in the household's spending. There are some non-significant indications of an increase in conflict in the household, which could possibly result from this increased involvement.

5.3 Robustness

In this section, I show that the large and positive effect on women's involvement in decision-making is robust to different variable definitions, a matching approach, different OLS specifications, a larger set of potential controls for the PDS Lasso algorithm, and to restricting the estimation to a subsample to take into account possible operator sorting. The results from the husband's data do not contradict a positive effect on the women's involvement in decision-making, but the point estimate is smaller.

5.3.1 Different variable definitions

I already demonstrated the robustness of the impact on women's involvement in decision-making to different variable definitions in the previous section in Table 19. As I showed then, the results also hold if I analyse the impact on women being involved (vs. not involved) in decision-making, or on being the sole decision-maker. There appears to be a slight negative effect on joint decision-making, which is not significant, however.

5.3.2 Matching

I implement the nearest-neighbour matching estimator suggested by Abadie et al. (2004) for the exposed and non-exposed comparison. I follow the strategy described in Section 4.3, and match on all variables for which I test balance in Table 15, in addition to factory, enumerator and month dummy variables. The results in Table A.19 confirm the positive and significant effect on women's involvement in decision-making, with an estimate of 0.24 SD. This is a third smaller than the estimate I obtain using PDS Lasso, of 0.36 SD. In addition, I also find additional evidence that there is some increase in conflict when I use the matching approach, mainly due to more efficient estimates. The effects on both recent domestic violence (0.04 percentage points) and hiding money (0.06 percentage points) in the economic game are significant at the 5 % significance level.

5.3.3 Different specifications

I next show that the positive and significant effect on women's involvement in decision-making is robust to different OLS specifications and a larger set of potential control variables for the PDS Lasso algorithm. The columns of Table A.20 are set out exactly as described in Section 4.3. The coefficient remains highly significant and large across all columns of Table A.20. I obtain the smallest estimate of 0.21 SD with OLS and all linear control variables in column (5), but even this remains significant at the 5 % significance level.

Table 20: Exposure analysis: Effect on income

	(1) Personal income (Taka)
exposed	226.16*
	(118.21)
N	488
Non-exposed mean	9387.21
Non-exposed SD	2863.31

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.

5.3.4 Husband's survey

Using data from the husband's survey does not contradict the previous results, though I do not find such a large impact on women's involvement in decision-making. Table A.21 shows the effects on the main outcomes using the husband's data. The effect on women's decision-making is again positive, but at 0.05 SD the effect size is much smaller than the estimate from the wife's survey, and insignificant. When looking into the seven decisions separately, I again see the largest effects on the decisions about the women's mobility, but none of the estimates are significant.[72] Similar to their wives, the husbands in the exposed group are significantly more likely to choose the private option in the economic game (column (4) of Table A.21), which could be considered another piece of evidence that conflict in the household increased.

5.3.5 Operator sorting

To investigate whether the results could be driven by operators sorting to the lines of the new female supervisors, I estimate the effects on the main outcomes for the subsample of operators who joined the lines exposed to the new female supervisors before the start of the promotion programme, comparing them to the previous sample of non-exposed operators. I use the data from the factory follow-up for Uckat and Woodruff (2019) to determine this condition. I show the results in Table A.22 in the appendix. Column (4) shows that the result is not due to operator sorting. The coefficient of 0.38 SD for this subsample is very similar to the result for the whole sample of exposed operators of 0.36 SD, and highly significant.

5.4 Unpacking the effects

5.4.1 Marshallian income effect

I again return to the framework introduced in Section 3.1 and first ask whether the large and positive effect on decision-making for the women working under the new female supervisors could be due to a Marshallian income effect. I show the effect on the exposed women's personal income in Table 20, estimated using Equation 5. The effect on the women's personal income is positive, but small and marginally significant at 2.40 % of the income in the non-exposed group. This small effect is not

[72] Results available upon request.

Table 21: Exposure analysis: Effects on other assignable goods expenditures

	(1) Men (% in HH income)	(2) Girls (% in HH income)	(3) Boys (% in HH income)
exposed	-1.06**	-0.21	-0.06
	(0.44)	(0.37)	(0.37)
N	488	255	262
Non-exposed mean	4.69	2.41	2.35
Non-exposed SD	5.25	2.50	2.41

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies. Column (2) only includes observations where the respondent reported having at least a daughter, or a girl living in the household. Column (3) only includes observations where the respondent reported having at least a son, or a boy living in the household.

Table 22: Exposure analysis: Relative wage effect

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)
exposed	-0.57	-1.27	0.02	0.38***	0.11**
	(0.90)	(1.35)	(0.02)	(0.09)	(0.05)
exposed*earns more	-0.24	0.20	0.02	-0.13	-0.06
	(1.21)	(1.55)	(0.03)	(0.10)	(0.05)
N	449	446	445	449	449
Non-exposed mean	6.55	8.51	0.02	-0.24	0.11
Non-exposed SD	6.67	12.04	0.13	1.08	0.31

Standard errors clustered at the production line level in parentheses. Sharpened q-values in squared brackets. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.

surprising since the exposed women continued working as line operators. At the time of the household surveys, none of the exposed or non-exposed women were working in a supervisor position.

Despite this small increase in the women's income, the main results in Section 5.2 showed that there was no effect on the budget shares of women's assignable goods expenditures or remittances, with point estimates actually being negative. I compare these results with the effects on the budget shares spent on other assignable goods expenditures in Table 21. I find no effects on assignable expenditures for girls and boys, and again find insignificant negative point estimates. The point estimate for assignable expenditures on men is negative and significant at 23 % of the comparison mean. The effect, however, is of similar size as for women's expenditures in Table 17, with -1.06 percentage points for men's goods compared to -0.89 percentage points for women.

Overall, these results suggest that a Marshallian income effect is unlikely to be at play, though the significant negative effect on men's assignable expenditures could be interpreted as a weak indication that expenditures are allocated away from men's goods.

5.4.2 Relative wage effect

I next analyse whether the effects differ depending on the share of income earned by the woman. I define an indicator *earnsmore* as equal to one if the woman earns more than 50 % of the combined income of

Table 23: Exposure analysis: Effects on attitudes and belifs

	(1) Overall self-beliefs (Index)	(2) Confidence (Index)	(3) Negotiating (Index)	(4) Gender attitudes (Index)	(5) Aspirations (Dummy)
exposed	-0.03	-0.00	0.17	-0.05	0.03
	(0.13)	(0.11)	(0.11)	(0.09)	(0.05)
N	488	488	488	488	488
Non-exposed mean	0.04	0.09	0.02	0.10	0.50
Non-exposed SD	1.23	0.84	1.04	0.93	0.50

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

the two respondents.[73] This is the case for 41 % of women in the sample. Table 22 shows the results when I interact this indicator with being exposed to a female manager. None of the interaction terms are significant. The interaction terms also do not point into the same direction for the five outcomes, with some being positive and some negative. For the decision-making outcome, the interaction terms is negative but insignificant. Taken together, this suggests that the woman earning more or less than the husband does not drive the results.

5.4.3 Exposure effect

The previous two subsections suggested that Marshallian income and relative wage effects are likely not explaining the large positive effect on the exposed women's involvement in decision-making. I therefore investigate whether exposure to new female supervisors could have affected the attitudes or beliefs of the women working under these new female managers. Table 23 presents the results, and Table A.3 in the appendix defines all variables.

The effect on the overall index and all individual components are small, except for the women's negotiation behaviour. For this variable, I find a positive effect of 0.17 SD, with a marginally insignificant p-value of 0.11. This outcome is an index derived from vignettes that present the women with a specific situation where a female character meets resistance from her husband against her plans (e.g. going to evening school to earn a school-leaving certificate). The respondent is then asked to describe how the character should respond to the husband to achieve her goals for each vignette, which is coded according to its sophistication (see Table A.3) and summarised in an index according to Footnote 30. The outcome therefore captures the women's willingness and ability to put herself forward in intra-household negotiations. Though this evidence can only be considered tentative, the large effect on this outcome suggests that the role model of a female supervisor in the workplace might have given the exposed women the ability or the confidence to get more involved in decision-making in the household.

6 Conclusion

This paper analysed the impacts of women's career advancement on intra-household bargaining in the context of the Bangladeshi garment industry. I investigated both the direct effects on the promoted women themselves as well as potential indirect effects on the women working under the new female

[73] I do not have baseline data for the sample in this analysis, so I do not know whether the respondents were already earning more than their husbands at the start of the programme.

supervisors. I find that the women who participated in the promotion programme gained significant bargaining power in the household, as compared to the shortlisted runners-up. The effects are strongest for spending on women's assignable goods expenditures, and remittances to family members who take care of the couple's children. When analysing the effects of working under a female manager, as compared to working on comparison lines with male supervisors, results indicate that exposed women are more involved in decision-making in the household. Overall, I find suggestive evidence that the findings for both the direct and indirect effects are driven by women gaining the confidence to get involved in bargaining in the household, rather than Marshallian income effects or changes in the relative wage in the household.

Viewed together, these results demonstrate that there are potential complementarities between women's power in the workplace and women's power in the household. They suggest that policies to promote female career advancement have the potential to address inequalities in the household at the same time. Importantly, I find that the direct effects on the promoted women are amplified by the impacts on women working as subordinates of the female managers. These indirect effects are potentially relevant for a much larger number of women than the direct effects of promotions, which will usually only affect a small share of the female working population.

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A Appendices

A.1 Description of the training modules

The training consisted of one soft-skills component of four days, and one hard-skills component of five days. The soft skills training aimed to equip female operators with the non-cognitive skills to be successful as line supervisors. It was developed in cooperation with a Bangladeshi psychologist and covered the following topics:

- Stress management, self-awareness and setting boundaries
- Assertive communications and confidence
- Leadership and worker management skills

The hard-skills component aimed to give female operators the technical skills required for a line supervisor. It was developed in cooperation with Bangladeshi industrial engineers experienced in the garment industry and covered the following topics:

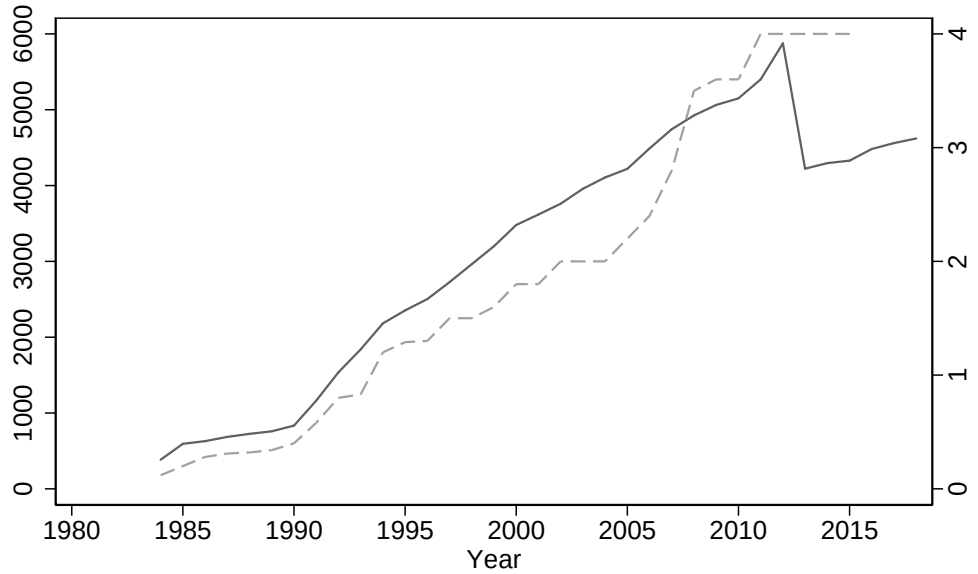
- Overview of the entire production process (from first contact with buyer to shipment)
- Planning and managing production lines (tracking production, efficiency calculations, machine layout, machine types and attachments, quality control)
- Solving problems on the production line (bottlenecks, line balancing, quality problems)

The soft-only and soft & hard-skills group both received the soft skills training immediately, whereas only the soft & hard-skills group received the hard-skills training immediately. The control group received training after the experimental period, about six months later.

The training was implemented part-time. Trainees participated in training at a training centre for two days a week, and worked in the factory the other four days of the working week. On the days in the factory, trainees from all three groups worked as Assistant Line Supervisors with gradually increasing responsibilities during the eight-week trial period. This set-up mirrors the traditional way in which workers are promoted to supervisors in the industry

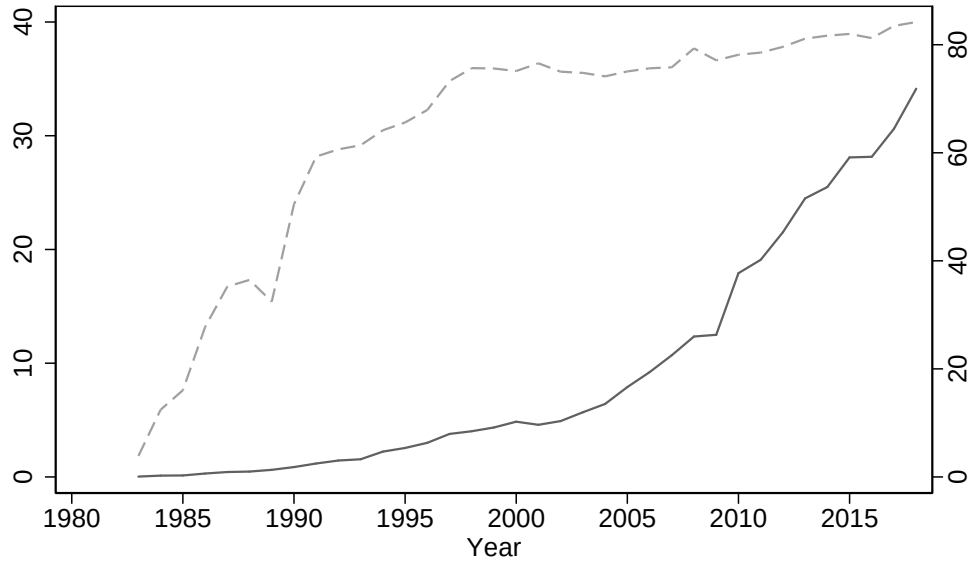
A.2 Figures

Figure A.1: Number of factories and employment in the Bangladeshi ready-made garment sector



Data source: BGMEA Trade Statistics 2017 and 2019

Figure A.2: Exports of the Bangladeshi ready-made garment (RMG) sector



Data source: BGMEA Trade Statistics 2019

Figure A.3: Timeline

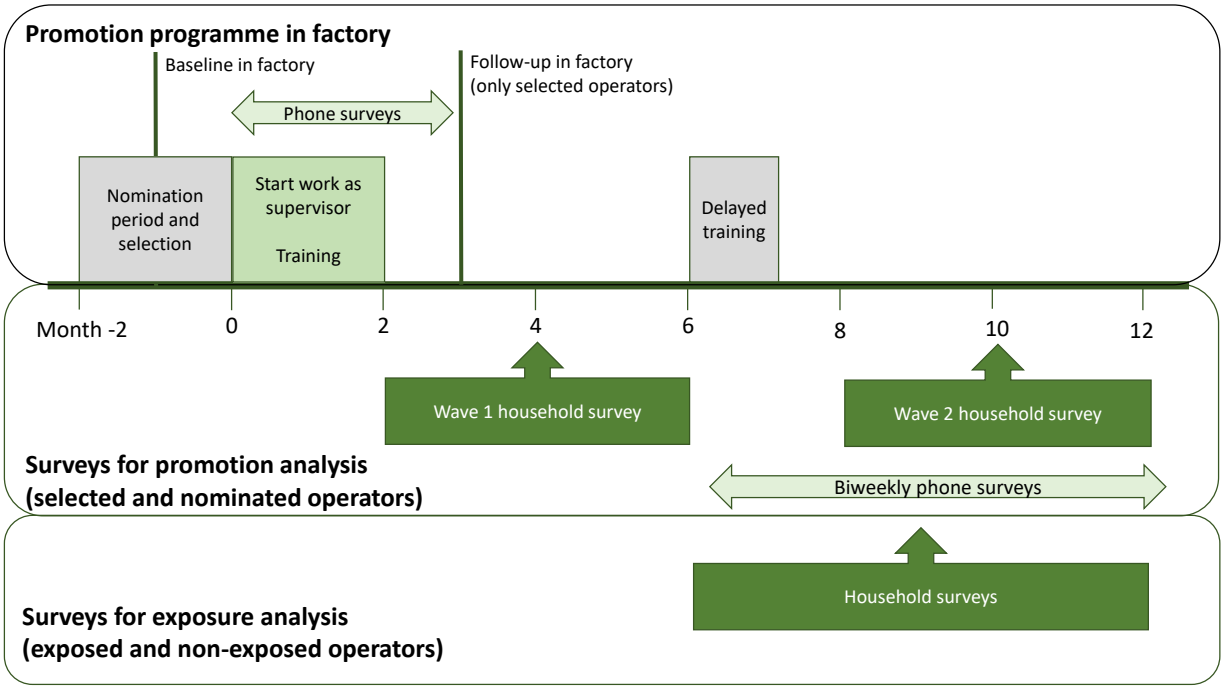
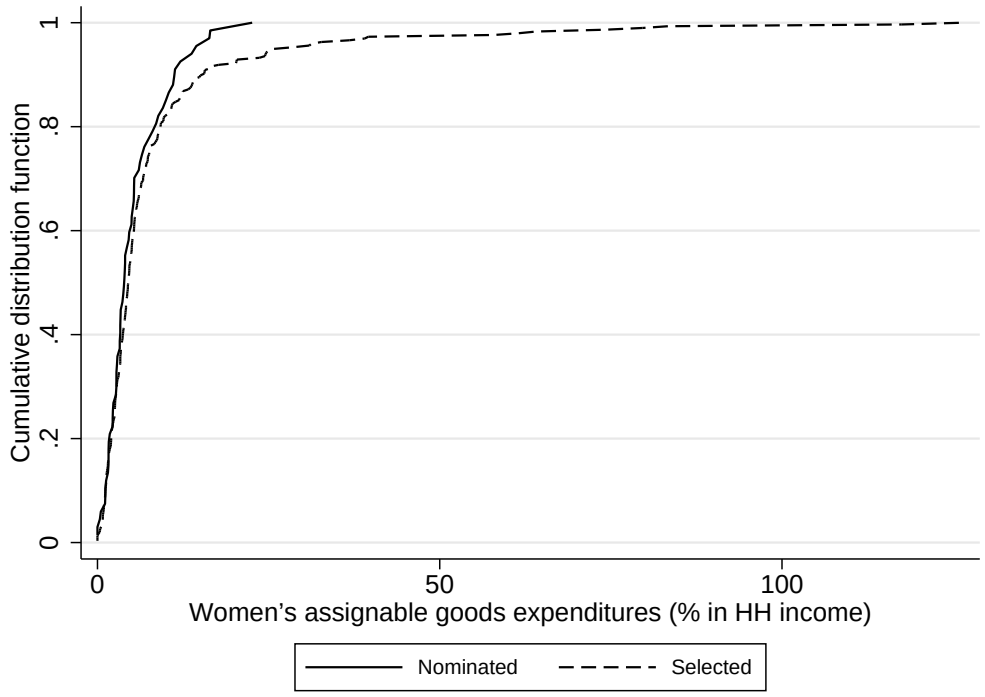
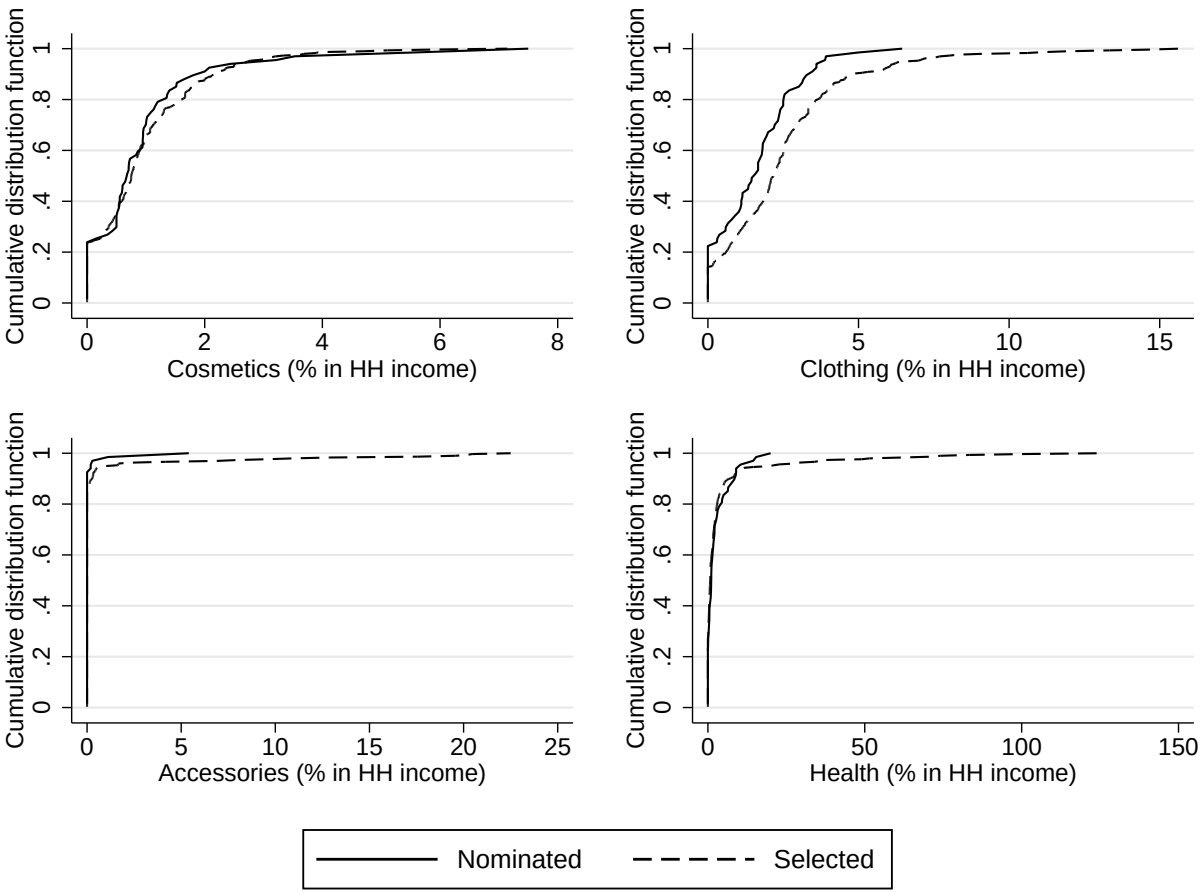


Figure A.4: Cumulative distribution functions of women's assignable goods expenditures for nominated and selected women



Note: Both waves of household surveys are pooled for this graph.

Figure A.5: Cumulative distribution functions of different women's assignable goods expenditures for nominated and selected women



Note: Both waves of household surveys are pooled for this graph.

A.3 Tables

Table A.1: Second respondents identified in pre-survey

Panel A: Promotion analysis (Wave 1)

	N	Percent
Husband	160	69.26
Father	11	4.76
Brother	4	1.73
Mother	4	1.73
Sister	5	2.16
Brother-in-law	1	0.43
No other adult	46	19.91
Total	231	100.00

Panel B: Exposure analysis

	N	Percent
Husband	453	72.36
Father	22	3.51
Brother	6	0.96
Mother	19	3.04
Sister	9	1.44
Other from your family	1	0.16
Father-in-law	1	0.16
Mother-in-law	1	0.16
Sister-in-law	2	0.32
No other adult	112	17.89
Total	626	100.00

Table A.2: Correlations between main outcome variables and demographics

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)
Age	0.07	0.04	-0.00	0.03	0.00
	(0.14)	(0.27)	(0.00)	(0.02)	(0.01)
Education years	0.18	0.43	-0.01	0.02	0.00
	(0.20)	(0.40)	(0.01)	(0.04)	(0.01)
Married	-5.32	-2.34	-0.00	0.69	-0.24*
	(3.43)	(2.95)	(0.03)	(0.58)	(0.12)
Household income	-0.00*	0.00	0.00	-0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Women's share in HH income	6.44**	-3.79	-0.04	-0.16	-0.01
	(2.76)	(4.92)	(0.05)	(0.32)	(0.15)
Nr Children	-1.15	1.73	-0.01	-0.00	-0.01
	(0.92)	(1.29)	(0.01)	(0.10)	(0.04)
Household members	1.43*	-2.75***	0.00	-0.05	-0.02
	(0.78)	(0.45)	(0.01)	(0.07)	(0.02)
Age at marriage	0.23	0.19	0.00	-0.04	-0.01
	(0.18)	(0.33)	(0.00)	(0.03)	(0.01)
Brought assets in marriage	1.29	-0.68	0.02	0.38***	-0.03
	(1.41)	(1.93)	(0.03)	(0.11)	(0.05)
Age gap of spouses	-0.05	0.25	0.00	0.01	0.01
	(0.15)	(0.22)	(0.00)	(0.02)	(0.01)
Education gap of spouses	-0.09	0.30	-0.00	-0.00	-0.00
	(0.15)	(0.28)	(0.00)	(0.02)	(0.01)
Constant	0.52	11.19	0.10	-0.59	0.53*
	(4.84)	(9.66)	(0.08)	(0.87)	(0.29)
N	238	238	237	238	238

Standard errors clustered at the production line level in parentheses. The table only includes observations in the non-exposed group for the exposure analysis. To include variables that are only defined for married respondents, these are set to zero for non-married respondents, and I include dummy variables that indicate whether they are missing as additional controls.

Table A.3: Variable definitions

Variable	Definition
Panel A: Demographic and control variables from surveys	
Age	Age in completed years
Married	Dummy variable =1 if married
Household members	Number of household members
Household head	Dummy variable =1 if respondent is the household head
Migrant	Dummy variable =1 if respondent is not born in Dhaka
Education years	Highest educational attainment in years
Nr Children	Number of children (only available if ever married)
Child outside HH	Dummy variable =1 if any children live outside the household (only available if Nr Children *geq* 1)
Combined income of spouses	Sum of husband and wife's income from economic activities last month (if married). Only available for promotion sample.
Household income	Total household income from economic activities last month
Education difference of spouses	Wife's - husband's education years (if married)
Age gap	Wife's - husband's age (if married)
Woman's share in spouses' income	Ratio of wife's income from economic activity and combined income of spouses last month (if married). Only available for promotion sample.
Woman's share in household income	Ratio of wife's income from economic activity and household income last month (if married)
Decision-making	See Table 3.
Supportive family	Anderson index of the following 3 questions measuring support for female relatives working in garment industry, computed following Footnote 30: 1. Whether the female relatives were ever asked to quit their jobs (=0 if Yes) 2. How supportive the family is towards female relatives working in the garment industry (categorical variable) 3. How supportive the family would be if a female relative received an offer for a promotion to supervisor (categorical variable)
Experience in garment sector	Number of completed years in the garment industry
Exposure	Dummy variable =1 if respondent has worked under a female supervisor or manager
Internal locus of control	Anderson index of 8 locus of control questions indicating whether nominee chooses the internal option, based on Rotter (1966)
Grit	Anderson index of 6 grit items, higher score means higher grittiness , based on Duckworth and Quinn (2009)
Self-efficacy	Anderson index of 10 self-efficacy items, higher score means higher self-efficacy, based on Schwarzer and Jerusalem (1995)
Emotional competence	Anderson index of 16 emotional competence items, higher score means higher competence, based on Mikolajczak et al. (2014)
Leadership	Anderson index of 3 multi-factor leadership items, higher score means higher leadership, based on Avolio and Bass (2014)
Life satisfaction	Rung chosen on Cantril ladder from 0-10, higher rung means higher satisfaction, based on Cantril (1965)
Self-assessment	Assessment of own performance as supervisor on a scale of 1-10 minus assessment of typical supervisor in factory
Ambition	Number of positions nominee would accept a promotion to (supervisor, line chief, assistant production manager)
Muslim	Dummy variable =1 if religion is Islam

(Table A.3 continued.)

Socioeconomic background	Anderson index of variables proxying nominee's socioeconomic background during youth: 1. Father's years of education 2. Mother's years of education 3. Type of house respondent lived in at 14 years of age 4. How often respondent went to bed hungry at 14 years of age
Marriage duration	Completed years of marriage (if married)
Assets into marriage	Dummy variable =1 if respondent brought assets into marriage (if married)
Age at marriage	Age - Marriage duration

Panel B: Variables from diagnostic tests

(See Uckat and Woodruff (2019) for details.)

Numeracy score	Nominee's score on 10 numeracy questions, score out of 100
Literacy score	Nominee's score on 20 literacy questions, score out of 100
Processing speed score	Nominee's score on 193 processing speed questions, score out of 100, based on the Wechsler Adult Intelligence Scale
Garment knowledge score	Nominee's score on 81 garment knowledge questions, score out of 100
Family support score	Nominee's score on 8 family support questions, score out of 100 • 5 statements about family support, asks the respondent to respond on a four-point scale from agree to disagree • 3 questions about the level of support given to other women in the family who work in garment factories
Interest score	Nominee's score on 6 interest questions, score out of 100 • 2 questions about whether respondent would want to be promoted to supervisor or line chief • 4 questions that indirectly ask whether they are interested in the supervisor position, four-point scale from agree to disagree
Confidence score	Nominee's score on 4 confidence questions, score out of 100 • Question how respondent would rate their performance compared to a typical supervisor on a 5-point scale, if they were promoted to supervisor today • 3 forced choice questions between one statement that says "I am confident" using various words, and a dummy statement about the factory

Panel C: Production line statistics

Product complexity	Standard minute value (SMV) of garment. Time that the industrial engineers of each factory estimate a production line with qualified operators working at a standard pace would require to produce one piece of the garment

(Table A.3 continued.)

Efficiency	Measures how the daily output of each line in garment units, adjusted for the standard minute value of the garment, compares to the total output possible that day with the workers present on the line and the hours worked. Calculated as $$efficiency = \frac{(Pieces\,produced \cdot SMV)}{Workers\,on\,the\,line \cdot Hours\,operated \cdot 60}. \quad (6)$$
Alteration rate	Percentage of all garments that needs to be altered out of all produced units
Absenteeism rate	Percentage of workers absent amongst all workers (present and absent) on the line

Panel D: Additional outcome variables

Men's assignable expenditures (% in household income)	Percent of household income spent on assignable expenditures for men, consisting of expenditures on 1. Tobacco and alcohol in the last 7 days, 2. Cosmetics for men and boys in the last calendar month, 3. Clothing and footwear for men in the last three months, 4. Jewellery and accessories for men in the last three months, 5. Health and medical expenditures for men in the last three months, Values are winsorised at a fraction of 0.01 in each tail. Expenditures are harmonised on the monthly level, summed up, divided by the household's total monthly income, and multiplied by 100.
Girls'/boys' assignable expenditures (% in household income)	Percent of household income spent on assignable expenditures for girls/boys, consisting of expenditures on 1. Clothing and footwear for girls/boys in the last three months, 2. Educational expenditures for female/male household members or female/male children living outside the household in the last three months, Values are winsorised at a fraction of 0.01 in each tail. Expenditures are harmonised on the monthly level, summed up, divided by the household's total monthly income, and multiplied by 100.
Domestic work	Time spent on domestic work on last working/non-working day.
Leisure	Time spent on leisure on last working/non-working day.
Sleep	Time spent on sleep on last working/non-working day.
Work	Time spent on work on last working day.
Confidence	Anderson index of women's confidence • 10 self-efficacy questions • 1 question whether feeling about self changed in last 5 months, recoded such that 1=Yes, for the better, 0=No, it is the same/Yes, for the worse. • 1 forced choice question about confidence at work, recoded such that =1 if respondent chooses "I hold my head high because I have done really well at work." over "With a bit of luck, things could have turned out better for me at work."

(Table A.3 continued.)

Negotiation	Anderson index of questions measuring negotiation behaviour • 2 questions with vignettes where women were asked in an open-ended way to describe what the woman in the vignette should have done to convince her husband. These are coded as follows: 1=She should have behaved as she did, 2=She should have apologised to the husband, 3=She should have asked nicely again, in a soft tone, 4=She should have explained to him why she wants to make this decision, 5=She should have explained to him why she wants to make this decision, and acknowledged his concerns, 6=She should have explained to him why she wants to make this decision and acknowledged his concerns. She should then try to find a solution that allows for a compromise. • 1 forced choice question, recoded such that =1 if respondent chose "If I really want something, I can easily convince my family." over "At times, I find it hard to explain what I want."
Gender attitudes	Anderson index of questions measuring attitudes about gender. 3 vignettes describing situations in which women exercised mobility and independence in purchases. Respondents were asked to state their personal opinion. Questions will be recoded such that 1=Socially very inappropriate, 2=Socially somewhat inappropriate, 3=Socially somewhat appropriate, 4=Socially very appropriate.
Aspirations	Dummy variable =1 if respondent agrees more with "There is still much more I want to achieve in my work career." over "I am looking forward to enjoying a quieter life back home as soon as I have enough money."

Table A.4: Joint orthogonality of observable characteristics for nominated and selected groups

	(1)	(2)	(3)
	Dependent variable: selected		
Age	0.00	0.01	0.00
	(0.01)	(0.02)	(0.01)
Married	-0.58		-0.06
	(0.57)		(0.10)
Household members	-0.02	-0.01	-0.01
	(0.02)	(0.03)	(0.02)
Household head	0.09	0.15	0.12
	(0.12)	(0.15)	(0.11)
Migrant	0.01	0.01	0.01
	(0.07)	(0.08)	(0.07)
Education years	0.01	0.02	0.01
	(0.02)	(0.02)	(0.02)
Decision-making	-0.02	-0.02	-0.02
	(0.04)	(0.04)	(0.04)
Supportive family	-0.01	-0.02	-0.01
	(0.04)	(0.04)	(0.03)
Experience in garment sector	-0.02	-0.01	-0.02
	(0.01)	(0.01)	(0.01)
Exposure to female SV	-0.05	-0.04	-0.05
	(0.07)	(0.07)	(0.06)
Internal locus of control	-0.08**	-0.06	-0.06
	(0.04)	(0.04)	(0.04)
Grit	0.05	0.07	0.04
	(0.04)	(0.04)	(0.04)
Self-efficacy	0.05	0.04	0.04
	(0.04)	(0.04)	(0.03)
Emotional competence	-0.01	-0.02	-0.01
	(0.04)	(0.04)	(0.04)
Multi-factor Leadership	-0.02	-0.02	-0.02
	(0.04)	(0.05)	(0.04)
Life satisfaction	0.01	0.01	0.01

(Continued on next page.)

(Table A.4 continued from previous page.)

	(0.02)	(0.02)	(0.02)
Self-assessment	-0.00	-0.00	-0.01
	(0.02)	(0.02)	(0.02)
Ambition	-0.00	-0.02	-0.01
	(0.04)	(0.04)	(0.04)
Literacy score	0.31	0.17	0.28
	(0.19)	(0.22)	(0.18)
Numeracy score	-0.04	0.03	-0.02
	(0.21)	(0.25)	(0.20)
Processing speed score	-0.49	-0.22	-0.65
	(0.48)	(0.54)	(0.45)
Garment knowledge score	-0.43	-0.44	-0.40
	(0.35)	(0.40)	(0.34)
Family support score	0.14	0.21	0.15
	(0.24)	(0.28)	(0.22)
Interest score	-0.03	-0.01	0.03
	(0.19)	(0.22)	(0.18)
Confidence score	0.01	-0.09	0.02
	(0.18)	(0.22)	(0.18)
Muslim	0.07	0.05	0.03
	(0.19)	(0.20)	(0.18)
Socioeconomic background	0.01	0.01	0.00
	(0.03)	(0.03)	(0.03)
Nr Children	0.03	0.04	
	(0.06)	(0.06)	
Combined income of spouses	0.00*	0.00	
	(0.00)	(0.00)	
Education difference	-0.02*	-0.02*	
	(0.01)	(0.01)	
Age gap of spouses	0.01	0.01	
	(0.01)	(0.01)	
Women's share in total spousal income	0.77**	0.65	
	(0.36)	(0.39)	
Marriage duration	-0.00	-0.01	
	(0.01)	(0.01)	
Brought assets in marriage	0.04	0.08	

(Continued on next page.)

(Table A.4 continued from previous page.)

	(0.07)	(0.08)	
Constant	0.82	0.12	0.98**
	(0.70)	(0.66)	(0.46)
N	198	166	198
p-value of F-statistic	0.87	0.96	0.92
Missing indicators	Yes		
Only married		Yes	

In column (1), to include the variables only defined for married women and include currently unmarried respondents, I follow two steps. First, I set these variables to zero if they are missing and, second, I include dummy variables in the regression that indicate whether the variables are missing. Column (2) does not make these adjustments and hence only includes married respondents. Column (3) includes only covariates defined for all respondents. * p<0.1, ** p<0.05, *** p<0.01

Table A.5: Effects on women's assignable expenditures in Wave 1 and Wave 2 separately

	(1) Wave 1	(2) Wave 2
selected	1.99*	3.22
	(1.16)	(1.98)
N	165	165
Nominated mean	5.21	5.68
Nominated SD	4.95	4.44

Table only includes respondents interviewed in both waves. Column (1) only includes observations from Wave 1, column (2) from Wave 2. Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.6: Effects on time use

Panel A: Women's time use (Hours)

	Last work day				Last non-work day		
	(1) Domestic work	(2) Leisure	(3) Sleep	(4) Work	(5) Domestic work	(6) Leisure	(7) Sleep
selected	0.09	-0.09	-0.20*	0.19	-0.10	-0.01	-0.07
	(0.13)	(0.16)	(0.11)	(0.27)	(0.13)	(0.23)	(0.18)
N	336	336	336	336	363	363	363
Nominated mean	2.86	1.83	7.02	10.26	4.56	4.00	8.96
Nominated SD	1.02	1.26	1.03	1.86	1.48	2.29	1.22

Panel B: Husband's time use (Hours)

	Last work day				Last non-work day		
	(1) Domestic work	(2) Leisure	(3) Sleep	(4) Work	(5) Domestic work	(6) Leisure	(7) Sleep
selected	0.03	-0.12	-0.13	0.05	0.11	0.05	0.01
	(0.11)	(0.15)	(0.13)	(0.25)	(0.18)	(0.19)	(0.17)
N	330	330	330	330	350	350	350
Nominated mean	1.06	2.92	7.34	10.60	2.33	4.89	9.05
Nominated SD	0.82	1.71	0.84	1.47	1.25	2.47	1.18

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.7: Regression discontinuity design—Effects on main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)	(6) Controlling money (Taka)
selected	4.94**	6.82*	0.01	-0.16	0.14	-35.46
	(2.00)	(3.98)	(0.08)	(0.44)	(0.24)	(82.44)
N	145	162	180	184	86	89
Nominated mean	4.37	5.45	0.09	-0.14	0.32	146.15
Nominated SD	3.49	7.59	0.29	1.16	0.48	147.60
IK bandwidth	0.24	0.26	0.31	0.33	0.28	0.32

Standard errors clustered at the individual level in parentheses. Regression discontinuity estimation allows for a linear function of the running variable on either side of the cutoff. Bandwidth selected following Imbens and Kalyanaraman (2012).

Table A.8: Matching—Effects on main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)	(6) Controlling money (Taka)
selected	3.98***	1.85	-0.02	0.32	0.07	9.89
	(1.00)	(1.25)	(0.04)	(0.20)	(0.14)	(30.12)
N	363	362	363	363	181	182
Nominated mean	5.32	6.00	0.09	-0.16	0.28	146.77
Nominated SD	4.48	8.63	0.29	1.13	0.45	147.72

Heteroskedasticity-robust standard errors in parentheses. One-to-one nearest-neighbour matching with replacement implemented on all balance variables in Table 5 as well as factory, enumerator and month dummies. The weighting matrix for matching variables is the diagonal matrix of the inverse sample standard errors.

Table A.9: Different variable definitions—Robustness for women's expenditures and remittances

	Women's expenditures			Remittances		
	(1)	(2)	(3)	(4)	(5)	(6)
	% in HH income	IHS of % in HH income	Taka	% in HH income	IHS of % in HH income	Taka
selected	2.71**	0.19	395.59**	3.48**	0.30	954.15***
	(1.07)	(0.13)	(186.12)	(1.40)	(0.24)	(324.62)
N	363	363	363	362	362	362
Nominated mean	5.32	2.07	1201.99	6.00	1.43	1305.97
Nominated SD	4.48	0.83	1050.47	8.63	1.60	1986.61

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies. IHS is the inverse hyperbolic sine transformation.

Table A.10: Different specifications—Robustness for women's expenditures and remittances

Panel A: Women's assignable goods expenditures (% in HH income)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
selected	2.71**	3.01***	2.63*	2.68**	2.67*	1.58	2.48**	2.04**
	(1.07)	(1.10)	(1.51)	(1.06)	(1.40)	(1.73)	(1.18)	(1.03)
N	363	363	363	363	363	363	354	363
Nominee mean	5.32	5.32	5.32	5.32	5.32	5.32	5.34	5.32
Nominee SD	4.48	4.48	4.48	4.48	4.48	4.48	4.51	4.48
Method	PDS	OLS	OLS	OLS	OLS	OLS	OLS	PDS
Controls	PDS	No	No	No	Linear	Linear	Theory	PDS & interactions
Factory F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Enumerator F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Month F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Imbalanced controls	PDS	No	No	Yes	No	Yes	No	PDS

Panel B: Remittances (% in HH income)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
selected	3.48**	3.47**	3.32**	3.13**	3.35**	4.13**	2.63*	3.32**
	(1.40)	(1.37)	(1.54)	(1.34)	(1.41)	(1.63)	(1.55)	(1.47)
N	362	362	362	362	362	362	353	362
Nominee mean	6.00	6.00	6.00	6.00	6.00	6.00	5.97	6.00
Nominee SD	8.63	8.63	8.63	8.63	8.63	8.63	8.70	8.63
Method	PDS	OLS	OLS	OLS	OLS	OLS	OLS	PDS
Controls	PDS	No	No	No	Linear	Linear	Theory	PDS & interactions
Factory F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Enumerator F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Month F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Imbalanced controls	PDS	No	No	Yes	No	Yes	No	PDS

Table A.11: Husband's survey—Robustness for main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Wife's decision-making (Index)	(4) Husband's Hiding money (Dummy)	(5) Husband's controlling money (Taka)
selected	2.21*	5.13***	-0.05	-0.03	-26.07
	(1.14)	(1.50)	(0.10)	(0.07)	(18.74)
N	350	349	349	173	175
Nominated mean	5.71	6.31	0.09	0.19	134.48
Nominated SD	4.52	9.39	0.65	0.40	131.68

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from the wife's demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.12: Phone surveys—Effects on women's and girls' assignable goods expenditures

	(1) Women's and girls' expenditures (Taka)	(2) Remittances to wife's family (Taka)
selected	657.55**	1008.50**
	(290.91)	(479.94)
N	1395	1390
Nominated mean	1781.96	2361.05
Nominated SD	2562.26	4787.86

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and survey round dummies.

Table A.13: Placebo test—Comparing top and bottom half of selected group

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)	(6) Controlling money (Taka)
placebo	0.31	0.60	0.01	0.04	-0.11	-46.06**
	(1.93)	(1.62)	(0.03)	(0.11)	(0.08)	(19.23)
N	296	295	296	296	145	151
Placebo control mean	8.13	9.25	0.06	-0.14	0.37	120.42
Placebo control SD	14.83	12.91	0.23	1.02	0.49	154.36

Table only includes women selected for the promotion programme. Standard errors clustered at the individual level in parentheses. Sharpened q-values in squared brackets. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.14: Attrition—Robustness for women's assignable expenditures and remittances

	Balanced panel		Wave 1 only	
	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Women's expenditures (% in HH income)	(4) Remittances (% in HH income)
selected	2.49**	2.93*	1.92*	3.08*
	(1.15)	(1.50)	(1.02)	(1.68)
N	330	329	181	181
Nominated mean	5.45	6.00	5.19	5.08
Nominated SD	4.67	8.89	4.62	8.22

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.15: Training effects—Robustness for women's assignable expenditures and remittances

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)
selected	3.32**	3.29*
	(1.43)	(1.71)
selected*softonly	-2.60	0.80
	(1.77)	(1.92)
selected*hardsoft	0.78	-0.22
	(2.68)	(1.96)
N	363	362
Nominated mean	5.32	6.00
Nominated SD	4.48	8.63
p(selected*softonly= selected*hardsoft=0)	0.13	0.87

Standard errors clustered at the individual level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.16: Exposure analysis: Joint orthogonality of observable characteristics for exposed and non-exposed groups

	(1)	(2)	(3)
	Dependent variable: exposed		
Age	0.01	0.02	0.00
	(0.01)	(0.01)	(0.01)
Married	0.40**		0.15*
	(0.17)		(0.09)
Household members	0.01	0.00	0.01
	(0.02)	(0.02)	(0.02)
Migrant	-0.06	-0.05	-0.06
	(0.05)	(0.05)	(0.05)
Education years	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)
HH income	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)
Muslim	-0.34	-0.53**	-0.31
	(0.23)	(0.25)	(0.23)
Socioeconomic background	-0.02	-0.03	-0.03
	(0.02)	(0.03)	(0.02)
Household head	0.13	0.07	0.21

(Continued on next page.)

(Table A.16 continued from previous page.)

	(0.17)	(0.30)	(0.17)
Women's share in HH income	-0.04	-0.17	-0.03
	(0.14)	(0.16)	(0.14)
Nr Children	0.01	-0.01	
	(0.04)	(0.05)	
Age gap of spouses	-0.00	-0.00	
	(0.01)	(0.01)	
Marriage duration	-0.00	-0.01	
	(0.01)	(0.01)	
Brought assets in marriage	-0.12**	-0.11*	
	(0.06)	(0.06)	
Education difference	0.00	0.00	
	(0.01)	(0.01)	
Constant	0.35	0.76**	0.55*
	(0.36)	(0.38)	(0.31)
N	488	427	488
p-value of F-statistic	0.41	0.29	0.33
Missing indicators	Yes		
Only married		Yes	

In column (1), to include the variables only defined for married women and include currently unmarried respondents, I follow two steps. First, I set these variables to zero if they are missing and, second, I include dummy variables in the regression that indicate whether the variables are missing. Column (2) does not make these adjustments and hence only includes married respondents. Column (3) includes only covariates defined for all respondents. * p<0.1, ** p<0.05, *** p<0.01

Table A.17: Exposure analysis: Effects on time use

Panel A: Women's time use (Hours)

	Last work day				Last non-work day		
	(1) Domestic work	(2) Leisure	(3) Sleep	(4) Work	(5) Domestic work	(6) Leisure	(7) Sleep
exposed	0.08	0.03	-0.16*	-0.05	-0.16*	0.12	0.09
	(0.08)	(0.08)	(0.10)	(0.18)	(0.10)	(0.14)	(0.10)
N	465	465	465	465	488	488	488
Non-exposed mean	3.17	1.57	6.85	9.80	4.58	3.27	8.22
Non-exposed SD	0.97	0.98	1.08	1.20	1.06	1.80	1.31

Panel B: Husband's time use (Hours)

	Last work day				Last non-work day		
	(1) Domestic work	(2) Leisure	(3) Sleep	(4) Work	(5) Domestic work	(6) Leisure	(7) Sleep
exposed	0.05	-0.19***	0.03	0.16	0.20***	-0.11	-0.20*
	(0.08)	(0.07)	(0.08)	(0.18)	(0.08)	(0.08)	(0.10)
N	414	414	414	414	449	449	449
Non-exposed mean	0.98	2.59	7.31	9.93	2.04	4.19	8.86
Non-exposed SD	0.64	1.32	0.75	1.37	0.94	1.99	0.93

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory and enumerator dummies.

Table A.18: Exposure analysis: Effects on components of decision-making with different definitions

Panel A: Involved in decision (Dummy)

	(1) Visit a friend	(2) Take a bus	(3) Purchase clothing	(4) House repairs	(5) Purchase appliances	(6) Accept promotion	(7) Take up work
exposed	-0.00	0.07**	0.02**	0.07**	0.01	0.02**	0.01*
	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)
N	488	488	488	488	488	488	488
Non-exposed mean	0.99	0.89	0.98	0.86	0.95	0.98	0.99
Non-exposed SD	0.11	0.32	0.13	0.35	0.21	0.14	0.11

Panel B: Sole decision (Dummy)

	(1) Visit a friend	(2) Take a bus	(3) Purchase clothing	(4) House repairs	(5) Purchase appliances	(6) Accept promotion	(7) Take up work
exposed	0.05	0.07**	0.07*	0.01	0.02	-0.00	-0.02
	(0.03)	(0.03)	(0.04)	(0.01)	(0.01)	(0.02)	(0.02)
N	488	488	488	488	488	488	488
Non-exposed mean	0.10	0.08	0.17	0.01	0.01	0.24	0.06
Non-exposed SD	0.30	0.27	0.37	0.11	0.09	0.43	0.24

Panel C: Joint decision (Dummy)

	(1) Visit a friend	(2) Take a bus	(3) Purchase clothing	(4) House repairs	(5) Purchase appliances	(6) Accept promotion	(7) Take up work
exposed	-0.12***	-0.05	-0.08**	0.05	0.00	0.01	0.00
	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
N	488	488	488	488	488	488	488
Non-exposed mean	0.54	0.75	0.77	0.84	0.95	0.66	0.91
Non-exposed SD	0.50	0.44	0.42	0.36	0.23	0.48	0.29

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.19: Exposure analysis: Matching—Main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)
exposed	-0.36	0.12	0.04**	0.24***	0.06**
	(0.63)	(1.30)	(0.02)	(0.07)	(0.03)
N	488	483	482	488	488
Non-exposed mean	6.48	8.49	0.02	-0.26	0.12
Non-exposed SD	6.63	11.81	0.13	1.08	0.33

Heteroskedasticity-robust standard errors in parentheses. One-to-one nearest-neighbour matching with replacement implemented on all balance variables in Table 15 as well as factory, enumerator and month dummies. The weighting matrix for matching variables is the diagonal matrix of the inverse sample standard errors.

Table A.20: Exposure analysis: Different specifications—Robustness for women's involvement in decision-making

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
exposed	0.36***	0.23**	0.36***	0.26***	0.21**	0.32***	0.32***	0.30***
	(0.09)	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)	(0.08)
N	488	488	488	488	488	488	488	488
Non-exposed mean	-0.26	-0.26	-0.26	-0.26	-0.26	-0.26	-0.26	-0.26
Non-exposed SD	1.08	1.08	1.08	1.08	1.08	1.08	1.08	1.08
Method	PDS	OLS	OLS	OLS	OLS	OLS	OLS	PDS & interaction
Controls	PDS	No	No	No	Linear	Linear	Theory	PDS
Factory F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Enumerator F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Month F.E.	PDS	No	Yes	No	No	Yes	Yes	PDS
Imbalanced controls	PDS	No	No	Yes	No	Yes	No	PDS

Table A.21: Exposure analysis: Husband's survey—Effects on main outcomes

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Women's decision-making (Index)	(4) Husband's Hiding money (Dummy)
exposed	0.41	-0.78	0.05	0.08*
	(0.89)	(1.28)	(0.10)	(0.04)
N	449	449	449	449
Non-exposed mean	6.55	9.03	0.07	0.12
Non-exposed SD	6.51	12.80	1.18	0.33

Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and baseline bargaining measures, their squares, and factory, enumerator and month dummies.

Table A.22: Exposure analysis: Operator sorting—Effect on main outcomes for subsample on production line before programme

	(1) Women's expenditures (% in HH income)	(2) Remittances (% in HH income)	(3) Violence (5 months, Dummy)	(4) Decision-making (Index)	(5) Hiding money (Dummy)
exposed	-1.00	-0.25	0.02	0.38***	0.02
	(0.70)	(1.34)	(0.02)	(0.09)	(0.04)
N	354	352	351	354	354
Non-exposed mean	6.48	8.49	0.02	-0.26	0.12
Non-exposed SD	6.63	11.81	0.13	1.08	0.33

Table drops the exposed women who joined the production line after the start of the promotion programme. Standard errors clustered at the production line level in parentheses. Controls chosen using pdslasso from demographic controls and bargaining measures, their squares, and factory, enumerator and month dummies.