On Her Account: Can Strengthening Women's Financial Control Boost Female Labor Supply?*

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Abstract

In collaboration with the state government of Madhya Pradesh, we experimentally varied whether women's wages from India's public workfare program were deposited into female-owned bank accounts instead of into the male household head's account (the status quo). This treatment increased women's work, both in the program and in the private sector, despite no change in market wages. Treatment effects are concentrated among two groups of women: those who had not previously worked for the program and those whose husbands disapprove of women working. These results are at odds with a model of household behavior in which labor force participation decisions only depend on wages and own-preference for leisure. Instead, we argue that they are consistent with a model in which gender norms internalized by men limit women's labor market engagement.

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1 Introduction

Female labor force participation has remained low and stagnant in many emerging economies despite rapid economic growth. India offers a particularly stark example. Despite robust economic growth, the labor force participation rate for women aged 15-64 declined from 37 percent in 1990 to 28 percent in 2015, making Indian women some of the least employed in the world (ILO, 2015). Yet nearly one third of Indian housewives express interest in working. Simply bringing these women into the labor force would increase Indian FLFP by nearly 80 percent (Pande et al., 2015).[1] What is preventing these latent workers from joining the labor force?

This paper examines the role of supply-side constraints linked to low household bargaining power and traditional gender norms in suppressing female employment. Having a wife who works is a source of social stigma or shame for many Indian men, who are expected to economically provide for the household (Boudet et al., 2012). Thus some latent female workers may stay out of the labor force in order to abide by their husbands' wishes, provided female bargaining power is sufficiently low. Increasing women's bargaining power could therefore draw women into the labor force. Increasing a woman's bargaining power may also increase her ability to control her own earnings and thereby further induce her to work. In contrast, a basic model of efficient household bargaining without social norms predicts that greater bargaining power would cause women to substitute leisure for work, and so work less rather than more (Chiappori et al., 2002).

To test whether strengthening a woman's bargaining power increases her labor supply, we leverage a large-scale randomized controlled trial, conducted in partnership with the Indian state of Madhya Pradesh (MP), which generates exogenous variation in women's control over household resources. Our experiment focuses on India's federal workfare program – the Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) – which provides rural households work at a fixed minimum wage. At the time of the experiment, the status quo in MP was for workers' earnings to be deposited into a single household bank account, almost exclusively owned by the male household head. As a result, a woman's MGNREGS earnings were transferred directly to her husband.

Our experiment targeted poor rural households in 197 local government units, called gram panchayats (GPs), in four districts of the state. In a randomly-selected subset of GPs,

[1] Drawing these women into the labor force may also work to address other gender inequities. Female employment has been shown to delay marriage, increase female work aspirations, improve child health, and reduce the male:female sex ratio (Qian, 2008; Atkin, 2009; Jensen, 2012; Heath and Mobarak, 2015). In the United States rapid growth in female labor force participation also preceded important changes in broader social norms regarding gender roles in both the economy and the household (Goldin, 2006).

we worked with the state to open bank accounts for women. In half of these communities, the woman's account was linked to the MGNREGS payment system so that her wages would flow directly into her account. Since most women had no previous banking experience, we cross-randomized (at the GP level) a two-hour information intervention, which reviewed the benefits and security of using last-mile banking providers. This design makes it possible to estimate the effect of increasing a woman's control over her wages separately from the effect of increasing her access to a bank account, while holding household resources constant.

Our analysis combines administrative data from the MGNREGS public database, administrative bank data, and a detailed endline survey to generate four key results. First, direct access to MGNREGS payments increased women's work under the program: relative to women who received only a bank account, those who received our most intensive intervention (linked bank accounts plus information on the utility of bank accounts, henceforth "link plus") were 34 percent more likely to be listed on MGNREGS rolls 15 months after the intervention. Moreover, the quantity of MGNREGS wages flowing into female-owned accounts was large: conditional on receiving a payment into an individual bank account, the average payment to women over the 15 month interval was $61 – equivalent to 26 percent of women's annual income from non-MGNREGS sources.

Second, private sector employment also increased in the same treatment arm. Household survey data show that, roughly six months after the intervention, labor force participation was 12 percentage points higher and annual private sector earnings increased by 24 percent among women assigned to "link plus". The results are driven entirely by work compensated in cash, so do not reflect women's response to a change in how private sector earnings are received. Instead, we interpret the results as a response to the change in bargaining power gained through greater control over MNREGS wages. Consistent with this interpretation, the gains are concentrated among women who were least attached to the labor force prior to our intervention and among women whose husbands were most opposed to female work. Both patterns support the hypothesis that the intervention increased female bargaining power, and that women were able to use their improved bargaining positions to push back against social norms internalized by men.

Third, these effects translate into other meaningful changes in women's lives. "Link plus" women were more likely to engage in economic transactions outside the household and stated higher levels of mobility relative to the control group.

Finally, financial inclusion alone did not move the needle on FLFP. Although our intervention roughly doubled the share of women who had bank accounts, receiving only a bank account – with or without supplementary information – had no observable impact on women's employment or earnings. Thus, giving a woman a safe place to keep her wages is

not enough; resources need to be explicitly directed to that savings account and women need to be given the basic tools to use it in order to precipitate behavior change.

Taken together, our results support the hypothesis that norms against female work, which are internalized by men, constrain FLFP. An alternative hypothesis is that the linking treatment increased women's effective return to MGNREGS work. Although this might explain some of the the increase in program participation, it can only account for the increase in private sector work in a world with substantial fixed costs to employment. However, we find no evidence that plausible fixed costs (e.g. childcare needs) mediate our treatment effects. Similarly, the large extensive margin results for women, coupled with our finding that male labor supply did not decline, suggests that the intervention did not realize its impacts by simply making it more difficult for men to appropriate women's wages.

Our results contribute to several strands of literature, starting with earlier research on how gender norms shape and constrain women's work. Akerlof and Kranton (2000) propose a model in which individuals' sense of "identity" dictates utility gains and losses over actions that either align or clash with identity norms. In our context, for example, part of men's gender identity may dictate that "a husband provides for his family's economic needs". If his wife goes to work on her own, this may threaten his gender identity, generating a utility loss.[2] Other work shows that norms regarding FLFP are persistent, culturally-specific, and passed down from generation to generation (Fernandez et al., 2004; Fernandez and Fogli, 2009; Alesina et al., 2013), but can change over time (Fernandez, 2013).

Our conceptual framework explicitly accounts for the idea that men and women may internalize different norms and that this, in turn, has consequences for household bargaining and the co-evolution of women's empowerment and labor supply. In this sense, we merge the literature on norms with work from developed country contexts studying how changes in women's household bargaining power impact female labor supply. The existing empirical literature on the relationship between female labor force participation and women's bargaining power leverages plausibly exogenous variation in sex ratios (Angrist, 2002), divorce laws (Gray, 1998; Chiappori et al., 2002; Stevenson, 2008), alimony rights (Rangel, 2006), and inheritance laws (Heath and Tan, 2015). One limitation of such policy experiments is that they impact more than female bargaining power within a marriage – e.g. many of the same papers find important impacts on marital timing and duration, which could impact women's incentives to work for reasons other than household bargaining power. Our experiment circumvents this concern by generating exogenous variation in the control of an important household entitlement, holding prices and marriage market factors constant.

[2]More recently, Bertrand et al. (2015) present evidence of norms against American women earning more than their husbands.

A number of studies show that conditional cash transfer programs that target women increase female bargaining power (Almas et al., 2015; Bobonis, 2009, 2011; Attanasio and Lechene, 2014). However, to the best of our knowledge none find changes in female labor force participation, likely because the associated income transfers are sizable enough to reduce labor supply (Skoufias et al., 2013; Hasan, 2010). Hence, these settings are less useful for generating evidence on the link between norms, bargaining power and women's employment, and for the same reason, cash transfer policies may be inappropriate for encouraging female labor force participation. In contrast, public workfare programs can be used to boost labor market engagement of marginalized groups by prescribing gender quotas and mandating equal pay while also encouraging women to take on paid employment or new categories of jobs. Indeed, several studies have found evidence that MGNREGS increased overall female employment (Azam, 2012; Shah and Steinberg, 2015). However, these studies are ill-positioned to isolate the impact of changing female bargaining power, since the MGNREGS rollout changed the number and type of jobs available for women and also changed private sector wages (Imbert and Papp, 2015).[3]

Finally, our study contributes to a body of work that examines how changes to the design of public benefit schemes, such as direct benefits transfers to bank accounts, impact program and beneficiary outcomes. Much of the existing research has focused on program outcomes, particularly efficiency and leakage (Muralidharan et al., 2016; Aker et al., 2016; Banerjee et al., 2016). One exception is Bachas et al. (2016), who find that giving Mexican conditional cash transfer recipients a debit card (which allowed them to easily check the balance of the bank account linked to their cash transfer), increased bank and overall savings. Our findings demonstrate the scope for gender targeting and the quality of program implementation to impact both program outcomes (e.g. work within MGNREGS) and broader economic outcomes (e.g. private sector work and female mobility).

The rest of the paper proceeds as follows: the next section provides an overview of gender norms, rural work opportunities, and MGNREGS in our study context, followed by a description of our experimental design. Section 3 develops a model of how gender norms can influence female labor supply in a household bargaining context. We then describe our data in Section 4, followed by our main results in Section 5. We conclude in Section 6.

[3]In fact, overall the program rollout appears to have reduced private sector employment, which likely reflects changes in the composition of labor demand (Islam and Sivasankaran, 2015).

2 Experimental Context and Design

We begin by providing background on men's and women's attitudes towards female work and institutional detail on MGNREGS. Then we discuss recent efforts to expand rural financial inclusion in our study area.

2.1 Gender Norms Regarding Women's Work and Mobility

Gender identity norms in Northern India strongly support a social representation of women as a group with limited agency, especially when it comes to the decision to engage with the external world. In 2011 the nationally representative India Human Development Survey (IHDS) found that 52 percent of adult women stated that their husband has the most say in the decision of whether they can work. More broadly, women's physical mobility is highly constrained: Eighty five percent of women in the IHDS report asking for permission to travel alone for short train or bus trips, and 47 percent say they are not allowed to travel alone for these trips at all. In our study sample, a high fraction of women similarly report requiring a male chaperone: three quarters of women asserted that women need to be accompanied by a male relative to go to the neighborhood bank, and only 41 percent of women reported making a trip by themselves to the village market in the past year.

Survey data show that Indian women tend to be more progressive than men when it comes to gender norms – a trend that extends to many other countries across the globe. Figure 1 combines 2010-2014 World Values Survey data on support for female work with International Labour Organization data on FLFP. We create a standardized index of support for female work by aggregating responses to four questions designed to measure attitudes towards women in the labor force.[4] We then calculate nationwide averages by gender. Panels A and B of Figure 1 show that FLFP is higher when both men (Panel A) and women (Panel B) are more supportive of female work. Panel C turns to the gender gap in attitudes. Here, two striking patterns emerge: First, women are more supportive of female work than men in 75 of the 76 countries in the World Values Survey. Second, the attitude gap is highest in countries where women work the least – these countries include India, Pakistan, and much of the Middle East, where women arguably have little bargaining power compared to other parts of the world.

[4]The index aggregates a dummy variable equal to one if the respondent agrees than men should have more rights to jobs than women when jobs are scarce, a dummy variable equal to one if the respondent agrees that men make better political leaders than women, a dummy variable equal to one if the respondent agrees that men make better business executives than women, and a dummy variable equal to one if the respondent agrees that being a housewife is just as fulfilling as working for pay. These variables are standardized (pooling across countries and years) and then averaged by country. The index is then multiplied by -1 so higher values correspond to more support for female work.

2.2 Rural Work Opportunities

Our study population is rural and unskilled. Broadly, such individuals have two work options: private sector work and MGNREGS work. We discuss these two options in turn.

2.2.1 Rural Private Sector Labor Market

We define private sector work to include self-employment and work for other private sector actors. Small businesses are very rare in our study context: just six percent of women and seven percent of men reported any business self-employment income in the year before the endline survey. Instead, most self employment consists of individuals working on their own land or engaging in animal husbandry. Apart from this, the most common type of private sector employment for both genders is casual wage labor. Casual workers are usually paid a fixed wage on a daily or weekly basis, are paid in cash, and work on others' land or construction sites. Casual wage work is seasonal, and almost always pays more per day than MGNREGS work.[5]

2.2.2 MGNREGS

MGNREGS entitles over 55 million Indian households to 100 days of minimum wage work per year, making it one of the largest welfare programs in the world (Subbarao et al., 2012). On paper, the program is "demand based", in that individuals wishing to work are supposed to place a request with the GP, after which GP leaders are obligated to arrange for work. On the ground, the system is often more supply based: GP leaders schedule work projects with some notion of worker demand (e.g. more projects take place in lean seasons), but not all households wishing to work receive the opportunity to do so (Dutta et al., 2012), and the 100 day cap is rarely binding.[6]

MGNREGS has explicitly encouraged female participation by mandating gender wage parity, instituting gender quotas for female participation, and providing work inside rural communities, an attractive proposition for mobility-constrained women. Despite this, the MGNREGS payment architecture runs the risk of systematically discouraging female workers. In 2008 the Government of India directed states to transition from cash to electronic payment of MGNREGS wages into beneficiary-owned bank accounts. The initial status quo was to deposit wages for all working members of a household into a single account,

[5]In our study area the median casual wage was Rs 200. By contrast, the official MGNREGS wage in Madhya Pradesh ranged from Rs 146-167 over the study period.

[6]The fraction of households working less than 100 days in MGNREGS administrative data (conditional on working at all) in our study area ranges from 79 percent to 96 percent between 2013 and 2015. We also observe households working more than 100 days, with little bunching at 100 days.

typically owned by the male head of household. Policy makers have noted this systematic gender inequity. In late 2012, as part of a broader plan to make MGNREGS more female-friendly, the Minister of Rural Development announced that women's MGNREGS wages should be deposited into an individual bank account in the woman's own name (UN-WOMEN, 2012), with the hope that this change would contribute to women's empowerment (Chatterji, 2016). However, actual implementation of this strategy has been heterogeneous both across and within states. For example, in 2016 the Ministry of Rural Development reported that 69 percent of all women were being paid wages into an individually owned bank account, yet state-specific shares ran from 12 percent (Karnataka) to 100 percent (Andhra Pradesh) (MGNREGS, 2016). The corresponding figure for MP is 30 percent.

2.2.3 Rural Financial Inclusion

At the outset of our study, MP was in the midst of a state-wide push to ensure that all citizens had access to a "last-mile" banking kiosk within 5 kilometers of home. Local kiosks (also called customer service points, or CSPs) are operated under the authority of a state-run bank. CSPs offer GP residents zero balance "no frills" accounts, which have no maintenance, deposit, or withdrawal fees, and pay nominal interest on a biannual basis. In our study areas, CSP accounts could only be accessed with an authenticated fingerprint.[7]

This fortuitous combination of policy initiatives made it possible for women to have MGNREGS wages paid into easy-to-access, secure bank accounts for the first time – at least in theory. In practice, local officials in our study areas were slow to target women. We therefore had wide scope to experimentally vary women's access to own accounts, and the "linkage" of these accounts to the MGNREGS system. The next subsection details the specifics of our experimental design.

2.3 Experimental Design

We purposively chose a cluster of four districts in northern MP with severe gender inequities: sex ratios in our study districts ranged from 0.84 to 0.90 females to every male (India Census, 2011).[8] For comparison, the Indian state with the lowest sex ratio in the 2011 Census was Haryana, at 0.88.

[7]In August 2014, the Government of India introduced a national version of this initiative, called Pradhan Mantri Jan Dhan Yojana (PMJDY), with the aim of opening at least one basic bank account for every Indian household. PMJDY precipitated massive account opening drives – by September 2016 over 240 million bank accounts (21.8 million in Madhya Pradesh) had been opened under the initiative. See http://www.pmjdy.gov.in/statewise-statistics. Accessed September 16, 2016.

[8]In drawing the sample frame, we first ranked districts by their sex ratio and literacy gender gap, and then chose the poor performing districts of Gwalior, Morena, Sheopur, and Shivpuri.

Appendix Figure A1 provides a timeline of experimental activities. First, we identified all GPs with functional local banking kiosks (CSPs) in the four study districts. We randomly assigned these 199 GPs to one of three groups: 66 GPs formed the control group, 68 GPs were assigned to receive bank accounts for eligible women, and 65 GPs were assigned to receive bank accounts and the "linking" of eligible women's newly-opened accounts to MGNREGS.[9] In our setting, linking an account means replacing the previous account number attached to a given worker's name in the MGNREGS system with a new account number.

Between November 2013 and January 2014, we conducted a baseline census of 14,088 households listed as having worked for MGNREGS in the past year. A married couple was "eligible" for the study if at least one household member self-reported having ever worked for MGNREGS[10] and the wife lacked a bank account in her own name. We identified 5,851 eligible couples and two GPs without any eligible couples. These two GPs (both initially assigned to the control group) were dropped from our sample, leaving us with 197 GPs.

We then individually informed eligible women in treatment GPs that they could open a bank account at their local CSP, free of charge, during an upcoming account opening drive.[11] On the day of the drive, our team returned to the household to inform the woman that she could visit the CSP with her documents to open an account. The study team also facilitated the account opening process at the kiosk with the cooperation of the local kiosk operator. Our team opened accounts for 2,864 of 3,862 eligible women in treatment GPs. In the 65 GPs selected to receive linking, our team submitted a request to have each newly-opened individual bank account entered into the MGNREGS administrative system – meaning future MGNREGS work a woman completed would be directed into her new account rather than a household account. Women provided consent for this linking and were informed of its implications.[12]

By the end of 2014, all accounts had been opened, but most women had not received account numbers and banking cards, and a number of accounts had yet to be linked to

[9]GP randomization was stratified by whether, at baseline, the GP had: below/above median number of households with joint bank accounts linked to MGNREGS, below/above median percentage of individual MGNREGS accounts, and whether the GP was located in Sheopur district. All randomization was done by computer, in Stata.

[10]More specifically, since our baseline census sample was drawn from MGNREGS administrative records, eligibility required positive MGNREGS work history both according to the programmatic administrative data and the household's census report.

[11]The only requirements for account opening were proof of address and a passport sized photo.

[12]Although women could, in theory, link their accounts on their own, in practice this was difficult: linking required a trip to the block (the administrative unit below the district) office and filing a formal request. GP-level government officials also have the power to link accounts on villagers' behalf, but have limited incentives to do so. Given this, very few women outside the linking group linked their new accounts on their own.

MGNREGS.[13] To address this, we conducted a second round of CSP drives and account linking between November 2014 and April 2015. During these drives we brought eligible women to the CSP, gave them their account numbers and bank cards, and conducted a practical demonstration of how to deposit and withdraw money from the CSP account.[14] Women with linked accounts were again informed that any future MGNREGS wages would be paid into their CSP account.

Per regulatory Reserve Bank of India (RBI) guidelines, rural bank branches are meant to conduct new customer information sessions that provide an overview of the benefits of using bank accounts, including saving for the future and receiving government benefits (RBI, 2016). However, these sessions were rarely conducted in our setting and our early qualitative work suggested that many women had a poor understanding of how their new accounts could be of use. We therefore decided to supplement our original design with an RBI-inspired new customer orientation. This information session was randomized as a third, cross-cutting treatment in half the GPs selected for bank accounts or bank accounts plus linking. We call GPs randomized into this treatment "Accounts Plus" GPs, given the enhanced assistance that was intended to help women understand and use their accounts more effectively. We refer to GPs that did not receive the additional training as "Accounts Basic" GPs. Given the cross-cutting nature of our design, this created four intervention packages: Accounts Basic, Accounts Basic Linking, Accounts Plus, and Accounts Plus Linking.

In Accounts Plus/Accounts Plus Linking GPs, eligible women were invited to participate in a group-based information session about the local CSP and their bank account. The sessions occurred in the GPs in March 2014, following the initial bank account opening camps. The meetings typically lasted around two hours, included 10-15 female attendees, and were interactive in nature. During the sessions, a facilitator used colored flashcards to tell the story of a fictional woman and her family and how she came to use a CSP account. The main aim of the story was to orient women to their local CSP and provide basic information such as what an account could be used for (including saving and receiving benefit transfers) and why money was safe when kept at the CSP. The sessions also emphasized the time and cost savings of transacting at the CSP. While the sessions also touched on the value of savings, this message was not the main one: our intent was primarily to help women understand very basic principles about their accounts.

[13]These issues reflected multiple factors, including overloaded and slow bank servers, poor connectivity in study areas that delayed uploading of applicant details to banks' central servers, and the complexity of the linking process, which required coordination by actors at multiple administrative layers and was often delayed.

[14]Each woman was given Rs 50 to attend the camp. She was given the opportunity to deposit and withdraw some amount of this money to get hands-on experience with banking at the CSP.

The interventions had two main aims. The first was to financially include women so that they would be capable of using low-cost bank accounts to save and receive government benefits payments. The second was to increase women's control over their MGNREGS wages by sending payments directly to their new bank accounts. In the next section, we present a model to highlight how these interventions might impact female labor supply.

3 A Model of Labor Supply with Social Constraints

Our interventions have the potential to impact female labor supply through multiple channels. First, financial inclusion combined with MGNREGS wage payment to women (the linking treatment) could strengthen a woman's control over her MGNREGS wages, thereby increasing her household bargaining power. It is also possible that financial inclusion alone could impact bargaining power if having a bank account improves women's ability to control resources. Second, greater financial inclusion could impact labor supply by easing savings constraints (Callen et al., 2014). Third, although official MGNREGS wages are fixed by the government, our interventions could impact labor supply by changing private sector wages through general equilibrium effects.[15]

In practice, we find limited evidence that accounts eased savings constraints and no evidence that wages in either sector changed (Appendix Table A1), which is not surprising given that our intervention only targeted a subset of female workers in each gram panchayat. Given this, we now develop a simple model of household labor supply to illustrate how, by changing bargaining power, our interventions might change women's participation in the public and private sector labor markets.

3.1 Setup

We draw on models of collective household labor supply in the tradition of Chiappori (1992). A household consists of two members ($i \in \{M, F\}$). Each spouse has a time endowment of 1 that s/he divides between leisure and private sector and MGNREGS work (h_s^i), where s denotes sector $s \in \{P, N\}$. Wages for person i in sector s are w_s^i. Individuals may work as much as they like on the private market, but each spouse's work for MGNREGS is constrained by the number of MGNREGS project days provided by the local government, given by $\overline{N}$. Further, each household receives non-labor income y.

Each spouse receives utility from private consumption (c^i) and leisure (l^i) according

[15] Imbert and Papp (2015) find that MGNREGS itself increased market wages by crowding out private-sector work.

to the utility function $u^i(c^i, l^i)$.[16] To capture norms that may constrain female labor force participation, we assume that both genders incur an additional utility cost, $\gamma^i \geq 0$, whenever $h_P^F + h_N^F > 0$. We do not take a stand on the source of these utility costs: they may reflect collectively-held social preferences or individual-specific tastes – what we wish to emphasize is that both genders may have preferences over whether women work for reasons beyond private enjoyment of leisure. In this sense, our definition of a norm is broader than that commonly used in psychology, so we refer to γ^M and γ^F as "social constraints" to female labor supply in what follows.[17] Finally, we assume that the wife's Pareto weight is given by μ. This weight may be a function of wages, non-labor income, and "distribution factors" (z), which affect the bargaining weight μ, but do not otherwise enter the household utility maximization problem (Blundell et al., 2005).

The household's allocation problem is given by:

$$\begin{aligned} \max_{h_s^i, c^i} \quad & \mu\left[u^F\left(1 - h_N^F - h_P^F, c^F\right) - \gamma^F 1\left(h_P^F + h_N^F > 0\right)\right] + \\ & (1-\mu)\left[u^M\left(1 - h_N^M - h_P^M, c^M\right) - \gamma^M 1\left(h_P^F + h_N^F > 0\right)\right] \quad \text{subject to} \\ & c^M + c^F \leq \textstyle\sum_{i=M,F} \sum_{s=N,P} w_s^i h_s^i + y \\ & h_s^i \geq 0 \\ & h_N^i \leq \overline{N} \end{aligned} \tag{1}$$

Where $1(\cdot)$ is the indicator function.

We now characterize labor supply under this setup. First, assume no social constraints to female work: $\gamma^F = \gamma^M = 0$. In this case, the household's problem can be represented in two stages. In the first stage, the household implements a sharing rule in which the wife receives a share of non-labor income given by $\phi^F\left(w_P^F, w_N^F, w_P^M, w_N^M, y, z\right)$, while the husband receives share $\phi^M = y - \phi^F$.[18] In the second stage, each spouse maximizes his or her own individual utility subject to the budget constraint $c^i \leq w_N^i h_N^i + w_P^i h_P^i + \phi^i$ and the hours constraints. The first order conditions give the familiar result that if an individual works in sector j (and in the case of MGNREGS the hours constraint is not binding), his or her marginal rate of substitution between consumption and leisure is equal to the wage: $u_l^i / u_c^i = w_j^i$.

If $\gamma^F > 0$ but $\gamma^M = 0$, the two-stage setup still delivers the same solution as program 1. However, in some cases a woman may be socially constrained, in that she does not work

[16] We assume $u^i\left(c^i, l^i\right)$ is twice continuously differentiable and strictly increasing and concave in both its arguments. The price of the consumption good is normalized to 1.

[17] In the psychology literature, a norm usually reflects beliefs of a group or an individual's perception of what the group believes (Bicchieri and Muldoon, 2014).

[18] An individual's income share can be negative or positive – the purpose of ϕ^i is to fix which point on the Pareto frontier the household ends up choosing.

even though $u_l^i/u_c^i < w_j^i$. Things look different when $\gamma^M > 0$, since women's preferences over consumption and leisure are no longer separable from men's. Instead, we can think of the household solving the allocation problem under two alternatives: one in which γ^F and γ^M are taken as fixed costs, which will yield the "unconstrained" allocation $(c^{i*}, h_N^{i*}, h_P^{i*})$ with female nonlabor income share ϕ^{F*}, and one in which $\gamma^M = \gamma^F = 0$ but the constraint $h_N^F = h_P^F = 0$ is imposed, yielding $\left(\hat{c^i}, \hat{h_N^i}, \hat{h_P^i}\right)$ and female income share $\hat{\phi^F}$. The household then chooses the allocation that generates the highest aggregate utility.

As in the case where $\gamma^F > 0$, the woman will sometimes be socially constrained. What is different about this case is that it is possible for constrained women to be held back from working by the preferences of their husbands. To see this, note that the solution to the household problem can be represented as a modified two-stage problem. Here the wife receives the female income share from the sub-problem that generates the highest household utility, but then maximizes

$$u^F\left(1 - h_N^F - h_P^F, c^F\right) - \left(\gamma^F + \frac{1-\mu}{\mu}\gamma^M\right) 1\left(h_P^F + h_N^F > 0\right)$$

subject to $c^F \leq w_N^F h_N^F + w_P^F h_P^F + \phi^F$ and the hours constraints. Here, the husband's preferences over the wife's work enter the wife's objective function, and are weighted by the husband's relative bargaining power. This formulation makes it clear that a husband's preference that his wife does not work will matter more when the wife is less empowered. In some cases, a woman may privately prefer working given income share ϕ^F, but will stay out of the labor force due to the weight she puts on her husband's preferences.

3.2 Women's Labor Supply and Control Over Income

We now ask how strengthening women's control over MGNREGS wages changes household allocations. We assume that greater control over own wages increases a woman's bargaining power, which amounts to an increase in female income shares ϕ^{F*} and $\hat{\phi^F}$.

First consider the case where there are no social constraints internalized by men: $\gamma^M = 0$. The intervention has a pure income effect in terms of labor supply: holding prices constant, women would work less and consume more, while their husbands would work more and consume less.

When $\gamma^M > 0$, this is no longer the case. The woman's share of non-labor income in each sub-problem increases, giving rise to the standard income effect *within* each sub-problem. However, the shift in bargaining power may also change which sub-problem solution the household prefers. In this way, an increase in female bargaining power may induce a housewife

to enter the labor market. The following proposition characterizes when this can be case:

Proposition 1 *Consider an increase in female bargaining power, $\Delta\mu > 0$, in a household where $\gamma^M > 0$. This increase in bargaining power can induce a non-working woman to enter the public and/or private labor market only if prior to the change the wife is "socially constrained" by her husband, in that – given her equilibrium income share – she would strictly prefer to work if $\gamma^M = 0$.*

Proof. See Appendix A. ■

This result is intuitive: increasing female bargaining power can increase female labor force participation only when women would prefer to work if left to their own devices, but are held back by their husbands' preferences. Note that a man may end up working *less* if an increase in bargaining power induces his wife to enter the labor force. Although the income effect works to increase male labor supply within each sub-problem, the switch from the constrained to the unconstrained equilibrium could reduce the income transfer a man needs to make to his wife. By way of contrast, the bargaining power effect will have the standard impact on households where women were already working before the change: female labor supply should decline while male labor supply should increase.

Note that proposition 1 does not deliver an unambiguous prediction regarding the overall impact of our interventions on female labor supply – rather, the proposition demonstrates that it is possible for the interventions to have a positive effect when social constraints are binding. The extent to which these constraints are practically important is an empirical question. The theory does, however, deliver the following testable predictions:

1. *Absent social constraints or if only the preferences of women are affected by social constraints, then interventions that increase female bargaining power should decrease female labor supply in all sectors, all else equal.*

2. *If social constraints affect the preferences of the husband, then interventions that increase female bargaining power will increase labor force participation among women who were previously socially constrained by their husbands, all else equal.*[19]

3. *The two sub-groups most likely to exhibit positive treatment effects when social constraints are binding are (1) women who were previously not working, and (2) women whose husbands experience disutility when their wives work $\left(\gamma^M > 0\right)$.*

[19] In a more general model where social costs of work are not fixed, but vary with the level of work, it would also be possible for our interventions to increase work along the extensive margin. It is still the case, however, that our only unambiguous prediction is for women not working before the intervention.

Recall that our intervention has four treatment groups: Accounts Basic, Accounts Plus, Accounts Basic Linking, and Accounts Plus Linking. We conjecture that financial inclusion alone – through either Accounts Basic or Accounts Plus – is unlikely to directly affect household bargaining power. In this case, the above analysis suggests that these two interventions should have no impact on female labor supply, unless they change outcomes by easing savings constraints. In contrast, linking – if well-implemented – could conceivably shift household bargaining power, particularly if MGNREGS wages are an important (potential) part of income. Here, we expect bargaining power shifts to be larger for Accounts Plus Linking (relative to Accounts Basic Linking) if the new customer information sessions were important for helping women successfully interact with the banking system. We now turn to our experiment to empirically assess these predictions.

4 Data and Empirical Strategy

4.1 Data

We use four data sources in our analysis. The first is data from our baseline census, which collected information on bank account ownership and self-reported participation in MGNREGS.

The second source of data is a follow-up survey, which took place between August and December 2015. We randomly selected 4,500 of 5,851 eligible couples to be surveyed.[20] Both the male and the female survey included modules on bank account ownership and banking activities, participation in MGNREGS, and private-sector labor force participation. The female survey collected additional data on proxies of female empowerment such as decision-making and mobility, as well as data on mental health and violence against women. The male survey collected additional data on household labor supply, assets, and interactions with local government officials.[21]

Third, we use administrative data from accounts opened under the auspices of the experiment. These data are only available from one of our two banking partners, but this banking partner serves 81 percent of our sample.[22] The administrative data runs from the date of account opening up until August 31, 2016 and includes a record of every transaction posted to each account.

[20]We dropped 345 eligible women who could no longer be matched to the MGNREGS administrative system before selecting the sample. Thus, the total pool of eligible women for the endline was 5,506.

[21]If the husband was not available to be interviewed, the woman was administered a long-form survey that covered these topics.

[22]Ninety-nine percent of women interviewed at follow-up consented to have their data released to the research team.

Finally, we use administrative data on MGNREGS work activities available from the program's management information system (MIS), available through September 10, 2016. These data include information on when an individual worked, how much s/he was paid, and what account the wages were deposited into.

Table 1 presents averages of individual, household, and community-level characteristics and tests whether averages vary by treatment status. Since the main goal of our analysis is to study effects of linking and Accounts Plus holding access to bank accounts constant, we treat the Accounts Basic group as the primary reference group in column 1. On average, eligible women (Panel A) are 40 years old and have little education; just 11 percent of women report that they can read and write. Husbands of eligible women (Panel B) have 4 years of schooling and 57 percent report that they can read and write. It is also worth noting that literacy rates in our sample are lower than GP-wide averages recorded in the 2011 Indian Census (Panel D); this gap is especially striking for women.

At baseline roughly two-thirds of sampled women reported that they had worked for MGNREGS at least once before, with 16 percent of women reporting that they worked for the program in the past year. Yet 70 percent of women were listed as having worked in MGNREGS administrative data in the year before the baseline. This likely reflects over-reporting of work (i.e. reporting "ghost workers") on the part of local leaders, an important form of program leakage (Niehaus and Sukhtankar, 2013). We observe similar discrepancies for men, though many more men report having worked for MGNREGS in the year before the baseline (32 percent, Panel B). This is not entirely surprising – given that MGNREGS has gender quotas, local leaders may over-report more female work than male work when FLFP is low.

As a balance check, columns 2-5 report coefficients on dummy variables for the Control group, the Accounts Basic group, the Accounts Plus group, and the Accounts Plus Linking group from a regression where the predetermined characteristics serve as outcome variables. Column 6 presents the p-value from an F-test of whether the treatment group coefficients are jointly equal to zero. Although the first row in panel A finds some evidence of imbalance in follow-up across treatment groups, the differences are small in magnitude, follow up rates are quite high (93 percent for women), and no treatment differs significantly from the follow-up rate in the Accounts Basic group. More broadly, three of 30 joint tests of equality are rejected (or two out of 28 when ignoring attrition tests), which suggests that the randomization functioned well.

4.2 Empirical Strategy

Most of our main analysis uses the following regression specification:

$$y_{ig} = \beta_0 + \beta_1 APL_g + \beta_2 ABL_g + \beta_3 AP_g + \beta_4 Control_g + \gamma_s + \lambda_d + \eta_m + x_{ig}^{'}\delta + \varepsilon_{ig} \qquad (2)$$

Where y_{ig} is the outcome of interest for individual i in GP g, APL_g indicates that GP g was randomly selected to receive Accounts Plus Linking, ABL_g indicates selection for Accounts Basic Linking, AP_g indicates selection for Accounts Plus, and $Control_g$ identifies Control GPs. All regressions include controls for strata fixed effects (γ_s), district fixed effects (λ_d), survey-month fixed effects (η_m), and all baseline/predetermined covariates in Table 1.[23] All tables report p-values from F-tests of whether the treatment packages had a significant impact on outcomes relative to the control group (i.e. tests of whether $\beta_j = \beta_4$, for $j = 1, 2, 3$). We also present p-values from two tests of whether linking had a significant impact on outcomes: $\beta_1 = \beta_3$, which tests the significance of linking conditional on Accounts Plus, and a joint test, given by $\beta_1 - \beta_3 = \beta_2 = 0$. Finally, note that all variables denominated in Indian Rupees are top-coded at the 99th percentile unless otherwise specified.

5 Main Results

We start by showing the impact of our intervention on women's financial inclusion and then turn to impacts on labor supply and related outcomes.

5.1 Impacts on Use of Bank Accounts

The first three columns of Table 2 focus on endline survey outcomes. Our field team opened accounts for 74 percent of sampled eligible women in the four treated groups. This had a substantial impact on women's financial access; column 1 of Table 2 shows that 90 percent of women in the Accounts Basic group reported owning a bank account at endline, with no significant differences in ownership across treatment groups. In contrast, women in the control group were 43 percentage points less likely to have an account at endline.[24] Columns 2 and 3 show that although the vast majority of targeted women in treated communities opened bank accounts, most women did not use them. Only 16 percent of women in the Accounts Basic group reported visiting the bank at least once in the past six months and

[23] When baseline variables are missing, we recode them to zero and include dummy variables that identify missing values.

[24] Recall that all women in our sample reported not owning a bank account at baseline. The rapid growth in financial inclusion in the control group is driven by the Indian Government's PMJDY initiative.

women in this group reported an average bank balance of just Rs 93 ($1.43 at an exchange rate of Rs 65 to one US Dollar). Although Accounts Plus (both with and without linking) increased both these outcomes by 43-79 percent, the gains are modest in absolute terms.

Columns 4-8 of Table 2 use administrative bank data to study impacts on accounts opened through our experiment. Here we limit the sample to women who lived in GPs served by the bank willing to share data with us and drop the control group. Even though we see no significant differences in overall financial access across treatment groups, column 4 shows that women in GPs that received Accounts Basic Linking were somewhat less likely to have opened a project account. When using the administrative data we therefore keep women with unopened accounts in the sample and code their usage measures to zero.[25] Column 7 shows that women who received Accounts Plus Linking made significantly more transactions at the bank in the post-implementation period. Average daily balances in this group were also significantly higher. Moreover, we have no evidence that our treatments became less effective over time – columns 7 and 8 show that if anything, treatment effects grew in the period after the endline.

Figure 2 illustrates this more directly. The figure uses bank administrative data to graph cumulative non-MGNREGS deposits (Panel A), MGNREGS deposits (Panel B), withdrawals (Panel C), and the average daily balance (Panel D) by month. The vertical dashed lines indicate our implementation period. Linking to MGNREGS clearly played an important role in driving growth in female account balances, especially when coupled with Accounts Plus. Yet on its own, Accounts Plus did not meaningfully increase savings. This contradicts our survey results, and is surprising given that most female account holders (87 percent) had just one bank account.

What is driving this difference? One possibility is that recall error differed across treatment groups. To explore this, we limit our attention to treated women who reported owning only one bank account, and we compare (un-topcoded) self-reported individual balances to the (un-topcoded) administrative average daily balance calculated between the end of implementation (May 1, 2015) up until a woman's survey date. The correlation between the self report and the administrative value allows us to assess the quality of respondent recall. We find striking differences in recall by treatment: the correlation between self reports and administrative data is 0.12 for Accounts Basic, -0.01 for Accounts Basic Linking, 0.55 for Accounts Plus, and 0.33 for Accounts Plus Linking. This suggests two things: First, women who received Accounts Plus do a much better job of recalling their bank balances, despite the fact that the group information session did not cover accounting or record keeping. If

[25]Non-consenting women are excluded from the administrative dataset. Ninety-nine percent of female account openers consented to let the bank release their administrative account data to our research team.

Accounts Plus increased women's intent to use their accounts in the future, these women may have exerted greater effort to remember and keep track of their balances. Second, women who received linking were only able to keep track of their balances when linking was combined with Accounts Plus. The Accounts Plus treatment emphasized that MGNREGS wages could be paid into individual bank accounts. Women who received Accounts Plus Linking may have had a better understanding of where their MGNREGS payments would be sent and how they could retrieve them.[26]

The results so far suggest that both linking to MGNREGS and the group based information sessions had important impacts on women's engagement with the financial system, especially when delivered in tandem. Importantly, Figure 2 makes it clear that the linking treatment successfully channeled MGNREGS wages into female-owned accounts. In the next subsection, we analyze how the interventions changed female control over MGNREGS wage payments more systematically.

5.2 Impacts on Women's Receipt of MGNREGS Payments

Table 3 studies how the treatments impacted women's direct receipt of MGNREGS funds using administrative data from the MGNREGS MIS and our banking partner. To do so, we focus on women's receipt of MGNREGS wages into female-owned individual accounts.[27] Columns 1 and 2 use MIS data to make two stark points: first, absent the linking intervention, virtually no women received payments into individual accounts – the mean for the Accounts Basic group is 0.002. Second, linking had its intended effect, with 21 and 11 percent of women in the Accounts Plus Linking and Accounts Basic Linking groups receiving at least one deposit into an individual account in the post-implementation period. Importantly, the amount of money flowing into female-owned accounts is meaningful in magnitude. Column 2 shows that women in the Accounts Plus Linking (Accounts Basic Linking) group received Rs 646 (Rs 306) more in MGNREGS deposits relative to the Accounts Basic group over 15.5 months. This is an important amount of income: conditional on receiving at least one deposit, the average total payment was Rs 3,965 ($61) – this is equivalent to 26 percent of non-MGNREGS annual income reported by the same group of women. Given the magnitude of these payments, it is plausible that linking could shift women's bargaining position in the

[26]The zero correlation in Accounts Basic Linking could also be driven by program leakage. If local leaders claimed that women worked without women's knowledge, this could drive a wedge between self-reported balances and actual balances.

[27]The MIS data list the bank account number for all wage deposits. We code an account as individual if no other member of a woman's household received wages into that account. To the extent that some female-owned accounts received wage deposits from other household members, we will underestimate the amount of money flowing to women. Given the experimental context, we expect such cases to be rare.

household.

Columns 3 and 4 present analogous estimates using bank administrative bank data. Here, the sample size is smaller for two reasons: we exclude the control group since they did not open project accounts with our banking partner, and we limit our attention to the GPs served by the banking partner willing to share data with us.[28] Results using the bank data parallel the MIS data: very few women linked accounts on their own, but money flowed into accounts that our field team linked.

The last four columns of Table 3 report the same set of outcomes, but we truncate the data at the date of each individual's endline survey. Although all the results are qualitatively similar, it is clear that treatment effects intensified over time on both the intensive and extensive margin. Finally, note that our F-tests for the statistical significance of linking ($\beta_1 = \beta_3$ for linking conditional on Accounts Plus and $\beta_1 - \beta_3 = \beta_2 = 0$ for a joint test) strongly indicate that linking increased women's direct receipt of MGNREGS income.

5.3 Impacts on Women's Labor Market Participation

Table 4 studies how the treatments impacted women's participation in the labor market. We begin by focusing on occupation, since India's official FLFP statistics classify a woman as working if her "usual principal activity" in the past year was working. The first two columns focus on the same measure (which was reported by husbands) in our survey data. By this measure only 29 percent of Accounts Basic women were in the labor force. Thus, FLFP in our study area is very similar to India-wide averages (28 percent among women aged 15-64 in 2015, according to the ILO). The Accounts Plus Linking intervention increased the share of women classified as workers by 11.6 percentage points (40 percent), which suggests that the intervention notably increased the share of women meaningfully attached to the labor market. The F-tests strongly reject the null that linking had no impact on women's occupations.

The next four columns focus on women's work for MGNREGS. Columns 3 and 4 use MIS data to calculate the share of women receiving any payment from MGNREGS between either May 2015 and September 2016 (column 3) or May 2015 and the endline survey (column 4). These data suggest that Accounts Plus Linking substantially increased female MGNREGS participation – eligible women who received Accounts Plus Linking were 11.2 percentage points more likely to appear in the MGNREGS MIS at least once between May 2015 and September 2016. This is a 34 percent increase above the mean in the Accounts Basic group. The treatment effect through the survey date is even larger. Columns 5 and

[28] We cannot directly identify MGNREGS deposits in the administrative data – instead we define a deposit to be an MGNREGS deposit if it is a multiple of the MGNREGS daily wage.

6 turn to the endline survey to report impacts on self-reported work for the program in the post-implementation period. Here, we find evidence that Accounts Plus Linking increased program participation, but only when we combine women's own reports of program work with reports from their husbands.[29]

Even using the "spousal union", the treatment effect on self-reported work in the survey is lower than the treatment effect on work according to the MIS (5.8 percentage points versus 14.5 percentage points). One concern is that the treatment effect in the MIS data could be driven by an increase in corruption (i.e. local officials listing non-working women as "ghost workers"). We think this is unlikely. The accounts we opened for women were biometrically authenticated with a fingerprint. If the previous account listed for a woman in the MGNREGS system (generally her husband's) was not authenticated, this would make the linked woman a less attractive ghost worker from the perspective of a local official, which would bias MIS treatment effects down. Additionally, note that as long as ghost payments flowed into women's accounts, the end result would still be that women gained control over more income. Alternatively, date recall could be an issue for the survey data, but it is not clear why recall would be worse in the Accounts Plus Linking group, unless marginal workers have a harder time remembering work for MGNREGS or they are less willing to admit to working for the program (which is possible, if they are more socially constrained women).

Given these issues we choose to interpret the self-reports with caution, and conclude that we have strong evidence that Accounts Plus Linking increased women's *de jure* participation in MGNREGS. The survey data suggest that at least some of this growth is driven by real work activity, but these results are less conclusive. Also note that we are not able to formally reject the null that linking had no impact on MGNREGS participation rates.

Columns 7 and 8 of Table 4 turn to the private sector. In order to measure women's engagement in market work, we asked them to separately report whether they worked for pay in 13 different sectors in the past year, and if so, how often they worked in a given sector and how much they earned from that work.[30] Column 7 shows that 75 percent of women in the Accounts Basic group worked in at least one sector in the past year. Thus, although most women in our sample do not spend the majority of their time working, many more engage in paid work on an occasional basis. Accounts Plus Linking increased private sector participation by 8.2 percentage points (11 percent) and the tests for the significance

[29]More specifically, in column 6 we code a woman has having worked for the program if she or her spouse reports that she worked. Although women's self-reported rate of working for MGNREGS is slightly higher than the cross-reported rate from husbands, the two do not always agree. To be as inclusive as possible we therefore take the superset of the two reports.

[30]The sectors include agriculture on own land, agriculture on leased land, casual farm labor, casual non-farm labor, animal husbandry, own microenterprise, employed by a firm, teaching, anganwadi work, domestic work, work for a bank, and any other work.

of linking (both conditional on Accounts Plus and using the joint test) are significant at the 5 percent level. Column 8 shows that results are very similar when we use the spousal union of reports of annual work. Appendix Table A3 breaks out work by sector. The table suggests diffuse, moderate impacts across a range of activities, with linking (conditional on Accounts Plus) significantly increasing rates of casual work in both the farm and non-farm sectors.

Table 5 provides additional detail on private sector work, with a focus on the intensive margin. Column 1 shows that Accounts Plus Linking led to an 18 percent increase in the number of months a woman works per year.[31] Women who received Accounts Plus Linking reported Rs 3,288 ($51) more in annual income from non-MGNREGS sources, a 24 percent increase over the Accounts Basic mean. Although this coefficient is only marginally significant, we are able to reject no effect of linking under Accounts Plus at the 5 percent level (the joint test has a p-value of 0.095). Moreover, taking a concave transformation of earnings (the inverse hyperbolic sine – column 3) reinforces the notion that linking had important effects on female earnings, especially at the lower end of the earnings distribution. Appendix Figure A2 provides a graphical illustration of distributional effects on months worked and earnings.

One concern is that some women mistakenly identified MGNREGS work as private sector work. To address this, we also asked women how they were paid for each type of work. Just 77 women (less than one percent of the sample) reported receiving payments for non-MGNREGS work into a bank account, and we find no treatment effect on bank account payment (column 5).[32] Rather, column 4 of Table 5 illustrates that the extensive margin effects on market work are driven by work paid in cash, which is consistent with how casual labor markets function in our study areas.

Appendix Table A4 presents a similar analysis of household work in the 30 days prior to the endline. These questions were answered by the husband of the eligible woman, unless the husband was not available for interview. Even using male reports, we find robust evidence that Accounts Plus Linking increased female work and earnings – the treatment increased short-term female labor supply by 9.3 percentage points, a 40 percent increase above the Accounts Basic mean of 23 percent.

To summarize, we find that linking female-owned bank accounts to MGNREGS successfully channeled more MGNREGS income into the hands of women. In response, women

[31]We asked women to report the total number of months they worked by sector. We create an upper bound on total months worked by summing months engaged in each activity and topcoding at 12. We create a lower bound by focusing on the activity with the most months of work. We use the average of the upper and lower bound as our main measure, but results are similar when using either one of the two bounds.

[32]Our qualitative field work found that villagers are able to clearly distinguish MGNREGS work from other types of casual work, as the recruitment and payment systems are very different.

work more for the program. What is especially striking is that women also work more in the private sector. Our theoretical discussion in Section 3 highlights that this pattern of results is at odds with a traditional model of household labor supply, but could be rationalized by a model that incorporates social constraints on women's work. In the next sub-section, we revisit our theoretical predictions to test this hypothesis more directly.

5.4 Are Women Socially Constrained?

Our model predicts that two groups of women should be positively impacted by our interventions: women who were not working at baseline, and women whose husbands are opposed to female work. In this subsection we study heterogeneity in treatment effects along both these dimensions.

Heterogeneity by Baseline Labor Market Engagement Theoretically, we expect linking to increase work among women previously kept out of the labor force. This observation is consistent with the results in Table 4, where we find significant effects of linking on the extensive margin. We test this further by studying how treatment effects vary by baseline labor force engagement, where we use baseline MGNREGS work as a proxy for whether or not a woman was working at baseline.[33]

Figure 3 shows how linking treatment effects vary with baseline MGNREGS work. In order to focus on the pure linking effect with high-quality implementation, we re-estimate our main specification, but with Accounts Plus (no linking) as the omitted category instead of Accounts Basic. We run separate regressions for women who reported they had/had never worked for MGNREGS at baseline and graph the coefficients on the Accounts Plus Linking dummy. The figure clearly illustrates that our linking treatment effects are concentrated among women with no prior involvement in MGNREGS, which supports the social constraints theory. Appendix Figure A3 shows that results are similar if we instead use predetermined controls to predict endline market-wide work behavior in the control group, and impute a work probability to women in the treatment groups.

Heterogeneity by Male Preferences Over Female Work Although social constraints internalized by both men and women may play an important role in a woman's decision to work, Section 3 highlighted that interventions that work to increase female bargaining power

[33]We only have baseline information on women's self-reported work for MGNREGS. Fortunately, our endline confirms that MGNREGS work history significantly predicts broader work behavior. In the control group, women who reported that they ever worked for MGNREGS at baseline were 15 percentage points more likely to have worked in the past year. This correlation is signifiant at the 0.001 level.

are most likely to help women overcome social constraints imposed by their husbands. To explore this hypothesis more directly, we construct a proxy of men's preferences regarding female work and map this to the effect of linking on labor force participation.

We conjecture that women are less likely to face intra-household constraints if their husbands assert that women should be allowed to work for MGNREGS whenever they want to (as opposed to never or only when the household is desperate for income). Since we only observe men's responses to this question at endline, we use the control group to predict male support for female work in the treatment groups. Within the control group we run a probit regression where the outcome is "man reports women should be able to work for MGNREGS whenever they want to" and the independent variables include all individual and household predetermined characteristics listed in Table 1. We use the coefficients from this regression to impute a probability for all women in our sample.[34]

Figure 4 gives a graphical illustration of how treatment effects vary with our proxy. Here, we limit the sample to women in the Accounts Plus and Accounts Plus Linking groups and run local linear regressions of female labor supply on predicted male support for female work. Dashed vertical lines demarcate tertiles of proxied male support. Absent linking, women are more likely to work when their husbands are more supportive of work. Linking to MGNREGS notably raises labor force engagement among women with the least supportive husbands, often completely eliminating the gradient. The only exception to this pattern is the occupation variable, which was reported by men. It is possible that patterns here are influenced by reporting bias – men opposed to female work may not have been comfortable classifying their wives as workers when speaking with our male enumerators.

Table 6 tests whether the patterns in Figure 4 are statistically significant. We classify women as being "highly constrained" if their proxy value places them in the bottom third of the sample in terms of male support for female MGNREGS work. Women in the middle third are classified as facing "medium constraints". In order to focus on linking under high-quality implementation, we limit the sample to women receiving Accounts Plus and run the following regression:

$$\begin{aligned} y_{ig} &= \alpha_0 + \alpha_1 \left(APL_g \times Hhh_{ig}\right) + \alpha_2 \left(APL_g \times Mhh_{ig}\right) + \alpha_3 APL_g + \\ &\quad \alpha_5 Hhh_{ig} + \alpha_6 Mhh_{ig} + \gamma_s + \lambda_d + \eta_m + x_{ig}^{'}\delta + \varepsilon_{ig} \end{aligned} \tag{3}$$

APL_g identifies Accounts Plus Linking communities, Hhh_{ig} indicates that woman i in panchayat g faces high intra-household constraints, Mhh_{ig} identifies moderate intra-household constraints, and the other controls are the same as in specification 2.

[34] Appendix Table A8 reports the probit results and the pseudo R^2.

Table 6 reports results. As expected, treatment effects are largest among the most constrained tertile of women for every outcome except occupation. Moreover, we reject equality of linking treatment effects across tertiles for 5 out of 8 measures on the table. Overall, this analysis suggests that men's preferences over female work play an important role in mediating women's engagement with the labor market.

5.5 Alternative Explanations

It is also important to consider alternative mechanisms by which Accounts Plus Linking could increase female labor supply. Indeed, a number of plausible mechanisms could explain why Accounts Plus Linking made working for MGNREGS more attractive: For example, households might spend money deposited into different bank accounts differently due to mental accounting or rules of thumb (Duflo and Udry, 2004). Or women might have more control over money deposited into an individual bank account if there is asymmetric information in the household (Ashraf, 2009). Put another way, these forces would work to increase the relative return to working for MGNREGS. But in this case, women equating the marginal return to work in different sectors should work *less* in the private sector, not more.

In order to rationalize an increase in both MGNREGS and private sector work, an alternative mechanism would need to impact the return to both forms of work. A natural possibility would be if women's increased participation in MGNREGS changed wages in the private sector. However, Appendix Table A1 shows that we find precisely estimated null effects on both male and female wages. Alternatively, Heath and Tan (2015) propose a non-cooperative model of the household in which a woman's outside option increases the share of her own earned income she can spend as she pleases (they refer to this share as female autonomy). In their model, an increase in female autonomy increases female labor supply while decreasing male labor supply, holding unearned income constant. Although this alternative fits our main results for women, it does not match our finding that men work more for MGNREGS and do not change work in the private sector.[35] Moreover, this mechanism does not deliver the same predictions regarding heterogeneity – if Accounts Plus Linking increased the share of income women were able to control, it is not clear why women least attached to the labor force, or women with husbands most opposed to female work, should respond the most.

Another possibility is that Accounts Plus Linking impacted labor supply by easing sav-

[35]A more general version of their model could allow for men to work more when women gain more autonomy, provided the increase in autonomy reduces the amount of income men are able to tax away from women. In this case, the labor supply effect on men whose wives enter the labor market in response to an increase in autonomy should still be negative.

ings constraints/increasing the rate of return on savings, as in Callen et al. (2014). This explanation seems unlikely, given that balances in female-owned bank accounts are low and there is no impact on labor supply among women who received Accounts Plus without linking. To rationalize this, one would have to assume that linking helped women learn about the benefits of bank accounts, which in turn stimulated a broader labor supply response. This hypothesis is not supported by the data, however: if anything, Accounts Plus Linking women make fewer non-MGNREGS deposits than Accounts Plus women (recall Figure 2).

A final possibility is that women face some fixed cost to working that is not driven by social norms. In this case, if Accounts Plus Linking improved the return to working for MGNREGS, it could induce women to pay the fixed cost and enter the labor market more broadly. One of the most common non-norms costs women might face when entering the labor market is finding child care. If this were the case, we would expect women with young (especially pre-school age) children to be most impacted by Accounts Plus Linking. Appendix Figure A4 estimates Accounts Plus Linking effects (using specification 2) by whether or not a household has a child under the age of 8. Here, we see that treatment effects are apparent for both subgroups, which suggests that our results are not driven by women who face the largest child care burdens at home. Another possibility is that working for MGNREGS helped women learn of work opportunities in the private sector. In the private sector, landlords or labor recruiters visit households and offer them short-term work opportunities. However, recruiters target both men and women, and since virtually all men work, it is unlikely that women's MGNREGS participation increased access to recruiters. Given these results, and the fact that one-off fixed costs may be less relevant given that MGNREGS and market work tend to take place in different seasons, we argue that there is no compelling evidence that non-norms fixed costs are driving our results.

5.6 Spillover Effects

To the extent that social norms are malleable, there is scope for our interventions to have spillover effects on untreated women. Appendix Tables A5-A7 report on how our interventions impacted financial inclusion, MGNREGS work, and private sector work of non-targeted women in sampled households. The tables also report results for men. Overall, we find some evidence that more intensive financial inclusion campaigns increased account ownership among other household members, especially women. We also find evidence that linking (coupled with Accounts Plus) increased MGNREGS work – at least according to administrative MIS data. In contrast, we find no evidence of spillover effects on the private sector labor market.

We can also test for cross-household spillovers by exploiting the partial compliance with our bank account opening camps. To do this, we run regressions separately for account openers and non-openers. The regressions follow specification 2 with one exception: there is no control group dummy, since we limit the sample to treated GPs. Figure 5 graphs the results. With the exception of work according to the MGNREGS MIS, treatment effects are concentrated among women who opened accounts. Thus we find no compelling evidence of spillovers on other women. There are, however, two important caveats to this observation. First, on average we observe women just 5-6 months after our final wave of implementation; if social norms are slow to change, then our endline survey may not have picked up on these effects. Second, our intervention only targeted a subset of women in the gram panchayat, and not all treated women changed their behavior. If norms only move when a sufficiently large fraction of women change their behavior, then our experiment is not well-positioned to identify these effects.[36]

5.7 Impacts on Women's Economic Empowerment

The results so far suggest that increasing female control over benefits payments can help women push past social constraints to labor force participation. Given this, it is natural to ask whether women became more empowered in other aspects of their lives. The first three columns of Table 7 study how the interventions changed women's economic activity outside the labor market. To do this, we asked women whether they made purchases for a range of different goods, and if so, where the money for those purchases usually came from.[37] For each good, we generated dummy variables for whether the woman ever personally purchased the good, whether she made purchases but only with money from others, and whether she ever made purchases with her own money. We follow Kling et al. (2007) to create indices. We standardize each dummy variable relative to the control group, and then take a simple average of the standardized variables.

The first index in column 1 aggregates the "any purchase" responses. Although Accounts Plus Linking did not significantly change overall purchasing behavior relative to the Accounts Basic group, the pure linking effect (conditional on Accounts Plus) is highly significant at the 1 percent level, though still modest at 0.14 standard deviation units. The joint test of linking is also significant at the 1 percent level. Columns 2 and 3 decompose purchases by whose money is being used. Linking robustly increased the rate at which women made

[36] We find no meaningful heterogeneity in treatment effects with respect to the fraction of women treated within a GP.

[37] The goods included daily food, own clothing, children's health, home improvement, festivals, and food and drink outside the home.

purchases with their own money while decreasing the rate at which women made purchases with others' money. This supports the hypothesis that linking transferred more income into the hands of women, and that women were able to retain control of that income in the process of household decision-making.

Columns 4 and 5 turn to female mobility. Our first measure is meant to capture attitudes: here, we asked women whether they agreed with the statement "women should only go to the CSP in the company of a male relative". We code a woman as asserting that "women can go to the CSP alone" if she somewhat or strongly disagrees with the statement. Since this question was only asked of women who had CSP accounts, we limit our attention to women outside the control area, where there are no across-treatment differences in rates of CSP account ownership (see Appendix Table A9). Note that only 23 percent of respondents in the Accounts Basic group assert that women can go to the CSP absent supervision – this reflects strong norms against female mobility in our study area. Both treatment arms incorporating Accounts Plus increased the share of women asserting that male supervision is not needed by 7.4-7.5 percentage points. Although the individual coefficients are only marginally significant, the joint test for Accounts Plus is highly significant, with a p-value of 0.005. In supplementary analysis in Appendix Table A9, we find that the group-based information session did not increase hard knowledge about CSP accounts, but did increase women's belief that the CSP is a safe place to save. When coupled with linking, information also increased the share of women stating that they would most prefer to receive wage payments in their CSP account. These results hint at one potential reason why linking and Accounts Plus had to be implemented together in order to precipitate meaningful economic change: the group session may have increased women's desire to engage with accounts, but without resources flowing into those accounts, women may have had limited latitude to make those desires a reality. Similarly, if women were not comfortable operating their bank accounts without assistance and/or supervision of their husbands, it may have been difficult to retain control of MGNREGS earnings.

Column 5 of Table 7 focuses on actual female mobility. Here, we asked whether women had ever been to 5 common destinations in the past year.[38] Although we find no significant effects of any of the treatments relative to Accounts Basic, we do find evidence that women in the Accounts Plus Linking group were significantly more mobile than their counterparts in the control group.

Finally, column 6 focuses on a standardized index of self-reported decision-making power, which is commonly found in the literature.[39] In spite of the fact that women are more actively

[38]These were the local market, the district market, the anganwadi center (a children's center in the GP), the primary health center, and the woman's natal home.

[39]The decisions in this index correspond to the same set of purchases in the makes purchases index, plus

engaged in making purchases, they report no change in actual decision making. There are two ways to interpret this result: the first is that even though women are more engaged in making household purchases, they do not play a role in determining what those purchases are. The second is that household decision-making dynamics change in slow and subtle ways and are not yet reflected in the self-reports, which require women to report on coarse household rules of thumb. Given that we observe women 5-6 months after the end of implementation, this is certainly possible. Ultimately, longer-run data on women's status within the household is needed to differentiate between these two possibilities.

Our endline also collected data on women's mental health and experiences of domestic violence. Ex-ante, it is not clear how these outcomes should change, especially in the short run: economic empowerment could translate into greater wellbeing, but increased economic agency could also introduce new sources of stress and anxiety. Similarly, if violence against women is a "good" that gives some men utility at the expense of female utility, then violence should decline as women gain more power (Aizer, 2010). However, if men use violence instrumentally to extract surplus from women (Bloch and Rao, 2002), or if men resort to violence to re-establish their male identity (Akerlof and Kranton, 2000), violence could increase as women become more economically empowered. Appendix Table A10 finds no significant impact of our interventions on any mental health or domestic violence outcomes.

6 Conclusion

In recent decades, economic progress in India has translated into better paying jobs and more attractive work opportunities, with wage growth in rural areas outstripping that in urban areas (Jacoby and Dasgupta, 2015). Yet this growth has failed to draw Indian women into the labor market. We argue that social norms – specifically norms internalized by men – play an important role in keeping Indian women out of the labor force, but that women are able to push back against these norms when they gain bargaining power.

Our empirical results support this argument: strengthening women's control over MGNREGS wages increased female participation in the program and in the private sector labor market. These changes run counter to the prediction of a basic model of efficient household decision making, where an increase in bargaining power (precipitated by greater female control over MGNREGS wages) would reduce female labor supply. The results are also incompatible with a banking channel, whereby women's new CSP accounts eased savings constraints – overall rates of account use are low in all treatment groups, and administrative

the decision for women to work outside the home and the decision of how to spend women's earnings. The dummy variables that go into the index identify cases where women state that they help decide.

bank data show that the Accounts Plus Linking group was no more likely to use their new accounts for non-MGNREGS deposits relative to other newly-banked women. Importantly, the norms channel rationalizes both our main treatment effects and key heterogeneity – treatment effects are concentrated among women least attached to the labor market at baseline and women with spouses most opposed to female work, two groups especially likely to be socially constrained by their husbands.

The Accounts Plus Linking treatment also impacted women's economic lives beyond the labor market: these women were more likely to make household purchases, especially with their own money, and experienced greater physical mobility relative to the control group. We find no evidence that these women experienced backlash in terms of mental health or gender based violence. One caveat to these results (as well as our other survey-based outcomes) is that they are relatively short run – the endline took place roughly 5.5 months after the last wave of implementation. Encouragingly, administrative data from both the MGNREGS system and one of our banking partners suggest that treatment effects on MGNREGS work were sustained over a much longer time horizon. Further data collection could shed light on longer-run impacts of our interventions, including impacts on slow-moving, difficult-to-change outcomes such as gender norms.

In addition to shedding light on the determinants of female labor supply in contexts with strong norms against female work, our results also have several important policy implications. First, we illustrate that gender targeting can impact women's engagement with both workfare programs and the labor market at large. This insight is especially important for programs like MGNREGS, which have explicit gender goals. Second, we demonstrate that quality implementation can be very important for programs to have their desired effect. Although we are able to consistently reject a null effect of linking even when pooling across both Accounts Basic and Accounts Plus, our results clearly show that Accounts Basic Linking failed to have substantive impacts on downstream outcomes, even though the intervention did successfully redirect MGNREGS wages into female-owned accounts. Why was Accounts Plus so critical here? It is important to keep in mind that we implemented the Accounts Plus intervention as a direct response to feedback from the field that women were ill-equipped to use their accounts under Accounts Basic. Giving a woman a bank account that she does not feel empowered to access on her own, or does not trust, may be little better than no account at all. In the Indian context, our results imply that new customer information sessions – like those developed by the Reserve Bank of India – may be key for ensuring that India achieves its ambitious financial inclusion goals.

More broadly, our results contribute to a growing literature on the importance of gender norms in mediating women's interactions with the labor market. Most existing work focuses

on the United States, where gender norms are (both currently and historically) more equitable than in countries like India, where women face large restrictions in terms of work, mobility, dress, and other aspects of their lives. As such, our work highlights a number of open questions for future research. For example, one interesting implication of our model is that intra-household dynamics could interact with the co-evolution of economic growth and female labor force participation – e.g. a negative cycle could occur if growth accrues to men, which erodes female bargaining power, which pushes women out of the labor force, which in turn reinforces restrictive norms regarding female work. Our results also raise the question of whether policies that encourage female labor force participation can directly alter the norms and preferences of both affected and unaffected households, or whether experience with female work (holding wages and preferences constant) impacts parents' aspirations for girls.

A Theoretical Appendix

Proof of Proposition 1. Suppose the woman was not socially constrained before the bargaining power shift. Then it must be that the woman's equilibrium nonlabor income share is weakly lower after the bargaining power shift – otherwise she would not enter the labor force. But this would imply that the woman is weakly worse off after the bargaining power shift, which would in turn imply that the new equilibrium is not on the Pareto frontier, which is a contradiction. ■

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Main Tables and Figures

Table 1: Balance on Predetermined Demographic Characteristics

	(1) Accounts Basic Mean	(2) Control	(3) Accounts Basic Linking	(4) Accounts Plus	(5) Accounts Plus Linking	(6) P-Value: Joint Test	(7) N
Panel A: Individual Characteristics of Eligible Women							
Interviewed at Midline	0.931	-0.018	0.002	0.008	0.023	0.046**	4500
Age	40.091	-0.469	-0.167	-0.260	-1.155	0.693	4179
Years Education	0.857	-0.193	-0.008	-0.198	0.075	0.141	4044
Can Read or Write	0.113	-0.015	0.009	-0.016	0.012	0.563	4153
Had No Children (at Baseline)	0.022	-0.002	-0.002	-0.000	-0.004	0.991	4149
Age Had First Child (at Baseline)	19.086	0.022	0.260	0.255	-0.161	0.267	4064
Ever Worked for MGNREGS (at Baseline)	0.643	0.020	-0.062	0.002	-0.020	0.620	4077
Worked for MGNREGS in Past Year (at Baseline)	0.159	0.004	0.009	0.009	0.030	0.937	3686
In MGNREGS MIS in Past Year (at Baseline)	0.728	0.006	-0.023	0.012	0.048	0.772	3902
Panel B: Individual Characteristics of Husbands							
Interviewed at Midline	0.869	-0.005	0.023	0.022	0.040*	0.174	4500
Age	44.517	-0.182	0.586	-0.029	-0.978	0.700	4089
Years Education	4.270	-0.385	-0.228	-0.143	0.350	0.268	4058
Can Read or Write	0.565	-0.068*	-0.058	-0.015	0.002	0.262	3956
Ever Worked for MGNREGS (at Baseline)	0.963	0.001	-0.016	0.004	0.015	0.110	4034
Worked for MGNREGS in Past Year (at Baseline)	0.319	-0.017	0.021	-0.027	0.009	0.899	3576
In MGNREGS MIS in Past Year (at Baseline)	0.794	-0.005	-0.038	-0.032	-0.002	0.874	3905
Panel C: Household/Couple Characteristics							
Male-Female Age Gap	4.637	0.277	0.587	0.111	0.040	0.539	4089
Male-Female Education Gap	3.463	-0.252	-0.265	0.041	0.218	0.493	3932
Hindu	0.961	0.002	0.000	-0.034	0.027*	0.036**	4147
Scheduled Caste or Tribe	0.361	0.076	0.031	0.065	0.046	0.881	3924
Other Backward Caste	0.552	-0.050	-0.023	-0.051	-0.036	0.959	3924
Number Household Members on Job Card	3.935	-0.160	-0.185	0.316	-0.035	0.465	4179
Panel D: GP Characteristics							
Number Eligible Women in GP	42.388	5.233	2.060	6.239	4.462	0.981	4179
Total GP Population	2625.126	1065.635***	1688.853**	855.384**	377.426	0.012**	4179
Fraction GP Population Female	0.462	0.000	-0.000	0.005	0.004	0.701	4179
Fraction GP Population SC/ST	0.311	0.017	-0.025	0.000	0.038	0.864	4179
Fraction Female GP Population Literate	0.403	0.001	0.012	-0.014	-0.012	0.867	4179
Fraction Male GP Population Literate	0.658	-0.038*	-0.021	-0.025	-0.024	0.428	4179
Female Workers / Female GP Population	0.322	0.032	-0.059	0.001	0.008	0.207	4179
Male Workers / Male GP Population	0.517	0.003	-0.002	0.007	-0.007	0.726	4179

Notes: Each row is a separate regression. The first column gives the mean among the Accounts Basic group, columns 2-5 give regression coefficients. Robust standard errors clustered at the GP level are omitted from the table for legibility. Column 6 gives the p-value from a test that all treatment coefficients are jointly equal to zero. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table 2: Impact of Treatments on Bank Account Use

	Survey Data			Bank Administrative Data+				
	(1) Has Bank Account	(2) Been to Bank in Past Six Months	(3) Personal Bank Balance	(4) Opened Project Bank Account	(5) Number Transactions (May 2015-Endline)	(6) Avg. Daily Balance (May 2015-Endline)	(7) Number Transactions (May 2015-Aug. 2016)	(8) Avg. Daily Balance (May 2015-Aug. 2016)
β_1: Accounts Plus Linking	0.036	0.070**	73.387**	-0.055	0.212	84.523**	1.323**	135.366***
	(0.028)	(0.034)	(29.510)	(0.048)	(0.141)	(35.281)	(0.517)	(38.935)
β_2: Accounts Basic Linking	-0.007	-0.005	-22.680	-0.092*	0.089	26.495	0.425	85.488**
	(0.039)	(0.037)	(24.379)	(0.052)	(0.185)	(39.029)	(0.497)	(37.699)
β_3: Accounts Plus	0.035	0.118***	49.774*	-0.016	0.215	-3.741	-0.059	-19.515
	(0.027)	(0.033)	(29.171)	(0.045)	(0.270)	(32.947)	(0.582)	(37.779)
β_4: Control Group	-0.431***	-0.078***	17.979					
	(0.030)	(0.028)	(26.539)					
P-values from F-Tests								
$\beta_1 = \beta_4$	0.000***	0.000***	0.028**					
$\beta_2 = \beta_4$	0.000***	0.019**	0.039**					
$\beta_3 = \beta_4$	0.000***	0.000***	0.216					
$\beta_1 = \beta_3$	0.944	0.173	0.444	0.356	0.991	0.011**	0.040**	0.001***
Joint Test: Linking=0	0.983	0.392	0.453	0.144	0.858	0.026**	0.042**	0.000***
Joint Test: Accounts Plus=0	0.169	0.000***	0.000***	0.707	0.315	0.307	0.351	0.513
Joint Test: All Coeffs.=0	0.000***	0.000***	0.001***	0.243	0.385	0.049**	0.056*	0.001***
Accounts Basic Mean	0.903	0.161	93.310	0.783	0.912	108.951	3.465	138.443
N	4179	4173	4037	2019	1993	1993	1993	1993

+Sample limited to non-Control group GPs served by the bank partner providing administrative data.

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. All variables denominated in Indian Rupees are topcoded at the 99th percentile. Personal bank balances include all balances in individually-held accounts plus half of balances in any jointly-held accounts.

Table 3: Impact of Treatments on Women's Receipt of MGNREGS Funds in Individual Accounts

	May 1, 2015 - Aug./Sept. 2015				May 1, 2015 - Endline			
	MGNREGS Admin Data		Bank Admin Data^{+}		MGNREGS Admin Data		Bank Admin Data^{+}	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any Work - Individual Account	Value Wages - Individual Account	Has MGNREGS Deposit in Project Account	Value MGNREGS Deposits in Project Account	Any Work - Individual Account	Value Wages - Individual Account	Has MGNREGS Deposit in Project Account	Value MGNREGS Deposits in Project Account
β_1: Accounts Plus Linking	0.205***	646.215***	0.257***	895.337***	0.125***	170.602***	0.107***	167.366***
	(0.025)	(102.300)	(0.042)	(250.542)	(0.016)	(24.850)	(0.022)	(40.524)
β_2: Accounts Basic Linking	0.109***	306.325***	0.126***	377.592*	0.048***	66.006***	0.054*	72.622
	(0.024)	(84.565)	(0.042)	(207.555)	(0.016)	(24.188)	(0.029)	(46.123)
β_3: Accounts Plus	0.003	10.580	-0.044*	-271.908	0.004	5.980	0.001	-9.829
	(0.013)	(49.265)	(0.025)	(175.405)	(0.007)	(10.895)	(0.014)	(24.803)
β_4: Control Group	0.017	82.389			0.015*	20.978*		
	(0.013)	(51.770)			(0.009)	(12.187)		
P-values from F-Tests								
$\beta_1 = \beta_4$	0.000***	0.000***			0.000***	0.000***		
$\beta_2 = \beta_4$	0.000***	0.005***			0.021**	0.029**		
$\beta_3 = \beta_4$	0.323	0.176			0.186	0.220		
$\beta_1 = \beta_3$	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Joint Test: Linking=0	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Joint Test: Accounts Plus=0	0.014**	0.027**	0.010**	0.036**	0.003***	0.008***	0.300	0.264
Joint Test: All Coeffs.=0	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Accounts Basic Mean	0.002	1.722	0.061	390.606	0.000	0.000	0.016	34.598
N	4077	4077	1993	1993	4077	4077	1993	1993

$^{+}$Sample limited to non-Control group GPs served by the bank partner providing administrative data.

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. All variables denominated in Indian Rupees are topcoded at the 99th percentile. The spousal union variable is equal to one if either the husband or the wife reports the wife worked for MGNREGS since May 1, 2015. MGNREGS administrative data runs from May 1, 2015 - September 10, 2016. Administrative bank account data runs through May 1, 2015 - August 31, 2016.

Table 4: Impact of Treatments on Women's Work for MGNREGS and the Private Sector

	Main Occupation		Work for MGNREGS				Work in Private Sector	
			MGNREGS Admin Data		Survey Data		Worked Last Year	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Worker	Housewife	Through Sept. 2015	Through Endline Survey	Own Report	Spousal Union	Own Report	Spousal Union
β_1: Accounts Plus Linking	0.116***	-0.117***	0.112**	0.145***	0.029	0.058**	0.082***	0.063**
	(0.037)	(0.036)	(0.048)	(0.042)	(0.020)	(0.027)	(0.029)	(0.026)
β_2: Accounts Basic Linking	0.018	-0.019	-0.006	0.043	0.016	0.010	0.034	0.020
	(0.032)	(0.032)	(0.056)	(0.040)	(0.017)	(0.022)	(0.028)	(0.027)
β_3: Accounts Plus	-0.005	0.003	0.031	0.078	0.013	0.027	-0.005	-0.008
	(0.032)	(0.032)	(0.058)	(0.047)	(0.024)	(0.028)	(0.034)	(0.030)
β_4: Control Group	0.031	-0.032	0.077	0.066*	0.008	0.017	0.020	0.019
	(0.028)	(0.028)	(0.047)	(0.038)	(0.017)	(0.020)	(0.025)	(0.022)
P-values from F-Tests								
$\beta_1 = \beta_4$	0.007***	0.006***	0.359	0.037**	0.208	0.062*	0.014**	0.049**
$\beta_2 = \beta_4$	0.567	0.586	0.062*	0.456	0.527	0.646	0.517	0.984
$\beta_3 = \beta_4$	0.162	0.177	0.377	0.798	0.823	0.701	0.400	0.282
$\beta_1 = \beta_3$	0.001***	0.001***	0.104	0.181	0.526	0.306	0.014**	0.020**
Joint Test: Linking=0	0.003***	0.003***	0.266	0.218	0.488	0.525	0.021**	0.045**
Joint Test: Accounts Plus=0	0.027**	0.025**	0.039**	0.008***	0.618	0.066*	0.214	0.237
Joint Test: All Coeffs.=0	0.010***	0.009***	0.038**	0.012**	0.619	0.215	0.042**	0.098*
Accounts Basic Mean	0.287	0.713	0.328	0.107	0.052	0.083	0.747	0.851
N	4167	4167	4077	4077	4179	3957	4174	3952

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. All variables denominated in Indian Rupees are topcoded at the 99th percentile. The spousal union variable is equal to one if either the husband or the wife reports the wife worked for MGNREGS since May 1, 2015. MGNREGS administrative data runs from May 1, 2015 - September 10, 2016. Administrative bank account data runs through May 1, 2015 - August 31, 2016.

Table 5: Impact of Treatments on Labor Market Engagement and Earnings in the Private Sector

		Earnings		Performed Work Compensated in:	
	(1) Months Worked	(2) Level	(3) Hyper-sine	(4) Cash	(5) Bank Account
β_1: Accounts Plus Linking	0.817**	3287.794*	0.895**	0.078***	0.004
	(0.355)	(1977.888)	(0.362)	(0.029)	(0.005)
β_2: Accounts Basic Linking	0.127	-922.471	0.176	0.050	0.002
	(0.322)	(1896.770)	(0.351)	(0.032)	(0.005)
β_3: Accounts Plus	0.251	-184.121	-0.062	-0.008	-0.004
	(0.368)	(1757.765)	(0.372)	(0.037)	(0.004)
β_4: Control Group	0.193	-169.786	0.090	0.026	0.003
	(0.326)	(1730.792)	(0.321)	(0.026)	(0.004)
P-values from F-Tests					
$\beta_1 = \beta_4$	0.026**	0.024**	0.008***	0.045**	0.950
$\beta_2 = \beta_4$	0.763	0.527	0.741	0.332	0.686
$\beta_3 = \beta_4$	0.820	0.991	0.600	0.296	0.054*
$\beta_1 = \beta_3$	0.069*	0.037**	0.008***	0.020**	0.109
Joint Test: Linking=0	0.169	0.095*	0.025**	0.014**	0.261
Joint Test: Accounts Plus=0	0.027**	0.039**	0.084*	0.691	0.565
Joint Test: All Coeffs.=0	0.086*	0.128	0.046**	0.038**	0.273
Accounts Basic Mean	4.598	13479.856	6.624	0.683	0.007
N	4131	3719	3719	4174	4174

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. Level values of earnings are topcoded at the 99th percentile.

Table 6: Heterogeneity in Linking Effects with Respect to Social Constraints

	MGNREGS Work		Other Work				Earnings	
	(1) Own Report	(2) Spousal Union	(3) Own Report	(4) Spousal Union	(5) Occupation Worker	(6) Months Worked	(7) Level (Rs 000s)	(8) Hyper-sine
α_1: Accts Plus Linking × High HH Const.	0.092**	0.115*	0.155***	0.099**	-0.064	1.325**	10.797***	1.940***
	(0.041)	(0.060)	(0.051)	(0.046)	(0.058)	(0.596)	(3.373)	(0.601)
α_2: Accts Plus Linking × Medium HH Const.	0.081	0.101*	0.079	0.066	-0.005	-0.129	1.580	0.718
	(0.049)	(0.057)	(0.060)	(0.049)	(0.056)	(0.580)	(3.706)	(0.712)
α_5: Accts Plus Linking	-0.043	-0.045	0.002	0.004	0.112**	-0.034	-0.980	0.030
	(0.037)	(0.051)	(0.046)	(0.046)	(0.049)	(0.450)	(2.721)	(0.473)
P-values from F-Tests								
$\alpha_1 = \alpha_2 = 0$	0.092*	0.148	0.011**	0.104	0.469	0.008***	0.002***	0.005***
Accounts Plus Mean	0.069	0.110	0.719	0.801	0.239	4.455	12.572	6.261
N	1217	1186	1216	1185	1217	1205	1085	1085

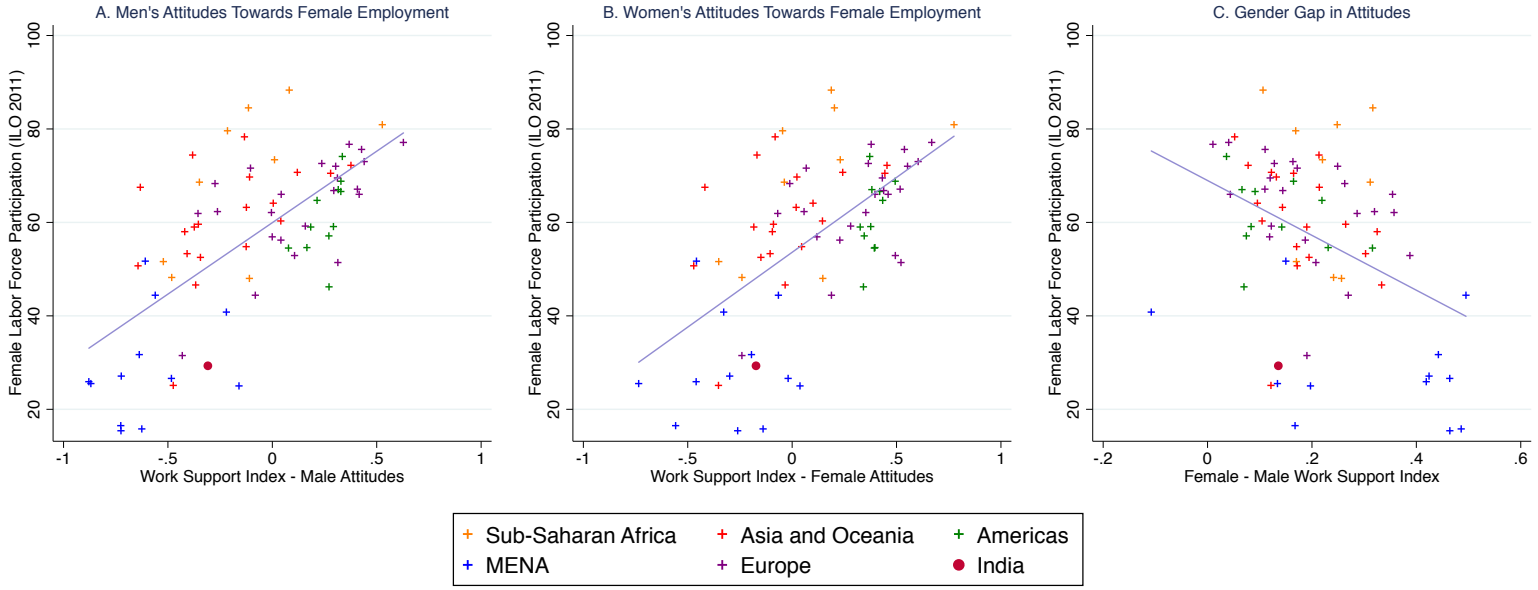
Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. Sample is limited to women in Accounts Plus Linking and Accounts Plus communities. To measure household constraints we use the Control group and predetermined characteristics in Table 1 to predict the probability that a husband agrees with the statement: women should be able to work for MGNREGS whenever they want to. A woman faces low household constraints if her husband's predicted probability is in the top tertile. Medium household constraints identify the middle tertile, and high household constraints identify the bottom tertile.

Table 7: Impact of Treatments on Female Economic Engagement and Mobility

	Makes Purchases Index:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Purchases	Own Money	Others' Money	Women Can Go to CSP Without Men[a]	Mobility Index: Past Year	Self-Reported Decision-Making Index
β_1: Accounts Plus Linking	0.079	0.113**	-0.072**	0.074*	0.063	0.021
	(0.048)	(0.052)	(0.032)	(0.041)	(0.042)	(0.051)
β_2: Accounts Basic Linking	-0.032	-0.029	-0.007	-0.017	0.001	0.002
	(0.060)	(0.065)	(0.038)	(0.037)	(0.042)	(0.056)
β_3: Accounts Plus	-0.060	-0.062	-0.004	0.075*	0.015	-0.002
	(0.050)	(0.052)	(0.035)	(0.044)	(0.040)	(0.056)
β_4: Control Group	-0.004	-0.004	-0.004		-0.039	0.021
	(0.053)	(0.056)	(0.033)		(0.036)	(0.050)
P-values from F-Tests						
$\beta_1 = \beta_4$	0.056*	0.009***	0.022**		0.002***	0.991
$\beta_2 = \beta_4$	0.614	0.653	0.950		0.224	0.694
$\beta_3 = \beta_4$	0.231	0.206	0.992		0.094*	0.664
$\beta_1 = \beta_3$	0.002***	0.000***	0.026**	0.986	0.189	0.636
Joint Test: Linking=0	0.009***	0.000***	0.078*	0.905	0.419	0.892
Joint Test: Accounts Plus=0	0.078*	0.034**	0.241	0.005***	0.255	0.933
Joint Test: All Coeffs.=0	0.039**	0.003***	0.091*	0.009***	0.034**	0.981
Accounts Basic Mean	0.032	0.023	0.025	0.232	-0.004	-0.013
N	4179	4179	4179	2085	4053	3740

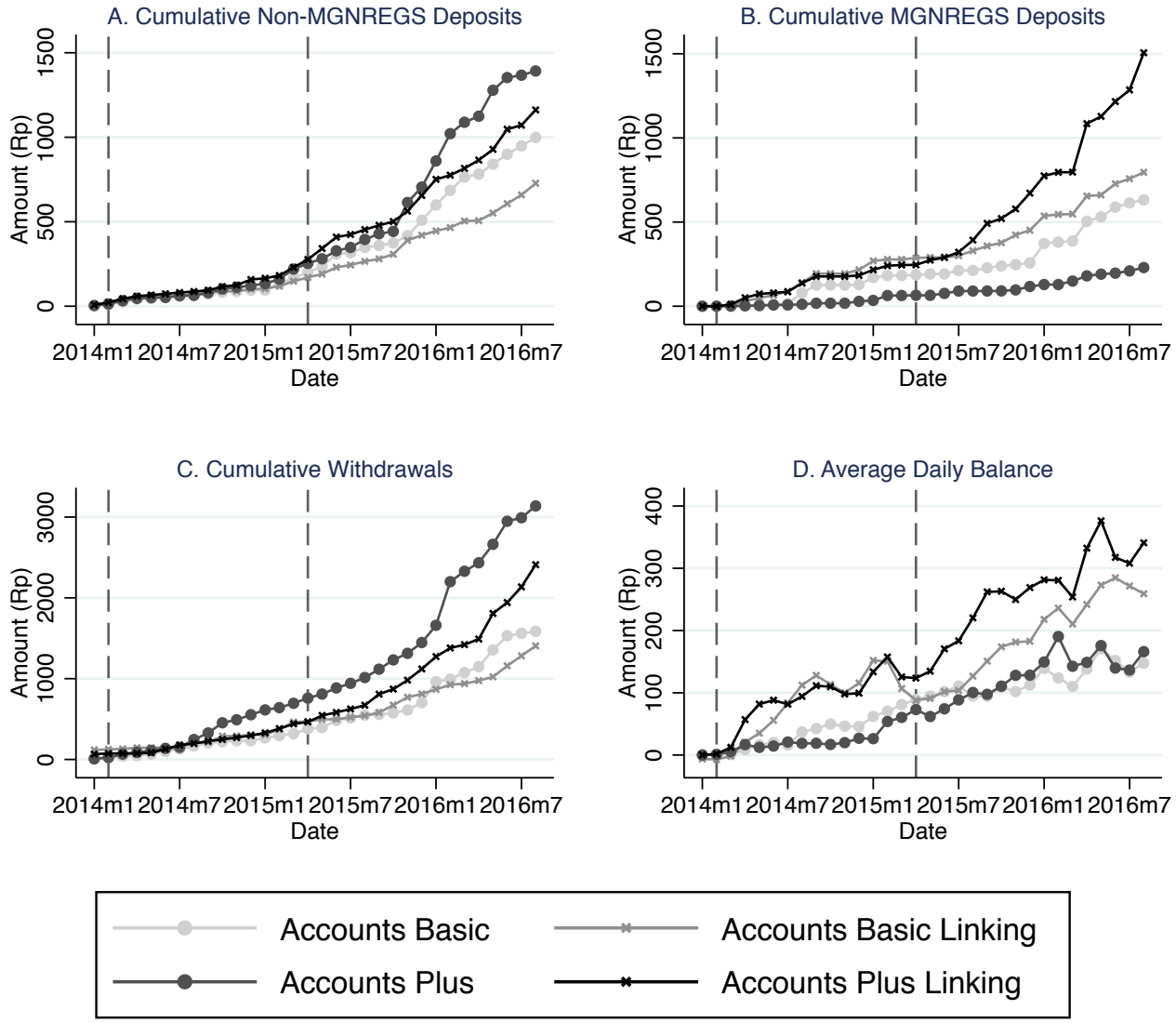
Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. [a]This question was only asked of women with CSP accounts, so the sample is limited to women in the treatment groups, where rates of CSP account ownership do not significantly differ across groups.

Figure 1: Relationship Between Gender-Specific Support for Women's Work and FLFP by Country



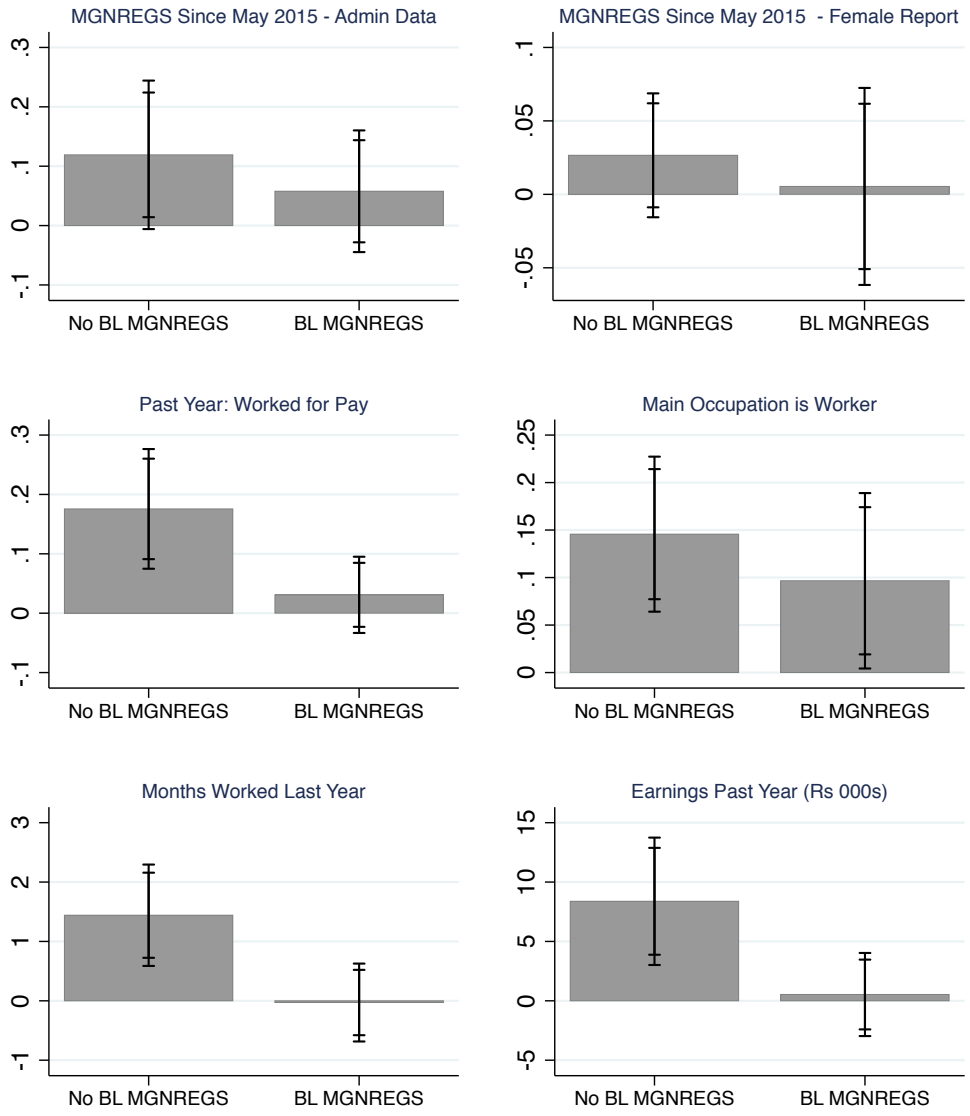
Notes: The work support index uses data from 2010-2014 World Values Surveys. The index aggregates a dummy variable equal to one if the respondent agrees than men should have more rights to jobs than women when jobs are scarce, a dummy variable equal to one if the respondent agrees that men make better political leaders than women, a dummy variable equal to one if the respondent agrees that men make better business executives than women, and a dummy variable equal to one if the respondent agrees that being a housewife is just as fulfilling as working for pay. These variables are standardized (pooling across countries and years) and then averaged by country. The index is then multiplied by -1 so higher values correspond to more support for female work. We then calculate country-wide averages by gender and correlate them with data from the International Labor Organization's modeled estimate of female labor force participation among women aged 15-64 in 2011.

Figure 2: Administrative Data - Use of Project Accounts Over Time (Excludes Control Group)



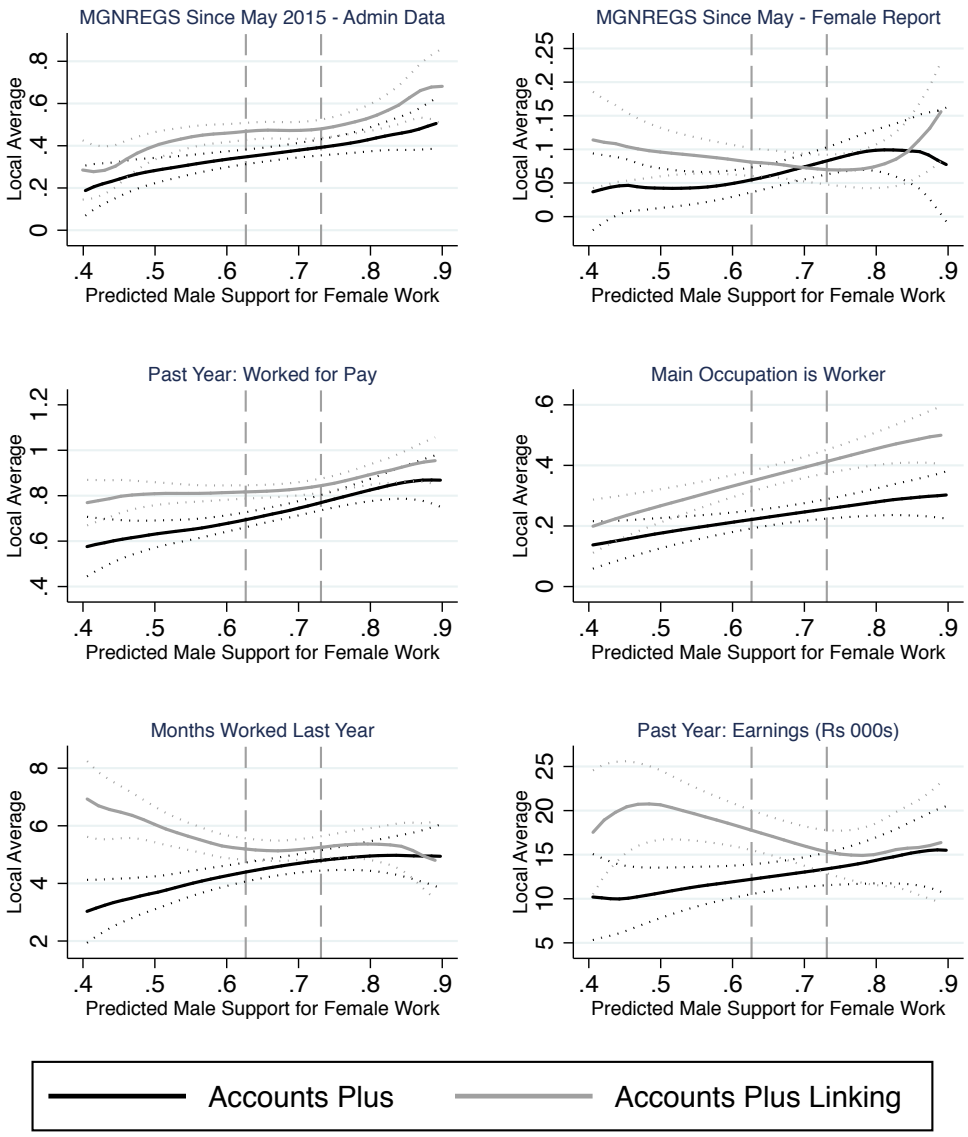
Notes: Administrative bank account data. All non-account openers are coded as having zero values for all measures. All outcomes are top-coded at the 99th percentile on a monthly basis.

Figure 3: Effects of Linking (Conditional on Accounts Plus) by Baseline MGNREGS Participation



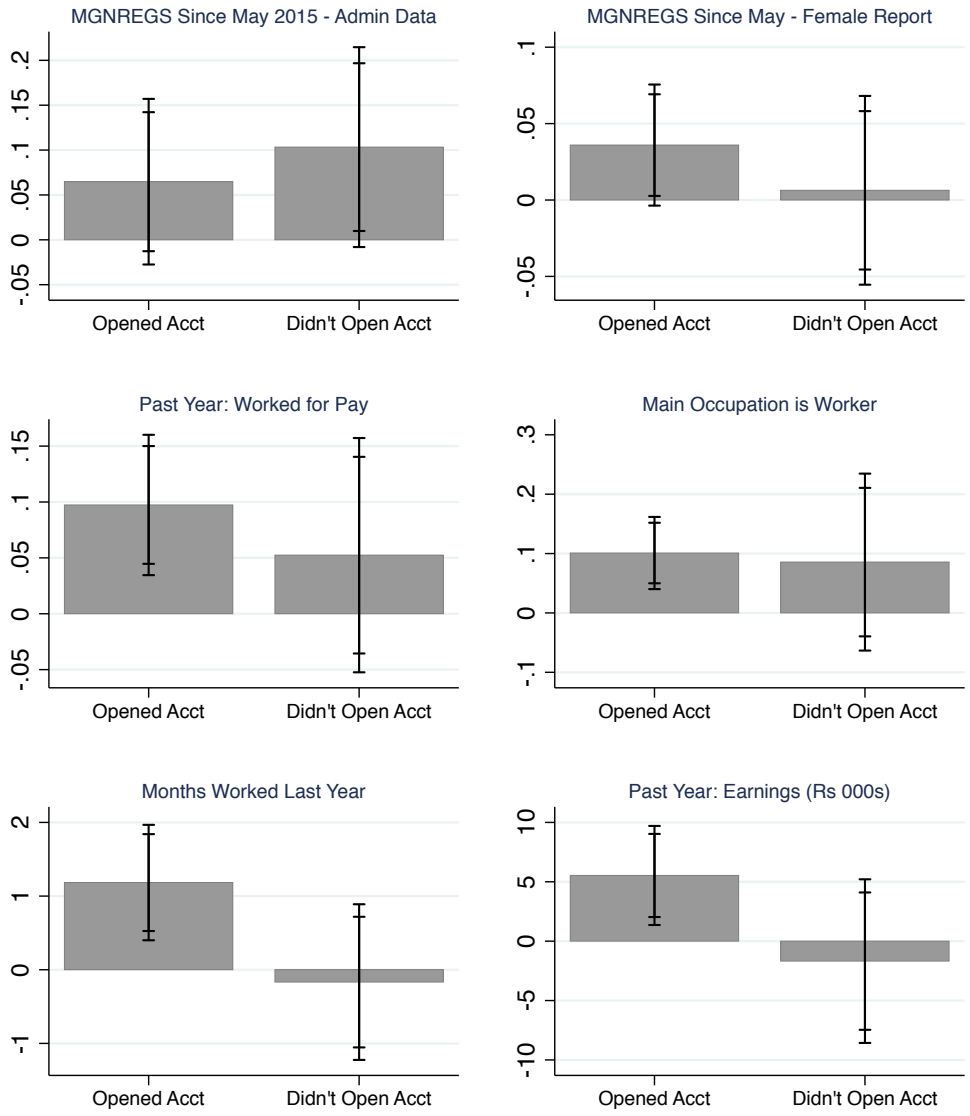
Notes: This figure graphs treatment effects of Accounts Plus Linking relative to Accounts Plus by whether or not a women reports she had ever worked for MGNREGS at baseline. Whiskers give 90 and 95 percent confidence intervals on point estimates. Robust standard errors are clustered at the gram panchayat level, and all regressions include controls for strata, district, survey month, and all predetermined variables listed on Table 1. Earnings are top coded at the 99th percentile.

Figure 4: Effect of Linking (under Accounts Plus) by Predicted Counterfactual Male Support for Female MGNREGS Work



Notes: In order to impute predicted male support for female work to all women we run a probit regression where the outcome is "male states women should be able to work for MGNREGS whenever they want to" on all predetermined characteristics in Table 1. This regression is only run in the control group, but the probability is predicted for all women. The figures plot local linear regression lines (plus 90 percent confidence intervals), which are trimmed at the first and 99th percentiles of work probability. Vertical dashed lines demarcate tertiles of predicted support for female work.

Figure 5: Impact of Linking + Fin. Lit vs. Other Treatments by Complier Status



Notes: This figure graphs treatment effects for linking plus training relative to accounts plus training by whether or not a women opened a bank account during a project account-opening camp. Whiskers give 90 and 95 percent confidence intervals on point estimates. Robust standard errors are clustered at the gram panchayat level, and all regressions include controls for strata, district, survey month, and all predetermined variables listed on Table 1. Earnings are top coded at the 99th percentile.

Appendix Tables and Figures

Table A1: Impact of Treatments on Daily Wages

	Female Wages			Male Wages		
	(1)	(2)	(3)	(4)	(5)	(6)
	Farm Labor	Non-Farm Labor	MGNREGS	Farm Labor	Non-Farm Labor	MGNREGS
β_1: Accounts Plus Linking	4.836	0.141	-15.079	1.381	-8.749	-2.501
	(8.846)	(11.140)	(12.630)	(8.425)	(10.165)	(7.743)
β_2: Accounts Basic Linking	-7.882	-5.546	-17.246	-11.848	-13.553	-20.979***
	(9.526)	(13.220)	(12.913)	(7.801)	(10.356)	(7.408)
β_3: Accounts Plus	-2.613	-1.833	-0.333	-1.876	0.575	-2.349
	(8.678)	(12.314)	(11.235)	(6.237)	(12.195)	(8.305)
β_4: Control Group	-5.928	-15.486	-6.606	-6.532	-7.865	-2.302
	(8.404)	(12.245)	(11.900)	(6.097)	(9.286)	(7.150)
P-values from F-Tests						
$\beta_1 = \beta_4$	0.121	0.114	0.485	0.326	0.917	0.980
$\beta_2 = \beta_4$	0.787	0.359	0.279	0.482	0.507	0.024**
$\beta_3 = \beta_4$	0.605	0.314	0.509	0.419	0.462	0.995
$\beta_1 = \beta_3$	0.336	0.860	0.248	0.695	0.439	0.987
Joint Test: Linking=0	0.468	0.893	0.236	0.270	0.301	0.020**
Joint Test: Accounts Plus=0	0.341	0.866	0.983	0.334	0.901	0.118
Joint Test: All Coeffs.=0	0.565	0.583	0.552	0.487	0.656	0.074*
Accounts Basic Mean	177.982	191.400	157.867	186.449	227.064	183.545
N	2192	457	400	1932	1686	1155

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10. Sample limited to individuals who report working for specified activity and earning a non-zero daily or weekly wage. Weekly wages converted to daily by assuming 6 working days per week. All wages are topcoded at the 99th percentile. Wages recorded for work in past year for farm and non-farm labor, in past two years for NREGA.

Table A2: Balance on Predetermined Demographic Characteristics - Bank Administrative Data Subsample

	(1) Accounts Basic Mean	(2) Accounts Basic Linking	(3) Accounts Plus	(4) Accounts Plus Linking	(5) P-Value: Joint Test	(6) N
Panel A: Individual Characteristics of Eligible Women						
Age	39.808	0.413	0.079	-0.676	0.827	1993
Years Education	0.904	-0.086	-0.265*	-0.014	0.272	1923
Can Read or Write	0.113	0.001	-0.026	0.002	0.535	1980
Had No Children (at Baseline)	0.018	0.000	0.006	0.001	0.913	1980
Age Had First Child (at Baseline)	19.086	0.232	0.136	-0.176	0.456	1940
Ever Worked for MGNREGS (at Baseline)	0.678	-0.063	0.018	0.002	0.592	1942
Worked for MGNREGS in Past Year (at Baseline)	0.160	0.016	0.046	0.043	0.730	1756
In MGNREGS MIS in Past Year (at Baseline)	0.741	-0.063	0.031	0.053	0.297	1863
Panel B: Individual Characteristics of Husbands						
Interviewed at Midline	0.931	0.025	0.019	0.018	0.526	1993
Age	44.182	0.858	0.041	-0.782	0.626	1950
Years Education	4.318	-0.326	-0.637	0.052	0.280	1931
Can Read or Write	0.568	-0.058	-0.057	-0.024	0.543	1885
Ever Worked for MGNREGS (at Baseline)	0.958	-0.014	0.004	0.016	0.196	1921
Worked for MGNREGS in Past Year (at Baseline)	0.303	0.019	0.001	0.020	0.983	1706
In MGNREGS MIS in Past Year (at Baseline)	0.793	-0.044	-0.010	0.017	0.695	1864
Panel C: Household/Couple Characteristics						
Male-Female Age Gap	4.697	0.164	-0.184	-0.364	0.604	1950
Male-Female Education Gap	3.469	-0.300	-0.402	-0.002	0.612	1865
Hindu	0.967	-0.006	-0.058	0.022	0.165	1983
Scheduled Caste or Tribe	0.381	0.011	0.030	0.044	0.946	1877
Other Backward Caste	0.533	0.017	-0.032	-0.033	0.831	1877
Number Household Members on Job Card	3.927	-0.243	0.271	-0.023	0.527	1993
Panel D: GP Characteristics						
Number Eligible Women in GP	45.778	2.354	5.096	7.310	0.968	1993
Total GP Population	2485.816	1217.332*	915.866**	478.515	0.049**	1993
Fraction GP Population Female	0.464	-0.001	0.006	0.005	0.488	1993
Fraction GP Population SC/ST	0.314	-0.012	-0.007	0.055	0.852	1993
Fraction Female GP Population Literate	0.396	0.010	-0.031	-0.018	0.439	1993
Fraction Male GP Population Literate	0.657	-0.026	-0.051	-0.034	0.428	1993
Female Workers / Female GP Population	0.350	-0.066	-0.016	0.016	0.308	1993
Male Workers / Male GP Population	0.523	-0.005	0.003	-0.006	0.868	1993

Notes: Each row is a separate regression. The first column gives the mean among the Accounts Basic group, columns 2-5 give regression coefficients. Robust standard errors clustered at the GP level are omitted from the table for legibility. Column 6 gives the p-value from a test that all treatment coefficients are jointly equal to zero. Sample limited to non-Control GPs served by the bank willing to share administrative data with the research team. The sample also drops individuals who did not provide consent to share bank administrative data. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10.

Table A3: Impact of Treatments on Paid Work by Type

	(1) Agriculture Own Land	(2) Agriculture Leased Land	(3) Casual Farm Labor	(4) Casual Non-Farm Labor	(5) Animal Husbandry	(6) Other Work
β_1: Accounts Plus Linking	0.056	0.018	0.063**	0.033	0.016	0.022
	(0.040)	(0.027)	(0.032)	(0.023)	(0.031)	(0.019)
β_2: Accounts Basic Linking	-0.024	-0.013	0.076**	0.011	-0.044	-0.031
	(0.034)	(0.023)	(0.035)	(0.023)	(0.028)	(0.019)
β_3: Accounts Plus	0.009	-0.014	-0.014	-0.013	-0.018	-0.006
	(0.045)	(0.028)	(0.039)	(0.024)	(0.030)	(0.019)
β_4: Control Group	-0.007	-0.010	0.042	0.020	-0.030	0.017
	(0.034)	(0.022)	(0.030)	(0.023)	(0.026)	(0.019)
P-values from F-Tests						
$\beta_1 = \beta_4$	0.055*	0.252	0.410	0.520	0.074*	0.774
$\beta_2 = \beta_4$	0.490	0.861	0.166	0.689	0.505	0.005***
$\beta_3 = \beta_4$	0.631	0.885	0.089*	0.119	0.598	0.120
$\beta_1 = \beta_3$	0.253	0.301	0.039**	0.057*	0.265	0.130
Joint Test: Linking=0	0.421	0.505	0.009***	0.152	0.147	0.086*
Joint Test: Accounts Plus=0	0.041**	0.452	0.815	0.527	0.065*	0.014**
Joint Test: All Coeffs.=0	0.158	0.783	0.046**	0.309	0.162	0.017**
Accounts Basic Mean	0.233	0.136	0.557	0.131	0.139	0.079
N	4179	4178	4176	4178	4179	4179

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table A4: Impact of Treatments on Household Labor Supply Outside MGNREGS - Monthly Lookback Period

	Eligible Women		Husbands		Other Female Members		Other Male Members	
	(1) Any Paid Work Last Month	(2) Earning Last Month	(3) Any Paid Work Last Month	(4) Earning Last Month	(5) Number Doing Paid Work Last Month	(6) Earning Last Month	(7) Number Doing Paid Work Last Month	(8) Earning Last Month
β_1: Accounts Plus Linking	0.093***	90.190*	0.044	56.268	0.019	-25.782	0.095**	414.312***
	(0.029)	(50.635)	(0.028)	(108.214)	(0.024)	(30.736)	(0.040)	(129.131)
β_2: Accounts Basic Linking	0.046	64.654	0.045	56.988	0.017	2.812	0.073	250.280*
	(0.034)	(55.227)	(0.034)	(120.202)	(0.029)	(39.414)	(0.046)	(146.991)
β_3: Accounts Plus	0.050	19.344	0.033	97.480	0.028	-5.513	0.052	394.851**
	(0.036)	(50.135)	(0.030)	(115.846)	(0.022)	(30.294)	(0.044)	(160.165)
β_4: Control Group	0.031	17.700	0.027	-4.548	0.029	-14.213	0.033	146.222
	(0.029)	(49.563)	(0.027)	(107.213)	(0.019)	(27.812)	(0.038)	(118.893)
P-values from F-Tests								
$\beta_1 = \beta_4$	0.008***	0.091*	0.506	0.575	0.626	0.618	0.053*	0.030**
$\beta_2 = \beta_4$	0.579	0.331	0.564	0.584	0.584	0.544	0.263	0.394
$\beta_3 = \beta_4$	0.554	0.970	0.823	0.331	0.973	0.727	0.607	0.085*
$\beta_1 = \beta_3$	0.201	0.127	0.726	0.722	0.695	0.457	0.270	0.901
Joint Test: Linking=0	0.186	0.177	0.394	0.849	0.764	0.741	0.155	0.235
Joint Test: Accounts Plus=0	0.116	0.824	0.544	0.702	0.443	0.675	0.422	0.027**
Joint Test: All Coeffs.=0	0.013**	0.306	0.542	0.861	0.571	0.825	0.135	0.013**
Accounts Basic Mean	0.230	306.291	0.437	1143.029	0.101	128.651	0.334	782.321
N	4089	4071	4003	4082	4176	4166	4140	4103

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. All variables in Indian Rupees are topcoded at the 99th percentile. Monthly work was reported by the husband of the eligible woman, unless the husband was unavilable for interview.

Table A5: Spillover Effects on Bank Account Use

	(1) Husband Has Bank Account	(2) Number Accounts Owned by Husband	(3) Number Accounts Owned by Other Male Members	(4) Number Accounts Owned by Other Female Members	(5) Total Number Accounts	(6) Bank Balance: Household
β_1: Accounts Plus Linking	0.025*	0.102	0.178**	0.172***	0.552***	147.576
	(0.014)	(0.094)	(0.079)	(0.056)	(0.193)	(331.069)
β_2: Accounts Basic Linking	0.006	0.019	0.067	0.131**	0.221	-68.031
	(0.020)	(0.073)	(0.079)	(0.061)	(0.172)	(321.243)
β_3: Accounts Plus	0.012	0.113	0.058	0.207***	0.452***	236.520
	(0.015)	(0.087)	(0.070)	(0.066)	(0.158)	(290.271)
β_4: Control Group	0.015	0.018	0.051	0.027	-0.409***	-58.594
	(0.016)	(0.069)	(0.065)	(0.054)	(0.150)	(254.972)
P-values from F-Tests						
$\beta_1 = \beta_4$	0.479	0.329	0.041**	0.002***	0.000***	0.475
$\beta_2 = \beta_4$	0.639	0.992	0.792	0.032**	0.000***	0.969
$\beta_3 = \beta_4$	0.838	0.206	0.912	0.005***	0.000***	0.251
$\beta_1 = \beta_3$	0.355	0.912	0.080*	0.583	0.549	0.788
Joint Test: Linking=0	0.594	0.960	0.163	0.087*	0.396	0.942
Joint Test: Accounts Plus=0	0.372	0.282	0.251	0.006***	0.004***	0.591
Joint Test: All Coeffs.=0	0.469	0.573	0.210	0.001***	0.000***	0.795
Accounts Basic Mean	0.928	1.385	0.856	0.576	3.686	1442.374
N	3957	3957	4179	4179	4179	3369

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. All variables constructed using male reports whenever available. All variables denominated in Indian Rupees are topcoded at the 99th percentile. Bank administrative data runs through February 2016.

Table A6: Spillover Effects on Work for MGNREGS (Since May 2015)

	MIS Admin Data			Survey Data			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Husband	Number Other Males	Number Other Females	Husband: Own Report	Husband: Spousal Union	Number Other Men	Number Other Women
β_1: Accounts Plus Linking	0.127**	0.171***	0.066***	0.049*	0.056*	0.012	0.019**
	(0.052)	(0.044)	(0.024)	(0.026)	(0.031)	(0.016)	(0.009)
β_2: Accounts Basic Linking	0.016	0.058	0.041	0.007	0.007	0.006	0.014
	(0.057)	(0.045)	(0.027)	(0.027)	(0.032)	(0.015)	(0.008)
β_3: Accounts Plus	0.030	0.002	0.020	0.027	0.036	0.032	0.021*
	(0.060)	(0.037)	(0.021)	(0.027)	(0.034)	(0.021)	(0.012)
β_4: Control Group	0.036	0.076**	0.018	0.012	0.024	0.006	0.013*
	(0.049)	(0.035)	(0.018)	(0.024)	(0.030)	(0.015)	(0.008)
P-values from F-Tests							
$\beta_1 = \beta_4$	0.026**	0.015**	0.023**	0.090*	0.194	0.631	0.543
$\beta_2 = \beta_4$	0.638	0.660	0.358	0.797	0.425	0.986	0.945
$\beta_3 = \beta_4$	0.920	0.030**	0.884	0.529	0.696	0.146	0.461
$\beta_1 = \beta_3$	0.063*	0.000***	0.042**	0.393	0.510	0.327	0.871
Joint Test: Linking=0	0.170	0.000***	0.041**	0.684	0.790	0.596	0.263
Joint Test: Accounts Plus=0	0.054*	0.077*	0.433	0.133	0.096*	0.253	0.136
Joint Test: All Coeffs.=0	0.056*	0.001***	0.065*	0.288	0.289	0.556	0.192
Accounts Basic Mean	0.361	0.142	0.060	0.097	0.151	0.042	0.012
N	4080	4088	4088	3947	3947	4094	4153

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. The spousal union variable is equal to one if either the husband or the wife reports the wife worked for MGNREGS since May 1, 2015. Administrative MGNREGS data runs through September 10, 2016.

Table A7: Spillover Effects on Household Work Outside MGNREGS

	Husband's Labor Supply			Other Householders	
	(1)	(2)	(3)	(4)	(5)
	Any Paid Work	Earnings: Level	Earnings: Hypersine	Other Males	Other Females
β_1: Accounts Plus Linking	0.009	4181.602*	-0.031	0.057	0.034
	(0.014)	(2518.923)	(0.206)	(0.053)	(0.035)
β_2: Accounts Basic Linking	-0.004	5994.027**	0.062	0.070	0.059
	(0.013)	(2989.519)	(0.184)	(0.060)	(0.037)
β_3: Accounts Plus	0.015	4899.055**	0.195	0.068	-0.010
	(0.013)	(2456.083)	(0.175)	(0.060)	(0.030)
β_4: Control Group	-0.001	4704.064*	0.021	0.020	0.055*
	(0.013)	(2405.067)	(0.178)	(0.050)	(0.029)
P-values from F-Tests					
$\beta_1 = \beta_4$	0.425	0.828	0.790	0.334	0.479
$\beta_2 = \beta_4$	0.761	0.646	0.790	0.280	0.893
$\beta_3 = \beta_4$	0.110	0.935	0.253	0.305	0.021**
$\beta_1 = \beta_3$	0.652	0.780	0.253	0.816	0.198
Joint Test: Linking=0	0.864	0.130	0.500	0.480	0.131
Joint Test: Accounts Plus=0	0.320	0.124	0.495	0.497	0.754
Joint Test: All Coeffs.=0	0.394	0.199	0.709	0.560	0.145
Accounts Basic Mean	0.955	30553.369	9.969	0.607	0.220
N	3956	3877	3877	4179	4179

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. Earnings measured in levels are topcoded at the 99th percentile.

Table A8: Predicting Male Support for Female Work in the Control Group

	(1)
Wife's Age	0.004
	(0.005)
Wife's Years Education	-0.097*
	(0.056)
Wife Can Read or Write	0.237
	(0.183)
Husband-Wife Education Gap	-0.050
	(0.053)
Hindu	-0.146
	(0.185)
Scheduled Caste/Tribe	-0.078
	(0.170)
Other Backwards Caste	-0.150
	(0.160)
Household Size on Job Card at Baseline	-0.040*
	(0.023)
Had No Children	-0.110
	(0.352)
Age Wife Had First Child	-0.007
	(0.011)
Husband's Age	-0.001
	(0.004)
Husband's Years Education	0.022
	(0.054)
Husband Can Read or Write	-0.129
	(0.099)
Wife Ever Worked for MGNREGS	0.307***
	(0.096)
Husband Ever Worked for MGNREGS	0.370**
	(0.181)
Wife Worked for MGNREGS in Past Year	-0.085
	(0.147)
Husband Worked for MGNREGS in Past Year	0.124
	(0.109)
Pseudo R-squared	.05
N	1564

Notes: Probit regression coefficients, robust standard errors clustered at the GP level in parentheses. The outcome of interest is a dummy variable equal to one if the husband agrees that a woman should be able to work for MGNREGS whenever she wants to. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10.

Table A9: Why Does Training Matter for Linking? (Treatment Groups Only)

					Agreement Rank	
	(1)	(2)	(3)	(4)	(5)	(6)
	Has CSP Account	CSP Account Knowledge Index	Prefers Wage Deposits into Account	Rank: CSP is Safe Place to Save^{+}	CSP is Trust-worthy^{+}	CSP is Welcoming^{+}
α_1: Accounts+Linking × Training	0.025	0.045	0.070**	0.220**	0.005	-0.005
	(0.032)	(0.051)	(0.034)	(0.094)	(0.177)	(0.184)
α_2: Accounts+Linking	0.004	0.011	-0.041	-0.107	-0.102	-0.087
	(0.036)	(0.053)	(0.038)	(0.089)	(0.173)	(0.185)
α_3: Accounts × Training	0.039	-0.003	-0.010	0.105	-0.313	-0.094
	(0.028)	(0.049)	(0.040)	(0.099)	(0.200)	(0.203)
P-values from F-Tests						
$\alpha_1 + \alpha_2 = 0$	0.290	0.201	0.433	0.163	0.683	0.571
$\alpha_1 = \alpha_3 = 0$	0.244	0.679	0.115	0.031**	0.281	0.897
$\alpha_1 = \alpha_2 = \alpha_3 = 0$	0.414	0.513	0.227	0.071*	0.437	0.935
Mean (Accounts Only)	0.859	0.803	0.618	3.954	2.608	2.693
N	2504	2062	2165	2414	1957	1987

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. $^{+}$Coefficients are from ordered logit regressions.

Table A10: Impact of Treatments on Mental Health and Gender Based Violence

	(1) Depression Index	(2) Anxiety Index	(3) Violence Index-Control	(4) Violence Index-Emotional	(5) Violence Index-Physical
β_1: Accounts Plus Linking	0.027	0.046	-0.027	-0.014	0.076
	(0.049)	(0.052)	(0.049)	(0.051)	(0.056)
β_2: Accounts Basic Linking	-0.034	-0.023	-0.050	-0.011	0.009
	(0.043)	(0.057)	(0.046)	(0.057)	(0.052)
β_3: Accounts Plus	0.024	-0.002	0.002	0.025	0.036
	(0.044)	(0.048)	(0.050)	(0.051)	(0.050)
β_4: Control Group	-0.012	0.029	-0.009	-0.020	0.025
	(0.041)	(0.053)	(0.044)	(0.044)	(0.043)
P-values from F-Tests					
$\beta_1 = \beta_4$	0.412	0.747	0.651	0.894	0.321
$\beta_2 = \beta_4$	0.597	0.344	0.205	0.863	0.722
$\beta_3 = \beta_4$	0.388	0.495	0.770	0.333	0.811
$\beta_1 = \beta_3$	0.956	0.318	0.544	0.491	0.522
Joint Test: Linking=0	0.728	0.526	0.457	0.767	0.804
Joint Test: Accounts Plus=0	0.409	0.475	0.871	0.891	0.378
Joint Test: All Coeffs.=0	0.699	0.672	0.654	0.910	0.687
Accounts Basic Mean	0.065	0.036	0.031	0.000	-0.016
N	4117	4117	4169	4176	4176

Notes: Robust standard errors clustered at the GP level in parentheses. All regressions include strata, survey month, and district fixed effects, as well as controls for the individual, spousal, household, and GP-level characteristics listed in Table 1. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Figure A1: Timeline of Experimental Activities

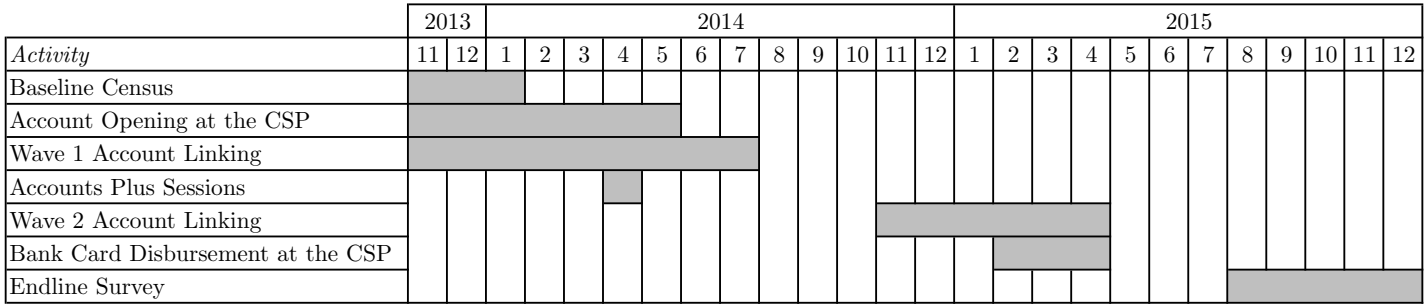
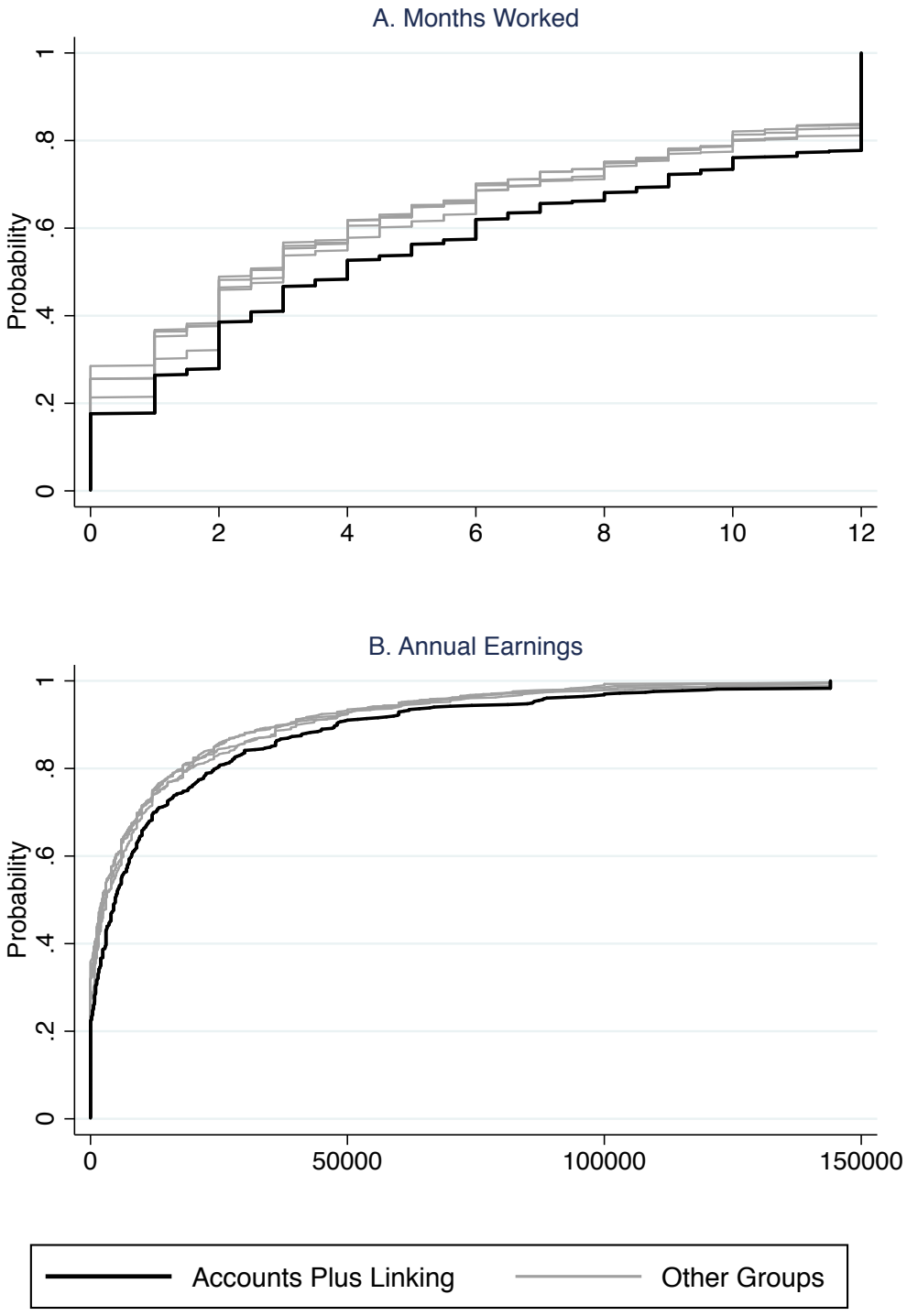
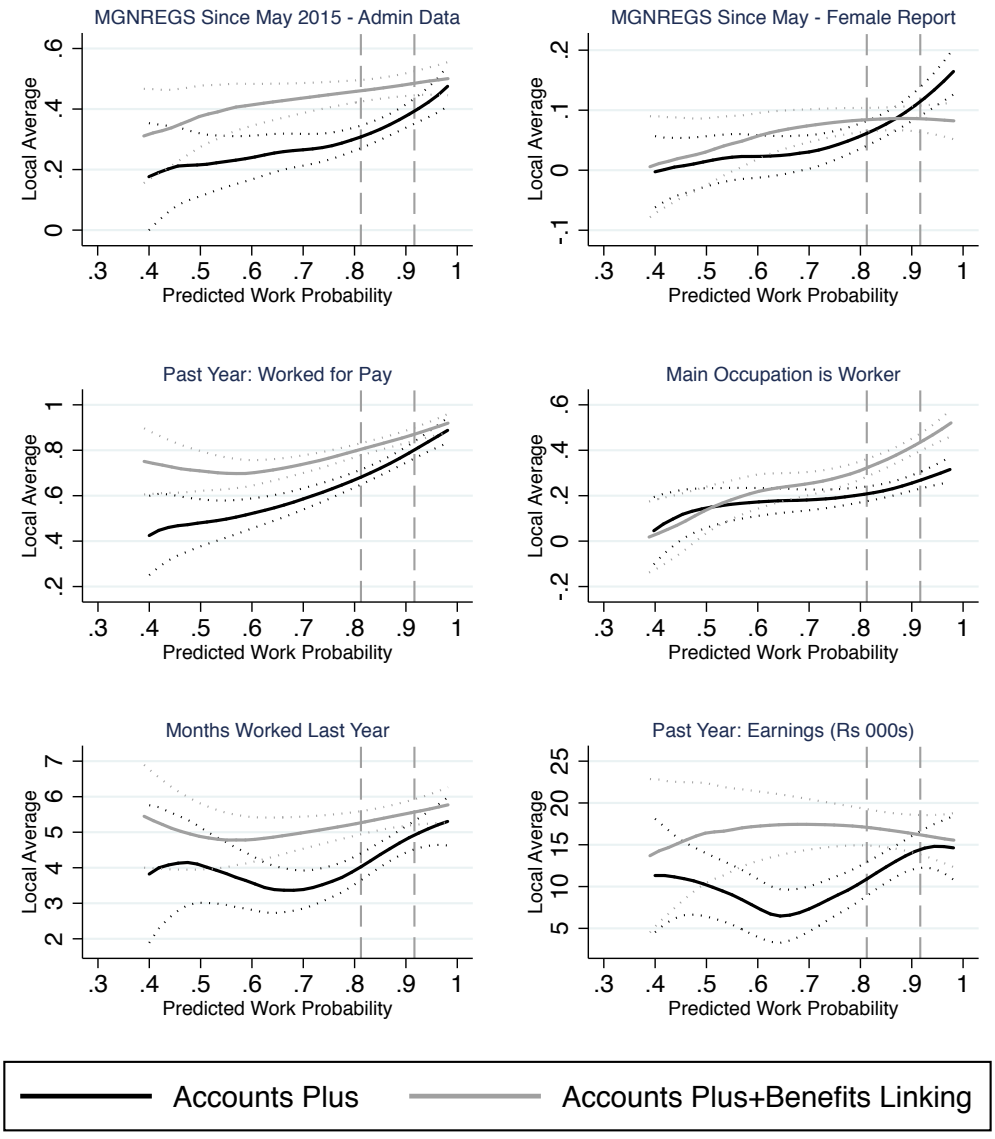


Figure A2: Distribution of Annual Months Worked and Earnings by Treatment Group



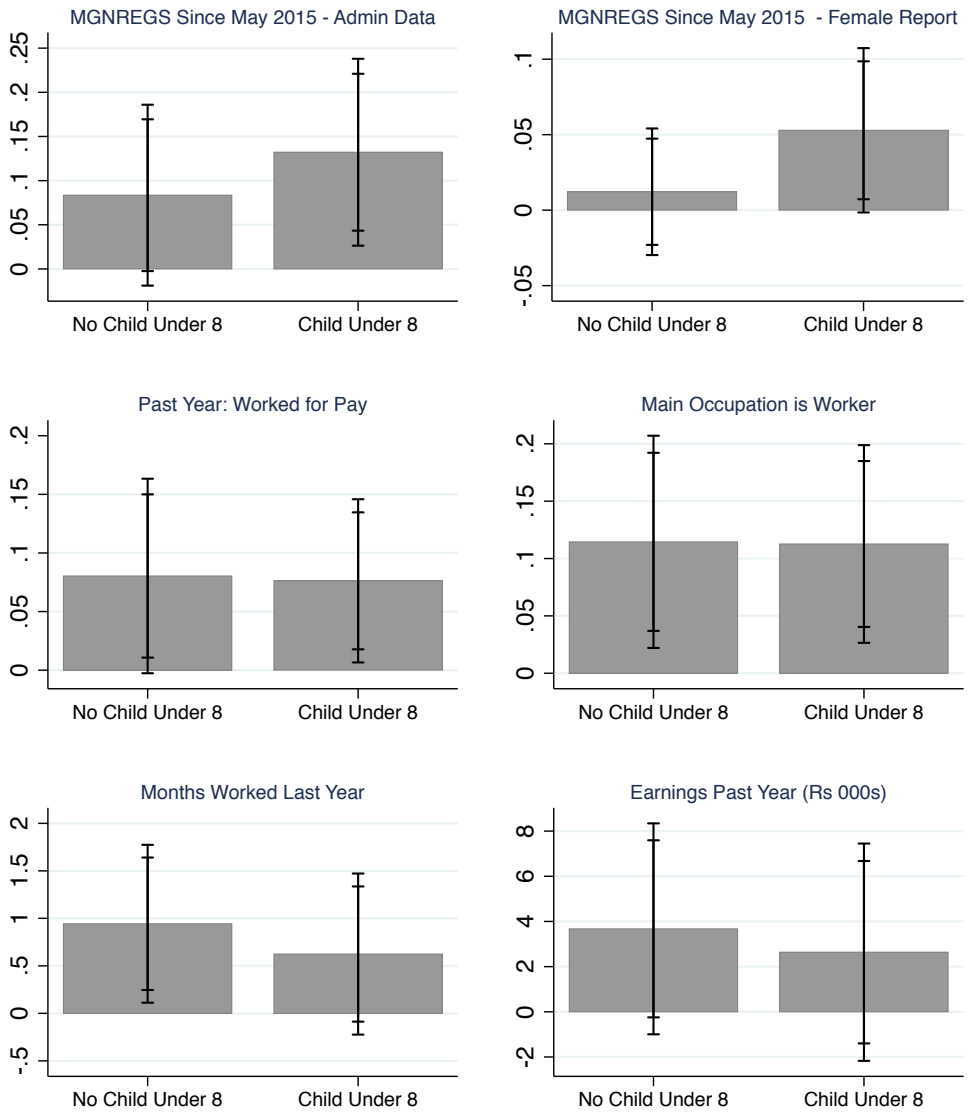
Notes: Earnings is top-coded at the 99th percentile. Months worked is the average of an upper and lower bound constructed using reports of months worked by sector.

Figure A3: Effect of Linking (under Accounts Plus) by Predicted Counterfactual Labor Force Participation



Notes: In order to impute a work probability to all women we run a probit regression where the outcome is "worked for pay in the past year or ever worked for NREGA" on all predetermined characteristics in Table 1, as well as district, strata, and survey month fixed effects. This regression is only run in the control group, but the probability is predicted for all women. The figures plot local linear regression lines (plus 90 percent confidence intervals), which are trimmed at the first and 99th percentiles of work probability. Vertical dashed lines demarcate tertiles of predicted work probability.

Figure A4: Effects of Accounts Plus Linking by Age of Youngest Child in the Household



Notes: This figure graphs treatment effects of Accounts Plus Linking relative to Accounts Basic by whether or not the household has a children under the age of eight. Whiskers give 90 and 95 percent confidence intervals on point estimates. Robust standard errors are clustered at the gram panchayat level, and all regressions include controls for strata, district, survey month, and all predetermined variables listed on Table 1. Earnings are top coded at the 99th percentile.