Female Bank Accounts Project Pre-Analysis Plan

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1   Introduction

Resource allocation is often at the core of bargaining that takes place within a household. Exogenous shifts in individual control over resources (as a result, for instance, of conditional transfer programs that allocate resources to a female household member) thus impact individual bargaining power and ex-post household consumption and investment decisions (Lundberg, Pollak, and Wales 1997; Chiappori, Fortin, and Lacroix 2002; Duflo 2003; Bobonis 2009). Can expected shifts in individual control over resources lead individuals to revise their investment decisions, since their expected benefit from the investment may have increased? If so, do these revisions impact individual labor supply decisions, savings decisions, or both? And, how does the shift in individual bargaining power affect other (non-resource based) collective decisions within the household?

These questions have natural extensions to the study of female labor force participation, savings behavior, and well-being. If women are given more control over future income or savings, will they choose to work or save more? We study these questions within the context of India's ambitious National Rural Employment Guarantee Scheme (NREGS), which entitles every rural household to 100 days of paid employment, making the program the largest welfare scheme in the world (Subbarao et al. 2013). Given its prominence, NREGS has been the focus of a growing body of empirical economic research. A central question of great policy import is how the scheme impacts the welfare of Indian women, who have very low labor force participation rates – 29 percent, compared to 80 percent for men (ILO Global Trends Report 2013) – and are paid an average of 27 percent lower market wages than their male counterparts (Dasgupta and Sudarshan 2011). While Indian policy makers have introduced a number of provisions explicitly designed to make NREGS more woman-friendly, and NREGS wage levels are relatively more attractive to women than to men, there is a real risk that the program could exacerbate intra-household gender inequalities if its pay-for-work structure systematically favors men (as opposed to, for example, cash transfers paid to women).

This risk is particularly important given the current process for paying NREGS wages to households: in response to concerns of program leakage and growing administrative costs, in 2008 the national government mandated that NREGS wages be directly deposited into beneficiary-owned bank accounts. Many NREGS households have the wages of all household members deposited into a single bank account (in our experimental context, for example, we find that 80 percent of households have all wages deposited to a single bank account). These accounts are typically owned by the male head of household. A key concern with this policy, as opposed to making cash disbursements to individuals, is that direct deposit into a household or male bank account may reduce women's control over their own labor income, which could in turn reduce women's decision-making power in the household (Browning and Chiappori 1998).

A natural alternative to the current policy would be to deposit wages into worker-specific individual bank accounts, which would allow male and female workers to retain direct control over their wages, just as they would under a cash payout system. Indian policy makers have taken note of this observation and taken steps to encourage its implementation: in December 2012, Minister for Rural Development Jairam Ramesh proposed a number of NREGS reforms meant to support women, including a proposal that all women have their NREGS wages deposited into an individual bank account in their name. Although states have yet to take action on this recommendation, there is increasing pressure on states to implement corresponding reforms.

The aim of this research is to investigate the impact of such a policy change on women's wellbeing and labor market outcomes. Although at first blush individual accounts would appear to make women indisputably better off, the impact of such a policy reform on women's welfare is ambiguous. On the one hand, non-unitary models of the household would posit that increased control over NREGS income may increase female bargaining power and consequently shift the intra-household allocation of resources towards expenditures favored by women. Then, the policy would be welfare-improving individually for the female and may be welfare-improving for other members of the household if female preferences favor investment in human capital, as has been shown in previous studies (see, for instance, Duflo 2003). These models also predict that other dimensions of women's welfare (such as mental health and self-reported well-being) should improve. On the other hand, sociological models of household decision-making suggest that directly depositing NREGS income to women could have unintended negative consequences (such as increased domestic violence or marital conflict), especially if some husbands react adversely to their wives' increased control over financial resources (Hotalling and Sugarman 1990, Molm 1997, Ashraf 2009). Such

concerns could be particularly important in rural India, where divorce is highly stigmatized and women therefore cannot exit a marriage without paying a very large social and economic cost. Existing empirical evidence also suggests that economic shocks that ostensibly improve women's bargaining positions can also have negative consequences, including increased rates of spousal violence against women (see, for instance, Luke and Munshi (2011), who exploit gender-specific differences in work on Indian tea estates to study the impact of increasing female income conditional on total household income). Evidence from the cash transfer literature finds mixed impacts of extending cash transfers to women on physical and psychological violence (Hidrobo and Fernald 2013, Bobonis et al. 2013, Angelucci 2008). An important limitation of existing evidence is that it cannot differentiate between impacts stemming from an overall increase in household income versus a change in the share of household income controlled by women. Finally, it may be that women would not take advantage of increased control over their income by working more because doing so would not be efficient from a household investment standpoint. It is possible that their husbands may have access to higher-return income-generating opportunities, as is suggested by some recent work on cash grants to entrepreneurs (de Mel et al. 2008).

Thus, theoretical and empirical ambiguities lead to uncertain predictions regarding the effect of individual control over earnings on female labor market choices. Depending on the net impact of individual bank accounts on female wellbeing, and depending on their relative returns to work, women may be encouraged to work more or less under the new policy. We use a field experiment to isolate the impact of this shift in bargaining power on female's labor force decisions, savings behavior, and well-being. In January – April 2014, 6,500 females from rural households in Madhya Pradesh were randomly assigned to one of three main treatment arms: (1) receive individual bank accounts, (2) receive bank accounts and direct transfer of their NREGS wages, or (3) receive nothing. Females who receive bank accounts are also randomized across a financial literacy cross-treatment.

This document summarizes the main tests that we intend to conduct. We do not rule out the possibility of running other tests. We will make clear in the paper which estimations are specified in this pre-analysis plan and which are not (Casey, Glennerster, and Miguel, 2012).

2 Experimental design

We use a randomized controlled trial (RCT) to evaluate our research questions. In partnership with the State Bank of India and the Central Bank of India, two public banks that operate in our study area, we assisted eligible females (eligibility is defined below) in opening individual bank accounts and setting up direct transfer of their NREGS wages. The interventions took place in February- April 2014. In conjunction with the main intervention, a random subset of women who were invited to open bank accounts were also invited to attend a short training on financial literacy and instructional training on how to use their bank account. Our sample selection and census surveys took place in September 2013- January 2014. Our follow-up survey data collection will take place from August 2015- January 2016.

2.1 Sample

2.1.1 District and Gram Panchayat selection:

We selected 199 local government units, or gram panchayats (GPs) in four districts (Gwalior, Shivpuri, Sheopur, and Morena) in Madhya Pradesh to enroll in the study. These four districts were selected because of their high levels of NREGS participation and low levels of female empowerment (as evidenced by sex ratios and male-female educational attainment). In September 2013, we conducted a survey of the community-based banking kiosks (known as an Ultra-Small Banking unit or USB) in our four districts, through which we would provide bank accounts. We found that 199 kiosks were well-functioning and these form our study sample. A USB typically serves a single GP which, therefore, is our unit of randomization. All 199 GPs were randomly assigned to one of our three treatment arms. GP randomization was stratified by whether, at the time of census, the GP had: below/ above median number of households with joint bank accounts linked to NREGS, below/ above median percentage of individual NREGS accounts, and whether the GP was located

in Sheopur district.[1]

The financial literacy cross-treatment was randomized across T1 (bank accounts) and T2 (bank accounts + DBT) GPs. Finally, then, we have the following treatment assignments:

- Control: 66 GPs

- T1 (bank accounts) and no financial literacy: 34 GPs

- T1 (bank accounts) and financial literacy: 34 GPs

- T2 (bank accounts + DBT) and no financial literacy: 32 GPs

- T2 (bank accounts + DBT) and financial literacy: 33 GPs

2.1.2 Household and eligible females selection:

The first criterion of household eligibility is that at least one household member has worked for NREGS in the past. Note that persons listed in NREGS administrative data as workers do not always self-report having worked for NREGS (we define these persons as "ghost workers"). For the purposes of this study, eligiblity requires positive NREGS work history both according to NREGS administrative data and according to self-reports. In December 2013- January 2014, we conducted a census of all households in our 199 GPs that met the first criterion (they appeared in NREGS administrative data as having worked) to establish eligibility of female household members. To be eligible, females have to (1) be married, (2) have worked for NREGS at least once or have a husband who has worked for NREGS at least once before (or both), and (3) not own an individual bank account at the time of the census survey. We found 5,909 women that met this set of criteria and all were selected for the intervention (control, bank accounts, bank accounts + direct benefit transfer, as determined by the GP-level randomization). Bank account openings occurred in February- April 2014. Before commencing our follow-up data collection in August 2015, we randomly selected 4,500 of our eligible women. 4,000 of these women are evenly weighted across treatment groups (control, T1, T2) and the additional 500 women are in the control group. [2]

2.2 Surveys and other data sources

1. Ultra-Small Banking (USB) Kiosk survey: We surveyed operators of USBs in our four study districts to determine whether the USB was operational at the time of survey and, if applicable, measures of accessibility (such as hours and location).

2. NREGA Management Information System (MIS): records of NREGA project activities, including persons worked, days worked, and wages paid are publicly available on the NREGA website.

3. Census: The census is a short survey of all household members used to determine eligibility for the intervention. Household members were surveyed regarding:

    (a) Age and marital status
    (b) NREGA work history (including last date worked for NREGA, size of last wage received, and method of payment of last wages).
    (c) Bank account ownership

4. Midline survey: The follow-up survey was administered to 4,500 females and their husbands. Respondents are surveyed regarding:

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[1]Joint bank account refers to an account that is under two persons' names (the vast majority of joint bank account owners are spouses).

[2]To simplify communication in the field, women with pre-existing individual bank accounts at the time of the census were made eligible for the financial capability building and account linking treatments.Thus, an additional 1,585 women with bank accounts were randomized across control, financial literacy intervention and the bank account linking intervention. 178 of these women had their bank accounts linked to their NREGS wages. We exclude them from analysis and from the midline data collection sample.

(a) Household labor supply decisions and other sources of income, including government transfers.

(b) Household assets.

(c) Female mobility and experience of domestic violence.

(d) Self-reported measures of female's subjective well-being.

(e) Household bank accounts, access to informal and formal finance, and experience with the formal banking sector.

5. Sarpanch survey.[3]

6. Bank administrative data: At least one of our two partner banks has committed to sharing transaction data for study respondents (including person-level data on amount and date of withdrawals and deposits).

7. 2011 Indian Census data.

8. Indian electoral data.

3 Experimental integrity

To establish experimental integrity we will compare each of the treatment groups to the control group on key baseline and arguably pre-determined outcome variables. In expectation, the mean and variance of these variables in each treatment group and the control group should not be distinguishable.

We will compare balance across both GP-level characteristics and person-level characteristics. For the balance tests, variables will be specified as below:

1. GP characteristics tests for balance:

   (a) Number of new NREGA work projects over the two years prior to census (from NREGA MIS)

   (b) Proportion of NREGA workers to GP population over the two years prior to census (from NREGA MIS and Indian Census)

   (c) Proportion of NREGA ghost workers at census: ghost worker defined as person who does not report working for NREGA, but is listed as having worked in the NREGA MIS (from census data and NREGA MIS data).

   (d) Sex ratio (from 2011 Indian Census data)

   (e) Caste composition (from 2011 Indian Census data)

   (f) Below-Poverty-Line ratio (from 2011 Indian Census data)

   (g) Median Income (from 2011 Indian Census data)

   (h) Caste of sarpanch (from Electoral data)

   (i) Gender of sarpanch (from Electoral data)

2. Person-level characteristics tests for balance:

   (a) Caste (from midline survey)

   (b) Age (from midline survey)

   (c) Level of literacy (from midline survey)

   (d) Household size (from midline survey)

   (e) Number of children over age 3 (from midline survey)

   (f) Whether the respondent has worked for NREGA prior to census (from census survey)

   (g) Difference in age between female respondent and her husband (from midline survey)

   (h) Difference in level of education between female respondent and her husband (from midline survey)

   (i) Distance to nearest banking kiosk (from banking kiosk survey)

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[3]At the time of writing, we have yet to confirm that we will require the sarpanch survey, or whether publicly available election data will suffice for our purposes. If conducted, the sarpanch survey would include questions regarding the sarpanch's basic demographics and political affiliation.

4 Regression Specification

Our main specification will evaluate the effect of the treatments- opening bank accounts, opening bank accounts and linking them to NREGS payments, and financial capability building- relative to the control and to one another:

$$Outcome_{ikj} = \alpha_0 + \alpha_1 T1_k * F_k + \alpha_2 T2_k * F_k + \alpha_3 T1_k + \alpha_4 T2_k + \mathbf{X}_{ikj} + \mathbf{M}_k + \delta_k + \gamma_k + \epsilon_{ikj}$$

In the regression above, i indexes the individual, j indexes her household, and k indexes her GP. $Outcome_{ikj}$ are the main outcome variables we will be looking in the following three main categories in which we theorize our treatments will have an effects: intra-household bargaining power, financial outcomes, and labor supply. Outcomes will also be presented at the level of the household. We will analyze the effects across our three treatment groups: $T1$ is whether the GP was assigned to the individual accounts treatment, $T2$ indicates whether we both opened individual accounts and linked accounts to NREGS payments, and F is whether a GP was assigned to receive financial capability building. Standard errors are clustered at the GP level. X_{ijk} is a vector of controls for individual i that include the variables listed above under the person-level characteristics for the tests for balance. We also include a vector of GP-level controls $\mathbf{M}_k$ which include the variables listed above under the GP characteristics for the tests for balance. δ_k are randomization strata fixed effects, γ_k are survey month fixed effects, and ϵ_{ikj} is the idiosyncratic error term.

The coefficients on each of these treatment arms signifies the following: α_3 denotes the effect of being assigned to a $T1$ GP, where we opened bank accounts; α_1 signifies the additional effect for those in $T1$ of being assigned to a GP in which we provided financial capability building; α_4 denotes the effect of being assigned to a $T2$ GP, where we opened bank accounts and linked those accounts to NREGA; α_2 denotes the additional effect for those in $T2$ of being assigned to a GP in which we provided financial capability building. We will also present results pooling the impact of the financial capability building, where β_1 denotes the effect of receiving financial capability building, for those in either $T1$ or $T2$:

$$Outcome_{ikj} = \beta_0 + \beta_1 F_k + \beta_2 T1_k + \beta_3 T2_k + \mathbf{X}_{ikj} + \mathbf{M}_k + \delta_k + \gamma_k + \epsilon_{ikj}$$

5 Heterogeneity Analysis

We expect the impact of our interventions to differ by female respondents' level of empowerment and mobility and by panchayats' level of financial services access and NREGA work access. Thus, in addition to analyzing the average effects of our interventions across treatment groups, we will present results from heterogeneity analysis along the following dimensions:

1. **Female empowerment and mobility:** Heterogeneity analysis will involve the following panchayat-level predictors: birth sex-ratios, male-female educational attainment, and basic demographics of the sarpanch (e.g. gender). Using Indian Census Data, we will construct panchayat-level sex-ratios and examine whether our treatments had differential effects in panchayats with more and less skewed sex ratios. Our measures of male-female educational attainment will come from midline data. Since demographics of the village sarpanch can also be predictors of female empowerment and mobility (see, for instance, Duflo et al. 2009), we will use data collected through our sarpanch survey or Indian electoral data to study treatment effects within sub-groups created by sarpanch demographics.

   Although we did not collect baseline data, we can use data from our control group (which was completely unaffected by our treatments) to generate predictions about levels of empowerment in our full sample. Aside from sex-ratios, other strong (and invariant) predictors of women's mobility are caste and religion (Field, Jayachandran, and Pande 2012). We will utilize mobility and household decision-making data in our control group and generate an index of empowerment (via principal component analysis, for example). Based on this index, we will then be able to generate an ordering of how empowered different caste and religion groups are. Just like with sex-ratios, we will then evaluate the impact of our interventions on more and less empowered castes/religious groups.

2. **Panchayat-level access to NREGA work and to financial services:** Heterogeneity analysis will involve the following panchayat-level predictors: number of NREGA projects and workers at baseline,

number of CSPs within the panchayat at baseline, and distance from the panchayat to the nearest bank branch at baseline. These indicators will be constructed using the NREGA MIS data and our CSP survey data.

6 Hypotheses

6.0.1 Hypothesis 1: Take-up

6.0.2 Hypothesis 2: Impact on banking activity of female household member

6.0.3 Hypothesis 3: Impact on banking activity of other household members

6.0.4 Hypothesis 4: Impact on female labor force participation

6.0.5 Hypothesis 5: Impact on other household members' labor force participation

6.0.6 Hypothesis 6: Impact on female bargaining power and mobility and on men's attitudes towards female economic empowerment

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