

Direct Benefit Transfers and Graft in the National Rural Employment Guarantee Scheme (NREGS): Pre-Analysis Plan

Erica Field, Rohini Pande, Natalia Rigol, Simone Schaner, and Charity Troyer-Moore

February 25, 2016

1 Introduction

Recent advances in the technological infrastructure of developing countries hold great promise for reducing corruption in the delivery of private and public services. Electronic payment systems—which eliminate the role of middlepersons in benefit transfers and, thus, opportunities for leakage—are of particular interest to policymakers and researchers. Indeed, the Indian national government has made direct benefit transfers a key component of their policy agenda for financial inclusion and a top development priority. In 2008, the government mandated that wages provided under the National Rural Employment Guarantee Scheme (NREGS), which entitles every rural household to 100 days of paid employment, be directly deposited into beneficiary-owned bank accounts. In 2015, the government expanded this mandate to cover all benefits distributed by the central government.

Yet, mounting evidence shows that, though enthusiasm for electronic payments is warranted, the system is far from a cure-all for corruption in public service delivery. Muralidharan et al (2014) use a randomized control trial design to evaluate the impact of transitioning from a cash-based NREGS payment system to an electronic infrastructure built around biometric-linked smartcards in Andhra Pradesh (AP), India. The authors find a 12.2 percentage point reduction in leakages of government funds. But, they also document that the AP system is far from perfect: after being operational for two years, take-up of the biometric system among rural bank branches is only 50 percent. Mukhopadhyay et al. (2013) point to a number of design problems with the AP mechanism that could inhibit the reduction in corruption, including the existence of mechanisms to bypass biometric verification in favor of cash payments, poor incentives for smartcard service point providers to supervise usage and maintain quality standards, and a lack of proper integration between the direct benefit transfer (DBT) system and the banking system.

Lack of transparency could also foster corruption within the current DBT system: in many Indian states, most direct benefit transfers are deposited into household—rather than individual—accounts. When household wage payments are transferred to a single account, it may be difficult for a household to track exactly how many hours each member worked and how much each member should have received. This may be especially important in settings with low levels of numerical literacy. When households cannot keep track of their entitlements, local officials may transfer less than the earned amount to a particular household and skim the rest.

We exploit random variation in ownership of individual accounts and direct benefit transfer of NREGS wages among households in four districts of Madhya Pradesh (MP) to examine levels of corruption and methods by which local officials appropriate (or reallocate) NREGS wages. As part of a related study, we selected a sample of 4,500 females from rural households in Madhya Pradesh to participate in an intervention designed to measure the impact of offering individual bank accounts and DBT on female economic empowerment and well-being. In January – April 2014, these women were randomly assigned to one of three main treatment arms: (1) receive individual bank accounts, (2) receive individual bank accounts and direct transfer of their NREGS wages, or (3) receive nothing. Females who received bank accounts were also randomized across a financial literacy cross-treatment.

We hypothesize that our interventions may impact corruption experienced both by recipient households and by other households in intervention villages. Increased transparency of transfer size and amounts among intervention households may lead to decreased appropriation of these households’ NREGS wages. If the cost of appropriating wages via our intervention recipients’ bank accounts increases, local officials may re-allocate to other household members or to other households’ accounts, or the level of corruption within the village may be reduced. On the other hand, individual accounts may be more conducive to leakage (e.g. if women are not financially literate or empowered enough to monitor village officials). And, having multiple household accounts increases the transactions costs of retrieving wages. Recipient households might then be subject to increased graft, since village officials could exploit the increase in number of household accounts by offering assistance in retrieving wages for an implicit or explicit “fee”.

Our field experiment will allow us to isolate the impact of a shift in composition of bank accounts and frequency of account linking to NREGS wages on the level and type of NREGS-related corruption experienced by households in our intervention sample and by other households within the same village. In order to capture spillover effects of our intervention on other households, we selected a sample of 2,000 non-intervention NREGS workers in our study villages and we will survey these individuals both about their activities in the NREGS program and about their finances. By comparing self-reports about work,

administrative bank data, and NREGS official work reports for our sample and non-sample households, we will establish whether these households indeed worked for the program and where money from the program was transferred.

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. Data collection for our midline follow-up survey and an additional NREGS audit survey will take place from August 2015- February 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 for bank accounts, DBT, and financial capacity-building interventions:

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, eligibility

¹Joint bank account refers to an account that is under two persons’ names on the job card (the vast majority of joint bank account owners are spouses).

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

2.1.3 Households selection for NREGS Audit:

The sample of individuals for our NREGS audit is formed of recent NREGS workers in each of our 199 GPs: two weeks prior to surveying in a given GP, we pull muster rolls of the most recent NREGS work in that GP from the NREGS website (most recent muster rolls include work completed at any point between May 1, 2015 and the date we pulled the muster rolls from the NREGS website).³ We then select 7 job cards at random from that set of muster rolls, starting with the most recent muster roll (if there are fewer than 7 job cards, we select all the job cards listed). Amongst workers listed on the muster rolls, we randomly select one male worker and one female worker per sampled job card (if there is only one worker on a job card, we select that one worker). If there are no male workers on a job card, we select two female workers, and vice versa. Thus, our maximum possible sample size is 2,786 individuals (7 job cards x 2 persons per job card x 199 GPs); however, based on analysis of levels of NREGS work in the months leading up to our audit data collection, we estimate that we will reach a sample of roughly 2,000 individuals, which is in line with our power calculations.

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. NREGS Management Information System (MIS): records of NREGS project activities, including persons worked, days worked, and wages paid are publicly available on the NREGS 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) NREGS work history (including last date worked for NREGS, size of last wage received, and method of payment of last wages).
 - (c) Bank account ownership
4. NREGS audit survey: The audit is administered to roughly 1,750 individuals, with one to two respondents per household. In addition to the household roster and basic demographic information, respondents are surveyed regarding:
 - (a) Relationship with local government officials

²To 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.

³A NREGS muster roll is a list of days worked and wage received by each worker on a NREGS project. A NREGS job card is a household-level identification card needed to receive NREGS work.

- (b) Their own and other household members' NREGS work history
 - (c) Their own and other household members' work history for other work completed for local government officials
5. Midline survey: The follow-up survey is administered to 4,500 females and their husbands. In addition to the household roster and basic demographic information, respondents are surveyed regarding:
 - (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.
 6. Sarpanch survey.⁴
 7. 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).
 8. 2011 Indian Census data.
 9. Panchayat-level elections results data.⁵

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 NREGS work projects over the two years prior to census (from NREGS MIS)
 - (b) Proportion of NREGS workers to GP population over the two years prior to census (from NREGS MIS and Indian Census)
 - (c) Proportion of NREGS ghost workers at census: ghost worker defined as person who does not report working for NREGS, but is listed as having worked in the NREGS MIS (from census data and NREGS MIS data).
 - (d) Sex ratio (from 2011 Indian Census data)
 - (e) Caste composition (from 2011 Indian Census data)
 - (f) Caste of sarpanch (from Electoral data)
 - (g) Gender of sarpanch (from Electoral data)
2. Person-level characteristics tests for balance:
 - (a) Caste (from midline survey)

⁴At 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.

⁵We are currently investigating whether elections results data is available at the panchayat-level.

- (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 NREGS 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)

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:

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

In the regression above, i indexes the individual, j indexes her household, and k indexes her GP. $Outcome_{ikj}$ are the main outcome variables across which we theorize our treatments will have effects. Outcomes will also be presented at the level of the household. We will analyze the effects across our treatment groups: $T1_k$ is whether the GP was assigned to the individual accounts treatment and $T2_k$ is whether the GP was assigned to the individual accounts + account linking to NREGS payments treatment. F_k is whether a GP was assigned to receive financial capability building. Standard errors are clustered at the GP level. \mathbf{X}_{ikj} 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_1 denotes the effect of being assigned to a $T1$ GP, where we opened bank accounts; α_2 denotes the effect of being assigned to a $T2$ GP, where we opened bank accounts and linked those accounts to NREGS; α_3 denotes the additional effect of being assigned to financial capability training for those in $T2$ or $T1$.

Findings from our qualitative fieldwork indicate that there is a large amount of corruption in our study areas and that levels of actual NREGS work has been relatively low over the past year. This may reduce the power of our experiment to detect the effects of each of our individual treatments. To increase power, we may use a more parsimonious specification, pooling across our treatments. In the specification below, T_k is whether the GP was assigned to either the individual accounts treatment or the individual accounts + account linking to NREGS payments treatment and F_k is whether a GP was assigned to receive financial capability building.

$$Outcome_{ikj} = \theta_0 + \theta_1 F_k * T_k + \theta_2 T_k + \mathbf{X}_{ikj} + \mathbf{M}_k + \delta_k + \gamma_k + \epsilon_{ikj}$$

We also examine the effect of account linking to NREGS payments with the following specification, where the control group and $T1$ are pooled and β_1 denotes the effect of being assigned to a $T2$ GP:

$$Outcome_{ikj} = \beta_0 + \beta_1 T2_k + X_{ikj} + \mathbf{M}_k + \delta_k + \gamma_k + \epsilon_{ikj}$$

5 Heterogeneity Analysis

We expect the impact of our interventions to differ by respondents' level of empowerment and by panchayats' level of financial services access and NREGS 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. **Local access to NREGS work and to financial services:** Panchayat-level predictors will include baseline measures of: number of NREGS projects, number of NREGS workers, and gender composition of NREGS workers. Village-level predictors will include: whether there is a CSP within the village at baseline, distance from the village to the nearest bank branch at baseline, and distance from the village to the sarpanch’s home. Panchayat- and village-level indicators will be constructed using the NREGS MIS data, our CSP survey data, and our follow-up survey data.
2. **Household-level measures of empowerment :** Heterogeneity analysis will involve the following panchayat-level predictors: birth sex-ratios, male-female educational attainment, and basic demographics of the sarpanch. 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. Through our midline and NREGS audit, we collected data on respondent households’ relationship with the sarpanch (familial as well as social closeness). We will use this information to examine heterogeneous effects of treatment based on social proximity to the sarpanch (we will first check that treatment did not effect this measure).
3. **Local levels of NREGS-related corruption at baseline:** Panchayat-level predictors will include baseline measures of: number of ghost workers, number of ghost days, and level of wage skimming. Village-level predictors will include CSP operator’s relationship with the sarpanch at baseline (familial as well as social closeness).

6 Hypotheses

6.0.1 Hypothesis 1: Impact on level of NREGS-related corruption experienced by female intervention recipients

We hypothesize that being assigned to any treatment intervention may alter the probability that participants experience NREGS-related corruption, such as through wage skimming, or ghost worker hours or days.

6.0.2 Hypothesis 2: Impact on level of NREGS-related corruption experienced by other household members of intervention recipients

We hypothesize that being assigned to any treatment intervention may alter the probability that other household members experience NREGS-related corruption, such as through wage skimming, or ghost worker hours or days.

6.0.3 Hypothesis 3: Impact on level of NREGS-related corruption experienced by non-intervention households in intervention GPs

We hypothesize that being assigned to a GP in which some households receive a treatment intervention may alter the probability that members of non-intervention household members experience NREGS-related corruption, such as through wage skimming, or ghost worker hours or days.

6.0.4 Hypothesis 4: Impact on level of NREGS-related corruption in intervention GPs

We hypothesize that GP-wide levels of NREGS-related corruption, such as through wage skimming or reporting of ghost worker hours or days, may be altered by assignment to any treatment intervention.

References

- [1] Casey, K., R. Glennerster & E. Miguel, 2012. “Reshaping Institutions: Evidence on Aid Impacts Using a Pre-Analysis Plan,” *Quarterly Journal of Economics*, 127(4):1755-1812
- [2] Dasgupta, S.; Sudarshan, R. 2011. Issues in labour market inequality and women’s participation in India’s National Rural Employment Guarantee Programme, Working Paper No. 98, Policy Integration Department (Geneva, ILO).
- [3] Mukhopadhyay, Piali, et al. Implementing a biometric payment system: The Andhra Pradesh experience. Tech. rep., AP Smartcard Impact Eval. Proj. Policy Rep, 2013.
- [4] Muralidharan, Karthik, Paul Niehaus, and Sandip Sukhtankar. Payments Infrastructure and the Performance of Public Programs: Evidence from Biometric Smartcards in India. No. w19999. National Bureau of Economic Research, 2014.
- [5] Subbarao, K., del Ninno, C., Andrews, C., C. and Rodriguez-Alas. 2013. “Public Works as a Safety Net - Design, Evidence and Implementation,” Washington DC: World Bank.