

Can E-Governance Reduce Capture of Public Programs? Experimental Evidence from a Financial Reform of India's Employment Guarantee*

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Abstract

Traditionally, fund flow to local bodies responsible for implementing social programs is based on intended expenditure ratified by higher levels of administration. This paper reports on a field experiment which evaluated an e-governance reform of the fund-flow system for the workfare program in the Indian state of Bihar. The reform changed the traditional fund flow practice by instead conditioning funds disbursement for wage payments on incurred expenditure as reflected in worker detail entry on a new electronic platform. This reform reduced the number of administrative tiers associated with wage disbursement and changed the informational requirements for requesting and disbursing program funds. We find that program expenditure and reported employment declined by 25%, but with no discernible impact on actual employment as measured by independent surveys. Our results suggest that e-governance can reduce leakages of public funds but may not by itself improve public service delivery.

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I Introduction

Implementation bottlenecks constrain the effectiveness of social programs the world over, but the corruption costs of poor implementation are particularly severe in lower income countries. Yet, somewhat perversely, the implementation bottlenecks that generate rent-seeking and apparent program inefficiency are often themselves a consequence of government-instituted monitoring and accountability mechanisms (Banerjee, 1997).

A classic manifestation of this two-way relationship between implementation bottlenecks and the potential for corruption is the traditional funds-flow mechanism for social programs in the developing world. Decentralization of program delivery to local bodies for a host of programs – ranging from health and education to community development projects and local road construction – raises the need for a fund flow mechanism to transfer funds from more centralized tiers of government to local bodies. Traditionally, the physical distance between central and local bodies and communication difficulties meant that funds needed to be released prior to expenditure if cash-strapped local bodies were to undertake any activity . This, however, opens the possibility of funds embezzlement by local bodies. In order to prevent such malfeasance, typical public sector management practice is to institute requirements that fund-requests be ratified by higher levels of administration prior to funds release. But, of course, creating a long chain of intermediaries also increases the number of players who would benefit from rent-seeking. Studies who track public expenditures in developing countries document significant “leakages”, and often find that these rents are increasing in the number of officials involved (Olken, 2006; Reinikka and Svensson, 2011; Niehaus and Sukhtankar, 2013).

Can the advent of e-governance help cut the gordian knot between administrative practices and corruption? This paper reports on a large-scale randomized experiment which sought to reform the fund-flow mechanism for India’s workfare program – Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS).¹ The evaluation was conducted between September 2012 and March 2013 in conjunction with the Department of Rural Development in Bihar and spanned twelve districts (with a population of 33 million). The status quo flow of

¹In 2013 close to 50 million households participated in MGNREGS and total expenditures amounted to 0.5% of India’s GDP. There is evidence that it led to an increase in rural wages (Azam, 2012; Berg et al., 2013; Imbert and Papp, 2014), a reduction in seasonal migration from rural areas (Imbert and Papp, 2014) and a reduction in poverty (Deininger and Liu, 2013).

funds from the state department to the village local body - Gram Panchayat (GP) - involved four tiers of administrative hierarchy (GP-block-district-State). The GP would submit its funds request and it had to be verified by block and district officials before submission to the state. In treatment GPs funds disbursement was instead linked to actual expenditures. Specifically, the GP official was required to directly input the names of those who had worked in the scheme and were owed wages into a front end data base. The information was uploaded to a central data base, and automatically led to the release of the funds to the GP account. The disbursement of funds from the GP account to villagers was unchanged.

The reform changes the administrative requirements for MGNREGS fund flow in two ways. First, by increasing the informational requirements for requesting funds, it enhanced transparency. In the status quo the GP requests advances from the block absent any documentation of expenditures to justify past spending until several months later, when they input the information into the main public data base (nrega.nic.in). Second, it reduced the number of administrative tiers involved in the fund flow process. The GP official, in principle, no longer requires approval from Block or district level officials for fund flow. Whether this reduces corruption depends on whether having more officials involved in the process reduces rent seeking by improved monitoring or increases rent seeking by enlarging the number of who asks for bribes.

We should note two important caveats on how the reform, in practice, worked – both related to implementation of a new technology in a poor state. First, unlike district officials, the block officials continued their involvement because they controlled the data entry infrastructure which only existed at the block-level. Second, the absence of a cross-walk between Bihar’s financing data base and the online public access data base implied that the separate requirement to enter the data in the public access data base remained. ²

The treatment had multiple impacts. First, administrative data shows that financial outlays for MGNREGS from the State to treatment GPs were 38% lower relative to control GPs. This effect is a combination of a 25.5% drop in GP account balance and a 24.5% decline in GP spending. The decline in spending is matched with a corresponding decline in the expenditures and the worker information, as entered in the public access data base. Second, data from an independent household survey conducted after the reform period ended showed no change

²Put differently, treatment GP officials had to enter same information twice: once to get paid and once to satisfy the nationwide standards of accountability.

in days worked and payment received by households. Instead, the number of “ghost workers” (individuals who are in the public access data base but have in fact never worked) declined, suggesting a reduction in leakage. In addition, there is no change in public infrastructures built under the program. Third, affidavit data on public employee assets shows a corresponding decline in reported assets at almost all quantiles of the distribution of (self reported) wealth in treatment GPs and blocks.

Thus, the financial reform was effective in reducing corruption and program costs, but actual demand that was met by the program was unchanged. This is contrary to the hypothesis that the red tape that generates implementation bottlenecks causes a large share of the inefficiency. Our analysis period was particularly difficult for MGNREGS in Bihar with the state funds pool running dry and GP functionaries going on strike in the three to four months of the seven month intervention period. We observe an initial increase in delays in payment for beneficiaries which disappeared after three months. Delays may be due to the fairly onerous data entry requirements for the GP under the new system, or to initial strain on the Central Bank of India which had to process many more payments. Hence, the clear reduction in leakage is impressive given the challenging context, but our findings also suggest a need for complementary reforms to improve delivery to beneficiaries.

Our paper makes contributions to multiple strands of the economic literature. First, it adds to a growing number of studies which evaluate administrative reforms in settings with weak state capacity (Banerjee et al., 2012; Duflo et al., 2013; Bó et al., 2013). Some of these studies focus on the use of information technology, or e-governance (Barnwal, 2014; Muralidharan et al., 2014). Most closely related is Muralidharan et al. (2014) who evaluate an experiment in Andhra Pradesh (from 2010 to 2012), in which biometric smart cards were provided to MGNREGS beneficiaries and wage disbursement was shifted from post office workers to locally hired bank employees. Our intervention, in contrast, did not affect this final chain in the process. Reflecting differences in what was reformed, we observe differences in the margins along which the reform reduced corruption. Muralidharan et al. (2014) find that biometric identification of beneficiaries reduced opportunities to over-report MGNREGS days for actual beneficiaries while we find a reduction in the incidence of fake workers. They find no evidence of payment delays increasing which possibly reflects the better infrastructure in the state and that biometric identification in treatment areas

was voluntary, not mandatory.³

Second, we contribute to the forensic economics literature which seeks to obtain objective estimates of corruption (see Olken and Pande (2012) for a review). Studies that track expenditures typically compare expenditures made by the state to what is disbursed by the lowest level of administration, or received by beneficiaries (Reinikka and Svensson, 2011; Olken, 2006). In the context of MGNREGS, Niehaus and Sukhtankar (2013) sample households which are reported to have worked for the program according to the official website nrega.nic.in and attempt to survey them, and to measure how much employment and wages they actually received. Ours is one of the few studies which combine this “forensic” method with a randomized control trial to provide experimental evidence on corruption mechanisms (Olken, 2007; Muralidharan et al., 2014). Specifically, rather than comparing levels in reported and actual outcomes, we identify leakages by comparing changes in reported and actual outcomes in randomly selected treatment and control areas, following the implementation of an administrative reform.

Third, we contribute to a nascent literature on the industrial organization of public service delivery, and its implications for corruption. The theoretical literature on corruption has long emphasized the importance of administrative structure and task-organization in affecting corruption (Shleifer and Vishny, 1993; Banerjee, 1997; Banerjee et al., 2012). However, most empirical studies have focused on the effect of information disclosure, increased monitoring, and monetary incentives, keeping the administrative structure constant (Olken, 2007; Reinikka and Svensson, 2011; Ferraz and Finan, 2011). Few examine the effect of changing the administrative structure itself. Two existing studies examine changes in the number of functionaries or jurisdictions at lower administrative levels, which compete to provide services to the public (Olken and Barron, 2009; Burgess et al., 2012). Our paper is one of the first to estimate empirically the effect of replacing intermediary layers of administration which in the status quo are in charge of funds release and monitoring by automatic transfers and online documentation of expenditures enabled by information technologies. Across the world, a system similar to our status quo is typically in place, and in most settings, it would now be technologically feasible to adopt the reformed system we study.

³They find higher payment to NREGS workers and higher household incomes, with no change in government outlay, indicating a reduction in leakage. They also find that the delays in payment and the time spent collecting payment decreased substantially.

The paper is structured as follows. Section Two presents the context and describes the intervention and its expected impact. Section Three details the data we use and our empirical strategy. Section Four presents the results, and Section Five concludes.

II Background and intervention

A MGNREGS in Bihar: Context and Performance

The Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) was created in 2005 by the National Rural Employment Guarantee Act. The Act guarantees 100 days of work per year per household for all rural adults who are willing to do unskilled manual labor at the stipulated minimum wage. Local officials have the responsibility to register beneficiaries and provide them work on local infrastructure projects. With close to 50 million beneficiary households in 2013, the MGNREGS is one of the largest social protection programs in the world today.

From the start, the quality of its implementation has been very unequal across Indian states. Employment provision under the act is concentrated in seven “star” states - including Andhra Pradesh, Madhya Pradesh, Rajasthan and Tamil Nadu - and consistently low in some of India’s poorer states - including Bihar Jharkhand, Orissa and West Bengal - Imbert and Papp (2015). As Dutta et al. (2012) note “the incidence of unmet demand tends to be higher in poorer states even though demand for the scheme is higher there”. This is particularly stark in the case of Bihar, which has one of the highest poverty rates in India and the highest unmet demand for MGNREGS work. Using NSS data for 2009-2010, we estimate that 35% of households in Bihar both want to work and could not find work (a further 10% did find work).⁴ By comparison, in the better performing state of Andhra Pradesh only 12% both want work and could not find work (and 39% of households participated in MGNREGS). Historically, Bihar has also had a poor record of governance and poverty reduction. However, since 2005, it has seen growth accelerate and alongside government efforts to promote economic development have been widely acknowledged (The Economist, 2010).

MGNREGS implementation has also been plagued by widespread corruption. Comparing

⁴Authors’ calculation based on National Sample Survey data for July 2009 to June 2010.

public reports on nrega.nic.in and National Sample Survey data from June 2007 and July 2008, Imbert and Papp (2011) find that employment estimates based on survey data only account for 42-56% of official figures on MGNREGS employment. Since the early years of implementation, the central and state governments took important steps to reduce corruption, e.g. regular implementation of administrative and independent audits, mandatory payments through banks or post offices. Applying the same method to data from July 2011 to June 2012 yields higher estimates about 80% (Imbert, 2014). In Bihar, similar efforts have been carried out but corruption is still widespread. Dutta et al. (2014) estimate that 20-25% of MGNREGS payments are unaccounted for in survey data. In our own survey, 48% of surveyed politicians mentioned corruption in the administration as a major implementation issue. A politician from Jamui district declared “Bribery is so common that it almost seems like it is the only way anything gets done in the GP”. Respondents described a system of “taxes” extracted by MGNREGS functionaries, equivalent to 21-30% of expenditures.⁵

Both supply- and demand-side factors have been identified for the continuing weak performance of MGNREGS in the state. On the supply side, a lack of awareness about the scheme among potential beneficiaries is often cited as an important reason. In qualitative interviews we conducted with 350 local politicians, 44% cite the lack of people’s willingness to work as a major implementation issue.⁶ However, experimental evidence provided by Ravallion et al. (2013) suggests that increasing awareness about MGNREGS and improving perceptions of the scheme is inadequate to increase participation. This suggests an important role for the lack of administrative capacity at the local level, which, among other things, has reduced the state’s ability to spend centrally-funded schemes in the past (Mathew and Moore, 2011). Certainly, local politicians are fast to ascribe poor implementation to a lack of funds.⁷ Below we describe the status quo system and some basic diagnostics.

⁵Qualitative interviews conducted with 350 GP heads. The data collection and methodology is described in section A below.

⁶The survey methodology is described in section A below.

⁷85% of Mukhiya interviewed by us (from a pool of 350 Mukhiya) made this claim. The methodology is described in section A below.

B Fundflow management in MGNREGS

1 . Overview

MGNREGS is financed by the federal government which transfers funds to the state on the basis of projected demand. Thus, MGNREGS fund-flow has two parts: from the central government to the state and then from the state to lower tiers of administration called district, block and Gram Panchayat (GP). The GP is the local implementing agency.

The transfer of funds from centre to state happens in tranches, the first tranche is made on the basis of anticipated demand and expenditure from previous years, and the next tranches are upon request by the state. To enable accountability of subsequent expenditures, the central government requires that at least some fraction of labor expenditures are “accounted” for before releasing the next tranche of funds (this fraction was 60% during the period we study). This reporting occurs in an electronic data collection system (nrega.nic.in). The Central Government enforces compliance with the entry in the data base by refusing money to states. In October 2012, less than a month after the launch of our intervention, the state pool of MGNREGS funds ran dry due to incomplete data entry on the part of the state. The state administration, in turn, argued that poor record keeping and incomplete data entry by GP officials were the reasons for incomplete data entry (Dutta et al., 2014).

Turning to within-state fund flow, the demand-driven nature of the program implies that funds requests originate from GP but then are aggregated up the chain to the state-level. Similarly, disbursements typically move incrementally down administrative tiers. First, from state to districts then from districts to the block and finally from the block to GP accounts. Since the money disbursed to a district is typically lower than what is requested, there is significant discretion in how each administrative tier redistributes resources to the next tier. Also given that the money is pushed down the chain in lumpy transfers, some units may run out of funds while others have large unspent amounts (“parking of funds”).

In the two years prior to our intervention (2010-11) the Bihar government introduced e-governance reforms to improve the monitoring of the fund flow and coordinate efforts to obtain funds from the Center. An electronic platform called Central Planning Scheme Monitoring System (CPSMS) was introduced to monitor account balances of all agencies. Alongside, it created a state level pool of funds which channelled all transfers from the Center and opened

Zero Balance Accounts for all districts, which suppressed any parking of funds at the district level.⁸ It also allowed districts to monitor GP balance and to transfer funds from the state pool to the GP account directly, without having to go through the blocks.

The state published guidelines, to request districts to transfer funds automatically to GP, whenever the balance of their account fell below INR 1 Lakh. However, due to various inefficiencies in the bureaucratic system, this push was not made automatically, and was in fact based on some bargaining between the block and the GP, and in turn between the district and the block. Based on our analysis of fund-flow data of GP accounts, between July 2011 and July 2012 in 12 districts of Bihar, the average time taken to replenish a GP account that was short of funds was about 3 months. GP officials interviews we conducted in May-July 2013 suggest block and district officials requested bribes to process payments. GP level officials would hence pay as kick-backs part of payments received after inflating the number of days worked by MGNREGS beneficiaries.

2 . Within-state fund flow: Status quo and the reform

In 2012 Bihar's Rural Development Department decided to implement and evaluate a reform to how MGNREGS fund flow within the state. We describe in turn the status quo and the reformed fund flow.

Figure 2 describes the fund flow in the status quo. At the beginning of each financial year, a first installment is sent to each GP account. If the district has not replenished the account automatically, when it runs out of funds, a GP needs to make a written or verbal request to the next administrative tier (block). The block officials ratify this request and pass it on to the district administration. The district administration then uses the CPSMS platform to request funds transfer from the state pool to the GP savings account (4).⁹ The funds request is typically based on anticipated need and, therefore, does not need to be supported by specific documentation of expenditure, although the verification process by higher levels is certainly intended to play a monitoring role. The documentation of worker details on the nrega.nic.in

⁸Since any payment made is automatically compensated by a transfer from the state pool, Zero Balance Accounts enable districts to transfer funds to local implementation agencies regardless of the amount of funds they individually received from the Center. The budget constraint is binding collectively at the state level.

⁹In principle, district officials can use CPSMS to monitor the balance of GP accounts and transfer money without waiting for the GP to request funds. The state administration gave them guidelines to do exactly this, but our field reports suggest they were not closely followed.

occurs independent of the within-state fund flow process.¹⁰

Figure 3 describes the reformed fund flow for labor payments: after entering beneficiary details the GP official can now directly log into CPSMS. After this, it can action the automatic transfer of incurred wage expenses from the state pool to the GP savings account. In practice, this data entry occurred at the Block office (with assistance from a data entry operator hired at that level), since most GPs do not have the necessary infrastructure.

Three important aspects of funds-flow system were not affected by our reform. First, the reform only affected the process through which funds for labor move from the State pool to the GP account. The last step (beneficiary payment) is the same under the old and the new system: the GP transmits a list of payment to be made to the postal office as well as an aggregate check, and the postal office credits each account. Possible frauds persist at that level, for example if the beneficiary entrusts his passbook to the village-level functionary or Mukhyas (Muralidharan et al., 2014). Second, it did not affect the process for payment for materials, which was still disbursed through CPSMS by the districts. It could still indirectly affect expenditures for material, however, because of the rule that at least 60% of the expenditure must be made on labor. Finally, the requirement that all the data be independently entered in the nrega.nic.in data base remained. The GP officials thus had to enter the same information twice: once to get paid, and once after the fact.

C How may financial reform affect rent-seeking and program efficiency?

1 . Impact on corruption

The shift from the status quo to the reformed system had two main consequences.

First, transparency was enhanced, since disbursements were now directly linked to an actual (reported) expenditure. As described above in the status quo, the GP gets an advance, and the District is supposed to replenish the account as soon as it falls below some threshold. District officials have the authority to require documentation before releasing the next tranche, but it is not done on a routine basis. In the new system, funds are released to the GP only when they document that a specific person has worked for a specific number of days. This both

¹⁰In this data base, information on every single job spell, including the identify of the beneficiary and the payment, is required, However, due to the lack of IT infrastructures at the local level, the data entry is often delayed by many months, which makes it more difficult to monitor spending and delays funds release.

creates a direct link between a transfer made by the Center and a documented expenditure and makes auditing (either by the government or by social auditors) easier. In the status quo, documentation occurs only ex-post through data entry in nrega.nic.in, and is only loosely tied in with actual expenditure. For example, in Bihar at the time of the intervention, there was a delay of six months in getting 60% of the data entered, despite intense pressure from the Center to the State. Hence, the ability to use expenditures documented by the GP as a monitoring tool may be limited.¹¹

Second, the intervention reduced the number of people involved in disbursing money. In contrast to the “overfishing” set up analyzed, for example in Olken and Barron (2009), where a number of players try to extract rent with no coordination, the setting here is one where the lower level official exerts effort (both the real effort and the corruption effort and risk) and the higher level officials (at the block and the district) extract a tax. Since the IT infrastructure and the data entry operator were at the block, the block official remain in a key position of power (in fact his power may be even bigger since he is now not officially responsible for anything, but the data entry cannot happen without his cooperation). However, the district officials were now entirely cut out. They were in fact quite aware of it, and our field report suggests that they actively opposed the system. The effect on corruption is ambiguous: this can be understood as a reduction in the tax rate on any corruption receipts of the lower GP official. The income effect would thus lead to a reduction in corruption. However, the substitution effect goes in the opposite direction if the GP official can keep a larger share of its gains.

Overall, the impact of the reform, which increased transparency and reduced the number of intermediaries, should thus be to reduce corruption, unless the substitution effects were very large.

2 . Impact on beneficiaries

The impact of the reform on how well MGNREGS targets work demand is less clear.

On the one hand, several argue that the reduction in the number of workdays provided under the scheme in states like Rajasthan, which is a star performer and has emphasised social audits,

¹¹With the long delay, and bulk entry, it is easy to argue that any discrepancy between the records and the reality seen in the field is the result of a clerical errors, or poor recollection by the workers. Even though the case can be made that *on average* there are both ghost days and ghost workers in the system, it is much more difficult to build an individual case based on that evidence.

can be partially attributed to the strong control on corruption: GP officials basically see very little upside in running the scheme. If the probability of an audit is large, and the consequence potentially severe, organising work sites and projects may be more trouble than it's worth. This is, in part, the argument that is routinely made by GP functionaries when they go on strike (including the strike that happened during our project). More specific to the implementation of this particular project, the new procedure turned out to be onerous for the GP officials, since they had to go to the Block to access the proper IT infrastructure and were often dependent on a DEO who was not necessarily very motivated. This added one more step for them to carry out their responsibilities to provide the program, that some of them came to resent. If there is a fixed cost to going to the Block, it also means that they prefer to do all the entry in bulk: they may thus either pay workers with extra delay, or postpone starting new projects until they got around to enter the data and get money.

On the other hand, several aspects of the status quo system could have lowered program activity. First, since the advances are not systematic, but depends on the actions both of Block and GP officials, there is considerable uncertainty on when and whether a GP will receive funds. This means that they can be quite reluctant to start a project until they see money in the account (but conversely the higher level officials may be reluctant to disburse money until they see activities). The lack of a link between the disbursement and the expenses also mean that some Panchayat may have money they don't spend while another cannot get money to finance projects, so that there are liquidity constraints at the local level even though there are funds in the system on aggregate. Both of these problems are solved by the new system, making funds availability less likely to be a constraint.¹²

3 . Impact on Program Costs

The reform in cash management systems should reduce financial costs for achieving program outcomes by two mechanisms. First, by eliminating float funds parked at the GP level it should reduce the overall requirement for funds to be transferred by the state. Second, by allowing GPs to pull funds as and when they are needed, the distribution of funds should be more efficient.

¹²Although unfortunately, the fact that the launch of the new system almost coincided with the State Pool running dry due to the conflict with the Center over data entry means that the GP officials may not have perceived this advantage, and may in fact have wrongly made the opposite causal inference.

This should allow GP s to harmonize their funds requests with their spending levels, aligning funds movement with program activity. Therefore, the improved cash management system should reduce the financial costs to the state for achieving the same level of program activity.

III Data and Experimental Design

A Data

Our analysis makes use of multiple data sources. To measure GP-level MGNREGS expenditures we combine two administrative sources. First, the CPSMS system which covers GP savings accounts from September 2011 to January 2014. We observe every credit and debit made in the account, which allows us to monitor daily spending in treatment and control GPs. The data does not, however, identify transfer recipients. Second, nrega.nic.in which reports fiscal year expenditures from April 1st to March 31st of every year - the year 2012-13 includes a part of the pre-intervention period and the treatment period. Data from nrega.nic.in allows us to decompose expenditures into four categories - unskilled labour, material, skilled labour and administrative expenses - which is not possible with CPSMS data.

To measure employment and wage payments we combine administrative reports and independent survey data. Administrative data include work days and MGNREGS worker payments as reported on the household job card in nrega.nic.in. These data likely include false worker names and false accounting of days to workers (ghost workers and ghost days).

In May-July 2013 we conducted a survey of 10,036 households in 390 GP to independently measure MGNREGS participation, employment and payments.¹³ Each household member was asked about his/her participation in MGNREGS every week since July 2012 as well as the amount, date and type of payments for each workspell.

To compare administrative reports and survey data for the same household, we attempted to match survey households and nrega.nic.in job cards. The paucity of household information provided on a job card restricts us to using household member name and age and we define households with at least one matched household member as matched. We matched 71% of the survey sample to at least one job card, and each matched household was on average matched

¹³We randomly sampled two GP per block, and 25 households per GP. The survey methodology is detailed in Appendix V.

with 5.2 job cards. Matching imprecision implies that a comparison of employment levels across survey and website data is unlikely to yield a reliable measure of total leakage. However, as long as matching quality is similar across treatment and control, we can compare differences in matching rates between treatment and control.

In parallel with the household survey, we also surveyed 4,165 MGNREGS infrastructure projects (10 per GP) randomly sampled from official reports in nrega.nic.in. Surveyors recorded whether they could find the asset and whether it was completed. Finally, we conducted a semi-qualitative interview of the head of each GP (Mukhiya). 358 Mukhiya could be located and interviewed. We collected information on characteristics of the Mukhiya and her family, including ownership of movable and immovable assets, as well as cattle. Part of the interview was dedicated to issues with MGNREGS implementation. Mukhiyas were asked to provide reasons for why two GPs in the same block could have different spending levels. Their answers were later transcribed and coded. At the end of the interview a simple lottery was used to measure their risk preferences.

B Sample and Reform Implementation

Our field experiment spanned 12 districts in the South and West of Bihar, covering a rural population of 33 million (see Figure 1). In each district one third of the blocks were randomly selected to implement the new fund flow (69 “treatment” blocks) and the remaining 126 “control” blocks kept the status quo. The intervention was launched in September 2012 and concluded in March 2013.

An important pre-requisite for implementing the reform was IT infrastructure for GPs to connect to CPSMS.¹⁴ Appendix Table A.1 shows in July 2012, two months before the reform, a minority of blocks had required facilities, and the number of computers and operators fell short of meeting the requirements of three per block on average. By January 2013, five months into the intervention period, a majority of treatment blocks had a generator, internet, scanner and printer and the number of computers was close to fulfill the requirements. In April 2013, immediately after the intervention was rolled back, treatment blocks still had the required infrastructure, and were better equipped than the control blocks. The number of data entry operators and

¹⁴These prerequisites included computers, data entry operators, generator to ensure constant power supply, internet access, scanner and printer.

computers, which were often hired on temporary contracts and could be laid off as soon as the data entry requirements were removed, had already fallen but remained higher than in control blocks.

The intervention was officially launched on September, 8th 2012 but in the following month the MGNREGS pool of funds ran dry. Specifically, the federal ministry refused to release funds until at least 60% of expenditures since April 2012 were documented on nrega.nic.in. This process took close to three months and state funds were only replenished mid-December 2012. Moreover, soon after GP functionaries went on strike for two weeks. Figure 4 shows that MGNREGS spending fell sharply in September and rose only slowly in January 2013.¹⁵ This sequence of events reduced treatment uptake and also led to perceptions that the reform may have contributed to funds drying up.

Another early implementation hurdle arose from the fact that the bank which processed payments at the CPSMS back end lacked resources to deal with large amounts of small invoices sent by the GP in treatment blocks, and gave priority to larger invoices coming from Control blocks. Treatment GPs eventually started sending larger invoices and the bank increased its capacity.

The new fund flow really started to function in January 2013. Figure 5 shows the fraction of treatment GP who have used CPSMS once, which went up from less than 20% in December 2012 across all districts to 60% in April 2013. There is large heterogeneity across districts: the best performing district, Begusarai had more than two third of Panchyat using the system in December 2012, and the proportion reached more than 90% in April 2013. By contrast, the fraction of GP using the system in Madhubani, the worst performing district only increased slowly after December to reach 40% by April 2013.

C Empirical Methodology

In order to evaluate the effects of the intervention, we compare the GP in the 69 treatment blocks with GP in the 126 control blocks. The random selection of treatment blocks ensures in principle that the two groups of GP are ex ante identical. As a check, we examine pre-treatment

¹⁵Part of this variation is due to the seasonality of MGNREGS works themselves, which close during the peak season of agriculture from July to December (Imbert and Papp 2014). However the dip was stronger in that year.

differences between treatment and control GPs from estimating regressions of the form:

$$X_{pd} = \alpha + \beta T_p + \eta_d + \varepsilon_p$$

where X_{pd} is a vector of baseline characteristics of GP p in district d , T_d is a dummy which is equal to one if GP p is in a treatment block, η_d are district fixed effects, and errors ε_p are assumed to be correlated within each block. The estimated coefficient β represent pre-treatment differences between treatment and control GP. Table 1 presents the results of the estimation. We observe few significant differences between the two groups. Villages in treatment and control GP have similar socio-demographic characteristics and have the same level of infrastructures according to 2001 census. Treatment GP are less likely to be reserved for women but the difference is small (1.5 percentage point), we find no difference in caste reservation. Finally, according to nrega.nic.in, Treatment GP had 15% higher MGNREGS labor expenditures between April 2011 and March 2012, i.e. the financial year preceding the intervention. The difference is significant at the 10% level. There is no difference in total MGNREGS spending between treatment and control GP at baseline according to CPSMS.

A first set of results considers the effect of the intervention on MGNREGS expenditures, which are measured at the GP level before, during and after the intervention. Let Y_{pdt} denote the outcome for GP p in district d for period t . As before, T_p is a dummy variable which equals to one if GP p is in a treatment block and η_d is a district fixed effect. We estimate the following equation separately for each period t :

$$Y_{pdt} = \alpha + \beta T_p + \eta_d + \varepsilon_{pt} \tag{1}$$

where the errors ε_{pt} are clustered at the block level. The coefficient β estimates the treatment effect when t is the treatment period (September 2012 to March 2013). Whenever possible, we split the intervention period to consider separately the September to December 2012 period, when the state pool of funds was dry and the PRS were on strike, and the January to March 2013 period, when the new system was running smoothly.

A second set of results explores the effect of the intervention on household outcomes, such as MGNREGS participation, employment and wages. Let Y_{hdt} denote the outcome for household

h in district d at period t . Let Z_h denote a vector of household characteristics, which includes religion, caste, gender and literacy of the head of the household, household size, the number of adults in the household, the type of house which the household occupies and a dummy variable for whether the household owns land. T_h is a dummy variable for whether the household lives in a treatment block. We estimate the following equation separately for each period t :

$$Y_{hdt} = \alpha + \beta T_h + \delta Z_h + \eta_d + \varepsilon_{ht} \quad (2)$$

where standard errors are clustered at the block level.

Finally, we estimate the effect of the program on outcomes measured for the GP head, the Mukhiya. We use a specification similar to the one for household outcomes, except that the set of controls now includes Mukhiya religion, caste category, gender, a dummy variable for whether the Mukhiya has completed high school, a dummy variable for whether the Mukhiya is older than 40, and two dummy variables for whether the Mukhiya or a family member was elected in past elections held in 2001 and 2006, respectively.

IV Results

This section starts by examining the effect of the intervention on MGNREGS expenditures and reported outcomes, before turning to actual MGNREGS outcomes measured by our independent survey. Finally, it provides some evidence on the margin of leakages which was affected by the reform.

A Program Financial Costs

1 . Reduction in Expenditure

CPSMS data on daily debits from GP Savings Account allows us to compare MGNREGS spending across treatment and control blocks. Figure 4 plots average daily spending in treatment and control GPs for the three year period from July 2011 to January 2014. We observe a seasonal pattern to spending in the fiscal year prior to treatment which reflects the fact that MGNREGS work largely occurs in agricultural lean season.

In the year prior to treatment the spending trends are similar across treatment and control

GPs. However, between September 2012 and March 2013 a clear difference emerges with lower spending in treatment as compared to control. After the intervention is rolled back on April 1 2013, treatment blocks rapidly converge back to control block spending levels. Panel C in Table 2 presents the same evidence with a regression analysis, using the estimating Equation 1 described in the previous section. There is no significant difference between treatment and control blocks before the implementation period. Between September to December 2012, spending is 19% lower in treatment blocks, and from January to March 2013 it is 31% lower. After April 2013 there is no significant difference in spending between treatment and control blocks.

The decline in MGNREGS spending in treatment blocks is confirmed by data on nrega.nic.in (which is entered independently from the financial data). Table 3 shows that for the financial year 2012-13, i.e. between April 1st 2012 and March 31st 2013, expenditures on labour and on materials were respectively 17% and 14% lower in treatment blocks. The intervention was shorter than a fiscal year, so the fiscal year does not quite represent our intervention period. Given their different time spans of 7 and 12 months respectively, CPSMS and nrega.nic.in data provide very consistent estimates for the negative effect of the intervention on spending. This is not surprising as, eventually, it appears that about 100% of the expenditures in CPSMS (and not just 60%) are eventually matched with records in the Nrega.nic.in data base.¹⁶

The treatment effect on material expenditures as reported in nrega.nic.in is an indirect impact of the fund flow reform: while materials were not affected by the change, there is a rule that constraints material expenditure to be at most 40% of the total spending on a project, and that rule is close to binding (it is close to be binding on aggregate as well in table 3). Thus, the reduction in reported labor expenditure must be matched by a reduction in the material expenditures.

2 . Reduction in Funds Float

I think this should go first. Discuss Panel A, B then C of Table 2. By linking funds released to reported expenditures the electronic transfer system reduced the amount of float held in treatment GP accounts. Panel B in Table 2 illustrates this effect. The balance of GP accounts was the same in treatment and control blocks at the beginning of the intervention in September

¹⁶Appendix Table A.2 compares annual expenditures per GP in CPSMS and nrega.nic.in. The discrepancies are only about 8-11% in 2012-13.

2012 and declined both in control and treatment until December 2012, because the state pool of funds was dry and the GP depleted their accounts. However, by December 2012, the state pool was replenished and control GP received large instalments, while treatment GP only received funds corresponding to the expenditures they had documented in the electronic system. By the end of the intervention in April 2013, the balance in treatment GP was 25% lower than the balance in control. Hence, on the top of the decline in expenditures, the state transferred Rs. 200 million, or 3.5 million dollars less to treatment GP.

By the end of the intervention, MGNREGS expenditure declined by 25% and GP account balances were reduced by 25%. In combination, the reduction in expenditure and funds float reduced the financial cost to the state for implementing the program in treatment GPs by 38% as the Panel A in Table 2 shows. However, this reduction in financial costs of the program could be due to a reduction in real outcomes (days of employment offered, and asset built), or a reduction in leakage, or both. We now turn to this question by using our household survey to measure real outcomes.

B Actual MGNREGS Outcomes

1 . Employment

Using household survey data we construct three measures of MGNREGS employment. The first is a binary indicator of participation in MGNREGS, the second is the number of weeks in which households declare having worked in MGNREGS, the third is the number of days worked. We regress these outcomes on a treatment dummy, household controls and district fixed effects using Equation 2 from the previous section.

Panel A of Table 4 reports treatment impacts on the probability of participating in MGNREGS. The estimates are small and insignificant for the whole of the period from July 2012 to July 2013, as well as for the intervention period only. Note that MGNREGS participation rates were low at 4%, arguably reflecting the relative lack of funds in Financial year 2012-2013.¹⁷

¹⁷The mean of 4% of households having participated in the scheme in the study period is lower than the 9% participation found for Bihar for July 2011 to June 2012 in the NSS. This is not a measurement issue, but to the time period. When we aggregate our weekly observations to an entire year, we also find a participation of 8%. We also asked the household head whether anybody had participated in the scheme “since the last rainy season”, and 9% of households report that they did. The treatment effect, presented in table A.3 is the same. The lower number during the intervention period is due to the fact that the intervention period did not cover the peak season of unemployment under the scheme: it started just after one and was cancelled just before the next

Nevertheless, given the small size of the standard errors, the results imply a precisely estimated zero effect of the intervention on MGNREGS employment. Treatment effect estimates on the number of weeks of public employment in Panel B of Table 4 yield the same conclusion. If anything, the probability that a household works in a given week is higher in treatment blocks, but the difference is not significant.

Panel C of Table 4 presents the estimated effect on the number of days provided, which may include more measurement error (since it is based on retrospective questions and recall of the exact number of days may be an issue). The treatment impacts are similar to those of the number of weeks and participation rate. The number of days worked was a little lower in treatment just before the intervention started. To the extent this was some underlying difference, the simple difference in the post period may actually under-estimate the treatment effect.¹⁸

2 . Labor Payments

Table 5 shows the impact on wage payment to households (from the household survey). For each spell worked in MGNREGS, the respondents declared whether, when, and how much they had been paid at the time of the survey.

As for the probability of working (Panel A), there was a small underlying difference between treatment and control before the intervention started (wage payments were somewhat lower in the treatment). There is however no significant effect on payments made for work spells during the intervention. The estimates are noisily estimated, equivalent to -11% of payments in control during the first four months and +14% during the next three months. The total effect is an insignificant -0.7%.

Panel B of Table 5 yields further insight about the effect of the intervention on MGNREGS payments. As compared to an average delay of 73 days in the control, workers employed during the first phase of the intervention (Sep-Dec 2012) in treatment blocks waited 44 more days for their payment. The effect is large, and statistically significant. Workers who worked during the second phase of the intervention also waited longer in treatment than in control blocks but the difference is smaller (11 days) and statistically insignificant. These results suggest that the

one.

¹⁸We also find that participation in MGNREGS was lower before the intervention in treatment GP, but the difference is not significant (see Appendix Table A.3).

intervention slowed down the disbursement of funds to GP, and further delayed workers payments, especially during the first phase of the intervention. Qualitatively, this is corroborated by the Mukhiyas whom we interviewed in parallel to the household survey. Results presented in Panel C of Appendix Table A.4 shows that twice as many Mukhiyas either spontaneously declared or agreed with the view that the CPSMS created delays in fund flow in treatment (34%) than in control blocks (17%).

This increase in delay in payment is not innocuous, and is a significant downside of the intervention, as implemented at least initially. The scheme is designed to serve as insurance in the lean period, and delays in payment significantly hampers its ability to play this role: a delay in 73 days essentially means that work done during the worst seasons is paid for during the high season. An objective of the scheme was to reduce this delay by removing steps in the fund flow, but it had the opposite effect. There are two reasons for this, which might both have been temporary: the first is that, in the early days, the bank handling CPSMS payment found itself completely deluged with the small payment request coming from the GP s. They responded by waiting for a batch of invoices to treat them together, which caused delays. The second is that GP level functionaries delayed the data entry (since it required traveling to the block office), and thus the payments. All these interpretations and the smaller and insignificant treatment effect during the second part of the intervention period suggests that the problem may have been temporary, but was still a failure of the intervention.

The increase in delays could have had a negative unintended consequence. One way in which Mukhiyas or other GP functionaries take advantage of the delays in payment is by lending worker money. They pay them immediately, but when the funds do come and get to the Postal Account, they get reimbursed. The interest is collected in advance by paying the workers less than what they are due. Repayment is enforced by collecting the worker's bank passbook, and cashing out their postal account on their behalf. Panel C of Table 5 suggests that this apparently was not the case: instances of illegal advance payment are indeed frequent (a quarter to a third of payments in the control group), but they are not increased by the intervention. Using our survey, we also compare household consumption levels in the treatment as compared to control GP and find no evidence of a long term cost on treatment households (Appendix Table A.5).

3 . MGNREGS projects

Another check of whether there was any decline in real activities is provided by looking at the infrastructure built under the program. Table 6 shows that the number of MGNREGS assets reported as having started (Column One) or being complete (Column Two) in the nrega.nic.in data base is the same in treatment and control (note the very low fraction of complete project relative to started project). We sent teams to the villages with a list of infrastructures to find. The fraction found is actually high (86%), and remains similar in the treatment and in the control blocks.

C Leakages: Over-reporting of days or Ghost workers?

Overall, our findings suggest that the 25% decline in MGNREGS spending in the treatment blocks does not reflect a drop in public employment provision, but rather reduced leakages of MGNREGS funds. The existing evidence reviewed in section II shows that corruption in MGNREGS administration is widespread, and available estimates suggest that more than 20% of MGNREGS funds are stolen by local officials. The local politicians we surveyed, who may themselves participate in the corruption nexus, often complain about corruption in the administration: in control blocks, 20% of them mentioned it spontaneously and 26% agreed that it was a main issue in MGNREGS implementation. Interestingly, the fraction of politicians who thought this was an issue is significantly lower, by 10 percentage point, in the treatment blocks (see Appendix Table A.4).

Since the reports in the nrega.nic.in data base decline proportionally to the decline in expenditures measured in CPSMS (table 3), and labor payments in that data base are directly linked to days worked by individual workers, we can directly investigate whether there are now fewer “ghost” workers (people who are reported to be paid but are non-existent, or exist but have never worked), or fewer over reporting of workdays (people who have worked under the scheme but for fewer days than what is reported).

The fund flow reform increases accountability: it now becomes easier to send an auditor to the field to verify that a particular person has been employed. However, while it is relatively easy to verify that someone indeed exists in the village (and is thus not a true “ghost”) or that someone who exists has in fact ever been employed (another form of “ghost” worker), it seems

that it would be much more difficult to obtain accurate, verifiable information on how many days someone worked, since this is all based on imperfect recall. Thus, we would expect this intervention to bring back a reduction in ghost workers but not in over-reporting of days.

Table 7 provides evidence on this issue, and suggests that the scheme led to a decline in the number of “ghost workers”, rather than over reporting of days for households who worked. Panel A reports the number of days reported to have been worked in the narega.nic.in data base: corresponding to what we find in Table 3, there is a significant decline in the number of days "worked" during the intervention period. Panel B shows, however, that the days worked per household in the data base does not decline: the entire decline is accounted for by a decline in the number of households that are reported to have worked (Panel C).

The next two panels provide more direct, if tentative, evidence. Recall that our matching is very partial: we only interviewed a sample of households and matching based on name leads to both inclusion and exclusion errors. However, these factors should be constant in treatment and in control. Hence, when the number of ghost workers decline, we should find a reduction in workdays for the households for whom we do not find a match. Indeed, Panel D shows that the decline in days worked is concentrated among job cards which were not matched with households in our survey. In contrast, Panel E shows that, among job cards which are indeed matched with households in our survey, there is no decline in the number of days reported (suggesting no change in over-reporting among real households).

These results strongly suggest that the decline in leakage comes from a reduction in ghost workers, rather than the over-reporting of days. In contrast, Muralidharan et al. (2014), which focused on a "front end" reform in payment, and found reduction in the over-reporting of days, not a reduction in ghost workers. In their context there was no reform in accountability and biometric identification was not imposed for all workers, so that opportunities for local officials to steal MGNREGS funds using ghost workers was unaffected. However, the over-reporting of days in the name of MGNREGS workers who used biometric identification became impossible without their consent. The two interventions are hence not only complementary in their design, but also in their effects: if combined they would close the two main sources of leakages of MGNREGS funds.

An important question is, why following the intervention, local officials did not make up for

the lost ghost workers by over-reporting more. It is possible that there is some limit to how much over-reporting of workdays can be done in the name of existing workers, e.g. because of the limited number of infrastructure projects carried out. Conversely, Muralidharan et al. (2014) do not find evidence of increased employment among ghost workers when over-reported days decline.

V Conclusion

This paper reports on a large-scale field experiment that evaluated a seven-months reform to the within-state fund flow system for MGNREGS – India’s federal workfare program. Our evaluation covered a population of 33 million in one of India’s poorest states – Bihar.

The introduction of an electronic funds management platform offered to the Bihar government an opportunity to pilot reforms to the MGNREGS fund flow. The reform linked fund flow to incurred expenditures and reduced the number of intermediaries involved in the process. The reform led to very significant reductions in funds leakages in treatment blocks: MGNREGS expenditures declined by 25% with no corresponding change in real outcomes.

To the extent that the reductions in expenditure reflects lower program leakage, we would expect earnings of some officials involved in fund flow for MGNREGS to be lowered. We explore this using survey data on assets of the GP head (Mukhiya) and assets of MGNREGS functionaries. While we are still finalizing data entry of the asset declaration by MGNREGS functionaries (at the GP and block level), Figure 7, 8 and 9 provides suggestive evidence. During the intervention (i.e. before they could accumulate more), the distribution of assets look similar in treatment and control (Figure 7). After the intervention, the distribution of asset appear to shift to the right in the treatment group (Figure 8). Quantile regression estimates presented in Figure 9 show that, up to the median, the quintile of the distribution of asset declines in the treatment group after the intervention, compared to the treatment group. The decline is about 20% for all those lower quantiles.¹⁹ The effect disappears at the higher quantiles, perhaps because at those higher levels, the assets cannot be explain any more by leakage in MGNREGS. The point estimate of the treatment on the average wealth reported is not significant (this may change with

¹⁹we present unclustered standard errors and block-bootstrapped standard errors—with an administrative block as the unit of block-bootstrapping

better data) but it is still 14%. We should also note that because the randomization was done within districts, our analysis precludes district officials who were arguably the biggest losers from the reform, which excluded them entirely from the wage funds.

On the flip side, the reform was not adequate to improve administrative ability to respond to villager needs—neither employment nor wages received by households rose and we find some evidence of increased payment delays for the first four months. The reform also likely increased administrative burdens on GP officials. The technical challenges with managing a computer-based system in areas with frequent electricity shortages and limited IT help should not be underestimated. In addition, lack of coordination between CPSMS and nrega.nic.in meant worker details had to be entered twice. As would be expected, the personnel costs, frustrations with lags in infrastructure rollout and dismay over a reformed system which reduced rents was well vocalized. In contrast, state officials in the capital city lacked information on whether the observed decline in expenditure reflected lower rent seeking or a genuine decline in employment provision. Thus, at the end of the fiscal year, state-level officials were well aware of the personnel and infrastructure costs of the program but had limited knowledge of the revenue benefits, which convinced them to roll back the intervention.

The reform also demonstrates that better cash management systems can achieve the same program outcomes with less financial resources, effectively reducing the overall cost of program implementation. However, this study only examines the implications of reducing parked funds at the GP level. It should be possible to implement similar cash management systems to eliminate parked funds at State, District and Block level agencies. These systems could be extended to cover beneficiary payments in a manner that allows implementation agencies to initiate beneficiary payments directly from a central pool of funds. Such cash management systems can completely eliminate the need to maintain parked funds at all levels of the programme implementation level, and support reduction in program costs at a much larger scale. The need for better cash management models is not exclusive to MGNREGS: the Government of India spends approximately Rs 3 trillion (\$50 billion) every year on Centrally Sponsored Schemes for which money is released to implementing agencies in lumpy instalments.

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Figure 1: Map of Sample Districts

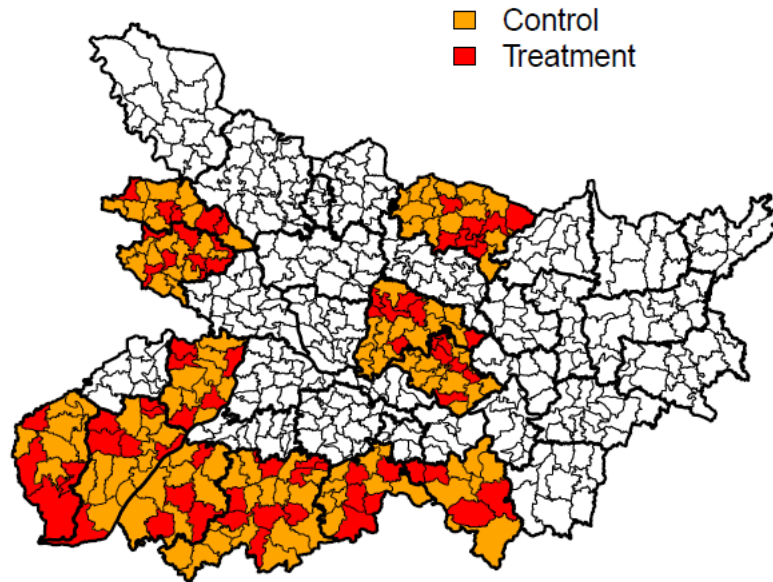


Figure 2: MGNREGS Fund-flow in Control Blocks

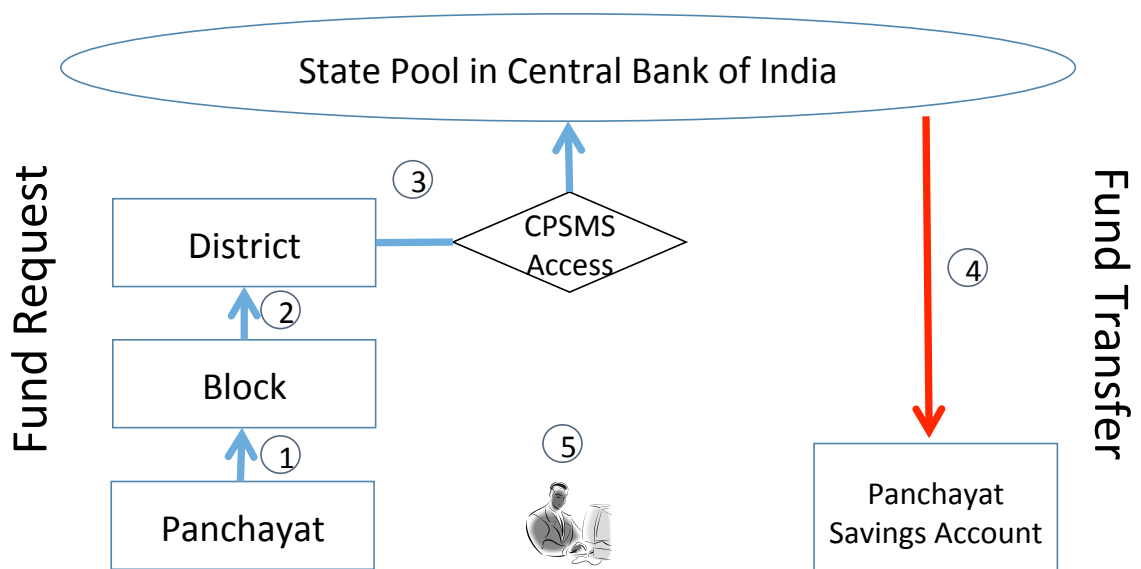


Figure 3: MGNREGS Fund-flow in Treatment Blocks

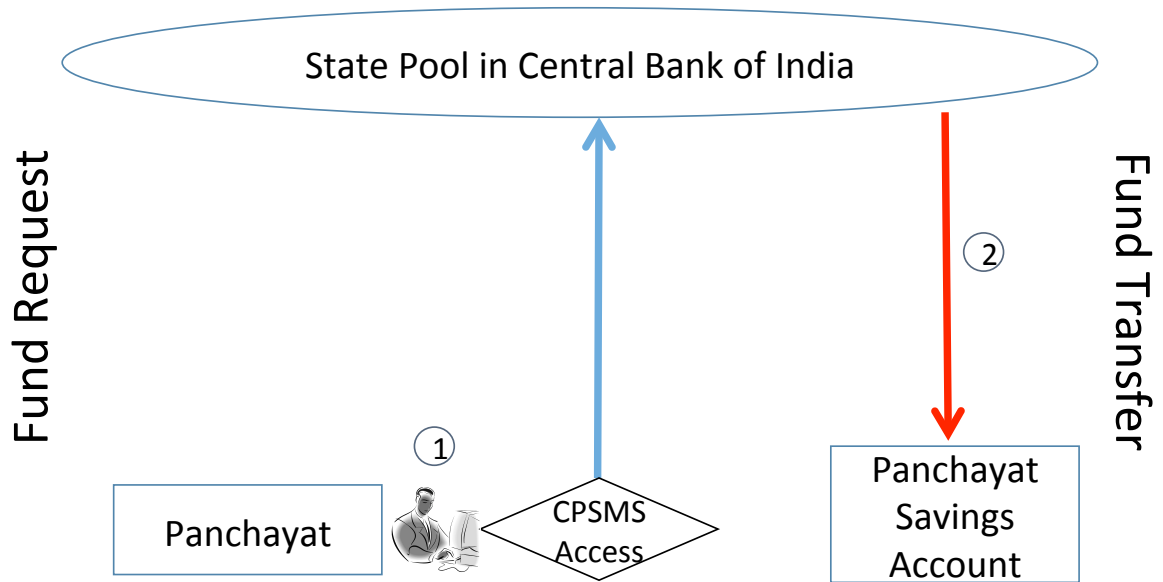


Figure 4: GP daily Expenditures on MGNREGS during the Study Period

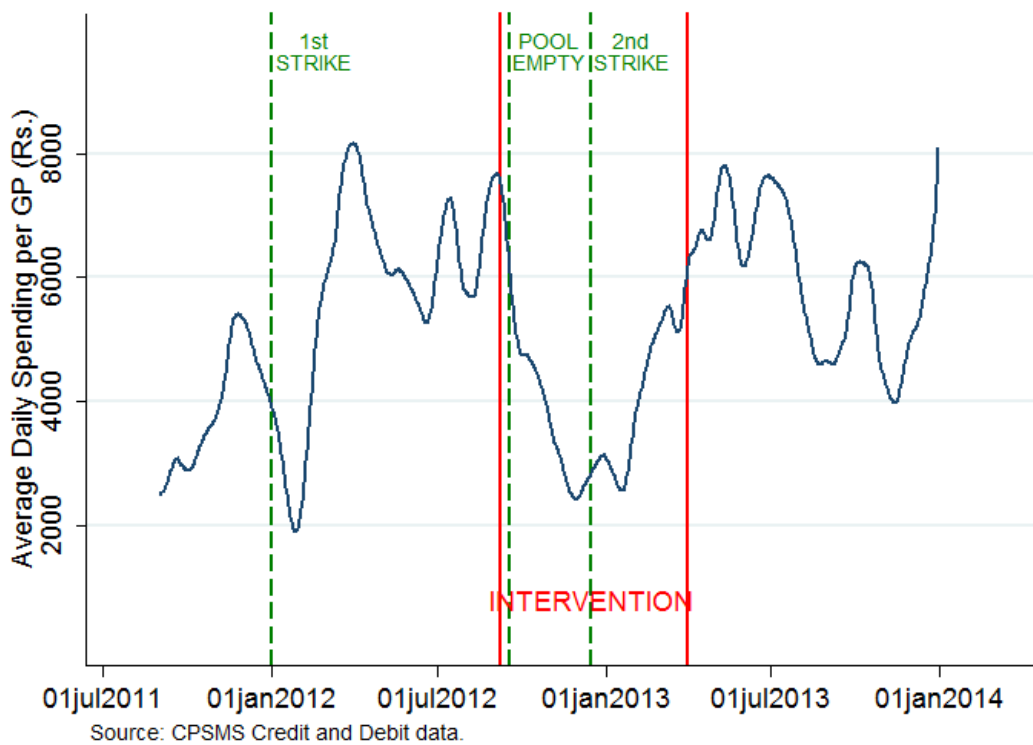


Figure 5: Fraction of Treatment GP which used CPSMS at least once

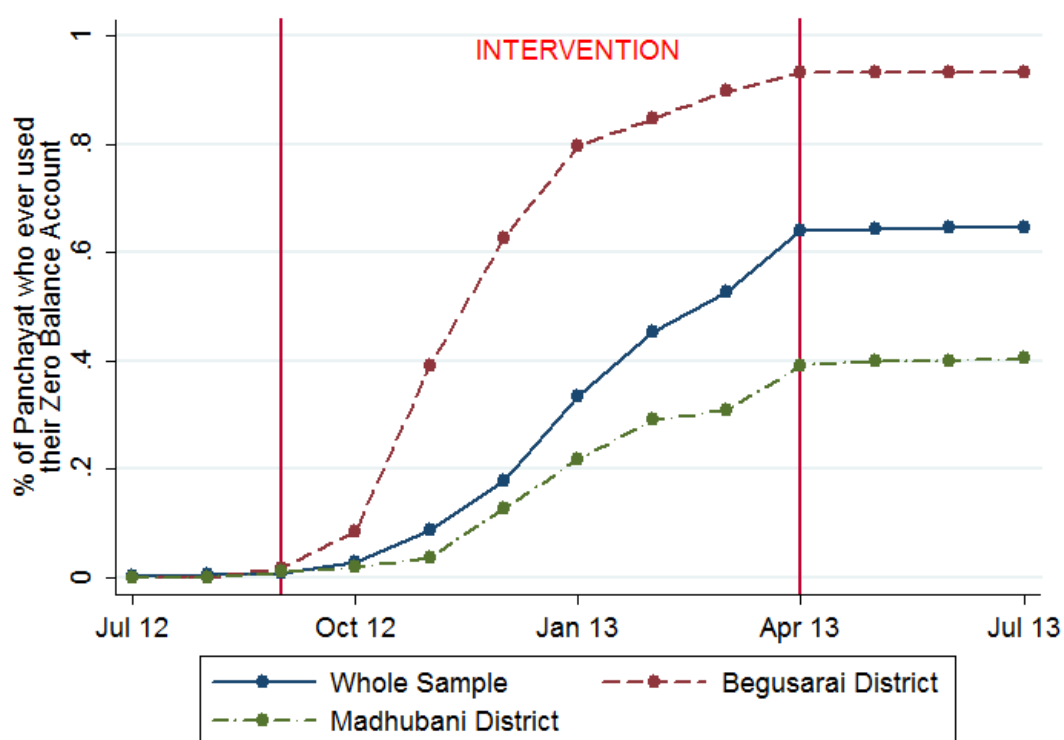
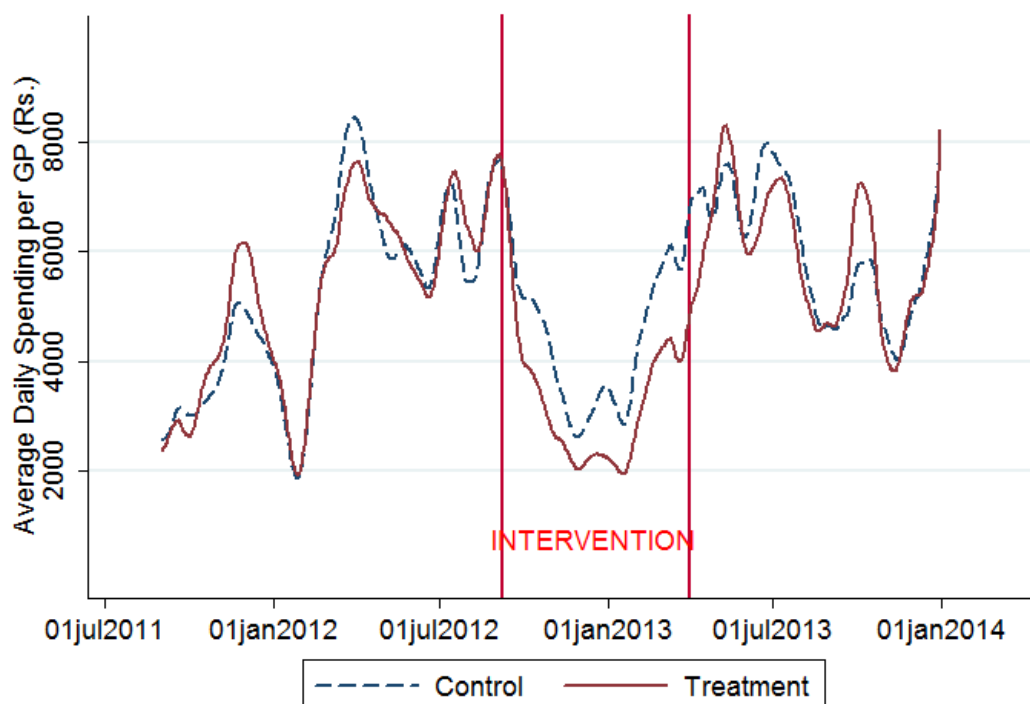
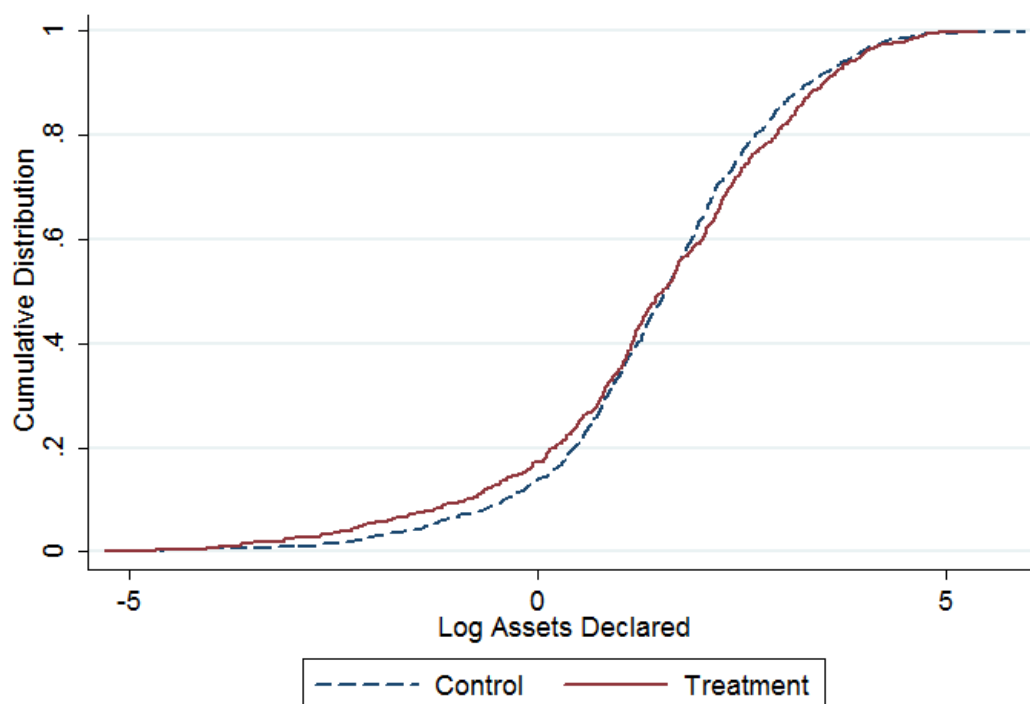


Figure 6: GP Daily Expenditures on MGNREGS in Treatment and Control Blocks



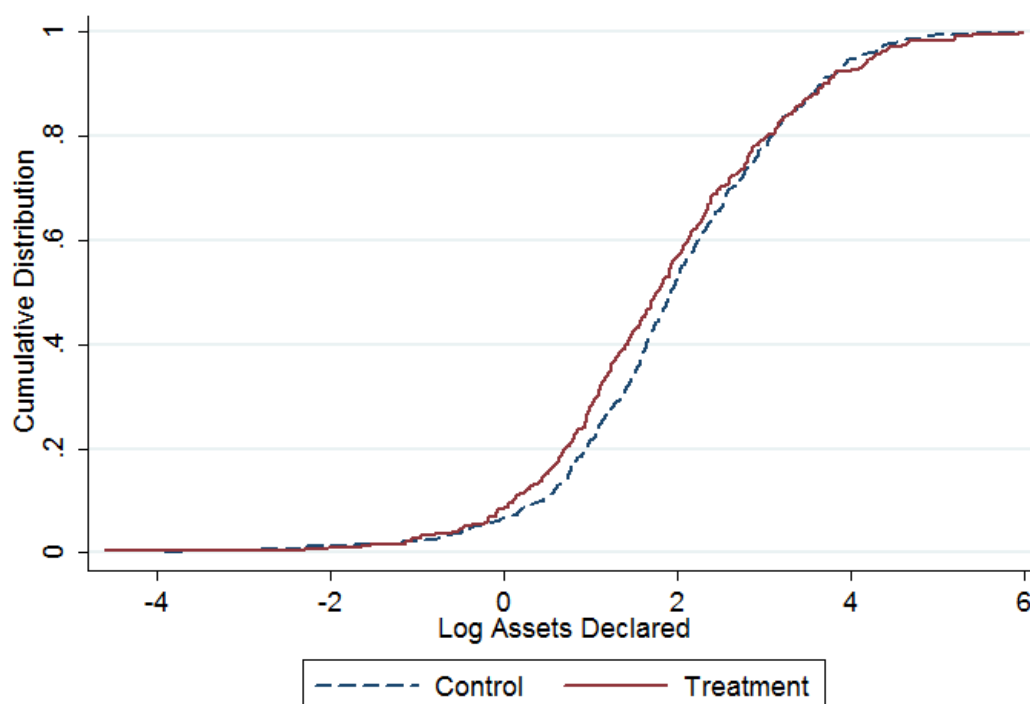
Source: CPSMS Credit and Debit data.

Figure 7: Asset of MGNREGS functionaries: during the intervention



Source: Annual Declaration of MGNREGS employees 2012-13, Government of Bihar.

Figure 8: Asset of MGNREGS functionaries: after the intervention



Source: Annual Declaration of MGNREGS employees 2013-14, Government of Bihar.

Figure 9: Quantile Treatment Effect on GP and Block functionaries after the intervention

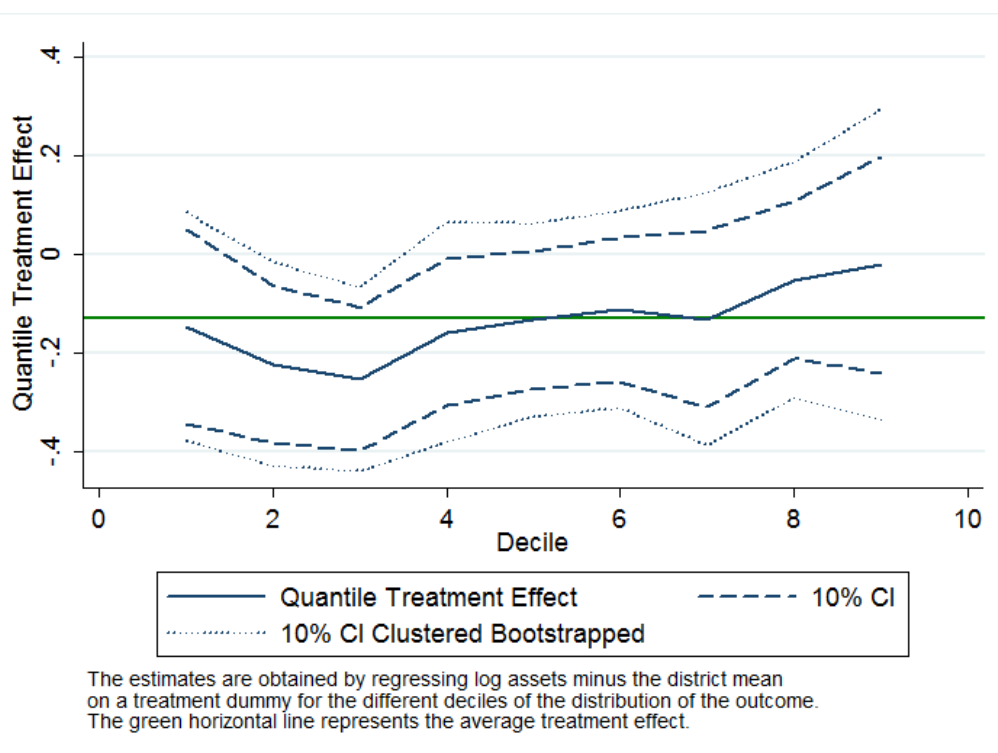
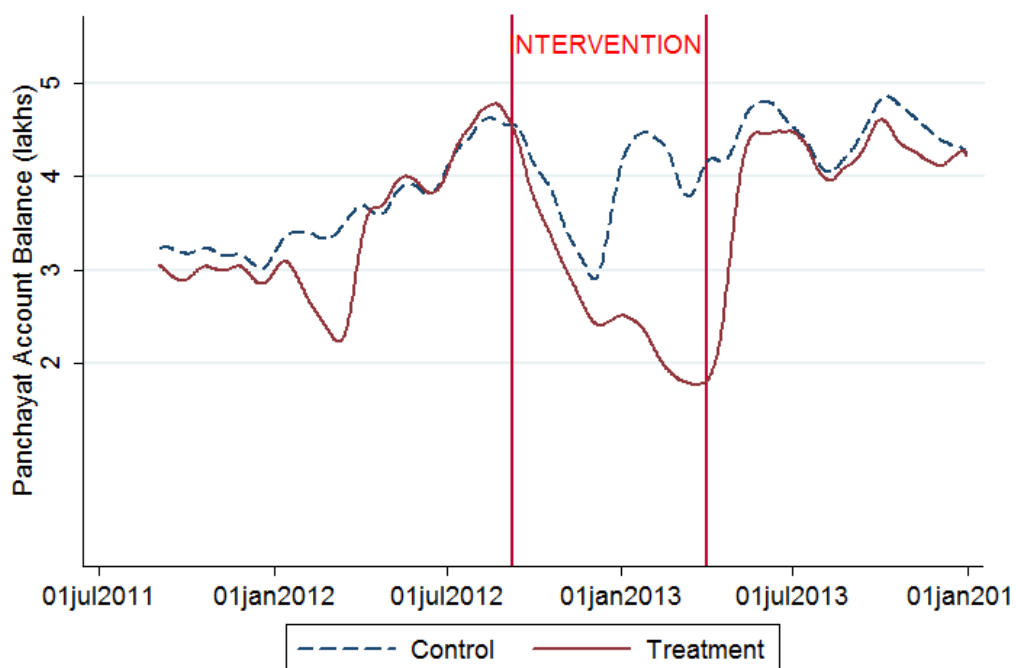


Figure 10: Treatment effect on the balance of GP accounts



Source: CPSMS Credit and Debit data.

Table 1: Balance test

Panchayat Characteristics	Source	Control Blocks	Treatment Blocks	Difference
Superficy (hectares)	2001 Census	1582	1617	34.95
Number of households	2001 Census	1302	1276	-26.21
% SC Population	2001 Census	0.248	0.247	-0.000538
% ST Population	2001 Census	-0.000679	0.0035	0.00418
Literacy Rate	2001 Census	0.554	0.547	-0.00675
% Population in village with education facility	2001 Census	0.233	0.206	-0.0273
% Population in village with medical facility	2001 Census	0.0569	0.0503	-0.00666
% Population in village with post office	2001 Census	0.0341	0.0362	0.00209
% Population in village with bank branch	2001 Census	0.0155	0.0138	-0.00162
% Population in village with electricity supply	2001 Census	0.0337	0.00721	-0.0265*
% Land Irrigated	2001 Census	0.108	0.1	-0.00720
Political reservation for women	State Election Commission	0.463	0.447	-0.0155**
Political reservation for Other Backward Caste	State Election Commission	0.177	0.169	-0.00833
Political reservation for Scheduled Caste	State Election Commission	0.238	0.241	0.00371
Political reservation for Scheduled Tribe	State Election Commission	-0.000774	0.00155	0.00232
MGNREGS beneficiary households	nrega.nic.in (Apr 2011-Mar 2012)	243.2	253.3	10.16
MGNREGS work days provided	nrega.nic.in (Apr 2011-Mar 2012)	9066	9485	418.9
MGNREGS labor expenditures (lakhs)	nrega.nic.in (Apr 2011-Mar 2012)	6.307	7.343	1.036*
MGNREGS material expenditures (lakhs)	nrega.nic.in (Apr 2011-Mar 2012)	7.073	7.645	0.572
MGNREGS panchayat spending	CPSMS (Jul 2011-Mar 2012)	16.11	15.95	-0.153
Number of Panchayat		1953	1003	

Note: The unit of observation is a Panchayat. The difference between control and treatment blocks is estimated using a regression of each Panchayat characteristic on a dummy equal to one for treatment blocks and district fixed effects. Standard errors are clustered to take into account correlation at the block level. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table 2: Treatment effect on MGNREGS spending (CPSMS)

	Before the intervention	During the intervention			After the intervention
	July 2011 - Sep 2012	Sept-Dec 2012	Jan - Mar 2013	Whole Period	Apr 2013 - Jan 2014
	(1)	(2)	(3)	(4)	(5)
Panel A: Total Credit to Panchayat Accounts					
Treatment	0.164 (1.093)	-2.199*** (0.376)	-1.284*** (0.339)	-3.471*** (0.557)	0.857 (0.964)
Observations	2,918	2,918	2,917	2,919	2,766
Mean in Control	19.47	5.102	3.984	9.079	16.69
Effect as % of Control Mean	0.842	-43.11	-32.24	-38.23	5.135
Panel B: Average Balance in Panchayat Accounts					
Treatment	-0.0811 (0.129)	-0.436** (0.190)	-1.577*** (0.213)	-1.005*** (0.187)	-0.347 (0.244)
Observations	2,918	2,918	2,917	2,919	2,766
Mean in Control	3.639	3.719	4.174	3.945	4.493
Effect as % of Control Mean	-2.227	-11.71	-37.78	-25.47	-7.724
Panel C: Total Debit from Panchayat Accounts					
Treatment	-0.360 (0.961)	-1.034*** (0.322)	-1.300*** (0.283)	-2.324*** (0.540)	-0.449 (0.974)
Observations	2,918	2,918	2,917	2,919	2,766
Mean in Control	18.38	5.367	4.126	9.487	16.71
Effect as % of Control Mean	-1.959	-19.26	-31.51	-24.50	-2.685

Note: The unit of observation is a Panchayat. In Panel A the dependent variable is the sum of credits made to the savings account of each Panchayat for each period (in lakhs Rupees). In Panel B the dependent variable is the average balance on the savings account of each Panchayat during each period (in lakhs Rupees). In Panel C the dependent variable is the sum of debits from the savings account of each Panchayat for each period (in lakhs Rupees). Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects. Standard errors are clustered at the block level.

Table 3: Treatment effect on MGNREGS spending (nrega.nic.in)

Expenditure items	Annual Panchayat Expenditures from nrega.nic.in					
	Apr 2011-Mar 2012		Apr 2012-Mar 2013		Apr 2013-Mar 2014	
	Labor (1)	Material (2)	Labor (3)	Material (4)	Labor (5)	Material (6)
Treatment	0.996** (0.495)	0.508 (0.432)	-2.270*** (0.760)	-1.077** (0.526)	-0.271 (0.729)	0.315 (0.534)
Observations	2,950	2,950	2,947	2,947	2,954	2,954
Mean in Control	7.551	6.504	13.83	7.717	13.66	8.377
Effect as % of Control Mean	13.19	7.807	-16.42	-13.96	-1.980	3.758

Note: The unit of observation is a Panchayat. The dependent variables are expenditures from MIS reports for financial years 2011-12, 2012-13, 2013-14 (in lakhs Rupees). Data was downloaded from the MGNREGS website (nrega.nic.in) in November 2014. The intervention started in September 2012 and ended on March 31st, 2013. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects. Standard errors are clustered at the block level.

Table 4: Treatment effect on MGNREGS employment (household survey)

	Before intervention	Intervention Period			Since Intervention
	Jul - Aug 2012 (1)	Sept-Dec 2012 (2)	Jan-Mar 2013 (3)	Whole Period (4)	Apr - Jun 2013 (5)
Panel A: MGNREGS Participation					
Treatment	-0.00673*** (0.00240)	0.000417 (0.00391)	0.00188 (0.00304)	0.00225 (0.00528)	0.00355 (0.00505)
Observations	9,969	9,969	9,969	9,969	9,969
Mean in Control	0.0124	0.0217	0.0174	0.0378	0.0391
Effect as % of Control Mean	-54.47	1.921	10.80	5.966	9.078
Panel B: Number of weeks worked					
Treatment	-0.00697 (0.0227)	0.0141 (0.0308)	0.0260 (0.0323)	0.0402 (0.0564)	0.00837 (0.0330)
Observations	9,969	9,969	9,969	9,969	9,969
Mean in Control	0.0853	0.151	0.172	0.324	0.184
Effect as % of Control Mean	-8.176	9.335	15.10	12.41	4.558
Panel C: Number of days worked					
Treatment	-0.153*** (0.0493)	0.0627 (0.147)	0.138 (0.139)	0.200 (0.225)	0.0873 (0.359)
Observations	9,969	9,969	9,969	9,969	9,969
Mean in Control	0.231	0.676	0.515	1.192	1.825
Effect as % of Control Mean	-66.47	9.266	26.69	16.80	4.786

Note: The unit of observation is a household. In Panel A the dependent variables is a dummy variable which is equal to one if any household member participated to MGNREGS. In Panel B the dependent variable is the total number of weeks worked by household members under MGNREGS. In Panel C the dependent variable is the total number of days worked by household members. The data was collected by a representative survey of 10,036 households in May-July 2013. Households were asked about work spells from July 2012 to the time of the survey. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects and household controls. Household controls include sets of dummies for religion, caste, type of housing, land ownership, gender and literacy of the household head, household size and number of adults.

Table 5: Treatment effect on MGNREGS payments (household survey)

	Before	Intervention Period			Since
	Intervention				Intervention
	Jul - Aug	Sept - Dec	Jan - Mar	Whole Period	Apr - Jun
	2012	2012	2013		2013
	(1)	(2)	(3)	(4)	(5)
Panel A: Wages received for MGNREGS employment					
Treatment	-18.06*** (5.854)	-6.821 (15.09)	6.106 (12.62)	-0.716 (22.44)	-25.03 (26.75)
Observations	10,036	10,036	10,036	10,036	10,036
Mean in Control	24.33	59.88	43.54	103.4	113.5
Effect as % of Control Mean	-74.22	-11.39	14.02	-0.692	-22.06
Panel B: Average delays in payment (days)					
Treatment	-18.54 (23.43)	44.09*** (15.31)	10.92 (8.770)	28.19*** (10.33)	-1.798 (6.104)
Observations	123	218	175	379	383
Mean in Control	73.44	72.61	45.15	60.12	38.41
Effect as % of Control Mean	-25.24	60.73	24.19	46.90	-4.682
Panel C: Illegal advance payments					
Treatment	-0.0488 (0.136)	-0.0163 (0.0802)	0.0625 (0.0912)	0.00235 (0.0590)	0.0436 (0.0565)
Observations	104	176	143	309	250
Mean in Control	0.394	0.273	0.294	0.291	0.380
Effect as % of Control Mean	-12.38	-5.989	21.29	0.805	11.47

Note: The unit of observation is a household. In Panel A The dependent variable is total wage payments received by each household for MGNREGS employment. In Panel B the dependent variable is the average number of days between the time of work spells and the time of each payment. When payments have not been made at the time of the survey, the delay is set equal to the time between the work spell and the survey date. In Panel C the dependent variable is a binary variable which is equal to one if any household member has received a payment for MGNREGS work in cash within 15 days of the work spell. The data was collected by a representative survey of 10,036 households in May-July 2013. Households were asked about all work spells since July 2012. The intervention period is Sept 1st 2012-March 31st 2013. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects and household controls. Household controls include sets of dummies for religion, caste, type of housing, land ownership, gender and literacy of the household head, household size and number of adults.

Table 6: Treatment effect on MGNREGS infrastructures

	Number Registered (1)	Number Completed (2)	Fraction Found (3)
Treatment	0.0494 (0.263)	0.372 (0.342)	0.0176 (0.0176)
Observations	390	390	4,165
Mean in Control	13.82	2.126	0.855
Effect as % of Control Mean	0.357	17.48	2.057

Note: In column one and two the unit of observation is a Panchayat. In column three the unit of observation is a MGNREGS infrastructure projects. The dependent variables are the number of projects registered in the MIS (nrega.nic.in) (1), the number of projects declared as complete in the MIS (2), and the fraction of assets sampled which were actually found by surveyors (3). Out of 5391 projects registered in nrega.nic.in, a random sample of 4165 projects were surveyed. All specifications include district fixed effects.

Table 7: Treatment effect on MGNREGS employment reported in nrega.nic.in

	MGNREGS days worked per household		
	Before intervention (1)	Intervention Period (2)	Since Intervention (3)
Panel A: Days worked (nrega.nic.in)			
Treatment	-245.8 (340.5)	-692.8* (364.1)	-890.2 (543.4)
Observations	2,941	2,941	2,941
Mean in Control	4956	5008	10567
Effect as % of Control Mean	-4.959	-13.83	-8.424
Panel B: Days per working household (nrega.nic.in)			
Treatment	-0.578 (0.797)	0.0530 (0.935)	-0.116 (0.841)
Observations	2,874	2,856	2,930
Mean in Control	28.54	33.57	40.29
Effect as % of Control Mean	-2.025	0.158	-0.288
Panel C: Number of working households (nrega.nic.in)			
Treatment	0.853 (9.940)	-14.20* (8.141)	-15.16 (9.941)
Observations	2,941	2,941	2,941
Mean in Control	168.2	139.6	249.5
Effect as % of Control Mean	0.508	-10.17	-6.079
Panel D: Days worked by household not matched with survey (nrega.nic.in)			
Treatment	-250.5 (336.8)	-704.0* (360.1)	-910.8* (534.4)
Observations	2,941	2,941	2,941
Mean in Control	4896	4954	10440
Effect as % of Control Mean	-5.117	-14.21	-8.725
Panel E: Days worked by household matched with survey (nrega.nic.in)			
Treatment	0.764 (30.66)	-3.829 (62.09)	27.98 (67.96)
Observations	372	372	372
Mean in Control	89.89	456.4	436.1
Effect as % of Control Mean	0.850	-0.839	6.416

Note: The unit of observation is a Panchayat. In Panel A the dependent variable is the total number of days provided. In panel B the dependent variable is the total number of days provided to households reported to have worked. In panel C the dependent variable is the number of households reported to have worked. In panel D the dependent variable is the number of days worked by households who could not be matched with survey households. In Panel E the dependent variable is the number of days worked by households matched with survey households. The data was extracted from Job card information on the nrega.nic.in server. It covers the period from July 2011 to Sept 2013. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects.

APPENDIX: FOR ONLINE PUBLICATION ONLY

Data Appendix

In this appendix, we describe the different sources of information we use in the analysis. We first present the official data on expenditures and employment, and then turn to the surveys we implemented to assess actual MGNREGS implementation.

We use two sources of official reports on MGNREGS expenditures and employment: CPSMS and nrega.nic.in.

CPSMS: In July 2014, we were granted access to detailed information MGNREGS expenditures via the Central Planning Scheme Monitoring (CPSMS) Portal. Both treatment and control GP were monitored in the system from July 2011 onward, and we could observe all credit and debit transactions from GP savings account. We use this information to compute MGNREGS spending per GP for the different periods of interests: from July 2011 to the start of the intervention in September 2012, from September 2012 to December 2012, from January 2013 to March 2013 and from the end of the intervention in April 2013 until July 2014.

NREGA.NIC.IN: The government website nrega.nic.in provides publicly available information on MGNREGS expenditures per GP for every financial year (a financial year start on April 1st). In July 2014, using a newly available facility called the Public Data Portal (jointly produced by the Ministry of Rural Development and Evidence for Policy Design) we downloaded data on GP spending on labor and material for the financial years 2011-12, 2012-13 and 2013-14. Labor expenditures figures in nrega.nic.in are aggregates of work and payment details of each MGNREGS worker which is also entered on the website and made publicly available in the form of job cards. We requested access to job card information from the Ministry of Rural Development and were provided with all job card details of workers in our sample districts for the financial years 2011-12, 2012-13 and 2013-14.

In order to provide independent measures of MGNREGS implementation, we carried out our own survey in the 12 sample districts between May and July 2013. Within each district, we visited every block – in total, we had 69 treatment blocks and 126 control blocks, 195 blocks in total. We surveyed 2 randomly sampled GP s in each block – this gave us a total of 390 GP s.

The survey consisted of three main surveys: a household survey, a survey of MGNREGS assets and a survey of GP head (or Mukhiya).

Household Survey: We have a household survey covering 10,036 households. In each GP, we covered at least 25 households. These households were sampled from the list of households obtained from the District Rural Development Authority (DRDA). These lists were initially compiled in 2002 for the purpose of identifying BPL households, so each household was given a poverty score, based on various criteria. From these lists, we sampled 66 per cent of households below the median poverty score and 33 per cent households from above the score. In the case a sampled household had left the village or all its members were defunct, surveyors were asked to interview a replacement household who had been randomly chosen from the initial list. Because the sampling lists were 10 years old and many areas had high migration rates, the proportion of households interviewed as replacements was also high, about 30%.

In order to compare MGNREGS employment in the survey data and in official reports, we matched survey households and nrega.nic.in job cards by name, gender and age of each household member. A survey household and a job card were considered as a match if any household member matched. Because the same name is frequently shared by many individuals in the same GP, our matching is very fuzzy: on average one survey household was matched with five job cards.

Asset Survey: We sampled 10 infrastructure projects from each GP. These were randomly sampled from the MIS (www.nrega.nic.in). In total, we sampled a total of 4165 infrastructure projects.

Mukhiya Survey: We attempted to interview the Mukhiya of every single GP we visited. We managed to locate and interview a total of 358 Mukhiyas. Unlike the other two surveys, the Mukhiya survey was conducted on paper and was both quantitative and qualitative in nature.

Table A.1: Infrastructure availability

<i>Infrastructure</i>	July '12		Jan '13	Apr '13		Required
	T	C	T	T	C	
Computers (Number)	1.32	1.06	2.48	2.06	1.61	3
Operators (number)	1.22	0.86	2.20	1.75	1.27	3
Generator (1=Yes 0=No)	0.67	0.56	0.97	0.90	0.85	1
Internet (1=Yes 0=No)	0.38	0.33	0.85	0.71	0.60	1
Scanner (1=Yes 0=No)	0.57	0.37	0.73	0.81	0.65	1
Printer (1=Yes 0=No)	0.59	0.43	0.71	0.83	0.76	1
Sampled Blocks	69	126	66	69	123	

Source: Phone surveys of Block Level MGNREGS functionaries (Program officers). The intervention started in September 2012 and ended in April 2013. "T" denotes treatment blocks and "C" denotes control blocks.

Table A.2: MGNREGS Spending levels from different data sources

Panel A		Control	Treatment	Difference	Pvalue
Debit in CPSMS					
	2012-13	19.27	16.84	-2.43	0.11
	2013-14	16.99	16.32	-0.67	0.65
Total Expenditures in MIS					
	2012-13	21.66	18.27	-3.38	0.05
	2013-14	21.48	21.27	-0.21	0.90
Difference CPSMS-MIS					
	2012-13	-2.39	-1.44	0.95	0.15
	2013-14	-4.49	-4.95	-0.46	0.63
Panel B		Control	Treatment	Difference	Pvalue
Payments in Job Cards					
	2011-12	8.30	9.26	0.96	0.24
	2012-13	15.74	14.25	-1.49	0.29
	2013-14	16.27	14.61	-1.66	0.26
Labor Expenditures in MIS					
	2011-12	7.59	9.04	1.45	0.08
	2012-13	13.91	11.66	-2.26	0.06
	2013-14	13.23	12.83	-0.41	0.71
Difference Job Cards-MIS					
	2011-12	0.71	0.22	-0.49	0.21
	2012-13	1.82	2.59	0.77	0.03
	2013-14	3.03	1.78	-1.25	0.02

Source: CPSMS Credit Debit Data, MIS Financial Reports (nrega.nic.in), Job Cards (nrega.nic.in). All amounts are annual panchayat averages in lakhs. CPSMS data is not available for the whole financial year 2011-12. p-values take into account correlation of errors at the block level. Years are financial years (Apr 1st-Mar 31st).

Table A.3: Household participation in MGNREGS (household survey)

	Household Participation in MGNREGS	
	Anytime before (1)	Since July 2012 (2)
Treatment	-0.0161 (0.0136)	0.000842 (0.00861)
Observations	10,018	10,007
Mean in Control	0.288	0.0936
Effect as % of Control Mean	-5.608	0.899

Note: The unit of observation is a household. In Column one the outcome is a binary variable equal to one if any member of the household worked for MGNREGS in the past. In Column Two the outcome is a binary variable equal to one if any member of the household did MGNREGS worked since July 2012. The data was collected by a representative survey of 10,036 households in May-July 2013. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects and household controls. Household controls include sets of dummies for religion, caste, type of housing, land ownership, gender and literacy of the household head, household size and number of adults.

Table A.4: MGNREGS Implementation issues reported by the GP head (Mukhiya survey)

	Main issue in MGNREGS implementation mentioned by the Mukhiya		
	Spontaneously	When Prompted	Either
	(1)	(2)	(3)
Panel A: Lack of funds from the government			
Treatment	0.0500	-0.0473	0.00273
	(0.0589)	(0.0467)	(0.0494)
Observations	346	346	346
Mean in Control	0.489	0.229	0.718
Effect as % of Control Mean	10.23	-20.63	0.381
Panel B: Corruption in the administration			
Treatment	-0.0377	-0.0656	-0.103*
	(0.0438)	(0.0501)	(0.0567)
Observations	346	346	346
Mean in Control	0.207	0.264	0.471
Effect as % of Control Mean	-18.22	-24.81	-21.91
Panel C: CPSMS fund-flow creates delays			
Treatment	0.127***	0.0513	0.179***
	(0.0443)	(0.0356)	(0.0525)
Observations	346	346	346
Mean in Control	0.0749	0.0925	0.167
Effect as % of Control Mean	170.1	55.47	106.7

Note: The unit of observation is a Mukhiya (head of Panchayat). The dependent variables are the fractions of Mukhiya who declared that the lack of funds from the government (panel A) corruption in the administration (panel B) and delays in fund-flow created by CPSMS (panel C) are important issues in MGNREGS implementation. The data was collected from a representative sample of 354 Mukhiya from treatment and control blocks in May-July 2013. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects and Mukhiya controls. Mukhiya controls include sets of dummies for Mukhiya's Religion, caste, gender, education (university education), age (above 42), whether any member of the family was elected Mukhiya in 2001 and 2006.

Table A.5: Treatment effect on household consumption

	Log Monthly Consumption			
	All	Frequent expenditures	Recurrent expenditures	Rare expenditures
	(1)	(2)	(3)	(4)
Treatment	-0.00764	-0.00788	-0.0400	0.00104
	(0.0212)	(0.0163)	(0.0261)	(0.0393)
Observations	10,033	10,032	10,016	10,009

Note: The dependent variable are the log of household monthly expenditures for different categories of expenditures. Frequent expenditures are expenditures reported every week. Recurrent expenditures are reported every month. Rare expenditures are reported over the past five months. The data was collected by a representative survey of 10,036 households in May-July 2013. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects and household controls. Household controls include sets of dummies for religion, caste, type of housing, land ownership, gender and literacy of the household head, household size and number of adults.