E-governance, Accountability, and Leakage in Public Programs: Experimental Evidence from a Financial Management Reform in India

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Can e-governance reforms improve government policy? By making information available on a real-time basis, information technologies may reduce the theft of public funds. We analyze a large field experiment and the nationwide scale-up of a reform to India's workfare program. Advance payments were replaced by "just-in-time" payments, triggered by e-invoicing, making it easier to detect misreporting. Leakages went down: program expenditures dropped by 24%, while employment slightly increased; there were fewer fake households in the official database; program officials' personal wealth fell by 10%. However, payment delays increased. The nationwide scale-up resulted in a persistent 19% reduction in program expenditure.
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When implementing policies at scale, governments run into a host of seemingly mundane logistical issues: how to transfers funds, how to procure goods, how to transport or store them, etc. Economists have only recently begun to pay attention to these types of problems, even though they can be important bottlenecks to effective government intervention. In recent years, policy makers have invested in " e-government" reforms, which harness new technologies (the internet, mobile phones, biometric identification, etc.) to improve their ability to effectively deal with these issues. But we still have little evidence on how promising these reforms are.

One prominent example is the transfer of money used to pay for government programs. When a program (a school, a health clinic, a workfare program, etc.) is funded centrally but implemented locally, what is the best way to transfer funds from the center to the local implementing body? The standard practice is to disburse advances, and require the local authority to submit ex-post receipts proving that they have spent the money. But it is notoriously difficult for the central governments to get complete accounting of what has been spent. This opens a wide door for fund " leakage" – when a substantial share of the money allocated to development expenditures never reaches the intended beneficiaries, and is instead siphoned along the way by the officials in charge of the program, or even by those in charge of monitoring them (Reinikka and Svensson, 2004).

In recent years, many governments have been attracted by the promise of digital financial platforms to reduce this leakage. By enabling " just-in-time" financing, such platforms can end the need for advance funds disbursement. Funds are only disbursed in response to a specific invoice, which makes it much easier to verify that they are used as claimed; all auditors have to do is to check with the final beneficiary named in the invoice that they have been paid. This can reduce leakage, and lower expenditures on monitoring. As a result, governments worldwide have invested in Integrated Financial Management Information Systems – the World Bank, for instance, has financed 87 such projects in 51 countries as of 2011 (Dener, Watkins and Dorotinsky, 2011).[1] Yet, the success of such programs in reducing fund leakage remains largely unexamined. We also know little about how, if at all, such reforms benefit potential program participants.

In this paper, we examine these questions in the context of the world's largest workfare program: India's Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS). Operational since 2006, the MGNREGS guarantees every rural household 100 days of unskilled manual labor per year at the stipulated minimum wage. The program has been suffering from widespread leakage, in particular through the creation of " ghost workers" : people paid for their work but who do not exist in reality. Starting in 2012, Indian states began reforming the fund-flow system underlying MGNREGS. We take advantage of this period of reform and provide two pieces of evidence on the impact of " just-in-time" financing. First, we report on an experiment from the Indian state of Bihar, where

[1]Early reform examples include Mexico's SIAFF in 1997 and Italy's SIPA in 2001 (Barbatz, 2013)

we worked with the state government to randomize the introduction of the fund flow reform across 195 blocks. This experiment was unusually large and spanned a population of 33 million, including six million registered MGNREGS workers, but was implemented only for seven months between September 2012 and March 2013. Next, we provide longer-run quasi-experimental evidence from the " just-in-time" financing reform that was rolled out across Indian states between 2012 and 2015. By examining the short- and long-run impacts of fund flow reform on program expenditures, we are able to compare the impact of the experimental reform to a similar intervention implemented permanently and at scale.

In the status quo, the local body (the Gram Panchayat, or GP) receives an advance then uses it to organizes worksites and pay for salaries and materials. When the GP runs out of money, local officials send a new advance request, which has to be ratified by two higher levels of the hierarchy. Finally, at some later date, they justify the utilization of their past advances, by manually entering the paper-based worker attendance register into an electronic data base.

In the reformed system, the GP receives no advance for wage payment. Fund release into the GP account is automatically triggered when GP officials enter worker details in the financial database and requested funds to pay their salary. No approval from intermediate levels of hierarchy is required for fund release.

This reform enables a more effective audit process. Specifically, the requirement that each fund request be accompanied by a list of beneficiaries eliminates the several-months lag between the fund transfer and when the auditor can check the names of those purportedly paid the wages. It also reduces the involvement of the intermediate administrative tiers in the fund release process. However, it introduces an additional data entry burden on the GP officials, who need to enter the data before their workers can get paid, and puts the workers at the receiving end of any delay in data entry or processing of the fund requests.

Easier and timelier audits per se should improve monitoring of the local officials and, thereby, minimize leakages. However, reducing the monitoring powers of higher up officials (and hence their ability to force the local officials to share their earnings) could perversely go the other way. Specifically, under the status quo system, the negative incentive effect of having multiple people preying on them potentially weakens the incentive of local officials to embezzle funds, because more would have to be shared with higher level officials. Being " taxed" less by their superiors may cause local officials to steal more. The effect on fund leakage is thus theoretically ambiguous. The effect on the welfare of beneficiaries is ambiguous as well: to the extent the program eliminated delays and uncertainty in fund transfers and improved the utilization of available funds, it could have increased the availability of jobs; to the extent that GP officials struggled to handle the new administrative requirements, it could reduce job availability and increase delays in payment.

We begin by analyzing the experiment. To identify the impact of the reform, our experimental evaluation triangulates across multiple data sources. While no single piece of evidence is a sufficient statistic, together they paint a clear picture that the reform reduced leakage, without adversely affecting real employment under the program.

Specifically, administrative data on daily GP finances shows a 24 percent expenditure reduction in treatment GPs relative to control GPs. This decline is corroborated by expenditure data reported in the MGNREGS public access database. In contrast, an independent household survey shows that the number of beneficiaries, wages, and projects built were unaffected, suggesting that fund leakages declined, *not* actual program outcomes. In fact, household surveys show a 25 percent increase in self-reported program participation (significant at the 10 percent level).[2] However, the treatment significantly increased the delay with which households were paid.

Two pieces of direct evidence show that there was a reduction in leakage in our experiment. First, we identify " ghost worker" households, (i.e. households that are listed as having worked in the program's public database but are absent from the village population census), by matching over six million names of reported beneficiaries from the program database with 34 million names from the 2012 Socio-Economic CasteCensus.[3] We find a 5 percent reduction in the fraction of unmatched single-worker households in treatment GPs, relative to control GPs, suggesting that there were fewer invented households. Second, we find suggestive evidence that the program reduced the disclosed wealth of public officials involved with MGNREGS. All public officials above a certain rank working on MGNREGS were required to disclose their assets. At the end of the reform period, MGNREGS official median reported wealth is 14 percent lower in treatment relative to control areas. Similarly, mean wealth is 10 percent lower, but this estimate is noisier.

This experiment was abruptly and unexpectedly stopped in Bihar after seven months. This was due to the intense pressure exerted on the State authority by the MGNREGS officials who had obvious reasons to resent the program. Our impression at the time (one of us was in the Indian Administrative Service, posted in Delhi) was that the lobbying was effective because the only visible data about the experiment (before the household survey was completed and analyzed) was administrative data, which showed a decline in expenditure and an increase in payment delays. The district officials could easily argue that the evidence suggested that the new requirements were destroying the program. However, in 2015, partly based on the full experimental results showing that the reduction in expenditures was stemming from a reduction in leakage, not a lower level of delivery of the program, the Ministry of Rural Development was able to successfully argue for a nationwide roll out of a financial-fund flow reform that linked worker

[2]Low MGNREGS participation during our study period implies that this, however, represents a very small absolute increase in number of household participating.

[3]This matching strategy implements at scale the audit approach pioneered by Niehaus and Sukhtankar (2013) and also used by Muralidharan, Niehaus and Sukhtankar (2016), where investigators physically track down workers reported in the public database.

payments to electronic invoicing by GP officials.[4]

Prior to the national roll out, this reform was implemented in a staggered fashion, with districts progressively adopting it. This staggered implementation between 2012 and 2015 forms the basis of our long-run evaluation of the reform. While we have more limited data on the impact of the national rollout, there is value in asking whether the national reform led to a similar decline in program expenditure. Using a difference-in-differences strategy, we observe a decline of a magnitude similar to what we found in Bihar: labor expenditures decline by 19 percent in the year the system is introduced and, importantly, the decline persists several years after implementation. This alleviates the frequently voiced concern that these types of reforms only have short term effects, until officials figure out a new way to game the system.

This paper makes several contributions to the literature.

First, we contribute to the forensic literature on measuring corruption. We build on and extend recent work that compares official records of funds release with actual receipt by beneficiaries (Reinikka and Svensson, 2004; Olken, 2007; Niehaus and Sukhtankar, 2013; Imbert and Papp, 2011; Muralidharan, Niehaus and Sukhtankar, 2016), by cross-validating official records with another administrative data source. We also build on Fisman, Schulz and Vig (2014) methodology and use officials' asset disclosures to examine wealth effects attributable to corruption in the context of a large scale experimental evaluation of an administrative reform.

Second, our paper contributes to the empirical literature on corruption by demonstrating how e-governance reforms can reduce leakage.[5] A closely related study is Muralidharan, Niehaus and Sukhtankar (2016); it evaluates a MGNREGS reform in which wages were disbursed through local banks based on biometric authentication of beneficiaries (via smart cards). The reform empowered beneficiaries and made collusion between GP officials and banks harder. Worker payments, and thus household income, increased, with no change in government expenditure, indicating lower leakage of funds. This reform complements the intervention we studied, which changes the fund flow and leaves the disbursement process constant. The key advantage of the smart card reform, relative to the one we studied, is that it led to direct gains to the program beneficiaries, not just savings for the government.

[4]The national reform went one step further by depositing the money directly in the worker's bank account, but unlike the Andhra Pradesh system studied by Muralidharan, Niehaus and Sukhtankar (2016) it did not require biometric identification.

[5]A few papers examine the effect of changes in the number of independent and potentially competing jurisdictions or officials (Olken and Barron, 2009; Burgess et al., 2012) and the impact of reducing bureaucratic discretion (Duflo et al., 2018; Rasul and Rogger, 2018). The bureaucratic architecture has been widely studied in the public administration literature (Klitgaard, 1988; Wallis, 1989; Peters and Pierre, 2003; Pollitt and Bouckaert, 2011). Recent evaluations of administrative reforms in low state capacity settings include Banerjee et al. (2012); Duflo et al. (2013); Bó, Finan and Rossi (2013); Banerjee et al. (2016*b*). Those focussing on the use of information technology include Barnwal (2014); Muralidharan, Niehaus and Sukhtankar (2016); Lewis-Faupel et al. (2016).

Third, our paper contributes to an emerging literature that examines whether experimental results predict the impact of larger scale and permanent versions of the same program (Bold et al., 2018; Allcott, 2015; Banerjee et al., 2017). Very few papers are able to directly compare experimental estimates to credible estimate of a nation-wide roll out of the same policy – Horton (2017) compared experimental estimates to the effect of a platform wide roll-out.

I. Understanding the reform

The MGNREGS program is a workfare program, financed by the central government, and implemented at the local level by the lowest administrative level of the government, the Gram Panchayat (or GP). After receiving MGNREGS funds from the national level, the state transfers funds to the GP accounts based on authorization.[6] The reform we study concerns the authorization process for within-state transfers. Below, we describe the status quo and reform authorization processes. While we focus on processes in Bihar (the setting of our experimental evaluation), the status quo authorization system and reform were similarly designed across Indian states.

A. Fund flow management in the MGNREGS

The status quo system. — Figure 1a summarizes the status quo fund flow authorization system. At the start of the financial year, each GP account received a first tranche of funds. When these funds were exhausted, the GP made a fund request to the higher administrative tier (the block). This request was intended to reflect anticipated need and was supported by an " utilization certificate" that declared that the previous tranche of funds had been spent. It did not include information on who was employed and how much they were paid. Next, block officials inspected and ratified GP requests and passed them on to the district administration, who had the power to authorize a fund transfer from the state treasury to the GP savings account, using an electronic platform called Central Planning Scheme Monitoring System (CPSMS). Upon authorization, funds were directly transferred from the state pool to the GP account. Our analysis of GP accounts fund flow data for our 12 sample districts between July 2011 and July 2012 shows that it took, on average, three months to replenish a GP account.

In the year prior to our intervention, the state administration sought to reduce the discretionary powers of block and district administration. Specifically, districts were asked to transfer funds to a GP whenever its account balance fell below Rs. 100,000. However, it was still up to the district to transfer the funds. There was no automatic trigger and no enforcement of these guidelines. District officials

[6]To enable expenditure accountability, the central government releases the funds in tranches, and each tranche is disbursed only after the state accounts for a minimum fraction of labor expenditures by documenting worker details (and amounts paid) on nrega.nic.in.

did not follow these instructions and fund requests continued to involve bargaining between the district, the block, and the GP. It is not particularly surprising that district officials did not carry out instructions that would have reduced their power. The state officials were partly reacting to this foot-dragging when they decided to directly short-circuit the process and cut out the intermediating officials. Dragging one's feet in replenishing the bank accounts is relatively easy to justify in a system where there a lot of slippages but district officials did not have the authority to directly resist a top-down change that cut them out completely from the fund flow system.

Full documentation of who worked under the program and how much they were paid was a lengthy process. The GP officials backed up the utilization certificate by the electronic entry of the " muster roll" (the register that includes information on each beneficiary, and how much they worked) in the public information database. Over time, reconciliation happened by ensuring that total labor payments corresponded to what was reported in the public information database. During our study period, it took about six months for 60 percent of expenditures in the average control GP to be entered on the public data portal, and one year for the payments recorded on this portal to match (on average) the amounts in the CPSMS data.[7]

The reformed system. — Figure 1b describes the reformed fund request system for labor payments: once a villager completed a work spell, the GP official could directly log into CPSMS and enter beneficiary details; this initiated an automatic transfer of incurred wage expenses from the state account to GP savings account. Since most GP officials lacked necessary infrastructure and/or know-how, they typically uploaded beneficiary data in batches at the block office with assistance from a block-level data entry operator. Accountability in the reformed system was, thus, much more immediate.

The reform left four elements of the fund flow system unaffected. First, in all cases authorized funds were directly transferred from the State pool to each GP account and could be tracked in the CPSMS platform. Second, the final step of payments from GP to beneficiaries was unchanged: the GP sent a cheque and a list of intended beneficiaries and amounts due to the local bank/post office, who then credited each beneficiary account. Third, the state continued to disburse payment for purchases of materials used in MGNREGS projects as advances, with district and block authorities acting as intermediaries. This created the possibility for treatment GPs to flout the rule and use advances received for materials to pay labor. Finally, all GPs were required to document every job spell – including beneficiary identity and associated payment – on the public database. In practice, treatment GP officials entered the same information twice: once to get paid, and once after the fact.

[7] Worker data on public portal did eventually tally with CPSMS data at the district level, though not necessarily at the GP level (Appendix Table A.2).

B. *What kind of impacts should we expect?*

The status quo and reformed fund system differed in two fundamental ways: reporting requirements associated with the release of funds for wage payments, and the associated administrative structure. We now discuss implications for fund leakage, and the potential impacts for villagers seeking work under the program.

Reform impact on leakage. — Changes in reporting requirements in the reformed system had (at least) two effects. First, they improved administrators' ability to monitor fund leakages via audits. As discussed above, under the status quo a GP would request funds for wages via a utilization certificate which did not require a listing of worker names. Subsequent delays between a work spell and when its details were available on the public data portal made auditing difficult. By the time an audit occurred, a villager could have, for example, migrated or simply forgotten the details of her work spell. In contrast, fund release to treatment GPs occurred only after beneficiary details were documented on the electronic platform. By reducing the time lag between work completion and data entry by the GP official, we hypothesize that the reform improved program transparency and made it easier to audit claims about work done and payments.

An analysis of MGNREGS audit reports conducted between May 2012 and June 2013 in our study districts in Bihar supports this hypothesis. Audits were frequent: between June 2012 and May 2013, 64 percent of the GPs in our sample districts were audited at least once (IDinsight, 2013). Appendix Table A.3 shows that on average, 35 projects were audited per block during the intervention period, and an additional 9.3 during the three subsequent months. For every 100 audits, there were 94 " show cause notices" (administrative inquiries on irregularities found), 2.8 complaints lodged with the police and 1.4 dismissals. During the intervention period, the share of audits finding irregularities was similar in both groups, but in the period immediately after, it was twice as large in the treatment group (5 percent of audits found irregularities in the control group compared to 10 percent in the treatment group; Appendix Table A.3, panel D). Since audits happened with a lag, this is consistent with the hypothesis that audits in treatment areas were better conducted (we, unfortunately, only know the audit date, not the date of the actual work). In particular, the probability of detecting an irregularity depends on the actual number of irregularities *and* detection probability. As we document below, the weight of the evidence suggests lower leakages in the treatment group. Thus, increased detection of irregularities in treatment GPs must reflect a greater probability of being caught, conditional on cheating. This should directly reduce the incentive to steal and hence fund leakage.

Second, the reform reduced the number of agencies involved in authorizing fund release. The net impact of this leaner administrative system on leakage is ambiguous. To see why, note that in this setting the GP official is the only one who can directly steal money: people above him in the hierarchy make money by taxing his gains. Lowering the bargaining power of other officials (or eliminating their claims altogether) reduces the " tax" on the panchayat officials earnings, and therefore could in fact *increase* his incentive to steal and therefore leakage.[8] Overall, the effect on leakage is thus theoretically ambiguous.

Reform impact on citizen well-being. — The reform, and specifically the change in reporting practices, may also have impacted work provision. A system of automatic reimbursements could increase work provision and, thereby, expenditures, as a GP official's decision to provide work is no longer constrained by the fund balance in their account. Overall, the funds available in the State should be spent more efficiently, since no money should be sitting idly in a GP bank account. This should increase the money available to the most active GP heads.

However, on the flip side, conditional on having worked, a lengthier administrative process had to be completed before a worker gets paid. Specifically, the reform increased the administrative burden on GP officials, who had to make an extra trip to the block office to input the data and request payment. It also increased invoice processing for the bank. These cumbersome program requirements could, at the margin, lower GP capacity to provide employment provision, or their willingness to work hard. In addition, program take-up could also have fallen in the initial months of the reform, while GP officials got used to a new system in which workers are hired without having the money for wage payments available in the GP bank account. Finally, the reform did not directly change how the beneficiaries interacted with the system: they continued to have to collect wages at the post office or bank via a cumbersome process that was often rife with corruption. Putting these factors together, it seems plausible that at least in the beginning switching away from the system of fund advances might have increased payment delays.

Thus, a key factor determining whether the net effect of the reform was positive for villagers seeking work was the pressure faced by administrative and elected officials to provide work. In Bihar, there is some evidence that this pressure was weak.[9] Thus, even if the program successfully reduced leakages there is no guarantee that the reform would benefit villagers seeking work under the program unless the savings from the intervention were plowed back into the program. In our empirical analysis, we will provide evidence on this issue.

[8]This is different from the effect highlighted in (Shleifer and Vishny, 1993; Olken and Barron, 2009), where all the officials are in a position to independently steal and do not rely on one person to do the actual stealing. In that case reducing the number of claimants would reduce total leakage. The working paper version of this paper works out a stylized model that clarifies this point (Banerjee et al., 2016*a*).

[9]Dutta et al. (2014) show that an awareness campaign on the right to work in rural Bihar increased the willingness to do MGNREGS work but had no effect on actual participation.

II. Context and experimental design

Below, we describe the study context for our experiment, the datasets we use, the experiment timeline, and a set of randomization checks.

A. Bihar MNREGS: performance and audit practice

Bihar has one of the highest poverty rates in India and very high unmet demand for MGNREGS work. Using National Sample Survey data for 2009-2010, we estimate that 77 percent of households in Bihar who wanted MGNREGS work could not obtain it, and at most 10 percent of households in Bihar worked on MGNREGS worksites during the year.[10]

Multiple forms of evidence suggest that fund leakages have constrained MGNREGS work availability in Bihar. Using data from our household survey, we find that households in control GPs that report working in MGNREGS sites account for only 59 percent of households listed as having worked in that period in the official database.[11] Next, in a survey of 346 GP heads (Mukhiyas), 47 percent of the control GP Mukhiyas identified corruption in the administration as a major implementation issue. On average, they estimated that the system of " taxes" extracted by MGNREGS officials made up 21-30 percent of program expenditures. About 72 percent of the Mukhiyas also identified the lack of funds as a constraint.

These findings, as well as the announcement of a MGNREGS corruption enquiry by India's federal vigilance authority (CBI) in the neighboring state of Orissa, led the Bihar administration to strengthen MGNREGS audit practices. In June 2011, the Bihar principal secretary for Rural Development sent district authorities a letter requiring that block officials undertake random weekly audits of ongoing and recently concluded works. In November 2011, revised department guidelines clarified that the MGNREGS public database should be used for audit and that audit teams should receive access to all additional MGNREGS documentation. Finally, roughly coinciding with the start of our reform the state government issued an audit reform letter on September 1, 2012, requiring that projects be chosen for audit from the set listed as completed in 2011-12 or ongoing in fiscal year 2012-13 on the public data portal.

B. Data

Our analysis exploits multiple data sources.

First, we gather data on daily MGNREGS fund flow data for all GPs from Bihar's CPSMS platform. We aggregate daily data for September 2011 to January 2014 to compute total credit and debit separately for pre-intervention period,

[10]In the better performing state of Andhra Pradesh, only 27 percent of those who wanted work could not find it, and 39 percent of households participated in MGNREGS.

[11]Using data from a household survey representative of the whole of Bihar in 2009-10, Dutta et al. (2014) estimate significant, but somewhat smaller leakages of MGNREGS funds (20-30 percent).

intervention period and post-intervention period. These data, however, neither distinguish between material and labor expenditures, nor do they provide worker details.

Second, we use the MGNREGS public database (nrega.nic.in) to obtain category-wise expenditures at the financial year level (i.e. April 1st to March 31st); the 2012-13 financial year includes three pre-reform months (April to June 2012), two set-up months (July and August 2012), and the seven reform months (September 2012 to March 2013). The database reports four expenditure categories: unskilled labor, material, skilled labor and administrative expenses. It also stores details on MGNREGS beneficiaries, including who has worked in the household, for how long and when. Previous research suggests that the beneficiary list includes genuine and ghost workers, i.e. fictitious persons or actual villagers who did not work but against whose name payment was released. Similarly, information on days worked includes genuine work spells and ghost days, i.e. days falsely claimed as worked by genuine participants. The database also reports wages earned, wages paid and the date at which payment transferred to the beneficiary's account, allowing us to compute duration of payment delay for each workspell.

Third, we match MGNREGS beneficiaries from nrega.nic.in with the 2012 India's Socio-Economic Caste Census (SECC). For our 12 study districts in Bihar, the SECC data cover 16,480 villages across 195 blocks. For each household listed in a village, the SECC database includes the name and age of each household member (and relationship to the household head). This provides data on 34 million individuals, living in more than five million households. Our outcome of interest is the match rate, defined separately for people reported to have worked during and after the intervention period: the basic idea is that a household (name) with a job card in the public information database but missing in the SECC database is more likely a " ghost" than a household (name) found in both. Our matching procedure implements a population-level version of Niehaus and Sukhtankar (2013) forensic method that cross-checks administrative data with household survey data. We use an algorithm to match village names across the databases.[12]Among matched villages, we use the same algorithm to find a match for each household with a job card in the public database in 2014 (for more details, see Appendix A.1).

The matching process is probabilistic (based on a threshold), with errors in both directions; however, matching errors should be similar across treatment and control groups.[13] In control GPs, our match rate is 50 percent during the intervention period (67 percent for single-worker job cards, and 28 percent for job cards with more than one worker; the difference reflects the fact that it is

[12]The MGNREGS basic administrative unit is the GP, and so the database lacks a village census code. 84 percent of GPs in the database have a village match in the SECC census. For the 16 percent remaining unmatched GPs, we look for matches among households living in all villages in the GP.

[13]Individuals may be omitted from the SECC data for example, or the matching could fail because names are spelled too differently to match. Conversely, two different persons with the same name could be incorrectly matched.

harder for two names to match than one). This is comparable to the 59 percent match rate we obtain by comparing official numbers of MGNREGS workers from nrega.nic.in to (population) estimates from our household survey. While this exercise identifies non-existent workers, it does not capture the leakages from genuine households who falsely report working or exaggerate the number of days when they worked.

We also collect data on local officials' personal wealth. From 2011 onward, district officials responsible for MGNREGS implementation (heads of the District Rural Development Authority) had to make an affidavit where they declared their and their spouse's personal assets. In 2012 and 2013, Bihar's Rural Development Department extended the obligation to all GP, block and district officials in charge of MGNREGS. While such self-reports should, of course, be treated with caution, recent studies, demonstrate that affidavit data contain useful signal.[14] We classify personal assets into movable (cash, savings and other financial assets, jewellary, vehicles) and immovable (land, real estate). In our analysis, we focus on the more liquid movable assets, which are more likely to respond to income shocks.

Finally, we directly surveyed 9,670 households in 390 GPs between May and July 2013. We randomly selected two GPs per sample block, and 25 households per GP, oversampling poorer households, who were more likely to participate in the MGNREGS (see Appendix Section A.1 for details). Survey respondents were asked to recall weekly MGNREGS participation and the amount, date and payment for each work-spell since July 2012. Our sense is that respondents could correctly recall MGNREGS participation, since MGNREGS work allotments require workers to report to a particular government worksite. For each reported workspell, we confirmed the place and nature of the work with the respondent; other researchers have used similar questions, e.g. Dutta et al. (2012). Since MGNREGS participation was low during our study period, the survey (despite its large sample size) only identified a small number of participants, which makes the data imprecise.

We also interviewed the GP head (the Mukhiya) in 346 of the 390 survey GPs about implementation issues for MGNREGS. Alongside, in May 2013, we downloaded the list of MGNREGS projects registered in nrega.nic.in. We surveyed a random sample of 4,165 ongoing and completed projects (10 per GP). For each project, surveyors recorded whether the project was found and whether it had been completed.

C. *Experiment implementation*

In June 2012, we randomly selected one-third of the blocks in each of 12 districts to implement a reformed fund flow system (see Figure A.1). In total, the study

[14]Fisman, Schulz and Vig (2014) use politician affidavit data and show a 3 percent to 4 percent higher estimated annual growth rate of wealth for winners than for runner-ups in close elections. Fisman, Schulz and Vig (2016) further show that the requirement to disclose discourages many politicians from running for office.

included 69 treatment blocks (1033 GPs) and 126 control blocks (2034 GPs).

A key prerequisite for the reform was IT infrastructure to enable GPs to connect with CPSMS, including computers, data entry operators, generator to ensure power supply, internet access, scanner and printer. Between June and August 2012 (the " set-up phase"), there was a big push to install this infrastructure in treatment GPs. While we lack pre-reform data, our sense is that few GPs had such infrastructure in place prior to this push. Appendix Table A.1 shows a reasonably fast ramp up in required facilities between July 2012 and January 2013.

The intervention launch on September 8, 2012 was almost immediately followed by a sharp decline in MGNREGS spending (Figure 2). This drop that continued till end December was, in part, driven by seasonality – MGNREGS work-sites often close during the peak agricultural season between July to December (Imbert and Papp, 2015). However, the dip in 2012 was accentuated by the central government's decision to respond to inadequate documentation of expenditures on nrega.nic.in by the Bihar government by freezing the release of funds to Bihar in September. The state government sustained some MGNREGS spending by using its own resources and both treatment and control GPs continued to be credited with a limited amount of funds. However, normal fund flow resumed only in mid-December after data documentation was completed and the central government released funds. That said, as soon as the money arrived in December, GP officials launched a two-week strike. Thus, MGNREGS operated at a reduced level between September and December, and at full speed only from January.

Figure A.3 shows that the fraction of treatment GPs that logged into CPSMS at least once to request funds increased from less than 20 percent in December 2012 across all districts to 60 percent in April 2013 (performance varied across districts).[15] Imperfect implementation of an at-scale reform is reasonably common (Muralidharan, Niehaus and Sukhtankar, 2016; Banerjee et al., 2016*b*).[16] While we focus on intent-to-treat analysis, Appendix Table A.11 reports very similar findings for treatment-on-the treated analysis.

D. Randomization check

To check whether the random selection of treatment blocks ensured that GPs in the 69 treatment blocks were ex ante identical to GPs in the 126 control blocks, we estimate:

$$X_{pd} = \alpha + \beta T_p + \eta_d + \varepsilon_p \tag{1}$$

[15]Treatment GPs that did not use CPSMS could still pay wages by depleting their savings accounts: only 1.5 percent of treatment GPs did not spend any money during the intervention.

[16]The nationwide implementation of the e-FMS system, a payment system similar to CPSMS which we study below, faced similar challenges. According to the official website (nrega.nic.in), it took more than two years (from June 2014 to August 2016) to get all blocks in Bihar to use the system.

where X_{pd} is a vector of baseline characteristics of GP p in district d, T_p is a dummy set equal to one if GPp is in a treatment block, η_d are district fixed effects, and errorsε_p are clustered at the block level. The estimated coefficient β represent pre-treatment differences between treatment and control GP. In each panel, we report a normalized index of all the variables, calculated as the average of all variables after normalizing each variable to have a mean zero and standard deviation of one in the control group.

Table 1 presents the results. 2011 census data shows that villages in treatment and control GPs had similar socio-demographic characteristics, and our survey of 390 GPs shows that households in treatment and control GPs have similar characteristics.[17] Finally, CPSMS reports show that treatment and control GPs received and spent similar amounts on MGNREGS before the reform. The one exception is that, in the public information database (panel C), treatment blocks report more beneficiary households prior to the reform (and correspondingly, more workdays provided and more labor expenditures). Overall, these differences are small (they represent a 5 percent difference in number of beneficiaries and 13 percent in labor payment), and go in the opposite direction to the treatment effects we report below. We do not control for baseline variables in our analysis, so our main results are potentially biased downwards. Appendix Table A.4 shows that the main results remain similar when we re-estimate the regressions controlling for the index of all the pre-reform labor variables in Panel C.

III. Results from the experimental evaluation

To estimate how the reform affected fund leakages, we first compare reform impacts on expenditure and employment as measured by administrative data to those observed in independent household survey data. Next, to obtain direct estimates of leakage we examine how the reform influenced the match rate of worker details across administrative data-sets and officials' reported wealth.

A. *Did the reform impact program spending?*

Using GP-level program finance as outcomes, we separately estimate regressions of the form described by equation (1) for: the pre-intervention period (September 2011-June 2012), the set-up period (July-August 2012), the intervention period (September 2012-March 2013), and the post-intervention period (April 2013-January 2014). We also report regressions for two intervention sub-periods: the September-December 2012 period, which spans the period of low fund availability and employee strike, and the January-March 2013 period, when MGNREGS was working relatively smoothly.

Figure 2 plots average daily expenditures in treatment and control GPs. Prereform spending trends show significant seasonality, but the patterns are similar

[17] We lack pre-program data on IT infrastructure: the difference we showed in June-August were a direct result of the effort to equip the treatment blocks.

across treatment and control GPs. In contrast, during the reform, expenditures in treatment GPs are significantly lower than control GPs. Post-reform, treatment and control GPs rapidly converge to similar expenditure levels.

Panel A of Table 2 summarizes these findings. Between September to December 2012 — when MGNREGS expenditures were low — spending was 19 percent lower in treatment GPs. Once regular fund flows resumed in January, the magnitude of reform effects doubles: between January and March 2013, spending was 31 percent lower in treatment GPs. Across the whole intervention period (from September 2012 to March 2013), spending was 24 percent lower. After April 2013, treatment and control GPs reported similar spending.

In Panel B, the outcome variable of interest is the closing balance in GP accounts. At the start of September 2012, treatment and control GPs reported similar balances and then, reflecting the freeze on central funds transfer, similar declines as all GPs depleted funds. The state account was replenished in December 2012. Then, control GPs received large inflows corresponding to outstanding tranches, while treatment GPs only received funds corresponding to expenditures documented in the electronic system. Treatment GPs immediately used these funds to pay wages. Reflecting this, by March 2013, treatment GPs report an account balance that is 30 percent below that in control GPs.

Panel C in Table 2 shows that the combination of a 24 percent decline in spending and a 30 percent decline in idle funds reduced program expenditure by 38 percent in treatment GPs,[18] implying a cost saving of roughly 6 million dollars.[19] Expenditures were not just postponed: in the six months following the intervention, the difference between treatment and control groups returned to zero. An immediate question -- which we address below -- is whether this reduction in program costs reflected a decline in real outcomes (days of employment offered, and assets built), a reduction in leakage, or both.

In Table 3, we use expenditure data from the program's public information database to examine program finance impacts. Treatment and control GPs faced identical requirements on electronically reporting beneficiary details (name, payment received, work spell), which feature on the public information database. Despite significant lags in data entry, these data eventually accounts for close to the full expenditures reported in the CPSMS financial database.[20] Since data is reported by fiscal year, we estimate regressions similar to equation 1 separately for fiscal years 2011-2012 (before the intervention), 2012-2013 (which includes the intervention and some non-intervention months), and 2013-2014 (post-intervention).

[18]We check that these results are not driven by outliers using an inverse hyperbolic sine transformation of the outcomes. If anything, the treatment effects become larger (see Appendix Table A.5)

[19]To obtain this figure, we multiply the expenditures reduction per GP by the number of treatment GPs, and convert the total of $3.44 \times 1003 = 3,410$ lakhs Rupees into million dollars using the April 1, 2013 USD/INR exchange rate of 0.0183.

[20]Appendix Table A.2 shows that discrepancies across annual expenditures per GP in CPSMS and nrega.nic.in are only about 11-14 percent in 2012-13.

Labor and material expenditures were respectively 16 percent and 14 percent lower in treatment GPs during fiscal year 2012-13. Accounting for the fact that the fiscal year includes three pre-intervention months, the public information database treatment estimates on spending are slightly more negative than those from CPSMS. We interpret the similar declines in both labor and material expenditures, despite the fact that the financial reform only affected labor expenditures directly, as reflecting the legal requirement that MGNREGS material expenditure cannot exceed 40 percent of total project spending. For the average GP, this requirement was close to binding: expenditures on material amounted to 37 percent and 36 percent of total expenditure in the financial year 2012-13 and 2013-14, respectively.

B. Did the reform impact beneficiary outcomes?

Effects using administrative data. — In Table 4, we show the treatment effects on beneficiary outcomes, as reported by GP officials in the public information database; the results are broadly in line with the patterns observed in expenditure data. In Panel A, we observe that treatment reduces the number of reported work days by 13 percent over the reform period (significant at the 10 percent level). Different from what we saw in the CPSMS data, the effects in the first half of the reform period exceed those in the second half – possibly because employment and expenditure are reconciled over a fiscal year, not on a monthly basis. Panel B suggests that the reform did not reduce the days worked per working household and Panel C shows that the decline in reported MGNREGS employment comes entirely from a 9.7 percent reduction in the number of individuals who have supposedly worked.[21] This divergence between the effect on the number of households and the number of days per household is consistent with our understanding of the change in reporting practices: the reform makes it riskier to create ghost workers (since an audit conducted with less of a time lag is more likely to identify such fraud) but may not affect the ability to create ghost days (since the auditor continues to rely on worker reports for authentication).

In Panel D, we find a decline in MGNREGS wages that is proportional to the decline in days worked (13 percent); both this and the negative treatment effects on households working persist after the end of the intervention (Column 6). This contrasts with the fact that both CPSMS and the public information database shows no difference in post-reform spending between treatment and control GPs. A possible explanation is that ghost workers are a stock variable: once they appear on the muster roll, they can easily be assigned workdays. If treatment GPs created fewer of them or removed some from the rolls during the reform period, the effect persists over time.

[21]Inclusion of the pre-intervention MGNREGS implementation index as a control increases the estimated treatment effects on workdays and number of workers to 17 percent and 13 percent respectively (Table A.4).

Finally, treatment blocks experienced a 38 percent increase in (reported) payment delays during the intervention (Panel E), suggesting slower fund disbursement to treatment GPs, especially during the first phase of the intervention. Consistent with this result, Panel E in Table A.8 shows that twice as many Mukhiyas reported that the CPSMS had created delays in fund flow in treatment blocks compared to control blocks (35 percent as against 17 percent).

EFFECTS USING SURVEY DATA. — We now examine whether independent household and asset surveys corroborate the administrative data findings, namely a reform-induced decline in MGNREGS expenditure which is accounted for by a drop in reported employment. In particular, we check whether households reported less work and whether fewer assets were constructed in treatment GPs. Triangulating across results based on survey and administrative data allows us to assess whether the reform reduced actual work or just the reports of ghost work. We aggregate household responses within the two GP in the block to compute block-level population averages, using sampling weights to account for over-sampling of poorer households. Let Y_{bdt} denote outcome for block b in district d at period t.[22] We estimate:

$$Y_{bdt} = \alpha + \beta T_b + \delta Z_b + \eta_d + \varepsilon_{bdt} \tag{2}$$

as before T_b is the treatment block dummy. Z_b denotes a vector of average household characteristics in the block.[23]

Using survey data, we construct three employment measures: first, a binary indicator of MGNREGS participation; second, the number of weeks in which households declared having worked in MGNREGS; and third the number of days worked. We estimate separate regressions for the set-up period, the two phases of the reform, and a short post-period. Given the recall-based nature of data and the relatively small sample of people who report MGNREGS work, we have the most confidence in the binary participation variable.

Panel A of Table 5 reports treatment impacts on the probability of participating in MGNREGS. The observed MGNREGS participation rate between September and March 2013, while low (just over 3 percent), is consistent with National Sample Survey data, which reports a participation rate of 9 percent for 2011-2012. When we consider survey responses for all work spells between July 2012 and June 2013, we find a participation rate of 8 percent.[24] Lower work rate during

[22]Aggregating the data at the block level reduces the number of cases were we surveyed zero workers in a block. This comes at no loss of precision since the intervention was randomized at the block level.

[23]These controls included are fraction Hindu households, fraction households belonging to lower castes, fraction households with male head, fraction households with literate head, average household size (total and adults), fraction households with temporary housing structures and fraction landless households. A no-control specification, available upon request, yields very similar results.

[24]In response to a separate question, 8 percent of households reported that they had participated in the scheme " since the last rainy season" . There is no treatment effect on this variable (Table A.6)

our reform period likely reflects the fact that it fell outside the peak MGNREGS work season.

Column (1) shows a negative treatment effect during the set-up period (July-August). We conjecture that this, at least in part, reflects GP officials being busy with infrastructure upgrading and training activities for the reform. Columns (2)-(4) show an *increase* in reported participation in treatment GPs during the intervention period (0.89 percentage point, which is 30 percent of the control mean, with a 95 percent confidence interval expressed as fraction of the control mean of [-0.5;+61]). While small in absolute value, this participation increase suggests that the observed decline in the number of hired households in the administrative database (minus 10 percent, panel C of Table 4) reflects fewer ghost workers, not an actual decline in work provision. After the intervention, employment participation returns to the same level in the treatment and control groups.

In Panel B we consider the numbers of days worked on MGNREGS (set as zero for non-participating households), and again find a negative point estimate for the set-up period, and positive point estimates for the two intervention sub-periods. The estimates for the second sub-period and the whole intervention period are significant at the 10 percent level. We can reject a reduction of 1 percent at the 95 percent confidence level, a much smaller decline than the 13 percent found in the public information database (Panel A of Table 4)

Panel C considers wage payments. For each MGNREGS work spell, the respondents declared whether, when, and how much they had been paid. We attribute each payment to when work occurred, regardless of when the payment occurred.[25] Consistent with a lower probability of working, MGNREGS wage payments were significantly lower in the treatment GPs during the set-up period. During the intervention, the point estimate is positive (14), and the 95 percent confidence interval, expressed in percentages, is [-33 percent; +69 percent].[26]

In Panel D, we examine worker-reported delays in MGNREGS payments. Relatively few observed work spells implies a small sample size. With this caveat, the household survey data confirms the finding from administrative data; if anything, households report higher delays in the survey compared to those observed in the administrative data for the intervention period. Compared to an average 70-day delay in the control during the first phase of the intervention (September to December 2012), surveyed workers in treatment blocks report waiting an extra 46 days for their payment. The adverse treatment effect persists in the second phase (January to March 2013) but is smaller (37 days).

We can identify two implementation-related reasons for increased payment delays. First, the bank handling CPSMS payment would receive multiple small

[25] A pending payment is set as zero. Replacing it by missing makes the treatment look slightly more positive, since delays increased in the treatment group.

[26] Given payments delays (2 and 3.5 months in control and treatment blocks respectively), our survey carried out in May-July 2013 could not fully capture payments made during the intervention period (which ends in March 2013). This may negatively bias the estimated treatment effect.

payment requests from a treatment GP and a single consolidated invoice from a control GP. The bank responded by processing invoices for treatment GPs only after cumulating a large batch of invoices, increasing delays. Second, travel costs caused treatment GP officials to often delayed data entry that needed to precede wage processing.

Payment delays raise the potential interpretation concern that lower employment during the set-up period combined with payment delays underlies the subsequent observed reduction in CPSMS expenditure. However, payment delays were too short to provide a full explanation: work in July and August was paid two and a half months later (according to both administrative and survey data) and hence these payments would not have spilled over to beyond December.

Appendix Table A.9 combines data on workdays reported in the administrative data base, and expenditures from CPSMS with the information from the survey to provide a first formal test of the hypothesis that leakage declined. We construct for each block the average days of work (or wage payment) per household, and subtract from it the average days of work per household (or the average CPSMS debit per household) for the same GPs. This is a noisy estimate of leakage in each block, due to the fact that we sampled a relatively small number of households in each block (50 over two GPs), and many of those never worked. Under the null that leakage stay constant, we should see no difference between treatment and control group in these variables. The estimate is positive (as it should be), but noisy for the days of work per household measure, but we reject the null quite decisively for wages payments. Our estimate of the effect on the reform on CPSMS debits is a more direct measure of leakage reduction, and it is much more precise than the estimate on the number of days reported in the public information data base, so it is not surprising that this is where we detect leakage reduction.

Finally, in Appendix Table A.10, we examine the reform impact on physical asset creation. This is an important welfare outcome in itself and provides useful corroboration for what we observe with employment outcomes. There were on average 14 projects per GP, most of them ongoing, and we see similar numbers across treatment and control GPs (Columns 1 and 2). Surveyors found 85 percent of registered projects, and there are no treatment effects on either completed or ongoing projects (Columns 3 and 4).

Overall, the reform did not lead to any income gains for treated households, no more than a small gain in employment, and a clear cost in terms of payment delays.[27] Thus, it appears reform benefits largely consisted of lower funds leakage. But the survey data is imprecise, due to the low participation of households to NREGA. Thus, below, we provide further evidence that leakage declined.

[27]In Appendix Table A.7, we show that the reform left household consumption levels unaffected.

C. Did reform impact fund leakage? Direct evidence

Administrative data shows a 24 percent decline in MGNREGS spending in the treatment GPs, relative to control, and a corresponding 10-13 percent reduction in the number of workdays and workers hired. In contrast, the household survey suggests that the treatment likely caused a small increase in participation; more specifically, we can reject – at a 95 percent confidence level – a decline in the number of workers that would be consistent with that observed in the administrative data. Built assets – as measured in either administrative data or in our asset survey – were unaffected by treatment. We can formally reject that the difference between average wage payment reported by households and average expenditure per household as reported in official data is the same in treatment and control, which is strongly suggestive that leakage declined. GP leaders also report a decrease in corruption: while 47 percent of surveyed GP leaders in control GPs identified corruption in the administration as a major constraint in MGNREGS implementation, 12 percent percentage points fewer reported the same in treatment GPs (see Table A.8, Panel D). Against this background, we present additional direct evidence on the reform's impact on fund leakage.

Effect on ghost workers. — In Section I we argued that the nature of the fund flow reform – which makes it easier to audit and verify the existence and employment status of a particular person – should have led to a reduction in the number of ghost workers. In contrast, conditional on having worked, accurate and verifiable information on number of days worked continues to be based on villager recall and remains hard to obtain. Consistent with this hypothesis, Table 4 shows that fewer workers, not fewer days per workers, accounts for the reform-induced decline in reported work creation.

To provide a direct test that the number of ghost workers declined, we match households listed on MGNREGS job cards with villager names in the Socio-Economic Caste Census village listing, and examine differences in match rate by GP treatment status. Since job cards with a single worker name are mechanically easier to match than those with two or more names, we compute the match rate separately for one-worker and multi-worker job cards (49 percent and 51 percent of all job cards, respectively).[28]

We estimate the treatment effect on the match rate using a specification of the form given by equation 1. We consider three different samples. First, all job cards in the MGNREGS database (as of 2014), then all job cards where someone was recorded as working during the intervention period, and finally all job cards

[28]In Appendix Table A.12, we show that, controlling for district fixed effects, MGNREGS employment is lower in GPs with a higher match rate. This does not appear to be a mechanical effect, since the entire effect is driven by number of households working, not by the number of days worked per households. This finding supports the hypothesis that a higher match rate of names across job cards and the household census implies a lower prevalence of ghost workers.

where someone was recorded as working in the post-reform period. In all cases, we separately consider single and multi-worker job cards.

Table 6 reports the results. For single-worker households, we match 64 percent of the control group job cards listed in the same village (or somewhere in the GP in the few cases where villages could not be matched). This is comparable to our previous estimate based on the comparison between the administrative data and the household survey reports: our household survey only accounts for 59 percent of the workdays in the database. In Column 1 we observe a significantly higher – by 1.90 percentage points – match rate in the treatment group. If we restrict the analysis to individuals who are reported as having worked during the reform period, we find a match rate of 68 percent in the control group; treatment significantly increases this match rate by 1.95 percentage points (Column 2). For individuals reported as having worked *after* the reform period (two thirds of all households in the public database), the treatment-induced increase in match rate is smaller and insignificant (Column 3), but still positive. This is consistent with findings in Table 4, and as discussed earlier potentially reflects a 'stock' phenomenon. That is, once created, fake names persist in the database. Reflecting the greater matching challenges, match rates are lower among households with multiple members on the job card. That said, the match rate is 1.32 percentage points higher in treatment GP for all multiple-worker job cards, 1.19 for job cards active during the intervention period, although it is only significant in Column 1. The estimates imply that the number of ghost workers (unmatched beneficiaries) declined by 5 percent in treatment GPs.

Effect on assests of block and GP officials. — Next, we use self-reported disclosure data on block and GP officials' assets to examine reform impacts on personal aggrandizement. As discussed in Section II.B we focus on movable assets, which includes cash, bank deposits and jewellery.

We use data from the first two years of the " affidavit declaration program" – 2012-13 (a period that spans our intervention) and 2013-14 (at least six months after the intervention had ended). Reporting was mandatory, and while it was self-reported, the signatories attested that the data is truthful, and lying on these affidavit is a punishable offense. These affidavits are used by the government to track and investigate suspicious wealth accumulation. Most " disproportionate asset" prosecutions reference rapid accumulation using the initial year of reporting as the benchmark. This arguably provides an incentive to overstate assets in the initial years, especially for officials who plan to " steal" in the future. This logic suggests that any treatment-induced mis-reporting should bias our estimate towards zero: a heightened fear of scrutiny among officials in the treatment group (due to greater transparency) should reduce current under-reporting by officials in order to avoid being caught under-reporting in the future.

Figures 3a and 3b show the combined CDF for the movable assets reported by block and GP officials. During the intervention year (2012-2013), officials in the treatment group declared relatively fewer movable assets than those in the control group. A Kolmogorov-Smirnov test of stochastic dominance rejects equality of asset distributions across treatment and control groups at the 5 percent level in 2012-2013. In 2013-2014, the year following the intervention, this difference is mostly gone. To examine these treatment effects in a regression framework, we estimate:

$$Y_{ibdt} = \alpha + \beta T_{bt} + \eta_d + \varepsilon_{ibdt} \tag{3}$$

where T_{bt} is a treatment dummy, Y_{idt} denotes log assets for officer i in block b of district d at time t, and Z_i is a vector of personal characteristics: age, age square, seniority, gender, and a dummy for being posted in one's home district. Columns 1 and 2 of Panel A in Table 7 show, on average, a reasonably large (12 percent) but statistically insignificant reduction in movable assets reported by block and GP officials in 2012-2013. Returning to Figure 3a, it is clear that the asset distribution is highly skewed with large outliers, and that the reform had no impact at the two ends of the distribution. In Columns 5 and 6 we, therefore, estimate median regressions and find a significant 10 percent decline in median movable assets (8.8 percent with control variables). Results in Panel B suggest that the decline in movable assets was reflected in total assets with a lag. In 2013-14, average log total assets for block and GP officials was 10 to 11 percent lower in treatment areas and median assets were 14 percent to 19 percent lower relative to control. The mean estimate for decline in total assets in 2013-14 implies a loss of 308 million Rupees, or 44 percent of the observed reduction in MGNREGS expenditures in treatment areas.

As a consistency check, we examine the correlation between our leakage measures: the match rate and officials' personal wealth. We re-estimate equation (3) replacing the treatment dummy by the match rate at baseline. The results in Columns 4 and 5 of Appendix Table A.12 confirm that in areas with a higher match rate (i.e.fewer ghost workers), GP and block officials declare fewer movable assets (holding personal characteristics fixed).

Effect on assets of district officials. — District officials, who were excluded from the new fund flow, were the main reform losers. As our treatment was randomized within a district, we cannot provide experimental estimates of reform impact use our experimental design on district officials' wealth. Instead, we compare officials' wealth in the 12 treatment districts to the other districts in the state. We have these data for three years, 2011-2014. However, in 2011-12 and 2013-14 only District Development Coordinators, who are in charge of MGNREGS at the district level, declared their assets.

The results in Appendix Table A.13 show a 40 percent reduction in the average movable assets reported by district officials in treatment districts in 2012-13. The median effect is larger (53 percent), and both sets of estimates are robust to the inclusion of controls for officer and district characteristics. Reassuringly, we observe no wealth difference the year before the intervention, and as in the case of GP and block officials, the decline persists the year after. The mean estimate for 2012-13 is equivalent to a loss of 255 million Rupees, or 39 percent of the reduction in MGNREGS expenditures. Hence, the estimated losses of MGNREGS officials at the three levels of administration (GP, block and district) account for 83 percent of the observed reduction in MGNREGS expenditure.

To examine whether district officials compensated for the reform by skimming off more from the control blocks we expand our sample to include 85 border blocks that share a boundary with treatment and/or control blocks (see Figure A.1) as an additional comparison group. At baseline, the border blocks have lower MGNREGS spending, receive less funds and have lower balance on their MGNREGS account than blocks in intervention districts (see Appendix Table A.14). We estimate:

$$Y_{bdt} = \alpha + \beta T_b + \gamma I_b + \delta Y_b^0 + \eta_d + \varepsilon_{bt} \tag{4}$$

which is similar to our main equation (1) with three changes. First, it includes a dummy I_b which is set equal to one for all blocks (whether control or treatment) in districts where the intervention took place. Second, it controls for Y_b^0, the value of the outcome pre-intervention (September 2011 to June 2012), to account for baseline differences. Third, we expand the definition of district fixed effects so that blocks, which are not part of the experimental sample are compared to blocks to which they are closest.[29] The coefficient γ measures potential treatment effects on control blocks. The results, presented in Appendix Table A.15, show that there is no difference in spending during the intervention period between control blocks and neighboring blocks from other districts (Panel A). There is no difference in funds credited into GP accounts (Panel C) either, but control blocks do have significantly more money lying on their account (Panel B). These results suggest that reduced spending in treatment blocks meant more money was available for control blocks, but that district officials were unable to compensate for losses in treatment blocks by inflating expenses in control blocks.[30]

D. *Epilogue: scaling down and scaling up*

The federal government bore the fiscal burden for MGNREGS and, therefore, benefitted from the reform-induced reduction in leakage of funds. In contrast, the reform failed to create any winners at the village- or state-level. The villagers saw, at best, a small gain in participation but alongside an increase in payment delays. Local officials, who saw their earnings from leakages fall actively lobbied

[29]For example, the fixed effects for Gopalganj district, which is in our sample, equals one for blocks in Paschchim Champaran, which are across the border.

[30]Not controlling for pre-intervention levels does not change the results (estimates not reported here).

against the program. Given this lobbying, the decline in program spending and issues with payment delay, state officials became concerned that program delivery was adversely affected by the fund flow reform. Thus, the reform was rolled back at the end of the fiscal year, in April 2013.

However, in the longer run, the political economy of this reform extended beyond the state of Bihar. The federal government had begun encouraging states to adopt a similar fund flow reform since 2012 (at the same time as our experiment). Our results strengthened the federal government's claim that this reform reduced leakage in NREGA, allowing them to scale up the reform nationwide. A briefing presented by the Ministry of Rural Development to the Cabinet meeting to justify the generalized adoption of e-FMS cited our research (Ministry of Rural Development, 2015).

We might expect that incentives to undermine the reform would be stronger in the long run, especially when the program is also functional in the peak season. The staggered nature of this nationwide reform provides us an opportunity to examine the longer term impacts of the fund flow reform on program expenditures. Specifically, we ask whether expenditure declines persisted in the longer-run or whether officials identified ways of subverting the reformed system such that spending went back up.

IV. Did the experimental impacts scale up?

Concurrent to the CPSMS platform based reform in Bihar, the federal Rural Development Ministry began encouraging other states to adopt the e-FMS platform for MGNREGS. e-FMS created an expenditure-based fund release system by linking online reporting of labor and material payments with direct fund transfer from state to beneficiaries' accounts.

Relative to the Bihar's fund-flow reform that required that data be entered both on CPSMS and nrega.nic.in, e-FMS required officials to enter data only once, in nrega.nic.in. It, therefore, lowered the administrative burden. It also expanded the reform's ambit – while CPSMS only applied to wage payments, e-FMS also covered material expenditure (on a separate schedule). Finally, unlike Bihar, where funds were transferred to the GP account, and GP officials subsequently deposited cheques in the beneficiaries' bank or post office, under eFMS funds were directly transferred to beneficiary accounts.

The last feature – direct deposits to beneficiaries – was, arguably, the biggest difference between the two reforms. However, e-FMS did not require biometric authentication of beneficiaries. Hence, collusion between bank and GP officials, wherein payments were deposited in the accounts of non-existent workers and withdrawn by officials, continued to be feasible (Adhikari and Bhatia, 2011).

Starting in 2012, e-FMS was gradually rolled out across Indian districts and in 2015 adopted almost nationwide. In Bihar, e-FMS was implemented in 2014, a year after our experiment ended. In most states, e-FMS was first implemented for wage payments and later extended to material expenditures.[31] We use a difference-in-differences strategy to evaluate the effect of e-FMS on MGNREGS expenditures. Our estimating equation is:

$$Y_{djt} = \alpha + \beta EFMS_{djt} + \eta_d + \mu_t + \varepsilon_{djt}, \tag{5}$$

for district d in year t, Y_{djt} are MGNREGS labor ($j = 1$) or material ($j = 0$) expenditure as reported in nrega.nic.in, and $EFMS_{djt}$ is a dummy variable equal to 1 if e-FMS is operational for labor ($j = 1$) or material ($j = 0$). We define e-FMS as operational in a district starting the first year when positive expenditures are reported in the e-FMS section of nrega.nic.in. We cluster standard errors by state to account for similar start dates and time-series auto-correlation. To assess persistence over time, we estimate:

$$Y_{djt} = \alpha + \beta EFMS_{djt} + \gamma EFMS_{djt-1} + \eta_d + \mu_t + \varepsilon_{djt} \tag{6}$$

where $EFMS_{djt-1}$ is a dummy set equal to 1 if the program was operational in year $t-1$. Since the program was never cancelled, whenever $EFMS_{djt-1}$ is equal to one, $EFMS_{djt}$ is also equal to 1. A negative coefficient γ means that the negative effect of e-FMS on expenditures increases with time, a positive coefficient that the effect decreases with time, and a zero that the effect is persistent and constant.

Table 8 presents the results. We first regress labor expenditures on EFMS implementation for wage payments (Column 1). e-FMS decreased expenditures by 19 percent, and this effect is significant at the 1 percent level. We observe a similar coefficient when controlling for e-FMS for material (Column 2), and the effect is persistent: the coefficient on the first lag is negative and insignificant (Column 3), and the coefficient on the second lag is negative and significant at the 5 percent level (Column 4). In Panel B, we observe strikingly similar effects for material expenditure. Column 1 shows that e-FMS implementation for wage expenditures reduced material expenditures by 19 percent, which is reminiscent of our experimental finding in Table 3. Since the rule that material expenditures cannot exceed 40 percent of total expenditures is binding in most districts, when wages drop material expenditures need to decline as well. In column 2, we find that the extension of e-FMS to material purchases further reduced material expenditures by 20 percent. Although the year-by-year estimates are not significant, the event study figures (Figures 4a and 4b) summarize the patterns. Over several years, there were no differential trends before e-FMS implementation (although there is a noisy positive estimate at $t-2$), so there could be a pre-trend between $t-2$ and t_0). Expenditures fall in the year of implementation (t_0) and then remain

[31]The Appendix Table A.16 shows the roll-out of e-FMS across states for wage and material payments. In most states, implementation happened simultaneously in all districts. We exclude Andhra Pradesh and Jammu and Kashmir, which did not implement e-FMS.

persitently low in subsequent years. In Appendix Table A.17, we check that our results are robust to controlling for district-specific trends. The estimates, are slightly smaller in magnitude (14 percent decline in both types of expenditures), but remain significant at the 1 percent level.

These results are consistent with our experimental results, both qualitatively and quantitatively: the eFMS " just-in-time" financing reform in MGNREGS fund flow decreased expenditures by close to 20 percent. Unlike in our experiment, we lack survey data to examine impacts on actual MGNREGS employment and material purchases; based on the Bihar evidence, we hypothesize that the decline in expenditures reflected lower fund leakage. The advantage of our e-FMS evaluation is to demonstrate a persistent decline in expenditure, which suggests that corrupt officials failed to circumvent the new transparency measures.

V. Conclusion

This paper evaluates an e-governance reform to the fund flow system underlying India's flagship social protection program: the MGNREGS. The reform linked fund flow to incurred expenditures and reduced the number of intermediaries involved in fund disbursement. Theoretically, the program impact on leakages is ambiguous, as we explain above.

We first examine the short-run impacts of such a reform through a large-scale field experiment in Bihar, one of India's poorest states. On net, the reform significantly reduced fund leakage, which benefitted the federal exchequer. However, the reform failed to make the program more responsive to villager needs: we find a small (absolute) increase in program participation and no significant changes in wages received. Moreover, an increase in payment delays further reduced the value of the program for villagers who were directly affected by the reform. Finally, the nature of program implementation caused the administrative burden of running the program to increase for local officials.

An interesting feature of the reform was the relatively clear delineation of reform winners and losers – the central government gained while local officials and villagers failed to benefit. This allows us to both trace out the political economy of reform, and provide additional longer-run evidence on the reform's impact on expenditures. Specifically, significant local opposition to the reform among district officials and the lack of support among villagers led the state of Bihar to disband the reform at the end of the fiscal year. However, the federal government remained interested in the reform and, citing as evidence our experimental results, was able to scale it up over the next few years as part of an anti-corruption agenda. The impacts of the reform's scale-up are very similar to our experimental results, and these effects persist over time, suggesting that officials did not manage to circumvent the system even in the longer run.

In June 2016, India's Finance Ministry issued orders to extend the use of the Public Finance Management System (the successor of CPSMS) for all Central Sector Schemes and for central assistance for State Plan Schemes. The announcement emphasized the system as a means to facilitate " just-in-time" (i.e. expenditure based) release of funds and ensure complete monitoring of funds down to the end user. Thus, the perception of e-governance as an anti-corruption tool is heralding significant reforms for India's government payment architecture. The next important step is to ask how these gains can be leveraged to better ensure timely delivery of program funds to the targeted beneficiaries, without which local support for such reforms will remain limited.

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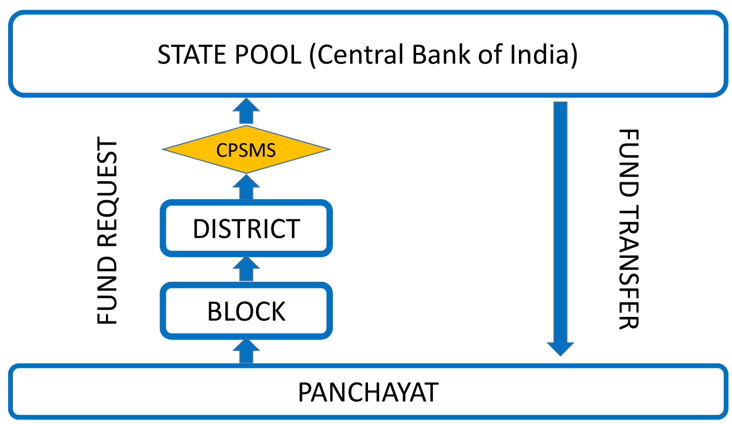
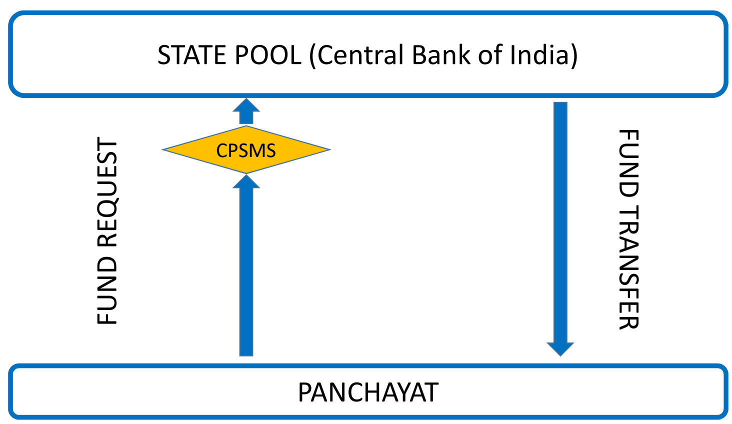
Figure 1. : MGNREGS Fund Flow in Control and Treatment Blocks

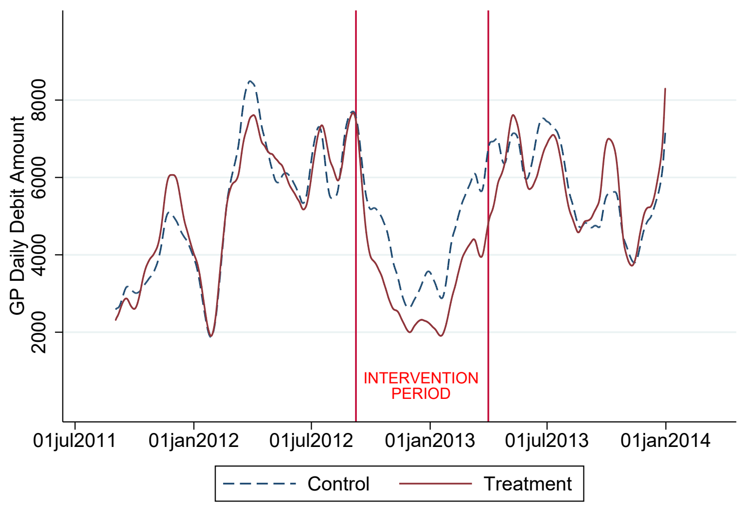


(a) Control Blocks



(b) Treatment Blocks

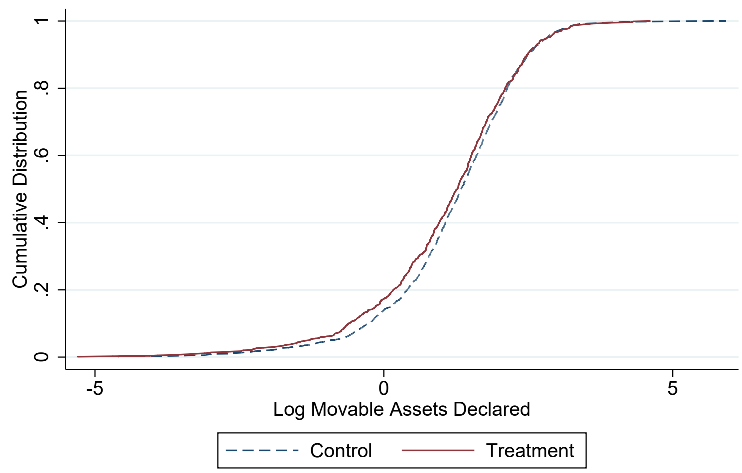
Figure 2. : GP Daily Expenditures on MGNREGS during the Study Period



Source: CPSMS data on GP savings accounts.

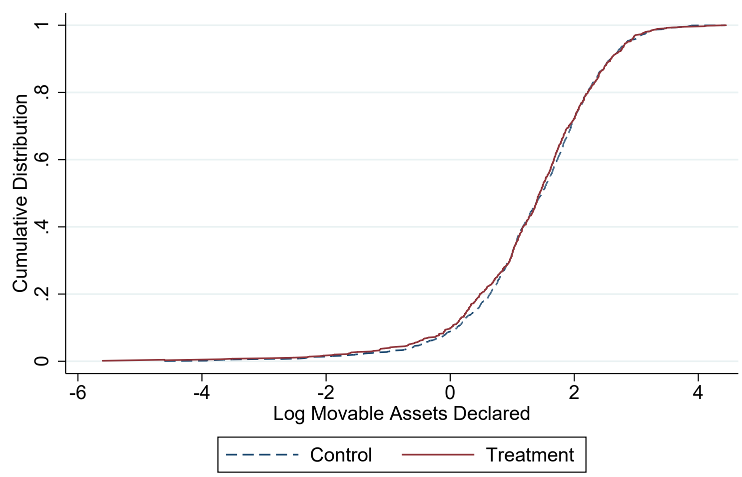
Figure 3. : Movable Assets of GP and Block officials During and After the Intervention

(a) During the Intervention



Source: Affidavits of block and GP level MGNREGS employees, financial year 2012-13, Government of Bihar. Movable assets include cash, bank deposits, bonds, jewellery and vehicles.

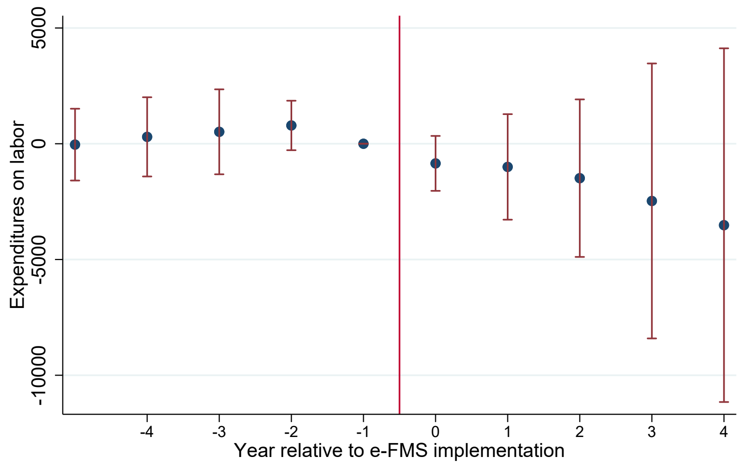
(b) After the Intervention



Source: Affidavits of block and GP level MGNREGS employees, financial year 2013-14, Government of Bihar. Movable assets include cash, bank deposits, bonds, jewellery and vehicles.

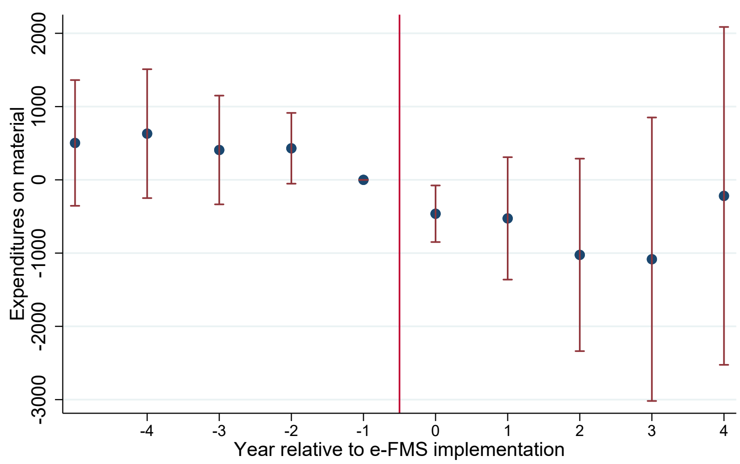
Figure 4. : Effect of e-FMS Implementation on Labor and Material Expenditures

(a) Effect of e-FMS Implementation on Labor Expenditures



Estimated coefficients and 95% confidence intervals from a linear regression of MGNREGS expenditures (reported in nrega.nic.in) on a dummy equal to one for the year of e-FMS implementation, including five lags and four forwards. The red vertical line indicates the year of e-FMS implementation.

(b) Effect of e-FMS Implementation on Material Expenditures



Estimated coefficients and 95% confidence intervals from a linear regression of MGNREGS expenditures (reported in nrega.nic.in) on a dummy equal to one for the year of e-FMS implementation, including five lags and four forwards. The red vertical line indicates the year of e-FMS implementation.

Table 1—: Randomization Check

	Control Blocks	Treatment Blocks	Difference Mean	Difference S.E.	Observations
	(1)	(2)	(3)	(4)	(5)
Panel A: Census 2011					
Area (hectares)	1095	1124	29.0	80.1	2,937
Number of households	1868	1853	-15.4	28.5	2,937
% Scheduled Castes and Scheduled Tribes	0.21	0.21	0.00	0.01	2,937
Literacy Rate	0.64	0.64	0.00	0.01	2,937
Normalized Index	0.00	0.02	0.02	0.12	2,937
Panel B: Household Survey					
% Scheduled Castes and Scheduled Tribes	0.27	0.25	-0.02	0.01	390
% Other Backward Castes	0.59	0.61	0.02	0.02	390
% House without a solid roof	0.38	0.40	0.02	0.02	390
% Owns Land	0.58	0.57	-0.01	0.02	390
% Male Head	0.78	0.77	-0.01	0.01	390
% Literate Head	0.56	0.55	-0.01	0.02	390
Household Size	6.15	6.03	-0.12	0.07	390
Number of adults in the household	3.41	3.35	-0.06	0.07	390
Normalized Index	0.00	-0.36	-0.36	0.31	390
Panel C: nrega.nic.in reports (April 2011- March 2012)					
MGNREGS beneficiary households	184	193	9.30	8.30	2,968
MGNREGS work days provided	6155	6533	378.20	332.20	2,968
MGNREGS labor expenditures (100,000 rupees)	7.53	8.52	0.99	0.49	2,968
MGNREGS material expenditures (100,000 rupees)	6.50	7.01	0.51	0.43	2,968
Normalized Index	0.00	0.36	0.36	0.20	2,968
Panel D: CPSMS reports (Sept 2011- March 2012)					
MGNREGS funds spent (CPSMS)	9.12	8.94	-0.18	0.47	2,942
MGNREGS funds received (CPSMS)	9.66	9.83	0.17	0.56	2,942
Normalized Index	0.00	0.00	0.00	0.13	2,942

Note: The unit of observation is a Gram Panchayat (GP). Out of 3067 GPs from our sample list, we were able to match 2937 GPs with census 2011 data (Panel A). We surveyed 390 GPs (Panel B). We were able to match 2968 GPs of our sample list with nrega.nic.in data (Panel C) and 2942 GPs with CPSMS data (Panel D). Normalized indexes are computed by substracting the control mean from each variable and dividing by the standard deviation in the control, and taking the sum across all variables in the panel. The difference between control and treatment blocks is estimated using a regression of each GP 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.

Table 2—: Reform Impact on MGNREGS Expenditure: Evidence from CPSMS Data

	Before	**Set-up**	**Intervention Period**			**After**
	Sept 2011 - June 2012	July - Aug 2012	Sept - Dec 2012	Jan - Mar 2013	Whole Period	Apr 2013 - Jan 2014
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total debit from GP accounts						
Treatment	0.0472	-1.039	-1.267	-2.306	-0.323	-0.0927
	(0.291)	(0.315)	(0.280)	(0.530)	(0.832)	(0.245)
Observations	3,025	3,025	3,025	3,025	3,025	3,025
Mean in Control	4.122	5.394	4.146	9.540	16.01	4.166
Panel B: Closing balance in GP accounts						
Treatment	0.133	-0.950	-1.299	-1.266	-0.147	-0.179
	(0.217)	(0.232)	(0.243)	(0.240)	(0.239)	(0.830)
Observations	3,025	3,025	3,025	3,025	3,025	3,025
Mean in Control	4.429	4.091	4.271	4.270	4.291	15.27
Panel C: Total credit to GP accounts						
Treatment	0.251	-2.192	-1.249	-3.441	0.919	0.919
	(0.338)	(0.367)	(0.335)	(0.548)	(0.819)	(0.819)
Observations	3,025	3,025	3,025	3,025	3,025	3,025
Mean in Control	4.282	5.146	4.006	9.151	15.90	15.90

Note: The unit of observation is a Gram Panchayat (GP). Data were downloaded from the CPSMS portal in November 2014. The dependent variable in Panel A is the sum of debits from the savings account of each GP for each period (in 100,000 Rupees). The dependent variable in Panel B is the closing balance on the savings account of each GP at the end of each period (in 100,000 Rupees). The dependent variable in Panel C is the sum of credits made to the savings account of each Panchayat for each period (in 100,000 Rupees). Treatment is a dummy set 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—: Reform Impact on MGNREGS Expenditure: Evidence from the Public information Database (nrega.nic.in)

	Before	**Set-up and Intervention**	**After**
	Apr 2011 - Mar 2012	Apr 2012 - Mar 2013	Apr 2013 - Mar 2014
	(1)	(2)	(3)
Panel A: GP expenditures on labor from nrega.nic.in			
Treatment	0.985	-2.246	-0.218
	(0.494)	(0.758)	(0.730)
Observations	2,968	2,965	2,972
Mean in Control	7.528	13.78	13.62
Panel B: GP expenditures on material from nrega.nic.in			
Treatment	0.510	-1.078	0.330
	(0.429)	(0.529)	(0.534)
Observations	2,968	2,965	2,972
Mean in Control	6.498	7.728	8.376

Note: The unit of observation is a Gram Panchayat (GP). The dependent variables are expenditures from MIS reports for financial years 2011-12, 2012-13, and 2013-14 (in 100,000 Rupees). Expenditures were available for 2,968 GP in 2011-12, 2,965 GP in 2012-13 and 2,972 GP in 2013-14. 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 set 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—: Reform Impact on MGNREGS Employment: Evidence from the Public Information Database (nrega.nic.in)

	Before	Set-up	Intervention Period			After
	Apr 2011 - June 2012	July-Aug 2012	Sept-Dec 2012	Jan - Mar 2013	Whole Period	Apr 2013 - Mar 2014
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Days worked						
Treatment	222.2	-130.3	-404.6	-267.8	-672.4	-859.5
	(474.8)	(111.5)	(227.6)	(163.3)	(363.6)	(542.7)
Observations	2,959	2,959	2,959	2,959	2,959	2,959
Mean in Control	9255	1058	2759	2269	5028	10603
Panel B: Days per working household						
Treatment	-0.0941	-0.712	-0.286	0.187	-0.00410	-0.308
	(1.018)	(0.605)	(0.805)	(0.701)	(0.930)	(0.838)
Observations	2,947	2,514	2,728	2,717	2,868	2,945
Mean in Control	35.64	17.35	29.14	25.14	33.65	39.54
Panel C: Number of working households						
Treatment	7.369	-3.132	-10.02	-8.342	-13.60	-15.03
	(11.85)	(5.151)	(6.233)	(5.700)	(8.150)	(10.33)
Observations	2,959	2,959	2,959	2,959	2,959	2,959
Mean in Control	252.7	59.92	91.68	90.37	140.2	257.2
Panel D: Wages received (100,000 rupees)						
Treatment	0.365	-0.189	-0.648	-0.410	-1.058	-1.529
	(0.726)	(0.178)	(0.353)	(0.255)	(0.570)	(0.911)
Observations	2,959	2,959	2,959	2,959	2,959	2,959
Mean in Control	13.86	1.671	4.319	3.461	7.780	17.64
Panel E: Average Delay in payments (days)						
Treatment	7.185	12.05	24.30	16.71	20.36	9.332
	(6.186)	(5.798)	(4.490)	(2.860)	(3.813)	(2.664)
Observations	2,583	2,268	2,529	2,559	2,735	2,842
Mean in Control	64.42	72.85	71.22	35.20	53.28	37.68

Note:+B18:K49 The unit of observation is a Gram Panchayat (GP). The dependent variable in Panel A is the total number of days provided. The dependent variable in Panel B is the total number of days provided to households reported to have worked; it is missing if there is no employment reported for a GP in a given period. The dependent variable in Panel C, is the number of households reported to have worked. In Panel D, it is the total payments made to beneficiaries (in 100,000 rupees). In Panel E, the dependent variable is the average number of days between work and payment; it is missing if no payment date is reported for a GP in a given period. Data for 2959 GPs were extracted from job card and muster roll information on the nrega.nic.in server in June 2014. They covers the period from April 2011 to March 2014. 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 5—: Reform Impact on MGNREGS Employment: Evidence from Household Survey

	Set-up	Intervention Period			Post-Intervention
	Jul - Aug 2012	Sept - Dec 2012	Jan - Mar 2013	Whole Period	Apr - Jun 2013
	(1)	(2)	(3)	(4)	(5)
Panel A: MGNREGS Participation					
Treatment	-0.00617	0.00550	0.00329	0.00892	0.00165
	(0.00288)	(0.00352)	(0.00322)	(0.00462)	(0.00519)
Observations	195	195	195	195	195
Mean in Control	0.0113	0.0132	0.0175	0.0296	0.0325
Panel B: Number of days worked					
Treatment	-0.124	0.124	0.279	0.403	0.359
	(0.0588)	(0.124)	(0.158)	(0.212)	(0.600)
Observations	195	195	195	195	195
Mean in Control	0.206	0.389	0.550	0.938	1.792
Panel C: Wages received for MGNREGS employment					
Treatment	-13.68	3.370	11.04	14.41	-3.789
	(6.782)	(13.22)	(13.79)	(20.53)	(35.64)
Observations	195	195	195	195	195
Mean in Control	21.87	36.33	42.40	78.73	102.4
Panel D: Average delays in payment (days)					
Treatment	-56.15	46.38	37.41	47.00	3.393
	(29.75)	(23.06)	(12.78)	(13.42)	(9.705)
Observations	74	90	118	148	144
Mean in Control	78.19	71.39	50.85	64.47	38.15

Note: The unit of observation is a block. The dependent variable in Panel A is the fraction of households who participated in MGNREGS. In Panel B, it is the average number of days worked by households under MGNREGS. In Panel C, it is average wage payments received by households for MGNREGS employment. In Panel D, it 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. The data was collected by a representative survey of 9,670 households in 390 GP (two per block) in May-July 2013. In Panel D, the sample only includes blocks in which we surveyed households with observed completed MGNREGS payments. Households were asked about work spells from July 2012 to the time of the survey. We compute GP-level averages using sampling weights. 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 the fraction of hindu households, of Other Backward Castes households, of Scheduled Castes households, of Scheduled Tribes households, of households who live in a house made of mud, and of land-owning households in the GP. It also includes average household size and average number of adults per household in the GP.

Table 6—: Reform Impact on Fake Beneficiaries: Evidence from Matching of Job Cards in the Public Information Data Base with SECC Census

	All job cards	**Job cards with at least one working member**	
	(as of April 2014)	Intervention period (Sept 2012-March 2013)	Post intervention (Apr 2013 - March 2014)
	(1)	(2)	(3)
Panel A: Match rate for job cards with one name only			
Treatment	0.0190	0.0195	0.0104
	(0.00742)	(0.00769)	(0.00693)
Observations	3,083	2,676	2,940
Mean in Control	0.643	0.682	0.699
Panel B: Match rate for job cards with two names or more			
Treatment	0.0132	0.0119	0.00953
	(0.00613)	(0.00810)	(0.00730)
Observations	3,081	2,803	2,924
Mean in Control	0.243	0.281	0.286

Note: The unit of observation is a GP. The dependent variable is the fraction of job cards from nrega.nic.in matched by name with households from the SECC census. A job card with two members or more is matched when at least two members have been matched by name with a census household. The nrega.nic.in data was extracted from the Ministry of Rural Development server, and covers the period from July 2011 to March 2014. The dependent variable is missing when no job card data was available for a given GP or when were unable to find a GP in the SECC census. Treatment is a dummy set 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 7—: Reform Impact on Assets of MGNREGS officials: Evidence from Affidavit Data

	Average Effect (OLS)				Effect at the Median (Quantile Regression)			
	2012-13		2013-14		2012-13		2013-14	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Movable assets								
Treatment	-0.117	-0.119	-0.0345	-0.0321	-0.101	-0.088	-0.073	-0.057
	(0.0968)	(0.0972)	(0.0753)	(0.0741)	(0.053)	(0.046)	(0.062)	(0.053)
Observations	2,453	2,453	1,734	1,734	2,453	2,453	1,734	1,734
Kolmogorov Smirnov p-value (for stochastic dominance)					.03		.63	
Panel B: Total assets								
Treatment	-0.0754	-0.0659	-0.102	-0.115	-0.117	0.005	-0.137	-0.193
	(0.130)	(0.128)	(0.103)	(0.102)	(0.073)	(0.068)	(0.074)	(0.069)
Observations	2,455	2,455	1,737	1,737	2,455	2,455	1,737	1,737
Kolmogorov Smirnov p-value (for stochastic dominance)					.11		.06	
Functionary Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: The unit of observation is a yearly asset declaration by a MGNREGS official. Declarations 2012-13 were made from August 2012 to June 2013. Declarations 2013-14 were made from July 2013 to September 2014. The intervention period was September 2012 to April 2013. GP-level functionaries are Panchayat Rozgar Sewak. Block-level functionaries are Program Officers, Accountants, Computer Operators, Junior Engineers, Program Technical Assistants, and Executive Assistants. In Panel A, the dependent variable is the log of total movable assets (cash, jewellery, bank deposits, bonds, vehicles). In Panel B, it is the log of all assets, including movable assets and immovable assets (e.g. land, buildings). Functionary Controls include the age, the square of age, dummies for gender and functionary designation, and a dummy for whether the functionary is posted in the district she was born in. All specifications include district fixed effects. Standard errors are clustered at the block level.

Table 8—: Effect of e-FMS Implementation on Wage and Material Expenditures

	District Expenditures (in 100,000 Rupees)			
	(1)	(2)	(3)	(4)
Panel A: Expenditures on labor from nrega.nic.in				
e-FMS for wage payments in year t	-770.4	-705.4	-703.4	-790.5
	(179.6)	(170.8)	(179.3)	(197.4)
	[0]	[0]	[0]	[0]
e-FMS for material payments in year t		-223.8	-234.0	-251.8
		(185.9)	(203.5)	(204.2)
e-FMS for wage payments in t and t-1			22.16	57.14
			(203.5)	(200.1)
e-FMS for wages payments in t, t-1 and t-2				-582.6
				(251.2)
Observations	4253	4253	4253	4253
Mean in Control	4140.4	4140.4	4140.4	4140.4
Panel B: Expenditures on material from nrega.nic.in				
e-FMS for wage payments in year t	-232.1	-247.1	-266.0	-0.0626
	(85.43)	(89.67)	(94.48)	(0.0372)
	[0]	[0]	[0]	[0.0100]
e-FMS for material payments in year t	-341.1	-264.2	-268.1	
	(80.73)	(85.20)	(85.32)	
e-FMS for wage payments in t and t-1		-166.4	-158.8	
		(86.08)	(84.20)	
e-FMS for wages payments in t, t-1 and t-2			-125.9	
			(93.25)	
Observations	4253	4253	4253	4240
Mean in Control	1703.5	1703.5	1703.5	7.734

Note: The unit of observation is a district*year. The dependent variables are expenditures from MIS reports for financial years 2008 to 2016 (in 100,000 Rupees). The data was downloaded from nrega.nic.in in April 2017. "e-FMS for wage (material) payments in year t" is a dummy variable set equal to one if e-FMS is effective for wage (material) payments that year. "e-FMS for wage (resp. material) payments in year t and t-1" is a dummy variable equal to one if e-FMS was used for wage (resp. material) payments both this year and the year before. It is thus the additional effect of having the program for two years (compared to one). All specifications include district fixed effects and year fixed effects. Robust Standard errors are in parentheses, and p-value from randomization inference (100 replications) in brackets.

A.1. Data Appendix

We first discuss the official data on expenditures and employment, then the surveys we implemented to assess actual MGNREGS implementation, and finally three additional sources we use to measure corruption.

A. Administrative data on MNREGS implementation

We use two sources of official reports on MGNREGS expenditures and employment.

CPSMS portal: In July 2014, we were granted access to detailed information on MGNREGS expenditures via the Central Planning Scheme Monitoring (CPSMS) Portal. Both treatment and control GPs 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 Januaray 2013 to March 2013 and from the end of the intervention in April 2013 until July 2014.

Website 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 starts on April 1st). 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 in July 2014 on GP spending on labor and material for the financial years 2011-12, 2012-13 and 2013-14. These expenditures include payments that are recorded and bills for which the payment date is missing (which are hence considered as pending in nrega.nic.in).

Labor expenditure figures in nrega.nic.in aggregate across work and payment details for specific MGNREGS workers. These worker-level data are also entered on the website and made publicly available in the form of muster rolls and job cards. The online job card mimics the physical job card delivered to all households who register for MGNREGS work: the rule of one job card per household is not always followed in practice, so that members of a given households may appear on different job cards. We requested access to job card information from the Ministry of Rural Development and were provided with the details of 4,197,904 job cards and 6,292,307 workers in our sample districts for the financial years 2011-12, 2012-13 and 2013-14.

The online muster roll mimics the attendance roll on which working days and earnings are recorded on site. It gives for each job card the total number of days worked, wages earned, payments received and the date of the payment, from which we compute the delay between work and payment.

B. Independent surveys on MGNREGS implementation

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. We visited every block in these districts, surveying a total of 195 blocks – 69 treatment blocks and 126 control blocks. We surveyed 2 randomly sampled GPs in each block, giving us a total of 390 GPs. 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 conducted a household survey covering 9,670 households. In each GP, we attempted to cover 25 households 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 Below Poverty Line households, so each household was given a poverty score, based on various criteria. From these lists, we sampled 72 percent of households below the median poverty score and 28 percent households from above the score. If 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 percent.

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, and managed to locate and interview a total of 346 Mukhiyas out of 390 GP visited. The response rate is balanced across treatment and control blocks. Unlike the other two surveys, the Mukhiya survey was conducted on paper and was both quantitative and qualitative in nature.

C. Additional administrative data

We use three additional sources of administrative data to provide evidence on corruption in MGNREGS implementation: the Socio-Economic Caste Census, affidavit data and audits data.

SECC and name matching: To identify " ghost workers," we attempt to match each working household reported on an nrega.nic.in job card to a household within the SECC data. The 2012 Socio-Economic Caste Census (SECC) is a national survey of all persons and households in rural and urban India. It is based on the National Population Register from the 2011 Population Census, but was conducted mostly in 2012 due to various implementation issues. The SECC data includes the name, father's name (or husband's name for married women), gender, education, and other information for each member of the household and the household overall. In the 12 districts of our sample (inclusive of rural villages only), the SECC data covers 16,480 villages, five million households, and 34 million individuals. The job cards data covers 18,513 villages, 4,197,904 working households, and 6,292,307 working household members.

In the first step, we pair villages in the job cards with corresponding villages in the SECC data to impose the restriction that we search for matching households only within the same village. In the second step, we match households from the job cards data to the SECC data within village pairs based on similarity of name, gender, and household composition. We calculate the closeness of village names in the first step and individuals' names in the second step using a modified levenshtein algorithm graciously made available by Paul Novosad (lev.py downloaded from `http://www.dartmouth.edu/~novosad/code.html`). We partially alter this algorithm to account for alternative spellings, missing/additional portions of names, and abbreviations.

In the first step, we take the following approach to determine village pairs. While the job cards data contains information on block, GP, and village name, the SECC data contains corresponding information for block and village name only. We attempt to match by name each of the 18,513 unique villages in the job cards data within block with a corresponding SECC village. We are able to match 84 percent of the job cards villages (containing 88 percent of households). We match the other 16 percent of the job card villages (12 percent of households), to all SECC villages which are matched with job card villages belonging to the same GP. For about 0.5 percent of villages (0.7 percent of households), we are unable to do either and match them with all the villages in the block.

In the second step, we attempt to find a match for each of the job cards from within the paired village or list of villages. We declare a household with one working member listed on the job card as matched if a single matching individual in the SECC data is found, and we declare a household with two or more members listed on the job card as matched if at least two individuals within the same SECC household are matched. The matching rate is thus mechanically lower for households with two working members (37 percent of households, of which 25 percent are matched) than for households with one working member (63 percent of households, of which 64 percent are matched). Individuals are matched based on two primary criteria: gender, which must match exactly, and name, which must be sufficiently close based on the algorithm described above. Note that once a suitable household match is found according to this process for one or more members, all other members of the job cards household are declared as coming from a matched household. In contrast, the matched SECC household is not removed from the pool of potential matches as the algorithm moves on.

Our outcome of interest is the match rate, calculated separately for people reported to have worked during the period of the intervention and people reported to have worked after the intervention; the idea is that a name or household who is supposed to have a job card in the MGNREGS data but is not found in the

SECC database is more likely to be a " ghost" than those who are found in both. This exercise is therefore a population-level version of the forensic method pioneered by Niehaus and Sukhtankar (2013), using exclusively administrative data. We recognize that the databases are both imperfect. There are surely errors in both directions (individuals might be omitted from the SECC census for example, or the matching could have failed because the names are spelled too differently to match, or someone could be matched to someone else with the same name). But these errors should not be different in treatment and control groups as the process of uploading worker details into nrega.nic.in was unaffected by the reform: in treatment blocks, it was independent from data entry into CPSMS.

Affidavit data: We also collected affidavits of MGNREGS employees. In the financial years 2012-13 and 2013-14, the Ministry of Rural Development of Bihar made it mandatory for all its employees to declare their personal assets, including cash, movable and immovable assets owned by them or a member of their household. The affidavits were scanned and the pdf files were made available online on the website of each district. Compliance was not perfect: in total we collected 2,463 affidavits for the financial year 2012-13 and 1,741 for the financial year 2013-14 in the 12 districts of our experimental sample. We construct our measure of MGNREGS employees' personal wealth by adding the value of movable (cash, bank deposits, bonds, jewellery, other financial assets, vehicles) and immovable assets (land, buildings, other immovables) of the employee and his or her spouse. When the value of the jewellery is missing but the weight of gold or silver owned is given, we impute the value using international prices from `http://www.bullion-rates.com`. For District Development Coordinators, who are in charge of MGNREGS implementation in each district, we have data for all districts of Bihar for three financial years: 2011-12, 2012-13 and 2013-14.

Audits data: Finally, we use reports on MGNREGS audits carried out by the administration of each district between May 2012 and June 2013. These reports were compiled in July 2013 by the Rural Development Department to inform the process evaluation of MGNREGS audits by IDinsight (2013). The data include the audit date, block and GP names, the number of MGNREGS projects audited and irregularities found. We aggregate this information and compute the number of audits, the number of projects audited, the number of irregularities found and the number of irregularities per project audited in each block for three periods: May to August 2012 (pre-intervention), September 2012 to March 2013 (intervention period) and April to June 2013 (post-intervention). Unfortunately, completion date of each project audited is not recorded, but the Rural Development Department letter no.120078 (September 1st, 2012) instructs audit teams to select projects undertaken in the financial years 2011-12 and 2012-13. Since the financial year 2012-13 ended in March 2013, projects audited in April to June 2013 had been undertaken during the intervention period. Data on administrative sanctions, dismissals and police investigations against MGNREGS officials responsible for these irregularities were not collected in a systematic manner.

Figure A.1. : Map of Sample Districts and Border Blocks

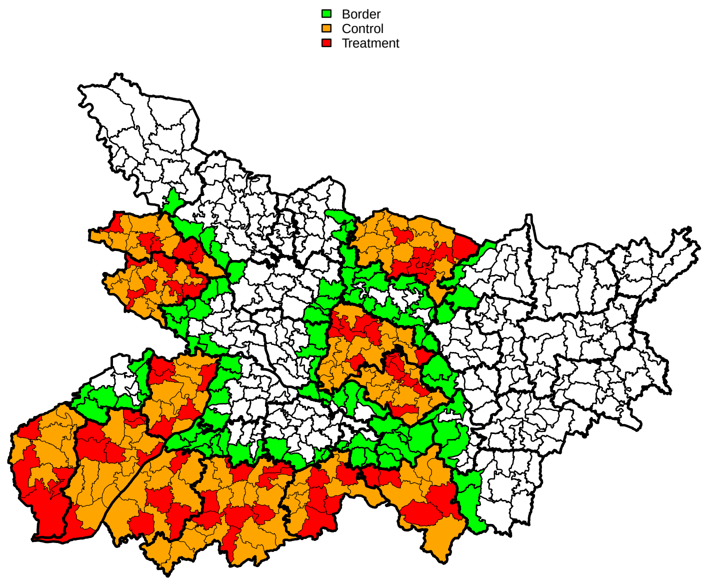


Figure A.2. : Total MGNREGS Expenditures (2006-2016)

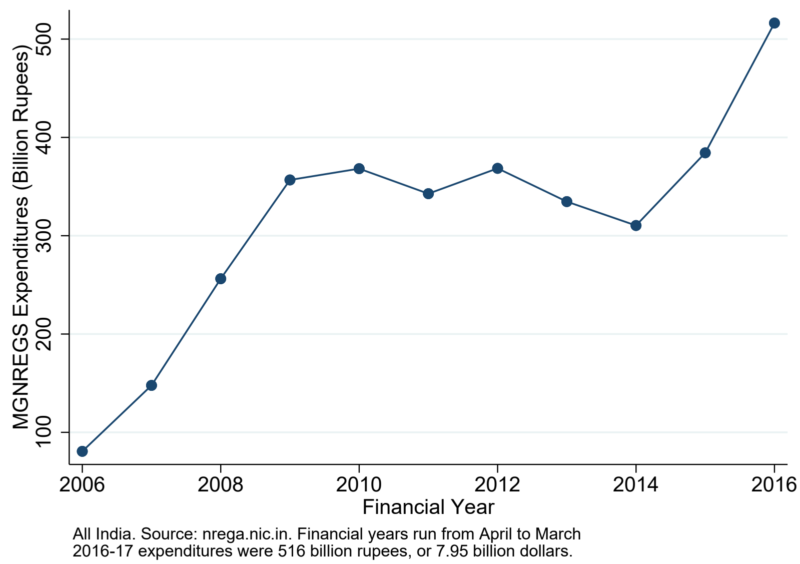


Figure A.3. : Fraction of Treatment GPs that used CPSMS at least once

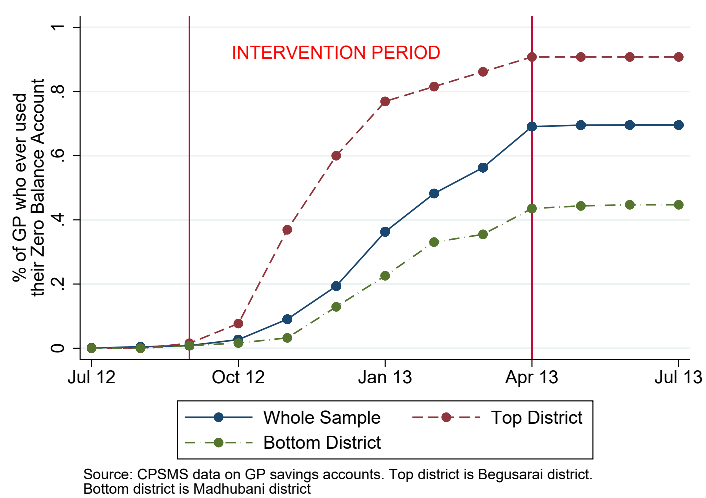


Table A.1—: Infrastructure Availability

	Required in Treatment	July '12		January '13	April '13	
		Treatment	Control	Treatment	Treatment	Control
Infrastructure	(1)	(2)	(3)	(4)	(5)	(6)
Computers (number)	3	1.32	1.06	2.48	2.06	1.61
Operators (number)	3	1.22	0.86	2.20	1.75	1.27
Generator (1=Yes 0=No)	1	0.67	0.56	0.97	0.90	0.85
Internet (1=Yes 0=No)	1	0.38	0.33	0.85	0.71	0.60
Scanner (1=Yes 0=No)	1	0.57	0.37	0.73	0.81	0.65
Printer (1=Yes 0=No)	1	0.59	0.43	0.71	0.83	0.76
Sampled Blocks		69	126	66	69	123

Source: Phone surveys of block level MGNREGS functionaries (Program officers). Only treatment blocks were called in January 2013. In preparation for the intervention, infrastructures requirement were communicated to treatment blocks at the end of June 2012. The intervention started in September 2012 and ended in April 2013.

Table A.2—: MGNREGS Spending Levels from Different Data Sources

	Control	Treatment	Difference	P-value
	(1)	(2)	(3)	(4)
Panel A: CPSMS and MIS				
Debit in CPSMS				
2012-13	19.23	16.81	-2.43	0.11
2013-14	16.18	16.04	-0.15	0.91
Total Expenditures in MIS				
2012-13	21.62	18.24	-3.38	0.05
2013-14	22.24	22.40	0.16	0.92
Difference CPSMS-MIS				
2012-13	-2.38	-1.43	0.95	0.15
2013-14	-6.05	-6.36	-0.31	0.63
Panel B: Job cards and MIS				
Payments in Job Cards				
2011-12	7.66	8.81	1.15	0.19
2012-13	15.69	14.24	-1.45	0.30
2013-14	17.71	16.16	-1.55	0.33
Labor Expenditures in MIS				
2011-12	7.57	9.02	1.45	0.07
2012-13	13.86	11.64	-2.23	0.06
2013-14	13.76	13.56	-0.20	0.86
Difference Job Cards-MIS				
2011-12	0.09	-0.21	-0.30	0.54
2012-13	1.82	2.60	0.78	0.03
2013-14	3.95	2.59	-1.35	0.05

Source: CPSMS Credit Debit Data, MIS Financial Reports (nrega.nic.in), Job Cards (nrega.nic.in). All amounts are annual GP averages in 100,000 rupees. 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—: Reform Impact on MGNREGS Audits

	Before	**Intervention Period**	**Post-Intervention**
	Jan 2011 - Aug 2012	Sep 2012 - Mar 2013	Apr - Jun 2013
	(1)	(2)	(5)
Panel A: Number of audits			
Treatment	0.173	0.113	0.0371
	(0.149)	(0.464)	(0.191)
Observations	195	195	195
Mean in Control	1.079	7.286	2.540
Panel B: Number of works audited			
Treatment	2.278	-1.483	0.519
	(4.847)	(2.984)	(1.091)
Observations	195	195	195
Mean in Control	16.82	34.72	9.341
Panel C: Number of works where irregularities were found			
Treatment	-0.863	-0.191	0.264
	(1.780)	(0.813)	(0.192)
Observations	195	195	195
Mean in Control	4.397	3.302	0.460
Panel D: Share of works where irregularities were found			
Treatment	-0.0476	0.00593	0.0452
	(0.0518)	(0.0194)	(0.0261)
Observations	113	188	143
Mean in Control	0.217	0.0889	0.0509

Note: The unit of observation is a block. Data was collected by the Rural Development Department, Government of Bihar. The dependent variables are the number of audits in each period (Panel A), the number of works audited (Panel B) the number of works were irregularities were found (Panel C), and the share of works where irregularities were found (Panel D). Each column presents results from a separate regression using data for a different time period. There are missing observations in Panel D for blocks that had no works audited in a given period.

Table A.4—: Main Results with and without Controlling for MGNREGS Employment and Expenditures Levels before the Reform

	Without Control	With Control		Without Control	With Control
	(1)	(2)		(3)	(4)
Table 2			**Panel D: Wages received from nrega.nic.in (100,000 rupees)**		
Panel A: Total debit from GP accounts			Treatment	-1.058	-1.347
Treatment/use system	-2.306	-2.567		(0.570)	(0.582)
	(0.530)	(0.520)	Observations	2,959	2,959
Observations	3,025	3,025	Mean in Control	7.780	7.780
Mean in Control	9.540	9.540	**Panel E: Average delays in payment from nrega.nic.in**		
Panel B: Closing balance in GP accounts			Treatment	20.36	20.04
Treatment/use system	-1.266	-1.313		(3.813)	(3.776)
	(0.240)	(0.244)	Observations	2,735	2,735
Observations	3,025	3,025	Mean in Control	53.28	53.28
Mean in Control	4.270	4.270			
Panel C: Total credit to GP accounts			**Table 5**		
Treatment/use system	-3.441	-3.660	**Panel A: MGNREGS participation**		
	(0.548)	(0.545)	Treatment	0.00892	0.00809
Observations	3,025	3,025		(0.00462)	(0.00469)
Mean in Control	9.151	9.151	Observations	195	195
			Mean in Control	0.0296	0.0296
Table 3			**Panel C: Wages received for MGNREGS employment**		
Panel A: GP expenditures on labor from nrega.nic.in			Treatment	14.41	13.11
Treatment/use system	-2.246	-2.839		(20.53)	(21.84)
	(0.758)	(0.707)	Observations	195	195
Observations	2,965	2,965	Mean in Control	78.73	78.73
Mean in Control	13.78	13.78	**Panel D: Average delays in payment (days)**		
Panel B: GP expenditures on material from nrega.nic.in			Treatment	47.00	47.99
Treatment/use system	-1.078	-1.351		(13.42)	(13.79)
	(0.529)	(0.519)	Observations	148	148
Observations	2,965	2,965	Mean in Control	64.47	64.47
Mean in Control	7.728	7.728			
			Table A.6 Fraction of assets found		
Table 4			Treatment	0.310	0.267
Panel A: Days worked from nrega.nic.in				(0.239)	(0.242)
Treatment	-672.4	-859.4	Observations	385	385
	(363.6)	(367.6)	Mean in Control	11.68	11.68
Observations	2,959	2,959			
Mean in Control	5028	5028	**Table 6**		
Panel B: Days per working household from nrega.nic.in			**Panel A: Match rate for job cards with one name only**		
Treatment	-0.00410	-0.0554	Treatment	0.0152	0.0161
	(0.930)	(0.934)		(0.00787)	(0.00783)
Observations	2,868	2,868	Observations	2,836	2,836
Mean in Control	33.65	33.65	Mean in Control	0.679	0.679
Panel C: Number of working households from nrega.nic.in			**Panel B: Match rate for job cards with two names or more**		
Treatment	-13.60	-18.65	Treatment	0.0119	0.0125
	(8.150)	(7.946)		(0.00810)	(0.00821)
Observations	2,959	2,959	Observations	2,803	2,803
Mean in Control	140.2	140.2	Mean in Control	0.281	0.281

Note: Column 1 presents the treatment effect for the whole intervention period estimated without controls. Column 2 presents the treatment effect for the whole intervention period estimated with a normalized index of four indicators of MGNREGS implementation in 2011-12 (the four indicators are presented in Panel C of Table 1). The panels correspond to the main tables of the paper. The unit of observation is the Gram Panchayat for Table 2, 3, 4, 6 and 7. The unit of observation is a block in Table 5. The data sources are CPSMS financial data (Table 2), official reports from nrega.nic.in (Table 3 and 4), our own survey data (Table 5 and 6) and the match between nrega.nic.in reports and socio-economic and caste census data (Table 7).

Table A.5—: Reform Impact on MGNREGS Expenditure: Evidence from CPSMS Data. Specification with Inverse Hyperbolic Sine (IHS).

	Before	**Set-up**	**Intervention Period**			**After**
	Sept 2011 - June 2012	July - Aug 2012	Sept - Dec 2012	Jan - Mar 2013	Whole Period	Apr 2013 - Jan 2014
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IHS of total debit from GP accounts						
Treatment	-0.0701	0.0518	-0.210	-0.371	-0.289	-0.00269
	(0.0613)	(0.0590)	(0.0496)	(0.0636)	(0.0542)	(0.0576)
Observations	3,025	3,025	3,025	3,025	3,025	3,025
Mean in Control	14.37	4.122	5.394	4.146	9.540	16.01
Panel B: IHS of closing balance in GP accounts						
Treatment	-0.0262	0.0180	-0.238	-0.324	-0.314	-0.0252
	(0.0516)	(0.0363)	(0.0451)	(0.0452)	(0.0450)	(0.0486)
Observations	3,025	3,025	3,025	3,025	3,025	3,025
Mean in Control	4.166	4.429	4.091	4.271	4.270	4.291
Panel C: IHS of total credit to GP accounts						
Treatment	0.00578	0.0926	-0.447	-0.205	-0.493	0.137
	(0.0678)	(0.0701)	(0.0604)	(0.0770)	(0.0689)	(0.0616)
Observations	3,025	3,025	3,025	3,025	3,025	3,025
Mean in Control	15.27	4.282	5.146	4.006	9.151	15.90

Note: The unit of observation is a Gram Panchayat (GP). Data was downloaded from the CPSMS portal in November 2014. The dependent variable in Panel A is the inverse hyperbolic sine of the sum of debits from the savings account of each GP for each period (in 100,000 Rupees). In Panel B, it is the inverse hyperbolic sine of the closing balance on the savings account of each GP at the end of each period (in 100,000 Rupees). In Panel C, it is the the inverse hyperbolic sine of the sum of credits made to the savings account of each Panchayat for each period (in 100,000 Rupees). Treatment is a dummy set 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 A.6—: Reform Impact on Household MGNREGS Participation (Household Survey)

	Household Participation in MGNREGS	
	Anytime Before	Since July 2012
	(1)	(2)
Treatment	-0.00646	0.00508
	(0.0144)	(0.00818)
Observations	195	195
Mean in Control	0.238	0.0775

Note: The unit of observation is a block In Column 1, the outcome is the fraction of households who worked for MGNREGS any time in the past. In Column 2, the outcome is a the fraction of household who worked for MGNREGS since July 2012. The data was collected by a representative survey of 9,670 households across 390 GP and 195 blocks in May-July 2013. Treatment is a dummy set equal to one for the blocks selected for the intervention. All specifications include district fixed effects and household controls. Household controls include the fraction of Hindu households, of Other Backward Castes households, of Scheduled Castes households, of Scheduled Tribes households, of households who live in a house made of mud, and of land-owning households in the GP. It also includes average household size and average number of adults per household in the GP.

Table A.7—: Reform Impact on Household Consumption (Household Survey)

	Log Monthly Consumption			
	All	Frequent Expenditures	Recurrent Expenditures	Rare Expenditures
	(1)	(2)	(3)	(4)
Treatment	0.0192	0.0123	-0.0149	0.0308
	(0.0251)	(0.0201)	(0.0304)	(0.0463)
Observations	195	195	195	195

Note: The unit of observation is a block. The dependent variables are the log of household monthly expenditures for different categories of expenditures. Frequent expenditures include cereals, milk and paan/tobacco expenditures in the last week. Recurrent expenditures include egg/fish/meat, personal care and mobile phone expenditures in the last month. Rare expenditures include clothing, health and celebration expenditures in the past five months. The data was collected by a representative survey of 9,670 households across 390 GP and 195 blocks in May-July 2013. Treatment is a dummy set 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.8—: Reform Impact on MGNREGS Implementation Issues: Evidence from GP Head (Mukhiya) Survey

Panel A: Lack of demand for MGNREGS work	
Treatment	0.0116
	(0.0458)
Observations	346
Mean in Control	0.379
Panel B: Mandated price of material lower than market price	
Treatment	0.0206
	(0.0284)
Observations	346
Mean in Control	0.833
Panel C: Lack of funds from the government	
Treatment	-0.0107
	(0.0490)
Observations	346
Mean in Control	0.718
Panel D: Corruption in the administration	
Treatment	-0.118
	(0.0556)
Observations	346
Mean in Control	0.471
Panel E: CPSMS fund flow creates delays	
Treatment	0.181
	(0.0508)
Observations	346
Mean in Control	0.167

Note: The unit of observation is a Mukhiya (head of GP). The dependent variables are the fractions of Mukhiya who declared that the lack of demand for MGNREGS work (Panel A), the mandated price of material lower than the market price (Panel B), the lack of funds from the government (Panel C) corruption in the administration (Panel D) and delays in fund flow created by CPSMS (Panel E) were important issues in MGNREGS implementation. The data was collected from a representative sample of 346 Mukhiya from treatment and control blocks in May-July 2013. Treatment is a dummy set 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, age, and whether any member of the family was elected Mukhiya in 2001 and 2006. Standard errors are clustered at the block level.

Table A.9—: Reform Impact on Leakages of MGNREGS funds: Difference between Survey-Based Population Estimates and Official Reports

	Set-up	Intervention Period			Post-Intervention
	July-Aug 2012	Sept-Dec 2012	Jan - Mar 2013	Whole Period	Apr - Jun 2013
	(1)	(2)	(3)	(4)	(5)
Panel A: Days worked per HH (survey vs MIS)					
Treatment	-0.177	0.263	0.0747	0.337	0.641
	(0.239)	(0.340)	(0.320)	(0.592)	(1.146)
Observations	194	194	194	194	194
Mean in Control	-0.456	-1.367	-0.894	-2.261	-4.897
Panel B: Wages received per HH (survey vs CPSMS)					
Treatment	0.0146	0.100	0.102	0.202	-0.0911
	(0.0301)	(0.0393)	(0.0338)	(0.0632)	(0.105)
Observations	193	193	193	193	193
Mean in Control	-0.225	-0.269	-0.176	-0.445	-0.866

Note: The unit of observation is a block. Within each block, we only use information about the two GP who were surveyed. The dependent variable in Panel A is the difference between the number of days worked per household in the survey and the number of days worked in the MIS divided by the number of households from the 2011 census. The dependent variable in Panel B is the difference between wages received per household according to the the survey and debits from GP accounts according to the CPSMS portal divided by the number of households from the 2011 census. The survey data was collected by a representative survey of 9,670 households in 390 GP in May-July 2013. The MIS data for 2959 GPs were extracted from job card and muster roll information on the nrega.nic.in server in June 2014. Data were downloaded from the CPSMS portal in November 2014. Treatment is a dummy which is equal to one for the blocks selected for the intervention. All specifications include district fixed effects.

Table A.10—: Reform Impact on MGNREGS Projects: Evidence from the Asset Survey

	Number Registered		Number Found	
	All Projects	Ongoing	All Projects	Ongoing
	(1)	(2)	(3)	(4)
Treatment	0.0494	-0.210	0.310	0.0269
	(0.263)	(0.413)	(0.239)	(0.265)
Observations	390	390	385	385
Mean in Control	13.8	11.69	11.68	9.75

Note: The unit of observation is a Gram Panchayat (GP). The dependent variables are the number of projects registered in the public information database (nrega.nic.in) on May 15, 2013 (Column 1), the number of projects declared as ongoing in nrega.nic.in (Column 2), the number of registered (Column 3) and ongoing (Column 4) projects found by surveyors in June-July 2013. We surveyed a random subset of 3900 projects (10 per GP) out of 5390 projects registered in nrega.nic.in for the 390 GPs in our survey sample. We scaled up the number of projects found in the survey using the number of registered projects divided by the number of sampled projects rate. 5 GPs (28 projects) could not be surveyed. All specifications include district fixed effects. Standard errors are clustered at the block level.

Table A.11—: OLS and IV Estimates of the Main Results

	OLS	IV		OLS	IV
	(1)	(2)		(3)	(4)
Table 2			**Panel D: Wages received from nrega.nic.in (100,000 rupees)**		
Panel A: Total debit from GP accounts			Treatment	-1.058	-1.622
Treatment/use system	-3.441	-5.363		(0.570)	(0.889)
	(0.548)	(0.910)	Observations	2,959	2,959
Observations	3,025	3,025	Mean in Control	7.780	7.780
Mean in Control	9.151	9.151	**Panel E: Average delays in payment from nrega.nic.in**		
Panel B: Closing balance in GP accounts			Treatment	20.36	30.40
Treatment/use system	-1.266	-1.973		(3.813)	(5.821)
	(0.240)	(0.385)	Observations	2,735	2,735
Observations	3,025	3,025	Mean in Control	53.28	53.28
Mean in Control	4.270	4.270			
Panel C: Total credit to GP accounts			**Table 5**		
Treatment/use system	-2.306	-3.593	**Panel A: MGNREGS participation**		
	(0.530)	(0.863)	Treatment/use system	0.00892	0.0131
Observations	3,025	3,025		(0.00462)	(0.00684)
Mean in Control	9.540	9.540	Observations	195	195
			Mean in Control	0.0296	0.0296
Table 3			**Panel C: Wages received for MGNREGS employment**		
Panel A: GP expenditures on labor from nrega.nic.in				14.41	21.17
Treatment/use system	-2.246	-3.442		(20.53)	(30.29)
	(0.758)	(1.192)	Observations	195	195
Observations	2,965	2,965	Mean in Control	78.73	78.73
Mean in Control	13.78	13.78	**Panel D: Average delays in payment (days)**		
Panel B: GP expenditures on material from nrega.nic.in			Treatment/use system	47.00	70.49
Treatment/use system	-1.078	-1.652		(13.42)	(21.71)
	(0.529)	(0.815)	Observations	148	148
Observations	2,965	2,965	Mean in Control	64.47	64.47
Mean in Control	7.728	7.728			
			Table A.6 Fraction of assets found		
Table 4			Treatment/use system	0.310	0.454
Panel A: Days worked from nrega.nic.in				(0.239)	(0.347)
Treatment	-672.4	-1,031	Observations	385	385
	(363.6)	(566.7)	Mean in Control	11.68	11.68
Observations	2,959	2,959			
Mean in Control	5028	5028	**Table 6**		
Panel B: Days per working household from nrega.nic.in			**Panel A: Match rate for job cards with one name only**		
Treatment	-0.00410	-0.00616	Treatment/use system	0.0152	0.0227
	(0.930)	(1.398)		(0.00787)	(0.0118)
Observations	2,868	2,868	Observations	2,836	2,836
Mean in Control	33.65	33.65	Mean in Control	0.679	0.679
Panel C: Number of working households from nrega.nic.in			**Panel B: Match rate for job cards with two names or more**		
Treatment	-13.60	-20.85	Treatment/use system	0.0119	0.0176
	(8.150)	(12.75)		(0.00810)	(0.0120)
Observations	2,959	2,959	Observations	2,803	2,803
Mean in Control	140.2	140.2	Mean in Control	0.281	0.281

Note: Column 1 presents the treatment effect for the whole set-up and intervention period estimated with OLS. Column 2 presents the treatment effect for the whole set-up and intervention period estimated using treatment as an instrument for the use of CPSMS system. The panels correspond to the main tables of the paper. The unit of observation is the Gram Panchayat for Table 2, 3, 4, 6. For Table 5, the unit of observation is a block The data sources are CPSMS financial data (Table 2), official reports from nrega.nic.in (Table 3 and 4), our own survey data (Table 5 and A.6) and the match between nrega.nic.in reports and socio-economic and caste census data (Table 6). GPs that were present in the survey or nrega.ni.in data but could not be found in CPSMS data were considered as non-compliers.

Table A.12—: Correlation between the Match Rate of Job Cards in the Public Information Data Base with SECC Census and Reported Employment

	Days Worked	Household Participants	Days per Household	Log Functionary Movable Assets	Log Functionary Total Assets
	(1)	(2)	(3)	(4)	(5)
Panel A: Job cards with one name					
Match Rate	-0.193	-0.241	0.0475	-1.818	-0.805
	(0.0999)	(0.0840)	(0.0499)	(0.455)	(0.557)
Observations	2,936	2,936	2,936	2,453	2,455
Mean in Control	8.798	5.304	3.494	1.162	1.644
Panel B: Job cards with two or more names					
Match Rate	-0.0309	-0.0333	0.00239	-0.859	0.575
	(0.104)	(0.0880)	(0.0531)	(0.447)	(0.545)
Observations	2,915	2,915	2,915	2,453	2,455
Mean in Control	8.798	5.304	3.494	1.162	1.644

Note: The dependent variable in Column 1 is the total number of days worked for MGNREGS to official data. In Column 2, it is the total number of households reported as having worked in official data. In Column 3, it is the average number of days worked per participating household according to official data. In Columns 1 to 3, the unit of observation is a GP. Outcomes pertain to the period April 2011 to June 2012 and have been collected from job cards publicly available in nrega.nic.in. The dependent variable in Column 4 is the log of the total personal assets declared by MGNREGS functionaries. In Column 5, it is the log of the total movable personal assets declared by MGNREGS functionaries. The unit of observation in Columns 4 and 5 is a MGNREGS functionary, and the specification includes functionary controls. Functionary Controls include the age, the square of age, dummies for gender and functionary designation, and a dummy for whether the functionary is posted in the district she was born in. In Panel A, the match rate is the fraction of job cards with one worker (49% of all job cards) that we were able to match with the SECC population census. In Panel B, the match rate is the fraction of job cards with two or more workers (51% of all job cards) that we were able to match by name with the SECC population census. All specifications include district fixed effects. Standard errors are clustered at the block level.

Table A.13—: Reform Impact on Assets of MGNREGS officials at the District Level: Non-experimental Evidence from Affidavit Data

	2011-12		2012-13		2013-14	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average Effects on Movable Assets (OLS)						
Intervention District	-0.174	0.0220	-0.399	-0.389	-0.634	0.340
	(0.229)	(0.238)	(0.226)	(0.187)	(0.612)	(0.552)
Observations	38	38	278	278	42	42
Panel B: Effects on Movable Assets at the Median (Quantile Regression)						
Intervention District	-0.0719	-0.0582	-0.527	-0.464	-0.453	-0.594
	(0.290)	(0.356)	(0.198)	(0.171)	(0.488)	(0.577)
Observations	38	38	278	278	42	42
Kolmogorov Smirnov p-value	.91		.01		.09	
(for stochastic dominance)						
Panel C: Average Effects on Total Assets (OLS)						
Intervention District	-0.110	-0.178	-0.300	-0.305	-0.800	-0.307
	(0.209)	(0.223)	(0.168)	(0.133)	(0.361)	(0.242)
Observations	38	38	278	278	41	41
Panel D: Effects on Total Assets at the Median (Quantile Regression)						
Intervention District	-0.344	-0.271	-0.341	-0.362	-1.197	-0.497
	(0.322)	(0.285)	(0.169)	(0.136)	(0.386)	(0.284)
Observations	38	38	278	278	41	41
Kolmogorov Smirnov p-value	.34		.03		.05	
(for stochastic dominance)						
Functionary Controls	No	Yes	No	Yes	No	Yes
District Controls	No	Yes	No	Yes	No	Yes

Note: The unit of observation is a yearly asset declaration by a MGNREGS official. Declarations 2011-12 were made from August 2011 to July 2012. Declarations 2012-13 were made from August 2012 to June 2013. Declarations 2013-14 were made from July 2013 to September 2014. The intervention period was September 2012 to April 2013. District level functionaries are Accountants, Assistants, Clerks, Computer Operators, District Development Coordinator, Engineers, Office Superintendants, Project Economists, Statistical Investigators and Technical Assistants. In Panels A and B, the dependent variable is the log of total movable assets (cash, jewellery, bank deposits, bonds, vehicles). In Panels C and D, the dependent variable is the log of all assets, including movable assets and immovable assets (e.g. land, buildings). In 2011-12 and 2013-14, the sample is smaller because only District Development Coordinators declared their personal wealth. Intervention District is a dummy set equal to one for districts in which the intervention was implemented. Functionary Controls include the age, the square of age, dummies for gender and functionary designation, and a dummy for whether the functionary is posted in the district she was born in. District controls include rural population (2011 census), MGNREGS wage expenditures and MGNREGS material expenditures (nrega.nic.in).

Table A.14—: Comparison between Border Blocks and Intervention Districts at Baseline

	Border Blocks	Intervention Districts	Difference Mean	Difference S.E.	Observations
	(1)	(2)	(3)	(4)	(5)
Panel A: Census 2011					
Area (hectares)	958.1	1004	46.00	23.08	4,208
Number of households	1997	1946	-50.65	28.75	4,208
% Scheduled Castes and Scheduled Tribes	0.175	0.191	0.0161	0.00293	4,208
Literacy Rate	0.609	0.626	0.0162	0.00254	4,208
Normalized Index	0.00	0.371	0.371	0.0655	4,208
Panel B: nrega.nic.in reports (April 2011- March 2012)					
MGNREGS beneficiary households	209	166	-42.79	4.894	4,241
MGNREGS work days provided	7834	5381	-2,453	236.4	4,241
MGNREGS labor expenditures (100,000 rupees)	8.95	7.05	-1.899	0.297	4,241
MGNREGS material expenditures (100,000 rupees)	6.69	6.74	0.0522	0.223	4,241
Normalized Index	0.00	-0.77	-0.771	0.0987	4,241
Panel C: CPSMS reports (Sept 2011- March 2012)					
MGNREGS funds spent (100,000 rupees)	9.65	8.53	-1.120	0.252	4,102
MGNREGS GP account balance (100,000 rupees)	3.81	3.57	-0.244	0.112	4,102
MGNREGS funds received (100,000 rupees)	10.12	8.86	-1.258	0.266	4,102
Normalized Index	0.00	-0.38	-0.375	0.0788	4,102

Note: The unit of observation is a Gram Panchayat (GP). Out of 4267 GPs from the border blocks and the intervention districts, we match 4208 GPs with census 2011 data (Panel A), 4241 GPs with nrega.nic.in data (Panel B) and 4102 GPs with CPSMS data (Panel C). Normalized Indexes are computed by substracting the control mean from each variable and dividing by the standard deviation in the control, and taking the sum across all variables in the panel. The difference between border blocks and intervention districts is estimated using a regression of each GP characteristic on a dummy equal to one for intervention districts and district fixed effects (modified so that border blocks were included in the intervention district they were next to). Standard errors are clustered to take into account correlation at the block level.

Table A.15—: Reform Impact on MGNREGS Spending in Control Blocks

	Set-up	Intervention Period			After
	July-August 2012	Sept-Dec 2012	Jan - Mar 2013	Whole Period	Apr 2013 - Jan 2014
	(1)	(2)	(3)	(4)	(5)
Panel A: Total Debit from GP Accounts					
Treatment	0.000168	-1.244	-1.406	-2.650	-0.826
	(0.249)	(0.388)	(0.290)	(0.598)	(0.844)
Intervention District	-0.306	0.0700	0.0895	0.160	0.541
	(0.246)	(0.385)	(0.348)	(0.630)	(0.971)
Observations	4,167	4,167	4,167	4,167	4,167
Mean in Border blocks	4.324	5.424	4.130	9.554	15.76
Panel B: Closing Balance in GP Accounts					
Treatment	0.215	-0.925	-1.176	-1.143	-0.0630
	(0.200)	(0.251)	(0.250)	(0.250)	(0.271)
Intervention District	0.142	0.701	0.618	0.594	1.232
	(0.189)	(0.218)	(0.247)	(0.249)	(0.218)
Observations	4,167	4,167	4,167	4,167	4,167
Mean in Border blocks	4.161	3.523	3.791	3.795	3.749
Panel C: Total Credit to GP Accounts					
Treatment	0.211	-2.467	-1.330	-3.797	0.371
	(0.285)	(0.435)	(0.377)	(0.618)	(0.836)
Intervention District	-0.215	0.665	-0.166	0.499	1.189
	(0.288)	(0.426)	(0.424)	(0.711)	(0.982)
Observations	4,167	4,167	4,167	4,167	4,167
Mean in Border blocks	4.429	4.853	4.148	9	15.61

Note: The unit of observation is a Gram Panchayat (GP). The sample includes treatment and control GP from the 12 districts of our study and GP in 85 neighboring blocks from other districts. Data was downloaded from the CPSMS portal in November 2014. The dependent variable in Panel A is the sum of debits from the savings account of each GP for each period (in 100,000 Rupees). In Panel B, it is the closing balance on the savings account of each GP at the end of each period (in 100,000 Rupees). In Panel C, the dependent variable is the sum of credits made to the savings account of each panchayat for each period (in 100,000 Rupees). Treatment is a dummy set equal to one for the blocks selected for the intervention. Intervention District is a dummy set equal to one for all blocks of districts where the intervention took place (whether control or treatment). Specifications include district fixed effects modified in order to include border blocks in the intervention districts they are closest to. Standard errors are clustered at the block level.

Table A.16—: Year of e-FMS Implementation for Wage and Material Payments by State

	Financial Year of EFMS Implementation							
	Wage Payments				Material Expenditures			
State	2012-13	2013-14	2014-15	2015-16	2012-13	2013-14	2014-15	2015-16
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ASSAM	0	23	0	0	0	1	22	0
BIHAR	0	1	26	9	0	0	17	19
CHHATTISGARH	1	15	0	0	0	13	3	0
GUJARAT	25	0	0	0	0	24	1	0
HARYANA	3	16	0	0	0	19	0	0
HIMACHAL PRADESH	0	10	0	0	0	1	9	0
JHARKHAND	1	17	0	0	0	10	8	0
KARNATAKA	24	3	0	0	3	24	0	0
KERALA	0	14	0	0	0	1	12	1
MADHYA PRADESH	20	25	0	0	1	44	0	0
MAHARASHTRA	5	28	0	0	0	33	0	0
ODISHA	30	0	0	0	0	30	0	0
PUNJAB	1	16	0	0	0	17	0	0
RAJASTHAN	23	9	0	0	0	32	0	0
TAMIL NADU	1	28	0	0	0	17	12	0
UTTAR PRADESH	1	69	0	0	0	64	6	0
UTTARAKHAND	1	0	2	9	0	0	3	9
WEST BENGAL	0	2	15	0	0	0	14	3
Total	136	276	43	18	4	330	107	32

Note: The table gives the number of districts that started to implement e-FMS in a given year in a given state. We define a district as implementing e-FMS for labor (resp. material) expenditures when a transaction was recorded that year in nrega.nic.in for labor (resp. material) expenditures.

Table A.17—: Effect of e-FMS Implementation on Wage and Material Expenditures including District-specific Trends

	District Expenditures (in 100,000 Rupees)			
	(1)	(2)	(3)	(4)
Panel A: Expenditures on labor from nrega.nic.in				
e-FMS for wage payments in year t	-579.3	-676.5	-642.9	-601.2
	(161.8)	(160.6)	(173.2)	(197.6)
	[0]	[0]	[0]	[0]
e-FMS for material payments in year t		352.5	228.7	235.6
		(183.3)	(199.2)	(198.2)
e-FMS for wage payments in t and t-1			283.8	302.2
			(217.1)	(224.6)
e-FMS for wages payments in t, t-1 and t-2				196.4
				(217.6)
Observations	4253	4253	4253	4253
Mean in Control	4140.4	4140.4	4140.4	4140.4
District-specific Time Trends	Yes	Yes	Yes	Yes
Panel B: Expenditures on material from nrega.nic.in				
e-FMS for wage payments in year t	-243.3	-181.4	-194.5	-192.9
	(80.20)	(79.16)	(84.30)	(89.63)
	[0]	[0]	[0]	[0]
e-FMS for material payments in year t		-224.4	-176.1	-175.9
		(75.00)	(81.15)	(81.29)
e-FMS for wage payments in t and t-1			-110.6	-109.9
			(84.36)	(86.78)
e-FMS for wages payments in t, t-1 and t-2				7.466
				(88.39)
Observations	4253	4253	4253	4253
Mean in Control	1703.5	1703.5	1703.5	1703.5
District-specific Time Trends	Yes	Yes	Yes	Yes

Note: The unit of observation is a district*year. The dependent variables are expenditures from MIS reports for financial years 2008 to 2016 (in 100,000 Rupees). The data was downloaded from nrega.nic.in in April 2017. "e-FMS for wage (material) payments in year t" is a dummy variable set equal to one if e-FMS is effective for wage (material) payments that year. "e-FMS for wage (resp. material) payments in year t and t-1" is a dummy variable equal to one if e-FMS was used for wage (resp. material) payments both this year and the year before. It is thus the additional effect of having the program for two years (compared to one). All specifications include district fixed effects, year fixed effects and district-specific time trends. Robust standard errors are in parentheses, and p-value from randomization inference (100 replications) in brackets.