

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

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Abstract

Can e-governance reforms that reduce advance funds available to local administrators enhance accountability and reduce the leakage of public funds? We investigate this question via a large field experiment and the nationwide scale-up of a financial reform to India's employment guarantee program. The reform replaced advance payments, authorized by intermediate administrative tiers, with direct invoicing and payments to local bodies. We find that corruption declined: program expenditures dropped by 24%, while an independent survey found no decline in actual program outcomes; matching the program database with a household census reveals a 5% reduction in ghost beneficiaries; and program officials' personal wealth fell by 10%. Consistent with the experimental findings, the nationwide scale-up resulted in a persistent 19% reduction in program expenditure. However, the ending of advance payments did come with a cost – increased payment delays.

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1 Introduction

Administrative monitoring processes - while essential for preventing rent-seeking – can, in and of themselves, engender opportunities for corruption. Public programs that are centrally funded but locally implemented provide a perfect example of this tradeoff. In the developing world, such programs span the range from school and health clinics to workfare and pension programs. The standard practice is for funds to be disbursed as advances, since fiscal prudence considerations limit borrowing by local bodies. The resulting time lag between when the money is disbursed and when it is accounted for creates opportunities for corruption. While monitoring can enable a system of checks and balances, it may also generate its own problems: administrators can hold up and extract rents from the local bodies they are meant to monitor.

In recent years, many governments have been attracted by the promise of digital financial platforms. By enabling just-in-time financing, such platforms can end the need for advance funds disbursement. This can enhance transparency and enable a leaner administrative structure. 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 et al., 2011).¹

Yet, the success of such programs in reducing agency problems within government structures and, thereby, reducing corruption 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.

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 we worked with the state government to randomize 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.

Under the status quo, advance fund requests by the local implementing body (the

¹Early reform examples include Mexico’s SIAFF (1997) and Italy’s SIPA (2001) (Barbatz, 2013).

Gram Panchayat, or GP) would require approval by officials at two intermediate levels of the hierarchy (block and district) before being sent to the state treasury. Expenditures were reconciled with funds released with a lag, and only when the paper-based worker records were entered in an electronic database. In contrast, the reformed system (in both our experimental and non-experimental studies) was based on just-in-time financing: fund release into the GP account was automatically triggered when GP officials entered worker details in the financial database and requested funds. No approvals from intermediate levels of hierarchy were required for fund release.

This reform enhanced transparency and enabled a more effective audit process. Specifically, the requirement that each fund request be accompanied by a list of beneficiaries eliminated the several-month lag between the fund transfer and when the auditor could check the names of those purportedly paid the wages. It also reduced the involvement of the intermediate administrative tiers in the fund release process.

Increased transparency *per se* should improve monitoring of the local officials and, thereby, lower corruption. However, reducing the monitoring powers of higher up officials could perversely increase corruption. Specifically, under the status quo system, the negative incentive effect of having multiple predators (as in Shleifer and Vishny (1993) and Olken and Barron (2009)) potentially weakens the incentive of local officials to extract rents, because more would have to be shared with higher level officials. Not being "taxed" by their superiors may cause local officials to steal more. Using a simple theoretical example, we show that the net corruption effect of the reform depends on how the local official trades off an increased threat of punishment (due to better monitoring) and gaining a bigger slice of the ill-gotten pie.

To empirically identify this net impact, 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 corruption. For the pan-Indian reform, we confirm that the long-run impacts on program expenditure are similar to those observed in the short-run.

We start with the experimental findings. First, administrative data on daily GP finances shows a 24% 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% increase in self-reported program participation (significant at the 10% level).²

Next, we turn to direct evidence on the reduction in corruption in our experimental sample. To identify "ghost worker" households, (i.e. households that are listed as having

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

worked in the program’s public database but are absent from the village population census), we match over six million names of reported beneficiaries from the program database with 34 million names from the 2012 Socio-Economic Census. While the matching is imperfect, the variation in matching rate across treatment and control areas provides a statistical test of the reform impact on corruption.³ We find a 5% reduction in the fraction of unmatched single-worker households in treatment GPs, relative to control GPs, suggesting that there were fewer invented households.

Our second direct measure of corruption in Bihar uses affidavit data on public employee assets to trace the “missing money”. At the end of the reform period, block and GP officials’ median wealth is 14% lower in treatment relative to control areas. Similarly, mean wealth is 10% lower, but this estimate is noisier. Taken at face value, the point estimate for the decline in mean wealth implies that reduced officials’ wealth accounts for half of the savings the reformed program achieved. While we lack experimental variation in treatment at the district level, wealth of district officials is significantly lower in the districts that entered the experiment (relative to other districts).

Turning to other dimensions of program performance, we observe a 30% decline in idle funds in treatment GP accounts. From a public accounting perspective, this represents an efficiency gain since disbursed funds constitute a government expense.⁴ But, from the villagers’ perspective the reform brought only a small increase in program participation and no increase in constructed assets. And while household consumption patterns were unaffected, payment delays increased.

The absence of local reform winners and the decline in rent-seeking opportunities led local Bihari officials to successfully lobby state officials to disband the experimental reform at the end of the fiscal year. However, the reform created a clear winner at the central level: the rural development ministry that was responsible for program funding. As we discussed earlier, the ministry had been encouraging states to undertake fund flow reforms since 2012. Aided by the results of the Bihar evaluation, a nationwide reform which linked worker payments to electronic invoicing by GP officials was implemented across India in 2015 (with Bihar readopting the reform in 2014).⁵ Its staggered rollout between 2012 and 2015 form the basis of our long-run evaluation. While we have more limited data on the impact of national rollout, there is significant value in asking whether the national reform led to a similar decline in program expenditure. Arguably, the permanent nature of the national reform gave officials stronger incentives to undermine the reform. However,

³This matching strategy implements at scale the audit approach pioneered by Niehaus and Sukhtankar (2013) and also used by Muralidharan et al. (2014), where investigators physically track down workers reported in the public database.

⁴Since the money is handled by public banks, only the expenses involved in handling this extra money is actually a cost to government. However, fewer idle balances also frees up money to be used elsewhere.

⁵The 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 et al. (2014) it did not require biometric identification.

we observe a magnitude of decline that is similar to what we found in Bihar: labor expenditures decline by 19% in the year the system was introduced and, importantly, the decline persists. These findings support the external validity of our experiment findings, and help us contribute 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., 2013; Allcott, 2015; Banerjee et al., 2016). To the best of our knowledge, very few papers are able to directly compare experimental estimates to credible non-experimental estimates of the same policy (Horton (2017) being a recent example).

Our paper also contributes to the forensic literature on measuring corruption. We extend the ambit of 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 et al., 2014), by cross-validating official records using another administrative data source. We also innovate on Fisman et al. (2014) methodology for using officials' affidavit data to examine wealth effects attributable to corruption by using these data in the context of a large scale experimental evaluation of an administrative reform.

Finally, our paper contributes to the empirical literature on corruption by demonstrating how e-governance reforms can reduce agency problems within the administration.⁶ A closely related study is Muralidharan et al. (2014); it evaluates a MGNREGS reform in which wage 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 incomes, increased, with no change in government outlay, indicating lower leakage of funds. This reform complements the intervention we study which changes the fund flow and leaves disbursement processes constant. A comparison of the two studies highlights the fact that reducing fund leakages in public programs, in itself, is unlikely to drive increased citizen demand for expansion of the (now better run) program. Rather, improved administrative transparency likely needs to be accompanied by citizen-facing transparency initiatives to ensure that program beneficiaries are informed and able to benefit from lower corruption.

⁶Empirical studies of anti-corruption reforms have typically focused on the effects of information disclosure, increased monitoring, and monetary incentives, while holding the administrative structure constant (Reinikka and Svensson, 2011; Olken, 2007; Ferraz and Finan, 2011). 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., 2014; Rasul and Rogger, 2016). 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 et al. (2013); Banerjee et al. (2016). Those focussing on the use of information technology include Barnwal (2014); Muralidharan et al. (2014); Lewis-Faupel et al. (2016).

2 Understanding the reform

After receiving MGNREGS funds from the centre, the state transfers fund to the GP accounts based on authorization.⁷ 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. Having provided these institutional details, we then develop a simple framework to clarify the reform’s anticipated impacts on fund leakage and citizen well-being.

2.1 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 (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, district officials did not follow these guidelines and fund requests continued to involve bargaining between the district, the block and the GP.

Accountability regarding who received payments was a lengthy process. The GP officials backed up the utilization certificate by the electronic entry of the “muster roll” (the 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

⁷To 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.

period, it took about six months for 60% 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 the amounts in the CPSMS data.⁸

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 saving account. Since most GP officials lacked necessary infrastructure and/or knowhow, 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.

2.2 Framework

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 rentseeking

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

⁸Worker data on public portal did eventually tally with CPSMS data (Appendix Table A.2).

work spell and when it’s 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% 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 (5% of audits found irregularities in the control group compared to 10% 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 corruption in the treatment group. Thus, increased detection of irregularities in treatment GPs must reflect a greater probability of being caught, conditional on cheating.

Second, the reform reduced the number of agencies involved in authorizing fund release. The net impact of a leaner administrative system, on program corruption is ambiguous. On one hand, fewer agencies reduces the number of potential rent-seekers (Shleifer and Vishny, 1993; Olken and Barron, 2009). However, on the other hand, the local GP official who is subject to rent-seeking by higher ups can also misappropriate MGNREGS program funds. Hence, a leaner administrative structure may encourage more rent-seeking at the GP level.

A simple theoretical example can help clarify the net effect of greater transparency and a leaner administration on funds leakage.

As discussed above, the administrative hierarchy associated with MGNREGS implementation has up to three tiers: P (GP), B (block), and D (district). Tier P implements the program ; assume that P can skim off amount s if she exerts a non-contractible non-pecuniary effort cost $\frac{1}{2}cs^2$. We capture the *transparency effect* of the reform by assuming that the penalty for skimming $\pi^r s$ varies across status quo and treatment regimes $r \in C, T$ such that $\pi^C < \pi^T$.

We now turn to the administrative structure associated with funds transfer: In the status quo regime, both B and D are required to sign off on a fund request before the bank processes the claim. In the treatment, P can, in principle, unilaterally claim the money from the bank. However, in reality P typically lacked the technological capacity to file the claim and continued to require B 's help in uploading the data. We, therefore, assume that the role of B is unchanged between treatment and control.

The role of D , however, changes in the treatment regime, since D has neither a signing off nor a data processing role in filing of a fund request by P . Given that D is supposed to monitor the program at the GP-level, cutting D out entirely is unrealistic (this is another reason why B continues to have a role). We, therefore, allow D to retain leverage over P and B , but assume that this leverage is reduced. We relegate to the appendix the case where D is entirely excluded which gives us similar but more extreme results.

How do B and D use their influence to extract rents? Following Shleifer and Vishny (1993), we assume that $i \in \{B, D\}$ can commit ex ante to a price p_i for approving *every rupee* of funds skimmed by P in the status quo regime. In the treatment regime, B retains the same ability but this ability is reduced for D such that with probability $\alpha < 1$, D has to be paid a price p^D (per rupee stolen). We also assume that there is a cap, \bar{p}^D , on how high p^D can go.⁹

In the status quo B and D choose p_B and p_D non-cooperatively to maximize earnings, and P chooses s to maximize

$$(1 - \pi^C)s - p_B s - p_D s - \frac{1}{2}cs^2,$$

In the treatment regime, P instead chooses s to maximize

$$(1 - \pi^T)s - p_B s - \alpha \bar{p}^D s - \frac{1}{2}cs^2,$$

In the treatment regime, the probability of detection goes up. At the same time, P can somewhat cut out the D official. Solving the model, we find that

Prediction 1 *The net impact of the fund transfer reform on corruption is ambiguous:*

1. *The earnings of district officials falls*
2. *The earnings of block and GP officials could either increase or decrease.*

Proof. In Appendix A. ■

⁹Without this additional (but realistic) assumption, the solution to the game is identical to the status quo, except that $\pi^N > \pi^T$. The reason is straightforward: D increases p^D exactly enough to cancel out the effect of $\alpha < 1$, and the problem is solved as before. But this relies on the arguably unrealistic ability of D to extract very large bribes. In particular, as $\alpha \rightarrow 0$, $p^D \rightarrow \infty$, which means that P will be, *ex post*, paying large amounts out of pocket to D whenever D is in a position to extract rents. It seems much more reasonable to cap the price D can charge.

This example is clearly stylized, but we should note that the linearity of the prices charged by B and D are not essential for our result. What matters is only that P is not a residual claimant and receives a higher share of the marginal rupee available for skimming when the bargaining power of D declines.¹⁰ A second feature to highlight is that the potential corruption of GP officials plays a key role in this model. A different view would be that GP officials “steal” money only to satisfy the rapacity of the district and block officials. If this is the case, cutting out the district officials and reducing the hold of the block officials would unambiguously reduce corruption.

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. 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 interest in providing employment provision. Program take-up could also have fallen in the initial months of the reform, while GP officials trust in a 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. If anything, switching away from an advance fund system could increase 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, this pressure was likely weak.¹¹ Thus, even if the program successfully reduced corruption the impacts of the reformed system could differ significantly across citizens who bear the tax burden of the program and villagers who seek work under the program. In our empirical analysis, we will provide evidence on this issue.

¹⁰An extensive theoretical literature shows that a sharing rule rather than making one party the residual claimant is optimal when outcomes are uncertain and the agent is risk averse or is credit constrained. However, linear contracts may be exactly or approximately optimal under some specific assumptions (Holmstrom and Milgrom, 1987; Chassang, 2013; Carroll, 2015).

¹¹Dutta 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.

3 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.

3.1 Bihar MGNREGS: 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% of households in Bihar who wanted MGNREGS work could not obtain it, and at most 10% of households in Bihar worked on MGNREGS worksites during the year.¹²

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 who report working in MGNREGS sites account for only 59% of households listed as having worked in that period in the official database.¹³ Next, in a survey of 346 GP heads (Mukhiyas), 47% 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% of program expenditures. About 72% of the Mukhiyas also identified a lack of funds as a constraint.

Such findings, and 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.

3.2 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, intervention period

¹²In the better performing state of Andhra Pradesh, only 27% of those who wanted work could not find it, and 39% of households participated in MGNREGS.

¹³Using 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%).

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.¹⁴ 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).

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.¹⁵ In control GPs, our match rate is 50% during the intervention period (67% for single-worker job cards, and 28% for job cards with more than one worker; the difference reflects the fact that it is harder for two names to match than one.) This is comparable to the

¹⁴The MGNREGS basic administrative unit is the GP, and so the database lacks a village census code. 84% of GPs in the database have a village match in the SECC census. For the 16% remaining unmatched GPs, we look for matches among households living in all villages in the GP.

¹⁵Individuals may be omitted from the SECC census 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.

59% match rate we obtain by comparing the public database to (population) estimates of MGNREGS workers from our household survey. While this exercise identifies non-existent workers, it neither identifies genuine households who falsely report working nor does it identify over-reporting of days worked by households that did work.

Next, we 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 declare 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.¹⁶ We classify personal assets into movable (cash, savings and other financial assets, jewellery, 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 sampled two GPs per block, and 25 households per GP, oversampling poorer households, who were more likely to participate in the MGNREGS (see Appendix section A for details). Survey respondents were asked to recall weekly MGNREGS participation and the amount, date and payments 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). We anticipate that answers to payment and delays may be less reliable. Since MGNREGS participation was low during our study period, the survey (despite its large sample size) only identified a small number of participants.

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.

3.3 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 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

¹⁶Fisman et al. (2014) use politician affidavit data and show a 3% to 4% higher estimated annual growth rate of wealth for winners than for runner-ups in close elections. Fisman et al. (2016) further show that the requirement to disclose discourages many politicians from running for office.

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 freezing fund release to Bihar in September. The state government sustained limited MGNREGS spending by using own-state resources to partially replenish the state pool. Hence, both treatment and control GPs continued to be credited with 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% in December 2012 across all districts to 60% in April 2013 (performance varied across districts).¹⁷ Imperfect implementation of an at-scale reform is reasonably common (Muralidharan et al., 2014; Banerjee et al., 2016).¹⁸ While we focus on intent-to-treat analysis, Appendix Table A.10 reports very similar findings for treatment-on-the-treated analysis.

3.4 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:

$$X_{pd} = \alpha + \beta T_p + \eta_d + \varepsilon_p \quad (1)$$

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 GP p is in a treatment block, η_d are district fixed effects, and errors ε_p are clustered at the block level. The estimated coefficient β represent pre-treatment

¹⁷Treatment GPs that did not use CPSMS could still pay wages by depleting their savings accounts: only 1.5% of treatment GPs did not spend any money during the intervention.

¹⁸The 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.

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.¹⁹ Finally, CPSMS reports show that treatment and control GP received and spent similar amounts before the reform. The one exception is that, in the public information database (panel C), treatment blocks report more beneficiary households prior to reform (and correspondingly, more workdays provided and more labor expenditures). Overall, these differences are small (they represent a 5% difference in number of beneficiaries and 13% 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.

4 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.

4.1 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 post-intervention period (April 2013-January 2014). We also report regressions for two intervention subperiods: 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. Pre-reform spending trends show significant seasonality, but the patterns are similar across treatment and control GPs. In contrast, during the reform, expenditures in treatment GPs is

¹⁹We 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.

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% 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% lower in treatment GPs. Across the whole intervention period (from September 2012 to March 2013), spending was 24% 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% below that in control GPs.

Panel C in Table 2 shows that the combination of a 24% decline in spending and a 30% decline in idle funds reduced program expenditure by 38% in treatment GPs,²⁰ implying a cost saving of roughly 6 million dollars.²¹ The expenditures were not just postponed: in the six months following the intervention, the difference between treatment and control group returns 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) that then 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.²² 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).

Labor and material expenditures were respectively 16% and 14% lower in treatment GPs during fiscal year 2012-13. Accounting for the fact that the fiscal year includes

²⁰We 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)

²¹To obtain this figure, we multiply the expenditure reduction per GP by number of treatment GPs, and convert the total of 3.44 lakhs Rupees $\times 1003 = 3,410$ lakhs Rupees into million dollars (using the April 1, 2013 USD/INR exchange rate of 0.0183).

²²Appendix Table A.2 shows that discrepancies across annual expenditures per GP in CPSMS and nrega.nic.in are only about 11-14% in 2012-13.

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, as reflecting the legal requirement that MGNREGS material expenditure cannot exceed 40% of total project spending. For the average GP, this requirement was close to binding: expenditures on material amounted to 37% and 36% of total expenditure in the financial year 2012-13 and 2013-14, respectively.

4.2 Did the reform impact beneficiary outcomes?

4.2.1 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% over the reform period (significant at the 10% 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% reduction in the number of individuals who have supposedly worked.²³ This is consistent with changed reporting practices: the reform makes it riskier to create ghost workers (since an audit conducted with less of a timelag would more likely identify such fraud) relative to creating 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%); 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.

Finally, treatment blocks experienced a 38% 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.9 shows that twice as many Mukhiyas reported that the CPSMS had created

²³Inclusion of the pre-intervention MGNREGS implementation index as a control increases the estimated treatment effects on workdays and number of workers to 17% and 13% respectively (Table A.4).

delays in fund flow in treatment blocks compared to control blocks (35% as against 17%).

4.2.2 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 report less work and whether fewer assets are 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 a GP to compute GP-level population averages, using sampling weights to account for over-sampling of poorer households. Let Y_{pdt} denote outcome for GP p in district d at period t . We estimate:

$$Y_{pdt} = \alpha + \beta T_p + \delta Z_p + \eta_d + \varepsilon_{ht} \quad (2)$$

as before T_p is the treatment block dummy. Z_p denotes a vector of average household characteristics in the GP.²⁴ Standard errors are clustered at the block level.

Using survey data, we construct three employment measures: first, a binary indicator of MGNREGS participation; second, the number of weeks in which households declares 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%), are consistent with National Sample Survey data, which reports a participation rate of 9% for 2011-2012. When we consider survey responses for all work spells between July 2012 and 2013, we find a participation rate of 8%.²⁵ Lower work rate during our reform period likely reflects the fact that it fell outside the peak MGNREGS work season.

Column (1) shows a negative, small in absolute value but proportionally large, 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

²⁴These 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

²⁵In response to a separate question, 9% 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.7)

for the reform. Columns (2)-(4) show an *increase* in reported participation in treatment GPs during the intervention period, significant at the 10% level (0.76 percentage point, which is 25% of the control mean). The 95% confidence interval, expressed in fraction of the control mean is [-4%; +54%]. 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%, 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, positive point estimates for the two intervention sub-periods, and a positive point estimate for the whole period (significant at the 10% level). Again, we can reject a reduction of 5% at the 95% confidence level, a much smaller decline than the 13% 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.²⁶ Consistent with a lower probability of working, MGNREGS wage payments are significantly lower in the treatment GPs during the set-up period. During the intervention, the point estimate is positive (13), and the 95% confidence interval, expressed in percentages, is [-31%; +64%]. Here, we cannot reject the hypothesis that wages declined by as much as reported wages in the administrative data (14% Panel D of Table 4) at the 5% level, although we also cannot reject large increases: the data is simply too noisy to be informative.²⁷

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 55 days for their payment. The adverse treatment effect persists in the second phase (January to March 2013) but is smaller (32 days).

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

²⁶A pending payment is set as zero. Replacing it by missing makes the treatment look slightly more positive, since delays increased in the treatment group.

²⁷Given 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.

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. On a related note, Panel E of Table 5 shows that treatment did not influence the reported incidence of high-interest worker loans that GP officials may have provided to cover the period between a work-spell and payment receipt.²⁸ Finally, in Appendix Table A.8, we show that the reform left household consumption levels unaffected.

In Appendix Table A.6, 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% 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 consumption gains for treated households, no more than a small gain in employment, and a clear loss in terms of payment delays. Thus, it appears reform benefits largely consisted of lower funds leakage. Below, we provide further evidence on this channel.

4.3 Did the reform impact fund leakage and corruption?

Administrative data shows a 24% decline in MGNREGS spending in the treatment GPs, relative to control, and a corresponding 10-13% 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% 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 – are unaffected by treatment.

We interpret this as *prima facie* evidence of a reform-induced reduction in corruption. Consistent with this interpretation, while 47% of surveyed GP leaders in control GPs identified corruption in the administration as a major constraint in MGNREGS implementation, 12% percentage points fewer reported the same in treatment GPs (see

²⁸Anecdotal evidence suggests that interest on these loans is collected in advance by paying the workers less than what they are due on work completion. The lender also keeps the worker's bank/postal passbook, and uses pre-signed withdrawal slips to take MGNREGS wage directly from their bank/postal account (usually in connivance with bank/postal employees).

Table A.9, Panel D). Against this background, we present additional direct evidence on the reform’s impact on corruption.

4.3.1 Effect on ghost workers

In Section 2 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 – points to a reduction in ghost workers as the primary accountability impact associated with the reform. In contrast, conditional on having worked, accurate and verifiable information on number of days worked continues to be based on villager recall and so remains as 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 further evidence on this channel, we match households listed on MGN-REGS 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% and 51% of all job cards, respectively).

We start by reporting the correlation between match rate and reported employment at baseline. We estimate:

$$Y_p = \alpha + \beta M_p + \eta_d + \varepsilon_p \quad (3)$$

where M_p is the match rate for job cards with one (or two or more) names and Y_p are different pre-reform measures of reported employment calculated over the time-period April 2011 and June 2012. Columns (1) to (3) of Table 6 reports the results. 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.

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

Table 7 reports the results. For single-worker households, we match 64% 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% 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% 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% in treatment GPs.

A remaining question is: why did officials in treatment GPs not respond by increased over-reporting on other margins? We conjecture that there may be some limit on over-reporting of workdays in the name of existing workers caused, for instance, by the fixed number of total infrastructure projects. Muralidharan et al. (2014) also do not find a compensating increase in ghost workers when ghost days decline.

4.3.2 Effect on assets of block and GP officials

Next, we use self-reported affidavit data on block and GP officials’ assets to examine reform impacts on personal aggrandizement. As discussed in Section 3.2 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). 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 3 and 4 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 treat-

ment and control groups at the 5% 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:

$$Y_{ibt} = \alpha + \beta T_{bt} + \eta_d + \varepsilon_{ibt} \quad (4)$$

where T_{bt} is a treatment dummy, Y_{ibt} 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 8 show, on average, a reasonably large (12%) but statistically insignificant reduction in movable assets reported by block and GP officials in 2012-2013. Returning to figure 3, 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% decline in median movable assets (8.8% 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% lower in treatment areas and median assets were 14% to 19% lower. The mean estimate for decline in total assets in 2013-14 implies a loss of 308 million Rupees, or 44% 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 (4) replacing the treatment dummy by the match rate at baseline. The results in columns (4) and (5) of Table 6 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).

4.3.3 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 using 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.11 show a 40% reduction in the average movable assets reported by district officials in treatment districts in 2012-13. The median effect is larger (53%), 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% of the reduction in MGNREGS expenditures. The estimated losses of MGNREGS officials at the three levels of administration (GP, block and district) account for 83% 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). We use these border blocks as an additional comparison group, and estimate:

$$Y_{pdt} = \alpha + \beta T_p + \gamma S_p + \eta_d + \varepsilon_{pt} \quad (5)$$

which is similar to our main equation (1) with two changes. First, it includes a dummy S_p which is set equal to one for blocks included in the experimental sample (whether control or treatment). Second, 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 the control blocks. The results, presented in Appendix Table A.13, show that there is less spending (and less money received) in the pre-intervention period in sample blocks than in neighboring blocks outside of the sample, which translates into higher account balances in the intervention and post-intervention periods. However, there is no difference in spending during the intervention period between control blocks and neighboring blocks from other districts (Panel A). These results suggest that district officials were unable to compensate for losses in treatment blocks by inflating expenses in control blocks.³⁰

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 corruption earnings fall actively lobbied against the program. Given this lobbying, and the decline in program spending, 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 fund flow reforms were an anti-corruption initiative, allowing them to scale up the reform nationwide.

We might expect that incentives to undermine the reform would be stronger in the

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

³⁰Controlling for pre-intervention levels does not change the results (estimates not reported here).

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, did the expenditure declines persist in the longer-run or did officials identify ways of subverting the reformed system such that spending went back up?

5 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 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:

$$Y_{djt} = \alpha + \beta EFMS_{djt} + \eta_d + \mu_t + \varepsilon_{djt}, \quad (6)$$

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

³¹The Appendix Table A.12 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. 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).

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:

$$Y_{djt} = \alpha + \beta EFMS_{djt} + \gamma EFMS_{djt-1} + \eta_d + \mu_t + \varepsilon_{djt} \quad (7)$$

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 9 presents the results. We first regress labor expenditures on EFMS implementation for wage payments (Column 1). e-FMS decreased expenditures by 19%, and this effect is significant at the 1% 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% 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%, which is reminiscent of our experimental finding in Table 3. Since the rule that material expenditures cannot exceed 40% 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%. Although the year-by-year estimates are not significant, the event study figures (Figures 5 and 6) summarize the patterns: There were no differential trends before e-FMS implementation. Expenditures fall in the year of implementation (year zero) and then remain low in subsequent years.

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%. 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.

6 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’s impact on corruption is ambiguous – it enhanced transparency but also reduced the monitoring faced by local officials. In addition, as increased transparency only operated within the administration, the reform did not improve citizens’ ability to hold officials accountable for the use of program funds.

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, a result that 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 initial 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 to 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 chose to scale it up over the next few years as part of an anti-corruption agenda. This both demonstrates how a reform that creates losers can be sustained if those who benefit also have the power to determine program design and provides us an opportunity to evaluate the at-scale counterpart of the Bihar reform. 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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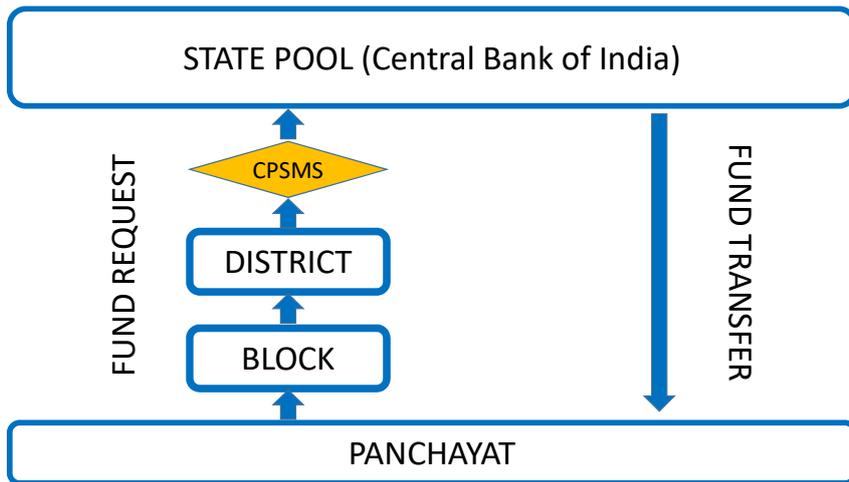
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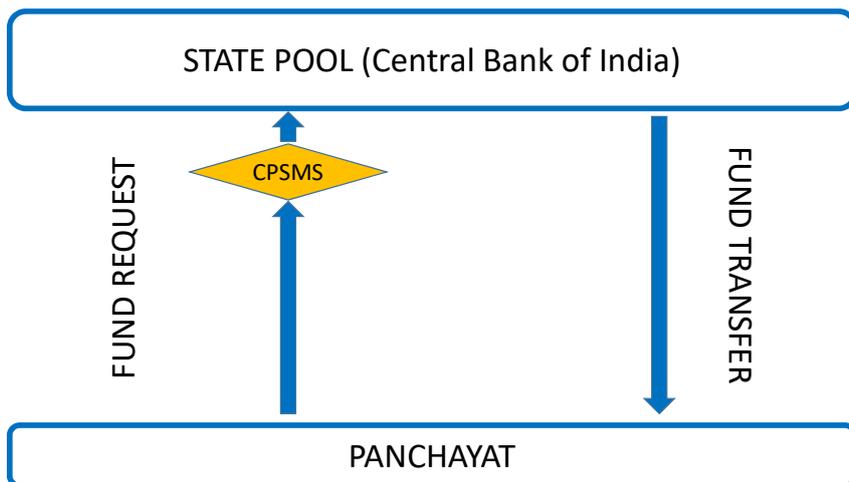
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Figure 1: MGNREGS Fund Flow in Control and Treatment Blocks

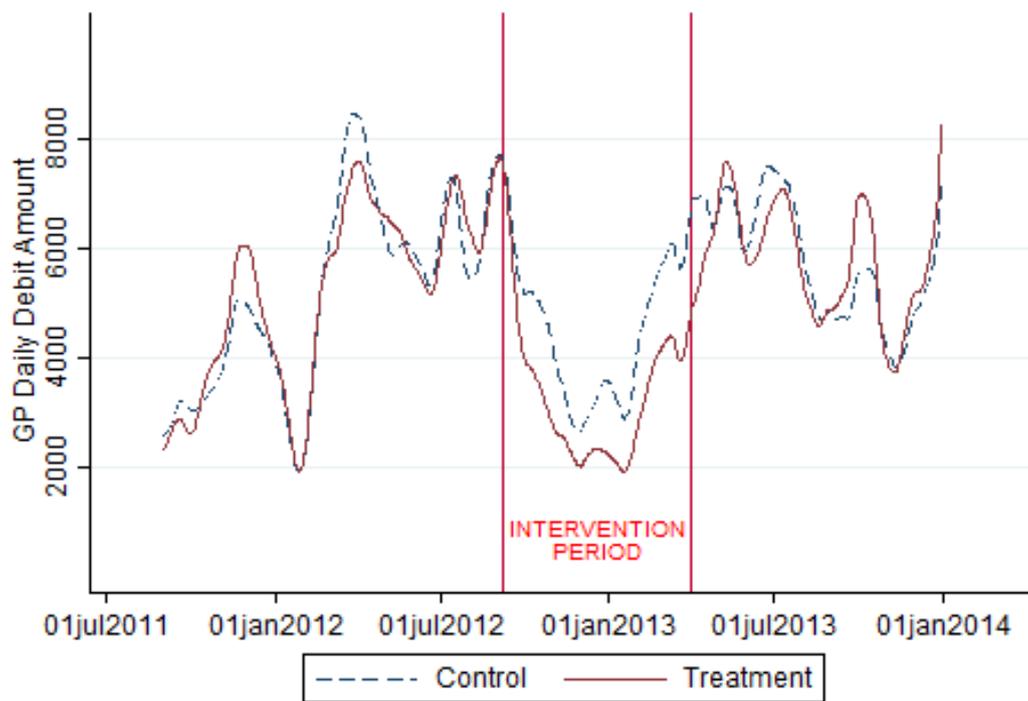


(a) Control Blocks



(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 the Intervention

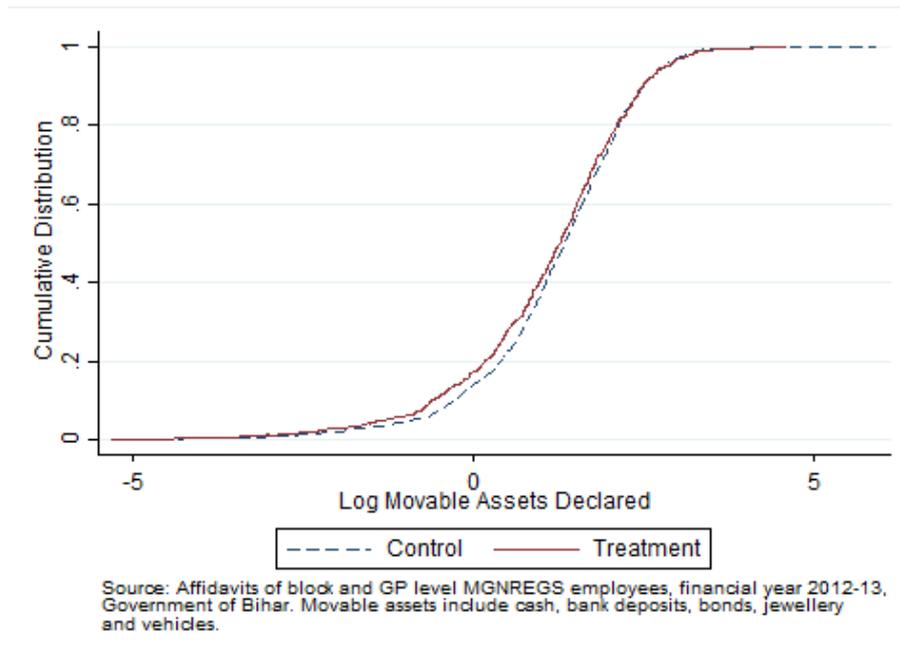


Figure 4: Movable Assets of GP and Block officials: After the Intervention

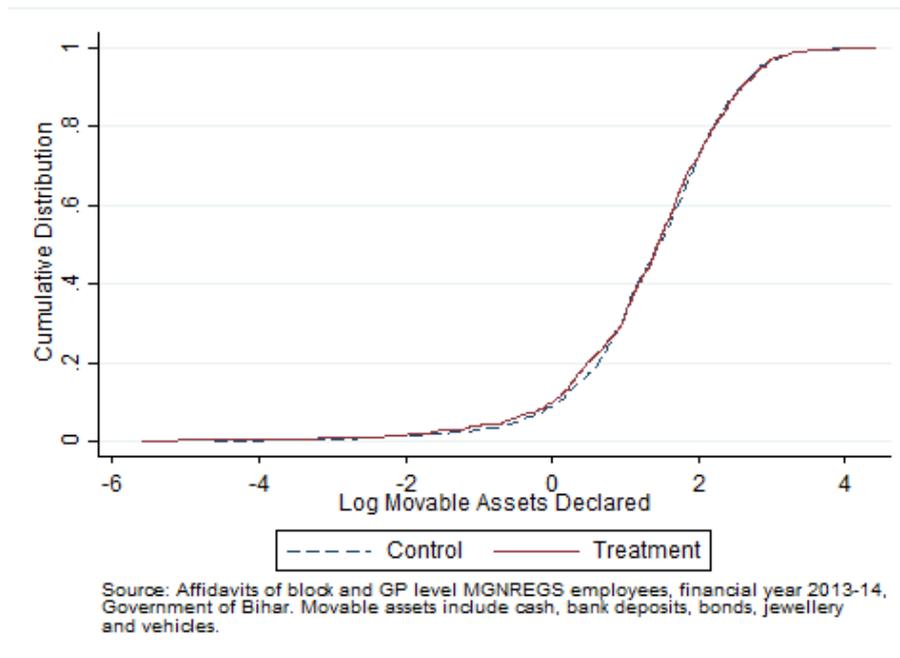


Figure 5: Effect of e-FMS Implementation on Labor Expenditures

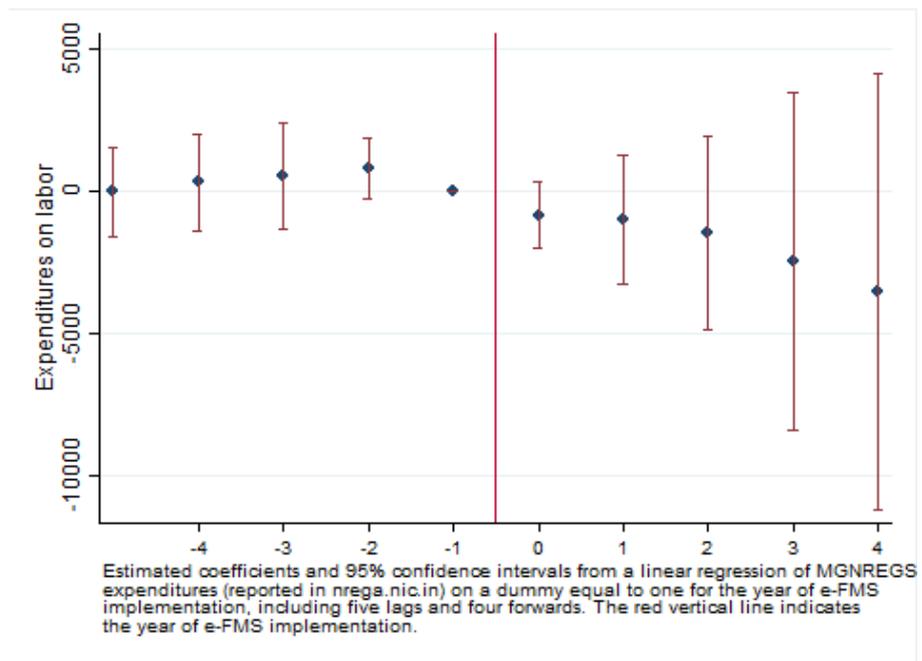


Figure 6: Effect of e-FMS Implementation on Material Expenditures

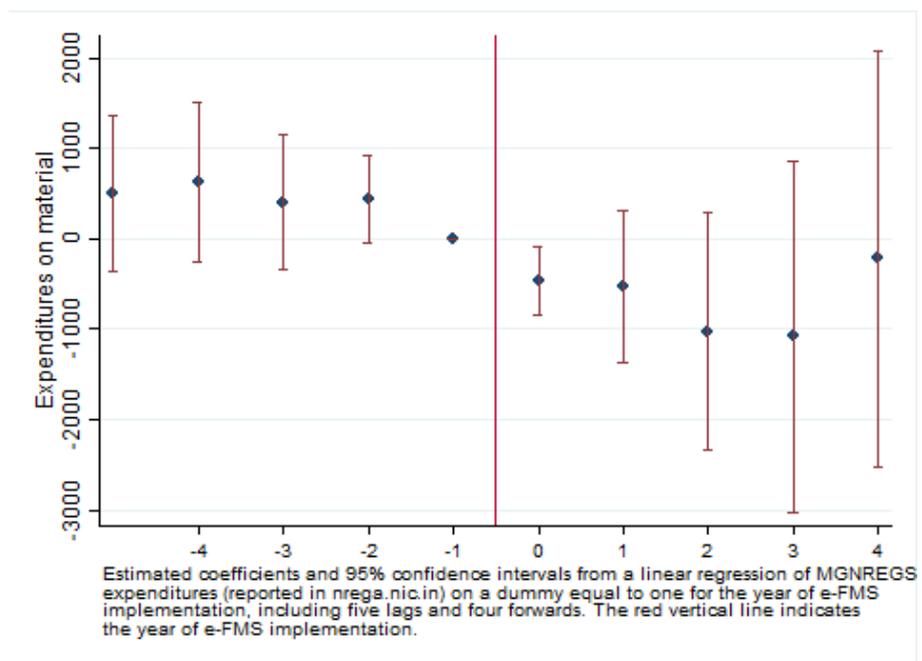


Table 1: Randomization Check

	Control Blocks	Treatment Blocks	Difference mean	Difference s.e.	Observations
	(1)	(2)	(3)	(4)	(5)
Panel A: Census 2011					
Area (hectares)	1095	1124	29.02	80.05	2,937
Number of households	1868	1853	-15.44	28.49	2,937
% Scheduled Castes and Scheduled Tribes	0.206	0.208	0.00151	0.00713	2,937
Literacy Rate	0.64	0.639	-0.000821	0.00563	2,937
Normalized Index	0.00	0.0153	0.0153	0.125	2,937
Panel B: Household Survey					
% Scheduled Castes and Scheduled Tribes	0.27	0.25	-0.0245**	0.0122	390
% Other Backward Castes	0.59	0.61	0.0210	0.0179	390
% House without a solid roof	0.38	0.41	0.0242	0.0198	390
% Owns Land	0.58	0.57	-0.00979	0.0185	390
% Male Head	0.78	0.77	-0.0128	0.0149	390
% Literate Head	0.56	0.55	-0.0124	0.0154	390
Household Size	6.15	6.03	-0.117	0.0716	390
Number of adults in the household	3.41	3.35	-0.0595	0.0666	390
Normalized Index	0.00	-0.36	-0.360	0.310	390
Panel C: nrega.nic.in reports (April 2011- March 2012)					
MGNREGS beneficiary households	184	193	9.297	8.303	2,968
MGNREGS work days provided	6155	6533	378.2	332.2	2,968
MGNREGS labor expenditures (100,000 rupees)	7.53	8.51	0.985**	0.494	2,968
MGNREGS material expenditures (100,000 rupees)	6.50	7.01	0.510	0.429	2,968
Normalized Index	0.00	0.36	0.362*	0.200	2,968
Panel D: CPSMS reports (Sept 2011- March 2012)					
MGNREGS funds spent (CPSMS)	9.12	8.95	-0.176	0.473	2,942
MGNREGS funds received (CPSMS)	9.66	9.83	0.165	0.559	2,942
Normalized Index	0.00	0.00	-0.00403	0.128	2,942

Note: The unit of observation is a Gram Panchayat (GP). Out of 3067 GPs from our sample list, we match 2937 GPs with census 2011 data (Panel A), we surveyed 390 GPs (Panel B), we match 2968 GPs with nrega.nic.in data (Panel C) and 2942 GPs with CPSMS data (Panel D). Normalized Indexes are computed by subtracting to each variable the control mean 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

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	-0.502 (0.729)	0.0472 (0.291)	-1.039*** (0.315)	-1.267*** (0.280)	-2.306*** (0.530)	-0.323 (0.832)
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: Closing balance in GP accounts						
Treatment	-0.0927 (0.245)	0.133 (0.217)	-0.950*** (0.232)	-1.299*** (0.243)	-1.266*** (0.240)	-0.147 (0.239)
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: Total credit to GP accounts						
Treatment	-0.179 (0.830)	0.251 (0.338)	-2.192*** (0.367)	-1.249*** (0.335)	-3.441*** (0.548)	0.919 (0.819)
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 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

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** (0.494)	-2.246*** (0.758)	-0.218 (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 (0.429)	-1.078** (0.529)	0.330 (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). 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

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 (474.8)	-130.3 (111.5)	-404.6* (227.6)	-267.8 (163.3)	-672.4* (363.6)	-859.5 (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 (1.018)	-0.712 (0.605)	-0.286 (0.805)	0.187 (0.701)	-0.00410 (0.930)	-0.308 (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 (11.85)	-3.132 (5.151)	-10.02 (6.233)	-8.342 (5.700)	-13.60* (8.150)	-15.03 (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 (0.726)	-0.189 (0.178)	-0.648* (0.353)	-0.410 (0.255)	-1.058* (0.570)	-1.529* (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 (6.186)	12.05** (5.798)	24.30*** (4.490)	16.71*** (2.860)	20.36*** (3.813)	9.332*** (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: 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 it 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table 5: Reform Impact on MGNREGS Employment: Evidence from Household Survey

	Set up		Intervention Period		Post-Intervention
	Jul - Aug 2012 (1)	Sept - Dec 2012 (2)	Jan - Mar 2013 (3)	Whole Period (4)	Apr - Jun 2013 (5)
Panel A: MGNREGS Participation					
Treatment	-0.00753*** (0.00279)	0.00479 (0.00342)	0.00274 (0.00314)	0.00760* (0.00449)	0.00325 (0.00495)
Observations	390	390	390	390	390
Mean in Control	0.0114	0.0136	0.0177	0.0302	0.0326
Panel B: Number of days worked					
Treatment	-0.145** (0.0573)	0.108 (0.121)	0.247 (0.151)	0.355* (0.205)	0.454 (0.545)
Observations	390	390	390	390	390
Mean in Control	0.209	0.398	0.549	0.947	1.789
Panel C: Wages received for MGNREGS employment					
Treatment	-16.06** (6.892)	2.677 (12.83)	10.22 (12.69)	12.90 (19.29)	-4.990 (32.45)
Observations	390	390	390	390	390
Mean in Control	22.17	37.55	42.07	79.62	99.83
Panel D: Average delays in payment (days)					
Treatment	-44.32 (27.67)	54.73*** (19.93)	32.27*** (10.70)	44.79*** (11.66)	4.384 (7.657)
Observations	91	111	148	200	205
Mean in Control	78.13	69.73	49.45	58.38	35.82
Panel E: Illegal advance payments					
Treatment	-0.0775 (0.129)	-0.115 (0.0841)	-0.0279 (0.0835)	-0.0778 (0.0631)	0.0358 (0.0719)
Observations	78	97	128	182	164
Mean in Control	0.382	0.249	0.299	0.286	0.374

Note: The unit of observation is a Gram Panchayat (GP). 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 dependent variable in Panel E is the fraction of households who declared having received a payment for MGNREGS work in cash within 15 days of the work spell. The data were collected by a representative survey of 9,670 households in 390 GP in May-July 2013. In Panel D and E the sample only includes GPs 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. Standard errors are clustered at the block level. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table 6: 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* (0.0999)	-0.241*** (0.0840)	0.0475 (0.0499)	-1.818*** (0.455)	-0.805 (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 (0.104)	-0.0333 (0.0880)	0.00239 (0.0531)	-0.859* (0.447)	0.575 (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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table 7: 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** (0.00742)	0.0195** (0.00769)	0.0104 (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** (0.00613)	0.0119 (0.00810)	0.00953 (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. They covers the period from July 2011 to March 2014. 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table 8: 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 (0.0968)	-0.119 (0.0972)	-0.0345 (0.0753)	-0.0321 (0.0741)	-0.101* (0.053)	-0.088* (0.046)	-0.073 (0.062)	-0.057 (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 (0.130)	-0.0659 (0.128)	-0.102 (0.103)	-0.115 (0.102)	-0.117 (0.073)	0.005 (0.068)	-0.137* (0.074)	-0.193*** (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: 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table 9: Effect of e-FMS Implementation on Wage and Material Expenditures

	Log district expenditures			
	(1)	(2)	(3)	(4)
Panel A: Expenditures on labor from nrega.nic.in				
e-FMS for wage payments in year t	-770.4*** (179.6) [0]	-705.4*** (170.8) [0]	-703.4*** (179.3) [0]	-790.5*** (197.4) [0]
e-FMS for material payments in year t		-223.8 (185.9)	-234.0 (203.5)	-251.8 (204.2)
e-FMS for wage payments in t and t-1			22.16 (203.5)	57.14 (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	-331.3*** (89.87) [0]	-232.1*** (85.43) [0]	-247.1*** (89.67) [0]	-266.0*** (94.48) [0]
e-FMS for material payments in year t		-341.1*** (80.73)	-264.2*** (85.20)	-268.1*** (85.32)
e-FMS for wage payments in t and t-1			-166.4* (86.08)	-158.8* (84.20)
e-FMS for wages payments in t, t-1 and t-2				-125.9 (93.25)
Observations	4253	4253	4253	4253
Mean in Control	1703.5	1703.5	1703.5	1703.5

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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

APPENDIX: FOR ONLINE PUBLICATION ONLY

A Proof

B and D choose p_B and p_D non-cooperatively to maximize earnings, and P chooses s to maximize

$$(1 - \pi^T)s - p_B s - p_D s - \frac{1}{2}cs^2,$$

which implies $s = \frac{1 - \pi^T - p_B - p_D}{c}$. Earnings of $i \in \{B, D\}$ in status quo regime are, therefore,

$$p_i \frac{(1 - \pi^T - p_i - p_{-i})}{c}.$$

$p_i = \frac{(1 - \pi^T - p_{-i})}{2}$ maximizes this expression. From the symmetry of the solution this yields

$$p_i = \frac{(1 - \pi^T)}{3}$$

and, therefore, the amount skimmed under the status quo is

$$s = \frac{(1 - \pi^T)}{3c}$$

Under the status quo, B and D therefore earn

$$Y^{BT}(\pi^T) = Y^{DT}(\pi^T) = \frac{(1 - \pi^T)^2}{9c}$$

while P earnings from skimming (which we observe) is

$$Y^{PT}(\pi^T) = \frac{(1 - \pi^T)(1 + 2\pi^T)}{9c}$$

Note that to compute P's utility we would need to deduct the expected penalties and the cost of her effort from this expression.

Two things change in the treatment regime: First π^T goes up to π^N . And second, P can, in principle, unilaterally claim the money. For α small enough that \bar{p}^D binds, B maximizes

$$p_B \frac{1 - \pi^N - p_B - \alpha \bar{p}_D}{c}.$$

$$\text{The } p^B \text{ chosen will be } p_B = \frac{(1 - \pi^N - \alpha \bar{p}_D)}{2},$$

Hence

$$s = \frac{(1 - \pi^N - \alpha \bar{p}_D)}{2c}$$

which implies that

$$Y^{BN}(\pi^N, \alpha) = \frac{(1 - \pi^N - \alpha\bar{p}_D)^2}{4c} \text{ and } Y^{PN}(\pi^N, \alpha) = \frac{(1 - \alpha\bar{p}_D)^2 - (\pi^N)^2}{4c}.$$

Clearly for $\pi^N < 1 - \alpha\bar{p}_D$ (which is the only case that makes sense), an increase in π^N reduces s , Y^{BN} and Y^{PN} , while a fall in α increases all three. As a result the reform's net impact on corruption is potentially ambiguous. Finally:

$$\frac{Y^{BN}(\pi^N, \alpha)}{Y^{PN}(\pi^N, \alpha)} = \frac{1 - \alpha\bar{p}_D + \pi^N}{1 - \alpha\bar{p}_D - \pi^N}$$

This ratio goes up when π^N goes up and down when α goes down. In summary, the net effect of changing both, as occurs with the reform we study, is ambiguous: the loss may be greater for P or B.

Finally, we discuss the case under treatment regime where the highest level in hierarchy is fully excluded, such that $p_D = 0$. From above it should be evident that

$$p_B = \frac{(1 - \pi^N)}{2}, \text{ and } s = \frac{(1 - \pi^N)}{2c}$$

which together imply that

$$Y^{BN}(\pi^N) = \frac{(1 - \pi^N)^2}{4c} \text{ while } Y^{PN}(\pi^N) = \frac{(1 - \pi^N)(1 + \pi^N)}{4c}.$$

A comparison of skimmed funds under the two schemes, $Y^{PT}(\pi^T)$ versus $Y^{PN}(\pi^N)$ or $Y^{BT}(\pi^T)$ versus $Y^{BN}(\pi^N)$, shows two countervailing effects: the negative effect of an increase from π^T to π^N and the positive effect of not having to pay D, reflected in the fall in the denominator. The net effect is ambiguous; for the negative effect to dominate, $1 - \pi^T$ needs to be reasonably close to zero or the increase in π^T to π^N must be very large in proportional terms. Otherwise, by reducing the number of officials involved, the reform increases corruption.

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.

Administrative data on MGNREGS implementation

We use two sources of official reports on MGNREGS expenditures and employment: **CPSMS**: 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 January 2013 to March 2013 and from the end of the intervention in April 2013 until July 2014.

NREGA.NIC.IN: The government website nrega.nic.in provides publicly available information on MGNREGS expenditures per GP for every financial year (a financial year 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.

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. These households were sampled from the list of households obtained from the District Rural Development Authority (DRDA). These lists were initially compiled in 2002 for the purpose of identifying Below Poverty Line households, so each household was given a poverty score, based on various criteria. From these lists, we sampled 72% of households below the median poverty score and 28% 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%.

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.

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% of the job cards villages (containing 88% of households). We match the other 16% of the job card villages (12% of households), to all SECC villages which are matched with job card villages belonging to the same GP. For about 0.5% of villages (0.7% 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% of households, of which 25% are matched) than for households with one working member (63% of households, of which 64% 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.³²

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 date of each audit, the name of the block and GP, the number of MGNREGS projects audited and the number of 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 are also available, but were not collected in a systematic manner.

³²The 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.

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

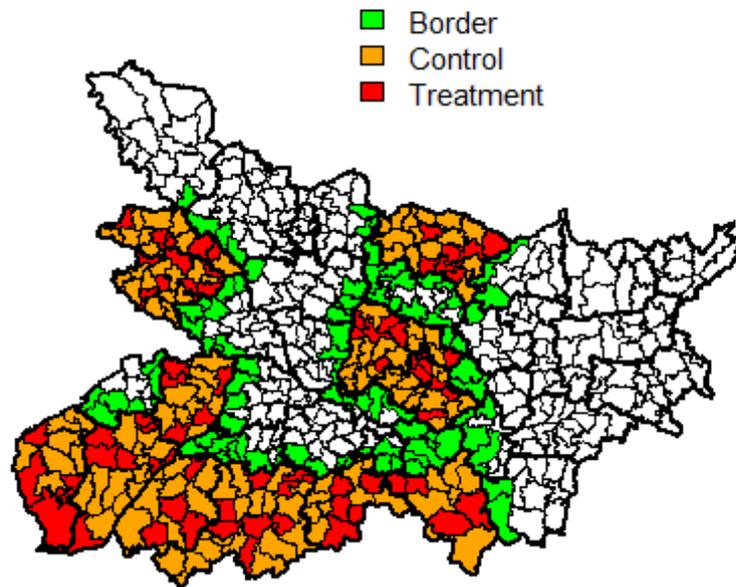


Figure A.2: Total MGNREGS Expenditures (2006-2016)

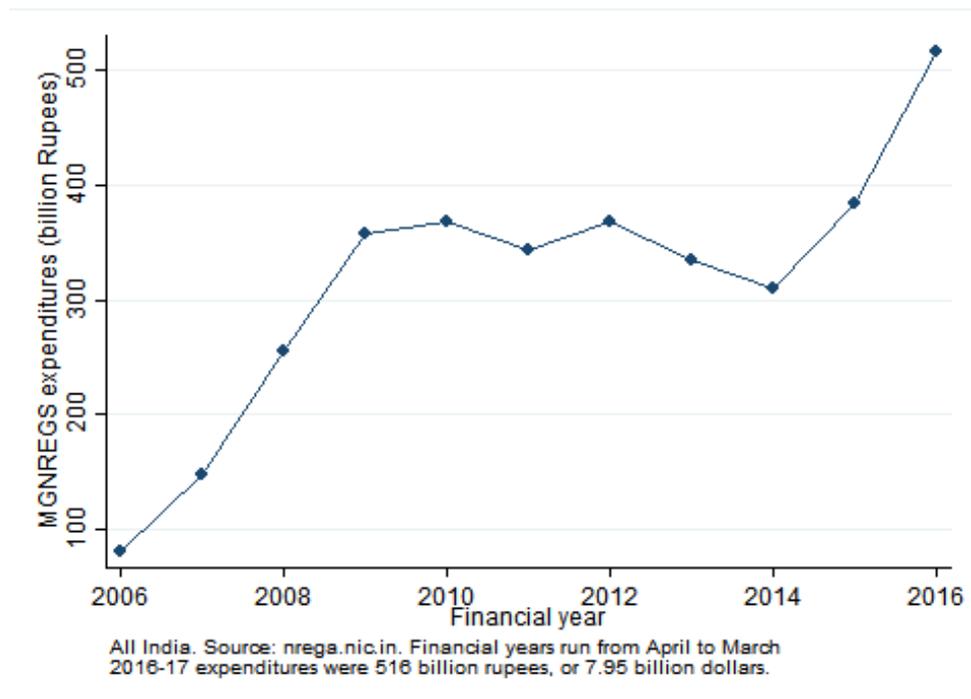


Figure A.3: Fraction of Treatment GPs that used CPSMS at least once

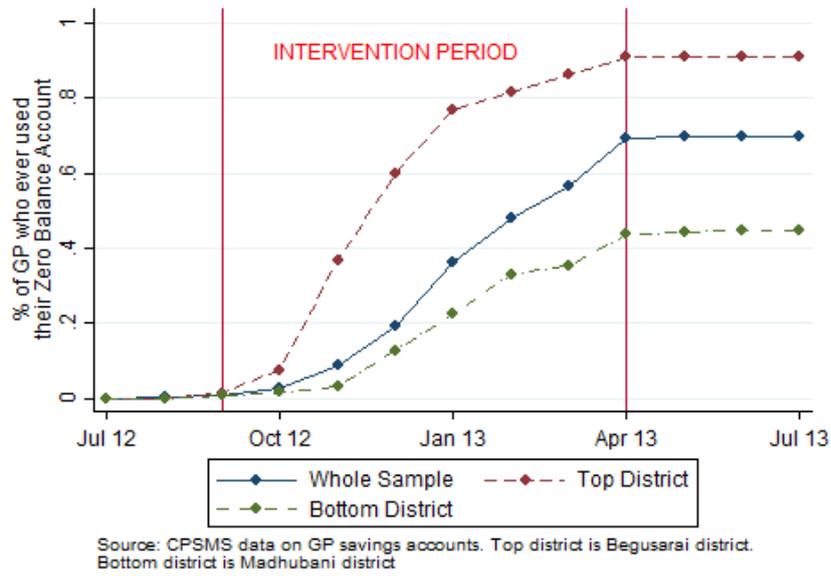


Table A.1: Infrastructure Availability

	Required in	July '12		January '13		April '13	
	Treatment	Treatment	Control	Treatment	Treatment	Control	Control
<i>Infrastructure</i>	(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). 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 panchayat 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	Post-
	Jan 2011 - Aug 2012	Sep 2012 - Mar 2013	Intervention Apr - Jun 2013
	(1)	(2)	(5)
Panel A: Number of audits			
Treatment	0.173 (0.149)	0.113 (0.464)	0.0371 (0.191)
Observations	195	195	195
Mean in Control	1.079	7.286	2.540
Panel B: Number of works audited			
Treatment	2.278 (4.847)	-1.483 (2.984)	0.519 (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 (1.780)	-0.191 (0.813)	0.264 (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 (0.0518)	0.00593 (0.0194)	0.0452* (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 where 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. Standard errors are clustered at the block level. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

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

	Without Control (1)	With Control (2)	Without Control (3)	With Control (4)
Table 2				
Panel A: Total debit from GP accounts				
Treatment/use system	-2.306*** (0.530)	-2.567*** (0.520)		
Observations	3,025	3,025		
Mean in Control	9.540	9.540		
Panel B: Closing balance in GP accounts				
Treatment/use system	-1.266*** (0.240)	-1.313*** (0.244)		
Observations	3,025	3,025		
Mean in Control	4.270	4.270		
Panel C: Total credit to GP accounts				
Treatment/use system	-3.441*** (0.548)	-3.660*** (0.545)		
Observations	3,025	3,025		
Mean in Control	9.151	9.151		
Table 3				
Panel A: GP expenditures on labor from nrega.nic.in				
Treatment/use system	-2.246*** (0.758)	-2.839*** (0.707)		
Observations	2,965	2,965		
Mean in Control	13.78	13.78		
Panel B: GP expenditures on material from nrega.nic.in				
Treatment/use system	-1.078** (0.529)	-1.351*** (0.519)		
Observations	2,965	2,965		
Mean in Control	7.728	7.728		
Table 4				
Panel A: Days worked from nrega.nic.in				
Treatment	-672.4* (363.6)	-859.4** (367.6)		
Observations	2,959	2,959		
Mean in Control	5028	5028		
Panel B: Days per working household from nrega.nic.in				
Treatment	-0.00410 (0.930)	-0.0554 (0.934)		
Observations	2,868	2,868		
Mean in Control	33.65	33.65		
Panel C: Number of working households from nrega.nic.in				
Treatment	-13.60* (8.150)	-18.65** (7.946)		
Observations	2,959	2,959		
Mean in Control	140.2	140.2		
Panel D: Wages received from nrega.nic.in (100,000 rupees)				
Treatment			-1.058* (0.570)	-1.347** (0.582)
Observations			2,959	2,959
Mean in Control			7.780	7.780
Panel E: Average delays in payment from nrega.nic.in				
Treatment			20.36*** (3.813)	20.04*** (3.776)
Observations			2,735	2,735
Mean in Control			53.28	53.28
Table 5				
Panel A: MGNREGS participation				
Treatment			0.00760* (0.00449)	0.00671 (0.00441)
Observations			390	390
Mean in Control			0.0302	0.0302
Panel C: Wages received for MGNREGS employment				
Treatment			12.90 (19.29)	9.824 (19.59)
Observations			390	390
Mean in Control			79.62	79.62
Panel D: Average delays in payment (days)				
Treatment			44.79*** (11.66)	44.77*** (11.75)
Observations			200	200
Mean in Control			58.38	58.38
Table A.6 Fraction of assets found				
Treatment			0.310 (0.239)	0.267 (0.242)
Observations			385	385
Mean in Control			11.68	11.68
Table 7				
Panel A: Match rate for job cards with one name only				
Treatment			0.0152* (0.00787)	0.0161** (0.00783)
Observations			2,836	2,836
Mean in Control			0.679	0.679
Panel B: Match rate for job cards with two names or more				
Treatment			0.0119 (0.00810)	0.0125 (0.00821)
Observations			2,803	2,803
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 household 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). Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

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 (0.0613)	0.0518 (0.0590)	-0.210*** (0.0496)	-0.371*** (0.0636)	-0.289*** (0.0542)	-0.00269 (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 (0.0516)	0.0180 (0.0363)	-0.238*** (0.0451)	-0.324*** (0.0452)	-0.314*** (0.0450)	-0.0252 (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 (0.0678)	0.0926 (0.0701)	-0.447*** (0.0604)	-0.205*** (0.0770)	-0.493*** (0.0689)	0.137** (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. Stars denote significance levels. *, ** and *** denote significant

Table A.6: Reform Impact on MGNREGS Projects: Evidence from the Asset Survey

	Number Registered		Number found	
	All Projects (1)	Ongoing (2)	All Projects (3)	Ongoing (4)
Treatment	0.0494 (0.263)	-0.210 (0.413)	0.310 (0.239)	0.0269 (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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table A.7: Reform Impact on Household MGNREGS Participation (Household Survey)

	Household Participation in MGNREGS	
	Anytime before (1)	Since July 2012 (2)
Treatment	-0.00259 (0.0141)	0.00508 (0.00788)
Observations	390	390
Mean in Control	0.238	0.0787

Note: The unit of observation is a GP. 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 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. Standard errors are clustered at the block level. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table A.8: Reform Impact on Household Consumption (Household Survey)

	Log Monthly Consumption			
	All	Frequent expenditures	Recurrent expenditures	Rare expenditures
	(1)	(2)	(3)	(4)
Treatment	0.0128 (0.0250)	0.00699 (0.0195)	-0.0280 (0.0302)	0.0310 (0.0471)
Observations	9,667	9,666	9,650	9,410

Note: 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 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. Standard errors are clustered at the block level. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table A.9: 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. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table A.10: 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		
Treatment/use system	-3.441***	-5.363***		-1.058*	-1.622*
	(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		
Treatment/use system	-1.266***	-1.973***		20.36***	30.40***
	(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	Table 5		
Panel C: Total credit to GP accounts			Panel A: MGNREGS participation		
Treatment/use system	-2.306***	-3.593***	Treatment/use system	0.00760*	0.0112*
	(0.530)	(0.863)		(0.00449)	(0.00666)
Observations	3,025	3,025	Observations	390	390
Mean in Control	9.540	9.540	Mean in Control	0.0302	0.0302
Table 3			Panel C: Wages received for MGNREGS employment		
Panel A: GP expenditures on labor from nrega.nic.in			Treatment/use system		
Treatment/use system	-2.246***	-3.442***		12.90	19.07
	(0.758)	(1.192)		(19.29)	(28.57)
Observations	2,965	2,965	Observations	390	390
Mean in Control	13.78	13.78	Mean in Control	79.62	79.62
Panel B: GP expenditures on material from nrega.nic.in			Panel D: Average delays in payment (days)		
Treatment/use system	-1.078**	-1.652**	Treatment/use system	44.79***	65.83***
	(0.529)	(0.815)		(11.66)	(18.41)
Observations	2,965	2,965	Observations	200	200
Mean in Control	7.728	7.728	Mean in Control	58.38	58.38
Table 4			Table A.6 Fraction of assets found		
Panel A: Days worked from nrega.nic.in			Treatment/use system		
Treatment	-672.4*	-1,031*		0.310	0.454
	(363.6)	(566.7)		(0.239)	(0.347)
Observations	2,959	2,959	Observations	385	385
Mean in Control	5028	5028	Mean in Control	11.68	11.68
Panel B: Days per working household from nrega.nic.in			Table 7		
Treatment	-0.00410	-0.00616	Panel A: Match rate for job cards with one name only		
	(0.930)	(1.398)	Treatment/use system	0.0152*	0.0227*
Observations	2,868	2,868		(0.00787)	(0.0118)
Mean in Control	33.65	33.65	Observations	2,836	2,836
Panel C: Number of working households from nrega.nic.in			Mean in Control	0.679	0.679
Treatment	-13.60*	-20.85	Panel B: Match rate for job cards with two names or more		
	(8.150)	(12.75)	Treatment/use system	0.0119	0.0176
Observations	2,959	2,959		(0.00810)	(0.0120)
Mean in Control	140.2	140.2	Observations	2,803	2,803
			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 and 7. For Table 5, the unit of observation is a household. 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). GPs that were present in the survey or nrega.nic.in data but could not be found in CPSMS data were considered as non-compliers. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table A.11: 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)						
Sample Districts	-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)						
Sample Districts	-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 (for stochastic dominance)	.91		.01		.09	
Panel C: Average Effects on Total Assets (OLS)						
Sample Districts	-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)						
Sample Districts	-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 (for stochastic dominance)	.34		.03		.05	
Functionary Controls	No	Yes	No	Yes	No	Yes
District Controls	No	Yes	No	Yes	No	Yes

Note: 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. 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). Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.

Table A.12: Year of e-FMS Implementation for Wage and Material Payments by State

State	Financial Year of EFMS implementation							
	Wage payments				Material expenditures			
	2012-13 (1)	2013-14 (2)	2014-15 (3)	2015-16 (4)	2012-13 (5)	2013-14 (6)	2014-15 (7)	2015-16 (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.13: Reform Impact on MGNREGS Spending in Control Blocks

	Before	Set up	Intervention Period			After
	Sept 2011 - June 2012	July-August 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	-0.386 (0.793)	-0.0458 (0.331)	-1.313*** (0.420)	-1.449*** (0.314)	-2.762*** (0.658)	-0.994 (0.948)
Sample	-2.334** (0.953)	-0.793** (0.326)	-0.344 (0.469)	-0.227 (0.398)	-0.571 (0.776)	-0.660 (1.092)
Observations	4,208	4,208	4,208	4,208	4,208	4,208
Mean in Border blocks	15	4.324	5.424	4.130	9.554	15.76
Panel B: Closing Balance in GP Accounts						
Treatment	-0.149 (0.258)	0.120 (0.275)	-0.996*** (0.306)	-1.223*** (0.282)	-1.190*** (0.282)	-0.103 (0.290)
Sample	-0.0532 (0.270)	0.100 (0.297)	0.711** (0.278)	0.599** (0.285)	0.581** (0.287)	1.187*** (0.231)
Observations	4,208	4,208	4,208	4,208	4,208	4,208
Mean in Border blocks	3.984	4.161	3.523	3.791	3.795	3.749
Panel C: Total Credit to GP Accounts						
Treatment	-0.229 (0.870)	0.193 (0.365)	-2.500*** (0.477)	-1.337*** (0.382)	-3.838*** (0.666)	0.283 (0.940)
Sample	-2.496*** (0.951)	-0.682* (0.370)	0.325 (0.491)	-0.480 (0.458)	-0.154 (0.830)	-0.0347 (1.103)
Observations	4,208	4,208	4,208	4,208	4,208	4,208
Mean in Border blocks	15.74	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. Sample is a dummy set equal to one for control and treatment blocks. Specifications do not include district fixed effects. Standard errors are clustered at the block level. Stars denote significance levels. *, ** and *** denote significant differences at the 10%, 5% and 1% levels respectively.