Payments Infrastructure and the Performance of Public Programs: Evidence from Biometric Smartcards in India*

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Abstract

Anti-poverty programs in developing countries are often difficult to implement as intended; one common challenge is governments' limited capacity to deliver payments securely to targeted beneficiaries. We evaluate the impact of biometrically-authenticated payments infrastructure on public employment and pension programs in the Indian state of Andhra Pradesh, using a large-scale experiment that randomized the rollout of the new system over 158 sub-districts and 19 million people. We find that, while far from perfectly implemented, the new system delivered a faster, more predictable, and less corrupt payments process without adversely affecting program access. Distributions of key outcomes in treated areas first-order stochastically dominated those in control areas, and beneficiaries overwhelmingly favored the new payments system. The investment was cost-effective, as time savings to beneficiaries alone were equal to the cost of the intervention (in the case of the employment scheme). Overall the results suggest that investing in secure authentication and payments infrastructure can significantly add to "state capacity" to effectively implement social programs in developing countries.

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1 Introduction

Sending and receiving money securely across space is fundamental to the scale and scope of an economy. Developed countries today are unusual in that their banking infrastructure and legal environments allow for relatively seamless remote transactions: mail order, online shopping, money wires, Electronic Benefit Transfers, and so on. In most times and places, however, payments infrastructure was – and remains – less advanced. Payments often move through informal networks (for example, the Maghribi traders studied by Greif (1993) or the present-day "hawala" system in South Asia and the Middle East) or not at all. In the public sector, weak payments infrastructure makes it difficult to deliver fast and reliable payments to transfer recipients, and facilitates graft.[1] Thus, the lack of a secure payments infrastructure constrains both public and private economic transactions.

Investing in payments infrastructure can therefore be seen as investing in "state capacity" that improves the state's ability to implement its welfare policies and expands its long-term policy choice set (Besley and Persson, 2009, 2010). More broadly, it can be seen as public infrastructure – akin to roads, railways, or the internet, which may have initially been set up by governments for their own use (such as moving soldiers to the border quickly, or improving intra-government communication), but eventually generated substantial spillovers to the private sector as well.

Given this logic, recent advances in payments technology have generated considerable interest regarding their potential to improve the performance of public welfare programs, as well as to provide financial inclusion for the poor.[2] This is nowhere more true than in India. In 2009, the Indian government embarked on an ambitious two-step agenda: deliver unique, biometric-linked IDs to all 1.2 billion residents via the Aadhaar ("foundation") initiative, and then introduce Direct Benefit Transfers for social program beneficiaries using Aadhaar-linked bank accounts.[3] The Unique ID Authority of India has argued that Aadhaar will empower poor and underprivileged residents in accessing services such as the formal banking system and give them the opportunity to easily avail various other services provided by the Government and the private sector. Finance Minister P. Chidambaram has simply said that the project would be "a game changer for governance."[4]

At the same time, there are at least five reasons to be skeptical about the impacts of

[1]For instance, Reinikka and Svensson (2004) and Niehaus and Sukhtankar (2013a,b) document cases of corruption where junior government officials simply steal funds meant for the poor rather than delivering them to the intended recipients.

[2]The Better than Cash Alliance advocates for the adoption of electronic payments on the grounds that they "advance financial inclusion and cost savings while giving governments a more efficient, transparent and secure means of disbursing benefits." See `http://betterthancash.org/`, accessed 29 January 2014.

[3]Malaysia, South Africa, and Indonesia have similar pilot programs under way.

[4]`http://uidai.gov.in/index.php?option=comcontent&view=article&id=58&Itemid=106`, accessed September 10, 2013; `http://www.nytimes.com/2013/01/06/world/asia/india-takes-aim-at-poverty-with-cash-transfer-program.html`, accessed 3 October 2013.

new payments technology and whether they warrant the cost entailed. First, implementation involves a complex mix of technical and logistical challenges, raising the concern that the undertaking might fail unless all components are well-implemented (Kremer, 1993). Second, vested interests might subvert the intervention if their rents are threatened (Prescott and Parente, 2000). Third, the new system could generate exclusion errors if genuine beneficiaries are denied payments due to technical problems. This would be particularly troubling if it disproportionately hurt the most vulnerable beneficiaries (Khera, 2011). Fourth, reducing rent-extraction could paradoxically hurt the poor if it dampened incentives for officials to implement anti-poverty programs in the first place (Leff, 1964). Finally, even assuming positive impacts, the best available estimates of cost-effectiveness depend on a number of untested assumptions (see e.g. NIPFP (2012)).

We contribute evidence to this debate by measuring the impacts, both positive and negative, of a large-scale rollout of biometric payments infrastructure integrated into social programs in India. Working with the Government of the Indian state of Andhra Pradesh (AP), we randomized the order in which 158 sub-districts introduced biometrically-authenticated electronic benefit transfers into two large social programs: the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) and Social Security Pensions (SSP). The intervention involved both technical as well as organizational changes to the payments process, and was referred to in short as the AP "Smartcards" project. Importantly, Smartcards provided beneficiaries with the same effective functionality as intended by Aadhaar-enabled Direct Benefit Transfers for these two programs. The experiment thus provides an opportunity to learn about the likely impacts of Aadhaar specifically, as well as modernized payments infrastructure more generally.

The experiment randomized the form of payments over a universe of about 19 million people, with randomization conducted over entire sub-districts, making it one of the largest randomized controlled trials ever conducted. Further, evaluating an "as is" implementation of a complex program by a government addresses one common concern about randomized trials in developing countries: that studying NGO-led pilots may not provide accurate forecasts of performance at scales relevant for policy-making (see for example Bold et al. (2013)). Our estimates yield the policy parameter that is of practical relevance, because it reflects the impacts that followed a decision by senior government officials to invest in the new payments system and is net of all the logistical and political economy challenges that accompany such a project in practice.

After two years of program rollout, the share of Smartcard-enabled payments in treated sub-districts had reached 50%.[5] Major impediments to implementation included included logistical challenges in enrolling beneficiaries and distributing cards, procuring

[5]This figure compares favorably to the pace of electronic benefit transfer (EBT) rollout in other contexts. The United States, for example, took over 15 years to convert all Social Security payments to EBT.

and deploying authentication devices and ensuring their functionality, appointing new payment agents, and cash management and security; administrative challenges such as designing optimal contracts with implementing banks and the difficulties in re-optimizing them under a public procurement regime; and political challenges such as attempts by local political elites to capture the new process by influencing field staff hiring (see Mukhopadhyay et al. (2013) for details). Such frictions illustrate the importance of evaluating at scale under real-world conditions. They also motivate our emphasis throughout the paper on intent-to-treat analysis, which correctly estimates the average return to as-is implementation following the "intent" to implement the new system.

We find that, even while incompletely implemented, Smartcards substantially improved the payment collection process, significantly so for NREGS recipients. NREGS workers workers spent 21 minutes less on collecting each payment (19% less than the control group) and payment delays between working and receiving wages fell by 10 days (29% of the control mean). The absolute deviation of payment delays also fell by 39% relative to control, suggesting that payments became more predictable.[6]

Beneficiaries also received significantly more money. The average NREGS household reported earning 23% more through the program, while individual labor supply on NREGS went up 12% (notsignificant). Government outlays on NREGS, on the other hand, did not change, resulting in a 12.2 percentage point reduction in "leakage" of funds (a 40% reduction relative to the control mean of 30.7%). SSP participants, meanwhile, saw a 1.8 percentage point reduction in the incidence of bribe demands for obtaining their payments (a 47% reduction relative to the control mean 3.8%) and the incidence of "ghost" SSP pensioners fell by 1.1 percentage points (not significant, control mean 7.3%).

Notably, these gains for participants were not offset by reduced access to programs in the first place. While many NREGS workers in the control group report difficulty getting work, these numbers look marginally if insignificantly *better* in treated areas. Overall, the data suggest that Smartcards improved beneficiary experiences in collecting payments, and also reduced corruption and increased payments received by program participants, without substantially altering fiscal burdens on the state. Consistent with this view, 84% of NREGS job card holders and 91% of SSP recipients who experienced the system reported that they prefer the new system to the old.

We find no evidence that poor or vulnerable segments of the population were worse off under the new system. For key outcomes such as the time to collect payments, payment delays, and NREGS payments received, the treatment distributions first-order stochastically dominate the control distributions. Thus, no treatment household was worse off relative to a control household at the same percentile of the outcome distribution.

[6]Payment collection times for SSP beneficiaries also improved, although improvements were small and statistically insignificant, reflecting the fact that status quo collection times and reliability for this program prior to the intervention was much better than NREGS.

Treatment effects also did not vary significantly as a function of village-level baseline characteristics, suggesting broad-based gains from access to the new payments system.

Finally, the Smartcard system was also cost-effective. Several of the impacts represent pure efficiency gains – in particular, saved beneficiary time and reduced variability in payment delays – and the value of these alone appears to exceed the government's cost of program implementation and operation, at least for the NREGS (\$4.44 million in time savings compared to \$4.25 million costs of implementation). Other impacts are inherently redistributive, and hence not Pareto improvements: for example, reduced mean payment lags benefit recipients at the cost of banks, and reduced leakage benefits recipients at the expense of corrupt officials. However, if a social planner places a greater weight on the gains to beneficiaries (who are likely to be poorer) than on the loss to corrupt officials, then the welfare gains from reduced leakage will be positive. The reduction in leakage is also much larger than the cost of implementation; for example, the NREGS leakage reduction of \$38.7 million/year is nine times the cost of implementation.

Our paper fits most directly within the recent literature on technology and service delivery in developing countries. An emerging theme in this literature is that technology may or may not live up to its hype. Duflo et al. (2012) find, for example, that digital cameras and monetary incentives increased teacher attendance and test scores in Indian schools (when implemented in schools run by an NGO). Banerjee et al. (2008) find, on the other hand, that a similar initiative to monitor nurses in health care facilities was subverted by vested interests (when implemented by the government in the public system). Such contrasting results highlight the importance of as-is evaluation in scaled-up settings.

Our results also add to a growing literature on the impact of payments and authentication infrastructure in developing countries. Jack and Suri (2014) find that the MPESA mobile money transfer system in Kenya improved risk-sharing; Aker et al. (2012) find that using mobile money to deliver transfers in Niger cut costs and increased women's intra-household bargaining power; and Gine et al. (2012) show how biometric authentication helped a bank in Malawi reduce default and adverse selection.

Finally, our results complement recent theoretical work on state capacity (Besley and Persson, 2009, 2010) by demonstrating that the returns to investing in payments infrastructure can be large and positive even over as short a time horizon as two years, and even when integrated into only two public welfare programs. Our analysis in this paper does not quantify potential future benefits to other public programs, or to private sector actors as they make use of the new infrastructure, e.g. to offer financial services. The "public infrastructure" role of payments systems thus remains open for study, as we discuss in the conclusion.

The rest of the paper is organized as follows. Section 2 describes the context, social programs, and the Smartcard intervention. Section 3 lays out the research design, and

Section 4 presents results. Section 5 discusses cost-effectiveness and welfare impacts; Section 6 concludes.

2 Context and Intervention

India runs several programs to reduce poverty, but they are typically poorly implemented (Pritchett, 2010). Most programs suffer from high levels of "leakage," defined as the fraction of money spent that does not reach the intended beneficiary. For example, the two flagship welfare schemes – the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) and the Targeted Public Distribution System (TPDS) – have been estimated to have leakage rates of 40% to 80% (Niehaus and Sukhtankar, 2013a,b; Programme Evaluation Organization, 2005). Benefits that do reach the poor are often delivered with long and variable lags, and typically require beneficiaries to make multiple trips (including unsuccessful ones) over considerable distances to collect their payments. The Andhra Pradesh (AP) Smartcard Program aimed to reduce leakage and improve beneficiary experiences in collecting payments by building a biometrically-authenticated payments infrastructure and integrating this into two major social welfare programs of the Department of Rural Development (NREGS and SSP). The AP Smartcard Program was India's first scaled up attempt to use a biometric payments infrastructure to deliver payments to program beneficiaries.[7] Key features of the two affected programs are described below, along with a discussion of the differences between the new and original payment systems.

2.1 The Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS)

The NREGS is one of the two main welfare schemes in India, and likely the largest workfare program in the world, covering 11% of the world's population. The Government of India's allocation to the program for fiscal year April 2013-March 2014 was Rs. 330 billion (US $5.5 billion), or 7.9 percent of its budget.[8] The program guarantees every rural

[7] A key motivation for India's decision to invest in biometrically-authenticated payments infrastructure using the Aadhaar platform was a desire to reduce leakage in public welfare programs and to improve beneficiary experiences in accessing their benefits. However, while the Aadhaar is an enabling infrastructure that can be used to better implement any program, evaluating its impact would require Aadhaar to be integrated with welfare programs, which has not yet taken place (since Aadhaar is still being rolled out). The AP Smartcard program therefore provides a functional precursor to the integration of Aadhaar in the NREGS and SSP programs. Evaluating its impact can help inform broader national policy decisions on the costs and benefits of integrating Aadhaar into other programs and beyond AP.

[8] NREGS figures: `http://indiabudget.nic.in/ub2013-14/bag/bag5.pdf`; total outlays: `http://indiabudget.nic.in/ub2013-14/bag/bag4.pdf`

household 100 days of paid employment each year. There are no eligibility requirements, as the manual nature of the work is expected to induce self-targeting.

To participate in the NREGS, workers must first obtain jobcards, which list household members and have empty spaces for keeping records of employment. Jobcards can be obtained from the local Gram Panchayat (GP, or village) or mandal (sub-district) government offices. Workers with jobcards can apply for work at will, and officials are legally obligated to provide either work or unemployment benefits (though in practice, the latter are never issued). The range of projects approved under NREGS is stipulated by the government and typically consists of minor irrigation projects or improvement of marginal lands. Implementation takes place under the supervision of officials called Field Assistants. These officials record attendance and output on muster rolls and send these to the sub-district for digitization, which triggers the release of funds to pay workers.

Figure 1 depicts the payment process in Andhra Pradesh before the introduction of Smartcards. In this original system (which is typical for NREGS payments across India), state governments transfer money to beneficiary post office savings accounts. Workers operate the accounts with physical passbooks to establish identity and withdraw cash. In practice, it is quite common for illiterate workers to hand over their passbooks to the Field Assistant, who controls and operates the accounts for multiple workers by taking sets of passbooks to the post office, withdrawing cash in bulk for workers, and paying cash in the villages. In cases where workers keep their passbooks and operate their own accounts, they have to travel individually to the post office to collect payments and often must make multiple trips due to unsuccessful attempts to withdraw cash.

Field reports, as well as data from our control group below, suggest that this payment process can be slow, unreliable, and prone to considerable leakage. As described in Niehaus and Sukhtankar (2013a,b), theft from the labor budget can take two forms: over-reporting or under-payment. For example, a worker might do Rs. 100 worth of work, but an official might report to the government that she is owed Rs. 150 and pocket Rs. 50 for himself; this is over-reporting. In addition, the official could pay the worker only Rs. 90; in this case he earns Rs. 10 from under-payment. "Ghost" or non-existent/fake workers are one extreme form of over-reporting. In particular, the control exercised by local officials on both the upward flow of information regarding work done, as well as the downward flow of cash (as seen in Figure 1), makes it feasible for them to over-report work done, and collude with post office officials to divert the payments. Local officials can also abuse their position of power by not paying workers the full amounts owed to them. Prior research (Niehaus and Sukhtankar, 2013a,b) suggests that over-reporting is much more common than under-payment, perhaps because the former is less politically costly. The status quo system also features considerable delays and uncertainty in payments, which in turn can limit the extent to which the NREGS serves as an insurance mechanism for the rural poor.[9] In extreme cases, delayed payments have even been reported to have led to worker suicides.[10]

2.2 Social Security Pensions (SSP)

Social Security Pensions (SSP) are monthly payments targeted to vulnerable populations. The program covers over 6 million beneficiaries and costs the state roughly Rs. 18 billion ($360 million) annually. Eligibility is restricted to members of families classified as Below the Poverty Line (BPL), who are local residents of the district in which they receive their pension and not covered by any other pension scheme. In addition, recipients must qualify in one of four categories: old age (> 65), widow, disabled, or certain displaced traditional occupations. Pension lists are proposed by local village assemblies (Gram Sabhas) and sanctioned by the mandal administration. Pension amounts are very modest and typically pay Rs. 200 (~$3) each month, except for the disability pension that pays Rs. 500 (~$8) per month.

Unlike the NREGS, pension payments are typically made in the village itself, with cash being disbursed by a designated government official (village development officer) each month. While rigorous evidence on leakage and payment delays in the SSP programs was not available at the start of our study, journalist accounts suggested that the most common forms of irregularities were "ghost" beneficiaries (especially non-removal of deceased beneficiaries from the roster), requirements to pay bribes to get put on the beneficiary roster, and demands for "commissions" to disburse payments.[11] Between the two programs, the government aims to provide social insurance to the able-bodied who can work (NREGS) as well as those unable to work (SSP), with benefits under the former being more generous.

[9] The imperfect implementation of government social insurance programs may even be a deliberate choice by local elites to preserve their power over the rural poor by being the default provider of insurance (see Anderson et al. (2013) for a detailed discussion of such deliberate non-implementation; Jayachandran (2006) shows that rainfall shocks benefit landlords and hurt workers due to the fall in wages induced by increased labor supply by poor workers attempting to meet subsistence needs; hence improved insurance for laborers may make landlords worse off).

[10] See, for example, http://www.hindustantimes.com/india-news/delayed-nrega-payments-drive-workers-to-suic article1-1167345.aspx).

[11] A large number of newspaper articles, from states all over India, record the presence of fake and ineligible pension beneficiaries. See, for example, http://indianexpress.com/article/india/india-others-do-not-use/70-000-and-still-counting-fake-old-age-pensioners/, http://articles.timesofindia.indiatimes.com/2013-10-12/chandigarh/42967727_1_old-age-pension-pension-amount-fake-beneficiaries, and http://archives.digitaltoday.in/indiatoday/20050620/web2.html. Note that unlike in the NREGS, over-reporting of the amount to be paid is more difficult in the SSP program since the amounts to be paid are fixed administratively.

2.3 Smartcard-enabled Payments and Potential Impacts

The Smartcard intervention modified the pre-existing payment system for NREGS and SSP participants in two ways. First, it required beneficiaries to biometrically authenticate their identity before collecting payments. Under the new system, beneficiaries were enrolled in the Smartcard program through a process that collected biometric data (typically all ten fingerprints) and took a digital photograph. This information was stored in a secure database and a linked bank account was created for each beneficiary, following which they were issued a "Smartcard" that included their photograph and (typically) an electronic chip that stored biographic, biometric, and bank account details.

The new process of collecting payments involved the following steps: (a) beneficiaries insert their Smartcard into a Point-of-Service device kept by a Customer Service Provider (CSP), which reads the Smartcard and retrieves account details; (b) the device prompts for a randomly generated fingerprint to be placed on the card reader (the beneficiary is typically assisted by the CSP in this process); (c) this fingerprint is matched with the records on the Smartcard, and transactions are authorized after a successful match; (d) the amount of cash requested is disbursed;[12] and (e) the authentication device prints out a receipt as it issues payments, in some cases even announcing transaction details in the local language (Telugu) to assist illiterate beneficiaries. Figure 2 illustrates a typical Smartcard and a fingerprint scan in progress.[13]

The second change is that the new system reduced the physical and social distance between the beneficiaries and the point of payment collection by routing payments through a village-level Customer Service Provider (CSP). Government regulations required that CSPs hired for this purpose be women who were residents of the villages they served, have completed secondary school, not be related to village officials, preferably be members of historically disadvantaged castes, and be members of a self-help group (a local group of micro entrepreneurs, targeted by the AP government for micro-lending). While meeting all these requirements proved difficult in some cases, these norms ensured that the social profile of the typical CSP was closer to that of beneficiaries, compared to post-office officials (who are usually government employees). They also typically made the payments in the village, thus reducing both the physical and social distance to collect payments.

To implement this intervention, the government contracted with private and state-run banks, who in turn wrote sub-contracts with technology service providers. While the

[12]In principle, beneficiaries could use the Smartcards as a savings account and leave money in it, but the regulatory approvals for using the Smartcards in this form had not been provided by the Bank regulator (the Reserve Bank of India) at the time of the study.

[13]Note that a physical Smartcard is not always required. For example, one Bank chose to issue paper cards with digital photographs and bar codes and to store the biometric details in the Point-of-Service device instead of the card. Beneficiaries still authenticate their fingerprints against those in the device in this system.

banks technically "owned" the accounts, it was the technology providers who built and managed the actual payments system, including enrolling recipients, issuing Smartcards, hiring CSPs and managing cash logistics.[14] Each district was assigned a single bank-technology provider pairing, which received 2% of the value of each transaction as a payment directly from the government (banks and technology providers reached their own arrangements on how this commission would be split between them, and entered the contract with the government as a combined entity). Figure 1 illustrates the flow of funds from the government through banks and technology providers to CSPs and beneficiaries under the new scheme.

While the Smartcard program was designed to improve beneficiary welfare, the impacts were nevertheless ambiguous a priori, with potential for both positive and negative impacts. Consider first the payment collection process. Smartcards could speed up payment if technology providers succeeded in a locating a CSP in each village, reducing travel time relative to long walks to the nearest post office. However, they could also slow down the process if CSPs are not reliably present, or if the checkout process slows down due to failures of biometric authentication (for example, increasing the time per transaction due to repeated attempts to authenticate). On-time cash availability could improve or deteriorate depending on how well technology providers managed cash logistics relative to the post office. Most troubling, Smartcards might cut off benefits to many beneficiaries if they have difficulty obtaining cards in time, misplace their cards, or are denied payments due to either malfunctioning authentication devices or errors in matching biometrics. Skeptics of biometric authentication have repeatedly raised these concerns (Khera, 2011).

Impacts on fraud and corruption are also unclear. In principle, Smartcards should reduce payments to "ghost" beneficiaries as these do not have fingerprints and cannot collect payments. It should also make it harder for corrupt officials to collect payments in the name of real beneficiaries, since beneficiaries must be present and provide biometric input, and are also given a receipt which they can cross-check against the amount they receive. However, these arguments assume that the field technology works as designed. Given the complexity of implementing the new system well, it was possible that the entire program would not be implemented well enough to be effective. For instance, under incomplete implementation (see below), it is possible that the main channels of leakage are not effectively plugged.

Even if Smartcards achieve their stated goal of reducing corruption, they could have

[14] This structure was a result of regulatory requirements of the Reserve Bank of India (RBI), which stipulated that accounts could only be created by banks. However, since the fixed cost of bank branches was too high to make it viable to profitably serve rural areas, the RBI permitted banks to partner with TSPs to jointly offer and operate no-frills accounts that could be used for savings, benefits transfers, remittances, and cash withdrawals.

other negative consequences. Making corruption more difficult on some margins could simply displace it to others (Yang, 2008; Niehaus and Sukhtankar, 2013a). For example, cleaning up bribery in SSP payments could drive up the illicit price of getting on the SSP list in the first place. In addition, cracking down on graft could reduce local officials' incentives to implement programs like the NREGS in the first place, which could in turn hurt workers on the extensive margin of access to work. In these cases, even if corruption is reduced, the savings may not be spent on the rural poor.

3 Research Design

3.1 Randomization

The AP Smartcard project started in 2006, but there were several implementation challenges that took time to resolve (including contracting, integration with the existing program structure in the field, CSP selection, logistics of enrollment and cash management, and development of systems for financial reporting and reconciliation). Further, the Government of Andhra Pradesh (GoAP) followed a "one district, one bank" implementation model for the Smartcard Program, which led to considerable heterogeneity among districts in program implementation as a function of the performance of the bank that was assigned to the district. In early 2010, GoAP decided to restart the Smartcard program in eight districts where the originally assigned banks had not made any progress, and re-allocated the contracts for these districts to banks that had demonstrated better performance in other districts. This "fresh start" provided an ideal setting for an experimental evaluation of Smartcards because the roll-out of the intervention could be randomized in these districts, after basic implementation challenges had been solved by the banks in other districts and the overall project had stabilized from an implementation perspective.

Our randomized evaluation of the impact of Smartcards was conducted in these eight districts of Andhra Pradesh, with a combined rural population of around 19 million. While not randomly selected, study districts look similar to the remaining 15 districts of AP on the major socioeconomic indicators, including proportion of rural, scheduled caste, literate, and agricultural labor populations. They are also geographically spread out across the state, with representation in all three historically distinct socio-cultural regions (2 in Coastal Andhra, and 3 each in Rayalseema and Telangana).[15] The study was conducted under a formal agreement between J-PAL South Asia and GoAP to randomize the order in which mandals (sub-districts) were converted to the Smartcard system.

[15]The districts were Adilabad, Anantapur, Khammam, Kurnool, Nalgonda, Nellore, Vizianagaram, and Kadapa. Note that the socio-cultural regions are distinct enough that the Indian Parliament has recently approved the split of the Telangana region from Andhra Pradesh to become a new state.

Mandals were randomly assigned to one of three waves: 113 to wave 1, 195 to wave 2, and 45 to wave 3 for a sequential roll out (Figure 3). Our evaluation design focuses on comparing outcomes in wave 1 (treatment) and wave 3 (control) mandals.[16] Randomization was stratified by revenue division (an administrative unit between the district and mandal) and by a principal component of numerous other mandal characteristics.[17] Table 1 presents tests of equality between treatment and control mandals along several characteristics reported in official sources, none of which differ significantly (unsurprisingly, as these data were used for stratification). Table 2 shows household characteristics from the baseline survey; here, NREGS availability is significantly different (although the baseline and endline control means are not comparable), as is time to collect payments for SSP households. Our main empirical results include controls for the village-level baseline mean value of each outcome to mitigate any imbalances arising through sampling variation.

3.2 Data Collection

Our data collection was designed to assess impacts broadly, including both the positive and negative potential effects discussed above. To capture these we collected a) official records on beneficiary lists and benefits paid; b) baseline and endline household surveys of representative samples of enrolled participants; c) independent audits of NREGS worksites; d) village-level surveys to measure political, social, and development indicators potentially connected to implementation; and e) surveys of officials to capture process and implementation issues. Household surveys asked details on receipts from and participation in NREGS and SSP programs, as well as information about income, employment, consumption, and assets more generally. We timed our field data collection exercises to coincide with the peak period of NREGS participation, which falls between May and July in most districts. We therefore conducted surveys in August through September of 2010 (baseline) and 2012 (endline), and the surveys collected data regarding program participation and payment collection for work done from late May to early July. The intervention was rolled out in the treatment areas shortly after the baseline surveys, and the lag between program rollout in treatment and control areas was over two years. Thus,

[16] A mandal in AP typically has a population of 50,000 - 75,000 and consists of around 25-30 units of village governance (called Gram Panchayats or GPs). There are a total of 405 mandals across the 8 districts. We dropped 51 of these mandals (12.6%) prior to randomization, since the Smartcard program had already started in these mandals. An additional mandal in Kurnool district was dropped because no NREGS data were available for that mandal. Of the remaining mandals, 15 mandals were assigned to treatment and 6 to control in each of Adilabad, Anantapur, Khammam, Kurnool, Nellore; 16 to treatment and 6 to control in Nalgonda; 10 to treatment and 5 to control in Vizianagaram; and 12 to treatment and 4 to control in Kadapa. Note that wave 2 was created as a buffer to maximize the time between program rollout in treatment and control waves and that our study does not use data from these mandals.

[17] Specifically: population, literacy, Scheduled Caste and Tribe proportion, NREGS jobcards, NREGS peak employment rate, proportion of SSP disability recipients, proportion of other SSP pension recipients.

we estimate the impact of the program after being implemented for two years (the gap between baseline and endline surveys).

We sampled 886 GPs using probability proportional to size (PPS) sampling, with six GPs per mandal in six districts and four GPs per mandal in the other two, and sampled one habitation from each GP again by PPS.[18] Within habitations we sampled six households from the full frame of all NREGS jobcard holders and four from the frame of all SSP beneficiaries. Our NREGS sample includes five households reported in the official records as having worked recently and one household which is not. This sampling design trades off power in estimating leakage (for which households reported as working matter) against power in estimating rates of access to work (for which all households matter). For our endline (baseline) survey we sampled 8826 (8579) households, of which we were unable to survey 268 (899), while 386 (298) households were confirmed as ghost households, leaving us with final set of of 8172 and 7382 households for the endline and baseline surveys respectively.

Note that we have a village-level panel dataset and not a household one (since the endline sample has to be representative of potential workers at that time). So, we test for differential attrition across treatment and control mandals in the sampling frames of the NREGS and SSP programs. While some jobcards drop out of the baseline sample frame because of death, migration, or household splits, and new jobcards also enter because of creation of new nuclear families, migration, and new enrollments, neither change differentially affects treatment mandals (Table A.2a). Similarly, SSP beneficiaries are equally likely to leave or enter the sample in treatment and control areas (Table A.2b). Finally, new entrants are also similar to control and treatment counterparts on demographics (household size, caste, religion, education) and socioeconomics (income, consumption, poverty status) in both the NREGS and SSP sample frames: there were hence no compositional changes in our sample frames (Table A.2).

3.3 Implementation and First-Stage

We present a brief description of program implementation and the extent of actual roll-out for two reasons. First, it helps us distinguish between de jure and de facto realities of the Smartcard program, and thereby helps to better interpret our results by characterizing the program as it was implemented. Second, understanding implementation challenges provides context that may be important if we wish to extrapolate the likely impacts to a different context.

As may be expected, the implementation of such a complex project faced a number of technical, logistical, and political challenges. Even with the best of intentions and admin-

[18] Strictly speaking, it is not always possible to sample more than one unit using PPS; some probabilities were top-censored at 1.

istrative attention, the enrollment of tens of millions of beneficiaries, physical delivery of Smartcards and Point-of-Service devices, identification and training of CSPs, and putting in place cash management protocols would have been a non-trivial task. In addition, local officials (both appointed and elected) who benefited from the status quo system had little incentive to cooperate with the project, and it is not surprising that there were attempts to subvert program activities aimed at reducing leakage and corruption (as also described in Banerjee et al. (2008)). In many cases, local officials tried to either capture the new system (for instance, by attempting to influence CSP selection), or delay its implementation (for instance, by citing difficulties to beneficiaries in accessing their payments under the new system).

On the other hand, the senior-most officials of GoAP (including the Principal Secretary and other top officials of the Department of Rural Development) were strongly committed to the project, and devoted considerable administrative resources and attention to successful implementation. GoAP was also committed to high-quality implementation of NREGS and was among the leading states across India in the utilization of funds earmarked for the program by the (federal) Government of India. Overall, implementation of the Smartcard Program was a priority for GoAP, but it faced an inevitable set of challenges as described above. Our evaluation is therefore based on an "as is" implementation of the Smartcard program at scale.

Figure 4 plots the rollout of the Smartcards program in treatment areas from the start of the implementation in 2010 to 2012 using administrative data for both NREGS and SSP programs. As the figure suggests, implementation was not complete in treated areas. About 90% of treatment group mandals had at least one GP that had converted to the Smartcards-based payment system before the endline in 2012, and conditional on being in a converted mandal, about 90% of GPs had switched the payment mechanism for NREGS payments (96% for SSP payments). At the GP level, being "converted" meant that all payments for NREGS and SSP were made through the Customer Service Provider (CSP) employed by the bank; this included authenticated payments, unauthenticated payments to workers with Smartcards,[19] and payments to workers without Smartcards. Within converted GPs, about 65% of payments were made to beneficiaries with Smartcards. Put together, slightly over 50% of all payments under these programs were "carded payments" – i.e., payments made to beneficiaries with cards – by May 2012.[20] This rate of coverage in two years compares favorably with other experiences of complex project roll-outs even in

[19]Transactions may not be authenticated for a number of reasons, including failure of the authentication device and non-matching of fingerprints.

[20]There was considerable heterogeneity in the extent of Smartcard coverage across the eight study districts, with coverage rates ranging from 31% in Adilabad to nearly 100% in Nalgonda district. This heterogeneity across districts does not affect our main estimates because they all include district fixed effects. Our extensive qualitative evaluation of the process of rolling out the Smartcards (Mukhopadhyay et al., 2013) suggests that the main determinant of this heterogeneity was variation in the effort put in by banks to achieve full coverage.

high-income countries. To put Andhra Pradesh's performance in perspective, the United States took over fifteen years to convert its own Social Security transfers to electronic payments.[21]

We find that treatment GPs are much more likely to be "carded", i.e. migrated to the new payment system, with 67% carded for NREGS payments (79% for SSP). We can also verify that there was practically no contamination in control areas, with less than 0.5% (0% SSP) of control GPs reporting having migrated to the new system (Table 3). The overall rate of transactions done with carded beneficiaries was 51% in treatment areas (59% SSP), with basically no carded transactions reported in control areas. We also asked beneficiaries who had recently worked on NREGS about their Smartcard use to corroborate these official figures, and find that about 38% (46% SSP) of beneficiaries in treatment GPs said that they used their Smartcards both generally or recently, while less than 1% claimed to do so in control areas. Note that the official and survey figures cannot be directly compared since the official figures are the proportion of *transactions* while the survey records the proportion of *beneficiaries*; moreover, the official figures do not separate out actually authenticated transactions from payments simply made to carded beneficiaries that were not authenticated. Meanwhile, the close to 1% figure in control areas may reflect beneficiary confusion between enrollment – when fingerprints were scanned and cards issued (which was done in a few control areas even before our endline) – and actual carded transactions (which were administratively not allowed to be activated by GoAP in control areas till the endline survey). These responses may therefore be cases where beneficiaries took their card to get paid even though the new system was not yet operational.

Overall, both official and survey records indicate that treatment was operational though incomplete in treatment areas, while contamination in control areas was miniscule. Thus, our analysis will focus on using the mandal-level randomization to generate intent-to-treat (ITT) estimates, which should be interpreted as the average treatment effects corresponding to an approximately half-complete implementation. These estimates reflect the magnitudes of impact that are likely under real-world implementation in other states over a similar time horizon, and likely provide a lower-bound of the long-term impacts of fully deploying a biometric payment system like Smartcards.

3.4 Estimation

We report intent-to-treat (ITT) estimates, which compare average outcomes in treatment and control areas. Most outcomes are measured at the household level, with some others (e.g. NREGS work) at the individual level. All regressions are weighted by inverse

[21] Direct deposits started in the mid-1990s; by January 1999 75% of payments were direct deposits; and check payments finally ceased for good on March 1, 2013. See `http://www.ssa.gov/history/1990.html`.

sampling probabilities to obtain average partial effects for the populations of NREGS jobcard holders or SSP beneficiaries. We include district fixed effects in all regressions, and cluster standard errors at the mandal level. We thus estimate

$$Y_{imd} = \alpha + \beta Treated_{md} + \delta District_d + \epsilon_{imd} \tag{3.1}$$

where Y_{imd} is an outcome for household or individual i in mandal m and district d, and $Treated_m$ is an indicator for whether the mandal was in wave 1. When possible we also report specifications that included the baseline panchayat-level mean of the dependent variable, $\overline{Y}^0_{pmd}$, to increase precision and to assess sensitivity to any randomization imbalances. We then estimate

$$Y_{ipmd} = \alpha + \beta Treated_{md} + \gamma \overline{Y}^0_{pmd} + \delta District_d + \epsilon_{ipmd} \tag{3.2}$$

where p indexes panchayats. Note that we easily reject $\gamma = 1$ in our data, and therefore do not report difference-in-differences estimates (since these would be misspecified).

Finally, we test for heterogeneity of program impact along key GP-level baseline characteristics using a standard linear interaction specification of the form:

$$Y_{ipmd} = \alpha + \beta_1 Treated_{md} + \beta_2 \overline{Characteristic}^0_{pmd} + \beta_3 \overline{Characteristic}^0_{pmd} \cdot Treated_{md} + \delta District_d + \epsilon_{imd} \tag{3.3}$$

where β_3 is the term of interest, which indicates whether treatment effects vary significantly by the corresponding initial characteristic (note that we test for heterogeneity by village-level means of each characteristic, since we have a village-level panel and not a household panel).

4 Effects of Smartcard-enabled Payments

4.1 Effects on Program Performance

4.1.1 Payments Process

We first examine impacts on the process of collecting payments. This is an important dimension of program performance in its own right, as payments often arrive after long and variable delays. NREGS recipients in control mandals report waiting an average of 34 days after finishing each spell of work, more than double the 14 days prescribed by law. Payments can also take a long time to collect; control households report spending almost two hours in total collecting an average payment, including both time waiting in line and also time spent on unsuccessful trips.

We find that Smartcards substantially improved the payment process for NREGS.

Columns 1 and 2 of Table 4 report that the total time required to collect a payment fell by 21 minutes in mandals assigned to treatment (a 19% reduction on a base of 112 minutes). The corresponding estimates for SSP recipients, although negative, are smaller and not statistically significant (Table 4). This is not surprising, since SSP payments were made in the village even under the old system. On the other hand, 82% of SSP beneficiaries who received or enrolled for Smartcards said that Smartcards increased the speed of payments (Table 8).

Recipients also faced smaller delays in receiving payments after working, andpayment times became more predictable. Columns 5 and 6 of Table 4 report that assignment to treatment lowered the mean number of days between working and collecting payments by 0 days, or 29% of the control group mean (and 0% of the amount by which this exceeds the statutory limit of 14 days). Columns 7 and 8 show that the *variability* of these lags – measured as the absolute deviation from the median mandal level lag, thus corresponding to a robust version of a Levene's test – also fell, dropping by 39% of the control group mean. While variability need not imply uncertainty, this at least suggests that recipients are exposed to less risk.[22]

4.1.2 Payment Amounts, Bribes, and Leakage

In addition to getting paid faster, recipients get paid more. For NREGS recipients, Columns 3 and 4 of Table 6 show that earnings per household per week during our endline study period increased by Rs. 35, or 24% of the control group mean. For SSP beneficiaries there is less scope for increased earnings, as their benefits are fixed and the control reports a fairly low rate of bribe demands (3.8%). However, we do see a 1.8 percentage point (47%) reduction in this rate. These results are all consistent with the Smartcard program's aspirations of making it more difficult for officials to underpay beneficiaries.

In contrast, we see no major impacts on fiscal outlays. For the NREGS, Figure 5 plots wage outlays in both treatment and control mandals over the entire two-year period from January 2010 (seven months before baseline surveys) to December 2012 (three months after endline surveys). The two series track each other closely, with no discernible differences at baseline, endline, or anywhere else. Columns 1 and 2 of Table 6 confirm this point statistically for the workers sampled into our endline survey; we find no significant difference between treatment and control mandals.[23] We do find a small and insignificant decline of 1.1 percentage points, or 15% of the control group mean, in the

[22] We did not ask questions on date of payment to SSP beneficiaries since payment lags were not revealed to be a major concern for them during our initial interviews. Moreover, since payments are made only once a month, they are spread out over a much longer time frame than the concentrated NREGS work months of May-July, and hence recall issues were a concern.

[23] This is also true for all workers in the full database, not just those sampled.

proportion of SSP beneficiaries identified as "ghosts" (Table 5b, Column 1), implying a small cost savings for government. We see no corresponding change for the NREGS (Table 5a, Column 1), which is consistent with the absence of any change in total outlays.[24]

The fact that recipients report receiving more while government outlays are unchanged suggests a reduction in leakage, particularly for the NREGS. Columns 5 and 6 of Table 6 confirm this, showing that the difference between official and survey measures of earnings per worker per week fell by Rs. 27.

One caveat to this result is that we estimate households in control mandals received Rs. 20 *more* per week than the corresponding official records indicate, implying a negative leakage rate (although this result is not significantly different from 0). We view these estimates of levels of leakage as less reliable than those of the differences, for several reasons.

First, households may have multiple jobcards as a result of multiple nuclear families living together.[25] While we sample from the universe of jobcards and not that of households, it is likely that households respond to the survey questions on the basis of total NREGS work and payment in the household as opposed to the basis of the specific jobcard that was sampled. While we discard survey records for individuals within the household who are not listed on the sampled jobcard for our main comparisons, it is still possible that some workers may be listed on multiple jobcards. Accordingly, the average amount of leakage we find in control areas in the full sample at endline is negative.

Using data from the National Sample Survey Round 68 (July 2011-June 2012) to estimate the number of households with jobcards per district, and our jobcard database to determine the number of jobcards in the district, we find that the number of jobcards exceeds the number of households by a factor of 1.9. Using district-specific factors to scale up official estimates of work done per household rather than per jobcard, we obtain leakage numbers of 30.7% in control areas and 18.5% in treatment areas at endline (p-value of difference = 0.11; results in Appendix Table A.4).[26] We find that the treatment has no effect on self-reported ownership of multiple jobcards, so to the extent that this issue interacts with treatment effects on leakage, these interactions are limited. Moreover, results are very similar with the sample of beneficiaries who told us their household had more than one jobcard (results not shown but available on request).

A further caveat is that it is possible that survey reports of higher payments through NREGS represent collusion between workers and officials and not reductions in leakage:

[24] We define a recipient as a "ghost" if we confirm that they either did not exist or had permanently migrated before the beginning of our study periods (31 May 2010 for baseline, 28 May 2012 for endline). Survey teams confirmed this information with two other neighboring households before making a designation.

[25] In theory, a Smartcard can only be linked to a single jobcard. In practice, however, the process for delinking multiple jobcards from Smartcard accounts was never conducted, partly due to the considerable time and expense involved in determining which was the "real" or active jobcard.

[26] Note that for these estimates we also include survey reports of *all* workers within the household.

while in both cases more money likely makes it way to the pockets of beneficiaries, our analysis of random audits of worksites helps us separate these stories. While the results are noisy, they suggest an increase in worker presence at worksites that is roughly proportional to the increase in survey reports, which suggests that collusion is unlikely to be driving increased payments reported in surveys (Appendix Table A.5).[27]

4.1.3 Program Access

Given that Smartcards appear to have curtailed corruption, one important question is whether they unintentionally reduced beneficiaries' *access* to the programs. While we find a reduction in leakage as intended, the worry is that if officials' rents are squeezed, the incentive to implement the program itself will be lower (Leff, 1964). Although in theory the NREGS guarantees employment at any time that a household wants it, in practice researchers have found that access to work is rationed (Dutta et al., 2012). In our data, 20% of control group households said that they had difficulty getting work on NREGS in May (slack labor demand), 42% had difficulty finding NREGS employment in January (peak labor demand), while only 3.5% said that anyone in their village can get work on NREGS whenever they want. All these indicators of program accessibility improve after the Smartcards treatment, although only the coefficient on the last mentioned indicator is statistically significant at the 10% level (Table 7). These perceptions of increased access to work are borne out by basic results on the extensive margin: during our study period, households were 7.5 percentage points (18% of the control mean) more likely to work in treatment areas than in control areas (Table 7).

Moreover, we find no evidence of reported increases in the incidence of bribes paid to enroll in either program. Bribes paid to enroll in SSP for recent enrollees – those enrolled after Smartcards implementation began – were down by 5.5 percentage points (72% of the control mean), although this result is not statistically significant (Column 5, Table 5b). Bribes paid to access work on NREGS during the study period were also (statistically insignificantly) lower (Column 5, Table 5a).

4.1.4 Overall Perceptions

The results above suggest that Smartcards uniformly improved recipients' experience of the SSP and NREGS programs; all of our estimates point towards a better user experience.

[27]We also find no evidence of Hawthorne effects of the experiment or audits on survey respondents or officials (Appendix Table A.6). Our worksite audits were conducted in 5 randomly selected GPs out of the 6 surveyed within each mandal, and we find no difference in survey reports between the audited and unaudited GPs. In addition, we did audits in an additional randomly selected GP that had no survey, and we find no differences in audit outcomes between surveyed and non-surveyed GPs. Finally, using the full official data, we find no effect of either audits or surveys on official data outcomes (all results not reported but available on request).

Of course, it is possible that we missed impacts on other important dimensions of program performance that push in the other direction. We therefore also directly asked recipients who were exposed to the Smartcard program – including both beneficiaries who had received a Smartcard and used it to pick up wages, and also beneficiaries who had enrolled for, but not received, a physical Smartcard – in treated mandals to describe the pros and cons of the new payment process and state which system they preferred.

Table 8 summarizes the results. Some of our own ex-ante concerns are reflected, with many recipients stating that they fear losing their Smartcards (53% NREGS, 62% SSP) or having problems with the payment reader (49% NREGS, 59% SSP). Most beneficiaries do not trust the Smartcards system enough to deposit money in their accounts. Yet strong majorities also agree that Smartcards make payment collection easier, faster, and less manipulable. Overall, 84% of NREGS beneficiaries and 91% of SSP beneficiaries prefer Smartcards to the status quo, with only 8% of NREGS and 5% of SSP beneficiaries disagreeing, and the rest neutral. These numbers reinforce the view that Smartcards significantly improved program performance in delivering payments.

It is worth highlighting the importance of these numbers from a policy point of view. In practice, senior officials in the government were much more likely to hear about cases where the Smartcard system was not working well relative to positive reports of improved beneficiary satisfaction. The setting provides an excellent example of the political economy of concentrated costs to those made worse off by the program (including low and middle-level officials whose opportunities for graft were reduced) versus the diffuse benefits to millions of beneficiaries.

4.2 Heterogeneity

4.2.1 Heterogenous Treatment Effects

An important concern regarding the new payments system was the possibility of adverse distributional consequences even if mean effects were positive. For instance, it is possible that the most vulnerable beneficiaries face greater difficulty in enrolling for Smartcards or in authenticating their biometrics, and may as a result be worse off under the new system. We plot quantile treatment effects of key outcomes (time to collect payment, payment delays, official payments, and payments received as per the survey) and find that the treatment distribution first-order stochastically dominates the control distribution for the major outcomes that show a significant average treatment effect (Figure 6). This suggests that not only are the average effects positive, but that no treatment household is worse off relative to a control household at the same percentile in the outcome distribution.

We also test whether treatment effects on key outcomes (time to collect payment, payment delays, official payments, and survey payments) varied significantly as a function of baseline characteristics at the village level (β_3 in Equation 3.3). We first focus on heterogeneity as a function of the baseline value of the outcome variable. Other characteristics include measures of village-level affluence (consumption, land ownership and value), importance of NREGS to the village (days worked and amounts paid), and measures of socio-economic disadvantage (fraction of the population below the poverty line (BPL) and belonging to historically-disadvantaged scheduled castes (SC)). We find no significant heterogeneity of program impact along any of these characteristics (Table 9).

Most important of these is the lack of any differential impact of treatment as a function of the baseline values of each of the outcome variables (first row of Table 9), which suggests broad-based program impacts at all initial values of these outcomes. To see this more clearly, Figures A.1 and A.2 plot non-parametric treatment effects on each outcome by percentile of the baseline value of the same variable. We see that reductions in time to collect payments and payment delays took place at all percentiles of their baseline values. Official payments remain unchanged at all percentiles, and survey payments show an inverted-U pattern, with the highest increases in the intermediate range of baseline payments.[28]

4.2.2 Channels of Program Impact

To better understand the channels of impact, Table 10 presents a non-experimental decomposition of the total treatment effects (on all the key outcomes) between carded and uncarded GPs and also between beneficiaries in carded GPs who are with and without Smartcards. We see that for most of the outcomes, significant effects are found only in the carded GPs, suggesting that the carded payments were indeed the mechanism for the impacts we find. In addition, we find that uncarded beneficiaries in carded GPs benefit just as much as carded beneficiaries in these GPs for payment process outcomes such as time to collect payments and reduction in payment lags. While these are non-experimental decompositions, they provide suggestive evidence that converting a village to carded payments may have been the key mechanism by which there were improvements in the process of collecting payments, and also suggest that the implementation protocol followed by GoAP did not inconvenience uncarded beneficiaries in GPs that were converted to the new system. The lack of negative impacts for uncarded beneficiaries may be due to GoAPs inability to insist on carded payments for all beneficiaries (due to the political cost of denying payments to genuine beneficiaries). While permitting uncarded payments may have permitted some amount of leakage to continue even under

[28]It is important to note that heterogeneity in our setting could reflect variation in implementation intensity as well as heterogeneous impacts from uniform implementation. Since implementation of the Smartcard program was incomplete, we also plot the treatment intensity (fraction of carded payments at the GP level) below each non-parametric plot (panels (c) and (d) of Figures A.1 and A.2). Overall, it appears that implementation heterogeneity along observables was limited.

the new system, it was probably politically prudent to do so in the early stages of the implementation.

Accordingly, payments to carded *beneficiaries* (as opposed to carded GPs) seem to be the main driver of reductions in leakage, suggesting that biometric authentication was important for these outcomes. While increases in survey payments and reductions in leakage are also found only in carded GPs (columns 8 and 12), they appear to be concentrated on households with Smartcards. Note that the lower official and survey payments to uncarded beneficiaries in converted GPs could also simply reflect less active workers (who will be paid less) being less likely to have enrolled for the Smartcards.

As described earlier, the intervention consisted of both technological changes (biometric authentication) as well as operational changes (payment made in the village by a CSP) in the payments system and our main results represent the composite impact of these two changes. However, the non-experimental decomposition presented here suggests that converting a GP to the new payment system was the main contributor to the increased convenience in collecting payments, while the use of biometric Smartcards at the beneficiary level may have been the main contributor to reductions in leakage.

5 Cost-Effectiveness and Welfare Impacts

We organize our discussion of cost-effectiveness and welfare impacts into two categories: pure efficiency gains and redistribution. The former includes the reduction in time taken to collect payment, and the reduction in the variability of the lag between completing NREGS work and getting paid for it. The latter include the shorter payment lags (which move the cash value of the interest "float" from banks to beneficiaries), and reduced leakage (which move funds from corrupt officials to beneficiaries).

We estimate the value of time saved in collecting payments conservatively using reported agricultural wages during June, when they are relatively low. Using June wages of Rs. 130/day and assuming a 6.5 hour work-day (estimates of the length of the agricultural work day range from 5 to 8 hours/day), we estimate the value of time at Rs. 20/hour. Since the treated areas saw a reduction in time cost of 21 minutes per payment collected (Table 4), we estimate the value of time saved at Rs 7 per payment collected. To calculate the cost of the program, we use the 2% commissions the government pays to banks (because this is supposed to cover all costs of the banks and TSPs for running the program). This overstates the *change* in costs because it treats the costs of running the status-quo delivery mechanism as zero.[29] We assume that recipients collect payments once per spell of work, which is consistent with the fact (presented earlier) that they

[29]However, we do not include estimates for the cost of time of officials in implementing and overseeing the Smartcard program because they will have had to exercise oversight of the older system as well.

do not keep balances on their Smartcards accounts. Using administrative data on all NREGS payments in our study districts in 2012, we calculate the average payment as Rs. 502, and thus estimate that GoAP would pay 2% of this or Rs. 10 per payment collected. Since commissions are only paid when GPs are converted, we scale this estimate down by two-thirds (to account for the fact that only two-thirds of GPs were converted during the study period) and estimate the cost per payment at Rs. 6.7.[30]

We conduct a similar exercise for the SSP payments, and compare the costs of program implementation with the benefits of time saved in Figure 7. We find that the value of saved time is roughly equal to the government's costs in the case of NREGS payments, and we cannot reject that they are equal in the case of the SSP payments (though the point estimate in this case is also not significant). Scaling up by the size of the two programs in the 8 study districts, we estimate the cost of the new payment system at $4.25 million for NREGS ($1.85 million SSP), and the value of time savings at $4.44 million for NREGS ($0.32 million for SSP). Further, the reduction in the variability of the lag to payment is likely to unambiguously benefit workers (though we do not attempt to quantify these gains here).

The shorter payment lag moves the float from banks to beneficiaries. We assume that the value of the float to banks is 5% per year (mean savings account interest rates) and to poor workers is 26% per year (benchmark interest rates for micro-finance loans, which are the most common form of credit in rural AP). We use our estimated reduction in payment lag (10 days, Table 4), and scale up by the total volume of NREGS payments in the eight study districts, and estimate the annual cost to banks at $0.43 million and the annual gains to beneficiaries at $2.25 million. We multiply the estimated reduction in leakage of 12.2% by the total annual outlay of NREGS in the eight study districts, in order to estimate an annual reduction in leakage of $38.7 million. The reduction of 1.1% in ghost beneficiaries in the SSP program scaled up by the volume of the SSP payments would translate into an annual reduction in excess payments of $1.3 million.

Since all of these effects are redistributive, we cannot estimate welfare gains without taking a view on the relative weights on the utility of winners and losers. However, since the redistribution is likely to be moving income from the rich to the poor in all these cases, the gains to social utility should be positive under a utilitarian social welfare function with equal weights on the utility of all citizens and concave individual utility functions. To the extent that the aim of the NREGS is explicitly redistributive, the gains in social utility are likely to be even larger if we put greater weight on the welfare of the poor

[30]Note that our estimated treatment effects are ITT effects and are based on converting only two-thirds of GPs. Since Table 10 suggests that the program impacts are driven by converting GPs to the new system, an alternative approach would be to use the randomization as an instrument to generate IV estimates of the impact of being a carded GP. However, this will simply scale up both the benefit and cost estimates linearly by a factor of 3/2. We prefer the ITT approach because it does not require satisfying an additional exclusion restriction.

than the rich. Finally, if taxpayers and the social planner place a zero weight on the utility loss to corrupt officials from being less able to siphon funds meant for the poor (because these are “illegitimate” earnings), then the welfare gains from reduced leakage are unambiguously positive and substantively large.

6 Conclusion

The value of improved payments infrastructure is an important open policy question in developing countries. New technologies promise to relax constraints in both the public and private sectors, but there are myriad reasons to be skeptical that this promise can be achieved. We examine this question empirically, conducting a large-scale, randomized, as-is evaluation of a new payment system built on biometric authentication and electronic benefit transfer introduced into two major social programs in Andhra Pradesh, India.

We find that, despite substantial implementation challenges which limited conversion to just over 50% of transactions, the poor gained significantly from the reform. Beneficiaries receive payments faster and more reliably, spend less time collecting payments, receive a higher proportion of benefits, and pay less in bribes. These gains do not come at the expense of program access, which if anything appears to improve slightly. Neither do they come at the expense of the most vulnerable beneficiaries, as we see little heterogeneity by baseline characteristics and treatment distributions that stochastically dominate those in control. Finally, beneficiaries themselves overwhelmingly report preferring the new payment system to the old. These results, and conservative cost-benefit calculations using them, suggest that improved payments technology can more than justify their costs in rural parts of developing countries.

As with any evaluation our results are valid only through a point in time, and we can only make intelligent guesses as to how outcomes will subsequently evolve. On the one hand, benefits could deteriorate if interested groups gradually find ways to subvert or capture the new systems installed. On the other hand, benefits could increase if and when the government is able to increase coverage and plug remaining loopholes. Perhaps most importantly, our results describe the value of Smartcards for public programs but do not capture their potential value as public infrastructure on which private-sector activity can be built. For example, recipients who now use Smartcards to collect government transfers may in future also use them to save, borrow, purchase insurance, or send and receive private transfers. Thus, our estimates (which only quantify the gains from the new system in two public programs) are likely to be a lower bound on the potential long-term returns to investing in modern secure payments infrastructure.

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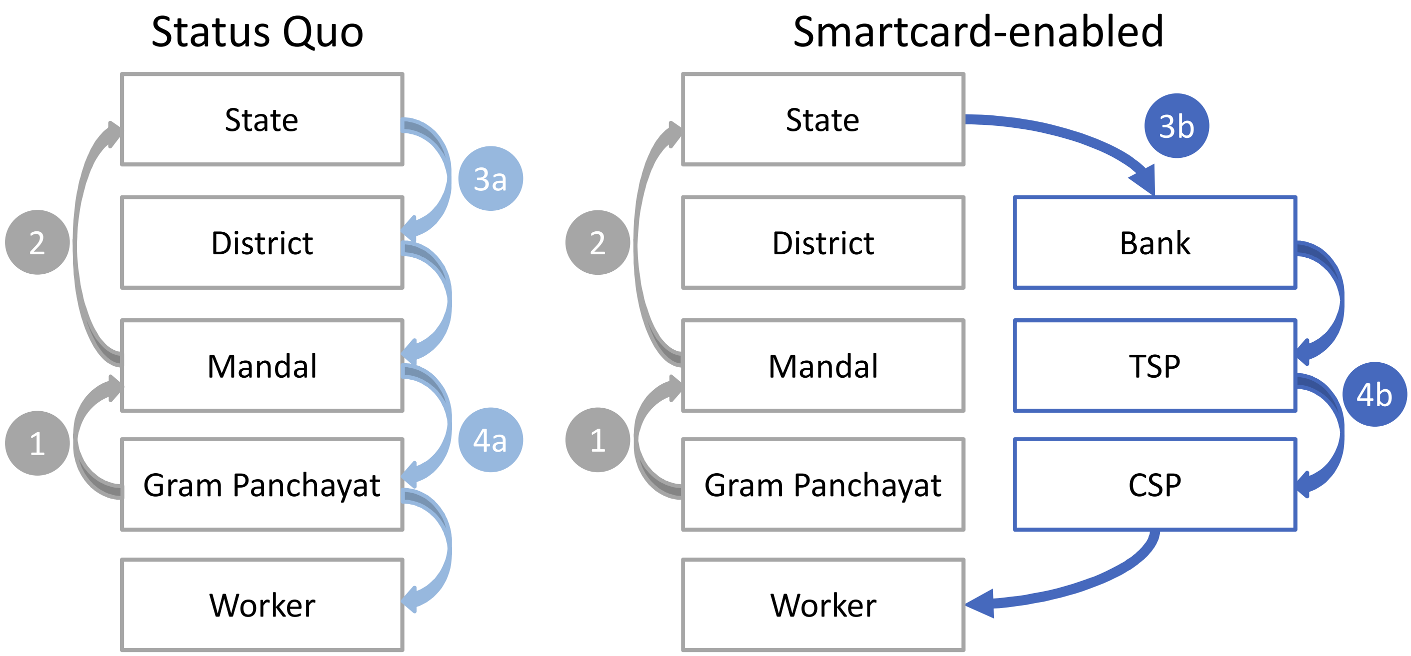


Figure 1: Comparison of treatment and control payment systems

"TSP" is a Technology Service Provider, a firm contracted by the bank to handle details of electronic transfers. "CSP" is a Customer Service Provider, from whom beneficiaries receive cash payments after authentication. In both systems, (1) paper muster rolls are maintained by the GP and sent to the mandal computer center, and (2) the digitized muster roll data is sent to the state financial system. In the status quo model, (3a) the money is transferred electronically from state to district to mandal, and (4a) the paper money is delivered to the GP (typically via post office) and then to the workers. In the Smartcard-enabled system, (3b) the money is transferred electronically from the state to the bank, to the TSP, and finally to the CSP, and (4b) the CSP delivers the cash and receipts to authenticated recipients.

(a) Sample Smartcard

(b) Point-of-Service device

Figure 2: The technology

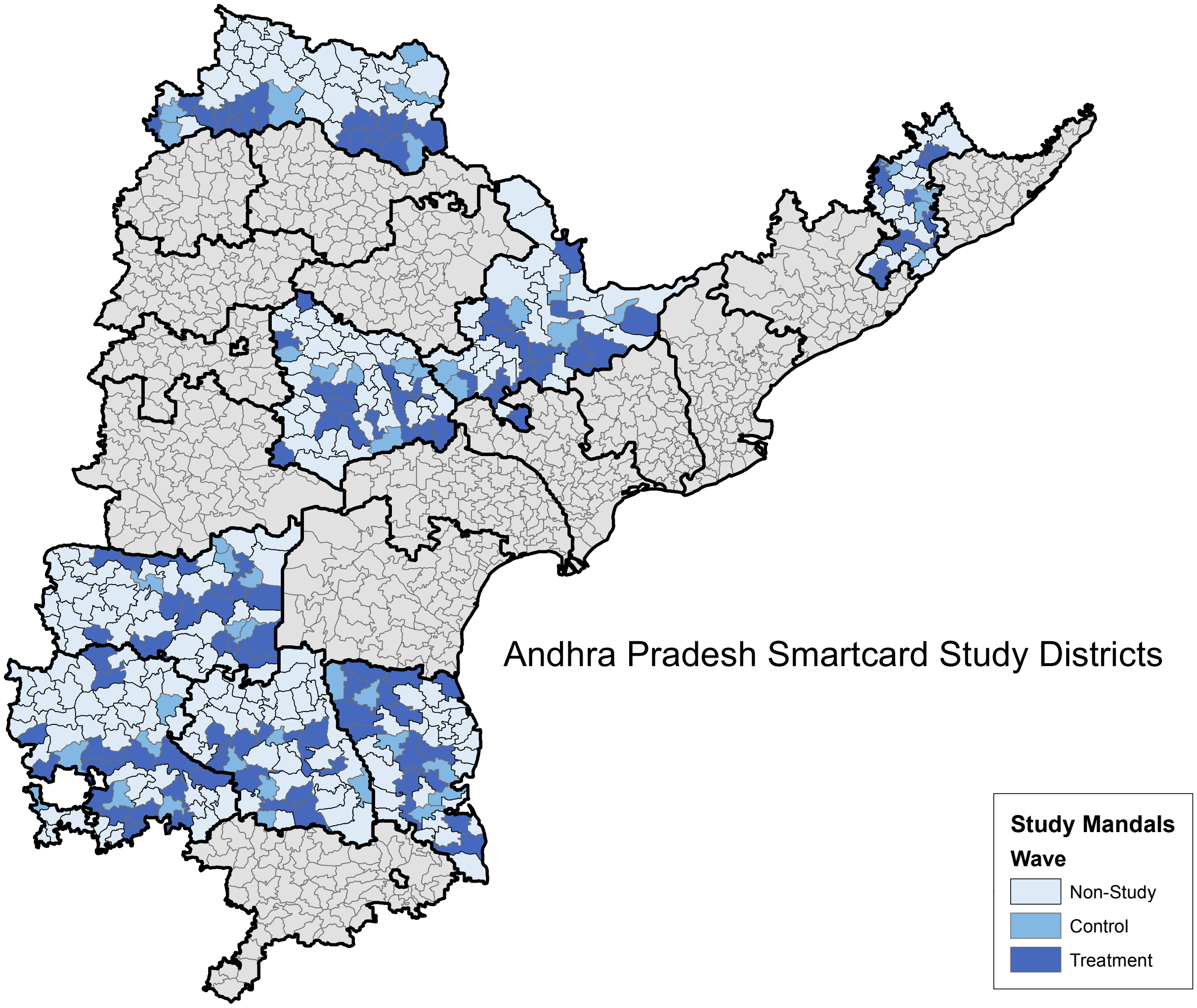


Figure 3: Study districts with treatment and control mandals

This map shows the 8 study districts and the assignment of mandals (sub-districts) to treatment and control groups. Mandals were randomly assigned to one of three waves: 113 to wave 1, 195 to wave 2, and 45 to wave 3. Wave 2 was created as a buffer to maximize the time between program rollout in treatment and control waves; our study does not use data from these mandals. Randomization was stratified by revenue division (an administrative unit between the district and mandal) and by a principal component of mandal characteristics including population, literacy, Scheduled Caste and Tribe proportion, NREGS jobcards, NREGS peak employment rate, proportion of SSP disability recipients, and proportion of other SSP pension recipients.

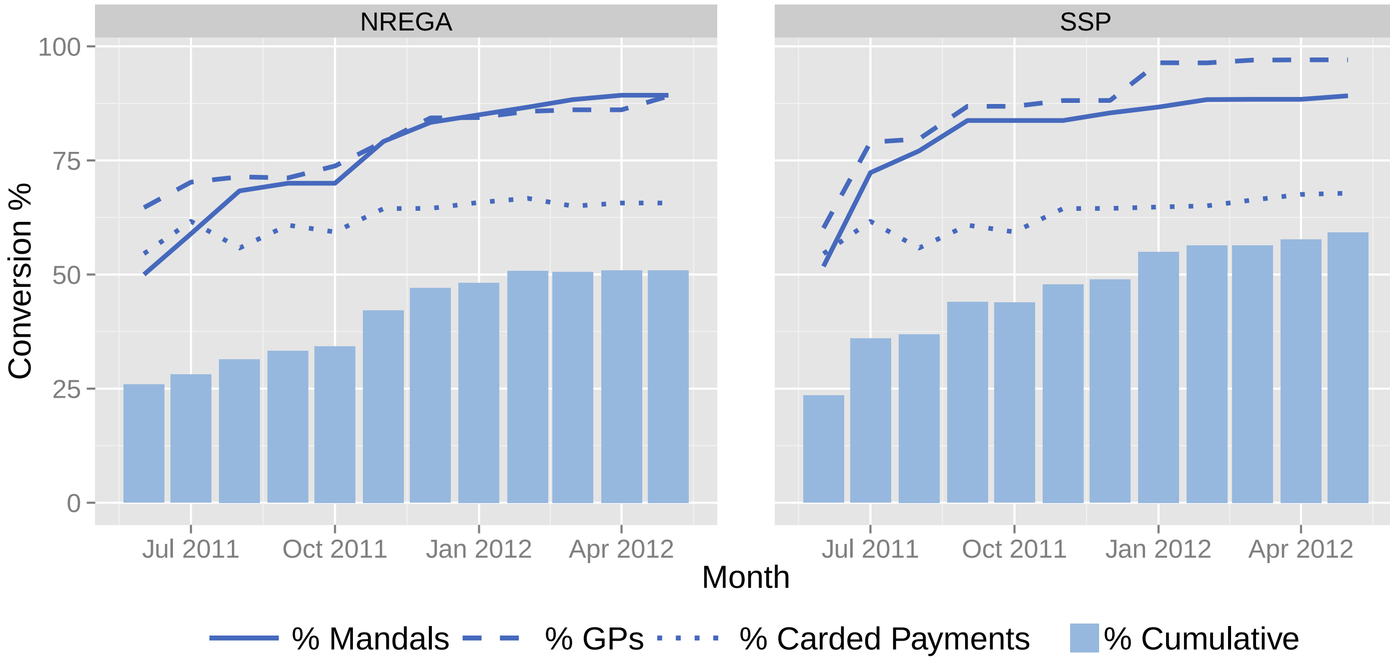


Figure 4: Rollout of Smartcard integration with welfare programs

This figure shows program rollout in aggregate and at different conversion levels. A GP converts to the Smartcard-enabled system based on beneficiary enrollment in the program. "% Mandals" is the percentage of mandals converted in a district. A mandal converts when at least one GP in the mandal converts. "% GPs" is the percentage of converted GPs in converted mandals. "% Carded Payments" is the percentage of payments in carded GPs transacted with carded beneficiaries. Multiplying these variables produces the cumulative program rollout.

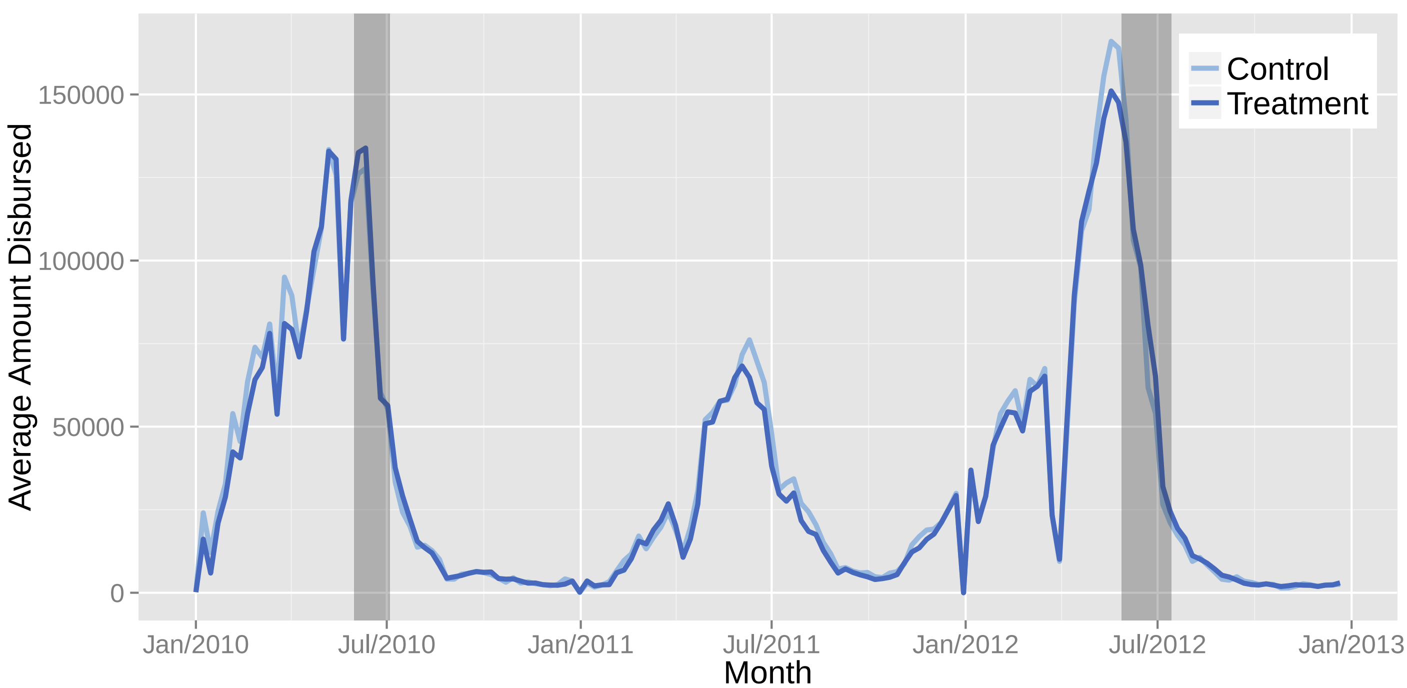
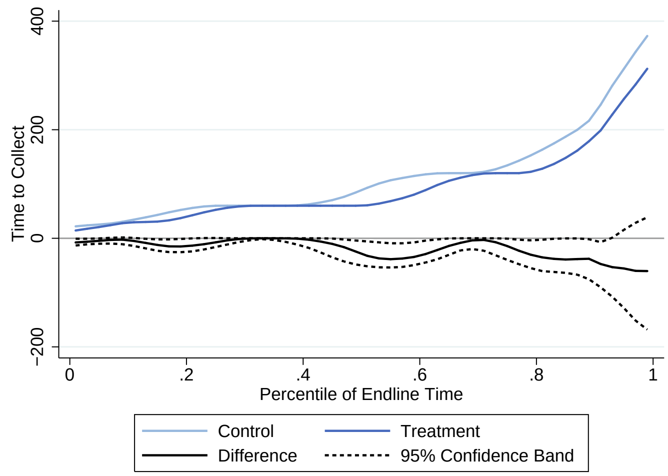
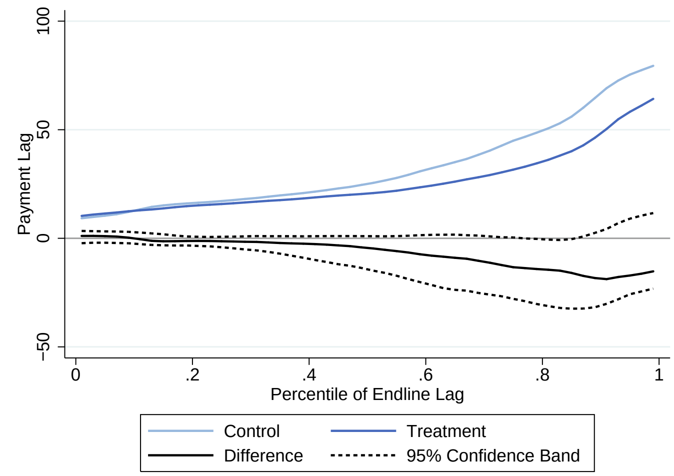


Figure 5: Official disbursement trends in NREGS

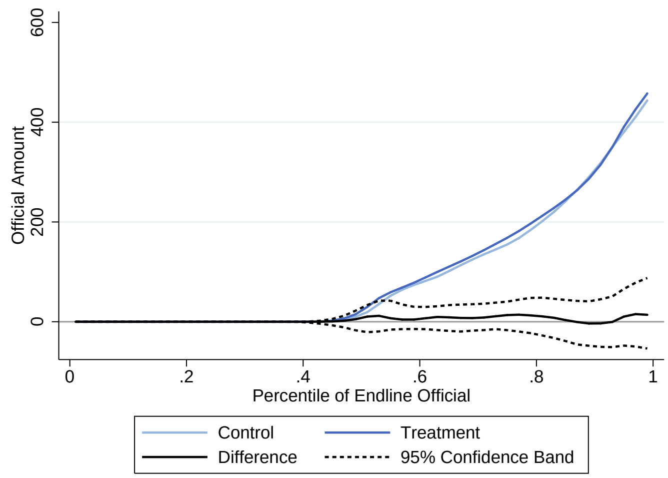
This figure shows weekly official NREGS payments for all workers averaged at the GP-day level for treatment and control areas. The grey shaded bands denote the study periods on which our survey questions focus (baseline in 2010 - May 31 to July 4; endline in 2012 - May 28 to July 15).



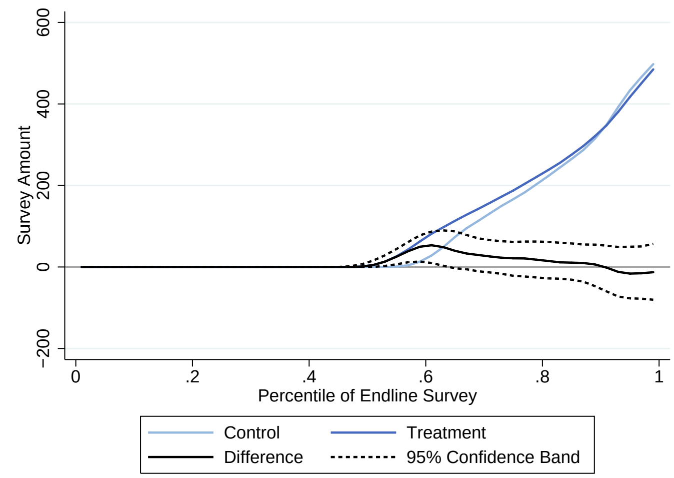
(a) Time to collect



(b) Payment Lag



(c) Official



(d) Survey

Figure 6: Quantile Treatment Effects on Key Outcomes

Panels (a)-(d) show nonparametric treatment effects. “Time to collect” is the average time taken to collect a payment, including the time spent on unsuccessful trips to payment sites. “Payment Lag” is the average lag (in days) between work done and payment received under NREGS. “Official” refers to NREGS payment amounts paid as listed in official muster records, while “survey” refers to payments received as reported by beneficiaries, both during study periods (baseline in 2010 - May 31 to July 4; endline in 2012 - May 28 to July 15). All lines are fit by a kernel-weighted local polynomial smoothing function with Epanechnikov kernel and probability weights, with standard errors bootstrapped.

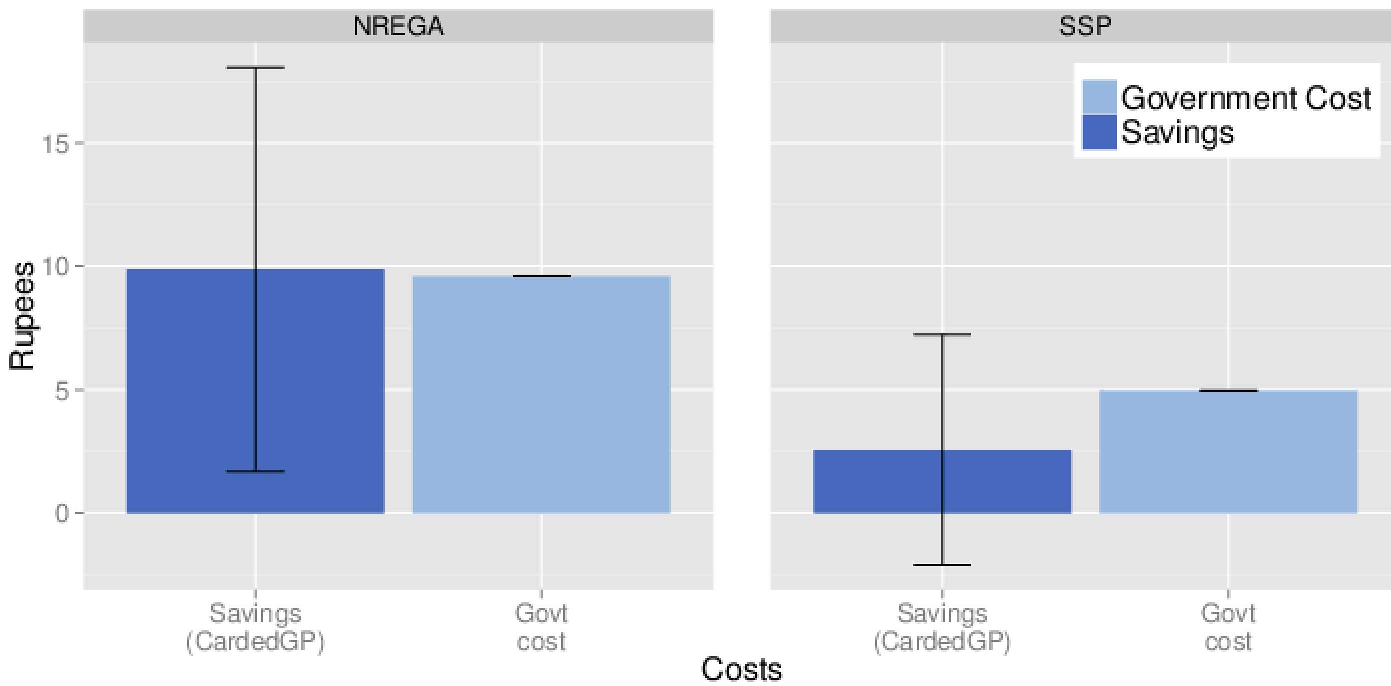


Figure 7: Time savings in collecting payments

This figure shows time savings quantified as the opportunity cost of a beneficiary's time to collect compared against the government costs for an average payment. "Savings" is calculated as the worker's private market wage during the time they need to collect a payment. The time includes time spent in line, making extra trips, etc. The "government cost", paid to the banks, is 2% of the payment amount, scaled down to payments made only in carded GPs. The statistic is calculated here from the average payment amount per workspell. The error bars represent a 95% CI. Standard errors clustered at mandal level.

Table 1: Balance on baseline characteristics: Official records

	Treatment	Control	Difference	p-value
Population	43734	43578	155	.94
Pensions per capita	.12	.12	.0013	.79
Jobcards per capita	.55	.55	-.0063	.84
Literacy rate	.45	.45	.0039	.74
% SC	.19	.19	.003	.81
% ST	.19	.19	.003	.81
% population working	.51	.51	.00018	.88
% male	.51	.51	.00018	.88
% old age pensions	.48	.49	-.0095	.83
% weaver pensions	.009	.011	-.0015	.71
% disabled pensions	.1	.1	.0021	.83
% widow pensions	.21	.2	.014	.48

This table presents outcome means from official data on mandal characteristics. Column 3 reports the difference in treatment and control means, while column 4 reports the p-value on the treatment indicator, both from simple regressions of the outcome with district fixed effects as the only controls. A "jobcard" is a household level official enrollment document for the NREGS program. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Constitution. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2: Balance on baseline characteristics: Household survey

	NREGS				SSP			
	Treatment	Control	Difference	p-value	Treatment	Control	Difference	p-value
HHD members	4.8	4.8	.02	.9	4.1	4.2	-.15	.4
BPL	.98	.98	.0042	.73	.98	.97	.0039	.65
Scheduled caste	.22	.25	-.027	.34	.19	.23	-.036*	.092
Scheduled tribe	.12	.11	.0061	.83	.096	.12	-.023	.45
Literacy	.42	.42	.0015	.93	.38	.39	-.013	.4
Annual income	41447	42791	-1387	.49	33554	35279	-2186	.31
Annual consumption	104607	95281	8543	.4	74602	77148	-3445	.55
Pay to work/enroll	.01	.0095	.0009	.83	.054	.07	-.016	.24
Pay to collect	.058	.036	.023	.14	.059	.072	-.008	.81
Ghost HHD	.031	.017	.014	.12	.012	.0096	.0018	.76
Time to collect	157	169	-7.3	.63	94	112	-18**	.027
Average Payment Delay	29	23	.22	.93				
Payment delay deviation	11	8.8	-.42	.77				
Official amount	167	159	12	.51				
Survey amount	171	185	-12	.56				
Leakage	-4.4	-26	25	.15				
NREGS availability	.47	.56	-.1**	.02				
HHD doing NREGS work	.41	.41	.0021	.95				

This table presents outcome means from the household survey. Columns 3 and 6 report the difference in treatment and control means, while columns 4 and 8 report the p-value on the treatment indicator, all from simple regressions of the outcome with district fixed effects as the only controls. "BPL" is an indicator for households below the poverty line. "Pay to work/enroll" refers to bribes paid in order to obtain NREGS work or to start receiving SSP pension. "Pay to Collect" refers to bribes paid in order to receive payments. "Ghost HHD" is a household with a beneficiary who does not exist (confirmed by three neighbors) but is listed as receiving payment on official records. "Time to Collect" is the time taken on average to collect a benefit payment, including the time spent on unsuccessful trips to payment sites, in minutes. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3: Treatment increased official and self-reported use of Smartcards

(a) NREGS

	Official data (%)		Survey data (%)	
	(1)	(2)	(3)	(4)
	Carded GP	Mean fraction carded pmts	Pmts generally carded (village mean)	Most recent pmt carded (village mean)
Treatment	.67***	.51***	.38***	.38***
	(.045)	(.04)	(.043)	(.043)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.45	.49	.37	.36
Control Mean	.0046	.0028	.039	.013
N. of cases	886	886	824	824
Level	GP	GP	GP	GP

(b) SSP

	Official data (%)		Survey data (%)	
	(1)	(2)	(3)	(4)
	Carded GP	Mean fraction carded pmts	Pmts generally carded (village mean)	Most recent pmt carded (village mean)
Treatment	.79***	.59***	.46***	.46***
	(.041)	(.037)	(.05)	(.048)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.57	.55	.38	.38
Control Mean	0	0	.07	.043
N. of cases	886	886	882	882
Level	GP	GP	GP	GP

This table analyzes usage of Smartcards for NREGS and SSP payments as of July 2012. Each observation is a gram panchayat ("GP": administrative village). "Carded GP" is a gram panchayat that has moved to Smartcard-based payment, which happens once 40% of beneficiaries have been issued a card. "Mean fraction carded pmts" is the proportion of transactions done with carded beneficiaries in carded GPs. Both these outcomes are from official data. Columns 3 and 4 report survey-based measures of average beneficiary use of Smartcards or a biometric-based payment system in the GP. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4: Payments are easier to access

	Time to Collect (Min)				Pmt Lag (Days)			
	(1) NREGS	(2) NREGS	(3) SSP	(4) SSP	(5) Average	(6) Average	(7) Deviation	(8) Deviation
Treatment	-21**	-21**	-5.9	-2.6	-7.1*	-10***	-2.9***	-4.7***
	(9.3)	(8.7)	(5.5)	(5.9)	(3.8)	(3.6)	(1.1)	(1.5)
Carded GP								
BL GP Mean		.08*		.23***		-.027		.043
		(.041)		(.073)		(.09)		(.054)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week Fe	No	No	No	No	Yes	Yes	Yes	Yes
Adj R-squared	.06	.08	.07	.11	.14	.31	.07	.17
Control Mean	112	112	77	77	34	34	12	12
N. of cases	10252	10181	3805	3591	14279	7254	14279	7254
Level	Indiv.	Indiv.	Indiv.	Indiv.	Indiv-Week	Indiv-Week	Indiv-Week	Indiv-Week
Survey	NREGS	NREGS	SSP	SSP	NREGS	NREGS	NREGS	NREGS

The dependent variable in columns 1-4 is the average time taken to collect a payment (in minutes), including the time spent on unsuccessful trips to payment sites, with observations at the beneficiary level. The dependent variable in columns 5-6 is the average lag (in days) between work done and payment received on NREGS, while columns 7-8 report results for absolute deviations from the median mandal lag. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 5: Corruption: bribes, ghosts, and awareness

(a) NREGS

	Ghost households (village mean %)		Bribe to work (%)				Bribe to collect (%)	
	(1)	(2)	(3) All	(4) All	(5) Recent	(6) Recent	(7)	(8)
Treatment	-.011	-.011	-.0026	-.0027	-.00016	-.00038	-.0021	-.0028
	(.02)	(.021)	(.0028)	(.0029)	(.0015)	(.0015)	(.0088)	(.0092)
BL GP Mean		-.0081		-.0066		-.0056**		.014
		(.067)		(.0041)		(.0027)		(.018)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.02	.02	.00	.00	.00	.00	.01	.01
Control Mean	.11	.11	.0047	.0047	.0022	.0022	.021	.021
N. of cases	5314	5278	10540	10469	7232	6908	10437	10366
Level	HHD	HHD	Indiv.	Indiv.	Indiv.	Indiv.	Indiv.	Indiv.

(b) SSP

	Ghost households (village mean %)		Bribe to enroll (%)				Bribe to collect (%)	
	(1)	(2)	(3) All	(4) All	(5) Recent	(6) Recent	(7)	(8)
Treatment	-.0096	-.0091	.0017	.0016	-.047	-.055	-.018	-.018*
	(.011)	(.011)	(.0074)	(.0075)	(.031)	(.039)	(.013)	(.011)
BL GP Mean		.22		.027		.025		.17***
		(.14)		(.021)		(.045)		(.043)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.01	.01	.01	.01	.05	.05	.06	.10
Control Mean	.063	.063	.019	.019	.076	.076	.038	.038
N. of cases	3512	3310	3802	3587	586	354	3801	3591
Level	HHD	HHD	Indiv.	Indiv.	Indiv.	Indiv.	Indiv.	Indiv.

"Ghost households" refer to households (or all beneficiaries within households) that were confirmed not to exist, or who had permanently migrated before the study period started on May 28, 2012 (May 31, 2010 for baseline). "Bribe to work" refer to bribes paid in order to obtain NREGS work, "bribe to collect" refer to bribes paid in order to receive payments on either SSP or NREGS, while "bribes to enroll" refer to bribes paid in order to get on the SSP beneficiary list. "Recent" in table (a) refers to the 2012 endline study period (May 28-July 15), while in table (b) it refers to 2011-2012. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 6: Weekly NREGS earnings increased, leakage reduced

	Official		Survey		Leakage	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	9.8	7.5	35**	35**	-25*	-27**
	(12)	(12)	(15)	(15)	(13)	(13)
BL GP Mean		.12***		.11***		.089**
		(.027)		(.037)		(.038)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.05	.05	.06	.03	.04
Control Mean	127	127	146	146	-20	-20
N. of cases	5180	5144	5180	5144	5180	5144

Each observation refers to household-level average weekly amounts for NREGS work done during the study period (baseline in 2010 - May 31 to July 4; endline in 2012 - May 28 to July 15). The regressions include all sampled beneficiaries who were a) found by survey team to match official record or b) listed in official records but confirmed as "ghost" beneficiary as described in Table 5. "Official" refers to amounts paid as listed in official muster records. "Survey" refers to payments received as reported by beneficiaries. "Leakage" is the difference between these two amounts. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7: Access to NREGS work marginally improved

	Proportion of HHDs doing NREGS work (%)	Do you have difficulty finding NREGS work in... (%)		Desired increase in NREGS work (days)	Is NREGS work available when anyone wants it (%)	
	(1) Study Period	(2) May	(3) January	(4) May	(5) General	(6) General
Treatment	.075**	-.025	-.031	-.93	.026*	.023
	(.033)	(.027)	(.033)	(1.3)	(.015)	(.015)
BL GP Mean						-.023
						(.027)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.05	.10	.10	.08	.02	.02
Control Mean	.42	.2	.42	6.6	.035	.035
N. of cases	4978	4783	4531	4978	4790	4750

This table analyzes household level access to NREGS. Column 1 reports the proportion of households doing work in the 2012 endline study period (May 28-July 15). "Difficulty finding NREGS work" denotes whether any member of household was unable to get work during May (slack labor demand) or January (peak labor demand). "Desired increase in NREGS work" is the number of extra days of work members of the household wanted on NREGS in May. "Is NREGS work available" is an indicator for whether the respondent believes anyone in the village who wants NREGS work can get it at any time. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 8: Beneficiary opinions of Smartcards

	NREGS				SSP			
	Agree	Disagree	Neutral/ Don't know	N	Agree	Disagree	Neutral/ Don't know	N
Positives:								
Smartcards increase speed of payments (less wait times)	.78	.03	.19	4891	.82	.06	.12	2245
With a Smartcard, I make fewer trips to receive my payments	.67	.04	.29	4868	.74	.04	.22	2242
I have a better chance of getting the money I am owed by using a Smartcard	.72	.02	.26	4867	.78	.03	.19	2242
Because I use a Smartcard, no one can collect a payment on my behalf	.72	.04	.24	4865	.76	.05	.20	2238
Negatives:								
It was difficult to enroll to obtain a Smartcard	.21	.60	.18	4877	.31	.55	.13	2244
I'm afraid of losing my Smartcard and being denied payment	.53	.15	.32	4720	.62	.14	.24	2172
When I go to collect a payment, I am afraid that the payment reader will not work	.49	.15	.36	4721	.59	.16	.25	2171
I would trust the Smartcard system enough to deposit money in my Smartcard account	.27	.33	.40	4868	.28	.39	.33	2240
Overall:								
Do you prefer the smartcards over the old system of payments?	.84	.08	.08	5072	.91	.05	.04	2280

This table analyzes beneficiaries' perceptions of the Smartcard program in treatment areas. Beneficiaries include household members that worked in the NREGS program or received SSP benefits. These questions were asked when beneficiaries had received a Smartcard and used it to pick up wages; and also if they had enrolled for, but not received, a physical Smartcard.

Table 9: No significant heterogeneity by baseline characteristics

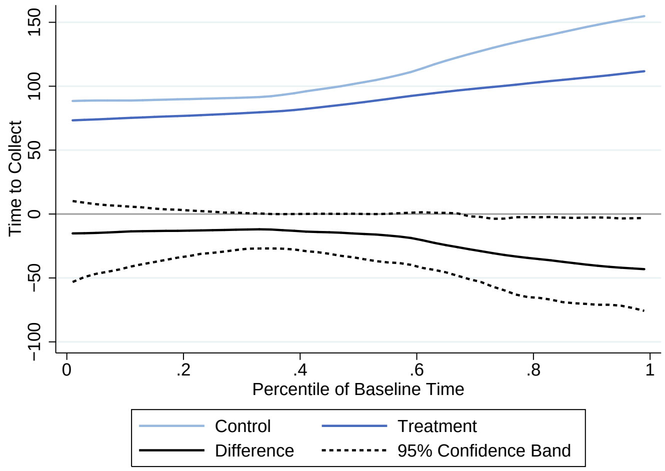
	Time to Collect	Official Payments	Survey Payments	Payment lag
	(1)	(2)	(3)	(4)
BL GP Mean	.025	.00013	.018	.16
	(.08)	(.044)	(.066)	(.25)
Consumption (Rs. 1,000)	-.087	.029	.038	-.01
	(.16)	(.076)	(.098)	(.027)
Land Value (Rs. 1,000)	.0037	.0032	.0024	-.0022
	(.0038)	(.0083)	(.011)	(.0035)
Own Land	3	-6.7	10	7.3
	(22)	(13)	(16)	(6.2)
NREGS GP Amount (Rs. 1,000)	.015**	.0043	.0016	-.00027
	(.0073)	(.0037)	(.0066)	(.0013)
NREGS GP Days Worked	.0011	.00036	.00029	.000089
	(.00073)	(.00035)	(.00055)	(.00014)
SC Proportion	.6	-11	-8.5	22
	(48)	(20)	(21)	(14)
BPL Proportion	-65	6.1	-29	-29
	(130)	(57)	(58)	(24)
District FE	Yes	Yes	Yes	Yes
Week Fe	No	No	No	Yes
Control Mean	112	55	63	34
Level	Indiv.	HHD	HHD	Indiv-Week

This table shows heterogenous effects on major endline outcomes from GP-level baseline characteristics. Each cell shows the coefficient on the baseline outcome interacted with the treatment indicator in separate regressions. "Consumption (Rs. 1,000)" is annualized consumption. "Land Value (Rs. 1,000)" is total land value. "Own Land" is a binary variable of whether household owns land. "NREGS GP Amount (Rs. 1000)" is total NREGS payment amounts for the period Jan 1, 2010 to July 22, 2010. "NREGS GP Days Worked (Rs. 1000)" is total NREGS days worked for the period Jan 1, 2010 to July 22, 2010. "SC Proportion" is the proportion of NREGS workspells performed by schedule caste workers in the period from Jan 1, 2010 to July 22, 2010. "BPL Proportion" is the proportion of households with a BPL card in the NREGS baseline survey. Total income is trimmed at the top .5% level in both treatment and control areas. Standard errors are clustered at the mandal level. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

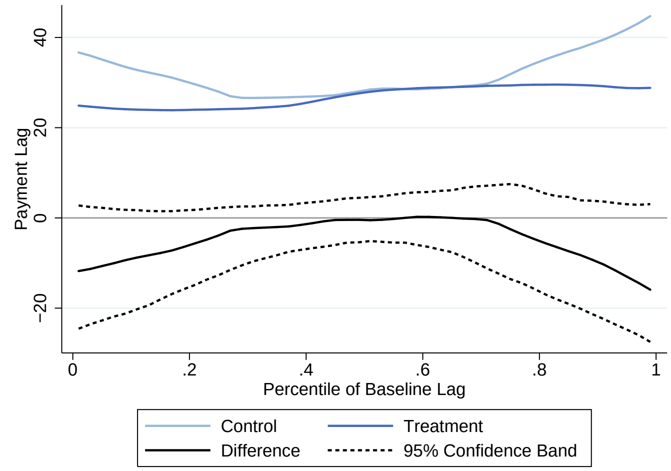
Table 10: Non-experimental decomposition of treatment effects by carded status

	Time to collect		Official		Survey		Leakage		Payment lag	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Carded GP	-33***		7.9		39**		-31**		-6.7**	
	(8.2)		(13)		(15)		(13)		(3.2)	
Have Scard, Carded GP		-33***		70***		136***		-66***		-4
		(8.5)		(16)		(23)		(22)		(2.5)
No Scard, Carded GP		-32***		-40***		-28		-15		-3.1
		(8.6)		(14)		(18)		(16)		(2.3)
Not Carded GP	5	4.9	7.5	19	22	46	-15	-26	-7.9	-6.7
	(13)	(13)	(16)	(21)	(22)	(28)	(19)	(24)	(5.4)	(5.2)
BL GP Mean	.071*	.071*	.28***	.2**	.25***	.1	.22***	.28***		
	(.039)	(.039)	(.066)	(.085)	(.088)	(.1)	(.078)	(.094)		
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week Fe	No	No	No	No	No	No	No	No	Yes	Yes
Adj R-squared	.10	.10	.04	.06	.06	.10	.04	.05	.14	.13
Control Mean	112	112	127	127	146	146	-20	-20	34	34
N. of cases	10181	10147	5144	4714	5144	4714	5144	4714	14279	14256
Level	Indiv.	Indiv.	HHD	HHD	HHD	HHD	HHD	HHD	Indiv-Week	Indiv-Week

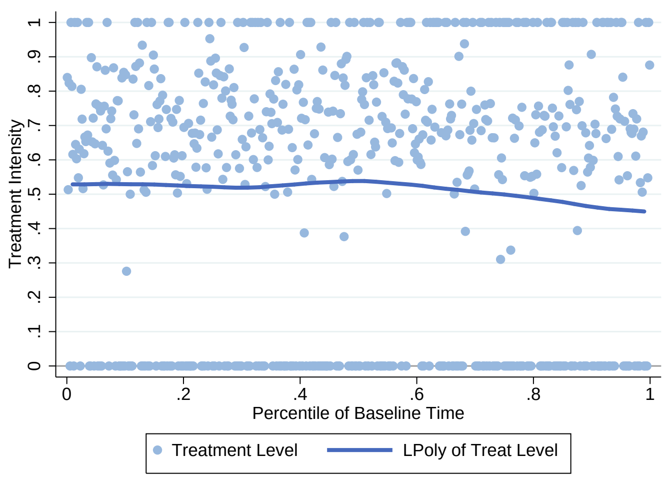
This table shows the main ITT effects decomposed by levels of program implementation. “Carded GP” is a gram panchayat that has moved to Smartcard based payments, which happens once 40% of beneficiaries have been issued a card. “Have SCard, Carded GP” and “No SCard, Carded GP” are based on whether the beneficiary or household lives in a carded GP and self-reported receiving a Smartcard (at least one Smartcard in the household for household-level variables). “Not Carded GP” is a gram panchayat in a treatment area that has not yet moved to Smartcard-based payments. A specification with the baseline mean is not reported for the payment lag outcome due to a large number of missing baseline observations, which makes decomposition difficult. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$



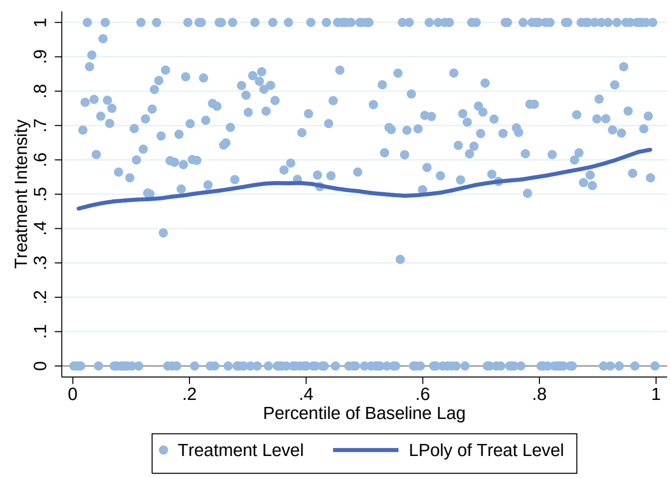
(a) Time to collect



(b) Payment Lag



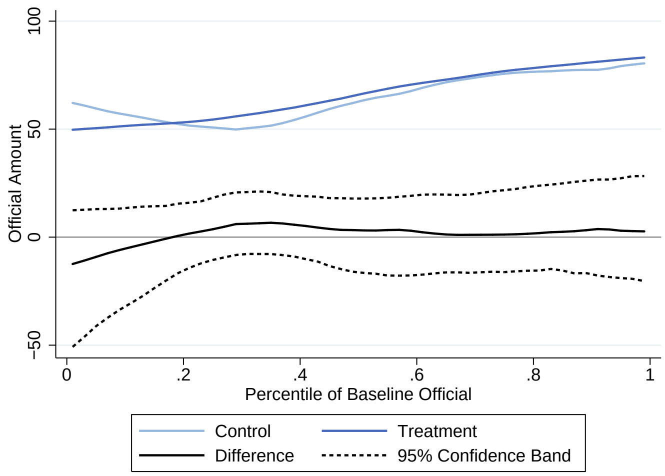
(c) Treatment Intensity - Time to collect



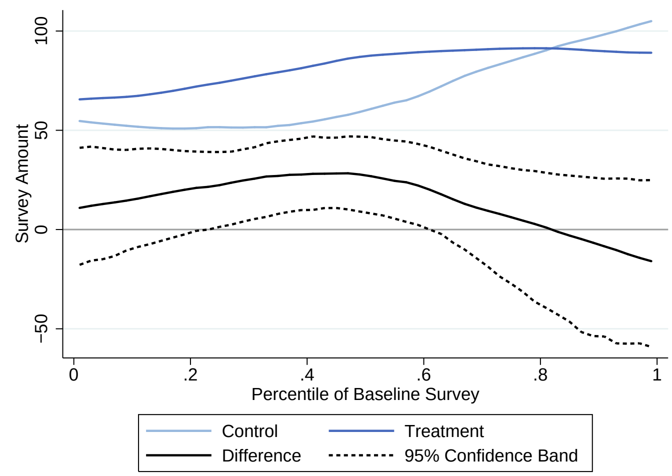
(d) Treatment Intensity - Payment Lag

Figure A.1: Heterogeneity in Time to collect and Payment Lags

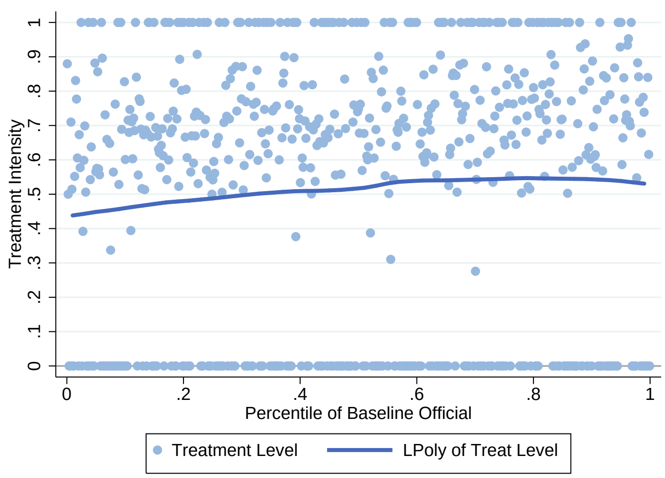
Panels (a) and (b) show nonparametric treatment effects by baseline percentile outcome. "Time to Collect" is the average time taken to collect a payment, including the time spent on unsuccessful trips to payment sites, in minutes. "Payment Lag" is the average lag (in days) between work done and payment received on NREGS. Panels (c) and (d) show how the intensity of treatment in treatment areas – measured as the proportion of endline transactions done with carded beneficiaries in carded GPs – varies by baseline percentile outcome. All lines are fit by a kernel-weighted local polynomial smoothing function with Epanechnikov kernel and probability weights, with standard errors bootstrapped.



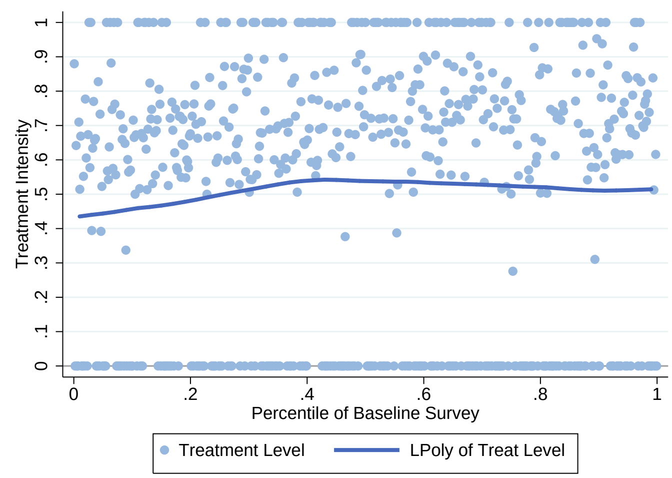
(a) Official



(b) Survey



(c) Treatment Intensity - Official



(d) Treatment Intensity - Survey

Figure A.2: Heterogeneity in Official and Survey Payments

Panels (a) and (b) show nonparametric treatment effects by baseline percentile outcomes. "Official" refers to amounts paid as listed in official muster records, while "survey" refers to payments received as reported by beneficiaries, both during study periods (baseline in 2010 - May 31 to July 4; endline in 2012 - May 28 to July 15). Panels (c) and (d) show how the intensity of treatment in treatment areas – measured as the proportion of endline transactions done with carded beneficiaries in carded GPs – varies by baseline percentile outcome. All lines are fit by a kernel-weighted local polynomial smoothing function with Epanechnikov kernel and probability weights, with standard errors bootstrapped.

Table A.1: No differential attrition

(a) NREGS

	(1) Attriters from Baseline	(2) Entrants in Endline
Control	.024	.059
Treatment	.013	.061
p-value	.22	.79

(b) SSP

	(1) Attriters from Baseline	(2) Entrants in Endline
Control	.097	.16
Treatment	.097	.17
p-value	.95	.36

These tables compare the entire NREGS sample frame – i.e., all jobcard holders – and the entire SSP beneficiary frame across treatment and control mandals. Column 1 presents the proportion of NREGS jobcards and SSP beneficiaries that dropped out of the sample frame after baseline, while column 2 presents the proportion that entered the sample frame at endline. Row 3 reports the p-value of the difference between treatment and control.

Table A.2: No compositional changes

(a) NREGS

	(1) N. of Members	(2) Hindu	(3) SC	(4) Any HHD Mem Reads	(5) BPL	(6) Total Consump	(7) Total Income	(8) Own Land
Treatment	.045	-.026	.023	-.031	-.0017	395	7010*	.06**
	(.11)	(.018)	(.022)	(.027)	(.022)	(4676)	(3772)	(.024)
El Entrants	-.16	.011	.029	.064	.067	-10734	-3259	-.054
	(.25)	(.047)	(.077)	(.049)	(.043)	(6852)	(10397)	(.12)
Treat*El Entrants	.14	-.029	-.077	-.089	-.05	4506	17303	.06
	(.34)	(.058)	(.089)	(.071)	(.058)	(9068)	(14190)	(.14)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.02	.07	.02	.01	.01	.01	.04	.01
Control Mean	4.3	.93	.19	.85	.89	90317	69708	.59
N. of cases	4944	4944	4944	4904	4922	4937	4910	4920
Level	HHD	HHD	HHD	HHD	HHD	HHD	HHD	HHD

(b) SSP

	(1) N. of Members	(2) Hindu	(3) SC	(4) Any HHD Mem Reads	(5) BPL	(6) Total Consump	(7) Total Income	(8) Own Land
Treatment	-.015	.019	-.025	-.048*	.0012	-1614	4459	.0043
	(.12)	(.021)	(.021)	(.027)	(.018)	(4001)	(4003)	(.032)
El Entrants	-.034	.0076	-.079**	-.017	.078***	-1573	-1422	.099*
	(.27)	(.042)	(.034)	(.044)	(.026)	(4029)	(4577)	(.056)
Treat*El Entrants	-.08	-.001	.049	.067	-.053	7490	5893	-.053
	(.3)	(.046)	(.04)	(.054)	(.033)	(5555)	(5666)	(.067)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.05	.04	.02	.02	.01	.02	.07	.02
Control Mean	3.5	.89	.21	.64	.87	63792	52763	.52
N. of cases	3168	3168	3168	3128	3147	3166	3153	3158
Level	HHD	HHD	HHD	HHD	HHD	HHD	HHD	HHD

These tables show that new entrants to the NREGS and SSP sample frames are no different across treatment and control groups. “EL Entrants” is an indicator for a household that entered the sample frame for the endline survey but was not in the baseline sample frame. “Treat*EL Entrants” is the interaction between the treatment indicator and the endline entrant indicator, and the coefficient of interest in these regressions. “N. of Members” is the number of household members. “Hindu” is an indicator for the household belonging to the hindu religion. “SC” is an indicator for the household belonging to a “Scheduled Caste” (historically discriminated-against caste). “Any HHD Mem Reads” is a proxy for literacy. “BPL” is an indicator for the household being below the poverty line. “Total Consump” is total consumption. “Own land” is an indicator for whether the household owns any land.

Table A.3: Baseline covariates do not predict program implementation

	Carded GP		Intensity	
	(1)	(2)	(3)	(4)
Time to Collect (1 hr)	-.015	-.018	.00066	-.0068
	(.018)	(.014)	(.013)	(.01)
Official Amount (Rs. 10,000)	-1	-.32	-.59	-.095
	(1.9)	(1.7)	(1.4)	(1.2)
Survey Amount (Rs. 10,000)	-1.4	-1	-.89	-.26
	(2.2)	(1.7)	(1.6)	(1.3)
Total Income (Rs. 10,000)	-.014	-.018**	.00048	-.0098
	(.013)	(.0086)	(.011)	(.0067)
District FE	No	Yes	No	Yes
Adj R-squared	.02	.26	.00	.39
N. of cases	631	631	631	631

This tables analyzes the effects of baseline covariate variability on endline program implementation in treatment areas. "Carded GP" is a gram panchayat that has moved to Smartcard based payment, which happens once 40% of beneficiaries have been issued a card. "Treatment intensity" is the proportion of transactions done with carded beneficiaries in carded GPs. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4: NREGS earnings: survey and official (scaled)

	Official		Survey		Leakage	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	9.6	2.5	33	32	-23	-28
	(25)	(24)	(21)	(20)	(21)	(20)
BL GP Mean		.16***		.1***		.13***
		(.025)		(.037)		(.033)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.05	.06	.07	.06	.07
Control Mean	260	260	180	180	80	80
N. of cases	5180	5144	5180	5144	5180	5144

Each observation refers to household-level average weekly amounts for NREGS work done during the study period (baseline in 2010 - May 31 to July 4; endline in 2012 - May 28 to July 15). The regressions include all sampled beneficiaries who were a) found by survey team to match official record or b) listed in official records but confirmed as "ghost" beneficiary as described in Table 5. "Official" refers to amounts paid as listed in official muster records, scaled by the average number of jobcards per household in the district. "Survey" refers to payments received as reported by beneficiaries. "Leakage" is the difference between these two amounts. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.5: Proportional increases in workers at worksite audits

	WSM		Survey		Official		Muster - WSM		Survey - WSM	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	12	10	69	43	26	17	14	6.7	57	33
	(12)	(10)	(49)	(36)	(27)	(20)	(20)	(17)	(44)	(35)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week Fe	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adj R-squared	.09	.13	.03	.09	.07	.16	.09	.14	.02	.07
Control Mean	28	28	173	173	108	108	81	81	145	145
N. of cases	513	513	513	513	513	513	513	513	513	513

Units represent estimated number of NREGS workers on a given day. The “WSM” numbers are the number of workers found in an independent audit of NREGA worksites in a GP. The “Survey” variable is the estimated number of workers we expected to find at these worksites from household survey data when we take into account the number of worksites in a GP and the average reported working hours for beneficiaries. “Official” is this same estimate for administrative data. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.6: No evidence of Hawthorne effects

	WSM	Official				Survey
	(1)	(2)	(3)	(4)	(5)	(6)
HHD Survey in GP	-3.4	10	-4.8			
	(8)	(34)	(33)			
WSM in GP		7.5	-13	3	-17	113
		(31)	(28)	(42)	(37)	(106)
WSM Survey in Week		-52	-71	-31	-39	34
		(51)	(52)	(39)	(39)	(84)
Recon Survey in Week		12	-.8	45	40	45
		(69)	(68)	(52)	(51)	(89)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Week Fe	Yes	Yes	Yes	Yes	Yes	Yes
BL GP Value	No	No	Yes	No	Yes	Yes
GP Size FE	No	Yes	Yes	No	No	No
Adj R-squared	.17					
Control Mean	48	758	758	755	755	1170
Level	Week	Week	Week	Week	Week	Week
Sample	WSM	All	All	HHD & WSM	HHD & WSM	HHD
N. of cases	682	52311	52311	7728	7728	6153

This table analyzes possible Hawthorne effects from the assorted treatment arms. Each cell represents a separate regression of the effect on the data source (column) from the survey type (row). Units are number of days worked in a GP per week. "HHD Survey in GP" is the being surveyed by the household survey. "WSM in GP" is being surveyed by the work site audit. Control mean represents the area with no survey. "WSM Survey in Week" is whether the work site audit happened in that specific week. "Recon Survey in week" is whether an enumerator to map the worksites in that specific week. Sample of "All" refers to all GPs in our study districts. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$