Building State Capacity: Evidence from Biometric Smartcards in India[†]

By Karthik Muralidharan, Paul Niehaus, and Sandip Sukhtankar*

Antipoverty programs in developing countries are often difficult to implement; in particular, many governments lack the capacity to deliver payments securely to targeted beneficiaries. We evaluate the impact of biometrically authenticated payments infrastructure ("Smartcards") on beneficiaries of employment (NREGS) and pension (SSP) programs in the Indian state of Andhra Pradesh, using a large-scale experiment that randomized the rollout of Smartcards over 157 subdistricts and 19 million people. We find that, while incompletely implemented, the new system delivered a faster, more predictable, and less corrupt NREGS payments process without adversely affecting program access. For each of these outcomes, treatment group distributions first-order stochastically dominated those of the control group. The investment was cost-effective, as time savings to NREGS beneficiaries alone were equal to the cost of the intervention, and there was also a significant reduction in the "leakage" of funds between the government and beneficiaries in both NREGS and SSP programs. Beneficiaries overwhelmingly preferred the new system for both programs. Overall, our results suggest that investing in secure payments infrastructure can significantly enhance "state capacity" to implement welfare programs in developing countries. (JEL H53, H55, I32, I38, J65)

Developing countries spend billions of dollars annually on antipoverty programs, but the delivery of these programs is often poor and plagued by high levels of corruption (World Bank 2003; Pritchett 2010). It is therefore plausible that investing in state capacity for better program delivery may have high returns. Yet, while the importance of state capacity for economic development has been emphasized in recent theoretical work (Besley and Persson 2009, 2010), there is limited empirical evidence on the returns to such investments.

One frequent constraint on effective program implementation is the lack of a secure payments infrastructure to make transfers to intended beneficiaries. Money meant for the poor is often simply stolen by officials along the way, with case studies estimating "leakage" of funds as high as 70 to 85 percent (Reinikka and Svensson 2004; Programme Evaluation Organisation 2005; Niehaus and Sukhtankar 2013b). Thus, building a secure payments infrastructure, which makes it easier for governments to accurately identify beneficiaries and transfer benefits directly into their bank accounts, may significantly improve state capacity for program implementation.[1]

This view has gained momentum from recent technological advances, which have made it feasible to issue payments via bank accounts linked to biometrically authenticated unique IDs. Biometric technology is seen as especially promising in developing countries, where high illiteracy rates make it unrealistic to universally deploy traditional forms of authentication, such as passwords or personal identification numbers (PINs).[2] The potential for such payment systems to improve the performance of public welfare programs (and also increase financial inclusion for the poor) has generated enormous global interest, with at least 230 programs in over 80 countries deploying biometric identification and payment systems (Gelb and Clark 2013). This enthusiasm is exemplified by India's ambitious *Aadhaar* initiative to provide biometric-linked unique IDs (UIDs) to nearly one billion residents, and then transition social program payments to Direct Benefit Transfers via UID-linked bank accounts. Over 850 million UIDs had been issued as of June 2015, with the former Finance Minister of India claiming that the project would be "a game changer for governance" (Harris 2013).

At the same time, there are a number of reasons to be skeptical about the hype around these new payment systems. First, their implementation entails solving 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 whose rents are threatened may subvert the intervention and limit its effectiveness (Krusell and Ríos-Rull 1996; Parente and Prescott 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 corruption on some margins could displace it onto others (e.g., Yang 2008a) or could paradoxically hurt the poor if it dampened incentives for officials to implement antipoverty programs in the first place (Leff 1964). Finally, even assuming

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[1] It may also expand the state's long-term choice set of policies that are feasible to implement, including replacing distortionary commodity subsidies with equivalent income transfers.

[2] Fujiwara (2015) provides analogous evidence from Brazil on the effectiveness of electronic voting technology in circumventing literacy constraints, and on increasing enfranchisement of less-educated voters.

positive impacts, cost-effectiveness is unclear as the best available estimates depend on a number of untested assumptions (see e.g., National Institute for Public Finance and Policy 2012). Overall, there is very limited evidence to support either the enthusiasts or the skeptics of biometric payment systems.

In this paper, we contribute toward filling this gap, by presenting evidence from a large-scale experimental evaluation of the impact of rolling out biometric payments infrastructure to make social welfare payments in India. Working with the Government of the Indian state of Andhra Pradesh (AP),[3] we randomized the order in which 157 subdistricts introduced a new Smartcard initiative for making payments in two large welfare programs: the National Rural Employment Guarantee Scheme (NREGS), and Social Security Pensions (SSP). NREGS is the largest workfare program in the world (targeting 800 million rural residents in India), but has well-known implementation issues including problems with the payment process and leakage (Dutta et al. 2012; Niehaus and Sukhtankar 2013a,b). SSP programs complement NREGS by providing income support to the rural poor who are not able to work (Dutta, Murgai, and Howes 2010). The new Smartcard-based payment system used a network of locally hired, bank-employed staff to biometrically authenticate beneficiaries and make cash payments in villages. It thus provided beneficiaries of NREGS and SSP programs with the same effective functionality as intended by UID-linked Direct Benefit Transfers.

The experiment randomized the rollout of Smartcards across 157 subdistricts covering some 19 million people. Randomizing at this scale lets us address one common concern about randomized trials in developing countries: that studying small-scale pilots (especially when non-governmental organization-led) may not provide accurate forecasts of performance when governments must implement the same technical intervention at a larger organizational scale.[4] Because we evaluate implementation by the government at full scale, we are more confident than usual that the results speak to the potential impacts of similar technologies in other settings (we discuss caveats to external validity in the conclusion).

After two years of program rollout, the share of Smartcard-enabled payments across both programs in treated subdistricts had reached around 50 percent. This conversion rate over two years compares favorably to the pace of electronic benefit transfer rollout in other contexts. For example, the United States took over 15 years to convert all Social Security payments to electronic transfers, while the Philippines took 5 years to reach about 40 percent coverage in a cash transfer program. In AP, the inability to reach a 100 percent conversion rate (despite the stated goal of senior policymakers to do so) reflects the nontrivial logistical, administrative, and political challenges of rolling out a complex new payment system (see Section IC and Mukhopadhyay et al. 2013 for details).

We therefore focus 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. These estimates yield the relevant policy parameter of interest, because they reflect the impacts that followed a decision by senior

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[3] The original state of AP (with a population of 85 million) was divided into two states on June 2, 2014. Since this division took place after our study, we use the term AP to refer to the original undivided state.
[4] See, for example, Banerjee, Duflo, and Glennerster (2008); Acemoglu (2010); and Bold et al. (2013).

government officials to invest in the new payments system and are net of all the logistical and political economy challenges that accompany such a project in practice.

We organize our analysis around three main dimensions of program performance: payment logistics, (prevention of) leakage, and program access. Beginning with payment logistics, we find that Smartcards delivered a faster and more predictable payment process for beneficiaries, especially under the NREGS program. NREGS workers spent 22 fewer minutes collecting each payment (20 percent less than the control group), and collected their payments 5.8 to 10 days sooner after finishing their work (17–29 percent faster than the control mean). The absolute deviation of payment delays also fell by 21–39 percent relative to the control group, suggesting that payments became more predictable. Payment collection times for SSP beneficiaries also fell, but the reduction was small and statistically insignificant.

Turning to leakage, we find that household NREGS earnings in treated areas increased by 24 percent while government outlays on NREGS did not change. The net result is a significant reduction in leakage of funds between the government and target beneficiaries. With a few further assumptions (see Section IIIB), we estimate a 12.7 percentage point reduction in NREGS leakage in treated areas (a 41 percent reduction relative to the control mean). Similarly, SSP benefit amounts increased by 5 percent, with no corresponding change in government outlays, resulting in a significant reduction in SSP leakage of 2.8 percentage points (a 47 percent reduction relative to the control mean).

These gains for participants on the intensive margin of program performance were not offset by reduced access to programs on the extensive margin. We find that the proportion of households reporting having worked on NREGS *increased* by 7.1 percentage points (a 17 percent increase over the control mean of 42 percent). We show that this result is explained by a significant reduction in the fraction of "quasi-ghost beneficiaries," defined as cases where officials reported work against a beneficiary's name and claimed payments for this work, but where the beneficiary received neither work nor payments. These results suggest that the introduction of biometric authentication made it more difficult for officials to overreport the amount of work done (and siphon off the extra wages unknown to the beneficiary), and that the optimal response for officials was to ensure that more actual work was done against the claimed wages, with a corresponding increase in payments made to workers. We find no impact on access to pensions, with the rate of SSP enrollment unchanged.

We also examine the distribution of impacts on each margin of performance. We find no evidence that poor or vulnerable segments of the population were made worse off by the new system. For each dimension of performance with significant positive average impacts, treatment distributions first-order stochastically dominate control distributions. Thus, no treatment household was worse off relative to a control household at the same percentile of the outcome distribution. Treatment effects also did not vary significantly as a function of village-level baseline characteristics, suggesting broad-based gains across villages from access to the new payments system.

The Smartcards intervention introduced two main sets of changes to the payments process. First, it changed the organizations responsible for managing the fund flow and making payments, and moved the point of payment closer to the village.

Second, it introduced biometric authentication. In a nonexperimental decomposition of the treatment effects, we find that improvements in the timeliness of payments are concentrated entirely in villages that switched to the new payment system, but do not vary within these villages across recipients who had or had not received biometric Smartcards. In contrast, increases in payments to beneficiaries and reductions in leakage are concentrated entirely among NREGS and SSP recipients who actually received biometric Smartcards. This suggests that organizational changes associated with the new payment system drove improvements in the payments process, while biometric authentication was key to reducing fraud.

Overall, the data suggest that Smartcards improved beneficiary experiences in collecting payments, increased payments received by intended beneficiaries, reduced corruption, broadened access to program benefits, and achieved these without substantially altering fiscal burdens on the state. Consistent with these findings, 90 percent of NREGS beneficiaries and 93 percent of SSP recipients who experienced Smartcard-based payments reported that they prefer the new system to the old.

Finally, Smartcards appear to be cost-effective. In the case of NREGS, our best estimate of the value of beneficiary time savings ($4.5 million) *alone* exceeds the government's cost of program implementation and operation ($4 million). Further, our estimated NREGS leakage reduction of $38.5 million/year is over nine times greater than the cost of implementing the new Smartcard-based payment system. The estimated leakage reduction in the SSP program of $3.2 million/year is also higher than the costs of the program ($2.3 million). The reductions in leakage represent redistribution from corrupt officials to beneficiaries, and are hence not Pareto improvements. However, if a social planner places a greater weight on the gains to program beneficiaries (likely to be poorer) than on the loss of illegitimate rents to corrupt officials, the welfare effects of reduced leakage will be positive.

The first contribution of our paper is as an empirical complement to recent theoretical work emphasizing the role of state capacity in economic development (Besley and Persson 2009, 2010).[5] An important theme in this literature is that politicians may perceive the returns to investments in state capacity as accruing in the long run, while their own time horizon of interest may be shorter. Further, both theory and evidence suggest that politicians' incentives to invest in general-purpose state capacity may be muted relative to incentives to fund specific programs that provide patronage to targeted voter and interest groups (Lizzeri and Persico 2001; Mathew and Moore 2011). Viewed through this lens, it is worth highlighting not only that Smartcards yielded large and positive returns, but also that these returns materialized in as short a period as two years. Thus, our results suggest that there may be large and rapid social returns to investing in better program implementation capacity—especially in developing countries with weak governance.[6]

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[5] Note that political scientists also use the term "state capacity" to represent the set of formal institutions that adjudicate conflicting claims in societies (including legislatures and judiciaries). Besley and Persson (2010) focus on fiscal and legal state capacity, but do not distinguish the legislative and executive aspects of such capacity. In practice, the poor implementation of existing laws, regulations, and policies in developing countries (including widespread tax evasion and leakage in spending) suggest that the executive side of state capacity is an important constraint in these settings. This is what our study focuses on.

[6] While set in a different sector, the magnitude of our estimated reduction in leakage relative to intervention cost is very similar to recent estimates showing that investing in better school governance in India may yield a ten-fold return on investment through reduced costs of teacher absence (Muralidharan et al. 2014).

We also contribute to work on reducing corruption in developing countries (Reinikka and Svensson 2005; Olken 2007). Our results demonstrate the potential of technology-enabled top-down improvements in governance, set in the context of a literature which has found mixed results. While Duflo, Hanna, and Ryan (2012) find, for example, that time-stamped photos and monetary incentives increased teacher attendance and test scores in NGO-run schools, Banerjee, Duflo, and Glennerster (2008) find that a similar initiative to monitor nurses was subverted by vested interests when it transitioned from an NGO-led pilot to government implementation. Our results suggest that technological solutions *can* significantly reduce corruption when implemented as part of an institutionalized policy decision to do so at scale. In this sense our results align with those of Banerjee et al. (2014), who find that a Government of Bihar initiative to modernize NREGS reporting and fund-flow systems lowered corruption. Similarly, Barnwal (2015) finds that a Government of India initiative to deliver cooking gas subsidies using bank accounts and biometric authentication reduced leakage to ghost beneficiaries.

Finally, our results complement 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. (2015) find that using mobile money to deliver transfers in Niger cut costs and increased women's intrahousehold bargaining power; and Giné, Goldberg, and Yang (2012) show how biometric authentication helped a bank in Malawi reduce default and adverse selection.

From a policy perspective, our results contribute to the ongoing debates in India and other developing countries regarding the costs and benefits of using biometric payments technology for service delivery. We discuss the policy implications of our results and caveats to external validity across both locations and programs in the conclusion.

The rest of the paper is organized as follows. Section I describes the context, social programs, and the Smartcard intervention. Section II describes the research design, data, and implementation details. Section III presents our main results. Section IV discusses cost-effectiveness. Section V concludes. We also include an extensive online Appendix with supplemental program details and analysis.

I. Context and Intervention

The AP Smartcard Project integrated new payments infrastructure into two major social welfare programs managed by the Department of Rural Development, which serve as a comprehensive safety net for both those able (NREGS) and unable (SSP) to work. This section summarizes these programs and how the introduction of Smartcards altered their implementation, with further details in online Appendix A.

A. *The National Rural Employment Guarantee Scheme*

The NREGS is one of the main welfare schemes in India and the largest workfare program in the world, covering 11 percent 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.[7] The program guarantees every rural 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.

Participating households obtain jobcards, which list household members and have empty spaces for recording employment and payment. Jobcards are issued by the local gram panchayat (GP, or village) or mandal (subdistrict) government offices. Workers with jobcards can apply for work at will, and officials are legally obligated to provide either work on nearby projects or unemployment benefits (though, in practice, the latter are rarely provided). NREGS projects vary somewhat but typically involve minor irrigation work or improvement of marginal lands. Project worksites are managed by officials called Field Assistants, who record attendance and output on "muster rolls" and send these to the mandal office for digitization, from where the work records are sent up to the state level, which triggers the release of funds to pay workers.

Panel A of Figure 1 depicts the payment process in AP prior to the introduction of Smartcards. The state government transfers money to district offices, which pass the funds to mandal offices, which transfer it to beneficiary post office savings accounts. Workers withdraw funds by traveling to branch post offices, where they establish identity using jobcards and passbooks. In practice it is common for workers (especially illiterate ones) to give their documents to Field Assistants who then control and operate their accounts: taking sets of passbooks to the post office, withdrawing cash in bulk, and returning to distribute it in villages.

Issues of payments logistics, leakage, and access have all dogged NREGS implementation. Both prior research (Dutta et al. 2012) and data from our control group suggest that even conditional on doing NREGS work, the payment process is slow and unreliable, limiting the extent to which the NREGS can effectively insure the rural poor. In extreme cases, delayed payments have reportedly led to worker suicides (Pai 2013).

The payments process is also vulnerable to leakage of two forms: overreporting and underpayment. Consider a worker who has earned Rs 100, for example: the Field Assistant might report that he is owed Rs 150 but pay the worker only Rs 90, pocketing Rs 50 through overreporting and Rs 10 through underpayment. Two extreme forms of overreporting are ghost workers who do not exist, but against whose names work is reported and payments are made; and quasi-ghost workers who do exist, but who have not received any work or payments though work is reported against their names and payments are made. In both cases, the payments are typically siphoned off by officials. Prior work in the same context suggests that overreporting is the most prevalent form of leakage, perhaps because it involves stealing from a "distant" taxpayer, and can be done without the knowledge of workers (Niehaus and Sukhtankar 2013a).[8]

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[7] NREGS figures: http://indiabudget.nic.in/ub2013-14/bag/bag5.pdf; total outlays: http://indiabudget.nic.in/ub2013-14/bag/bag4.pdf (both accessed June 23, 2015).

[8] A growing literature has examined overinvoicing as a form of corruption and the effects of government policies on it. See Fisman and Wei (2004); Olken (2007); Yang (2008b); and Mishra, Subramanian, and Topalova (2008), among others.
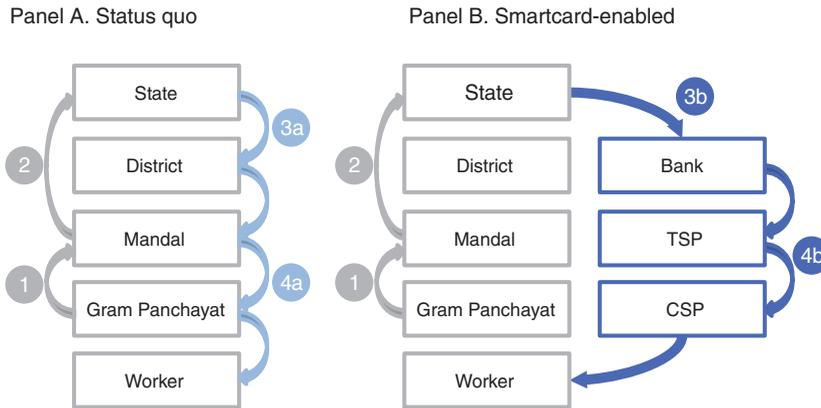
FIGURE 1. COMPARISON OF TREATMENT AND CONTROL PAYMENT SYSTEMS

Notes: This figure shows the flow of information and funds for NREGS payments, pre- and post-Smartcards. 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. The upward flow of information about work done is the same 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. However, the downward flow of funds is different. 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 (4b) the TSP transfers cash to the CSP, who delivers the cash and receipts to beneficiaries (both with and without Smartcards). Beneficiaries with Smartcards were required to biometrically authenticate identity before getting paid. Beneficiaries without Smartcards were issued "manual payments" with status quo forms of authentication and acknowledgment of payment receipt. The flow of information and funds for SSP payments differs in the following ways: (1) there is no weekly flow of information up from GP level to determine beneficiaries (no muster rolls, etc.); (2) in the status quo model, GP officials directly made payments to beneficiaries, sometimes in their homes; the post office was not involved; (3) in the Smartcard-enabled system, payments were made in the same way as for NREGS beneficiaries. In both models, SSP payments are made monthly at the beginning of the month, rather than weekly or biweekly like in NREGS. Note that the Bank/TSP/CSP structure for the Smartcard-based payments reflects Reserve Bank of India (RBI) regulations requiring that accounts be created only by licensed banks. Since the fixed cost of bank branches is typically too high to make it viable to profitably serve rural areas, the RBI allows 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. In practice, the accounts were only used to withdraw government benefits and not to make deposits or maintain balances.

Finally, program access is imperfect, although by design NREGS work and payments should be constrained only by worker demand. In practice, supply appears to be the binding constraint, with NREGS availability being constrained by the level of budgetary allocations and by limited local administrative capacity and willingness to implement projects (Dutta et al. 2012; Witsoe 2014). We confirm this in our data, where less than 4 percent of workers in our control group report that they can access NREGS work whenever they want it.

B. *Social Security Pensions*

Social Security Pensions are unconditional monthly payments targeted to vulnerable populations. The program covers over 6 million beneficiaries and costs the state of AP roughly Rs 18 billion ($360 million) annually. Eligibility is restricted to members of families classified as Below the Poverty Line (BPL) who are 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 (more than 65 years old), widow, disabled, or certain displaced traditional occupations. Pension lists are proposed by village assemblies (gram sabhas) and sanctioned by the mandal administration. Pensions pay Rs 200 (approx. $3) per month except for disability pensions, which pay Rs 500 (approx. $8). Unlike the NREGS, pension payments are typically disbursed in the first week of each month in the village itself by a designated village development officer. Payments were made in cash with beneficiaries acknowledging receipt of benefits by signature or thumb-print on a paper beneficiary roster.

The SSP program appears to be better implemented than NREGS. Dutta, Murgai, and Howes (2010) find that it is well targeted with relatively low levels of leakage (about 17 percent in Karnataka, less than one-half of the rate found in other comparable welfare programs). We also did not find documented evidence on beneficiary complaints regarding the SSP payment process.[9] This is likely to be because it is a straightforward process, with a mostly fixed list of beneficiaries who receive a fixed amount of payment at a fixed time every month for every month of the year, as opposed to the NREGS where the government needs to figure out who to pay among 65 percent of the rural population with jobcards, and how much they should be paid—both of which can be different from week to week.

C. *Smartcard-Enabled Payments*

The Smartcard project was India's first large-scale attempt to implement a biometric payments system.[10] It was a composite intervention, introducing two complementary but conceptually distinct bundles of reforms: one set of technological changes, and one set of organizational ones.

Technologically, the intervention changed the way in which beneficiaries were expected to establish their identity when collecting payments. Under the status quo, beneficiaries proved identity by exhibiting identifying documents to the agent issuing payments, who was responsible for verifying these. Under the Smartcards scheme, biometric data (typically all ten fingerprints) and digital photographs were collected during enrollment campaigns and linked to newly created bank accounts. Beneficiaries were then issued a physical Smartcard that included their photograph and (typically) an embedded electronic chip storing biographic, biometric, and bank account details. Beneficiaries use these cards to collect payments as follows: (i) they insert them into a point-of-service device operated by a customer service provider (CSP), which reads the card and retrieves account details; (ii) the device prompts for one of ten fingers, chosen at random, to be scanned; (iii) the device compares this scan with the records on the card, and authorizes a transaction if they match; (iv) the

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[9]Our pilots confirmed this, and we therefore did not collect data on SSP payment delays.

[10]The central (federal) government had similar goals for the Aadhaar (UID) platform. However, the initial rollout of Aadhaar was as an enabling infrastructure, and it had not yet been integrated into any of the major welfare schemes as of June 2014. The Smartcard intervention can therefore be seen as a functional precursor to the integration of Aadhaar into the NREGS and SSP.

amount of cash requested is disbursed;[11] and (v) the device prints out a receipt (and in some cases announces transaction details in the local language, Telugu). Figure A.1 in the online Appendix shows a sample Smartcard and a fingerprint scan in progress.

Organizationally, the intervention changed the vendors and staff responsible for managing the flow of funds and delivering payments. The Government of Andhra Pradesh (GoAP) contracted with banks to manage payments for both schemes, and these banks in turn contracted with technology service providers (TSPs) to manage the accounts; the TSPs then hired and trained CSPs to handle the last-mile logistics of cash management and payments. Panel B of Figure 1 illustrates the flow of funds from the government through banks, TSPs, and CSPs to beneficiaries under this scheme. GoAP assigned each district to a single bank-TSP pairing, and compensated them with a 2 percent commission on all payments delivered in GPs that were migrated to the new Smartcard-based payment system (banks and TSPs negotiated their own terms on splitting the commission).

GoAP required a minimum of 40 percent of beneficiaries in a GP to be enrolled and issued Smartcards prior to converting the GP to the new payment system; this threshold applied to each program separately. Once a GP was "converted," all payments—for each program in which the threshold was reached—in that GP were routed through the Bank-TSP-CSP system, even for beneficiaries who had not enrolled in or obtained Smartcards. Beneficiaries who were not enrolled for a Smartcard received payments in cash from the CSP with manual record-keeping against the roster of beneficiaries.[12]

GoAP also stipulated norms for CSP selection, and required that CSPs be women resident in 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.[13] While meeting all these requirements was often difficult and sometimes impossible, the selected CSPs were typically closer socially to beneficiaries than the post-office officials or village development officers (both government employees) who previously disbursed payments (for NREGS and SSP, respectively). Moreover, because CSPs were stationed within villages, they were also geographically closer to beneficiaries.

The efficacy of a reform as complex and ambitious as Smartcards necessarily depends as much on success in execution as on design on paper. Indeed, GoAP faced a number of technical, logistical, and political challenges in implementing Smartcards. Even with the best of intentions and administrative attention, enrolling tens of millions of beneficiaries, distributing Smartcards and point-of-service devices, identifying and training CSPs, and establishing cash management protocols

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[11] While beneficiaries could in principle leave balances on their Smartcards and use them as savings accounts, NREGS guidelines required beneficiaries to be paid in full for each spell of work. Thus, in practice, workers almost always withdrew their wages in full, and rarely deposited other funds into their Smartcard-linked bank account or used it as a savings account.

[12] Bank accounts were not created for nonenrolled beneficiaries. They were paid in cash, and cash management and reconciliation took place through the CSP's own cash float account. In the case of these manual payments, status quo forms of identification and acknowledgment of payment receipt were used. The photograph in online Appendix Figure A.1 shows both a case of Smartcard-based authentication taking place and also shows the accompanying beneficiary roster for manual record keeping for beneficiaries without Smartcards (with payments being acknowledged through fingerprint stamps).

[13] Self-help groups are groups of women organized by the government to facilitate microlending.

would have been a nontrivial task. On top of this, local officials who benefited from the status quo system had little incentive to cooperate with the project, and attempted at times to capture it (e.g., by influencing CSP selection) or delay its implementation (e.g., by citing problems it was creating for beneficiaries). On the other hand, senior officials of GoAP prioritized the project, giving it considerable administrative resources and attention. More generally, GoAP was strongly committed to NREGS and AP was a leader in utilization of federal funds earmarked for the program. Our estimates capture all these factors: they measure the impact of a policy-level decision to implement Smartcards at scale, and are net of all the practical complexities of doing so.

D. *Potential Impacts of Smartcards*

A priori, the Smartcards intervention could have affected program performance on multiple dimensions. To help structure the analysis that follows we organize it around three main dimensions of impact: payments logistics, leakage, and program access.

First, payments logistics could improve or deteriorate. Smartcards could speed up payments, for example, by moving transactions from the (typically distant) post office to a point within the village. They could just as easily slow down the process, however, if CSPs were less reliably present or if the checkout process were slower due to technical problems.[14] Similarly, on-time cash availability could either improve or deteriorate depending on how well banks and TSPs managed fund flow and cash logistics relative to the status quo. In a worst-case scenario the intervention could cut off payments to beneficiaries who were unable to obtain cards, lost their cards, or faced malfunctioning authentication devices.

Second, leakage might or might not decrease. In principle, Smartcards should reduce payments to ghost beneficiaries as ghosts do not have fingerprints, and also make it harder for officials to collect payments in the name of real beneficiaries as they must be present, provide biometric input, and receive a receipt which they can compare to the amount disbursed. These arguments assume, however, that the field technology works as designed and that CSPs are not more likely to be corrupt than local GP officials and post office workers. Moreover, achieving significant leakage reductions might require near complete implementation and yet the intervention was complex enough that complete implementation was unlikely.[15]

Finally, program access could also improve or suffer. In the case of NREGS, reducing rents may reduce local officials' incentives to create and implement projects, which could reduce access. On the other hand, a reduction in officials' incentives to overreport work done (because the money now goes directly to beneficiaries) might induce them to increase the actual amount of work done (to better correspond to the inflated muster rolls), which could increase access to NREGS. In other words, if Smartcards make it more difficult for officials to siphon off funds, more of these

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[14] For example, case-study-based evidence suggests that manual payments were faster than e-payments in Uganda's cash transfer program (Bohling and Zimmerman 2013b).

[15] Specifically, leakage reduction may be convex in the extent of coverage if those who enroll for Smartcards are genuine workers, and if the nonenrollees are the ghosts. In such a setting, there may be limited impact on leakage reduction unless Smartcard coverage is near complete and uncarded payments are stopped.

funds could be available for actual work and may lead to NREGS implementation becoming closer to what the program framers intended (with more work, more payments to workers, and more rural assets created). In the case of SSP, reducing leakage could drive up the illicit price of getting on the SSP beneficiary list.

The Smartcards intervention included both *technological* and *organizational* innovations, and our intent-to-treat estimates reflect the combined impact of both sets of changes. We present a nonexperimental decomposition of the relative contribution of these two components in Section IIIF. Finally, we present results for NREGS and SSP programs in parallel to the extent possible, but there is no reason to expect similar impacts because both the fundamental payments challenge and preexisting implementation quality were different.

II. Research Design

A. *Randomization*

The AP Smartcard project began in 2006, but took time to overcome initial implementation challenges including contracting, integration with existing systems, planning the logistics of enrollment and cash management, and developing processes for financial reporting and reconciliation. Because the government contracted with a unique bank to implement the project within each district, and because multiple banks participated, considerable heterogeneity in performance across districts emerged over time. In 8 of 23 districts the responsible banks had made very little progress as of late 2009; in early 2010 the government decided to restart the program in these districts, and reallocated their contracts to banks which had implemented Smartcards in other districts. This "fresh start" created an attractive setting for an experimental evaluation of Smartcards for two reasons. First, the roll-out of the intervention could be randomized in these eight districts. Second, the main implementation challenges had already been solved in other districts, yielding a "stable" implementation model prior to the evaluation.

Our evaluation was conducted in these eight districts (see online Appendix Figure C.1), which have a combined rural population of around 19 million. While not randomly selected, they look similar to AP's remaining 13 nonurban districts on major socioeconomic indicators, including proportion rural, scheduled caste, literate, and agricultural laborers (see online Appendix D.1). They also span the state geographically, with representation in all three historically distinct sociocultural regions: two in Coastal Andhra and three each in Rayalseema and Telangana.

The study was conducted under a formal agreement between J-PAL South Asia and the Government of Andhra Pradesh (GoAP) to randomize the order in which mandals (subdistricts) were converted to the Smartcard system. We assigned a total of 296 mandals to treatment and control status by lottery as follows: 112 mandals were assigned to the treatment group, 139 to a "buffer" group, and 45 to a control group (online Appendix Figure C.1).[16] We collected survey data only in the

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[16] Note that there were a total of 405 mandals in the 8 study districts, but we excluded 109 mandals from the universe of our study (mainly because Smartcard enrollment had started in these mandals before the agreement with GoAP was signed). The remaining 296 mandals comprised the universe of our study and randomization. See online

treatment and control groups; we created the buffer group to ensure we would have time to conduct endline surveys after Smartcards had been deployed in the treatment mandals but before they were deployed in the control mandals (during which period, enrollment could take place in the buffer group without affecting the control group). The realized lag between program rollout in treatment and control mandals was over two years. Randomization was stratified by district and by a principal component of socioeconomic characteristics. Online Appendix Table C.1 presents tests of equality between treatment and control mandals along characteristics used for stratification, none of which (unsurprisingly) differ significantly. Table C.2 reports balance along all of our main outcomes as well as key socioeconomic household characteristics from the baseline survey; 3 of 28 differences for NREGS and 2 of 17 for SSP are significant at the 10 percent level. In the empirical analysis we include specifications that control for the village-level baseline mean value of our outcomes to test for sensitivity to any chance imbalances.

B. *Data Collection*

Our data collection was designed to capture impacts broadly, including both anticipated positive and negative effects; full details are provided in online Appendix B. We first collected official records on beneficiary lists and benefits paid, and then conducted detailed baseline and endline household surveys of samples of enrolled participants. Household surveys included questions on receipts from and participation in the NREGS and SSP as well as questions about general income, employment, consumption, and assets. We conducted surveys in August through early October of 2010 (baseline) and 2012 (endline) in order to obtain information about NREGS participation between late May and early July of those years, as this is the peak period of participation in most districts (see Figure 2).[17] The intervention was rolled out in treatment mandals shortly after baseline surveys. We also conducted unannounced audits of NREGS worksites during our endline surveys to independently verify the number of workers who were present.

Full details and discussion of the sampling procedure used are in online Appendix C.2. In brief, we sampled 880 GPs in which to conduct surveys. Within each GP we sampled ten households, six from the frame of NREGS jobcard holders, and four from the frame of SSP beneficiaries. Our NREGS sample included five households in which at least one member had worked during May and June according to official records and one household in which no member had worked. 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 survey we sampled 8,774 households, of which we were unable to survey or confirm existence of 295, while 365 households were confirmed as ghost households, leaving us with survey data

Appendix C.1 for full details on the randomization, and D.3 for comparisons between the 109 nonstudy mandals and the 296 study mandals.

[17] There is a trade-off between surveying too soon after the NREGS work was done (since payments would not have been received yet), and too long after (since recall problems might arise). We surveyed on average ten weeks after work was done, and also facilitated recall by referring to physical copies of jobcards (on which work dates and payments are meant to be recorded) during interviews.
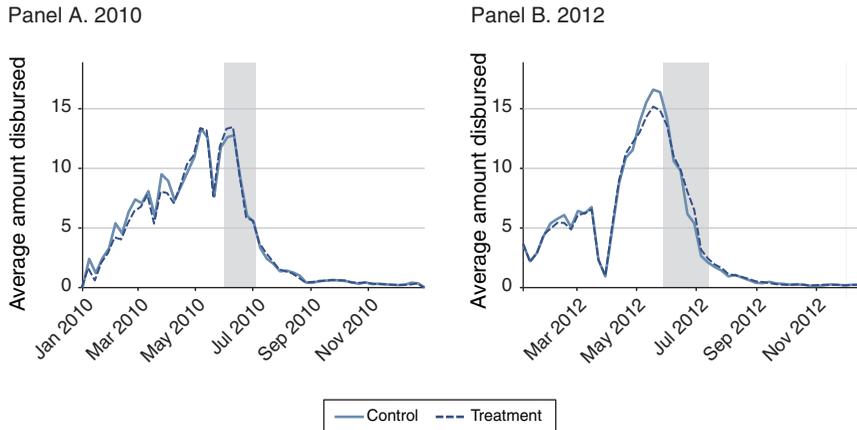
FIGURE 2. OFFICIAL DISBURSEMENT TRENDS IN NREGS

Notes: This figure shows official NREGS payments for all workers averaged at the GP-week level for treatment and control areas. The gray 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).

on 8,114 households (corresponding numbers for baseline are 8,572, 1,000, 102, and 7,425, respectively).

The resulting dataset is a panel at the village level and a repeated cross-section at the household level. This is by design, as the endline sample should be representative of potential participants at that time. We verify that the treatment did not affect either the size or composition of the sampling frame (online Appendix C.3), suggesting that our estimated treatment effects are not confounded by changes in the composition of potential program beneficiaries.

While details are available in tables notes as well as online Appendix Table B.1, we briefly describe the construction of our main outcome variables here. Payment process and program earnings outcomes for NREGS are focused on a seven-week study period (May 28 to July 15, 2012), while those for SSP pertain to May, June, and July 2012. For each program, individual beneficiaries were asked to report the average time taken to collect payments in these periods (in minutes), including the time spent on unsuccessful trips to payment sites. For the NREGS, we also asked the precise date of payment receipt for each week of work done, allowing us to calculate the payment delay as the number of days between the end of the week and the date of the payment. In addition, we calculate the deviation in payment lag as the absolute value of the difference between individual payment delay in week w and the mandal median delay in week w. We did not collect analogous data on date of payment from SSP beneficiaries as payment lags had not surfaced as a major concern for them during initial fieldwork.

Official payments for both programs come from official disbursement data. For the NREGS, we assign officially recorded spells to correspond to survey study weeks, average by the number of endline study weeks (7), and aggregate data at the household level (in case work/payments are misassigned to household members). For the SSP, this is simply the average disbursement across the months of May, June, and July 2012 to individual beneficiaries. For both programs, we consider official payments to
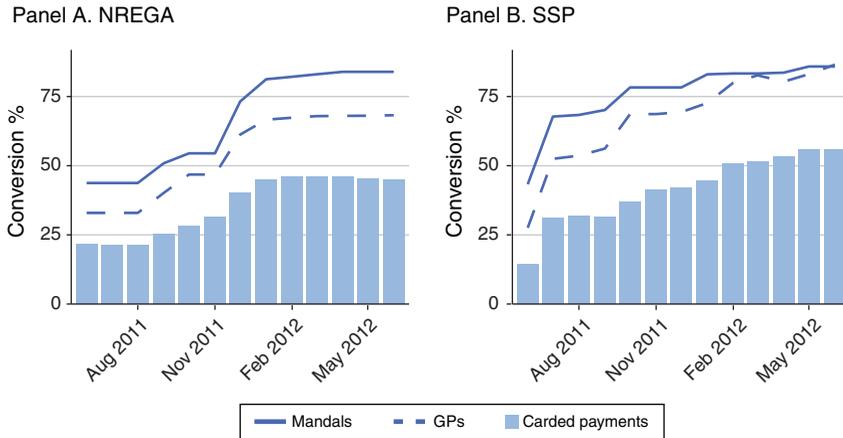
FIGURE 3. ROLLOUT OF SMARTCARD INTEGRATION WITH WELFARE PROGRAMS

Notes: This figure shows program rollout in aggregate and at different conversion levels. Each unit converts to the Smartcard-enabled system based on beneficiary enrollment in the program. Mandals plot shows 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 across all districts. Percent carded payments is obtained by multiplying percent mandals by percent converted GPs in converted mandals and percent payments to carded beneficiaries in converted GPs.

be all disbursals (including to ghost beneficiaries). To capture earnings in the survey, we ask every individual NREGS beneficiary listed on the officially sampled jobcard details of work done and payment received for each of the study weeks, generate average weekly receipts, and aggregate data at the household level. For SSP beneficiaries, we ask whether they made any payments to officials in order to receive their benefits in the study months, and subtract these payments from the amount their pension is supposed to pay every month. For both programs, if an official payment was made, but the household or beneficiary was a ghost, we consider the payment received to be zero. Finally, leakage is simply the difference between official and survey reports.

C. *First Stage and Compliance*

Figure 3 plots program rollout in treatment mandals from 2010 to 2012 using administrative data. By July 2012, 82 percent (89 percent) of treatment group mandals were "converted" (defined as having converted at least one GP) for NREGS (SSP) payments. Conditional on being in a converted mandal, 83 percent (93 percent) of GPs had converted for NREGS (SSP) payments, where being "converted" meant that payments were made through the new Bank-TSP-CSP system. These payments could include authenticated payments, unauthenticated payments to workers with Smartcards, and payments to workers without Smartcards.[18] Payments made to beneficiaries with Smartcards ("carded payments," both authenticated and unauthenticated) made up about two-thirds of payments within converted GPs by the endline. All told, about 50 percent of payments in treatment mandals across both programs were "carded" by May 2012. This coverage compares favorably with the

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[18] Transactions may not be authenticated for a number of reasons, including failure of the authentication device and nonmatching of fingerprints.

performance of changes in payments processes elsewhere. For example, a conditional cash transfer program in the Philippines (4Ps) took five years to reach 40 percent coverage (2008–2013) (Bohling and Zimmerman 2013a).[19]

Turning to compliance with the experimental design, sampled GPs in treated mandals were much more likely to have migrated to the new payment system, with 67 percent (79 percent) being "carded" for NREGS (SSP) payments, compared to 0.5 percent (0 percent) of sampled control GPs (Table 1). The overall rate of transactions done with carded beneficiaries was 45 percent (59 percent) in treatment areas, with no carded transactions reported in control areas. We can also assess compliance using data from our survey, which asked beneficiaries about their Smartcard use. About 38 percent (45 percent) of NREGS (SSP) beneficiaries in treated mandals said that they used their Smartcards recently, while 1 percent (4 percent) claimed to do so in control mandals. This latter figure likely reflects some beneficiary confusion between enrollment (the process of capturing biometrics and issuing cards) and the onset of carded transactions themselves, as the government did not allow the latter to begin in control areas until after the endline survey. Official and survey figures are also not directly comparable since the former describe *transactions* while the latter describe *beneficiaries*.

Given this first stage, we focus below on intent-to-treat (ITT) estimates, which can be interpreted as the average treatment effects corresponding to an approximately half-complete implementation.[20]

D. *Estimation*

We report ITT estimates, which compare average outcomes in treatment and control areas. All outcomes are estimated at the individual beneficiary level for SSP, and at the level which they were asked—individual, individual by week, or household— for NREGS, unless aggregation is necessary in order to compare with official data. All regressions are weighted by inverse sampling probabilities to obtain average partial effects for the populations of NREGS jobcard holders or SSP beneficiaries. We include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization (PC_{md}) in all regressions, and cluster standard errors at the mandal level. We thus estimate

$$(1) \qquad Y_{imd} = \alpha + \beta Treated_{md} + \delta District_d + \lambda PC_{md} + \epsilon_{imd},$$

where Y_{imd} is an outcome for household or individual i in mandal m and district d, and $Treated_{md}$ is an indicator for a mandal in wave 1. When possible, we also report specifications that include the baseline GP-level mean of the dependent

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[19] There was considerable heterogeneity in the extent of Smartcard coverage across the eight study districts, with coverage rates ranging from 31 percent in Adilabad to nearly 100 percent in Nalgonda district. Thus, we focus our analysis on ITT effects, and all our estimates include district fixed effects. We present correlates of implementation heterogeneity in online Appendix D, and provide a qualitative discussion of implementation heterogeneity in a companion study (Mukhopadhyay et al. 2013).

[20] Given implementation heterogeneity across districts and the possibility of nonlinear treatment effects in the extent of Smartcard coverage, our results should be interpreted as the average treatment effect across districts with different levels of implementation (averaging to around 50 percent coverage) and not as the impact of a half-complete implementation in all districts.

Table 1—Official and Self-Reported Use of Smartcards

	Official data		Survey data	
	Carded GP (1)	Mean fraction carded payments (2)	Payments generally carded (village mean) (3)	Most recent payment carded (village mean) (4)
Panel A. NREGS				
Treatment	0.67	0.45	0.38	0.38
	(0.045)	(0.041)	(0.043)	(0.042)
District fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.45	0.48	0.36	0.36
Control mean	0.0046	0.0017	0.039	0.013
Observations	880	880	818	818
Level	GP	GP	GP	GP
Panel B. SSP				
Treatment	0.79	0.59	0.45	0.45
	(0.042)	(0.038)	(0.052)	(0.049)
District fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.57	0.57	0.38	0.38
Control mean	0	0	0.069	0.044
Observations	880	880	878	878
Level	GP	GP	GP	GP

Notes: 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 usually happens once 40 percent of beneficiaries have been issued a card. Mean fraction carded payments is the proportion of transactions done with carded beneficiaries in treatment mandals. 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. The difference in number of observations between official and survey measures for NREGS is due to missing data for (mainly control) GPs where enrollment had not even started; assuming that there were no carded payments in these GPs increases the magnitude of the treatment effect on implementation. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at mandal level in parentheses.

variable, $\overline{Y}^0_{pmd}$, to increase precision and assess sensitivity to any randomization imbalances. We then estimate

$$(2) \qquad Y_{ipmd} = \alpha + \beta Treated_{md} + \gamma \overline{Y}^0_{pmd} + \delta District_d + \lambda PC_{md} + \epsilon_{ipmd},$$

where p indexes panchayats or GPs. Note that we easily reject $\gamma = 1$ in all cases and therefore do not report difference-in-differences estimates.

III. Effects of Smartcard-Enabled Payments

A. *Effects on Payment Logistics*

Data from our control group confirm that NREGS payments are typically delayed. Recipients in control mandals waited an average of 34 days after finishing a given spell of work to collect payment, more than double the 14 days prescribed by law (Table 2). The collection process is also time-consuming, with the average recipient in the control group spending almost two hours traveling and waiting in line to collect a payment.

TABLE 2—ACCESS TO PAYMENTS

	Time to collect (min)				Avg. payment lag (days)		Abs. payment lag deviation (days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	−22	−22	−6.1	−3.5	−5.8	−10	−2.5	−4.7
	(9.2)	(8.7)	(5.2)	(5.4)	(3.5)	(3.5)	(0.99)	(1.6)
BL GP mean		0.079		0.23		0.013		0.042
		(0.041)		(0.07)		(0.08)		(0.053)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.06	0.08	0.07	0.11	0.17	0.33	0.08	0.17
Control mean	112	112	77	77	34	34	12	12
Observations	10,191	10,120	3,789	3,574	14,213	7,201	14,213	7,201
Level	Indiv.	Indiv.	Indiv.	Indiv.	Indiv.-week	Indiv.-week	Indiv.-week	Indiv.-week
Survey	NREGS	NREGS	SSP	SSP	NREGS	NREGS	NREGS	NREGS

Notes: 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 and 6 is the average lag (in days) between work done and payment received on NREGS. The outcome in columns 7 and 8 is the absolute deviation from the week-specific median mandal-level lag. Since the data for columns 5–8 are at the individual-week level, we include week fixed effects to absorb variation over the study period. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at mandal level in parentheses.

Smartcards substantially improved this situation. The total time required to collect a NREGS payment fell by 22 minutes in mandals assigned to treatment (20 percent of the control mean). Time to collect payments also fell for SSP recipients, but the reduction is not statistically significant (Table 2; columns 1 and 2 for NREGS, columns 3 and 4 for SSP). We also find that over 80 percent of both NREGS and SSP beneficiaries who had received or enrolled for Smartcards reported that Smartcards had sped up payments (Table 6).

NREGS recipients also faced shorter delays in receiving payments after working, and these lags became more predictable. Columns 5 and 6 of Table 2 report that assignment to treatment lowered the mean number of days between working and collecting NREGS payments by 5.8 to 10 days, or 17–29 percent of the control mean (and 29–50 percent of the amount by which this exceeds the statutory limit of 14 days). There is also suggestive evidence that uncertainty about the timing of payments fell. While we do not directly measure beliefs, columns 7 and 8 show that the *variability* of payment lags—measured as the absolute deviation from the median mandal level lag, thus corresponding to a robust version of a Levene's test—fell by 21–39 percent of the control mean. This reduced variability is potentially valuable for credit-constrained households that need to match the timing of income and expenditure.

B. *Effects on Payment Amounts and Leakage*

Recipients in treatment mandals also received more money. For NREGS recipients, columns 3 and 4 of panel A of Table 3 show that earnings per week during our endline study period increased by Rs 35, or 24 percent of the control group mean. For SSP beneficiaries, earnings per beneficiary during the three months preceding

TABLE 3—OFFICIAL AND SURVEY REPORTS OF PROGRAM BENEFITS

	Official		Survey		Leakage	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. NREGS						
Treatment	11	9.6	35	35	−24	−25
	(12)	(12)	(16)	(16)	(13)	(13)
BL GP mean		0.13		0.11		0.096
		(0.027)		(0.037)		(0.038)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.03	0.05	0.05	0.06	0.04	0.04
Control mean	127	127	146	146	−20	−20
Observations	5,143	5,107	5,143	5,107	5,143	5,107
Panel B. SSP						
Treatment	4.3	5.1	12	12	−7.5	−7
	(5.3)	(5.4)	(5.9)	(6.1)	(3.9)	(3.9)
BL GP mean		0.16		0.0074		−0.022
		(0.092)		(0.022)		(0.026)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.00	0.01	0.01	0.01	0.01	0.01
Control mean	251	251	236	236	15	15
Observations	3,330	3,135	3,330	3,135	3,330	3,135

Notes: This table reports regressions of program benefits (in rupees) as reported in official or survey records. Both panels include all sampled households (NREGS)/beneficiaries (SSP) who were (i) found by survey team to match official records or (ii) listed in official records but confirmed as ghosts. Ghosts refer to households or beneficiaries within households who were confirmed not to exist, or who had permanently migrated before the study period started on May 28, 2012. In panel A, each outcome observation refers to household-level average weekly amounts for NREGS work done during the study period (May 28 to July 15 2012). Official refers to amounts paid as listed in official muster records. Survey refers to payments received as reported by beneficiaries; we only include beneficiaries listed on the officially sampled jobcard. Leakage is the difference between these two amounts. BL GP Mean is the GP average of household-level weekly amounts for NREGS work done during the baseline study period (May 31 to July 4, 2010). In panel B, each outcome observation refers to the average SSP monthly amount for the period May, June, and July 2012. Official refers to amounts paid as listed in official disbursement records. Survey refers to payments received as reported by beneficiaries. Leakage is the difference between these two amounts. BL GP Mean is the GP average SSP monthly amounts for the baseline period of May, June, and July 2010. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at mandal level in parentheses.

our endline survey (May–July) increased by Rs 12, or 5 percent of the control mean. In contrast, we see no impacts on fiscal outlays. For the workers sampled into our endline survey, we find no significant difference in official NREGS disbursements between treatment and control mandals. Similarly, SSP disbursements were also unaltered (columns 1 and 2 of panels A and B, respectively).

The fact that recipients report receiving more while government outlays are unchanged implies a reduction in leakage on both programs. Columns 5 and 6 of panel A confirm that the difference between official and survey measures of earnings per week on NREGS fell significantly by Rs 25. Results on the SSP program mirror the NREGS results: we find a reduction in leakage of Rs 7 per pension per month. This represents a 2.8 percentage point reduction in leakage relative to fiscal outlays, which is a 47 percent reduction relative to the control mean (panel B).

While we find evidence of a significant reduction in NREGS leakage, estimating the magnitude of this reduction as a proportion of average leakage requires additional data. We cannot simply compare what a given *household* reports receiving

to what the government reported paying out on the *jobcard* based on which that household was sampled because, in practice, many households have more than one jobcard listed in their name.[21] Using official records to count the total number of jobcards in our study districts, and data from the sixty-eighth round of the National Sample Survey (July 2011–June 2012) to estimate the number of households in those districts with at least one jobcard, we calculate that the number of jobcards exceeds the number of households with jobcards by an average factor of 1.9. This implies that we will substantially underestimate leakage if we do not account for multiple jobcards. Indeed, panel A of Table 3 shows that the naïve estimate for the control group is a *negative* leakage rate of Rs 20 per week.

To obtain a consistent estimate of average leakage we need to correct for multiple jobcards. We do so as follows: we scale up official records of payments issued in each district by the estimated number of jobcards per jobcard-holding household in that district, calculated as above. We then compare average amount disbursed per household (as opposed to per jobcard) to the average received per household. Using this method we estimate an endline leakage rate of 30.7 percent in control areas and 18 percent in treatment areas (Table E.1), implying that Smartcards reduced leakage by roughly 41 percent.[22]

Margins of Leakage Reduction.—We examine leakage reduction along the three margins discussed earlier: ghosts, overreporting, and underpayment. For the SSP decomposing leakage into these components is relatively straightforward since entitlements are fixed for each category of beneficiary. For the NREGS it is more difficult, as workers' entitlements are determined by applying a complex schedule of piece rates to the quantities of various kinds of work they perform, and we were not able to measure the latter (e.g., cubic feet of soil excavated). We therefore focus on the incidence rather than the magnitude of the three channels for NREGS: first, the incidence of ghost households; next, an indicator for jobcards with zero reported survey payments but positive official payments, a proxy for overreporting; and finally, the incidence of bribes paid to collect payments, a measure of underpayment.

Reductions in NREGS ghost beneficiaries are insignificant, though the incidence of ghosts is a nontrivial 11 percent (panel A of Table 4, columns 1 and 2). This is not surprising given the incomplete coverage of Smartcards, and the government's political decision to not ban unauthenticated payments. Thus, beneficiary lists were not purged of ghosts, and payments to these jobcards are likely to have continued. We also find limited impact on underpayment, measured as whether a bribe had to be paid to collect payments (panel A, columns 5 and 6). As we find little evidence of underpayment to begin with (control group incidence rate of 2.6 percent), Smartcards may have limited incremental value on this margin.

However, our proxy measure for overreporting in the NREGS drops substantially. The proportion of jobcards that had positive official payments reported but zero survey amounts (excluding ghosts) dropped significantly by 8.4 percentage points,

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[21] This issue is not solved by only including survey reports of individuals listed on the sampled jobcard—which we indeed do—since payments made to those individuals may be listed on other jobcards.

[22] However, this procedure leads to a loss of precision, as scaling up by a constant increases variance by the square of the constant (*p*-value 0.11). Online Appendix E.1 provides more detail on this procedure as well as an example to illustrate how the multiple-jobcard issue affects our calculations.

TABLE 4—ILLUSTRATING CHANNELS OF LEAKAGE REDUCTION

	Ghost households (percent)		Other overreporting (percent)		Bribe to collect (percent)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. NREGS						
Treatment	−0.0095	−0.0091	−0.082	−0.084	−0.0035	−0.0036
	(0.02)	(0.021)	(0.033)	(0.036)	(0.0085)	(0.0085)
BL GP mean		−0.017		0.016		0.000041
		(0.067)		(0.044)		(0.000041)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.02	0.02	0.05	0.04	0.01	0.01
Control mean	0.11	0.11	0.26	0.26	0.021	0.021
Observations	5,278	5,242	3,953	3,672	10,375	10,304
Level	Hhd	Hhd	Hhd	Hhd	Indiv.	Indiv.
	Ghost payments (Rs)		Other overreporting (Rs)		Underpayment (Rs)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. SSP						
Treatment	−2.9	−2.4	−2.7	−3.1	−2.3	−2.4
	(2.7)	(2.7)	(2.9)	(3)	(1.9)	(2)
BL GP mean		0.19		0.024		−0.02
		(0.16)		(0.01)		(0.045)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.01	0.01	0.01	0.01	0.01	0.01
Control mean	11	11	1.7	1.7	2.5	2.5
Observations	3,330	3,135	3,165	2,986	3,165	2,986

Notes: This table analyzes channels of reduction in leakage. Panel A reports the incidence of the three channels—ghosts, overreporting, and underpayment—for NREGS, while panel B decomposes actual amounts (in rupees) into these channels in the case of SSP. In both tables, Ghost households refers to households (or all beneficiaries within households) who were confirmed not to exist, or who had permanently migrated before the study period started on May 28, 2012 (May 31, 2010 for baseline). Other overreporting for NREGS is the incidence of jobcards that had positive official payments reported but zero survey payments (not including ghosts); note that the drop in observations as compared to Panel A of Table 3 is because here we drop jobcards with zero official payments. Bribe to collect refers to bribes paid in order to receive payments on NREGS. Other overreporting for SSP is the difference between what officials report beneficiaries as receiving and what beneficiaries believe they are entitled to (not including ghosts). Underpayment for SSP is the monthly amount paid in order to receive their pensions during May to July 2012. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at mandal level in parentheses.

or 32 percent (columns 3–4 of panel A). This result is mirrored in Figure 4, which presents quantile treatment effect plots on official and survey payments; here we see (i) no change in official payments at any part of the distribution; (ii) a significant reduction in the incidence of beneficiaries reporting receiving zero payments; and (iii) no significant change in amounts received relative to control households who were reporting positive payments.

These results suggest that leakage reduction was mainly driven by a reduction in the incidence of quasi-ghosts: real beneficiaries who did not previously get any NREGS work or payments, though officials were reporting work and claiming payments on their behalf. If some of these households were to have enrolled for a Smartcard, it would no longer be possible for officials to siphon off payments without their knowledge, following which officials' optimal response appears to have been to provide actual work and payments to these households (see results on
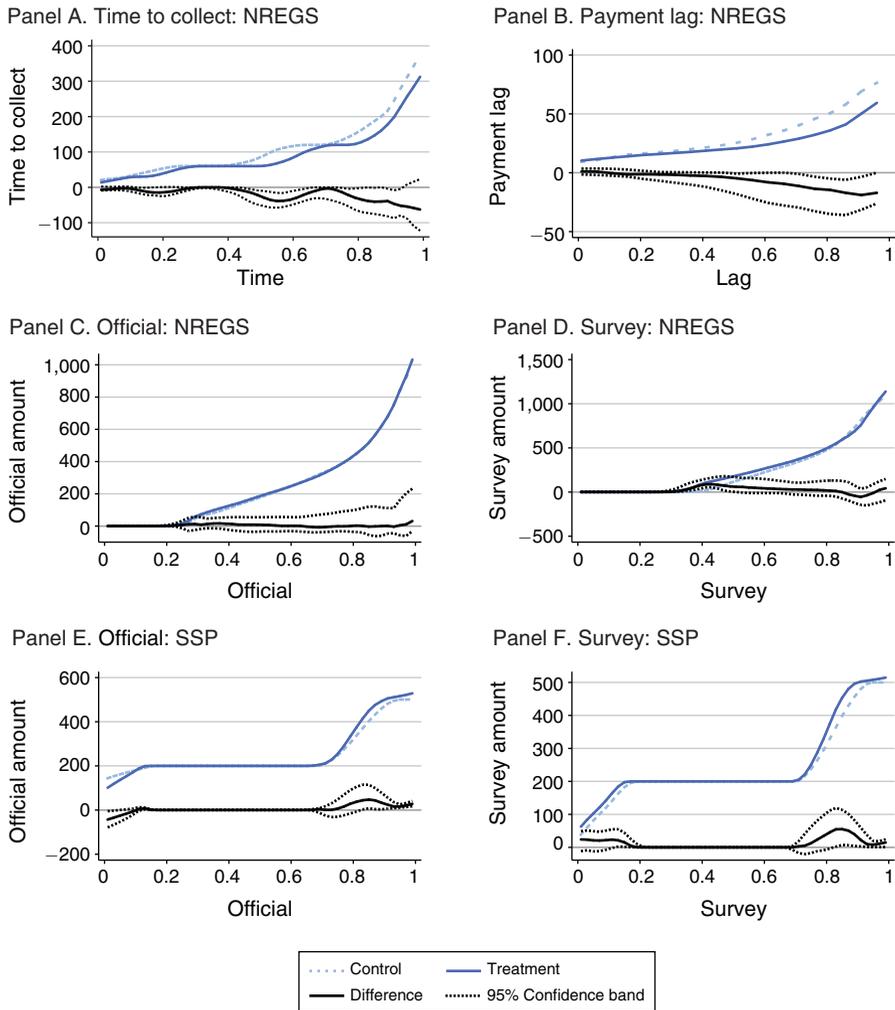
FIGURE 4. QUANTILE TREATMENT EFFECTS ON KEY OUTCOMES

Notes: Panels A–F show nonparametric treatment effects. Time to collect: NREGS (panel A) is the average time taken to collect a payment, including the time spent on unsuccessful trips to payment sites. Payment lag: NREGS (panel B) is the average lag (in days) between work done and payment received under NREGS. The official payment amounts, Official: NREGS and Official: SSP (panels C and E), refer to payment amounts paid as listed in official muster/disbursement records. The survey payment amounts, Survey: NREGS and Survey: SSP (panels D and F), refer to payments received as reported by beneficiaries. The NREGS data are taken from the study period (endline was 2012: May 28 to July 15), while SSP official data are an average of June, July, and August disbursements. All lines are fit by a kernel-weighted local polynomial smoothing function with Epanechnikov kernel and probability weights, with bootstrapped standard errors. The dependent variable is the vector of residuals from a linear regression of the respective outcome with the first principal component of a vector of mandal characteristics used to stratify randomization and district fixed effects as regressors.

access below). A decomposition of the reduction in SSP leakage (panel B of Table 4) reveals a reduction in all three forms of leakage, suggesting that Smartcards may have improved SSP performance on all dimensions (though none of the individual margins are significant).

The reduction in NREGS overreporting raises an additional question: if Smartcards reduced officials' rents on NREGS, why did they not increase the total amounts claimed (perhaps by increasing the number of ghosts) to make up for lost rents? Conversations with officials suggest that the main constraint in doing so was the use of budget caps within the NREGS in AP that exogenously fixed the maximum spending on the NREGS for budgeting purposes (also reported by Dutta et al. 2012). If enforced at the local level, these caps would limit local officials' ability to increase claims in response to Smartcards.

While we cannot directly test the hypothesis above, our result finding no significant increase in official payments in treated areas (panel A of Table 3) holds even when we look beyond our study period and sampled GPs. The evolution of official disbursements for *every* week in 2010 and 2012 (baseline and endline years) and in *all* GPs shows no discernible difference in treatment and control mandals at any time (Figure 2), with the treatment and control series tracking each other closely even after Smartcards began to roll out in the summer of 2010. This strongly suggests the existence of constraints that limited local officials' ability to increase the claims of work done.[23]

C. *Effects on Program Access*

Although Smartcards may have benefited participants by reducing leakage, they could make it harder for others to participate in the first place. Access could fall for both mechanical and incentive reasons. Mechanically, beneficiaries might be unable to participate if they cannot obtain Smartcards or successfully authenticate. Further, by reducing leakage, Smartcards could reduce officials' primary motive for running programs in the first place. This is particular true for the NREGS which—despite providing a de jure entitlement to employment on demand—is de facto rationed (Dutta et al. 2012). Indeed, in our control group 20 percent (42 percent) of households reported that someone in their household was unable to obtain NREGS work in May (January) when private sector demand is slack (tight); and only 3.5 percent of households said that anyone in their village could get work on NREGS any time (Table 5). Thus, the question of whether Smartcards hurt program access is a first-order concern.

We find no evidence that this was the case. If anything, households with jobcards in treated mandals were 7.1 percentage points *more* likely to have done work on the NREGS during our study period, a 17 percent increase relative to control (Table 5, columns 1 and 2). Combined with the results in the previous section showing a significant reduction in the incidence of quasi-ghost NREGS workers, these results suggest that the optimal response of officials to their reduced ability to report work without providing any work or payments to the corresponding worker, was to provide more actual work and payments to these workers. Beyond the increase in actual work during our survey period, columns 3–6 show that self-reported access to work also improved at other times of the year. The effects are insignificant in all but one

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[23] Budgetary allocations are likely to be the binding constraint for NREGS volumes in AP because the state implemented NREGS well and prioritized using all federal fiscal allocations. In contrast, states like Bihar had large amounts of unspent NREGS funds, and ethnographic evidence suggests that the binding constraint in this setting was the lack of local project implementation capacity (Witsoe 2014).

TABLE 5—ACCESS TO PROGRAMS

	Proportion of Hhds doing NREGS work		Was any Hhd member unable to get NREGS work in:		Is NREGS work available when anyone wants it?		Did you have to pay anything to get this NREGS work?		Did you have to pay anything to start receiving this pension?	
	Study period (1)	Study period (2)	May (3)	January (4)	All months (5)	All months (6)	NREGS (7)	NREGS (8)	SSP (9)	SSP (10)
Treatment	0.072	0.071	−0.023	−0.027	0.027	0.024	−0.0003	−0.00054	−0.046	−0.055
	(0.033)	(0.033)	(0.027)	(0.033)	(0.015)	(0.015)	(0.0015)	(0.0015)	(0.031)	(0.039)
BL GP mean		0.14				−0.023		−0.0064		0.025
		(0.038)				(0.027)		(0.0031)		(0.046)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.05	0.06	0.10	0.11	0.02	0.02	0.00	0.00	0.05	0.05
Control mean	0.42	0.42	0.2	0.42	0.035	0.035	0.0022	0.0022	0.075	0.075
Observations	4,943	4,909	4,748	4,496	4,755	4,715	7,185	6,861	581	352
Level	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	Indiv.	Indiv.	Indiv.	Indiv.

Notes: This table analyzes household level access to NREGS and SSP. Columns 1 and 2 report the proportion of households doing work in the 2012 endline study period (May 28 to July 15). If any member of the household did work on NREGS during that period, the household is considered "working." In columns 3 and 4, the outcome is an indicator for whether any member of household was unable to obtain work despite wanting to work during May (slack labor demand) or January (peak labor demand). In columns 5 and 6, the outcome is an indicator for whether the respondent believes anyone in the village who wants NREGS work can get it at any time. In columns 7 and 8, the outcome is an indicator for whether the respondent had to pay a bribe in order to obtain NREGS work during the endline study period. Note that only NREGS beneficiaries who worked during the endline study period are considered in columns 7 and 8. In columns 9 and 10, the outcome is an indicator for whether the respondent had to pay a bribe to get on the SSP beneficiary list in the years 2011 and 2012 (hence only new enrollees are included). All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at mandal level in parentheses.

case, but inconsistent with the view that officials "stop trying" once Smartcards are introduced. Bribes paid to access NREGS work were also (statistically insignificantly) lower (columns 7 and 8).

Given the theoretical concerns about potential negative effects of reducing leakage on program access, how should we interpret the lack of adverse effects in the data? One hypothesis is that officials simply had not had time to adapt their behavior (and reduce their effort on NREGS) by the time we conducted our endline surveys. However, the average converted GP in our data had been converted for 14.5 months at the time of our survey, implying that it had experienced two full peak seasons of NREGS under the new system. More generally, we find no evidence of treatment effects emerging over time in any of the official outcomes which we can observe weekly (e.g., Figure 2). On balance it thus appears more likely that we are observing a steady-state outcome.

A more plausible explanation for our results is that the main NREGS functionary (the Field Assistant) does not manage any other government program, which may limit the opportunities to divert rent-seeking effort. Further, despite the reduction in rent-seeking opportunities, implementing NREGS projects may have still been the most lucrative activity for the Field Assistant (note that we still estimate leakage rates of 20 percent in the treatment mandals). This may have mitigated potential negative extensive margin effects.[24]

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[24] The limited jurisdiction of the NREGS Field Assistant also suggests that there may have been limited opportunities for displacement of corruption to other programs (Yang 2008a). While we cannot measure corruption in

We similarly find no evidence of reduced access to the SSP program. Since pensions are valuable and in fixed supply, the main concern here would be that reducing leakage in monthly payments simply displaces this corruption to the registration phase, increasing the likelihood that beneficiaries must pay bribes to begin receiving a pension in the first place. We find no evidence that reduced SSP leakage increased the incidence of bribes at the enrollment stage. Columns 9 and 10 of Table 5 show that the incidence of these bribes among SSP beneficiaries who enrolled after Smartcards implementation began is in fact 5.5 percentage points lower in treated mandals (73 percent of the control mean), although this result is not statistically significant.[25]

D. *Heterogeneity of Impacts*

Even if Smartcards benefited the average program participant, it is possible that it harmed some. For instance, vulnerable households might have a harder time obtaining a Smartcard and end up worse off as a result. While individual-level treatment effects are by definition not identifiable, we can test the vulnerability hypothesis in two ways.

First, we examine quantile treatment effects for official payments, and survey outcomes that show a significant mean impact (time to collect payment, payment delays, and payments received). We find that the treatment distribution first-order stochastically dominates the control distribution for each of these outcomes (Figure 4). Thus, no treatment household is worse off relative to a control household at the same percentile in the outcome distribution.

Second, we examine whether treatment effects vary as a function of baseline characteristics at the village level. We find no heterogeneity in impacts as a function of the baseline value of each of the main outcome variables, suggesting broad-based program impacts (online Appendix Table F.3, row 1). Overall, the data do not identify any particular group that appears to have been adversely affected by Smartcards. We discuss the remainder of Table F.3 in online Appendix F.

E. *Beneficiary Perceptions of the Intervention*

The estimated treatment effects thus far suggest that Smartcards unambiguously improved service delivery. It is possible, however, that our outcome measures miss impacts on some dimension of program performance that deteriorated. We therefore complement our impact estimates with beneficiaries' stated preferences regarding the Smartcard-based payment system as a whole. We asked recipients in converted GPs within treatment mandals who had been exposed to the Smartcard-based

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other sectors, we find no evidence of strategic displacement of NREGS corruption to nontreated mandals (see online Appendix E.3).

[25] Note that while we have shown a range of results, our inference is not confounded by multiple hypothesis testing because the results represent tests of six different families of hypotheses regarding the impact of Smartcards on access to payments, leakage, and access to programs for NREGS and SSP, respectively. Within each family, we have either a single lead indicator of impact or a small number of indicators, and our inference is based on consistent results across these indicators. For instance, survey payments went up and leakage went down (for both NREGS and SSP); access to NREGS payments improved across all indicators, and access to NREGS work did not deteriorate on any indicator.

TABLE 6—BENEFICIARY OPINIONS OF SMARTCARDS

	NREGS				SSP			
	Agree	Disagree	Neutral/don't know	Observations	Agree	Disagree	Neutral/don't know	Observations
Positives								
Smartcards increase speed of payments (less wait times)	0.83	0.04	0.13	3,336	0.87	0.07	0.06	1,451
With a Smartcard, I make fewer trips to receive my payments	0.78	0.04	0.18	3,334	0.83	0.04	0.12	1,450
I have a better chance of getting the money I am owed by using a Smartcard	0.83	0.01	0.16	3,333	0.86	0.03	0.11	1,450
Because I use a Smartcard, no one can collect a payment on my behalf	0.82	0.02	0.16	3,331	0.86	0.03	0.11	1,446
Negatives								
It was difficult to enroll to obtain a Smartcard	0.19	0.66	0.15	3,338	0.29	0.60	0.11	1,451
I'm afraid of losing my Smartcard and being denied payment	0.63	0.15	0.21	3,235	0.71	0.15	0.14	1,403
When I go to collect a payment, I am afraid that the payment reader will not work	0.60	0.18	0.22	3,237	0.67	0.18	0.14	1,403
I would trust the Smartcard system enough to deposit money in my Smartcard account	0.29	0.41	0.30	3,334	0.31	0.46	0.24	1,448
Overall								
Do you prefer the Smartcards over the old system of payments?	0.90	0.03	0.07	3,346	0.93	0.03	0.04	1,454

Notes: This table analyzes beneficiaries' perceptions of the Smartcard program in GPs that had switched over to the new payment system (carded GPs). These questions were asked when NREGS and SSP 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. We are thus missing data for those beneficiaries who received but did not use Smartcards (10.5 percent of NREGS beneficiaries and 3.5 percent of SSP beneficiaries who enrolled).

payment system to describe the pros and cons of the new process relative to the old one and state which they preferred.

Beneficiary responses reflect many of our own ex ante concerns, but overall are overwhelmingly positive (Table 6). Many recipients report concerns about losing their Smartcards (63 percent NREGS, 71 percent SSP) or having problems with the payment reader (60 percent NREGS, 67 percent SSP). Most beneficiaries do not yet trust the Smartcards system enough to deposit money in their accounts. Yet, strong majorities (over 80 percent in both programs) also agree that Smartcards make payment collection easier, faster, and less manipulable. Overall, 90 percent of NREGS beneficiaries and 93 percent of SSP beneficiaries prefer Smartcards to the status quo, with only 3 percent in either program disagreeing, and the rest neutral.[26]

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[26] These questions were asked when beneficiaries had received a Smartcard and used it to pick up wages or had enrolled for, but not received, a physical Smartcard. We are thus missing data for those beneficiaries who received but did not use Smartcards (10.4 percent of NREGS beneficiaries and 3.4 percent of SSP beneficiaries who

F. *Mechanisms of Impact*

As discussed earlier, the Smartcards intervention involved both technological changes (biometric authentication) and organizational changes (fund flow managed by banks and payments delivered locally by CSPs). The composite nature of the intervention does not allow us to decompose their relative contributions experimentally. However, we have variation in our data in both whether organizational changes took place (because not all GPs converted to the new payments system) and in whether biometric IDs were used for authentication (because not all beneficiaries in converted GPs received or used Smartcards). Hence, we can compare outcomes within the treatment mandals to get a sense of the relative importance of these two components of the Smartcards intervention.[27]

Table 7 presents a nonexperimental decomposition of the total treatment effects along these dimensions. For each of the main outcomes that are significant in the overall ITT estimates (payment process, leakage, and access), we find significant effects only in the carded GPs for six of seven outcomes, suggesting that the new Smartcard-based payment system was indeed the mechanism for the ITT impacts we find.

In addition, we find that in converted GPs, uncarded beneficiaries benefit just as much as carded beneficiaries for payment process outcomes such as time to collect payments and reduction in payment lags (columns 2 and 4). These nonexperimental decompositions provide suggestive evidence that converting a village to the new payments system may have been the key mechanism for the improvements in the process of collecting payments. They also suggest that the implementation protocol followed by GoAP for manual payments to beneficiaries without Smartcards in GPs that were converted to the new system (described in Section IC) was effective at ensuring that uncarded beneficiaries were not inconvenienced.

However, reductions in leakage for both NREGS and SSP beneficiaries are found only among households with Smartcards, and we see no evidence of reduced leakage for uncarded beneficiaries (columns 10 and 12), suggesting that biometric authentication was important for leakage reduction. Note that the lower survey payments to uncarded NREGS beneficiaries in converted GPs could simply reflect less active workers (who will be paid less) being less likely to have enrolled for the Smartcards, and so our main outcome of interest is leakage. The decomposition of program access is less informative for the same reason (since more active workers are more likely to have enrolled in the Smartcard), but we again see that all the increases in access are concentrated among households who had received a Smartcard.

In short, the data suggest that the organizational shift to routing payments through banks and ultimately through village-based CSPs is what drove improvements in

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enrolled). Even if all of these beneficiaries for whom data are missing preferred the old system over Smartcards, approval ratings would be 80 percent for NREGS and 90 percent for SSP.

[27] While only suggestive, this is a policy-relevant question because these are aspects of the intervention that could in principle have been deployed individually. For instance, GoAP could have transitioned responsibility for payments delivery to banks and TSPs without requiring biometric authentication. Alternately, the government could have retained the status quo payment providers and required biometric authentication.

Table 7—Nonexperimental Decomposition of Treatment Effects by Carded Status

Panel A	Time to collect		Payment lag		Survey			
	NREGS (1)	NREGS (2)	NREGS (3)	NREGS (4)	NREGS (5)	NREGS (6)	SSP (7)	SSP (8)
Carded GP	−33 (8.1)		−5 (2.8)		37 (17)		14 (6.2)	
Have SCard, carded GP		−33 (8.4)		−4.4 (3)		152 (24)		24 (7.1)
No SCard, carded GP		−33 (8.6)		−5.9 (2.8)		−55 (17)		−2.2 (9.9)
Not carded GP	4.9 (13)	4.9 (13)	−7.4 (5)	−7.5 (5)	22 (26)	19 (26)	8.3 (9.6)	7.7 (9.6)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	No	No	Yes	Yes	No	No	No	No
BL GP mean	Yes	Yes	No	No	Yes	Yes	Yes	Yes
p-values								
Carded GP=not carded GP	< 0.001		0.45		0.5		0.54	
Have SC=no SC		0.88		0.37		< 0.001		0.017
Adjusted R^2	0.1	0.1	0.17	0.17	0.06	0.11	0.0063	0.0093
Control mean	112	112	34	34	166	166	236	236
Observations	10,120	10,086	14,165	14,165	4,915	4,915	3,131	3,131
Level	Indiv.	Indiv.	Indiv-Week	Indiv-Week	Hhd	Hhd	Hhd	Hhd

Panel B	Leakage				Proportion of Hhds doing NREGS work	
	NREGS (9)	NREGS (10)	SSP (11)	SSP (12)	NREGS (13)	NREGS (14)
Carded GP	−30 (15)		−4.5 (4.4)		0.063 (0.036)	
Have SCard, carded GP		−71 (23)		−12 (4.7)		0.25 (0.043)
No SCard, carded GP		3.1 (14)		7.1 (6.2)		−0.12 (0.044)
Not carded GP	−13 (21)	−12 (21)	−12 (5.8)	−12 (5.8)	0.064 (0.044)	0.056 (0.047)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	No	No	No	No	No	No
BL GP mean	Yes	Yes	Yes	Yes	Yes	Yes
p-values						
Carded GP=not carded GP	0.38		0.21		0.98	
Have SC=no SC		< 0.001		0.0028		< 0.001
Adjusted R^2	0.044	0.052	0.0085	0.013	0.054	0.11
Control mean	−22	−22	15	15	0.48	0.48
Observations	4,915	4,915	3,131	3,131	4,717	4,717
Level	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd

Notes: 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 (NREGS: 5,038 individuals, 2,462 households; SSP: 1,529 households). Not carded GP is a gram panchayat in a treatment mandal that has not yet moved to Smartcard-based payments (NREGS: 2,256 individuals, 1,083 households; SSP: 690 households). Control mean is the mean in the control mandals, which are the omitted category in the regressions (remaining observations). Have SCard, carded GP (NREGS: 2,619 individuals, 1,403 households; 959 households) and No SCard, carded GP (NREGS: 2,419 individuals, 1,059 households; 570 households) 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; ghost households classified as not having Smartcards). A small number of households (NREGS: 79; SSP: 2) and an additional 18 (NREGS) individuals in carded GPs were dropped from the analysis since we could not determine their Smartcard status. Not carded GP is a gram panchayat in a treatment mandal that has not yet moved to Smartcard-based payments (NREGS: 2,256 individuals, 1,083 households; SSP: 690 households). For each outcome, we report the *p*-values from a test of equality of the coefficients on Carded GP and Not carded GP (odd columns), and Have SCard and No SCard (even columns). 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. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at mandal level in parentheses.

the payments process, while the biometric authentication technology is what drove leakage reductions.[28]

G. *Robustness*

In this section we address two main threats to the validity of the leakage results: differential misreporting on our survey, and spillovers. Misreporting may be deliberate, because respondents collude with officials and report higher payments than they are entitled to, or inadvertent due to recall problems. If treatment affects collusion or recall, our results may be biased. We present several pieces of evidence that differential misreporting is not driving the results, and provide further details and additional checks in online Appendix E.

First, note that Figure 4 shows a significant increase mainly in payments received by those who would have otherwise received no payments (relative to the control group). Since there is no reason to expect collusion only with this subgroup (if anything, it would arguably be easier for officials to collude with workers with whom they were already transacting), this pattern is difficult to reconcile with a collusion-based explanation. Since recalling whether one worked or not is easier than recalling the precise payment amount, this pattern also suggests our leakage results are not driven by differential recall.

Second, we conducted independent audits of NREGS worksites in treatment and control mandals during our endline surveys, and counted the number of workers who were present during unannounced visits to worksites. While imprecise, we find an insignificant 39.3 percent increase in the number of workers found on worksites in treatment areas during our audits (online Appendix Table E.2), and cannot reject that this is equal to the 24 percent increase in survey payments reported in panel A of Table 3. Thus, the audits find that the increase in survey payments reported are proportional to the measured increase in workers at worksites, suggesting that misreporting either because of collusion or recall bias is unlikely.

In addition, we directly test for differential rates of false survey responses by asking survey respondents to indicate whether they had ever been asked to lie about NREGS participation—using the "list method"[29] to elicit mean rates of being asked to lie without forcing any individual to reveal their answer—and find no significant difference between the treatment and control groups on this measure (online Appendix Table E.3). Next, we saw that beneficiaries overwhelmingly prefer the new payment system to the old, which would be unlikely if officials were capturing most of the gains. We also find evidence that Smartcards increased wages in the *private* sector, consistent with the interpretation that it made NREGS employment a

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[28] Note that we also cannot distinguish between the impact of having a bank account and biometric authentication since bank accounts were only opened for beneficiaries who enrolled for Smartcards. However, NREGS beneficiaries used to receive payments via their post-office bank accounts *even before* the Smartcard intervention but field assistants would often operate these accounts and control the passbooks, which made it easier for leakage to take place (as described in Section IA). This suggests that a bank account per se may not have been enough to reduce leakage and that the requirement for biometric authentication (which made it difficult for someone else to operate the account), may have been the key to reducing leakage. Finally, all results in Table 7 are robust to including demographic controls and GP fixed effects (Tables F.1 and F.2).

[29] The list method is a standard device for eliciting sensitive information and allows the researcher to estimate population average incidence rates for the sensitive question, though the answers cannot be attributed at the respondent level (Raghavarao and Federer 1979; Coffman, Coffman, and Mazilli Ericson 2013).

more remunerative alternative, and a more credible outside option for workers (see Section IV).

Finally, we use the fact that our survey was spread over two months to check whether there was indeed differential recall. Holding constant the week in which work was actually done, survey lag does not affect the estimated treatment effect on leakage (online Appendix Table E.4). While each of these pieces of evidence is only suggestive, taken together, they strongly suggest that our results do not reflect differential rates of collusion or recall bias in treatment mandals.

So far we have assumed that the Stable Unit Treatment Value Assumption (SUTVA) is satisfied; however, it is possible that one mandal's treatment status affects outcomes in other mandals. Such spillovers could occur if, for example, higher level officials reallocate funding to control mandals as it is easier to steal from them. We address this issue in two ways.

First, we note that there is no reallocation of funds to control mandals from treatment mandals; Figure 2 shows that average official spending is virtually identical in the two in both baseline and endline years. This is inconsistent with "strategic" spillover effects in which senior officials route funds to the places where they are easiest to steal. Second, we test for spatial spillovers by estimating the effect of a measure of exposure to treatment in the neighborhood of each GP (controlling for own treatment status). We find no evidence of spatial spillovers across any of our main outcomes (online Appendix Table E.7).

Online Appendix E explores two additional robustness checks. Since we asked directly about when completed payments were made, we can check that our survey reports do not simply reflect the fact that treatment reduced payment delays so more respondents in treatment areas would have been paid by the time they were surveyed (online Appendix Table E.2). Next, we designed our data collection activities to allow us to test whether the activities themselves affected measurement, and find no indication that they did (Hawthorne effects, online Appendix Table E.9).

IV. Cost-Effectiveness

We estimate the cost-effectiveness of Smartcards as of our endline survey. We begin with costs and efficiency gains and then discuss redistributive effects and potential welfare weightings.

We cost the Smartcard system at the 2 percent commission the government paid to banks on payments in converted GPs. This commission was calibrated to cover *all* implementation costs of banks and TSPs (including the one-time costs of enrollment and issuing of Smartcards), and is a conservative estimate of the incremental social cost of the Smartcard system because it does not consider the savings accruing to the government from decommissioning the status-quo payment system (e.g., the time of local officials who previously issued payments). Using administrative data on all NREGS payments in 2012, and scaling down this figure by one-third (since costs were only paid in carded GPs, and only two-thirds of GPs were carded), we calculate the costs of the new payment system at $4 million in our study districts. The corresponding figure for SSP is $2.3 million.

Efficiency gains include reductions in time taken to collect payment, and reductions in the variability of the lag between doing work and getting paid for it. We

cannot easily price the latter, though it is generally thought to be costly for NREGS workers. To price the former, we estimate the value of time saved conservatively using reported agricultural wages during June, when they are relatively low. We use June wages of Rs 130/day and assume a 6.5 hour work-day (estimates of the length of the agricultural work day range from 5 to 8 hours/day). We assume that recipients collect payments once per spell of work (as they do not keep balances on their Smartcards). Time to collect fell 22 minutes per payment (Table 2), so we estimate the value of time saved at Rs 7.3 per payment. Scaling up by the number of transactions in our study districts, we estimate a total saving of $4.5 million, suggesting that the value of time savings to beneficiaries *alone* may have exceed the government's implementation costs (for NREGS).

Redistributive effects include reduced payment lags (which transfer the value of interest "float" from banks to beneficiaries) and reduced leakage (which transfers funds from corrupt officials to beneficiaries). To quantify the former, we assume conservatively that the value of the float is 5 percent per year, the mean interest rate on local savings accounts.[30] Multiplied by our estimated 5.8 to 10-day reduction in payment lag and scaled up by the volume of NREGS payments in our study districts, this implies an annual transfer from banks to workers of $0.24 million to $0.42 million. To quantify the latter, we multiply the estimated reduction in leakage of 12.7 percent by the annual NREGS wage outlay in our study districts and obtain an estimated annual reduction in leakage of $38.5 million. Similarly, the estimated reduction in SSP leakage of 2.8 percent implies an annual savings of $3.2 million.[31]

While valuing these redistributive effects requires subjective judgments about welfare weights, the fact that they transferred income from the rich to the poor suggests that they should contribute positively to a utilitarian social welfare function with diminishing marginal utility of income. Further, if citizens place a low weight on losses of "illegitimate" earnings to corrupt officials, then the welfare gains from reduced leakage are again large.

The estimates above are based on measuring the direct impact of the Smartcards project on the main targeted outcomes of improving the payment process and reducing leakage. In preliminary work we have also found evidence that the intervention led to significant increases in rural private-sector wages (Muralidharan, Niehaus, and Sukhtankar 2016b), a general equilibrium effect which most likely represents the spillover effects to private labor markets of a better implemented NREGS (Imbert and Papp 2015). Since improving the outside options of rural workers in the lean season was a stated objective of the NREGS (Dréze 2011), these results further suggest that Smartcards improved the capacity of the government to implement NREGS as intended.

V. Conclusion

Recent theoretical work emphasizes the importance of state capacity for economic development. Yet the political case for investments in capacity depends on the

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[30] Given costs of credit-market intermediation, workers may value capital above the deposit rate, implying additional efficiency gains from this transfer. The benchmark rate for microloans in rural Andhra Pradesh, for example, was 26 percent at the time.

[31] Total study district outlays in 2012 were $303.5 million for NREGS wages and $112.7 million for SSP pensions.

magnitude and immediacy of their returns. Advocates argue that improved payments infrastructure may be a particularly high-return investment. Yet there are many reasons to be skeptical: payments reforms must overcome logistical complexity and the resistance of vested interests; they could backfire by excluding the most vulnerable, or by eroding bureaucratic incentives to implement rent-generating programs; or they could simply cost more than they are worth.

This paper has examined these issues empirically, presenting a large scale, as-is evaluation of the introduction of biometric authentication and electronic benefit transfers (through Smartcards) into two major social programs in the Indian state of Andhra Pradesh. We find that implementation concerns are well founded, as only 50 percent of transactions were converted after two years. Yet the poor gained significantly from the reform: beneficiaries received payments faster and more reliably, spent less time collecting payments, received a higher proportion of benefits, and paid less in bribes. These mean gains did not come at the expense of vulnerable beneficiaries, as treatment distributions stochastically dominated those in control. Nor did they come at the expense of program access, which if anything improved. Beneficiaries overwhelmingly preferred the new payment system to the old, and conservative cost-benefit calculations suggest that Smartcards more than justified their costs.

Despite these successes, the Smartcards project was vulnerable to a withdrawal of political support. Local officials (whose rents were being reduced) were much more likely to relay negative anecdotes about Smartcards than positive ones, creating doubts among political leaders about the merits of the Smartcards project. This bias was so pronounced that GoAP nearly scrapped the project in 2013, but ultimately decided not to do so in part because of our results, and data on beneficiary preference for Smartcards. This example highlights the classic political economy problem of how concentrated costs and diffuse benefits may prevent the adoption of social-welfare improving reforms (Olson 1965), and also highlights the policy value of credible impact evaluations with large near-representative samples.

The breadth of beneficiary support for Smartcards also raises the question of why the theoretically posited perverse side-effects did *not* materialize. We suspect that GoAP's decision to *not mandate* biometric authentication played an important role here. Initially, we viewed this as a design loophole, and indeed it may explain the persistence of ghost beneficiaries even in treated areas. Yet it also ensured that legitimate beneficiaries were not excluded even if they were unable to obtain Smartcards or to authenticate. The choice made by GoAP illustrates the general trade-off between Type I (exclusion) and Type II (inclusion) errors in the design of public programs, and our results suggest that it may have been prudent to accept some Type II errors in return for minimizing Type I errors. A similar approach to the ongoing transition to UID-linked benefit transfers in other welfare programs across India, may help prevent exclusion errors during the transition phase of other programs as well.

A further conjecture supported by the AP Smartcards experience is that reducing leakage incrementally, as opposed to trying to eliminate it rapidly, may mitigate potential negative effects. For instance, the fact that NREGS Field Assistants still found it lucrative to implement projects (albeit with lower rents than before) may explain the lack of adverse effects on the extensive margin of program access. The gradual reduction of leakage may have also reduced the risk of political vested interests subverting the entire program.

As usual, extrapolating results to other settings and programs requires care. While AP matches all-India averages for many development indicators, it is also perceived as relatively well administered, and devoted significant resources and senior management time to implementing Smartcards. Implementation might thus be less successful in other settings. On the other hand, the upside might be greater in other places where the problems that Smartcards were designed to address—slow, unpredictable, and leaky payments—are more severe. On net it is unclear whether the social returns would be higher or lower elsewhere.

Similarly, forecasting the evolution of impacts requires care. Benefits could dissipate if interest groups find new ways to subvert the Smartcards infrastructure, or increase if the government continues to increase coverage and plug loopholes. Finally, though we find that Smartcards reduced leakage in both the antipoverty programs we study (with different preprogram structures of identifying beneficiaries and making payments), the extent to which a similar intervention may improve the delivery of other antipoverty programs will clearly depend on the design details of the concerned program, and the preexisting sources of leakage. Overall, our results are best interpreted as demonstrating that in settings where the implementation quality of government programs and policies is poor, there may be *potential* for large returns in a relatively short time period should governments choose to implement similar biometric payment systems for improving the delivery of social programs.

Payments infrastructure may also facilitate future increases in the scale and scope of private sector economic transactions and payments. In the absence of such infrastructure, payments often move through informal networks (Greif 1993) or not at all. Payments systems can thus be seen as public infrastructure—akin to roads, railways, or the Internet, which while initially set up by governments for their own use (e.g., moving soldiers to the border quickly, or improving intragovernment communication) eventually generated substantial benefits for the private sector as well as individual citizens. Our estimates do not capture any such potential benefits, and may therefore be a lower bound on the long-term returns of investing in secure payments infrastructure.

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