ONLINE APPENDIX
Building State Capacity:
Evidence from Biometric Smartcards in India
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A   Further Background on Programs and Smartcard Intervention

This Section provides further information on the two welfare programs - NREGS and SSP - as well as the Smartcards intervention that changed the payment system for the two programs, focusing on supplemental information that was not provided in the main text in order to conserve space.

A.1   NREGS

The National Rural Employment Guarantee Act (NREGA) of 2005 - ex-post renamed the Mahatma Gandhi National Rural Employment Guarantee Act (MNREGA) - mandated federal and state governments to set up employment programs which would guarantee one hundred days of paid employment to any rural household in India. The employment programs, or "schemes", which are collectively referred to as NREGS, are meant to be a self-targeting safety net, with those in need of wage labor accessing work during slack labor seasons. There is no eligibility requirement in order to get work through the program.

The first step in obtaining NREGS employment is to obtain a jobcard. This is a household level document that lists all adult members of the household, and also has assigned pages for recording details of work done and payment owed, including dates of employment and payment. Obtaining a jobcard is generally a simple process, and 65.7% of rural households in Andhra Pradesh have jobcards according to National Sample Survey data; this likely comprises the universe of households who might consider working on NREGS.

Program beneficiaries do (mainly) physical labor at minimum wages. These wages are set at the state level, and can be daily wages or piece rates. Most of the work done in Andhra Pradesh is paid on the basis of piece rates. These rates vary by difficulty of task, and are supposed to enable workers to attain the daily minimum wage with roughly a day's worth of effort. Available tasks depend on the project undertaken, which generally include road construction, field clearing, and irrigation earthworks.

Local village officials are responsible for the implementation of NREGS projects, which are meant to be chosen in advance at a village-wide meeting (the "Gram Sabha"). Project worksites are managed by officials called Field Assistants, who record attendance and output on "muster rolls" and send these to the sub-district for digitization, from where the work records are sent up to the state level, which triggers the release of funds to pay workers. In the status quo, payment was made often by the same Field Assistants in workers' villages, or through the local post office, with no formal authentication procedure required.

Although the program is meant to be demand driven, in practice work is available when there is a project active in the village, and not otherwise. As Figure 2 suggests, there is very high seasonality in when the program is active, with the main periods of activity being the dry season months of April, May and June. Thus the 100 day limit rarely binds per se for particular households, particularly since it may be possible to get around the limit by creating multiple jobcards per household. For example, Imbert and Papp (2015) note that in 2009-10 the median household worked for only 30 days out of the year (mean was 38 days). Moreover, participation varies at high frequency as participants move in and out of the program; Ravi and Engler (2015) find that only about 30% of households in a panel survey of ultra poor households (very likely NREGS participants) in Andhra Pradesh worked in both 2007 and 2009 even though the survey was conducted at the same time of year.

In addition to rationing, other implementation issues are also rife. NREGS workers have to wait over a month to receive payments after working, spend about 2 hours per payment to collect payments, and face much uncertainty over when exactly they will be paid. Of these issues, the long wait to be paid has created some outcry in the media, who have reported on beneficiaries committing suicide because of the inordinate delay (Pai, 2013).

Workers must also worry about whether they will receive the full payment due to them, as corrupt officials may pocket earnings along the way (Niehaus and Sukhtankar, 2013a,b). Leakage from the labor budget may take two forms: underpayment, in which an official simply pays the worker less than she is owed, and over-reporting, in which the official invoices the government for more than what the worker is owed, and pockets the difference. Over-reporting includes invoicing for "ghost" workers, i.e. workers who do not exist, or "quasi-ghost" workers, who exist in the database but have actually not participated on the program at all. Leakage from other parts of the budget is also possible, for example by overinvoicing for materials, but as can be seen in Table E.8, spending on wages is over 91% of the overall budget.

A.2 SSP

The Social Security Pension (SSP) program is a welfare scheme that contrasts with the NREGS on multiple dimensions. First, there are clear eligibility criteria, with pensions restricted to those who are below the poverty line and have restricted earnings ability in some form, due to old age, disability, or being member of a traditional and now outdated profession. Second, if the eligibility criteria are satisfied, the program provides an unconditional cash transfer: these are no work or other requirements. Finally, in contrast to the NREGS in which participation varies at high frequency, SSP beneficiaries are more or less permanent participants after enrollment. The only churn is as a result of death or migration, although these rates are higher than those of the general population given that SSP beneficiaries are targeted for being elderly and disabled.

While there is far less academic research on this program as compared to the NREGS, the little that is available suggests that the program is far better implemented. Dutta et al. (2010) examine the program functioning in Karnataka and Rajasthan, and find that it is well targeted, with poorer households far more likely to obtain benefits than richer households. Moreover, levels of leakage are low: about 17% in Karnataka, less than half comparable rates on an in-kind transfer program (the Public Distribution System) in the same sample.

We did not find any documented evidence on the functioning of the actual payment process for SSP, likely because it is a straightforward process and does not suffer from the types of problems observed in the NREGS programs. The SSP program has a more or less fixed list of beneficiaries, who receive a fixed amount of payment at a fixed time every month (usually in the first week of the month). Our pilots on this issue corroborated this view of the payments process on SSP, and we therefore did not collect data on this aspect of the program.

Overall, we can think of SSP beneficiaries as salaried permanent employees, and NREGS beneficiaries as spot workers on the casual labor market who may or may not show up to obtain work on a given day. The pensioners are paid a fixed wage (entitlement) each month of the year at a specific time of the month (like receiving a monthly paycheck or direct deposit at the end of the month). Meanwhile, NREGS workers are paid based on how much work they did, and this participation varies at high frequency.

A.3 Smartcards intervention

The Smartcards project began in Andhra Pradesh in 2006 in order to improve the payments system for two main welfare schemes in the state. By 2010, Smartcards had been rolled out in 13 out of 21 non-urban districts in the state. The Smartcards system was implemented by private and public sector banks who worked with Technology Service Providers (TSPs)

to manage the technological details last-mile delivery and authentication. Each district was assigned to a single bank via a system of competitive bidding. In Nalgonda district, the winning entity was actually the post office. Banks were paid 2% of every transaction in villages in which they handled the payment system. The bank was responsible for sharing this commission with the TSP as per their contract.

In some cases TSPs subcontracted the actual last-mile delivery to another entity, called a "banking correspondent," (BC) who handled the village level Customer Service Providers (CSPs) who actually made the payments. The TSP or BC was responsible for hiring CSPs as per the criteria laid down by the government, and making sure actual cash was delivered to these local agents. Typically a mandal-level coordinator handled the delivery of cash to CSPs, and assisted in training and providing other support to the CSPs. 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.

Banks opened "no-frills" accounts for NREGS and SSP beneficiaries who had enrolled for Smartcards, and payments were deposited into these accounts. These "no-frills" accounts were not maintained on the "core banking server", which has real-time connectivity and allows accounts to be accessed through any branch or ATM. Rather, the accounts were maintained on small local Point-of-Service (PoS) devices managed by the CSPs. Individual beneficiaries could only access their accounts and be paid through the CSPs who held their accounts. CSPs were supposed to verify beneficiary identity via fingerprint authentication. Beneficiaries in GPs that had switched over to the Smartcard system but did not have a Smartcard were still paid by the CSP, but with manual identification (typically the job-card) and manual acknowledgment of payment (typically with an ink fingerprint collected on a paper ledger to confirm receipt). See Figure 1 and the notes there for details on how NREGS and SSP payments were made before and after the introduction of Smartcards.

Authentication was also performed via the PoS devices, pictured in Figure A.1. The devices did not require internet connectivity in order to authenticate, as they simply matched the fingerprint placed on the device with the biometric information stored on the Smartcard that was inserted into the device at the same time. A truly "smart" card was not required or always issued: one Bank chose to issue paper cards with digital photographs and bar codes while storing biometric data in the Point-of-Service device (as opposed to on the card). All machines were battery powered, and did not need to be plugged in to an external source of electricity. At the end of the day, after cash was dispensed, the machines could be charged back up and connected via GPRS to the banks' network for reconciliation of accounts.

The Smartcards system was a precursor to the nationwide Aadhaar/ biometric Unique ID system. While functionally equivalent for making NREGS and SSP payments, there are some differences between Aadhaar and Smartcards. Most importantly, Aadhaar requires connectivity to a central server for authentication, while Smartcards authentication is offline. Aadhaar can thus be used across various platforms across states, while the use of Smartcards was restricted to making payments for NREGS and SSP beneficiaries within Andhra Pradesh.

B Data

This section describes various data we use in the paper, as well as the collection process involved in obtaining the data.

B.1 Official data

B.1.1 NREGS

We received two types of data from Tata Consultancy Services, which manages the Monitoring and Information System for the Department of Rural Development of the Government of Andhra Pradesh. The first dataset is the full jobcard database, i.e. every single jobcard in the system at the moment of data transfer in each of our study districts. Each jobcard entry in this database contains a listing of family members, including name, sex, age, as well as caste status of the household and address details. The second dataset is the muster roll or disbursement data, which contains details of participation on NREGS for the study period. These details include the jobcard number, dates worked, project worked on, and amount disbursed by the government.

We received both sets of data at two separate points in time: in mid-July 2010 prior to the baseline survey, and mid-July 2012 prior to the endline survey. Note that treatment did not affect the collection or reporting of data in any way, which was managed by the same officials at the village level and the same agency at the state level in all areas at all times over the course of this study. We explain the sampling procedure, which uses both these sets of data, in section C.2 below.

B.1.2 SSP

The official SSP data mirrored those from the NREGS, with one dataset corresponding to the full list of SSP beneficiaries and the second dataset pertaining to recent disbursements. The Department of Rural Development of the Government of Andhra Pradesh directly gave

us both datasets in mid-July 2010 and 2012. The SSP beneficiary list contains data on the individual beneficiary, including name, sex, age, caste states, address, and type of pension. The disbursement list contains beneficiary names and disbursement amounts for May, June, and July. Since benefit amounts do not change over the course of our study and we already have the list of beneficiaries, the only advantage of the disbursement data is that it may reflect slightly more current information on payments, and basically serve as confirmation that money was indeed disbursed by the government. Like the NREGS program, the Smartcards intervention did not affect collection or reporting of official data.

B.2   Survey data

We conducted two rounds of household surveys, a baseline survey in August-September 2010 and an endline survey in August-September 2012. We also conducted a midline survey in September 2011, but that survey collected process data on the progress of the Smartcards intervention rather than data on outcomes. Accordingly, there were only 996 households surveyed in that round as compared to the 7425 at baseline and 8114 at endline. In addition to the household survey, we also had a village-level survey answered by a village elder, schoolteacher, or local official; we do not use these data in this paper. Finally, we also attempted to survey the mandal coordinators and CSPs, but had limited success in reaching them in the time frame that the survey team was in the area, with less than a 50% response rate for these surveys.

The household survey was comprised of seven modules. Module A was the household roster, collecting demographic data on individual members and household characteristics. Module B asked about enrollment and experiences with Smartcards. Module C asked about payments and involvement with the welfare programs, with separate modules for SSP and NREGS samples. Module D asked about consumption, Module E about income, Module F on assets and Module G on other household balance sheet items. Modules B and C, which asked about beneficiary experience with Smartcards and the welfare programs, were asked to the individual beneficiary herself, with separate sets collected for each individual beneficiary within the household. The other modules could be answered by either the male or female head of household.

Table B.1 describes in further detail the construction of each of the main outcome variables we report in the paper.

B.2.1   Matching household records to official records

As explained in detail in the section on sampling below, we sampled NREGS jobcards and individual SSP beneficiaries. Matching SSP beneficiaries to official records is straightforward since there is only one sampled beneficiary. Below we describe the process of matching NREGS official records with our household survey.

Complications may arise in this matching process because of two reasons. First, the set of household members as listed on the sampled NREGS jobcard may be different from the set of household members living under one roof that we surveyed. This complication is relatively easy to fix, as we know the names, ages, and genders of everyone listed on sampled jobcard as well as all members of the surveyed household. Although we surveyed every beneficiary living in the household about their NREGS employment, for our main leakage regressions (Table 3) we can match individuals by name and only include survey records for those individuals listed on the officially sampled jobcard.

The second complication is that the same surveyed household may have more than one jobcard, with potentially different sets of household members listed on each jobcard. This issue is more difficult to deal with, since reverse matching individuals from the surveyed household to the full set of jobcard records is close to impossible.

The following example illustrates these complications more concretely. Suppose that Karthik, Paul, and Sandip live in one household that is surveyed. Only Karthik and Paul are listed on the officially sampled jobcard (let's call it jobcard 1). For our main leakage regressions (Table 3), we do not include Sandip's reported work. It is also possible that (with or without their knowledge) Karthik, Paul, and Sandip are listed on a different job-card (jobcard 2) that is not sampled. Reverse matching Karthik, Paul, and Sandip by name the to full jobcard list is basically impossible. In Section E.1 below we describe how we use a scaling factor to estimate overall leakage rates given that households may hold multiple jobcards.

B.3   Worksite audits

In addition to the household surveys in which we asked NREGS beneficiaries about their work experiences on the program, we also conducted "stealth" worksite audits in which an enumerator visited active worksites on a motorcycle during work hours and simply counted up the number of workers present. These visits happened precisely during the study period - May 28 to July 15 - that we asked about at the endline survey. The visits were conducted in 6 GPs per mandal - 5 GPs which also had household surveys, and 1 additional randomly sampled GP that was not part of our household survey. Thus we have one GP that was

surveyed but not audited, and one GP that was audited and not surveyed, in order to test for effects of each activity on the other (see Section E.5 below for discussion of potential Hawthorne effects).

The stealth audit process was complicated by the fact that we did not want to rely too much on local officials to conduct it, and also because there is generally at least a two week delay in digitizing records and hence being able to electronically access the list of active worksites. Our procedure was to obtain the list of active worksites in a given GP from the official website, send an enumerator on a reconnaissance mission in which he asked villagers about the location of these worksites within the GP, but then wait about a week before the actual worksite visit in order to avoid any response by local officials to the reconnaissance mission itself. Given the lag in reporting and the fact that activity on worksites is fluid, we were not able to always find all listed and sampled worksites. However, the procedure followed was exactly the same in treatment and control mandals.

C   Randomization, sampling, and attrition

C.1   Randomization

Under the terms of the MoU signed with the Government of Andhra Pradesh, we assigned the mandals in our eight study districts to treatment status as follows.

Our study districts contain a total of 405 mandals. Of these, we excluded 2 which were fully urban and so had no NREGS activity, 106 in which the government had already begun rolling out Smartcards at the time the MoU was signed, and 1 for which we were unable to obtain administrative data for stratification. We then randomized the remaining 296 mandals into three groups: treatment, buffer, and control. The government agreed to roll out treatment sequentially across those three groups: first in the treatment group, then in the buffer group, and finally in the control group. We included the buffer group in the design to ensure that we would have adequate time to collect endline data after Smartcards had deployed in treatment mandals, but before they deployed in control mandals.

Because the government was eager to roll out Smartcards quickly, they limited the number of mandals we could allocate to the control group relative to treatment in each district. Specifically, the government agreed to allocate 15 mandals to treatment and 6 to control in each of Adilabad, Anantapur, Khammam, Kurnool, Nellore, and Nalgonda; 12 to treatment and 5 to control in Kadapa, and 10 to treatment and 4 to control in Vizianagaram, for a total of 112 treatment mandals and 45 controls, with the remaining 139 mandals to be allocated to the buffer group. We assigned mandals to group by lottery, stratifying on revenue

division (an administrative grouping of mandals within districts) and the first principal component of a vector of mandal characteristics. Revenue divisions do not serve a major administrative function but provided a convenient way to ensure geographic balance. Since integer constraints meant that we could not ensure that every revenue division has at least one treated and one control mandal, we do not include revenue division fixed effects but rather district fixed effects in our analysis (probability of treatment and control assignment is fixed within district). Including revenue division fixed effects rather than district fixed effects does not affect any of our results qualitatively. The mandal characteristics used were population, literacy rate, number of NREGS jobcards, peak season NREGS employment rate, proportion Scheduled Caste, proportion Scheduled Tribe, proportion SSP disability recipient, and proportion other SSP pension recipient.

Table C.1 reports balance on mandal characteristics from administrative data, including both variables we included in the stratification and others we did not. Unsurprisingly, the samples are well-balanced. Table C.2 reports balance on household characteristics from our baseline survey, which were not available at the time we conducted our randomization. Again the two samples appear well-balanced, with significant differences appearing no more often that would be expected by chance.

C.2 Sampling

For data collection activities we selected a total of 880 GPs: six GPs per mandal in six districts and four GPs per mandal in the remaining two. We sampled fewer GPs per mandal in the latter group because GoAP reallocated these two districts to new banks (and told us we could include them in the study) after we had already begun planning and budgeting, and our funding was limited. We sampled GPs using probability (approximately) proportional to size (PPS) sampling without replacement. As is well known, it is not possible to guarantee strict PPS sampling of more than one unit from a group as the probabilities implied by PPS may exceed one for large units; in these cases we top-coded sampling probabilities at one. A GP typically consists of a few distinct habitations, with an average of 3 habitations per GP; for logistical convenience we selected one habitation within each selected GP using strict PPS sampling.

We selected households within these habitations in the same way for baseline and endline surveys. We sampled a repeated cross-section (rather than a panel) of households to ensure that the endline sample was representative of program participants at that time. In each round of surveys we sampled a total of 10 households in each habitation, ensuring that a field team could complete surveys in one habitation per day. Of these we sampled 6 from

the frame of NREGS jobcards and 4 from the frame of SSP recipients. Sampling in fixed proportions enabled our survey enumerators to specialize in administering NREGS or SSP survey modules. Finally, of the 6 NREGS jobcards we drew 5 from the list of households in which at least one member had worked during May-June according to official records and one household in which no member had worked. We over-sampled the former group in order to increase our precision in estimating leakage, since households that were not paid according to the official records are unlikely to have in fact received funds. At the same time we included some households from the latter group to ensure we could pick up treatment effects on access to work; sampling only among households that had participated in the NREGS would have precluded this. Note that treatment did not change the probability that a household was reported as working in the official data, nor did it change the number of days reported (Table C.3). Finally, we re-weight all our regressions using inverse sampling probabilities to ensure that all estimates are representative of the full frame of jobcards.

For our baseline survey we sampled 8,527 households, of which we were unable to survey or confirm existence of 1,000, while 102 households were confirmed as ghost households, leaving us with a final set of 7,425 households. The corresponding numbers for endline were 8,774 sampled, 287 not confirmed or surveyed, 8 physically missing surveys, and 365 households confirmed as ghosts, leaving us with 8,114 usable surveys with data. Tables C.4 and C.5 show that the households not confirmed or surveyed do not differ across treatment and control from the ones that were surveyed. The relatively high count of omitted households at baseline is due mainly to surveyor errors in coding the status of hard-to-locate households – for example, not confirming status of "ghost" households by writing down names of three neighbors willing to testify that no such household/beneficiary exists. Recognizing these difficulties we simplified the flowchart for coding household status so that in the endline survey we omitted far fewer households, and the 287 we do omit were nearly all left out because we were genuinely unable to trace them. In any case, we use the baseline data only to control for village-level means of outcome variables, so that non-completion of individual baseline surveys affects only the precision and not the consistency of our estimates. Note that ghost households in whose name official payments are made *will* be included in our leakage regressions, increasing observation count in those regressions.

C.3   Sampling frame turnover

The databases of beneficiaries from which we sample (NREGS jobcards and SSP pensioners) evolve over time as new records are created and old ones removed. New jobcards are created in response to applications from eligible (i.e. rural) households; old records may

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be removed from the database when someone dies, migrates out of state, or when families change structure (e.g. divorce) or separate (e.g. joint household splits), in which case each new household gets a new jobcard and old ones are removed. In the case of the SSP, new pensioners are recorded as they are moved off of waiting lists onto active lists, and old pensioners are removed when they die or migrate.

Because of these sources of churn, and because we sample a repeated cross-section of households from the NREGS and SSP frames, it is possible that our estimates of treatment effects confound the effects of Smartcards on a *given* participant with effects on the *composition* of participants. To examine this we test for differences by treatment status in the rate or composition of change in each of our two sampling frames.

In control mandals, 2.4% of NREGS jobcards that were in our baseline frame drop out by endline sampling. On the other hand, 5.9% of jobcards in the endline frame are new entrants. Neither of these rates are significantly different in treatment mandals (Table C.6a) and there is also no difference in the total number of jobcards across treatment and control mandals (Table C.7). This is not particularly surprising as most potential NREGS participants likely had job cards already by the time of Smartcards rolled out: 65% of rural households in Andhra Pradesh had jobcards as of 2010 (authors calculations using National Sample Survey Round 66 (2009-2010)).

Turning to the SSP frame, churn rates are somewhat higher (9.7% dropout rate and 16% entrance rate) but again balanced across treatment and control (Table C.6b). Moreover, new entrants to both frames are similar across control and treatment on demographics (household size, caste, religion, education) and socioeconomics (income, consumption, poverty status) for both NREGS and SSP programs (Table C.8). Finally, the households surveyed at baseline are similar to households surveyed at endline on socio-demographic characteristics such as age composition, literacy, and religion (Table C.9). These results suggest that exposure to the Smartcard treatment did not affect the size or the composition of the frame of potential program participants.

D   Correlates of Smartcard Implementation

This section presents and discusses the correlates of Smartcard implementation at various levels. We start with the selection of districts for the evaluation, and compare them to other districts in the state to assess the extent to which our study districts are representative. Within these districts, the introduction of Smartcards was randomized at the mandal (sub-district) level. However, not all treatment mandals actually implemented Smartcards; within implementing mandals, not all villages converted to the Smartcards-based payment

system; and within converted villages, not all households obtained a Smartcard. This is why our experimental analysis focuses on the intent to treat estimates. Nevertheless, it is of independent interest to understand the correlates of program implementation, as it may help predict roadblocks in implementation elsewhere. We show these results below.

D.1 Districts

As mentioned earlier, the eight study districts were not randomly chosen. Table D.2 (extended version of previously submitted table) compares the study districts to the other rural districts of AP (since NREGS was only implemented in rural areas). Overall, we see that study districts have a slightly lower rural population, but are otherwise similar to the non-study districts on several indicators including demographics, the fraction of agricultural laborers, and village-level facilities, suggesting that our estimates are likely to generalize to all of rural Andhra Pradesh. These similarities also suggest that the main reason for the non-performance of the banks who had initially been assigned these districts was related to bank-specific factors as opposed to district-specific ones .[32]

D.2 Mandals

While mandals that were randomized into treatment status were all supposed to be converted to the Smartcard-based payment system over the course of two-years, in practice only 80% of the mandals got converted (defined as having at least one GP that had converted to the new system). Table D.3 presents correlations between baseline characteristics at the mandal-level and whether a mandal was converted to the new system for NREGS (columns 1-4) and SSP (columns 5-8). We present coefficients from both binary and multiple regressions, and look at both the extensive margin (whether a mandal had converted) and the intensive margin (the fraction of GP's converted).

Overall, we find no noticeable pattern in mandals getting converted for NREGS payments, except that mandals that got converted had slightly lower baseline levels of time to collect payments. For SSP however, we see that mandals that had a higher proportion of residents below the poverty line (BPL) and had a higher total volume of payments were more likely to get converted, and converted more GP's.

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[32]One example of such a bank-specific challenge was the quality of the Bank-TSP partnership. An important reason for non-implementation of Smartcards in some districts was that the banks and TSP's (who were jointly awarded the Smartcard contract for the district) were not able to manage their contracts, commitments, and commissions adequately, which stalled implementation in these districts. Such challenges were more likely to be a function of the organizations rather than a function of specific districts (see Mukhopadhyay et al. (2013)for more details on implementation challenges).

D.3 Villages (GPs)

We find a similar set of correlations with whether a village got converted to the Smartcard system and with the treatment intensity (defined as the fraction of total transactions that are conducted with carded beneficiaries). Table D.4 shows these correlations, and we see that villages with a higher fraction of BPL population were more likely to be carded for both NREGS and SSP and that villages with a larger total amount of SSP payments were more likely to be converted.

D.4 Households

Finally, we present individual-level correlates of having a Smartcard in Table D.5. A similar pattern to the village-level correlates emerges at the individual level for the NREGS, with more vulnerable (lower income, female, scheduled caste, and being more active in NREGS) beneficiaries more likely to have Smartcards. No such pattern is seen for SSP households (perhaps because all participants are vulnerable to begin with, whereas NREGS is a demand-driven program).

Overall, the results in this section are consistent with the idea that banks prioritized enrolling in mandals and GPs with more program beneficiaries and hence more potential commission revenue, while conditional on a village being converted the more active welfare participants were more likely to enroll. Further, since enrollment typically took place in short-duration camps (typically lasting 1-2 days) that beneficiaries had to attend to get enrolled, villages with more (potential) beneficiaries may have also had a greater incentive to make sure that beneficiaries were informed about these camps and encouraged to enroll for a Smartcard.

E   Further leakage results and robustness

E.1   Estimating average leakage

As discussed in the text, we cannot estimate average levels of leakage in our data by simply comparing receipts per *household* with official disbursements per *jobcard*, since there are many more jobcards in Andhra Pradesh than there are households with at least one jobcard. In this section we illustrate with an example how this affects our calculations, and explain in detail how we correct for it.

To illustrate the problem, return to the example introduced earlier in Section B.2.1, where Karthik, Paul, and Sandip form one surveyed household that has two jobcards. Figure E.1

depicts a situation where we sampled Jobcard 1, which only has partial records of payments to Karthik and Paul, but not Jobcard 2, which has additional details of payments made to Paul and Sandip. Actual leakage is the sum of all payments made to the household (Jobcard 1 + Jobcard 2 = 30 + 35 + 50 = 115) minus total receipts by the household ($30 + 20 + 40 = 90$), which equals Rs. 25. If we naively compared household earnings to jobcard disbursements, however, our estimate of leakage would be Rs. -60. Even if we matched workers by name (as we do for all the analysis in the main paper) and removed Sandip, who is not listed on Jobcard 1, we would still under-estimate leakage at Rs. -20.

In principle one possible solution to this problem would be to find Jobcard 2 in the official data, but in practice this is infeasible as it would involve trying to reverse match by name across a very large number of records. Reliably making such matches is particularly difficult given the frequency of misspellings, alternative spellings, errors in transliteration, and similarities between names that are actually different, and by the fact that we do not know what (sub)set of family members may be listed on any given jobcard. We therefore focus instead on adjusting our estimates for the rate at which we under-sample jobcards relative to households. If we knew that the household in this example had two jobcards, we could simply multiply our estimate of official disbursements by 2 to obtain a corrected estimate of total disbursements to the household. While this would not necessarily calculate the correct amount disbursed given that we sampled Jobcard 1, it does yield the correct amount in expectation since we are equally likely to sample Jobcard 1 or Jobcard 2.

The challenge with this approach is that we do not know how many jobcards are associated with any given household. There are two ways we can potentially deal with this: we can estimate the average number of jobcards per household, or ask households directly how many jobcards they have. The latter approach gives us household-specific answers and so is likely to be more precise, but this comes at the cost of three sources of bias. First, households need not know about all the job cards issued in their name, especially cards created by officials for the express purpose of stealing money. Second, households that do have multiple jobcards would possibly be uncomfortable reporting this, as by law each household should have a single jobcard. Finally, our survey methodology may have led to undercounting jobcards; the question that asked about the number of jobcards accompanied instructions to produce jobcards in order to write down the jobcard number, and if all household jobcards were not physically available at the time of the survey, it is possible that enumerators may not have counted them.

Given these biases, a more reliable way of estimating the ratio of jobcards to households is to use independent, representative records from the National Sample Survey, which we can use to estimate the number of jobcards per household at more aggregate levels. We do

this at the district level and estimate an average ratio of 1.9 jobcards per household holding at least one jobcard. (In contrast, surveyed households reported 1.2 jobcards on average to us.) We then scale up official payments to each household using the scaling factor specific to their district. For comparison we calculate the earnings reported by *all* workers in the same household (not just those matched to sampled jobcards, as we do in the main analysis).

The downside of this approach is of course that it introduces a substantial source of noise into the dependent variable and our estimates in order to achieve consistency.[33] To see why, consider a typical household with two jobcards, A and B, on which amounts Y_A and Y_B are paid out. Suppose for purposes of illustration that these variables are iid. If we observed both then the variance of our estimate of the total would be $Var(Y_A + Y_B) = 2Var(Y_A)$. But since we only observe Y_A and have to estimate $Y_A + Y_B$ using $2 \times Y_A$, the variance of our estimate is now $Var(2 \times Y_A) = 4Var(Y_A)$. In other words, our precision is half what it would be if we know both the jobcards associated with the household, as opposed to just one of them.

Using this method, we estimate an average leakage rate of Rs. 80 per household, or 30.7% of average official outlays (Table E.1). We also estimate treatment effects on official and actual payments as well as leakage which are similar to the main results, albeit noisier, with the p-value of the treatment effect on leakage equal to 0.18 (column 7). This is unsurprising given that scaling gives us an unbiased estimate of average leakage, but an inefficient test for changes in leakage relative to the test in Table 3a. We can improve the precision by exploiting the fact that for official payments we observe the jobcard-specific baseline value, and not just the GP average (as we do for actual payments). Since auto-correlation in official payments over time is clearly higher at the jobcard level than at the GP level, this provides a meaningful increase in precision. Controlling for these jobcard specific values reduces the p-value on our leakage estimates to 0.11 (column 8) and increases the magnitude of the estimated coefficient.[34]

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[33]Note that this procedure is not mechanically affected by treatment, as the introduction of Smartcards did not affect the number of jobcards (Table C.7). While the biometric data collected during Smartcard enrollment was intended to be used to de-duplicate the beneficiary database, this was never done as Smartcard enrollment was still far from complete and many jobcards could not be linked to a Smartcard.

[34]Note that controlling for the jobcard-specific baseline value makes no difference to our main results. While it reduces magnitude and increases precision of impact on official payments (so that there is an even more precise zero result), it does not meaningfully change leakage results. We therefore stick with standard specification that uses baseline GP-level means in Table3a for simplicity and consistency with the rest of the main tables.

E.2   Collusion and recall

The main threat to the validity of the leakage results is differential mis-reporting on our survey across treatment and control areas. This may be possible for a number of reasons. First, survey respondents might collude with officials and thus report higher payments than they should have received, and this collusion increases with treatment. Second, treatment may differentially affect recall, if for example respondents in treatment areas are able to better remember payment amounts, or pay more attention because the Smartcards intervention makes payments more salient.

We assuage both concerns through a number of methods. We first report results that suggest both collusion or recall bias are unlikely, and then point to indicators that separately rule out either collusion or recall bias.

Our first piece of evidence comes from the quantile plot of survey payments. As Figure 4 shows, we see a significant increase only 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 sub-group (if anything, it would arguably be easier for officials to collude with workers with whom they were already transacting), this pattern seems harder to reconcile with a collusion-based explanation. Similarly, it is highly unlikely the recall bias takes the form of respondents in treatment areas suddenly remembering that they had worked some versus not worked at all, given how salient NREGS is in the lives of these workers; a more plausible explanation involving recall bias would suggest respondents remember the actual payment more accurately.

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. As described in Section B.3 above, these measures are somewhat noisy. However, we do see an insignificant 39.3% increase in the number of workers found on worksites in treatment areas during our audits (Table E.2), and cannot reject that this is equal to the 24% increase in survey payments reported in Table 3a. Thus, the audits suggest that the increase in survey payments reported are proportional to the increase in workers found at the worksites during our audits, indicating that misreporting either because of collusion or recall bias is unlikely.

Next, 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" to elicit mean rates of being asked to lie without forcing any individual to reveal their answer. 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 et al., 2013). We present a subset of respondents with the following statement - "Members of this household have been asked by officials to lie about the amount of work they did on NREGS") - but respondents do not respond directly about whether the agree with the statement; instead they are also presented with five other statements, and asked to tell us how many of the statements they would agree with. A second subset of respondents is presented with the other five statements, but not the sensitive statement. A third subset is presented with the other five statements along with a statement they would certainly disagree with (in order to determine whether simply presenting more statements leads to more "yes" responses). This statement says "Members of this household have been given the chance to meet with the CM of AP to discuss problems with NREGS." We can then compare the differences in numbers between the first and second groups in treatment and control areas, while adjusting for any increases coming purely from the increase in question numbers. Using simply the differences in numbers between the first and second subsets, we find that at most 15% of control group respondents report having been asked to lie and find no significant difference between the treatment and control groups on this measure (Table E.3). However, data from the third subset suggests that simply asking more questions leads to more "yes" responses, so it is possible that no one in the control group may have been asked to lie.

Other indicators also rule out differential collusion. 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. Finally, we find evidence that Smartcards increased wages in the *private* sector, consistent with the interpretation that it made NREGS employment a more remunerative alternative, and a more credible outside option for workers (see section 5).

With respect to differential recall, we paid close attention to the measurement of data on NREGS employment, learning from and improving on our previous work on this issue (Niehaus and Sukhtankar, 2013a,b). One of the main methods through which we helped respondents recall is the recording of work in the physical jobcard. Neither the format nor the recording of jobcard entries were affected by treatment, and hence differential recall bias appears a priori unlikely. Moreover, the average treatment GP had been treated for 14.5 months (or 2 full NREGS seasons), hence the Smartcards intervention was not that new. Most concretely, we can use the fact that our survey was spread over two months to check whether there was indeed differential recall. If differential recall is driving our results, then, holding constant the week in which work was actually done, the estimated treatment effect on leakage should be more negative (higher in magnitude) if the survey was conducted with a greater lag as opposed to a shorter lag after actual work. Table E.4 shows that there is

no consistent pattern across survey weeks, suggesting that survey lag and differential recall bias do not affect our results.

E.3   Spillovers

E.3.1   Geographic and strategic spillovers

While the main estimates in the paper assume that program performance in a given mandal depends only on that mandal's treatment status, it is possible that our outcomes are also affected by the treatment status in adjacent mandals. Spillovers effects that are "positive" (i.e. have the same sign as direct treatment effects) will simply lead us to under-estimate the direct effects, but spillovers that are "negative" (i.e. opposite sign as direct effects) could lead us to over-estimate the direct effects. For example, if officials in control mandals hear about Smartcards and try to steal more in anticipation of future rollout, we could over-estimate effects on corruption.

First, we note that we see no reallocation of funds away from treated mandals towards control mandals – average official outlays in the two track each other closely from baseline to endline (Figure 2). This is inconsistent with spillover effects in which senior officials route funds to the places where they are easiest to steal.

In addition, we test for spatial spillovers. We first construct a measure of exposure to treatment in the neighborhood of each GP. Specifically, we calculate the fraction of neighboring GPs that are (i) within a radius R of the given GP, and (ii) located in a different mandal, that are treated. We impose condition (ii) because the treatment status of neighboring GPs in the same mandal is identical to own treatment status, so we cannot separately identify their effects.

Tables E.5, E.6, and E.7 report results from this estimation for the payment process and leakage, with NREGS and SSP outcomes separately. Consistent with the fact that the main unit of program implementation is the village (GP), there are no spillovers on the payment process, while the treatment effect remains invariant to the inclusion of our measure of exposure. Moreover, there is no evidence of an effect of neighbors' treatment status on leakage in either NREGS or SSP.

E.3.2   Spillovers to other parts of program budget

Our estimates of leakage are entirely focused on the NREGS labor budget, since Smartcards affected wage payments. It is possible that while leakage from the labor budget is reduced, leakage is displaced to other parts of the overall NREGS budget. In order to test for this

possibility, we collected NREGS budget data disaggregated by category for the months of May, June, and July 2010 and 2012.

To begin with, the data support our decision to focus on the labor budget, as the labor budget is over 91% of the overall budget. This suggests that displacement effects, if any, will be limited. There are no statistically significant effects of treatment on other areas of the budget such as materials or contingency expenses (Table E.8). While we cannot directly measure leakage, since we do not measure actual materials expenditure, the fact that official material expenses did not increase suggests that there was no large-scale displacement.

E.4   Payment timing

A further concern is that survey reports simply reflect the fact that treatment reduced payment delays, so more respondents in treated areas would have been paid at the time of survey, rather than a reduction in leakage. While we minimized this risk by surveying households an average of ten weeks after NREGS work was completed (while the mean payment delay is five weeks), it is still possible that some households had not been paid by the time we surveyed. Since we asked respondents when exactly they got paid for each spell of work, as well as whether they have been paid yet for the spell in question, we can simply verify that the rate of completed payments was identical across treatment and control mandals (Table E.2).

E.5   Hawthorne effects

A final concern might be that the various types of data collection activities affect the reporting of survey or official data. For example, it is possible that officials or workers noticed our stealth auditors, and somehow connected them to our survey (which took place an average of ten weeks after NREGS work was completed), and adjusted their reporting of official quantities or survey responses. We carefully designed our data collection procedures to test for this possibility. First, we can check using the full set of official records whether official payment quantities are affected by the presence of our auditors or surveyors in the village (by comparing villages sampled for these activities to those not sampled). As Table E.9 shows, there is no evidence of effects on official reports. Note that each cell in the table reports results from a separate regression, testing whether conducting audits or surveys overall in a GP affected official records, as well as separately whether reports from that particular week were affected (in case there was only a short-term response). Since these regressions include the full set of official muster data, we can see that the effects are precisely measured and close to zero.

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Second, as Section B.3 described we conducted audits in 5 out of 6 surveyed GPs, and conducted surveys in 5 out of 6 audited GPs, allowing us a comparison GP in each case. Again, Table E.9 shows that there is no evidence of either activity affecting the other. Admittedly the results here are somewhat noisy given limited power, but we have no evidence - quantitative or anecdotal - to suggest that our data collection itself affected measurement.

F   Further heterogeneity results

The two main dimensions of heterogeneous impacts we focus on in the text are the non-parametric plots of quantile treatment effects, and linear interactions between the treatment and the baseline value of the outcome for each outcome studied (4.4). We explore robustness of these results by first including controls and interactions with household and individual level covariates, along with interactions of these variables with the baseline GP-level mean of the outcome. As Table F.1 shows, the results are qualitatively similar to those in Table 7 in the main text, with the single exception that having a Smartcard now makes no additional difference to reducing leakage in the SSP regressions. Further, including GP fixed effects makes no difference to these results either (Table F.2).

In addition, we also examine heterogeneity of impact along other measures of vulnerability such as consumption, measures of socio-economic disadvantage (fraction of the BPL population and belonging to historically-disadvantaged scheduled castes (SC)), as well as the importance of the program to the village (official amounts paid). Overall, we find little consistent evidence of heterogeneity of program impact (Table F.3). Two out of 20 tests in Panel A (NREGS) are significantly different from zero at the 10% level, which is the expected rate of rejection under a null hypothesis of no significant heterogeneity of impacts.

Similarly, for SSP we find no evidence of heterogeneous impacts for either official or survey payments. The only suggestive evidence of heterogeneity is for the time to collect SSP payments but there is no clear pattern here. Time to collect appears to have gone down more in villages that had higher consumption, but also in villages with a greater BPL proportion. We also plot the quantile treatment effects on the time take to collect SSP payments in Figure F.1 and see no significant impact at any percentile of the endline distribution of time to collect payments, which is not surprising given the lack of impact on the mean time to collect SSP payments.

(a) Sample Smartcard

(b) Point-of-Service device

Figure A.1: The technology

Table B.1: Data sources and variable construction for main outcomes

Variable used in Table(s)	Description	Source
Carded GP (T1)	A GP is considered "carded" or "converted" when payments are moved to the Smartcards-based payment system run by Banks/TSPs. This happens separately for NREGS and SSP, usually when 40% of beneficiaries of each program are issued Smartcards. The outcome for SSP is based on May 2012 data as June 2012 data were not available.	DoRD; some missing data downloaded from program websites
Mean fraction carded payments (T1)	This refers to the fraction of payments to NREGS beneficiaries/pension recipients who had Smartcards, averaged over May and June 2012 (NREGS) or May 2012 (SSP, see above).	Program websites
Payments generally carded (village mean) (T1)	Hhd survey section B asks individuals whether they usually swipe their Smartcards or use their fingerprints as ID to collect NREGS/SSP payments. Using habitation and GP sampling probabilities, we construct a weighted GP average for the proportion of payments generally carded.	Hhd survey, section B
Most recent payments carded (village mean) (T1)	Analogous to the question above, only now the outcome question is whether an individual swiped the card or used finger prints when they *last* collected a payment.	Hhd survey, section B
Time to Collect (Min) (T2, T8, F3)	The average duration of time taken to collect payments (including unsuccessful trips). This question is asked once per survey to individual NREGS/SSP beneficiaries. We discarded missing values (not replacing them with zeros).	Hhd survey, section C
Payment Lag: Ave Payment Delay (T2, T8, F3)	This outcome is available for NREGS only, and is constructed for each week that each individual beneficiary worked in the endline study period. We ask for the date (e.g. 7/17/12) that the individual collected her NREGS payment for each study week. We then calculate the # days between the end of the respective study week (e.g. 6/24/12) and the date of the payment. In this example the payment delay is 22 days.	Hhd survey, section C
Payment Lag: Deviation (T2)	Using the average payment delay at the individual-week level, we calculate the mandal-week median payment delay. The outcome is then absolute value of the difference between individual payment delay in week w and the mandal median delay in week w.	Hhd survey, section C
Official (NREGS) (T3, T8, F3)	Our data include start date, end date, and amount paid for every work spell in our study mandals for baseline and endline years. We assign officially recorded spells to correspond to survey study weeks. We obtain average weekly payment by dividing by the # of endline study weeks (7), and aggregate data at the household level. We include in our official measure payments made to ghost households, but do not include sampled jobcards who we were unable to find in our household survey exercise.	Tata Consultancy Services
Survey (NREGS) (T3, T8, F3)	We ask every individual NREGS beneficiary in the household about details of work done and payment received for each of the study weeks, generate average weekly receipts and aggregate data at the household level. We only include payments received by individuals who are listed on the sampled official jobcard. We include payments made to ghost households as 0.	Hhd survey, section C
Official (SSP) (T3, F3)	The SSP data lists every monthly disbursement made to beneficiaries. We take the average disbursement across the months of May, June and July 2012 as the outcome variable. We include in our official measure payments made to ghost beneficiaries.	DoRD

Variable	Description	Source
Survey (SSP) (T3, F3)	We ask SSP receipts how much their pension is supposed to pay every month, and subtract from this amount their reports of bribes they paid to obtain the payment or reductions from the payment amount. We only include data for the sampled SSP beneficiary corresponding to disbursement records.	Hhd survey, section C
Leakage (T3, T8, F3)	Leakage is defined as the difference between "official" and "survey" payments for both NREGS and SSP.	
Ghost hhds (T5)	Ghost households are households (or sampled individual beneficiaries within households) who either do not exist or had permanently migrated before the start of the NREGS study period/ SSP disbursement period. The non-existence, death, or permanent migration must be confirmed by 3 neighbors. Households who had temporarily migrated, were confirmed by neighbors to be "in the area" but could not be found, or whose status could not be confirmed are not considered ghost households and are simply excluded from the analysis.	Hhd survey
Other overreporting: NREGS (T5)	Consider the set of complete surveys, i.e. exclude ghosts, and the outcome variables "official" and "survey" as described above. If "official" is positive but "survey" is 0, the overreporting indicator is 1; otherwise it is 0. We construct this variable at the household level, and exclude any households with 0 official payments in the study period.	
Other overreporting: SSP (T5)	The difference between what the official records ("official") list as disbursed and what a pensioner thinks she is entitled to is the amount overreported	Hhd survey, section C
Bribe to Collect (NREGS) (T5)	We ask each individual beneficiary if she had to pay anything in order collect her earnings for the most recent work spell. We limit estimation to beneficiaries who worked in the study period.	Hhd survey, section C
Underpayment (SSP) (T5)	We ask the sampled pensioner whether she usually has to pay anything in order to obtain work in whether any amount is deducted from the pension amount.	Hhd survey, section C
Proportion of Hhds doing NREGS work (T6)	We classify a individual as working if "survey" (see above) is positive. A household is classified as working if *any* member is classified as working.	Hhd survey, section C
Was any Hhd member unable to get NREGS work in May/December (T6)	This question is asked only once per household. The question asks whether any member of the household wanted to work on NREGS during the month of May (or December) but was unable to obtain employment.	Hhd survey, section C
Is NREGS work available when anyone wants it (T6)	This question is also asked only once per household. It ask whether "in general, can anyone in this village who wants work on NREGS get it?"	Hhd survey, section C
Did you have to pay anything to get this NREGS work (T6)	We ask individuals who worked in the study period whether they had to pay anything to obtain work in the study period.	Hhd survey, section C
Did you have to pay anything to receive this pension (T6)	We ask pension beneficiaries who started received their pension after baseline whether they had to pay anything to start receiving their pension.	Hhd survey, section C
Official disbursement trends in NREGS (F1)	Using the comprehensive muster roll dataset that contains information on all work spells done on NREGS, we aggregate the data to the GP-week level. We take weekly totals (weeks defined by the start of work spell) across all work spells within a GP. The time series plotted in the Figure is the sum across all GPs in a given week.	Tata Consultancy Services

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% (converted) Mandals % (converted) GPs (F2)	A mandal is classified as converted if at least one GP within that mandal is listed as converted. A GP is classified as converted when payments are moved to the Smartcards-based payment system run by Banks/TSPs, usually when 40 % of beneficiaries have been issued a Smartcard. Data are restricted to treatment mandals for both lines	DoRD; some missing data downloaded from program websites
% Carded Payments (F2)	Within converted GPs, we obtain the ratio of carded payments to total payments each month, and create an average of the proportion of carded payments across treatment mandals in the 8 study districts. We multiply this average by the fraction of converted GPs in converted mandals, and by the fraction of converted mandals (see above row), to obtain the overall % of carded payments.	Program websites

“DoRD”: Department of Rural Development, Government of Andhra Pradesh

Program websites: `www.nrega.ap.gov.in` and `www.rd.ap.gov.in`

Table C.1: Balance on baseline characteristics

	Treatment	Control	Difference	p-value
	(1)	(2)	(3)	(4)
	Numbers based on official records from GoAP in 2010			
% population working	.53	.52	.0062	.47
% male	.51	.51	.00023	.82
% literate	.45	.45	.0043	.65
% SC	.19	.19	.0025	.81
% ST	.1	.12	-.016	.42
Jobcards per capita	.54	.55	-.0098	.63
Pensions per capita	.12	.12	.0015	.69
% old age pensions	.48	.49	-.012	.11
% weaver pensions	.0088	.011	-.0018	.63
% disabled pensions	.1	.1	.0012	.72
% widow pensions	.21	.2	.013	.039
	Numbers based on 2011 census rural totals			
Population	45580	45758	-221	.91
% population under age 6	.11	.11	-.00075	.65
% agricultural laborers	.23	.23	-.0049	.59
% female agri. laborers	.12	.12	-.0032	.52
% marginal agri. laborers	.071	.063	.0081	.14
	Numbers based on 2001 census village directory			
# primary schools per village	2.9	3.2	-.28	.3
% village with medical facility	.67	.71	-.035	.37
% villages with tap water	.59	.6	-.007	.88
% villages with banking facility	.12	.16	-.034	.021
% villages with paved road access	.8	.81	-.0082	.82
Avg. village size in acres	3392	3727	-336	.35

This table compares official data on baseline characteristics across treated and control mandals. Column 3 reports the difference in treatment and control means, while column 4 reports the p-value on the treatment indicator from simple regressions of the outcome with district fixed effects as the only controls. A "jobcard" is a household level official enrollment document for the NREGS program. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Indian Constitution. "Old age", "weaver", "disabled" and "widow" are different eligibility groups within the SSP administration. "Working" is defined as the participatin in any economically productive activity with or without compensation, wages or profit. "Main" workers are defined as those who engaged in any economically productive work for more than 183 days in a year. "Marginal" workers are those for whom the period they engaged in economically productive work does not exceed 182 days. The definitions are from the official census documentation. The last set of variables is taken from 2001 census village directory which records information about various facilities within a census village (the census level of observation). "# primary schools per village" and "Avg. village size in acres" are simple mandal averages - while the others are simple percentages - of the respective variable (sampling weights are not needed since all villages within a mandal are used). Note that we did not have this information available for the 2011 census and hence use the 2001 data.

Table C.2: Balance on baseline characteristics: household survey

	NREGS				SSP			
	Treatment	Control	Difference	p-value	Treatment	Control	Difference	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hhd members	4.8	4.8	.022	.89	4.1	4.2	-.15	.41
BPL	.98	.98	.0042	.73	.98	.97	.0039	.65
Scheduled caste	.22	.25	-.027	.35	.19	.23	-.038	.08
Scheduled tribe	.12	.11	.0071	.81	.097	.12	-.022	.46
Literacy	.42	.42	.0015	.93	.38	.39	-.012	.42
Annual income	41,482	42,791	-1,290	.52	33,622	35,279	-2,078	.34
Annual consumption	104,717	95,281	8,800	.39	74,612	77,148	-3,342	.56
Pay to work/enroll	.011	.0095	.00099	.82	.054	.07	-.016	.26
Pay to collect	.058	.036	.023	.13	.06	.072	-.0078	.81
Ghost Hhd	.012	.0096	.0019	.75	.012	.0096	.0019	.75
Time to collect	156	169	-7.5	.62	94	112	-18	.03
Owns land	.65	.6	.058	.06	.52	.48	.039	.18
Total savings	5,863	5,620	3.7	1.00	4,348	3,670	729	.30
Accessible (in 48h) savings	800	898	-105	.68	704	9,576	-9,211	.29
Total loans	62,065	57,878	5,176	.32	43,161	43,266	-813	.81
Owns business	.21	.16	.048	.02	.16	.19	-.025	.29
Number of vehicles	.11	.12	-.014	.49	.1	.093	.0039	.83
Average payment delay	28	23	.036	.99				
Payment delay deviation	11	8.8	-.52	.72				
Official amount	172	162	15	.45				
Survey amount	177	189	-10	.65				
Leakage	-5.1	-27	25	.15				
NREGS availability	.47	.56	-.1	.02				
Hhd doing NREGS work	.43	.42	.0067	.85				
NREGS days worked, June	8.3	8	.33	.65				
NREGS hourly wage, June	13	14	-1.3	.13				
NREGS overreporting	.15	.17	-.015	.55				
# addi. days hhd wanted NREGS work	15	16	-.8	.67				

This table compares household survey data on baseline characteristics across treatment and control mandals. Columns 3 and 6 report the difference in treatment and control means, while columns 4 and 8 report the p-value on the treatment indicator, all from simple regressions of the outcome with district fixed effects as the only controls. "BPL" is an indicator for households below the poverty line. "Pay to work/enroll" refers to bribes paid in order to obtain NREGS work or to start receiving SSP pension. "Pay to Collect" refers to bribes paid in order to receive payments. "Ghost HHD" is a household with a beneficiary who does not exist (confirmed by three neighbors) but is listed as receiving payment on official records. "Time to Collect" is the time taken on average to collect a benefit payment, including the time spent on unsuccessful trips to payment sites, in minutes. "Accessible (in 48h) savings" is the amount of savings a household could access within 48h. "Payment delay deviation" is the absolute value of the difference between an individuals payment delay and the mandal median. "NREGS availability" is an indicator for whether a household believes that anybody in the village could get work on NREGS when they want it. "NREGS overreporting" is the incidence of jobcards that had positive official payments reported but zero survey amounts (not including ghosts). Standard errors are clustered at the mandal level.

Table C.3: Impacts on official records of NREGS participation

	Worked on NREGS (%)		Days worked on NREGS	
	(1)	(2)	(3)	(4)
Treatment	.015	.016	.32	.39
	(.016)	(.016)	(.32)	(.32)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.03	.03	.04	.02
Control Mean	.4	.36	5.9	4.9
N. of cases	2116302	900404	2116302	900404
Level	Hhd	Hhd	Hhd	Hhd
Data used	All GPs	Survey GPs	All GPs	Survey GPs

This table analyzes whether treatment affected the extensive margin of work reported in official records. The unit of analysis is the jobcard. The outcome in columns 1 and 2 is a binary variable equal to 1 if any household member listed on the jobcard is reported to have worked in the endline study period between May 28 and July 15, 2012. The outcome in columns 3 and 4 is the number of household-days worked during the same period as recorded on the official jobcard. Columns 2 and 4 restrict the sample to the 880 GPs sampled for the household survey. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as control variable as well as district fixed effects. Standard errors clustered at mandal level in parentheses.

Table C.4: Comparing surveyed and non-surveyed sampled households - NREGS

	# members	% female	Avg. age	ST/SC	Worked in May	#BL spells per member	#EL spells per member	Avg. implied daily wage BL	Avg. implied daily wage EL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	-.038	-.011	-.57	-.017	-.024	-.0026	.0053	-3.1	-3.7
	(.052)	(.01)	(.28)	(.028)	(.03)	(.01)	(.013)	(1.9)	(2.5)
Non-surveyed hhd	-.31	-.00076	-.095	-.023	-.27	-.048	-.064	2.5	1.2
	(.17)	(.052)	(1.1)	(.057)	(.068)	(.035)	(.047)	(3.9)	(5.3)
Non-surveyed hhd X treatment	-.35	.014	1.4	.029	.025	.058	-.027	2.4	-7.5
	(.19)	(.061)	(1.4)	(.079)	(.084)	(.044)	(.052)	(5.6)	(6.1)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.01	.01	.01	.05	.03	.01	.01	.10	.10
Control Mean	2.6	.25	37	.4	.6	.15	.21	97	106
N. of cases	5078	5078	5078	5078	5078	5078	5078	1716	2450

This table compares sampled NREGS households who were surveyed to sampled NREGS households who could not be surveyed (excluding confirmed ghost households), using official data. Reasons for missing surveys could be temporary migration, repeated absence on survey dates or refusal to participate in the survey. There were 4943 completed and 135 unsuccessful surveys. "Non-surveyed hhd" is an indicator variable equal to 1 if a household was not surveyed and 0 otherwise, while "Non-surveyed hhd X treatment" is an interaction term. All outcomes are taken from official jobcard records (demographics of workers listed on jobcard) and muster rolls (information on work spells completed by members on the jobcard). "Worked in May" is an indicator for whether work was reported on the jobcard for May 2012. The periods "BL" and "EL" refer to May 31 - July 4, 2010 and May 28 - July 15, 2012 respectively. "Work spells per member" is the total number of distinct work spells reported on a jobcard divided by the number of members listed on the jobcard. "Avg. implied daily wage" is the total amount earned on a jobcard during the respective period divided by the total number of work days during the respective period. Note that in column 8-9 only jobcards with positive numbers of work days in the respective period were used. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as control variable. Standard errors clustered at mandal level in parentheses.

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Table C.5: Comparing surveyed and non-surveyed sampled households - SSP

	% female	Age	% ST/SC	% Old age	% Widow	% Disabled	% Abhayahastam or Toddy Tappers	Avg. disburs. in 2010	Avg. disburs. in 2011	Avg. disburs. in 2012	Avg. disburs. during BL	Avg. disburs. during EL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	.043	.36	.012	.005	-.00037	.0038	-.0085	-3.5	-2.6	10	-4.5	8.1
	(.019)	(.6)	(.028)	(.015)	(.014)	(.012)	(.013)	(5.2)	(4.2)	(4.1)	(5.8)	(4.1)
Non-surveyed hhd	-.1	4.6	.19	.16	-.041	-.11	-.014	-28	-36	-36	-26	-44
	(.077)	(1.8)	(.097)	(.059)	(.057)	(.031)	(.04)	(14)	(14)	(15)	(15)	(16)
Non-surveyed hhd X treatment	.14	-.58	-.18	-.047	.075	.002	-.03	19	-2	8.3	23	-3.1
	(.088)	(2.2)	(.11)	(.077)	(.073)	(.036)	(.044)	(16)	(15)	(17)	(17)	(20)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.01	.00	.06	.01	.00	.00	.01	.00	.01	.01	.00	.01
Control Mean	.58	58	.32	.52	.27	.12	.088	204	258	256	197	253
N. of cases	3317	3317	3317	3317	3317	3317	3317	3317	3317	3317	3317	3317

This table compares sampled SSP households who were surveyed to sampled SSP households who could not be surveyed (excluding confirmed ghost households), using official data. Reasons for missing surveys could be temporary migration, repeated absence on survey dates or refusal to participate in the survey. There were 3171 completed surveys and 152 unsuccessful surveys (another 6 surveys were dropped since no beneficiary in the household could be name-matched to name on the pension card). 'Non-surveyed hhd" is an indicator variable equal to 1 if a household was not surveyed and 0 otherwise, while "Non-surveyed hhd X treatment" is an interaction term. Outcomes in columns 1-3 are taken from the official database of registered pension beneficiaries. Columns 4-7 compare the proportion of pensioners within a certain eligibility category across groups. Column 7 in particular compares the prevalence of Abhayatstham pension - a pension scheme for women active in self-help groups - and "Toddy Tappers" - paid to the historic trade of palm wine producers. Columns 8 to 10 compare official disbursements averaged across all 12 months of the respective year while columns 11 to 12 compare average disbursements during months May, June and July of the respective year (where "BL" refers to 2010 and "EL" to 2012). All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as control variable. Standard errors clustered at mandal level in parentheses.

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Table C.6: Attrition from and entry into sample frames

(a) NREGS

	Treatment	Control	Difference	p-value
Attriters from Baseline	.013	.024	-.012	.19
Entrants in Endline	.06	.059	.0018	.74

(b) SSP

	Treatment	Control	Difference	p-value
Attriters from Baseline	.097	.097	-.000016	1
Entrants in Endline	.17	.16	.0056	.37

These tables compare the entire NREGS sample frame – i.e., all jobcard holders – and the entire SSP beneficiary frame across treatment (column 1) and control (column 2) mandals. Column 3 reports the difference in treatment and control means, while column 4 reports the p-value on the treatment indicator, both from simple regressions of the outcome with district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization as the only controls. Row 1 presents the proportion of NREGS jobcards and SSP beneficiaries that dropped out of the sample frame between baseline and endline. Row 2 presents the proportion that entered the sample frame between baseline and endline. Standard errors are clustered at the mandal level.

Table C.7: Endline number of jobcards

	Endline # of JCards	
	(1)	(2)
Treatment	8.5	5.6
	(7.5)	(7.3)
District FE	Yes	Yes
Baseline Level	Yes	Yes
Adj R-squared	.97	.97
Control Mean	664	675
N. of cases	2897	874

This table examines whether treatment led to any changes in the number of NREGS jobcards at the GP-level between baseline (2010) and endline (2012). It uses data from the full jobcard data frame in treatment and control mandals. Column 1 includes all GPs within study mandals. Column 2 shows only GPs sampled for our household survey. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors are clustered at the mandal level.

Table C.8: Compositional changes in sample at endline

(a) NREGS

	# members	Hindu	SC	Any mem. reads	BPL	Total consump.	Total income	Owns land
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	.042	-.024	.022	-.031	-.0022	-767	7201	.054
	(.11)	(.018)	(.022)	(.027)	(.023)	(4653)	(3839)	(.024)
EL entrant	-.16	.0094	.03	.065	.067	-10564	-3281	-.052
	(.25)	(.047)	(.077)	(.049)	(.043)	(6874)	(10373)	(.12)
EL entrant X treatment	.12	-.024	-.082	-.089	-.049	5029	16803	.056
	(.34)	(.058)	(.088)	(.071)	(.057)	(9075)	(14119)	(.14)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.02	.07	.03	.01	.01	.01	.04	.01
Control Mean	4.3	.93	.19	.85	.89	90317	69708	.59
N. of cases	4909	4909	4909	4869	4887	4902	4875	4887

(b) SSP

	# members	Hindu	SC	Any mem. reads	BPL	Total consump.	Total income	Owns land
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.0067	.02	-.027	-.047	-.0014	-1511	4607	.0029
	(.12)	(.021)	(.021)	(.027)	(.018)	(4006)	(4010)	(.032)
EL entrant	-.035	.0083	-.08	-.02	.076	-1883	-1695	.095
	(.27)	(.041)	(.034)	(.043)	(.026)	(4010)	(4565)	(.056)
EL entrant X Treatment	-.081	.0008	.049	.067	-.051	7688	6028	-.051
	(.3)	(.046)	(.04)	(.055)	(.034)	(5569)	(5664)	(.068)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.04	.05	.02	.02	.01	.02	.07	.02
Control Mean	3.5	.89	.21	.64	.87	63792	52763	.52
N. of cases	3152	3152	3152	3113	3131	3150	3137	3142

These tables show that new entrants to the NREGS and SSP samples are no different across treatment and control groups. "EL entrant" is an indicator for a household that entered the sample for the endline survey but was not in the baseline sample frame. "EL entrant X treatment" is the interaction between the treatment indicator and the endline entrant indicator, and the coefficient of interest in these regressions. "# members" is the number of household members. "Hindu" is an indicator for the household belonging to the hindu religion. "SC" is an indicator for the household belonging to a "Scheduled Caste" (historically discriminated-against caste). "Any mem. reads" is a proxy for literacy. "BPL" is an indicator for the household being below the poverty line. "Total consump." is total consumption. "Total income" is total household income with the top .5% percentile of observations censored. "Owns land" is an indicator for whether the household owns any land. 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.

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Table C.9: Comparing characteristics of surveyed households at baseline and endline

(a) NREGS

	# hhd members	% non-working age	% children	% female members	% Hindu	% Muslim	% Christian	% SC	% ST	% hhd head is widow	% members can read	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	-.11	-.0063	-.0011	-.0012	-.0053	.0078	-.0056	-.014	.0067	.012	.0025	-.0003
	(.1)	(.017)	(.0087)	(.016)	(.012)	(.008)	(.01)	(.024)	(.025)	(.02)	(.013)	(.0037)
EL survey	-3.8	1	.28	1.6	.017	-.0073	-.0089	-.0035	.018	-.032	-.17	-.002
	(.09)	(.04)	(.019)	(.046)	(.0095)	(.0069)	(.0086)	(.019)	(.012)	(.021)	(.013)	(.0042)
EL survey X treatment	.11	.068	.043	-.00071	-.0062	.0027	.007	.0043	-.013	-.017	-.0054	-.0031
	(.1)	(.049)	(.026)	(.055)	(.013)	(.0081)	(.011)	(.022)	(.013)	(.025)	(.016)	(.0048)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	.63	.27	.079	.49	.059	.015	.049	.031	.12	.0038	.1	.00031
BL Control Mean	4.8	.35	.098	.51	.9	.039	.052	.26	.12	.15	.61	.014
N. of cases	9555	9555	9555	9555	9555	9555	9555	9532	9532	8104	9512	9555

(b) SSP

	# hhd members	% non-working age	% children	% female members	% Hindu	% Muslim	% Christian	% SC	% ST	% hhd head is widow	% members can read	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	-.23	.0037	-.004	.032	.013	.0055	-.017	-.028	-.022	.054	-.03	-.00022
	(.17)	(.023)	(.01)	(.023)	(.014)	(.011)	(.0094)	(.022)	(.025)	(.027)	(.016)	(.0055)
EL survey	-3.3	.93	.18	1.3	.0037	.014	-.012	-.032	.0021	-.072	-.16	-.0087
	(.16)	(.063)	(.023)	(.055)	(.012)	(.011)	(.0095)	(.018)	(.014)	(.03)	(.016)	(.0056)
EL survey X treatment	.22	.033	.013	-.049	.011	-.024	.0077	.03	.0096	-.037	.03	-.0013
	(.17)	(.071)	(.027)	(.066)	(.014)	(.012)	(.011)	(.022)	(.016)	(.036)	(.02)	(.0064)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	.45	.23	.046	.37	.043	.022	.024	.019	.1	.015	.056	.0027
BL Control Mean	4.3	.39	.065	.57	.89	.049	.055	.23	.14	.43	.48	.015
N. of cases	5931	5931	5931	5931	5931	5931	5931	5922	5922	5327	5886	5931

The regressions above compare households surveyed at baseline versus those surveyed at endline on basic socio-economic characteristics. The dependent variables are: the number household members, the percentage of members younger than 18 or older than 65, the percentage of members younger than 7, the percentage of households of the respective religion or of the respective category (columns 5 to 9), the percentage of households whose head is a widow and finally the percentage of household members who can read. "EL survey" is a binary variable indicating an observation from the endline survey. "EL survey X treatment" is an interaction effect of being surveyed at endline and being in treatment. "BL control mean" is the mean of the outcome within the control group at baseline. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as control variable. Standard errors clustered at mandal level in parentheses.
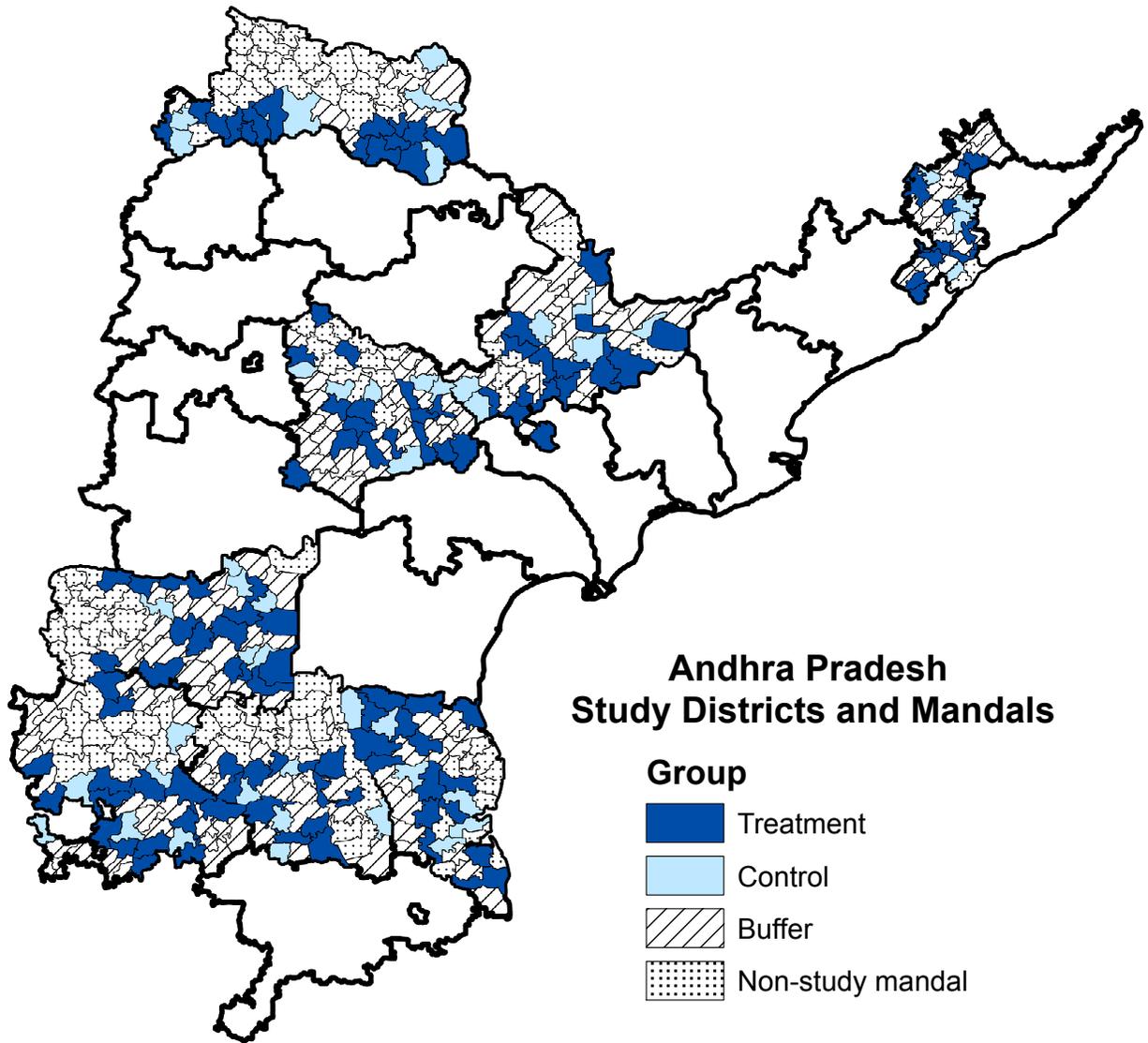
Figure C.1: Study districts with treatment and control mandals

This map shows the 8 study districts - Adilabad, Anantapur, Kadapa, Khammam, Kurnool, Nalgonda, Nellore, and Vizianagaram - and the assignment of mandals (sub-districts) within those districts to one of four study conditions. Mandals were randomly assigned to one of three waves: 112 to wave 1 (treatment), 139 to wave 2, and 45 to wave 3 (control). Wave 2 was created as a buffer to maximize the time between program rollout in treatment and control waves; our study did not collect data on these mandals. A "non-study mandal" is a mandal that did not enter the randomization process because the Smartcards initiative had already started in those mandals (109 out of 405). Randomization was stratified by district and by a principal component of mandal characteristics including population, literacy, Scheduled Caste and Tribe proportion, NREGS jobcards, NREGS peak employment rate, proportion of SSP disability recipients, and proportion of other SSP pension recipients.

Table D.1: Comparison of study districts and other AP districts

	Study Districts	Other AP	Difference	p-value
	(1)	(2)	(3)	(4)
	Numbers based on 2011 census rural totals			
% population rural	.74	.73	.0053	.89
Total rural population	2331398	2779458	-448060	.067
% male	.5	.5	.0026	.22
% population under age 6	.11	.11	.0047	.35
% ST	.18	.19	-.0094	.69
% SC	.13	.083	.045	.25
% literate	.52	.54	-.022	.37
% working population	.53	.51	.016	.23
% female working population	.24	.22	.015	.34
% main agri. laborers	.23	.22	.0094	.65
% main female agri. laborers	.12	.1	.014	.29
% marginal agri. laborers	.067	.064	.0032	.64
	Numbers based on 2001 census village directory			
# primary schools per village	2.3	2.4	-.14	.68
% villages with medical facility	.56	.67	-.11	.13
% villages with tap water	.53	.56	-.037	.76
% villages with banking facility	.11	.2	-.094	.32
% villages with paved road access	.72	.78	-.06	.39

This table compares characteristics of our 8 study districts and the remaining 13 non-urban (since NREGS is restricted to rural areas) districts in erstwhile Andhra Pradesh, using data from the 2001 and 2011 censuses. Column 3 reports the difference in means, while column 4 reports the p-value on a study district indicator, both from simple regressions of the outcome with no controls. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Indian Constitution. "Working" is defined as participating in any economically productive activity with or without compensation, wages or profit. "Main" workers are defined as those who engaged in any economically productive work for more than 183 days in a year. "Marginal" workers are those for whom the period they engaged in economically productive work does not exceed 182 days. Note that the difference in "main" and "marginal" workers only stems for different periods of work. An "agricultural laborer" is a person who works for compensation on another person's land (compensation can be paid in money, kind or share). The definitions are from the official census documentation. The second set of variables is taken from 2001 census village directory which records information about various facilities within a census village (the census level of observation). "# primary schools per village" and "Avg. village size in acres" are simple district averages - while the others are simple percentages - of the respective variable (sampling weights are not needed since all villages within a district are used). Note that we did not have this information available for the 2011 census and hence use the 2001 data.

Table D.2: Comparison of study mandals and dropped mandals

	Mandals considered for randomization	Mandals not considered	Difference	p-value
	(1)	(2)	(3)	(4)
	Numbers based on 2011 census rural totals			
% population rural	.89	.89	-.015	.58
Total rural population	46380	45582	-1580	.27
% male	.5	.5	.00039	.64
% population under age 6	.11	.12	-.005	.00028
% SC	.19	.18	.014	.031
% ST	.12	.14	-.026	.095
Literacy rate	.53	.51	.01	.061
% working population	.53	.53	-.0011	.8
% female working population	.24	.24	-.0039	.28
% main agri. laborers	.23	.21	.0019	.77
% female main agri. laborers	.12	.11	-.0019	.59
% marginal agri. laborers	.069	.066	.0043	.24
	Numbers based on 2001 census village directory			
# primary schools per village	2.9	2.6	.31	.052
% village with medical facility	.68	.62	.044	.082
% villages with tap water	.6	.62	-.052	.081
% villages with banking facility	.13	.12	.0015	.87
% villages with paved road access	.78	.76	.018	.49
Avg. village size in acres	3404	3040	298	.12

This table compares characteristics of the 296 mandals that entered the randomization (and were randomized into treatment, control and buffer) to the 108 rural mandals in which the Smartcard initiative had begun prior to our intervention, using data from the 2001 and 2011 censuses. One mandal (Kadapa mandal in Kadapa district, i.e. the district's capital) is excluded since it is fully urban (hence has no NREGS). Column 3 and 4 report the point estimate and the respective p-value associated with entering the randomization pool from a simple regression of the outcome and the respective indicator variable. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Indian Constitution. "Working" is defined as the participating in any economically productive activity with or without compensation, wages or profit. "Main" workers are defined as those who engaged in any economically productive work for more than 183 days in a year. "Marginal" workers are those for whom the period they engaged in economically productive work does not exceed 182 days. Note that the difference in "main" and "marginal" workers only stems for different periods of work. An "agricultural laborer" is a person who works for compensation on another person's land (compensation can be paid in money, kind or share). The definitions are from the official census documentation. The second set of variables is taken from 2001 census village directory which records information about various facilities within a census village (the census level of observation). "# primary schools per village" and "Avg. village size in acres" are simple district averages - while the others are simple percentages - of the respective variable (sampling weights are not needed since all villages within a district are used). Note that we did not have this information available for the 2011 census and hence use the 2001 data.

Table D.3: Baseline covariates and program implementation at mandal level

	NREGS				SSP			
	Mandal converted		Intensity		Mandal converted		Intensity	
	(1) Binary	(2) Multiple	(3) Binary	(4) Multiple	(5) Binary	(6) Multiple	(7) Binary	(8) Multiple
Time to Collect (1 hr)	-.093 (.043)	-.1 (.044)	-.0085 (.026)	-.013 (.027)	.061 (.058)	.072 (.057)	.012 (.047)	.022 (.047)
Official Amount (Rs. 100)	.019 (.028)	.011 (.038)	.016 (.017)	.012 (.023)	.22 (.084)	.26 (.088)	.14 (.069)	.17 (.072)
Survey Amount (Rs. 100)	.023 (.033)	.015 (.044)	.016 (.02)	.0042 (.027)	-.021 (.045)	-.092 (.047)	-.021 (.036)	-.071 (.039)
SC Proportion	-.032 (.22)	.009 (.22)	-.085 (.13)	-.058 (.14)	-.072 (.2)	.083 (.19)	-.027 (.16)	.072 (.16)
BPL Proportion	.96 (1.2)	1.2 (1.2)	.71 (.69)	.64 (.72)	1.1 (.68)	1.5 (.69)	.94 (.55)	1.2 (.57)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared		.14		.57		.22		.44
N. of cases	112	112	112	112	112	112	112	112

This tables analyzes the effects of baseline covariates on endline program implementation in treatment areas. The columns labeled "binary" show coefficients from regressions with each covariate regressed separately. Hence every cell in columns 1, 3, 5 and 7 shows the result from a separate regression. In contrast, the columns labeled "multiple" run one single regression with all covariates. A "converted mandal" is a mandal in which at least one GP has converted to Smartcard based payments. As of July 2012, 92 of 112 (82%) mandals were converted for NREGS payments, while 100 of 112 (93%) were converted for SSP payments. "Treatment intensity" is the mandal mean of the proportion of transactions done with carded beneficiaries in carded GPs. All regressors are mandal-level averages. "Time to collect (1 hr)" is the average time taken to collect a payment (in hours), including the time spent on unsuccessful trips to payment sites. "Official amount (Rs. 100)" refers to amounts paid as listed in official records. "Survey amount (Rs. 100)" refers to payments received as reported by beneficiaries. "SC proportion" is GP proportion of Scheduled Caste households. "BPL proportion" is GP proportion of households below the poverty line. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization.

Table D.4: Baseline covariates and program implementation at GP level

	NREGS				SSP			
	Carded GP		Intensity		Carded GP		Intensity	
	(1) Binary	(2) Multiple	(3) Binary	(4) Multiple	(5) Binary	(6) Multiple	(7) Binary	(8) Multiple
Time to Collect (1 hr)	-.018 (.015)	-.022 (.014)	-.0007 (.01)	-.0035 (.01)	-.021 (.027)	-.022 (.026)	-.016 (.022)	-.016 (.021)
Official Amount (Rs. 100)	-.0093 (.014)	-.0032 (.017)	.0053 (.0094)	.0065 (.011)	.056 (.028)	.095 (.033)	.035 (.024)	.059 (.03)
Survey Amount (Rs. 100)	-.011 (.014)	-.011 (.017)	.0034 (.011)	-.0034 (.012)	-.0092 (.012)	-.023 (.012)	-.0045 (.0087)	-.013 (.0091)
SC Proportion	-.067 (.078)	-.037 (.077)	-.061 (.054)	-.041 (.054)	-.069 (.055)	-.046 (.051)	-.035 (.042)	-.023 (.039)
BPL Proportion	.81 (.37)	.91 (.45)	.51 (.3)	.55 (.34)	.38 (.12)	.43 (.14)	.25 (.092)	.27 (.1)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared		.26		.45		.35		.43
N. of cases	627	625	627	625	586	584	586	584

This tables analyzes the effects of baseline covariates on endline program implementation at the GP-level. The columns labeled "binary" show coefficients from regressions with each covariate regressed separately. Hence every cell in columns 1, 3, 5 and 7 shows the result from a separate regression. In contrast, the columns labeled "multiple" run one single regression with all covariates. "Carded GP" is a gram panchayat that has converted to Smartcard based payment, which usually happens once 40% of beneficiaries have been issued a card. "Treatment intensity" is the proportion of transactions done with carded beneficiaries in carded GPs. All regressors are GP-level averages. "Time to collect (1 hr)" is the average time taken to collect a payment (in hours), including the time spent on unsuccessful trips to payment sites. "Official amount (Rs. 100)" refers to amounts paid as listed in official records. "Survey amount (Rs. 100)" refers to payments received as reported by beneficiaries. "SC proportion" is GP proportion of Scheduled Caste households. "BPL proportion" is GP proportion of households below the poverty line. 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.

Table D.5: Correlates of owning a Smartcard

	NREGS		SSP	
	(1) Binary	(2) Multiple	(3) Binary	(4) Multiple
Income (Rs. 10,000)	-.0043 (.0020)	-.0039 (.0020)	.0015 (.0020)	.0010 (.0019)
Consumption (Rs. 10,000)	-.0014 (.0012)	-.00088 (.0012)	.0013 (.0021)	.00096 (.0021)
Official amount (Rs. 100)	.0041 (.00083)	.0043 (.00082)	.00024 (.0028)	.000075 (.0028)
SC	.070 (.037)	.074 (.036)	.017 (.029)	.018 (.029)
Female	.039 (.017)	.042 (.017)	-.021 (.024)	-.022 (.024)
District FE	Yes	Yes	Yes	Yes
Adj R-squared		.27		.21
Dep Var Mean	.51	.47	.73	.73
N. of cases	5200	5164	1872	1862
Level	Indiv.	Indiv.	Indiv.	Indiv.

This tables analyzes how endline covariates predict which individuals use the Smartcard system to collect payments within villages that have moved to Smartcard based payments ("Carded GPs"). The outcome variable is hence an indicator equal to 1 if an individual uses her Smartcard or swipes her fingerprint to collect a payment and 0 otherwise. The columns labeled "binary" show coefficients from regressions with each covariate regressed separately. Hence every cell in columns 1 and 3 shows the result from a separate regression. In contrast, the columns labeled "multiple" run one single regression with all covariates. "Income (Rs. 10,000)" is household income with units as 1 = Rs. 10,000. "Consumption (Rs. 10,000)" is household consumption. "Land value (Rs. 10,000)" is household land value. "NREGS amount (Rs. 1,000)" is household NREGS income during the study period. "SC" is a dummy for whether household is Scheduled Caste. "Total Income" is total household income with the top .5% percentile of observations censored. 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.

Table E.1: Scaled NREGS earnings and leakage regressions

	Official			Survey		Leakage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	9.7	4.6	-7.3	33	32	-23	-27	-33
	(25)	(24)	(23)	(21)	(20)	(21)	(20)	(21)
BL GP Mean		.16			.1		.14	
		(.025)			(.038)		(.034)	
BL jobcard payment			.24					.16
			(.048)					(.053)
BL jobcard payment > 0			185					86
			(32)					(34)
BL GP Mean survey payment								-.1
								(.047)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.05	.19	.06	.07	.06	.07	.12
Control Mean	260	260	260	180	180	80	80	80
N. of cases	5143	5107	5107	5143	5107	5143	5107	5107

This table reports regressions of program benefits (in Rupees) as reported in official or survey records. Regressions include all sampled NREGS households who were a) found by survey team to match official records or b) listed in official records but confirmed as "ghosts". "Ghosts" refer to households or beneficiaries within households that were confirmed not to exist, or who had permanently migrated before the study period started on May 28, 2012. 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, *scaled by the average number of jobcards per household in the district*. "Survey" refers to payments received as reported by beneficiaries. "Leakage" is the difference between these two amounts. "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). The "BL GP Mean" for "Official" was scaled the same way the dependent variable was. "BL jobcard payment" was the official average weekly disbursement on the sampled jobcard during the baseline study period; "BL jobcard payment > 0" is an indicator for this payment being positive. Note that the regressions no longer include only individuals listed on sampled jobcards but rather household-level average weekly amounts using data from all working household members. 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.

Table E.2: Other leakage robustness results

	# of workers found in audit		Paid yet for a given period			
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	13 (12)	10 (10)	.029 (.033)	.032 (.035)		
Treatment X First 4 weeks					.04 (.034)	.044 (.036)
Treatment X Last 3 weeks					-.035 (.059)	-.034 (.063)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	Yes	Yes	Yes	Yes	Yes
BL GP Mean	No	No	No	Yes	No	Yes
p-value: first 4 weeks = last 3 weeks					.19	.21
Adj R-squared	.097	.14	.085	.085	.087	.087
Control Mean	28	28	.9	.9	.9	.9
N. of cases	508	508	11854	11174	11854	11174
Level	GP	GP	Indiv-Week	Indiv-Week	Indiv-Week	Indiv-Week

In columns 1 and 2, units represent estimated number of NREGS workers on a given day, found in an independent audit of NREGS worksites in GPs. In columns 3-6, the outcome is an indicator for whether an NREGS respondent had received payment for a given week's work at the time of the survey, weighted by the official payment amount. 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.

Table E.3: Summary statistics and treatment effects from the list experiment

(a) Summary statistics

	Treatment (1)	Control (2)	Difference (3)	p-value (4)	N (5)
Version 1	2.13	2.19	-.06	.54	1601
Version 2	2.21	2.34	-.13	.25	1616
Version 3	2.34	2.46	-.11	.32	1572

(b) Regression-adjusted treatment effects

	All versions (1)	Versions 1 & 2 (2)
Treatment	-.057 (.11)	-.054 (.11)
Version 2	.15 (.11)	.16 (.11)
Version 3	.27 (.1)	
Version 2 X treatment	-.089 (.13)	-.095 (.13)
Version 3 X treatment	-.056 (.12)	
District FE	Yes	Yes
p-val: version 2 X treat. = 0	.49	.46
p-val: version 3 X treat. = 0	.63	
Adj R-squared	.14	.12
Version 1 control mean	2.19	2.19
N. of cases	4789	3217

This table presents results of the "list experiment" conducted within the survey to determine whether officials asked households to lie about their NREGS participation and payments. Columns 1-2 in panel a) show means for the treatment and control group respectively. Column 3 shows the regression-adjusted difference from a regression with the district FE and the first principal component of a vector of mandal characteristics used to stratify randomization as covariates. The p-value in column 4 is from a two-sided test in which the null hypothesis is that the difference in column 3 is equal to 0. "Version 1" denotes respondents who were asked how many of 5 statements they would agree with. "Version 2" denotes those were presented with the same 5 statements as Version 1 as well as an additional sensitive statement: "Members of this household have been asked by officials to lie about the amount of work they did on NREGS". "Version 3" denotes those were presented with the same 5 statements as Version 1 "Members of this household have been given the chance to meet with the CM of AP to discuss problems with NREGS?"). Panel b) reports regression-adjusted treatment effects. Column 1 compares version 1 to version 2 and version 3 while column 2 only compares version 1 and 2. "Version 2 X treatment" and "Version 3 X treatment" are interaction terms of having faced the respective survey version and being in the treatment group. Standard errors clustered at the mandal level in parentheses.

Table E.4: Analyzing potential recall bias in leakage results

	Survey		Leakage	
	(1)	(2)	(3)	(4)
Treatment X surveyed in week 1	54	38	35	55
	(86)	(78)	(88)	(83)
Treatment X surveyed in week 2	77	88	-92	-97
	(44)	(45)	(44)	(44)
Treatment X surveyed in week 3	35	33	-42	-52
	(41)	(40)	(33)	(33)
Treatment X surveyed in week 4	35	35	-37	-42
	(44)	(45)	(47)	(44)
Treatment X surveyed in week 5	70	77	-37	-48
	(35)	(38)	(31)	(31)
Treatment X surveyed in week 6	46	35	-34	-37
	(37)	(36)	(35)	(36)
Treatment X surveyed in week 7	-43	-29	42	38
	(69)	(66)	(54)	(54)
Treatment X surveyed in week 8	19	11	12	24
	(24)	(30)	(24)	(20)
Treatment X surveyed in week 9	106	105	-28	-23
	(28)	(27)	(25)	(25)
Treatment X surveyed in week 10	-52	-58	-28	-29
	(48)	(42)	(42)	(43)
BL GP Mean		.13		.12
		(.041)		(.044)
District FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
Adj R-squared	.07	.07	.05	.05
Control Mean	165	165	-21	-21
N. of cases	4803	4769	4803	4769

The regressions include all sampled households who were a) found by survey team to match official record or b) listed in official records but confirmed as "ghosts". "Ghosts" refer to households or beneficiaries within households that 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). "Treatment X surveyed in week x" is an interaction term of treatment and the household survey taking place in week x. Note that the household surveys took place in August, September and the early weeks of October 2012. Note all regressions include week fixed effects. The number of observations is different compared to Table 3a because for some surveys the survey date information was corrupted or missing. "Survey" refers to payments received as reported by beneficiaries. "Leakage" is the difference between the survey amount and the offical amount disbursed. "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). 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.

Table E.5: Geographical spillovers on payment collection time

(a) NREGS

	Time to collect (Min)				
	(1)	(2)	(3)	(4)	(5)
Treatment	-23	-23	-23	-23	-22
	(8.9)	(8.4)	(8.5)	(8.6)	(8.6)
Fraction GPs treated within 10km	-3.6				
	(7.6)				
Fraction GPs treated within 15km		-6.2			
		(9.8)			
Fraction GPs treated within 20km			-11		
			(13)		
Fraction GPs treated within 25km				-15	
				(15)	
Fraction GPs treated within 35km					-13
					(24)
BL GP Mean	.067	.074	.072	.076	.077
	(.04)	(.04)	(.04)	(.041)	(.041)
District FE	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.07	.08	.08	.08	.08
Control Mean	112	112	112	112	112
N. of cases	9081	9781	9978	10045	10074

(b) SSP

	Time to Collect (Min)				
	(1)	(2)	(3)	(4)	(5)
Treatment	-5	-5.6	-5	-4.9	-4.3
	(5.7)	(5.3)	(5.3)	(5.3)	(5.4)
Fraction GPs treated within 10km	-12				
	(6.6)				
Fraction GPs treated within 15km		-12			
		(7.8)			
Fraction GPs treated within 20km			-11		
			(9.5)		
Fraction GPs treated within 25km				-12	
				(12)	
Fraction GPs treated within 35km					-15
					(16)
BL GP Mean	.28	.23	.22	.22	.22
	(.08)	(.074)	(.073)	(.072)	(.072)
District FE	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.13	.11	.11	.11	.11
Control Mean	77	77	77	77	77
N. of cases	3209	3462	3524	3545	3551

The dependent variable in all columns in both panels 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 "fraction GPs treated within x" is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are excluded in both the denominator and numerator. "BL GP Mean" is the GP-average of the dependent variable at baseline. 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.

Table E.6: Geographical spillovers on timeliness of payments

(a) Payment lag (Days)

	Ave Payment Delay				
	(1)	(2)	(3)	(4)	(5)
Treatment	-12 (3.8)	-10 (3.8)	-9.7 (3.5)	-10 (3.5)	-10 (3.3)
Fraction GPs treated within 10km	-1.3 (4.7)				
Fraction GPs treated within 15km		-.34 (5.1)			
Fraction GPs treated within 20km			2 (5.6)		
Fraction GPs treated within 25km				-.81 (6.1)	
Fraction GPs treated within 35km					-.17 (8.7)
BL GP Mean	.023 (.089)	.012 (.087)	.0075 (.081)	.0096 (.08)	.013 (.079)
District FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.34	.33	.33	.33	.33
Control Mean	34	34	34	34	34
N. of cases	6446	6897	7087	7169	7201

(b) Absolute deviation from Mandal median (Days)

	Payment Lag Deviation				
	(1)	(2)	(3)	(4)	(5)
Treatment	-5.4 (1.6)	-4.5 (1.7)	-4.5 (1.6)	-4.7 (1.6)	-4.6 (1.5)
Fraction GPs treated within 10km	.19 (1.5)				
Fraction GPs treated within 15km		.67 (2.2)			
Fraction GPs treated within 20km			.72 (2.6)		
Fraction GPs treated within 25km				-.22 (2.9)	
Fraction GPs treated within 35km					1 (4.3)
BL GP Mean	.059 (.054)	.034 (.051)	.036 (.051)	.037 (.051)	.041 (.052)
District FE	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.18	.17	.17	.17	.17
Control Mean	12	12	12	12	12
N. of cases	6446	6897	7087	7169	7201

The dependent variable in panel a) is the average lag (in days) between work done and payment received on NREGS. The outcome in panel b) 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. The "fraction GPs treated within x" is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are excluded in both the denominator and numerator. "BL GP Mean" is the GP-average of the dependent variable at baseline. Each regression includes the stratification principal component as a control variable. Standard errors clustered at mandal level in parentheses.

Table E.7: Geographical spillover effects on leakage

(a) NREGS

	Leakage				
	(1)	(2)	(3)	(4)	(5)
Treatment	-27 (14)	-25 (13)	-27 (13)	-28 (13)	-27 (13)
Fraction GPs treated within 10km	1.7 (15)				
Fraction GPs treated within 15km		-5.2 (22)			
Fraction GPs treated within 20km			-15 (27)		
Fraction GPs treated within 25km				-26 (29)	
Fraction GPs treated within 35km					-22 (35)
BL GP Mean	.1 (.044)	.087 (.039)	.1 (.041)	.098 (.039)	.098 (.039)
District FE	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.04	.04	.04	.04	.04
Control Mean	-20	-20	-20	-20	-20
N. of cases	4564	4932	5032	5062	5073

(b) SSP

	Leakage				
	(1)	(2)	(3)	(4)	(5)
Treatment	-5.3 (4.3)	-5.3 (3.8)	-5.3 (3.8)	-5.5 (3.9)	-5.7 (4)
Fraction GPs treated within 10km	2.2 (4.5)				
Fraction GPs treated within 15km		3 (5.2)			
Fraction GPs treated within 20km			1.5 (6.1)		
Fraction GPs treated within 25km				.032 (7.5)	
Fraction GPs treated within 35km					-4.2 (11)
BL GP Mean	-.049 (.031)	-.044 (.032)	-.044 (.032)	-.044 (.032)	-.044 (.032)
District FE	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.01	.01	.01	.01	.01
Control Mean	15	15	15	15	15
N. of cases	2806	3034	3086	3106	3112

The regressions in both panels include all sampled households (NREGS)/beneficiaries (SSP) who were a) found by survey team to match official record or b) listed in official records but 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). In panel (b), each outcome observation refers to the average SSP monthly amount for the period May, June, and July 2012. "Leakage" is the difference between the amount disbursed as indicated by official records and the amount a household reported as received in the survey. The "fraction GPs treated within x" is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are excluded in both the denominator and numerator. In panel a) "BL GP Mean" is the GP average of household-level weekly, amounts for work done during the baseline study period (May 31 to July 4 2010). In panel b) "BL GP Mean" is the GP average monthly amounts based on official disbursements from baseline period of May, June, and July 2010 and baseline survey information on pension proceedings (see Table 3b). 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.

Table E.8: Impacts on NREGS projects and budget categories

	Works		Person-days		Wages paid		Material expend.		Contingent expend.		Total expend.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	-4.1	-.55	-67	-114	-11299	-14924	7495	7465	-81	-154	-3885	-7534
	(7.4)	(6.9)	(306)	(221)	(32456)	(24308)	(7744)	(7589)	(219)	(197)	(36238)	(28525)
Baseline		.84		.47		.54		.061		.11		.44
		(.078)		(.04)		(.048)		(.016)		(.013)		(.037)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	0.10	0.26	0.21	0.43	0.20	0.42	0.02	0.02	0.09	0.14	0.17	0.36
Control Mean	88	88	3940	3940	420493	420493	34459	34459	5030	5030	459981	459981
N. of cases	8736	8730	8736	8730	8736	8730	8736	8730	8736	8730	8736	8730

This table analyzes impacts on NREGS projects and various budget categories at the GP level for the months of May, June and July 2012. "Works" is the number of projects that official data sources placed in a GP in a month. A "personday" means one day of work for one person. "Wages paid" is the total amount of wage outlays on NREGS for a GP within the given month. "Material expen." and "contingent expen." are monthly totals for NREGS work in a given GP. "Total expend." is the sum of wage outlays, material and contingent expenditure within a GP in a given month. "Baseline" is the lagged dependent variable as observed during the respective months in 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.

Table E.9: Hawthorne effects

	WSM	Official				Survey
	(1)	(2)	(3)	(4)	(5)	(6)
Survey in GP	-3.7	10	-4.8			
	(8)	(34)	(33)			
Audit in GP		7.5	-13	6.8	-12	116
		(31)	(28)	(42)	(37)	(106)
Audit in Week		-52	-71	-26	-34	40
		(51)	(52)	(39)	(39)	(84)
Recon in Week		12	-.8	49	44	45
		(69)	(68)	(53)	(52)	(90)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
BL GP Value	No	No	Yes	No	Yes	Yes
GP Size FE	No	Yes	Yes	No	No	No
Adj R-squared	.18					
Control Mean	49	758	758	756	756	1175
Level	Week	Week	Week	Week	Week	Week
Sample	Audit	All	All	Survey & Audit	Survey & Audit	Survey
N. of cases	676	52311	52311	7679	7679	6111

This table analyzes possible Hawthorne effects from various data collection activities. Each cell represents a separate regression of the effect on the data source (column) from the survey type (row). Units are number of days worked in a GP per week. "Survey in GP" is an indicator for whether a GP was part of the household survey. "Audit in GP" is a binary variable equal to 1 if the GP was sampled for work site audits while "Audit in week" indicates that the work site audit happened in a specific week. "Recon in week" is an indicator for whether an enumerator went to map the worksites in a specific week. "All", "Audit", and "Survey" indicate that the data came from all mandals in the study district, the GPs sampled for the work site audits or from the GPs sampled for the household survey respectively. The regressions in column 1 as well as columns 4 to 6 include the first principal component of a vector of mandal characteristics used to stratify randomization. Note that the regressions in columns 2 and 3 use data from all mandals in AP and the principal component of mandal characteristics is only available for those that entered the randomization pool, i.e., waves 1 and 3 as well as the buffer wave. Therefore, it is not included in the regressions in columns 2 and 3. Standard errors clustered at mandal level in parentheses.

Household (surveyed)	
Name	Payment
Karthik	30
Paul	20
Sandip	40

Jobcard (sampled)	
Name	Payment
Karthik	30
Paul	0

Jobcard *not* sampled)	
Name	Payment
Paul	35
Sandip	50

Figure E.1: Illustrating multiple jobcards

Table F.1: Non-experimental decomposition of treatment effects by carded status: With household controls

	Time to collect		Payment lag		Survey				Leakage				Proportion of Hhds doing NREGS work	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS	SSP	SSP	NREGS	NREGS	SSP	SSP	NREGS	NREGS
Carded GP	-36 (21)		-2.5 (5.2)		162 (70)		54 (34)		-65 (67)		-13 (21)		.27 (.15)	
Have SCard, Carded GP		-37 (22)		-2.2 (5.2)		316 (80)		32 (33)		-172 (74)		-3 (23)		.69 (.16)
No SCard, Carded GP		-33 (21)		-2.8 (5.4)		149 (71)		52 (34)		-57 (68)		-11 (22)		.23 (.15)
Not Carded GP	2.9 (12)	2.9 (12)	-7.3 (4.3)	-7.4 (4.3)	37 (28)	37 (28)	15 (6.4)		-19 (24)	-19 (24)	-9.4 (6.2)		.061 (.045)	.063 (.046)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	No	Yes	Yes	No	No	No	No	No	No	No	No	No	No
BL GP Mean	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-values:														
Carded GP=Not Carded GP	.15		.49		.080		.25		.5		.86		.2	
Have SC=No SC		.39		.7		<.001		.062		.0035		.17		<.001
Adj R-squared	.12	.12	.18	.18	.12	.13	.12	.12	.054	.058	.015	.013	.12	.14
Control Mean	112	112	34	34	190	190	252	252	-26	-26	4.3	4.3	.48	.48
N. of cases	9907	9879	13777	13777	4618	4618	2927	2927	4618	4618	2927	2927	4618	4618
Level	Indiv.	Indiv.	Indiv-Week	Indiv-Week	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd

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: 4947 individuals, 2332 households; SSP: 1427 households). "Have SCard, Carded GP" (NREGS: 2569 individuals, 1378 households; SSP: 941 households) and "No SCard, Carded GP" (NREGS: 2378 individuals, 954 households; SSP: 486 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. Note that the group sizes differ from Table 7 because the set of control variables (see below) was not always complete for individuals and households. "Not Carded GP" is a gram panchayat in a treatment mandal that has not yet moved to Smartcard-based payments (NREGS: 2222 individuals, 1012 households; SSP: 653 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. In addition, we include controls for age, gender, scheduled caste, scheduled tribe, Below Poverty Line status, an indicator for whether the head of household is a widow, an indicator for whether at least 1 member can read and GP mean baseline income (top .% censored). In household level regressions, age and gender refer to the head of the household. Each variable is also interacted with carded GP status. Standard errors clustered at mandal level in parentheses.

Table F.2: Non-experimental decomposition of treatment effects by carded status: GP Fixed Effects

	Time to collect	Payment lag	Survey		Leakage		Proportion of Hhds doing NREGS work
	(1) NREGS	(2) NREGS	(3) NREGS	(4) SSP	(5) NREGS	(6) SSP	(7) NREGS
Have SCard, Carded GP	2.9 (1.8)	-.61 (.41)	219 (15)	31 (13)	-28 (14)	-27 (6.7)	.45 (.03)
Gram Panchayat FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	Yes	No	No	No	No	No
BL GP Mean	Yes	No	Yes	Yes	Yes	Yes	Yes
p-values							
Have SC=No SC	.1	.13	<.001	.021	.055	<.001	<.001
Adj R-squared	.65	.63	.37	-.018	.31	.04	.47
Control Mean	112	34	166	236	-22	15	.48
N. of cases	10157	14165	4948	3325	4948	3325	4748
Level	Indiv.	Indiv-Week	Hhd	Hhd	Hhd	Hhd	Hhd

This table shows the main ITT effects decomposed by whether a household had a Smartcard, restricted to within GP variation as we include GP fixed effects. "Have SCard, Carded GP" (NREGS: 2619 individuals, 1403 households; SSP: 959 households) is based on whether the beneficiary or household lives in a carded GP (a gram panchayat that has moved to Smartcard based payments) 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 (100) and an additional 18 individuals were dropped from the analysis since we could not determine their Smartcard status. For each outcome, we report the p-value of a two-sided t-test with the null hypothesis being that "Have SCard, Carded GP" is equal to 0. Since every observation either has or does not have a SC conditional on the GP being converted, this amounts to a test whether the two groups are equal to one another. Robust standard errors in parentheses.

Table F.3: Heterogeneity in impacts by baseline characteristics

(a) NREGS

	Time to Collect	Payment Lag	Official Payments	Survey Payments
	(1)	(2)	(3)	(4)
BL GP Mean	.024	.19	.012	.048
	(.08)	(.25)	(.042)	(.074)
Consumption (Rs. 1,000)	-.085	.032	-.19	-.17
	(.16)	(.032)	(.11)	(.2)
GP Disbursement, NREGS (Rs. 1,000)	.015	.00014	.014	.0044
	(.0078)	(.0013)	(.01)	(.016)
SC Proportion	.31	25	3.6	13
	(48)	(13)	(49)	(51)
BPL Proportion	-61	122	-64	-161
	(127)	(75)	(84)	(122)
District FE	Yes	Yes	Yes	Yes
Week FE	No	Yes	No	No
Control Mean	112	34	127	146
Level	Indiv.	Indiv-Week	Hhd	Hhd
N. of cases	10143	12770	5115	5115

(b) SSP

	Time to Collect	Official Payments	Survey Payments
	(1)	(2)	(3)
BL GP Mean	.21	-.015	.078
	(.1)	(.091)	(.093)
Consumption (Rs. 1,000)	-.26	-.014	-.096
	(.11)	(.099)	(.23)
GP Disbursement, SSP (Rs. 1000)	-.083	.13	.026
	(.094)	(.064)	(.17)
SC Proportion	18	-29	-20
	(16)	(23)	(37)
BPL Proportion	-66	126	95
	(35)	(53)	(83)
District FE	Yes	Yes	Yes
Control Mean	77	259	313
Level	Indiv.	Indiv.	Indiv.
N. of cases	3573	2943	2943

This table shows heterogeneous effects on major endline outcomes from GP-level baseline characteristics. Each cell shows the coefficient on the baseline characteristic interacted with the treatment indicator in separate regressions. "BL GP Mean" is the baseline GP-level mean for the outcome variable. "Consumption (Rs. 1,000)" is annualized consumption. "GP Disbursement (Rs. 1000)" is total NREGS/SSP payment amounts for the period Jan 1, 2010 to July 22, 2010. "SC Proportion" is the proportion of NREGS workspells performed by schedule caste workers/SSP beneficiaries in the period from Jan 1, 2010 to July 22, 2010. "BPL Proportion" is the proportion of households with a BPL card in the baseline survey. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level in parentheses.
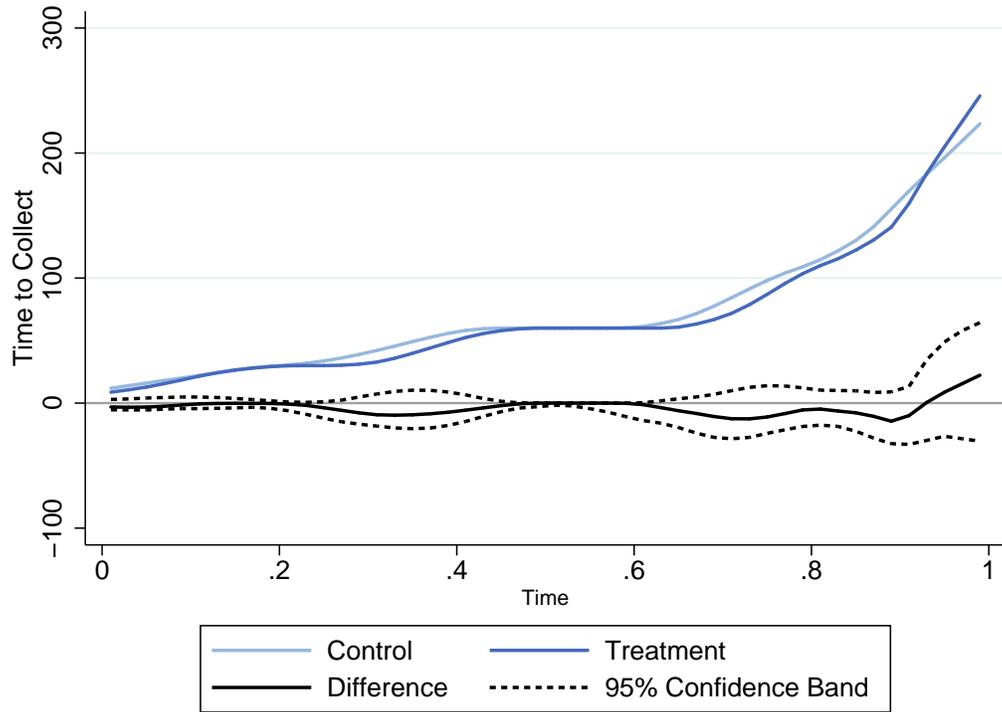
Figure F.1: Quantile treatment effect on payment collection time - SSP

This figure shows non-parametric treatment effects. "Time to collect: SSP" is the average time taken to collect a payment, including the time spent on unsuccessful trips to payment sites. 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.