

General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India*

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Abstract

A public employment program's effect on poverty depends on both program earnings and market impacts. We estimate this composite effect, exploiting a large-scale randomized experiment across 157 sub-districts and 19 million people that improved the implementation of India's employment guarantee. Without changing government expenditure, this reform raised low-income households' earnings by 13%, driven primarily by market earnings. Real wages rose 6% while days without paid work fell 7%. Effects spilled over across sub-district boundaries, and adjusting for these spillovers substantially raises point estimates. The results highlight the importance and feasibility of accounting for general equilibrium effects in program evaluation.

JEL codes: D50, D73, H53, J38, J43, O18

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1 Introduction

Public employment programs, in which the government provides jobs to those who seek them, are among the most common anti-poverty programs in developing countries. The economic rationale for such programs (as opposed to unconditional income support for the poor) include self-targeting through work requirements, public asset creation, and making it easier to implement a wage floor in informal labor-markets by making the government an employer of last resort.¹ An important contemporary variant is the National Rural Employment Guarantee Scheme (NREGS) in India. It is the world’s largest workfare program, with 600 million rural residents eligible to participate and a fiscal allocation of 0.5% of India’s GDP.

A program of this scale and ambition raises several fundamental questions for research and policy. First, how does it affect rural incomes and poverty? In particular, while the wage income provided by such a scheme should reduce poverty, the market-level general equilibrium effects of public employment programs could amplify or attenuate the direct gains from the program for beneficiaries.² Second, what is the relative contribution of direct gains in income from the program and indirect changes in income (gains or losses) outside the program? Third, what are the impacts on wages, employment, assets, and migration?

Given the importance of NREGS, a growing literature has tried to answer these questions, but the evidence to date has been hampered by three factors. The first is the lack of experimental variation, with the consequence that studies often reach opposing conclusions depending on the data and identification strategy used (see Sukhtankar (2017) and the discussion in section 2.1). Second, “construct validity” remains a challenge. Specifically, the wide variation in program implementation quality (Imbert and Papp, 2015), and the difficulty of measuring *effective* NREGS presence makes it difficult to interpret the varied estimates of the impact of “the program” to date (Sukhtankar, 2017). Third, since market-level general equilibrium effects of NREGS are likely to spill over across district boundaries, existing estimates that use the district-level rollout for identification may be biased by not accounting for spillovers to untreated units (as in Miguel and Kremer (2004)).

In this paper we aim to provide credible estimates of the anti-poverty impact of public employment programs by combining exogenous experimental variation, a demonstrable first-stage impact on implementation quality, units of randomization large enough to capture general equilibrium effects, and geocoded units of observation disaggregated enough to test

¹Workfare programs may also be politically more palatable to taxpayers than unconditional “doles.” Such programs have a long history, with recorded instances from as early as the 18th century in India (Kramer, 2015), the public works constructed in the US by the WPA during the Depression-era in the 1930s, and more modern programs across Sub-Saharan Africa, Latin America, and Asia (Subbarao et al., 2013).

²These general equilibrium effects include for example changes in market wages and employment, relative prices, and broader changes in economic activity induced by the program.

and correct for spatial spillovers. Specifically, we worked with the Government of the Indian state of Andhra Pradesh (AP) to randomize the order in which 157 sub-districts (mandals) with an average population of 62,500 each introduced a new system (biometric “Smartcards”) for making payments in NREGS.³ In prior work, we show that Smartcards substantially improved the performance of NREGS on several dimensions: it reduced leakage or diversion of funds, reduced delays between working and getting paid, reduced the time required to collect payments, and increased real and perceived access to work, without changing fiscal outlays on the program (Muralidharan et al. (2016), henceforth MNS). Thus, Smartcards brought NREGS implementation closer - in specific, measured ways - to what its architects intended. This in turn lets us open up the black box of “implementation quality” and link GE effects to these tangible improvements in NREGS implementation.⁴

The impacts of improving NREGS implementation are unlikely to be the same as the impacts of rolling out the program itself. Yet, given well-documented implementation challenges – including poor access to work, high rates of leakage, and long delays in receiving payments (Mehrotra, 2008; Imbert and Papp, 2011; Khera, 2011; Niehaus and Sukhtankar, 2013b) – improving implementation on these metrics is likely to meaningfully increase any measure of *effective* NREGS. As one imperfect summary statistic, we find (below) that treatment raised prime aged adults’ reservation wages for market labor by 5.8%; one can thus think of the experiment as capturing the effects of making the NREGS that much more attractive and beneficial to workers. Further, since improvements in the effective presence of NREGS were achieved without increasing NREGS expenditure in treated areas, our results are likely to be a lower bound on the anti-poverty impact of rolling out a well-implemented NREGS from scratch (which would also transfer incremental resources to rural areas).

We report five main sets of results. First, using our survey data, we find a large (12.7%) increase in incomes of households registered for the NREGS (49.5% of all rural households) in treated mandals two years after the Smartcards rollout began.⁵ We also find evidence of significant income gains in the entire population using data from the Socio-Economic and Caste Census (SECC), a census of both NREGS-registered and non-registered households conducted by the national government independently of our activities.

Second, the majority of these income gains are attributable to indirect market effects

³The original state was divided into two states on June 2, 2014. Since this division took place after our study, we use “AP” to refer to the original undivided state. The combined rural population in our study districts (including sub-districts randomized into a “buffer” group) was 19 million people.

⁴Smartcards also reduced leakage in delivering rural pensions, but these are unlikely to have affected labor markets because pension recipients were typically physically unable to work (see Section 2.3).

⁵Putting the magnitude of these effects in the context of policy debates on the trade-off between growth and redistribution, it would take 12 years of an extra percentage point of growth in rural GDP to generate an equivalent rise in the incomes of the rural poor.

rather than direct increases in NREGS income. Among NREGS-registered households in the control group, the mean household earned 7% of its income from NREGS and 93% from other sources. Treatment increased earnings in similar proportions, with 10% of the gain coming from NREGS earnings and the other 90% from outside the program. Thus, the general equilibrium impacts of NREGS through the open market appear to be a much more important driver of poverty reduction than the direct income provided by the program.

Third, these gains in non-NREGS earnings are driven by a significant increase in earnings from market labor. During the period for which we have the most detailed data, market wages rose by 6.1% and employment in the private sector rose (insignificantly) by 6.7% in treated areas, enough to account for the observed income gain. We also find a 5.8% increase in reported reservation wages in treated areas. Importantly, these wage gains accrue to *all* NREGS-registered households and do not vary as a function of whether they actually participated in NREGS, highlighting the general equilibrium nature of the wage effects. While we have less precise measures of wages year-round, point estimates suggest that wages in treated mandals increased throughout the year (consistent with several mechanisms we discuss later, e.g. productivity-enhancing asset creation or nominal wage rigidity). We find no evidence of corresponding changes in consumer goods prices, implying that earnings and wage gains were real and not merely nominal.

Fourth, we find little evidence of efficiency-reducing effects on factor allocation. As mentioned above, private sector employment weakly *increased*, and this increase is significant once we adjust the estimates for spatial spillovers (below). Days idle or doing unpaid work fell significantly by 7.1%. We find no impacts on migration or on available measures of land use, and in most cases can rule out sizeable effects.

Fifth, we find evidence that households used the increased income to purchase major productive assets, which may have contributed to further income gains. We find an 8.3% increase in the rate of land ownership among NREGS-registered households. We also find a significant increase in overall livestock ownership using data from an independent government livestock census. Households in treated mandals also had higher outstanding informal loans, suggesting reduced credit constraints that may have facilitated asset accumulation.

All the results above compare treated to control regions. If effects spill over across administrative boundaries, they may mis-estimate the “total treatment effect” of a scaled-up policy that treats *all* regions (compared to not having the program at all). We therefore develop simple methods to test and correct for such spillovers. We find evidence of spillover effects on most outcomes, which are consistent in sign with the main effects, and validate these using a different source of variation.⁶ More importantly, adjusting for these spillovers

⁶Specifically, while the experimental ITT effects are based on a mandal’s own treatment status, the

yields estimates of the total treatment effect that are significant and typically double the magnitude of the unadjusted estimates, suggesting that research designs that ignore spatial spillovers may understate the total effects of NREGS.

The results above present the policy-relevant general-equilibrium estimates of the total effect on wages, employment, income, and assets of increasing the effective presence of NREGS. Mapping these magnitudes into mechanisms is subtle since – unlike in a partial equilibrium analysis – we cannot equate treatment effects with any particular partial elasticity, or even to the decomposable sum of some set of distinct “channels.” Instead our estimates reflect a potentially complex set of feedback loops, multipliers, and interactions between several channels operating in general equilibrium. This makes isolating or quantifying the role of individual mechanisms an implausible exercise. Thus, while we do find significant evidence of some mechanisms – such as increased labor market competition, credit access, and ownership of productive assets – we do not rule out the possibility that other factors and the interplay between them also contributed to the overall effects (see discussion in Section 6).

This paper contributes to several literatures. The first is the growing body of work on the impact of public works programs on rural labor markets and economies (Imbert and Papp, 2015; Beegle et al., 2017; Sukhtankar, 2017). In addition to confirming some prior findings, like the increase in market wages (Imbert and Papp, 2015; Berg et al., forthcoming; Azam, 2012), our data and methodology allow us to report several new results. The most important of these are: (a) the significant gains in income and reduction in poverty, (b) finding that 90% of the impact on income was due to indirect market effects rather than direct increases in NREGS income, and (c) finding *positive* effects on private sector employment. The last finding is particularly salient for the larger policy debate on NREGS and is consistent with the idea that public employment programs can be efficiency-enhancing if they enable the creation of productive assets (public or private), or if local labor markets are oligopsonistic.

Second, our results highlight the importance of accounting for general equilibrium effects in program evaluation (Acemoglu, 2010). Ignoring these effects (say by randomizing program access at an individual level) would have led to us to sharply underestimate the impact of a better-implemented NREGS on rural wages and poverty. Even analyzing our own data while ignoring geographic spillovers meaningfully understates impacts. On a more optimistic note, our study demonstrates the feasibility of conducting experiments with units of randomization large enough to capture general equilibrium effects on outcomes of interest for program evaluation (Cunha et al., 2017; Muralidharan and Niehaus, 2017).

Third, our results contribute to the literature on wage determination in rural labor markets in developing countries generally (Rosenzweig, 1978; Jayachandran, 2006; Kaur, forthcoming)

spillover results use variation in the exposure of sample villages to treated neighbors.

and on the impacts of minimum wages specifically (e.g. Dinkelman and Ranchhod (2012)). This literature also relates directly to policy debates about the NREGS, whose critics have argued that it could not possibly have meaningfully affected rural poverty because NREGS work constitutes only a small share (under 4%) of total rural employment (Bhalla, 2013). Our results suggest that this argument is incomplete. Much larger shares of rural households in AP are registered for NREGS (~50%) and actively participate (32%) in the program, and the data suggest that the existence of a well-implemented public employment program can raise wages for these workers in the private sector (Dreze and Sen, 1991; Basu et al., 2009).

Fourth, our results highlight the importance of implementation quality for the effectiveness of policies and programs in developing countries. Our estimates of the wage impacts of improving NREGS implementation, for example, are about as large as the most credible estimates of the impact of rolling out the program itself (Imbert and Papp, 2015). More generally, in settings with high corruption and inefficiency, investing in better implementation of a program could be a more cost-effective way of achieving desired policy goals than spending more on the program as is. For instance, Niehaus and Sukhtankar (2013b) find that increasing the official NREGS wage had no impact on workers' program earnings, while we find that improving NREGS implementation significantly increased their earnings from market wages (despite no change in official NREGS wages).⁷

Finally, we contribute to the literature on the political economy of anti-poverty programs in developing countries. Landlords typically benefit at the cost of workers from low wages and from the wage volatility induced by productivity shocks, and may be hurt by programs like NREGS that raise wages and/or provide wage insurance to the rural poor (Jayachandran, 2006). Anderson et al. (2015) have argued that “a primary reason... for landlords to control governance is to *thwart* implementation of centrally mandated initiatives that would raise wages at the village level.” While we do not directly observe landlord or employer profits, the fact that improving NREGS substantially raised market wages underscores their incentive to oppose such improvements and helps rationalize their widely documented resistance to the program (Khera, 2011; Jenkins and Manor, 2017; Mukherji and Jha, 2017).

The rest of the paper is organized as follows. Section 2 describes the context, related literature, and Smartcard intervention. Section 3 describes the research design, data, and estimation. Section 4 presents our main results on income, wages, employment, and assets. Section 5 examines spillover effects. Section 6 discusses mechanisms, and Section 7 concludes with a discussion of policy implications.

⁷In a similar vein, Muralidharan et al. (2017) show that reducing teacher absence by increasing monitoring would be ten times more cost-effective at reducing *effective* student-teacher ratios (net of teacher absence) in Indian public schools than the default policy of hiring more teachers.

2 Context and intervention

2.1 The NREGS

The NREGS is the world’s largest public employment program, entitling any household living in rural India (i.e. 11% of the world’s population) to up to 100 days per year of guaranteed paid employment. It is one of the country’s flagship social protection programs, and the Indian government spends roughly 3.3% of its budget ($\sim 0.5\%$ of GDP) on it. Coverage is broad: 50% of rural households in Andhra Pradesh have at least one jobcard, which registers them for the NREGS and entitles them to request work. Legally, they may do so at any time, and the government is obligated either to provide work or pay unemployment benefits (though the latter are rare in practice).

NREGS jobs involve manual labor compensated at statutory piece rates, and are meant to induce self-targeting. NREGS projects are typically public infrastructure improvement such as irrigation or water conservation works, minor road construction, and land clearance for cultivation. Projects are proposed by village-level local governance bodies (Gram Panchayats) and approved by sub-district (mandal) offices.

As of 2010, NREGS implementation quality suffered from several known issues. Rationing was common even though *de jure* jobs should be available on demand, with access to work constrained both by budgetary allocations and by local capacity to implement projects (Dutta et al., 2012). Corruption occurred through over-invoicing the government to reimburse wages for work not actually done and paying workers less than their due, among other methods (Niehaus and Sukhtankar, 2013a,b). Finally, the payment process was slow and unreliable: payments were time-consuming to collect, and were often unpredictably delayed for over a month beyond the 14-day period prescribed by law.

The impact of the NREGS on labor markets, poverty, and the rural economy have been extensively debated (see Sukhtankar (2017) for a review). Supporters claim that it has transformed the rural countryside by increasing wages and incomes, creating useful rural infrastructure, and reduced negative outcomes like distress migration (Khera, 2011). Skeptics claim that funding is largely captured by middlemen and wasted, arguing that the scheme could not meaningfully affect the rural economy since it accounts for only a small share of rural employment (“how can a small tail wag a very very large dog?” Bhalla (2013)). Even if it did increase rural wages, others have argued that this would come at the cost of crowding out more efficient private employment (Murgai and Ravallion, 2005). The debate continues to matter for policy: Although NREGS is implemented through an Act of Parliament, national and state governments can in practice decide how much to prioritize it

by adjusting fiscal allocations to the program.⁸

Evidence to inform this debate is inconclusive. Most empirical work has exploited the fact that the NREGS was rolled out across districts in three phases between 2006-2008, with districts prioritized in part based on an index of deprivation and in part on political considerations (Chowdhury, 2014). Difference-in-differences and regression discontinuity approaches based on this rollout have known limitations.⁹ NREGS implementation quality also varies widely and has typically not been directly measured. Thus, differences in findings across studies may reflect differences in unmeasured implementation quality. Estimates that exploit the staggered NREGS rollout are especially sensitive to this issue because implementation in the early years of the program was thought to be particularly weak, so that the impacts of rollout need not predict steady state effects once teething problems were resolved (Mehrotra, 2008). Finally, it has proven difficult to test and correct for potential spillovers from program to non-program (control) areas, simply because the available identifying variation and the geocoding of the available outcome data are both at the district level.¹⁰ In practice, findings to date for a range of outcomes have varied widely. For wages, for example, studies using a difference-in-differences approach estimate a positive 4-5% effect on rural unskilled wages (Imbert and Papp, 2015; Berg et al., forthcoming; Azam, 2012) while a study using a regression discontinuity approach finds no impact (Zimmermann, 2015).¹¹

2.2 Smartcards

To address leakage and payments challenges, the Government of Andhra Pradesh (GoAP) introduced a new payments system. This intervention – which we refer to as “Smartcards” for short – had two major components. First, it changed the flow of payments in most cases from government-run post offices to banks, who worked with Technology Service Providers and Customer Service Providers (CSPs) to manage the technological back-end and make last-mile payments in cash (typically in the village itself). Second, it changed the process of identifying payees from one based on paper documents and ink stamps to one based on

⁸For instance, work availability fell sharply in the second half of 2016 following a budget contracting: <http://thewire.in/75795/mnrega-centre-funds-whatsapp/>, accessed November 3, 2016.

⁹Specifically, the parallel trends assumption required for differences-in-differences estimation does not hold for many outcomes without additional controls, while small sample sizes limit the precision and power of regression discontinuity estimators at reasonable bandwidth choices (Sukhtankar, 2017).

¹⁰In one recent exception, Merfeld (2017) finds some evidence of spillovers using ARIS/REDS data with village geo-identifiers. While imprecise due to sample size, these results suggest that ignoring spatial spillovers may bias existing estimates of the impact of NREGS.

¹¹Findings on other outcomes (such as education and civil violence related to the leftist Naxalite or Maoist insurgency) vary similarly across otherwise well-executed studies, suggesting that the differences may reflect variation in identification, and NREGS implementation quality across study sites and time periods; see (Sukhtankar, 2017) for a detailed review of this evidence.

biometric authentication. More details on the Smartcard intervention and the ways in which it changed the process of authentication and payments are available in MNS.

Using the randomization design described in Section 3.1, we find in MNS that Smartcards significantly improved NREGS implementation on most dimensions. Two years after the intervention began, payments in treatment mandals arrived in 29% fewer days, with arrival dates 39% less varied, and took 20% less time to collect. Households earned more working on NREGS (24%), and there was a substantial 12.7 percentage point ($\sim 41\%$) reduction in leakage (defined as the difference between fiscal outlays and beneficiary receipts). Program access also improved: both perceived access and actual participation in NREGS increased (17%). These positive effects were found even though the implementation of Smartcards was incomplete, with roughly 50% of payments in treated mandals being authenticated at the time of our endline surveys. These effects were achieved without any increase in fiscal outlay on NREGS itself in treated areas. Finally, gains were widely distributed. We find little evidence of heterogenous impacts, and treatment distributions first order stochastically dominate control distributions for all outcomes on which there was a significant mean impact. Reflecting this, users were strongly in favor of Smartcards, with 90% of households preferring it to the status quo and only 3% opposed.

2.3 Interpreting Smartcards’ impacts on the economy

Given that Smartcards brought the *effective* presence of NREGS in treated areas closer to the intentions of the program’s framers, a natural interpretation is to think of the randomized rollout of Smartcards as an instrumental variable for a composite endogenous variable called “effective NREGS.” However, given the many dimensions on which NREGS implementation quality can and did change, constructing such a uni-dimensional endogenous variable is implausible. Our results are therefore best interpreted as the reduced form impact of improving NREGS implementation quality on multiple dimensions.

A separate question is whether Smartcards could have affected the rural economy directly, independent of their effects on the NREGS. Three relevant channels are pensions, financial inclusion, and identity verification. We consider each of these below.

In addition to NREGS, Smartcards were also used to make payments in the rural social security pensions (SSP) program, raising the possibility that they might have affected rural markets through this channel. This appears unlikely for at least four reasons. First, the scale and scope of SSP is narrow: only 7% of rural households are eligible (whereas 49.5% have NREGS jobcards). Second, the benefit is modest, with a median and mode of Rs. 200 per month ($\sim \$3$, or less than two days earnings for a manual laborer). Third, the improvements

in pensions from Smartcards were much less pronounced than those in NREGS: there were no improvements in the payments process, and the reduction in leakage was small in absolute terms (falling from 6% to 3%) – in part because payment delays and leakage rates were low to begin with. Fourth, and perhaps most important, the SSP programs were targeted to the poor who were *not able to work* (and complemented the NREGS, which was the safety net for those who could).¹² Thus, SSP beneficiaries are those least likely to have affected or been affected by the labor market. As we show later, treatment did not generate income gains in households where all adults were eligible for the SSP (see Section 4.3).

The creation of Smartcard-linked bank accounts might also have affected local economies by promoting financial inclusion. In practice, this appears not to have been the case. This was the result of a conscious choice by the government, which was most concerned about delayed payments, underpayment, and ghost accounts, and therefore did not allow undisbursed funds to remain in Smartcard accounts. Instead they pressured banks to fully disburse NREGS wages as soon as possible to improve compliance with the 14-day statutory requirement for payment delivery. Further, the bank accounts created had limited functionality: they were not connected to the online core banking servers and instead relied on offline authentication with periodic reconciliation, and as a result could only be accessed through a single Customer Service Provider. Reflecting these factors, only 0.3% of households in our survey reported having money in their account, with an (unconditional) mean balance of just Rs. 7 (~5% of daily wage for unskilled labor).¹³ Note that we only need to rule out *direct* financial inclusion through Smartcard-enabled bank accounts. Increases in borrowing and access to informal credit that result from an improved NREGS are one of the mechanisms for potential economic impact which we examine (and find evidence of) in Section 4.7.

Finally, Smartcards were not considered legally valid proof of identity or otherwise usable outside the NREGS and SSP programs, in contrast with the more recent national ID program *Aadhaar*. Specifically, unlike *Aadhaar*, the database of Smartcard accounts was never de-duplicated, which precluded the legal use of Smartcards as a proof of identity.

Overall, the Smartcard intervention was run by GoAP's Department of Rural Development with the primary goal of improving the payments process and reducing leakage in the NREGS and SSP programs, but was not integrated into any other program or function either by the government or the private sector. Since (as described above) we can rule out the SSP improvement channel and financial inclusion channel, we interpret the results below as consequences of improving NREGS implementation.

¹²Specifically, pensions are restricted to those who are Below the Poverty Line (BPL) *and* either widowed, disabled, elderly, or had a displaced traditional occupation.

¹³See Mukhopadhyay et al. (2013), especially pp. 54-56, for a more detailed discussion on why Smartcards were not able to deliver financial inclusion.

3 Research design

3.1 Randomization

We summarize the randomization design here, and refer the reader to MNS for further details. The experiment was conducted in eight districts with a combined rural population of around 19 million in the erstwhile state of Andhra Pradesh.¹⁴ As part of a Memorandum of Understanding with JPAL South Asia, GoAP agreed to randomize the order in which the Smartcard system was rolled out across mandals (sub-districts). We randomly assigned 296 mandals - with average population of approximately 62,500 - to treatment (112), control (45), and a “buffer” group (139). Figure 1 shows the geographical spread and size of these units. We created the (temporal) buffer group to ensure that we could conduct endline surveys before Smartcard deployment began in control mandals, and restricted survey work to treatment and control mandals. We stratified randomization by district and by a principal component of Mandal socio-economic characteristics.

We examine balance in Tables A.3 and A.4. The former shows balance on variables used as part of stratification, as well as other Mandal characteristics from the census. Treatment and control mandals are reasonably well balanced, with differences significant at the 5% level in 2 out of 22 cases. The latter shows balance on focal outcomes for this paper along with other socio-economic household characteristics from our baseline survey. Four out of 34 variables are significantly different at the 10% level, slightly more than one might expect by chance. We test the sensitivity of results to chance imbalances by controlling for village level baseline mean values of the outcomes.

3.2 Data

Our first data source is the Socio-Economic and Caste Census (SECC), an independent nation-wide census for which surveys in Andhra Pradesh were conducted during 2012, our endline year. The SECC aimed to enable governments to rank households by socio-economic status in order to determine which were “Below the Poverty Line” (BPL) and thereby eligible for various benefits. The survey collected data on income categories for the household member with the highest income (less than Rs. 5000, between Rs. 5000-10,000, and greater than Rs. 10,000), the main source of this income, household landholdings (including amount of irrigated and non-irrigated land), caste, and the highest education level completed for

¹⁴The 8 study districts are similar to AP’s remaining 13 non-urban districts on major socioeconomic indicators, including proportion rural, scheduled caste, literate, and agricultural laborers; and represent all three historically distinct socio-cultural regions (see Table A.1). Tables A.1 and A.2 compare study and non-study districts and mandals, and are reproduced exactly from MNS.

each member of the household. The SECC was conducted using the layout maps and lists of houses prepared for the 2011 Census. The SECC data include slightly more than 1.8 million households in our study mandals.

We complement the broad coverage of the SECC data with original and more detailed household surveys, that are representative of the universe of NREGS jobcard holders - who are the intended beneficiaries of the program. We conducted these surveys during August to October of 2010 (baseline) and 2012 (endline). Surveys covered both participation in and experience with NREGS, annual earnings and expenditure, and the current stock of assets and liabilities. Within earnings, we asked detailed questions about household members' labor market participation, wages, reservation wages, and earnings during June, the month of peak NREGS participation in Andhra Pradesh.

We drew a sample of jobcard holders over-weighting those who had recently participated in the program according to official records.¹⁵ In Andhra Pradesh, 49.5% of rural households have a jobcard (our calculations from the National Sample Survey (NSS) Round 68 in 2011-12). Consistent with NREGS's aim of supporting the rural poor who depend on manual labor, jobcard-holding households are much more likely to work as agricultural laborers, and are less likely to be self-employed outside agriculture; they are also larger and more likely to belong to historically disadvantaged scheduled castes (Table A.5).¹⁶

We sampled a panel of villages and a repeated cross-section of households from these villages using the full universe of jobcard holders at the time of each survey as the frame.¹⁷ The sample included 880 villages, with around 6 households per village. This yielded us 5,278 households at endline, of which we have survey data on 4,943 households; of the remaining, 200 were ghost households, while we were unable to survey or confirm existence of 135 (corresponding numbers for baseline are 5,244; 4,646; 68 and 530 respectively).¹⁸

We also use administrative data from several sources. We use data on land under cultivation and the extent of irrigation from the District Statistical Handbooks (DSH) published each year by the Andhra Pradesh Directorate of Economics based on data from the Office

¹⁵We over-weighted recent NREGS participants (as per official payment records) to have more precise estimates of the impact of Smartcards on leakage (reported in MNS), but all results reported in this paper are re-weighted to be representative of the universe of jobcard holders.

¹⁶Thus, while our survey data do not allow us to measure effects on employers of labor, they allow us to measure GE effects on the universe of *potential* NREGS beneficiaries accounting for half the rural population (and not just those who actually worked on the program).

¹⁷As discussed in MNS, we sampled a repeated cross-section (over-weighting households reported to have worked on NREGS recently) because a household panel would have yielded less precise estimates of leakage (since there is considerable variation in household NREGS participation over time).

¹⁸These numbers reflect the NREGS jobcard holder sample and differ from MNS, where we report a larger total sample size that reflects the pooling of two independently drawn samples of NREGS jobcard holders and SSP beneficiaries (to study leakage in representative samples of beneficiaries of each program).

of the Surveyor General of India.¹⁹ We use unit cost data from Round 68 (2011-2012) of the National Sample Survey (NSS) published by the Ministry of Statistics and Programme Implementation. The NSS contains detailed household \times item-level data for a sample representative at the state and sector level (rural and urban). The data cover over 300 goods and services in categories including food, fuel, clothing, rent and other fees or services over mixed reference periods varying from a week to a year. Note that because the overlap between villages in our study mandals and the NSS sample is limited to 60 villages, we use the NSS data primarily to examine price levels, for which it is the best available data source. We also use data on mandal-wise headcounts of livestock from the Livestock Census of India, which is conducted quinquennially by the Government of India. We use data from the 19th round conducted in 2012, which is also the year of our endline survey. Finally, we use geocoded point locations for each census village from the 2001 Indian Census.

Figure 2 presents a summary of the data sources used in this paper, the recall period that they correspond to, and the specific outcomes for which each data source is used.

3.3 Estimation strategy

We first report simple comparisons of outcomes in treatment and control mandals (i.e. intent-to-treat estimates). Our base specification includes district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization (PC_{md}),²⁰ with standard errors clustered at the mandal level:

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

where Y_{imd} is an outcome for household or individual i in mandal m and district d , and $Treated_{md}$ is an indicator for a treatment group mandal. In some cases we use non-linear analogues to this model to handle categorical data (e.g. probit). When using our survey data, we also report specifications that include (when available) the baseline GP-level mean of the dependent variable \bar{Y}_{pmd}^0 to increase precision and assess sensitivity to any randomization imbalances (recall that we have a village-level panel and not a household-level one):²¹

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

¹⁹Details on data sources for the DSH are at: <http://eands.dacnet.nic.in/>, accessed March 22, 2016.

²⁰As in MNS, we include the principal component itself rather than fixed effects based on its strata as treatment status does not vary within a few strata, so that the latter approach implies dropping a few observations and estimating effects in a less representative sample.

²¹We verify in MNS that treatment did not affect either the size or composition of the sampling frame of jobcard holders. Thus, the reported treatment effects are not confounded by changes in the composition of potential NREGS beneficiaries.

where p indexes panchayats or GPs. We easily reject $\gamma = 1$ in all cases and therefore do not report difference-in-differences estimates. Regressions using SECC data are unweighted, while those using survey samples are weighted by inverse sampling probabilities to be representative of the universe of jobcard-holders. When using survey data on wages and earnings we trim the top 0.5% of observations in both treatment and control groups to remove outliers, but results are robust to including them.

An improved NREGS is likely to affect wages, employment, and income through several channels that not only take place simultaneously, but are also likely to interact with each other. Thus, β in Equation 1 should be interpreted as reflecting a composite mix of several factors. This is the policy-relevant general-equilibrium estimate of the total effect on rural economic outcomes of increasing the *effective* presence of NREGS, and is our primary focus; we discuss specific mechanisms of impact in Section 6.

If outcomes for a given unit (household, GP, etc.) depend only on that unit’s own treatment status, then β in Equation 1 identifies a well-defined treatment effect. However, general equilibrium effects need not be confined to the treated units. Upward pressure on wages in treated mandals, for example, might affect wages in nearby areas of control mandals. In the presence of such spillovers, β in Equation 1 could misestimate the “total treatment effect” (TTE), conceptualized as the difference between average outcomes when all units are treated and those when no units are treated. We defer estimation of this TTE to Section 5 and note for now that our initial estimates are likely to be conservative.

4 Results

4.1 Effects on earnings and poverty

Figure A.1 compares the distributions of SECC income categories in treatment and control mandals, using raw data (without district fixed effects conditioned out) to show the absolute magnitudes. We see that the treatment distribution first-order stochastically dominates the control, with 4.1 percentage points fewer households in the lowest category (less than Rs. 5,000/month), 2.6 percentage points more households in the middle category (Rs. 5,000 to 10,000/month), and 1.4 percentage points more in the highest category (greater than Rs. 10,000/month). Table 1a reports experimental estimates of impact, showing marginal effects from logistic regressions for each category individually and an ordered logistic regression across all categories. Treatment significantly increased the log-odds ratio of being in a higher income category, with estimates unaltered by controls for (arguably predetermined) demographic characteristics such as age of household head, caste, and literacy.

The SECC data let us test for income effects in the entire population, but have two limitations when it comes to estimating magnitudes. First, much information is lost through discretization: the 4.1% reduction in the share of households in the lowest category which we observe does not reveal the magnitude of their income increase. Second, because the SECC only captures the earnings of the top income earner in each household, it is possible that it over- or under-states effects on overall household earnings.

We therefore turn to our survey data, which are representative of the households registered for NREGS (comprising half the rural population), for a better sense of magnitudes of impact on the population that the program aimed to serve. Columns 1 and 2 of Table 1b report estimated impacts on annual household income, with and without controls for the mean income in the same village at baseline. In both specifications we estimate that that treatment increased annual income by over Rs. 8,700 (90% confidence of [3350,15700]). This is a large effect, equal to 12.7% of the control group mean or 17.9% of the national expenditure-based rural poverty line for a family of 5 in 2011-12, which was Rs. 48,960 (Government of India, 2013). Of course, expenditure- and income-based poverty lines may differ and this comparison is illustrative only. But if these lines were taken as equivalent, we estimate a 4.9 percentage point or 17.4% reduction in poverty for the universe of potential NREGS beneficiaries (Figure A.2).

4.2 Direct versus indirect effects on earnings

In an accounting sense, the effects on earnings and poverty we find above must work through some combination of increases in households' earnings from the NREGS itself and increases in their non-program (i.e. private sector) earnings. We examine this decomposition using our survey data, which includes measures of six income categories: NREGS, agricultural labor income, other physical labor income, income from own farm, income from own business, and miscellaneous income (which includes all remaining sources, including salaried income). In the control group, the average household earns roughly 1/3 of its income from wage labor, primarily in agriculture; 1/3 from self-employment activities, also primarily in agriculture; and the remaining 1/3 from salaried employment and public programs, with the latter making up a relatively small share. NREGS earnings specifically account for just 7% of control group earnings, compared to 93% from other sources (which is broadly consistent with nationally representative statistics, in which the NREGS is a relatively small source of employment).

Columns 3-8 of Table 1b report treatment effects on various income categories separately. Earnings in most categories increase, with significant gains in wage labor – both agricultural and other. Effects on own farm earnings (which include earnings from livestock) are positive

but insignificant. NREGS earnings increase modestly ($p = 0.12$) and the increase in *annual* NREGS earnings is consistent with treatment effects on *weekly* NREGS earnings reported in MNS (estimated during the peak NREGS period).²²

Overall, the increase in NREGS income accounts for only 10% of the increase in total earnings (proportional to the share of NREGS in control group income). Nearly 90% of the income gains are attributable to non-NREGS earnings, with the primary driver being an increase in earnings from market labor, both in the agricultural and non-agricultural sectors.

4.3 Distribution of earnings gains

Figure A.2 plots the empirical CDF of household earnings for treatment and control groups in our survey data. We see income gains throughout the distribution, with the treatment income distribution in the treatment group first-order stochastically dominating that in the control group. Finally, the broad-based gains seen in the universe of NREGS jobcard holders (comprising two thirds of the rural population) are also seen in the the SECC data representing the full population (Figure A.1). One caveat, given the wage results we report below, is that the SECC earnings measure likely does not capture effects on the profits of landholders because it is coarsely topcoded.²³

Table A.6 tests for differential treatment effects in our survey data by household characteristics using a linear interaction specification. We find no differential impacts by caste or education, suggesting broad-based income gains consistent with Figure A.2. More importantly, we see that the treatment effects on earnings are *not seen* for households who are less likely to work (those headed by widows or those eligible for social security pensions). Since a household with a pension-eligible resident may also have working-age adults, we examine heterogeneity by the fraction of adults in the households who are eligible for pensions, and see that there are no income gains for households where all adults are eligible for pensions. This confirms that (a) labor market earnings are the main channel for increased income, and (b) improvements in SSP payments from Smartcards are unlikely to be responsible for the large increases in earnings we find.

²²In MNS, we report a significant increase in weekly earnings of Rs. 35/week during seven weeks corresponding to the peak NREGS season. Average weekly NREGS earnings per year are 49.6% of the average weekly NREGS earnings in these seven weeks (calculated using official payment records in the control mandals as shown in Figure A.3). Thus, the annualized treatment effect on NREGS earnings should be Rs. 35 X 52 weeks X 0.496 or Rs. 903/year, which is exactly in line with the Rs. 914 measured in the annual recall data reported in Table 1b. However, the results here are marginally insignificant ($p = 0.12$) compared to the significant ones in MNS, likely due to the lower precision of annual recall data compared to the more precise data collected for the seven-week reference period in MNS, with job cards on hand to aid recall.

²³The SECC measure is also based on a question phrasing that respondents could have interpreted as referring to labor income.

In summary, evidence on the distribution of effects suggests that the increase in earnings were broad-based across categories of households who were registered for the NREGS, but did not accrue to households whose members were unable to work.

4.4 Effects on private labor markets

4.4.1 Wages

To examine wage effects we use our survey data, as the SECC does not include wage information. We define the dependent variable as the average daily wage earned on private-sector work across all respondents who report a private-sector wage. We report results for the full sample of workers reporting a wage, and also check that results are robust to restricting the sample to adults aged 18-65, with additional checks for robustness with respect to sample composition in Section 4.8 below.

We estimate a significant increase of Rs. 7.8 in daily market wages (Table 2, Column 2). This is a large effect, equal to 6.1% of the control group mean. In fact, it is slightly larger than the highest estimates of the market-wage impacts of the rollout of the NREGS itself as reported by Imbert and Papp (2015).

One mechanism that could contribute to this effect is labor market competition: a (better-run) public employment guarantee may improve the outside option for workers, putting pressure on labor markets that drives up wages and earnings. Theoretical models emphasize this mechanism (Ravallion, 1987; Basu et al., 2009), and it has motivated earlier work on NREGS wage impacts (e.g. Imbert and Papp (2015)), but prior work has not been able to directly test for this hypothesis in the absence of data on reservation wages.

We are able to test this prediction using data on reservation wages that we elicited in our survey. Specifically, we asked respondents if in the month of June they would have been “willing to work for someone else for a daily wage of Rs. X,” where X started at Rs. 20 (15% of average wage) and increased in Rs. 5 increments until the respondent agreed. One advantage of this measure is that it applies to everyone, and not only to those who actually worked. Respondents appeared to understand the question, with 98% of those who worked reporting reservation wages less than or equal to the wages they actually earned (Table A.7).

We find that treatment significantly increased workers’ reservation wages by approximately Rs. 5.5, or 5.7% of the control group mean (Table 2, columns 3-4). The increase in reservation wage in treated areas provides direct evidence that making NREGS a more appealing option would have required private employers to raise wages to attract workers. Finally, as further evidence of general equilibrium effects, we find that there is no difference in the increase in market wages as a function of whether the worker actually worked on NREGS (Table A.9).

Consistent with market wages increasing for *all* workers, we see that the gains in income seen in Figure A.2 occur all the way up to per-capita incomes of Rs. 40,000/year (or 4 times the poverty line), which likely includes workers who did not actively participate in NREGS.

4.4.2 Employment and Migration

Next, we examine how labor market participation was affected by this large wage increase. We classify days spent during the month of June into three categories: days spent idle or doing unpaid work, days spent working on the NREGS, and days spent working in the private sector. We report results for the full sample of workers and also check that results are robust to restricting the sample to adults aged 18-65 in Section 4.8 below.

We find a significant *decrease* of 1.2 days per month in days spent idle, equal to 7.1% of the control group mean (Table 3, columns 5 & 6). This time appears to have been reallocated across both NREGS work and private sector work, which increase by roughly 0.5 days per month each (though these changes are not individually significant) (columns 1-4).²⁴ The lack of a decline in private sector employment is not simply because there is no private sector work in June. Figure A.4 plots the full distribution of private sector days worked for treatment and control mandals separately, showing gains spread fairly evenly throughout the distribution and 51% of the sample reporting at least some private sector work in June.^{25, 26}

This pattern of labor supply impacts may or may not be consistent with those in Imbert and Papp (2015). They estimate a 1-for-1 reduction in “private sector employment” as NREGS employment increases, but their measure of private sector employment (based on NSS data) includes wage employment for others *as well as* domestic work and self-employment. They also study impacts on a different population at a different time.²⁷

²⁴Note that in Table 5 of MNS, we report impacts on the extensive margin of whether a household worked on NREGS (and find a significant positive impact in treated areas) because our main concern there was with impact on *access to work*. Here we focus on decomposing total change in employment across NREGS and market labor, and hence present results on average days worked.

²⁵Note that the number of observations for days worked on NREGS is larger: This is because we can impute zero time spent working on the NREGS in June for individuals who reported never working on NREGS. In contrast, we do not impute missing values for private-sector work. Response rates for private-sector work do not differ by treatment status (Table A.7), and results are unchanged if we restrict attention to respondents for whom we observe all three outcomes (Table A.8a)

²⁶Our focus in this paper is on household-level economic outcomes and not on intra-household heterogeneity. For completeness, we examine heterogeneity of wage and employment effects by gender in Table A.11. Point estimates of the impacts on female wages are lower than those on male wages, but not significantly so. On employment, the increase in days worked is always greater for men than for women, but the differences are not always significant.

²⁷In particular, they study NREGS during its early years, when the focus was on providing employment as opposed to construction of productive assets. There is evidence that the emphasis of NREGS shifted towards creating productive public assets by the time of our study (Narayanan, 2016), which may partly explain the positive effects on employment (after adjusting for spillovers) that we find in Section 5.2.

Finally, we examine impacts on labor allocation through migration. Our survey asked two questions about migration for each family member: whether or not they spent any days working outside of the village in the last year, and if so how many such days. Table A.10 reports effects on each measure. We estimate a small and insignificant *increase* in migration on both the extensive and intensive margins. The former estimate is more precise, ruling out reductions in the prevalence of migration greater than 1.0 percentage point, while the latter is less so, ruling out a 58 percent or greater decrease in total person-days. As our migration questions may fail to capture permanent migration, we also examine impacts on household size and again find no significant difference. These results are consistent with the existence of countervailing forces that may offset each other: higher rural wages may make migration less attractive, while higher rural incomes make it easier to finance the search costs of migration (Bryan et al., 2014; Bazzi, 2017).

4.5 Seasonality and Magnitude of Overall Income Effects

Our point estimates for *annual* earnings are broadly consistent with those for wages and employment during June 2012. Specifically, the 13.4% increase in non-NREGS earnings is roughly equal to the sum of the 6.1% increase in wages and (insignificant) 6.7% increase in employment. These are our most precise measures of labor market activity, as they referred to a period shortly before surveys were conducted (see data collection timeline in Figure 2).

While we do not have similarly detailed data for the entire annual cycle, those we have suggest that impacts persisted throughout the year. In interviews with village leaders we asked them to report the “going wage rate” for each month of the year. Figure A.5 plots impacts on this measure by month; the estimates are imprecise (since we have only one data point per village), but suggest that wage appreciation persisted throughout the year.

This pattern is consistent with several (non mutually exclusive) interpretations. To the extent that wage impacts are due to increased NREGS asset creation, we would expect them to persist and potentially even increase throughout the year. To the extent they are driven by the effects of a more attractive NREGS “outside option,” we would expect them to mirror the availability of NREGS work; as Figure A.6 shows, almost all study villages had at least one NREGS project active for a majority of 2012, with availability dropping to a low of 40-50% of villages towards the end of the year.²⁸ Finally, wages may be linked across time due to various forms of nominal rigidity, including concerns for fairness (Kaur, forthcoming) and labor tying over the agricultural cycle (Bardhan, 1983; Mukherjee and Ray, 1995). The latter literature in particular suggests that landlords who provide wage insurance in the lean

²⁸While not much NREGS work appears to have been done during the end of the year (Figure A.3), the presence of active projects suggests that NREGS may still have been a viable outside option in many villages.

season pay lower wages in the peak season. In these models, better NREGS availability and higher market wages in the lean season would imply a reduced need for insurance from landlords and a resulting higher wage in the peak (non-NREGS) season.

To summarize, the magnitude of the *annual* income gains we find are consistent with the estimated changes in wages and employment estimated precisely during the *peak NREGS period* where we have detailed data. However, as the discussion above suggests, there are several reasons for why the wage increases may persist throughout the year, and the (limited) data we have are consistent with this.

4.6 Effects on consumer goods prices

One potential caveat to the earnings results above is that they show impacts on nominal, and not real, earnings. Given that Smartcards affected local factor (i.e. labor) prices, they could also have affected the prices of local final goods, and thus the overall price level facing consumers, if local markets are not sufficiently well-integrated into larger product markets. To test for impacts on consumer goods prices we use data from the 68th round of the National Sample Survey. The survey collected data on expenditure and number of units purchased for a wide variety of goods; we define unit costs as the ratio of these two quantities. We restrict the analysis to goods that have precise measures of unit quantities (e.g. kilogram or liter) and drop goods that likely vary a great deal in quality (e.g. clothes and shoes). We then test for price impacts in two ways. First, we define a price index P_{vd} equal to the price of purchasing the mean bundle of goods in the control group, evaluated at local village prices, following Deaton and Tarozzi (2000):

$$P_{vd} = \sum_{c=1}^n \bar{q}_{cd} \tilde{p}_{cv} \quad (3)$$

Here \bar{q}_{cd} is the estimated average number of units of commodity c in panchayats in control areas of district d , and \tilde{p}_{cv} is the median unit cost of commodity c in village v . Conceptually, treatment effects on this quantity can be thought of as analogous to the “compensating variation” that would be necessary to enable households to continue purchasing their old bundle of goods at the (potentially) new prices.²⁹

The set of goods for which non-zero quantities are purchased varies widely across households and, to a lesser extent, across villages. To ensure that we are picking up effects on prices (rather than compositional effects on the basket of goods purchased), we initially re-

²⁹Theoretically we would expect any price increases to be concentrated among harder-to-trade goods. Since our goal here is to understand welfare implications, however, the overall consumption-weighted index is the appropriate construct.

strict attention to goods purchased at least once in every village in our sample. The major drawback of this approach is that it excludes roughly 40% of the expenditure per village in our sample. We therefore also present a complementary set of results in which we calculate (3) using all available data. In addition, we report results using (the log of) unit cost defined at the household-commodity level as the dependent variable and including all available data. While these later specifications potentially blur together effects on prices with effects on the composition of expenditure, they do not drop any information.

Regardless of method, we find little evidence of impacts on price levels (Table 4). The point estimates are small and insignificant and, when we use the full information available, are also precise enough to rule out effects as large as those we found earlier for wages. These results suggest that the treated areas are sufficiently well-integrated into product markets that higher local wages and incomes did not affect prices of the most commonly consumed items, and can thus be interpreted as real wage and income gains for workers.

4.7 Balance sheet effects

If households interpreted the income gains measured above as temporary (or volatile), we would expect to see them translate into the accumulation of liquid or illiquid assets. Our survey collected information on two asset categories: liquid savings and land-ownership. We find positive estimated effects on both measures (Table 5), with the effect on land-ownership significant; treatment increased the share of households that owned some land by 4.9 percentage points, or 8.3%. This likely reflects the sale of land from those that had a lot of it (who were outside our sample of jobcard holders) to those that had none, and we find that the distribution of landholding in treatment group first-order stochastically dominates that in the control group ($p = 0.013$) (Figure A.7).

We also see a 16% increase in total borrowing, which could reflect either crowding-in of borrowing to finance asset purchases or the use of those assets as collateral. Importantly, this is driven entirely by increases in informal borrowing, with no increase in borrowing from formal financial institutions, consistent with the fact that Smartcards were not a viable means of accessing financial services beyond public-sector benefits (Table A.12).

After land, livestock are typically the most important asset category for low-income households in rural India, and a relatively easy one to adjust as a buffer stock. We test for effects on livestock holdings using data from the Government of India’s 2012 Livestock Census. The Census reports estimated numbers of 13 different livestock categories; in Table 6 we report impacts on the 9 categories for which the average control mandal has at least 100 animals. We find positive impacts on every category of livestock except one, including sub-

stantial increases in the number of buffaloes ($p < 0.001$), dogs ($p = 0.067$), backyard poultry ($p = 0.093$), and fowls ($p = 0.104$). A Wald test of joint significance across the livestock categories easily rejects the null of no impacts ($p = 0.01$). The 50% increase in buffalo holdings is especially striking since these are among the highest-returning livestock asset in rural India, but often not accessible to the poor because of the upfront costs of purchasing them (Rosenzweig and Wolpin, 1993).

Overall, we see positive impacts on holdings of arguably the two most important investment vehicles available to the poor (land and livestock). This is consistent with the view that households saved some or all of the increased earnings they received due to Smartcards, and acquired productive assets in the process. The livestock results are particularly convincing as evidence of an increase in total productive assets in treated areas because they (a) come from a census, and (b) represent a *net* increase in assets, whereas increased land-ownership among NREGS jobcard holders must reflect net sales by landowners.

Any residual earnings not saved or invested should show up in increased expenditure, but our power to detect such effects is limited, as expenditure was not a focus of our household survey.³⁰ With that caveat in mind, Table A.13 shows estimated impacts on household expenditure on both frequently (columns 1 & 2) and infrequently (columns 3 & 4) purchased items from our survey. Both estimates are small and statistically insignificant, but not very precisely estimated. In particular, we cannot rule a 8% increase in expenditure on frequently purchased items or a 15% increase in spending on infrequently purchased items. In Column 5 we use monthly per capita expenditure as measured by the NSS, which gives us a far smaller sample but arguably a more comprehensive measure of expenditure. The estimated treatment effect is positive and insignificant, but again imprecise, and we cannot rule out a 16% increase in expenditure (consistent with a marginal propensity to consume ranging from 0 to 1, and hence not very informative).

4.8 Robustness & other concerns

The estimated income effects in Table 1b are robust to a number of checks. Results are similar using probits or linear probability models instead of logits (Table A.14). They are also robust to alternative ways of handling possible outliers; including observations at the top 0.5% in treatment and control does not change the results qualitatively (Table A.15).

Our wage results are robust to alternative choices of sample. The main results include

³⁰The entire expenditure module in our survey was a single page covering 26 categories of expenditure; for comparison, the analogous NSS consumer expenditure module is 12 pages long and covers 23 categories of *cereals alone*. The survey design reflects our focus on measuring leakage in NREGS earnings and impacts on earnings from deploying Smartcards.

data on anyone in the household who reports a market wage. Restricting the sample to only those of working age (18-65) again does not affect results for either wages (Table A.16) or employment (Table A.8b). Next, dropping the small number of observations who report wages but zero actual employment again does not matter (Table A.16). Results are also robust to estimating wage effects in logs rather than levels, though impacts on reservation wages become marginally insignificant ($p = 0.11$, not reported).

Given that we only observe wages for those who work, the effects we estimate could potentially reflect changes in who reports work (or wages) rather than the distribution of market wages. We test for such selection effects as follows. First, we confirm that essentially all respondents (99%) who reported working also reported the wages they earned, and that non-response is the same across treatment and control (first row of Table A.7). Second, we check that the probability of reporting any work is not significantly different between treatment and control groups (Table A.7). Third, we check composition and find that treatment did not affect composition of those reporting in Table A.17. Finally, as we saw above treatment also increased reservation wages, which we observe for nearly the entire sample (89%) of working-age adults (including those reporting no actual work).

5 Spatial spillovers

Improving NREGS implementation in one mandal may have affected outcomes in other neighboring mandals. We turn now to testing for such spillover effects and to estimating “total” treatment effects that account for them. Our goal is twofold: First, to validate the ITT results using a different source of variation, and second, to provide a sense of how the policy effects of rolling out NREGS universally would compare with the naive ITT estimates.

As with any such spatial problem, outcomes in each GP could in principle be an arbitrary function of the treatment status of all the other GPs. No feasible experiment could identify these functions nonparametrically. We therefore take a simple approach, modeling spillovers as a function of the fraction of GPs (N_p^R) within various radii R of a given panchayat p that were assigned to treatment. Figure 3 illustrates the construction of this measure.³¹

The random assignment of mandals to treatment does not necessarily ensure that the neighborhood measure N_p^R is also “as good as” randomly assigned. To see this, consider constructing the measure for GPs in a treated mandal: on average, GPs closer to the center of the mandal will have higher values of N_p^R (as more of their neighbors are from the same

³¹Note that we implicitly treat GPs assigned to mandals in the “buffer group” as untreated here. Treatment rolled out in these mandals much later than in the treatment group and we do not have survey data to estimate the extent to which payments had been converted in these GPs by the time of our endline.

mandal), while those closer to the border will have lower values (as more of their neighbors are from other mandals). The opposite pattern will hold in control mandals. Thus, we cannot interpret a coefficient on N_p^R as solely a measure of spillover effects without making the (strong) assumption that the direct effects of treatment are unrelated to location.³²

To address this issue, we construct a second measure \tilde{N}_p^R defined as the fraction of GPs within a radius R of panchayat p which were assigned to treatment and *within a different mandal*, so that both the numerator and denominator in \tilde{N}_p^R exclude the GPs in the same mandal. This has the advantage of being exogenous conditional on own treatment status, and the disadvantage that it is not defined for a GP when R is so small that p is more than R kilometers from the nearest border. We use this measure both to test for the existence of spillovers and as an instrument for estimating the effects of N_p^R , which we view as the “structural” variable of interest.

To examine the sensitivity of our conclusions to the definition of “neighborhoods”, we measure neighborhood treatment intensity at radii of 10, 15, 20, 25, and 30 kilometers. These distances are economically relevant given what we know about rural labor markets. For instance, workers can travel by bicycle at speeds up to 20 km / hour, so that working on a job 30 km from home implies a high but not implausible daily two-way commute of 3 hours. Of course, effects could also propagate much further than the distance over which any single actor is willing to arbitrage, with changes in one market rippling on to the next.

Figure A.8 plots smoothed kernel density estimates by treatment status for N_p^R and for \tilde{N}_p^R . As discussed above, the former is mechanically correlated with own treatment status (Panel A), while the latter is not (Panel B). Tables A.18 and A.19 report tests showing that our outcomes of interest are balanced with respect to these measures at baseline.³³

5.1 Testing for spillovers

To test for the existence of spillovers, we estimate

$$Y_{ipmd} = \alpha + \beta \tilde{N}_p^R + \delta District_d + \lambda PC_{md} + \epsilon_{imd} \quad (4)$$

separately for the treatment and control groups. This approach allows for the possibility that neighborhood effects differ depending on one’s own treatment status. We also estimate a variant that pools both treatment and control groups (and adds an indicator for own

³²Merfeld (2017) finds intra-district differences in wages as a function of distance to the district border, suggesting that this assumption may not hold.

³³A richer model of spillovers might allow for treated GPs at different radii to have different effects – for example, the share of treated GPs at 0-10km, 11-20km, etc. might enter separately into the same model. We do not have sufficient power to distinguish these effects statistically, however (results not reported).

treatment status), which imposes equality of β across groups. In either case, we interpret rejection of the null $\beta = 0$ as evidence of spillover effects.

We find robust evidence of spillover effects on market wages, consistent in sign with the direct effects we estimated above (Table 7, columns 1-5). The effects are strongest for households in control mandals, where we estimate a significant relationship at all radii greater than 10km; for those in treatment mandals the estimates are smaller and significant for three out of five radii, but uniformly positive. For days spent on unpaid work or idle, the estimated effects are all negative, and significant except at smaller radii when we split the sample (Table 7, columns 6-10). Since we never reject equality of β across control and treatment groups, the pooled samples provide the most power, and we estimate significant spillover effects at all radii (except at $R = 10$ for days spent on unpaid work or idle).³⁴

These spillover results validate the ITT ones with a completely different source of variation (since the measure of exogenous spatial exposure to treatment that we define and use is uncorrelated with the direct treatment status of a sampled village). They also suggest that the ITT results may be biased downwards and need to be corrected for, which we do next.

5.2 Estimating total treatment effects

The conceptual distinction between the unadjusted and total treatment effects can be seen in Figure 4. The difference between the intercepts (β_T) represents the effect of a village being treated when none of its neighbors are treated, and movement along the x-axis represents the additional effect of having more neighbors treated. Thus, the unadjusted treatment effects (reported in Section 4), represented by y_{ITT} , captures both the effect of a village being treated, and the mean difference in the fraction of treated neighbors between treatment and control villages (x_{ITT}), which is positive but less than 1 (as seen in Figure A.8). The total treatment effect, represented by y_{TTE} , is the difference in expected outcomes between a village in a treated mandal with 100% of its neighborhood treated ($T_m = N_p^R = 1$), and that for a village in a control mandal with 0% of its neighborhood treated ($T_m = N_p^R = 0$). This is inevitably a partially extrapolative exercise, as much of our sample is in neither of these conditions (as seen in Panel A of Figure A.8). Nevertheless, we are interested in estimating it since it is this “total” effect one would ideally use for determining policy impacts under a universal scale up of the program.

³⁴We can also use the same methods to estimate the effects of having GPs in mandals assigned to the intermediate “buffer” wave as neighbors. These effects are hard to interpret, as we do not have data from the field to assess the extent to which buffer mandals had been treated at the time of our endline survey. In practice, the point estimates on the percent of neighboring GPs in buffer mandals are generally the same sign as the point estimates on treated neighbors but smaller and insignificant (consistent with some rollout in these mandals - greater than zero, but less than the extent in the treated mandals)

To estimate this effect with our data, we first estimate

$$Y_{ipmd} = \alpha + \beta_T T_m + \beta_N N_p^R + \beta_{TN} T_m \cdot N_p^R + \delta District_d + \lambda PC_{md} + \epsilon_{imd} \quad (5)$$

Here β_T captures the effect of own treatment status, β_N the effect of neighborhood treatment exposure, and β_{TN} any interaction between the two. The total treatment effect of a universal rollout of treatment (with all neighbours being treated) is then given by:

$$\bar{\beta} = \beta_T + \beta_N + \beta_{TN} \quad (6)$$

Since N_p^R is potentially endogenous for reasons discussed above, we instrument for it in (5) using \tilde{N}_p^R (and instrument for its interaction with T_m using the corresponding interaction). Not surprisingly, we estimate a strong first-stage relationship between the two, with a minimum F -statistic across specifications reported here of $F = 115$. For completeness we also report OLS estimates of (5) in Table A.20; these are qualitatively similar to the results we present here, and mostly significant, but less precise.

After adjusting for spillovers, we estimate total treatment effects (TTE) on wages and employment – which are (i) significant for most or all values of R , (ii) consistent in sign with those we estimate in simple binary treatment specifications, and (iii) meaningfully larger, suggesting that the unadjusted estimates may be significantly biased downwards (as suggested by Figure 4).

Table A.21 presents the results of estimating Equation (5) above, and also calculates the TTE as shown in Equation (6). We focus our discussion on the results in Table 8, which present the TTE calculated above, the unadjusted treatment effects, and formal tests of equality of the two.³⁵

We begin in panel (a) with wage outcomes. Depending on choice of R , the estimated TTE on realized wages is 14-20% of the control mean, and uniformly significant. Further, these estimates are typically three times as large as the unadjusted estimates in Table 2, suggesting that not accounting for spatial spillovers could substantially understate the impact of NREGS on market wages. The estimated TTE on reservation wages is 8-10% of the control mean, and also larger than the unadjusted estimates (though not significantly so).

In panel (b) we examine impacts on labor allocation. As above, we see that adjusting for spillovers makes the results in Table 3 stronger. Most importantly, we see that TTE of

³⁵To do this, we use equation-by-equation generalized method of moments estimation, and estimate our specifications for unadjusted and total treatment effects on the same analysis sample (which matches the analysis sample of Table A.21). Note that the estimates for the unadjusted treatment effect differ slightly from those from Tables 1b, 2, and 3 as the analysis sample only includes observations where the spatial exposure measures are defined.

work done in the private sector is positive and significant (for all $R > 10\text{km}$), suggesting that improved NREGS raised not only wages, but also raised private sector employment. The differences with the unadjusted estimates are substantial and underscore the extent to which estimates that do not adjust for spillovers may be biased. Correspondingly, we also see a larger reduction in the total number of days spent idle or doing unpaid work.³⁶

One natural question about these estimates is whether specifying outcomes as linear functions of N_p^R yields a good fit. We prefer linear models as the relationship between N_p^R and our main outcomes of interest does not display any obvious curvature (Figures A.9 and A.10), implying that fitting higher-order polynomials to the data is very likely to over-fit them. We also estimated variants of (5) that include higher-order terms, however, and obtained similar estimates of the TTE (available on request).

To summarize, our results suggest that there were meaningful spatial spillovers to both treatment and control villages as a function of the fraction of treated neighbors, with the direction of the effects being the same as those of the main effects. These results have three substantive implications. First, they validate our core results using a different source of variation that is orthogonal to the randomization of mandals to treatment status. Second, they reinforce the GE nature of our results by highlighting that the right way to think about the policy-relevant treatment effect is not at the level of the individual or even village, but rather the overall labor market. Third, they suggest that the total policy effects of NREGS are likely to be larger than our experimental ITT estimates and that existing estimates that do not account for spillovers may understate the total effects of NREGS (as shown in a different setting by Miguel and Kremer (2004)).

6 Discussion

Like any change to a complex economy, an improved NREGS could affect economic outcomes in many ways, with multiplier effects, feedback loops, and interactions all contributing to the overall impact. It is thus implausible to decompose effects into distinct, independent channels as in a partial equilibrium analysis. We can still test, however, whether there is evidence that specific mechanisms suggested by theory are operative.

One hypothesis is that a (better-run) employment guarantee puts competitive pressure

³⁶We focus the analysis in Tables 7 and 8 on wages and employment in June, because these are measured relatively precisely with a one-month recall window. We present analogous results for income (measured on the basis of annual recall) in Table A.22 and also report robustness to different levels of top-censoring because the distribution of earnings is right-skewed. Overall, we find the same pattern of results including evidence of spillovers, and point estimates of TTE that are always larger than the unadjusted ITT estimates (though not significantly so).

on labor markets, driving up wages and earnings. Consistent with this, we find a significant positive impact not only on market wages, but also on reservation wages. It is also suggestive that the difference between the unadjusted and total treatment effects in Table 8 is smaller ($\sim 75\text{-}100\%$) and insignificant for reservation wages compared to the difference for market wages ($>300\%$, statistically significant). Changes in reservation wages appear to depend mainly on whether a worker’s own village was treated (reflecting the improvement of NREGS as a *local* outside option), while changes in market wages appear to also depend on the proportion of treated villages – and hence, the number of treated workers – in the *neighborhood*. This suggests that market wages had to respond to the *extent* of increased competition from NREGS in other nearby villages.³⁷

An improved NREGS may also have created additional productive assets – roads, irrigation facilities, soil conservation structures, etc. – which raised productivity. We can rule out large changes in the number and composition of projects *reported* (Table A.23). However, the reductions in leakage, and proportional (albeit insignificant) increase in workers observed at works-sites during unannounced visits reported in MNS, suggest that actual asset creation may have increased (though we do not have independent measures of assets created). We can also test for productivity gains by examining changes in land utilization, which might result from land improvements or minor irrigation works. We do not see effects on the amount of land sown or on the total area irrigated (Table A.24), ruling out effect sizes larger than 16% and 10%, respectively. A caveat, however, is that the irrigation data in district handbooks may reflect only major works undertaken by the Ministry of Irrigation and not the smaller ones typically undertaken under NREGS. Overall, while we find no direct evidence that assets created under NREGS improved productivity, we cannot rule out the possibility.³⁸

Increased cash flow could also potentially stimulate local economic activity, driving up wages and earnings (Magruder, 2013). Because the intervention did not affect NREGS disbursements, this effect is relevant only to the extent that the *redistribution* of some income from officials to workers increased local spending. In practice we find insignificantly positive effects on household expenditure (Table A.13): this is not direct evidence *for* an aggregate demand mechanism, but leaves open the possibility that redistribution towards workers with a higher propensity to consume locally played some role in the effects we find.

Improved NREGS access could also improve credit access. We do see increased borrowing

³⁷Note that changes in reservation wages can affect market wages even without direct bargaining between workers and employers. In practice, workers are often hired in spot labor markets where contractors post wages and hire workers; any reduction in the supply of labor to these markets at a *given* wage will tend to increase the market-clearing wage.

³⁸Unfortunately, we were unable to obtain data on cropping patterns or land prices, which might have provided further evidence on productivity gains.

in treated areas (Table 5), all of which is informal, along with the increased asset ownership among the poor noted above. These changes could then have had secondary effects: land redistribution could increase productivity (Banerjee et al. (2002)) and livestock increases could raise the marginal product of labor (Bandiera et al. (2017)).

Finally, the fact that private sector employment did *not* fall as wages rose is notable given that rising wages in a competitive labor market should reduce employment. This suggests either that productivity improved through one of the channels above and increased labor demand, or that labor markets in rural Andhra Pradesh may be oligopsonistic. There is a long-standing debate over the market power of rural employers, with several qualitative accounts suggesting the existence of such power (e.g. Griffin (1979); Ghosh (2007)). Our evidence is indirect, but is consistent with the possibility that imperfectly competitive labor markets contributed to the overall result pattern.³⁹

In summary, rural economies are complex, and a public employment program is likely to affect them through many mechanisms that operate simultaneously, interact with each other, and accumulate over time. We find evidence consistent with several channels, but emphasize that the absence of evidence for others should *not* be interpreted as evidence of absence.

7 Conclusion

This paper contributes to our understanding of the economic impact of public employment programs in developing countries. In particular, it contributes (a) improved identification: using experimental variation with units of randomization large enough to capture general equilibrium effects and units of measurement granular enough to capture spatial spillover effects; (b) measures of implementation quality: enabling us to interpret impacts as the results of demonstrable changes in *actual* presence of the program; and (c) new outcome measures: including reservation wages, income, and assets, with independent census data on the latter two.

The results suggest that well-implemented public employment programs can be highly effective at raising the incomes of the rural poor in developing countries. In addition, these gains are largely attributable not directly to income from the program, but indirectly to general equilibrium changes that it induced. The fact that market employment *increased* despite higher wages further suggests that these changes may have been efficiency-enhancing.

One natural question is how the effects of *improving* the NREGS compare to the (hypothetical) comparison between a “well-implemented NREGS” and “no NREGS.” We expect that the effects would be broadly comparable, but with larger income effects. Smartcards

³⁹See Manning (2011) and Naidu et al. (2016) for recent evidence of oligopsony in other settings.

increased the labor-market appeal of and participation in NREGS without increasing fund flows. In contrast, the NREGS per se transfers large sums from urban to rural areas. A de novo rollout of a *well-implemented* NREGS would thus likely have effects on wages, employment and income larger than those we find here, though in the same direction.

Our results speak directly to active policy debates on the design of anti-poverty programs in developing countries. For instance, the latest Economic Survey of India asks whether the NREGS budget would be better-spent on direct cash transfers (a “universal basic income”) to the poor (Government of India, Ministry of Finance (2017)). Past analyses have emphasized three reasons that it might not: an employment scheme (a) induces self-targeting towards those willing to do hard physical labor; (b) could create valuable public goods (e.g. roads, shared irrigation facilities); and (c) could address labor market imperfections such as oligopsony power among local employers by forcing wages up towards perfectly competitive levels (e.g. Besley and Coate (1992); Murgai and Ravallion (2005)). Comprehensively assessing these issues is beyond the scope of the paper, but our results do bear on on them. The reductions in unpaid time and increases (adjusting for spillovers) in private sector employment we see are consistent with either (b) or (c). To the extent they result from reducing labor market imperfections, they are clearly efficiency enhancing. To the extent they result from the creation of public goods, they *could* be efficiency-enhancing. Of course the gains may not have been worth the incurred costs, but given the widespread prior that NREGS assets are worthless (World Bank, 2011), even this interpretation is a positive update. On net, the results raise our own posterior beliefs that an EGS could be a cost-effective anti-poverty strategy relative to a direct transfer.

Finally, our results illustrate how the costs of corruption and weak implementation may go beyond the direct costs of diverted public resources and extend to the broader economy (Murphy et al., 1993). Empirical work on corruption has made great strides quantifying leakage as the difference between fiscal outlays and actual receipts by beneficiaries (Reinikka and Svensson, 2004; Niehaus and Sukhtankar, 2013a; Muralidharan et al., 2017) and studying the impacts of interventions on these measures (Olken, 2007; Muralidharan et al., 2016). However, it has been more difficult to identify the broader economic costs of corruption. Our results suggest that weak NREGS implementation may hurt the poor much more through diluting its general equilibrium effects than through the diversion of NREGS wages themselves.⁴⁰ Consequently they also underscore the importance of building state capacity in developing countries for better implementation of social programs.

⁴⁰Analogously, the reduced human potential resulting from corrupt and inefficient health and education service delivery in developing countries likely far exceeds the direct fiscal costs, which are now well-documented (World Bank, 2003; Chaudhury et al., 2006).

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Table 1: Income
(a) SECC data

	Lowest bracket		Middle bracket		Highest bracket		Income bracket 3 levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.041 (.014)	-.039 (.014)	.026 (.011)	.025 (.011)	.014 (.0065)	.012 (.0061)	-.041 (.014)	-.039 (.014)
Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	.01	.028	.014	.024	.015	.041	.008	.024
Control Mean	.83	.83	.13	.13	.038	.038		
N	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M
Estimator	Logit	Logit	Logit	Logit	Logit	Logit	Ordered logit	Ordered logit

(b) Survey data (Rs. per year)

	Total income		NREGA	Agricultural labor	Other labor	Farm	Business	Miscellaneous
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	9511 (3723)	8761 (3722)	914 (588)	3276 (1467)	3270 (1305)	2166 (2302)	-642 (1325)	528 (2103)
BL GP Mean		.025 (.071)						
Adj. R-squared	0.04	0.04	0.01	0.06	0.06	0.02	0.01	0.01
Control Mean	69122	69122	4743	14798	9322	20361	6202	13695
N	4908	4874	4907	4908	4908	4908	4908	4908

This table shows treatment effects on various measures of household income. Panel (a) uses data from the Socioeconomic and Caste Census (SECC), which reports income categories of the highest earner in the household (HH): the “Lowest bracket” corresponds to earning < Rs. (Rupees) 5000/month, “Middle bracket” to earning between Rs. 5000 & 10000/month, and “Highest bracket” to earning > Rs. 10000/month. Columns 1-6 report marginal effects using a logit model. Columns 7-8 report the marginal effects on the predicted probability of being in the lowest income category using an ordered logit model. Control variables, when included, are: age of the head of HH, an indicator for whether the head of HH is illiterate, indicator for whether the HH belongs to a Scheduled Caste/Tribe. Panel (b) shows treatment effects on types of income (in Rupees) using annualized data from our survey. “BL GP Mean” is the GP-level mean of the dependent variable at baseline. “Total income” is total annualized HH income. “NREGS” is earnings from NREGS. “Agricultural labor” captures income from agricultural work for someone else, while “Other labor” is income from physical labor for someone else. “Farm” combines income from a HH’s own land and animal husbandry, while “Business” captures income from self-employment or a HH’s own business. “Miscellaneous” is the sum of HH income not captured by the other categories. We censor observations that are in the top 0.5% percentile of total income in treatment and control. Note that income sub-categories were not measured at baseline so we cannot include the respective lags of those dependent variables. All regressions (in both panels) include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table 2: Wages (June)

	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.6 (3.6)	7.8 (3.6)	4.9 (2.9)	5.5 (2.8)
BL GP Mean		.15 (.053)		.12 (.043)
Adj. R-squared	.07	.07	.05	.05
Control Mean	128	128	97	97
N	7304	7090	12905	12791

This table shows treatment effects on wage outcomes from the private labor market in June using survey data. “Wage realization (Rs.)” in columns 1-2 is the average daily wage (Rs. = Rupees) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” in columns 3-4 is the daily wage at which he or she would have been willing to work for someone else in June 2012 (and is available for nearly all respondents and not just those who reported working for a wage). The outcome is elicited through a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. Observations in the top 0.5% percentile of the respective wage outcome in treatment and control are excluded from each regression. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level in parentheses.

Table 3: Employment (June)

	Days unpaid/idle		Days worked on NREGS		Days worked private sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-1.2 (.59)	-1.2 (.59)	.59 (.39)	.5 (.37)	.44 (.57)	.53 (.56)
BL GP Mean		.16 (.052)		.2 (.04)		.22 (.068)
Adj. R-squared	0.06	0.07	0.03	0.04	0.01	0.02
Control Mean	17	17	3.6	3.6	7.9	7.9
N	14163	14078	18330	18194	14514	14429

This table analyzes labor supply outcomes for June using survey data. “Days unpaid/idle” in columns 1-2 is the sum of days an individual did unpaid work or stayed idle in June 2012. “Days worked on NREGS” in columns 3-4 is the number of days an individual worked on NREGS during June 2012. “Days worked private sector” in columns 5-6 is the number of days an individual worked for somebody else in June 2012. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses. Note that observation count varies between columns due to differences in non-response rates in their corresponding survey questions. A test of non-response rates by treatment status is shown in Table A.7.

Table 4: Prices

	Log of Price Index		Log of Individual Prices
	(1) Uniform goods	(2) All goods	(3)
Treatment	.0041 (.066)	.0048 (.025)	-.011 (.011)
Item FE	No	No	Yes
Adjusted R-squared	0.98	1.00	0.95
Control Mean	11	11	
Observations	60	60	18242
Level	Village	Village	Item x Household

This table reports tests for impacts on price levels using 68th Round National Sample Survey (NSS) data on household consumption and prices. Columns 1 - 2 show analysis of village-level price indices constructed from NSS data using Equation 3. The outcome variable in each column is the log of the respective price index. In column 1 we construct a price index on a set of “uniform goods”, restricting attention to goods purchased at least once in every village in our sample. The major drawback of this approach is that it excludes roughly 40% of the expenditure per village in our sample. In column 2, we calculate the price index using all available data. In column 3, we report impacts on log observed commodity prices at the household level using all available data. The outcome is the log price. Standard errors clustered at the mandal level are in parentheses. All regressions include district fixed effects. Regressions using survey data also include the first principal component of a vector of mandal characteristics used to stratify randomization.

Table 5: Savings, assets and loans

	Total savings (Rs.)		Total loans (Rs.)		Owns land (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1064 (859)	1120 (877)	11210 (4741)	11077 (4801)	.056 (.024)	.049 (.024)
BL GP Mean		.027 (.071)		.038 (.039)		.21 (.042)
Adj. R-squared	0.00	0.00	0.01	0.01	0.01	0.03
Control Mean	2966	2966	68108	68108	.59	.59
N	4916	4882	4943	4909	4921	4887

This table reports impacts on reported household assets (at time of endline survey) using survey data. “Total savings (Rs.)” in columns 1-2 is defined as the total amount of a household’s current cash savings, including money kept in bank accounts or Self-Help Groups. “Total loans (Rs.)” in columns 3-4 is the total principal of the household’s five largest active loans. “Owns land (%)” in columns 5-6 is an indicator for whether a household reports owning any land. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table 6: Livestock

	Cattle	Buffaloes	Sheep	Goats	Pigs	Dogs	Fowls	Ducks	Backyard Poultry
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	1261 (2463)	4671 (1776)	-35 (4593)	2473 (1877)	120 (116)	244 (132)	8150 (4961)	209 (286)	8381 (4980)
Adj. R-squared	.20	.14	.21	.09	.04	.13	.11	.04	.11
Control Mean	11483	9328	33857	10742	275	387	29147	220	29383
N	157	157	157	157	157	157	157	157	157

This table reports impacts on livestock headcounts using mandal-level data from the 2012 Livestock Census. Results for animals with average headcounts greater than 100 in control mandals are included. A Wald test of joint significance rejects the null of no impacts ($p = 0.01$). All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Robust standard errors are included in parentheses.

Table 7: Testing for existence of spatial spillovers

(a) Wage (June)										
	Wage realization (Rs.)					Reservation wage (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	7.8 (6.6)	17 (6.9)	22 (8.6)	21 (8.6)	24 (11)	5.5 (4.9)	7.2 (5.3)	8.3 (6.2)	5.7 (7.1)	-.8 (7)
Treatment	12 (4.4)	11 (5.9)	14 (7.5)	14 (10)	15 (12)	3 (3.1)	2.6 (4)	3.8 (5.1)	4.1 (6.8)	5.5 (8.4)
Pooled	9.9 (3.6)	12 (4.8)	13 (6.2)	13 (7.8)	16 (9.5)	3.3 (2.9)	3.4 (3.5)	3.6 (4.4)	3.1 (5.6)	2.9 (6.4)
F-test for equality	.4	.48	.59	.32	.34	.42	1.2	.76	.067	.86
p-value	.53	.49	.44	.57	.56	.52	.27	.38	.80	.35
N	6560	7049	7192	7245	7269	11614	12498	12732	12818	12852
% of pooled sample	90	97	99	100	100	90	97	99	100	100
(b) Employment (June)										
	Days worked private sector					Days unpaid/idle				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	1.2 (1.1)	1.3 (1.2)	1.8 (1.6)	2.9 (2)	4.3 (2.3)	-.76 (1.2)	-1.5 (1.4)	-1.7 (1.8)	-3.4 (2)	-5.4 (2.2)
Treatment	.42 (.8)	1.3 (1)	1.8 (1.3)	2.5 (1.6)	3.3 (1.9)	-1.1 (.81)	-2.3 (1.1)	-3.1 (1.3)	-3.5 (1.5)	-4.3 (1.8)
Pooled	.81 (.71)	1.4 (.85)	1.8 (1.1)	2.5 (1.3)	3.3 (1.5)	-1.1 (.72)	-2 (.91)	-2.5 (1.1)	-3.3 (1.3)	-4.2 (1.5)
F-test for equality	.78	.0015	.00014	.071	.32	.15	.58	.95	.003	.33
p-value	.38	.97	.99	.79	.57	.69	.44	.33	.96	.56
N	13008	13995	14300	14397	14441	12722	13689	13977	14064	14095
% of pooled sample	90	97	99	100	100	90	97	99	100	100

This table reports tests for the existence of spillovers effects (the impact of \tilde{N}_p^R) on main wage and employment outcomes at radii of 10, 15, 20, 25, 30km from survey data using Equation 4. Analysis was conducted separately for control and treatment subgroups, and then the entire (pooled) sample (this specification includes a treatment indicator). We report the F-statistic and p-value for an adjusted Wald test of equality between estimated spillovers in control and treatment areas for each radius. \tilde{N}_p^R is the ratio of the number of GPs in treatment mandals over the total GPs within a given radius of R km. Note that wave 2 GPs are included in the denominator, and that same-mandal GPs are *excluded* in both the denominator and numerator. Standard errors clustered at the mandal level are in parentheses. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. “% of sample” refers to the % of total observations for an outcome that are used in estimation. Note that the variation in observation counts is due to the construction of the spatial exposure measure: larger radii will include more GPs and observations, particularly since same-mandal GPs are excluded.

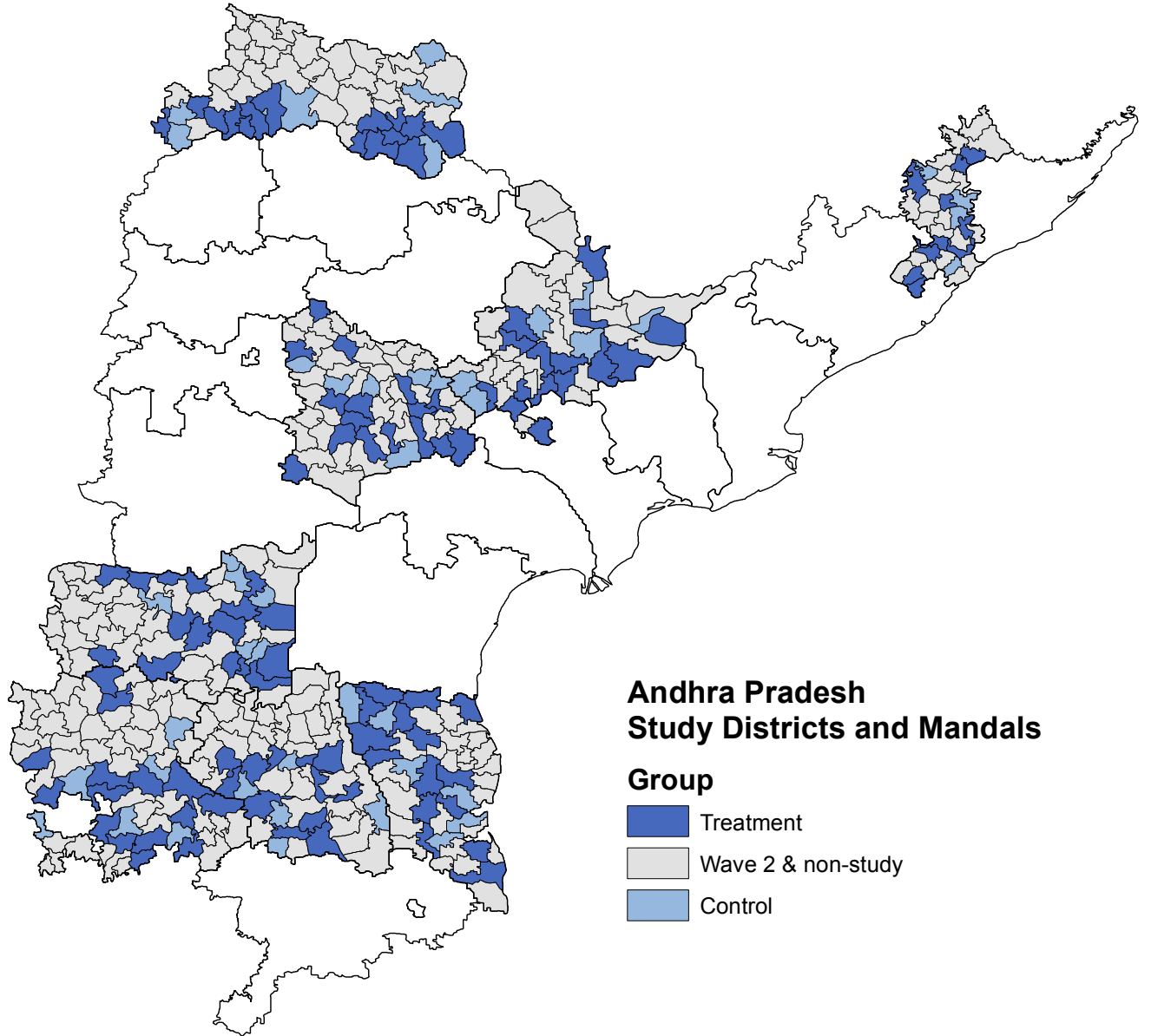
Table 8: Test of equality between unadjusted and total treatment effect estimates

(a) Wage (June)										
	Wage realization (Rupees)					Reservation wage (Rupees)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Total treatment effect	18 (5.6)	23 (6.6)	24 (7.8)	22 (9.1)	26 (11)	8.5 (4)	9.8 (4.6)	9.9 (5.4)	8.7 (6.5)	7.8 (7.4)
Unadjusted treatment effect	7.6 (3.5)	7.1 (3.6)	7 (3.6)	6.6 (3.6)	6.5 (3.6)	4.3 (3)	5 (2.9)	5.1 (2.9)	4.9 (2.9)	4.9 (2.9)
Difference	11 (6.6)	15 (7.5)	17 (8.6)	16 (9.8)	19 (11)	4.2 (5)	4.8 (5.4)	4.7 (6.1)	3.7 (7.1)	3 (7.9)
Chi-square statistic	2.6	4.3	3.8	2.6	2.9	.68	.78	.61	.27	.14
Control Mean	127	128	128	128	128	97	97	97	97	97
N	6560	7049	7192	7245	7269	11614	12498	12732	12818	12852

(b) Employment (June)										
	Days worked private sector					Days unpaid/idle				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Total treatment effect	1.6 (1.1)	2.3 (1.3)	2.8 (1.5)	3.5 (1.7)	4.4 (1.9)	-2.6 (1)	-3.7 (1.3)	-4.3 (1.5)	-5.1 (1.6)	-6.3 (1.8)
Unadjusted treatment effect	.47 (.59)	.45 (.57)	.44 (.57)	.45 (.57)	.45 (.57)	-1.4 (.63)	-1.3 (.59)	-1.2 (.59)	-1.3 (.59)	-1.3 (.59)
Difference	1.1 (1.2)	1.9 (1.4)	2.4 (1.6)	3.1 (1.8)	3.9 (2)	-1.2 (1.2)	-2.5 (1.4)	-3 (1.6)	-3.9 (1.7)	-5 (1.9)
Chi-square statistic	.84	1.7	2.1	2.9	3.9	1.1	3.2	3.5	5.1	7.2
Control Mean	7.8	7.9	7.9	7.9	7.9	17	17	17	17	17
N	13008	13995	14300	14397	14441	12677	13640	13928	14015	14046

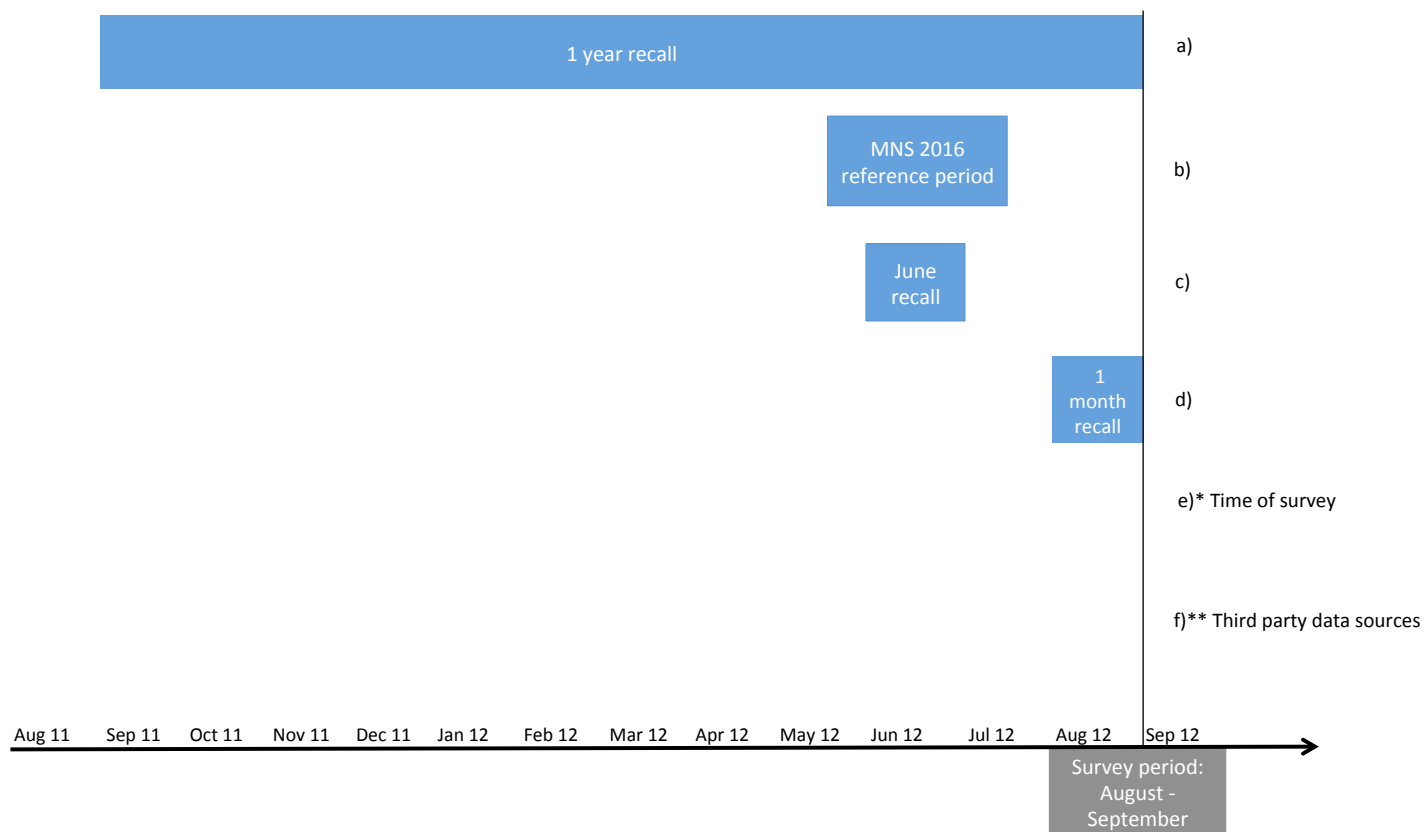
The table reports tests for the equality of total treatment effect (Equations (5) and (6)) and unadjusted treatment effect estimates (Equation (1)) for main wage and employment outcomes at radii of 10, 15, 20, 25, 30km. Using equation-by-equation generalized method of moments estimation, we jointly estimate our specifications for unadjusted and total treatment effects on the same analysis sample (which matches the analysis sample of Table A.21). We report the joint estimates for the unadjusted treatment effect and total treatment effect on this same analysis sample for each outcome and radius. We then report “Difference”, the estimated numerical difference between the unadjusted and total treatment effect estimate for each outcome and radius. Note that the estimates for the unadjusted treatment effect differ slightly from those from Tables 2 and 3 as the analysis sample only includes observations where the spatial exposure measures are defined. Standard errors clustered at the mandal level are in parentheses.

Figure 1: Study districts with treatment and control mandals



This map (reproduced from Muralidharan et al. (2016)) 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 our 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 or because it was entirely urban and had no NREGS (109 out of 405). Randomization was stratified by district and by a principal component of mandal characteristics including population, literacy, proportion of Scheduled Caste and Tribe, NREGS jobcards, NREGS peak employment rate, proportion of SSP disability recipients, and proportion of other SSP pension recipients.

Figure 2: Timeline of endline survey reference periods



This figure shows the timeline of reference periods from our endline survey, which was conducted August - September 2012.

a) The 1 year recall period corresponds to questions about household earnings/income and longer-term expenses.

b) “MNS 2016 reference period” refers to the 7-week reference period from May 18 - July 14, 2012 used in outcomes for MNS 2016, including NREGS work and leakage.

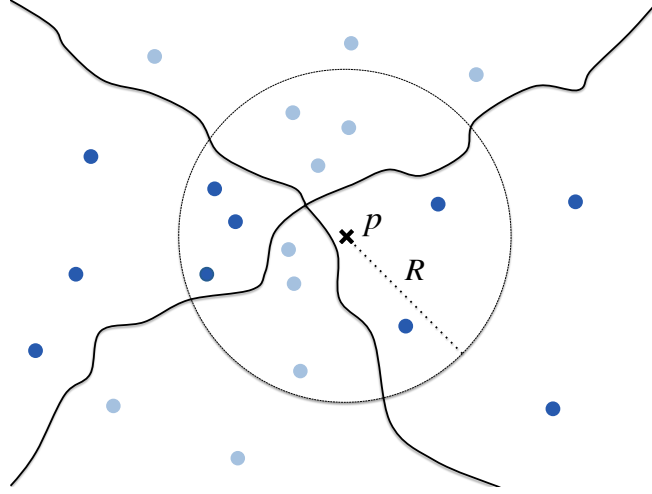
c) The June recall period corresponds to private sector wage outcomes and employment outcomes. To construct NREGS outcomes for June (e.g. days worked on NREGS), we use data from weeks that overlap with month of June, allocating time for the first and last week in proportion to the number of days overlapping in June.

d) The 1 month reference period corresponds to questions on shorter-term expenses.

e)* Respondents were asked about savings, assets (including landholdings), and loans at time of survey.

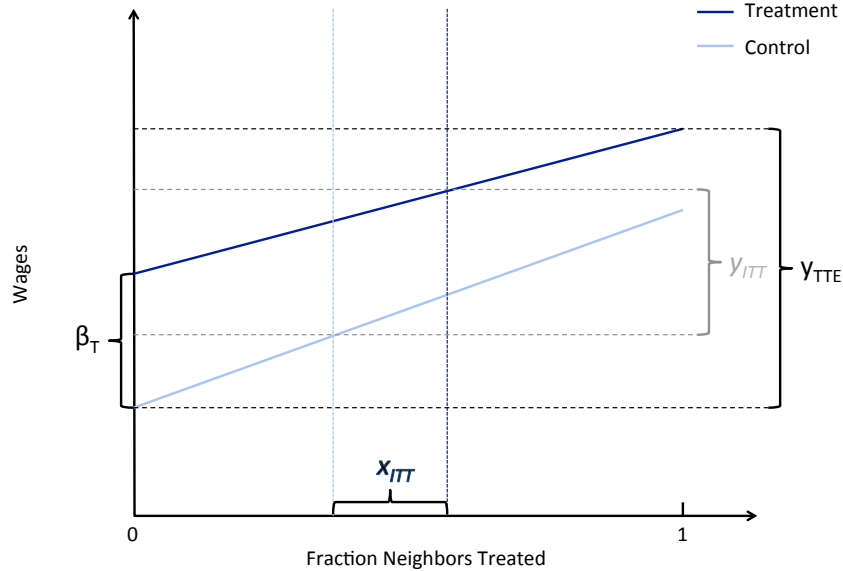
f)** Third party data sources include the 2011- 2012 Socio-Economic and Caste Census (SECC), the 2012 Livestock Census, the 68th Round of the National Sample Survey, and the AP District Statistical Handbook data. The 2011-2012 SECC was conducted in rural AP during 2012. The data contains household-level data on earnings (in month up to time of survey) and land holdings (at time of survey). The 2012 Livestock Census was conducted with October 15, 2012 as the reference date. The data contains mandal-level livestock headcounts by livestock category. The 68th Round of the National Sample survey was conducted in AP between July 2011 - June 2012. The data contains household-level expenditure and number of units purchased for a wide variety of goods (in month up to time of survey). The District Statistical Handbooks (DSH) data, which is published each year by the Andhra Pradesh Directorate of Economics, is from 2012-2013. We use the DSH a for mandal-level data on land utilization and irrigation.

Figure 3: Constructing measures of exposure to spatial spillovers



This figure illustrates the construction of measures of spatial exposure to treatment for a given panchayat p (denoted by the black X symbol) and radius R in a treatment mandal. Dark (light) blue dots represent treatment (control) panchayats; black lines represent mandal borders. As in the text, N_p^R is the fraction of GPs within a radius R of panchayat p which were assigned to treatment. \tilde{N}_p^R is the fraction of GPs within a radius R of panchayat p and within a different mandal (excluding GPs in the same mandal from both the numerator and denominator) which were assigned to treatment. The entire sample of census GP in mandals that were used in randomization are included. In the figure above, these measures are $N_p^R = \frac{5}{11}$ and $\tilde{N}_p^R = \frac{1}{3}$.

Figure 4: Conceptual illustration of Total Treatment Effect (TTE) after adjusting for spillovers



This figure corresponds to Equations (5) and (6) in the text and illustrates how an outcome (say wages) is a function of both the treatment status of one's own village as well as the fraction of treated neighbors. β_T on the y-axis represents the effect of a village being treated when none of its neighbors are treated. Moving along the x-axis corresponds to increasing the fraction of treated neighbors, and the corresponding changes in wages are represented by light and dark blue solid lines (we allow the gradient to vary by a village's own treatment status as in Equation (5)). Vertical dotted lines represent mean exposure of treatment (dark blue) and control (light blue) groups. The bracket range on the x-axis, labelled x_{ITT} , represents the mean difference in the fraction of treated neighbors between treatment and control villages, which is positive but less than 1 (as seen in Panel (a) of Figure A.8). The gray bracket range, labelled y_{ITT} , represents the unadjusted treatment effect. The black bracket range, labelled y_{TTE} , represents the total treatment effect that a policy maker would care about and corresponds to the estimate in Equation (6).

For Online Publication: Appendix

This section provides further background on the NREGS program, including the status quo payments system, as well as the changes introduced by Smartcards and subsequent impacts on the payments process and leakage.

A Further details on NREGS

NREGS refers collectively to state-level employment schemes mandated by the National Rural Employment Guarantee Act (NREGA) of 2005. The Act guarantees one hundred days of paid employment to any rural household in India, with no eligibility requirement for obtaining work. After beneficiaries obtain a jobcard - a household level document that lists adult members, with pages assigned to record details of work done, payment owed, dates of employment, etc - they are meant to approach local level officials for employment, and work must be provided within two weeks and within a five kilometer radius of the beneficiary's residence. In practice, obtaining a jobcard is not a significant hurdle, and almost anyone who might conceivably work on the program has a jobcard (49.5% of rural households in Andhra Pradesh according to National Sample Survey data). The greater hurdle is obtaining employment, which is available when there is a project working in the village, with greatest availability during the slack labor seasons of April, May and June.

Given the seasonality, the 100 day limit is rarely a binding constraint, particularly since practical work-arounds (obtaining multiple jobcards per household) are possible. In 2009-10 the average number of days worked was 38 (mean is 30), according to Imbert and Papp (2015), with participants moving in and out of the program at high frequency. Altogether, this means that 32.1% of all households (and 64.8% of households with jobcards) in Andhra Pradesh worked on NREGS at some point during 2009. This work involves (for the most part) manual labor paid at minimum wages that are set at the state level. In Andhra Pradesh most wages are piece rates, set to allow workers to attain the daily minimum wage with roughly a day's worth of effort. Projects, chosen in advance via consultation with villagers at a village-wide meeting (the "Gram Sabha") and mandal and district officials, generally involve public infrastructure such as road construction, clearing fields for agricultural use, and irrigation earthworks.

Project management is delegated for the most part to local village officials, including elected village chiefs (Sarpanch) and a variety of appointed officials (Field Assistants, Technical Assistants, NREGS Village Workers, etc). These officials record attendance and output, creating paper "muster rolls" which are digitized at the sub-district level. These digitized records upon approval trigger the release of funds to pay workers.

A.1 Smartcard-introduced Changes in Payments

The Smartcards system was introduced in Andhra Pradesh in 2006, and while rollout in treatment areas in our study districts began in 2010. The payments system was based on electronic benefit transfers into special “no-frills” bank accounts tied to individual beneficiaries, and biometric authentication of beneficiaries before withdrawing these transfers. Figure A.11 shows the status quo payment system and the changes introduced by Smartcards.

In the status quo, money was transferred electronically from the State government to the district to the mandal, and from there cash moved on to the local post-office. Beneficiaries either traveled to the local post-office to get payments themselves, or, more commonly, simply handed over jobcards to local NREGS officials (Sarpanch, Field Assistant) and collected money from them in the village (since most post offices are far from local habitations). There was no formal authentication procedure required, which allowed the informal practice to continue.

In the Smartcards system, money was transferred electronically from the State government to private and public sector banks; banks worked with Technology Service Providers (TSPs) to manage last-mile delivery and authentication. Together, the bank and TSP received 2% of every transaction in villages in which they handled the payment system. Bank/TSP pairings competitively bid to manage transactions in every district. Last-mile delivery of cash was done by village level Customer Service Providers (CSPs), who were hired by TSPs as per the criteria laid down by the government. CSPs typically authenticated fingerprints and made payments locally at a central village location.

Payments were deposited into no-frills accounts for beneficiaries who had enrolled for Smartcards. These accounts were not maintained on the “core banking server”, but rather on small local Point-of-Service (PoS) devices managed by the CSPs. Since there was no real-time connectivity on these devices and no linkage with central bank servers, beneficiaries could only access their accounts through CSPs; they had no ability to go to a bank branch or an ATM to access this account. Beneficiaries therefore typically did not make deposits into accounts, and would not be able to even figure out whether there was a balance without contacting the CSP. Although in theory they could simply not claim payment if they wanted to leave a balance in the account, in practice only 0.3% of respondents claimed to leave money in the account; moreover, only 29% of beneficiaries who experienced the system said that they trusted the Smartcards system enough to deposit money into their Smartcard accounts if they could. In Nalgonda district, where the winning bid was actually from the post office, there were no bank accounts at all.

Compared to other documents that the household would have had (e.g. jobcard that was required in order to obtain Smartcard, voter ID card, etc), the Smartcard’s value as an identity document was limited. Unlike the national Unique ID (*Aadhaar*), Smartcards were not de-duplicated at the national level, and were therefore not legally admissible as

ID for purposes other than collecting NREGS/SSP payments.⁴¹ 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 PoS device (as opposed to on the card). Smartcards were also not portable; while Aadhaar cards are linked to a central server for authentication, Smartcards authentication was done offline. Thus while Aadhaar can be used across states and platforms (both public and private), Smartcards could only be used to make payments for NREGS and SSP beneficiaries within Andhra Pradesh.

A.2 Impacts of Smartcards on Payments Process and Leakage

Given changes in fund flow management as well as payments now being made by a CSP locally and visibly in the village, the Smartcards system significantly improved the payments process. Payment delays - the time between doing the actual work and getting paid - reduced significantly, by 10 days (29%). Since the CSP predictably delivered payments on set dates, the variability in payment date was also reduced (39%). Finally, the actual time taken to collect payment also went down, by 22 minutes (20%).

These improvements in the payment process were likely very important in making NREGS into a viable outside option; previous press reports had highlighted the suffering caused by delays and uncertainty in payments Pai (2013). Such payment process issues were mainly not relevant for SSP beneficiaries; the time to collect payments fell, but not significantly given that the control group time to collect was not as high as for NREGS beneficiaries. Meanwhile, we did not even collect data on SSP payment delays since such delays were not revealed to be an issue during our initial fieldwork, likely because of the fixed timing of payment collection at the beginning of the month.

In addition, the actual amount of payments received by households went up, while official disbursements remained the same, thus indicating a substantial reduction in leakage. Survey reports of payments received went up by Rs. 35, or 24% of control group mean, for NREGS beneficiaries. Other evidence reveals that the increases in earnings were reflected in actual increases in work done by beneficiaries; for example, our stealth audits of worksites reveals a commensurate increase in workers present at the worksite. The main margin of leakage reduction was thus via a reduction in “quasi-ghosts”: these are over-reports of payments to existing workers. Together, these results point to an increase in actual amount of work done under NREGS and hence an increase in assets created. Meanwhile, there were also increases in SSP payments (and reductions in SSP leakage); however, these are small in actual magnitude, with an extra Rs. 12/month received (5% of control mean, vs Rs. 35/week for NREGS).

⁴¹Meanwhile an Aadhaar card can be legally used to verify identity in airports, banks, etc.

Table A.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 (reproduced from Muralidharan et al. (2016)) 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. “ST” (“SC”) refers to Scheduled Tribes (Castes), 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 A.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 (reproduced from Muralidharan et al. (2016)) 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) is excluded since it is fully urban and 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 A.3: Baseline balance in administrative data

	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 agricultural laborers	.12	.12	-.0032	.52
% marginal agricultural 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 (reproduced from Muralidharan et al. (2016)) compares official data on baseline characteristics across treatment and control mandals. Column 3 reports the estimate for the treatment indicator from a simple regression of the outcome with district fixed effects as the only controls; column 4 reports the p-value for this estimate. A “jobcard” is a household level official enrollment document for the NREGS program. “SC” (“ST”) refers to Scheduled Castes (Tribes). “Old age”, “weaver”, “disabled” and “widow” are different eligibility groups within the SSP administration. “Working” is defined as the participation 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 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 (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 used 2001 census data.

Table A.4: Baseline balance in survey data

	Treatment	Control	Difference	p-value
	(1)	(2)	(3)	(4)
Household members	4.8	4.8	.022	.89
BPL	.98	.98	.0042	.73
Scheduled caste	.22	.25	-.027	.35
Scheduled tribe	.12	.11	.0071	.81
Literacy	.42	.42	.0015	.93
Annual income	41,482	42,791	-1,290	.52
Total annual expenditure	687,128	657,228	26,116	.37
Short-term Expenditure	52,946	51,086	1,574	.45
Longer-term Expenditure	51,947	44,390	7,162	.45
Pay to work/enroll	.011	.0095	.00099	.82
Pay to collect	.058	.036	.023	.13
Ghost household	.012	.0096	.0019	.75
Time to collect	156	169	-7.5	.62
Owens land	.65	.6	.058	.06
Total savings	5,863	5,620	3.7	1.00
Accessible (in 48h) savings	800	898	-105	.68
Total loans	62,065	57,878	5,176	.32
Owens business	.21	.16	.048	.02
Number of vehicles	.11	.12	-.014	.49
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
Household doing NREGS work	.43	.42	.0067	.85
NREGS days worked, June	8.3	8	.33	.65
Private sector days worked, June	4.8	5.3	-.49	.15
Days unpaid/idle, June	22	22	.29	.47
Average daily wage private sector, June	96	98	-3.7	.34
Daily reservation wage, June	70	76	-6.8	.03
NREGS hourly wage, June	13	14	-1.3	.13
NREGS overreporting	.15	.17	-.015	.55
Additional days household wanted NREGS work	15	16	-.8	.67

This table compares baseline characteristics across treatment and control mandals from our survey data. Column 3 reports the estimate for the treatment indicator from a simple regression of the outcome with district fixed effects as the only controls; column 4 reports the p-value for this estimate. “BPL” is an indicator for households below the poverty line. “Accessible (in 48h) savings” is the amount of savings a household could access within 48h. “NREGS availability” is an indicator for whether a household believes that anybody in the village could get work on NREGS when they want it. Standard errors are clustered at the Mandal level.

Table A.5: Household characteristics by NREGS jobcard ownership

	Households with jobcard (1)	Households without jobcard (2)	Difference (3)	p-value (4)
Household size	4.1	3.2	.88	.00
Scheduled Caste	.25	.18	.099	.06
Scheduled Tribe	.1	.062	.032	.29
Land owned in hectares	.7	.82	-.16	.18
Has post-office savings account	.91	.11	.79	.00
Self-employed in non-agriculture	.095	.23	-.14	.00
Self-employed in agriculture	.24	.26	-.05	.28
Agricultural labor	.54	.19	.38	.00
Other labor	.1	.13	-.014	.57

This table reports statistics for household characteristics by jobcard ownership for rural areas in our study districts estimated using NSS Round 66 data (collected in 2009-10, prior to the Smartcards intervention). Column 3 reports the estimate for an indicator of whether household owns a NREGS jobcard from a simple regression of the outcome with district fixed effects as the only controls. Column 4 reports the p-value for this estimate.

Table A.6: Heterogeneity in earnings gains by household characteristics

	Total income (Rs.)				Total labor income (Rs.)			
	(1) Hhd is ST or SC	(2) Any hhd member can read	(3) Hhd fraction eligible for SSP	(4) Head of hhd is widow	(5) Hhd is ST or SC	(6) Any hhd member can read	(7) Hhd fraction eligible for SSP	(8) Head of hhd is widow
Treatment	7854 (4528)	5994 (5642)	11511 (4708)	10008 (3897)	6634 (2094)	4484 (3479)	8511 (2213)	7950 (2067)
Treatment X Covariate	4821 (6650)	5247 (6202)	-10863 (8841)	-3917 (8063)	143 (3048)	2509 (3337)	-10606 (3397)	-10767 (4186)
Covariate	-11703 (5467)	30106 (4927)	-39118 (6707)	-20355 (6577)	6825 (2394)	3287 (2586)	-13060 (2711)	727 (3287)
Treatment + Treatment X Covariate	12676 (5460)	11241 (4120)	649 (6407)	6091 (8198)	6777 (3134)	6993 (2040)	-2095 (3132)	-2817 (4252)
Standard error	0.02	0.01	0.92	0.46	0.03	0.00	0.50	0.51
Adj. R-squared	0.04	0.09	0.09	0.06	0.07	0.06	0.10	0.06
Control Mean	69122	69122	69122	69122	24120	24120	24120	24120
N	4887	4868	4908	4847	4887	4868	4908	4847

In this table we analyze heterogeneity by household characteristics in earning gains using our survey data. Since we do not have panel data at the household level, we only test for heterogeneity on characteristics that are unlikely to have been affected by treatment (caste, education, and eligibility for pensions). “Total income (Rs.)” is total annualized HH income. “Total labor income (Rs.)” combines annualized income from agricultural work and physical labor for someone else. “Hhd is ST or SC” is an indicator for whether the household belongs to a Scheduled Tribe/Caste. “Any hhd member can read” is an indicator for whether any household member can read. “Hhd fraction eligible for SSP” is the fraction of household members who identify as eligible for SSP, though they may not actually receive pension. “Head of hhd is a widow” is an indicator for whether the head of household is a widow. For each covariate, we include the interaction terms between the respective variable and the binary treatment indicator. The table reports estimates for the sum of the estimates for treatment and the interaction between treatment and the respective binary covariate. For all outcomes, we censor observations that are in the top .5% percentile of treatment and control for “Total income”. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.7: Non-response and response composition rates by treatment status

(a) Full sample					
	Treatment	Control	Difference	p-value	N
	(1)	(2)	(3)	(4)	(5)
Wage realization (Rs.)	.013	.011	.0018	.59	7370
Reservation wage (Rs.)	.4	.39	.0073	.64	21437
Days worked private sector	.33	.3	.031	.037	21437
Days unpaid	.36	.34	.021	.11	21437
Days idle	.35	.33	.02	.12	21437
Days unpaid/idle	.34	.33	.019	.13	21437
Days worked on NREGS	.15	.13	.027	.02	21437
Days worked private sector > 0	.52	.49	.028	.2	14514
Wage realization \geq reservation wage	.98	.99	-.0029	.57	7287

(b) People of working age (18-65)					
	Treatment	Control	Difference	p-value	N
	(1)	(2)	(3)	(4)	(5)
Wage realization (Rs.)	.013	.012	.0014	.68	7055
Reservation wage (Rs.)	.15	.15	-.002	.92	14425
Days worked private sector	.085	.082	.0034	.63	14425
Days unpaid	.098	.097	.0016	.86	14425
Days idle	.088	.088	-.000085	.99	14425
Days unpaid/idle	.086	.087	-.00095	.89	14425
Days worked on NREGS	.019	.017	.002	.6	14425
Days worked private sector > 0	.54	.52	.016	.44	13210
Wage realization \geq reservation wage	.98	.99	-.0025	.62	6973

This table analyzes response rates to key questions regarding labor market outcomes. Columns 1-2 show the proportion of missing answers to the respective question in treatment and control. Column 3 reports the regression-adjusted treatment difference between treatment and control from a linear regression with the first principal component of a vector of mandal characteristics used to stratify randomization and district fixed effects as the only control variables. Column 4 reports the p-value of a two-sided t-test with the null-hypothesis being that the difference (Column 3) is equal to 0. Column 5 reports the number of individuals who should have answered the question. “Wage realization (Rs.)” is the average daily wage (Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage at which he or she would have been willing to work for someone else in June 2012. “Days worked private sector” is the number of days an individual worked for somebody else in June 2012. “Days idle” and “Days unpaid” is the number of days an individual stayed idle or did unpaid work in June 2012. “Days unpaid/idle” is the sum of the latter two variables. Note that the base group for “Wage realization (Rs.)” is the set of individuals who reported a strictly positive number of days worked for someone else. Similarly, the base group for “Days worked > 0” is the set of individuals that reported non-missing values for days worked for someone else. “Wage realization \geq Reservation wage” is the set of individuals that reported average daily wages higher than their Reservation wage. Panel b) restricts the sample to individuals of age between 18 and 65 years. Standard errors clustered at the mandal level are in parentheses.

Table A.8: Robustness checks for employment outcome impacts (June)

(a) Restricting to common sample

	Days unpaid/idle		Days worked on NREGS		Days worked private sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-1.3 (.59)	-1.2 (.59)	.69 (.49)	.61 (.47)	.3 (.57)	.38 (.55)
BL GP Mean		.16 (.052)		.22 (.052)		.24 (.07)
Adj. R-squared	0.06	0.06	0.06	0.07	0.02	0.02
Control Mean	17	17	4.7	4.7	8.2	8.2
N	13922	13837	13922	13837	13922	13837

(b) Restricting sample to age 18-65

	Days unpaid/idle		Days worked on NREGS		Days worked private sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-1.2 (.59)	-1.2 (.59)	.72 (.46)	.68 (.44)	.25 (.57)	.36 (.56)
BL GP Mean		.16 (.055)		.24 (.049)		.24 (.07)
Adj. R-squared	0.05	0.06	0.05	0.06	0.01	0.02
Control Mean	17	17	3.6	3.6	7.9	7.9
N	13182	13107	14159	14076	13210	13135

This table reports robustness checks for results reported in Table 3. In panel a, we restrict the estimation sample to respondents for whom we observe all three outcomes. In panel b) we restrict the estimation sample to adults aged 18-65. “Days unpaid/idle” in columns 1-2 is the sum of days an individual did unpaid work or stayed idle in June 2012. “Days worked on NREGS” in columns 3-4 is the number of days an individual worked on NREGS during June 2012. “Days worked private sector” in columns 5-6 is the number of days an individual worked for somebody else in June 2012. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses. Note that observation count varies between columns due to differences in non-response rates in their corresponding survey questions. A test of non-response rates by treatment status is shown in Table A.7.

Table A.9: Heterogeneity in wages outcomes by whether worked on NREGS in June

	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	8.3 (4.4)	11 (4.5)	8.3 (3.8)	8.9 (3.8)
Treatment X Did not work on NREGS in June	-1.4 (6.2)	-3.4 (6.4)	-4.9 (4)	-4.9 (4)
Did not work on NREGS in June	16 (5.6)	17 (5.7)	13 (3.6)	13 (3.6)
BL GP Mean		.15 (.05)		.088 (.032)
p-value of Treatment + Interaction	.16	.14	.26	.18
Adj. R-squared	.079	.085	.06	.063
Control Mean	131	131	99	99
N	7227	7014	12788	12674

This table shows heterogeneity in wage outcomes by whether an individual worked on NREGS in June using survey data. “Did not work on NREGS in June” is an indicator of whether an individual worked on NREGS in June. “Treatment X Did not work on NREGS in June” is an interaction term between “Treatment” and “Did not work on NREGS in June”. “Wage realization (Rs.)” in columns 1-2 is the average daily wage (Rs. = Rupees) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” in columns 3-4 is an individual’s reservation wage at which he or she would have been willing to work for someone else in June 2012. The outcome is elicited through a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. Observations in the top .5% percentile of the respective wage outcome in treatment and control are excluded from each regression. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level in parentheses.

Table A.10: Migration

	Did migrate?		Days migrated		Household size		Migration common in May?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	.024 (.017)	.022 (.018)	1.1 (4.9)	.75 (5.1)	.059 (.1)	.054 (.1)	.047 (.055)	.049 (.038)
BL GP Mean		.093 (.09)		.3 (.19)		.044 (.048)		
Migration previously common								.54 (.044)
Adj. R-squared	0.03	0.03	0.01	0.02	0.02	0.02	0.12	0.45
Control Mean	.075	.075	16	16	4.3	4.3	.21	.21
N	4907	4873	4943	4909	4943	4909	809	808
Level	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	GP	GP

This table illustrates treatment effects on various measures of migration using data from both our household survey and a separately conducted village survey. In columns 1 and 2, the outcome is an indicator for whether any household member stayed away from home for the purpose of work during the last year. In columns 3 and 4, the outcome is sum of all days any household member stayed away from home for work, while in columns 5 and 6 the number of household members is the dependent variable. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. In columns 7-8, the outcome is an indicator for whether it was common for workers to migrate out of the village in search of work during the month of May since the implementation of NREGS. “Migration previously common” is an indicator for whether the same type of migration during the same time was common prior to the start of NREGS. Note that “prior to NREGS” does not refer to the Smartcards intervention but rather to the rollout of the entire employment guarantee scheme. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.11: Heterogeneity in wage and labor market outcomes by gender

	Wage realization (Rs.)		Reservation wage (Rs.)		Days worked on NREGS		Days worked private sector		Days unpaid/idle	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	6.1 (5.2)	8.4 (5.2)	5.8 (4)	6.5 (4)	1.4 (.69)	1.3 (.66)	.92 (.65)	1 (.65)	-1.7 (.68)	-1.6 (.67)
Treatment X Female	-1 (5.3)	-3.1 (5.3)	-1.6 (3.4)	-1.9 (3.5)	-.81 (.42)	-.8 (.42)	-.96 (.56)	-.92 (.56)	.97 (.57)	.92 (.59)
Female	-60 (4.6)	-59 (4.6)	-37 (2.8)	-36 (2.9)	1.3 (.33)	1.3 (.34)	-1.6 (.49)	-1.7 (.49)	1.1 (.51)	1.1 (.52)
BL GP Mean		.15 (.048)		.11 (.043)		.14 (.043)		.22 (.067)		.16 (.052)
Adj. R-squared	.31	.31	.23	.24	.10	.10	.03	.03	.07	.07
Control Mean	128	128	97	97	8.2	8.2	7.9	7.9	17	17
N	7297	7083	12894	12780	10496	10423	14501	14416	14152	14067

In this table we analyze heterogeneity in June wage and labor market outcomes by gender using household survey data. “Female” is an indicator for whether the respondent is female and “Treatment X Female” is the interaction between “Treatment” and “Female”. “Wage realization (Rs.)” in columns 1-2 is the average daily wage (Rs. = Rupees) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” in columns 3-4 is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. The outcome is elicited through a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. Observations in the top .5% percentile of the respective wage outcome in treatment and control are excluded from all wage regressions. “Days worked on NREGS” in columns 5-6 is the number of days an individual worked on NREGS during June 2012. “Days worked private sector” in columns 7-8 is the number of days an individual worked for somebody else in June 2012. “Days unpaid/idle” in columns 9-10 is the sum of days an individual did unpaid work or stayed idle in June 2012. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.12: Loans by type of lender

	Total		Formal		Semi-Formal		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	11210 (4741)	11077 (4801)	2606 (2398)	2982 (2358)	-457 (1001)	-534 (1027)	8562 (4144)	7710 (4157)
BL GP Mean		.038 (.039)		.063 (.063)		.022 (.073)		.088 (.042)
Constant	49155 (7419)	47618 (7647)	5734 (2335)	5187 (2352)	3053 (1035)	3037 (1062)	39564 (6416)	37556 (6611)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.01	.01	.01	.02	.01	.01	.01	.01
Control Mean	68108	68108	15358	15358	4970	4970	46832	46832
N. of cases	4943	4909	4943	4909	4942	4908	4942	4908
Survey	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS

This table breaks down the estimates on total household loans reported in columns 3 and 4 of Table 5. “Formal” loans in columns 1 and 2 are defined as those loans granted by a commercial bank or a finance company, as mentioned in the household survey. In contrast, the loans labeled “Semi-Formal” were taken out from a micro-finance institution, a self-help group, a cooperative or from a Chit fund. A Chit fund is communal savings scheme regulated by Chit Fund Act from 1982 in which members make periodical contributions to be paid out to members at a specified point in time. Finally, “informal” loans are defined as those which households borrow money from money lenders, clients, shopkeepers, friends, neighbors or family members. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level in parentheses.

Table A.13: Expenditure

	Short-term Expenditure (Rs. per month)		Longer-term expenditure (Rs. per year)		Monthly Per Capita Expenditure
	(1)	(2)	(3)	(4)	(5)
Treatment	-108 (1029)	-428 (1033)	-24 (3239)	-646 (3227)	71 (122)
BL GP Mean		.051 (.02)		-.003 (.006)	
Adj. R-squared	0.01	0.02	0.01	0.01	0.03
Control Mean	18915	18915	38878	38878	1894
N	4943	4909	4943	4909	478
Recall period	1 month	1 month	1 year	1 year	1 month
Survey	NREGA	NREGA	NREGA	NREGA	NSS

This table analyzes different categories of household expenditure using survey and 68th Round NSS data. “Short-term expenditure” in columns 1- 2 (reference period 1 month) is the sum of spending on items such as on food items, fuel, entertainment, personal care items or rent measured from our survey data. “Longer-term expenditure” in columns 3-4 (reference period 1 year) comprises medical and social (e.g. weddings, funerals) expenses, tuition fees, and durable goods. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. “Monthly Per Capita Expenditure” (MPCE) in column 5 is measured at the household-level in the NSS data; the variable includes household expenditure as well as the imputed value of household production. Note that the households from the NSS data are not the same as our sample households. All regressions include district fixed effects and those from our survey data include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.14: Robustness of income impacts (SECC)
(a) Probit and ordered probit

	Lowest bracket		Middle bracket		Highest bracket		Income bracket 3 levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.04 (.014)	-.039 (.014)	.025 (.011)	.024 (.011)	.013 (.0065)	.012 (.0061)	-.04 (.014)	-.039 (.014)
Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.01	0.03	0.01	0.02	0.02	0.04	0.01	0.02
Control Mean	.83	.83	.13	.13	.038	.038		
N	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M

(b) Linear probability model

	Lowest bracket		Middle bracket		Highest bracket		Income bracket 3 levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.041 (.015)	-.04 (.015)	.027 (.012)	.026 (.012)	.015 (.0072)	.014 (.0071)	.056 (.02)	.054 (.02)
Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared								
Control Mean	.83	.83	.13	.13	.038	.038	1.2	1.2
N	1.8M	1.8 M	1.8M	1.8 M	1.8M	1.8 M	1.8M	1.8 M

This table shows robustness of the treatment effects on SECC income category in Table 1a using probit and linear probability models. Both panels use data from the Socioeconomic and Caste Census (SECC), which reports income categories of the highest earner in the household (HH): the “Lowest bracket” corresponds to earning < Rs. 5000/month, “Middle bracket” to earning between Rs. 5000 & 10000/month, and “Highest bracket” to earning > Rs. 10000/month. The tables report marginal effects, or changes in the predicted probability of being in the respective income bracket (columns 1-6) resulting from a change in a binary treatment indicator from 0 to 1. In columns 7-8, we show the marginal effects on the predicted probability of being in the lowest income category. Control variables, when included, are: the age of the head of HH, an indicator for whether the head of HH is illiterate, indicator for whether a HH belongs to Scheduled Castes/Tribes. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.15: Income (Survey Data), no censoring

	Total income		NREGA	Agricultural labor	Other labor	Farm	Business	Miscellaneous
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	10308 (4638)	9580 (4628)	905 (589)	3675 (1485)	4471 (1585)	1738 (2704)	-773 (1359)	293 (2437)
BL GP Mean		.055 (.05)						
Adj. R-squared	0.03	0.03	0.02	0.04	0.03	0.01	0.01	0.01
Control Mean	71935	71935	4743	14784	9315	21708	6620	14765
N	4932	4898	4931	4932	4932	4932	4932	4932

This table reports robustness check of earnings gains in Table 1b, which shows treatment effects on various types of income using annualized data from our survey, to including all observations (instead of censoring the top 0.5%). “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. “NREGS” is the earnings from NREGS. “Agricultural labor” captures income from agricultural work for someone else, while “Other labor” is income from physical labor for someone else. “Farm” combines income from a households’ own land and animal husbandry, while “Business” captures income from self-employment or through a household’s own business. “Other” is the sum of household income not captured by any of the other categories. Note that the income categories were not as precisely measured at baseline which is why we cannot include the respective lag of the dependent variable “BL GP Mean”. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.16: Robustness checks for private sector wage outcomes

(a) No censoring				
	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	5.6	6.8	5	5.6
	(4.1)	(4.1)	(3.3)	(3.2)
BL GP Mean		.15		.091
		(.054)		(.039)
Adj R-squared	0.05	0.05	0.03	0.03
Control Mean	131	131	99	99
N	7326	7112	12955	12841
(b) Restricting sample to age 18-65				
	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.6	7.9	5	5.7
	(3.7)	(3.8)	(3)	(2.9)
BL GP Mean		.16		.1
		(.049)		(.033)
Adjusted R-squared	0.07	0.07	0.05	0.06
Control Mean	129	129	98	98
N	6989	6782	12227	12124
(c) Excluding respondents who did not work in June				
	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.4	7.6	6.8	7.3
	(3.6)	(3.6)	(3.2)	(3.1)
BL GP Mean		.16		.13
		(.048)		(.05)
Adjusted R-squared	0.07	0.07	0.06	0.06
Control Mean	128	128	98	98
N	7256	7043	7282	7212

This table reports several robustness checks for results reported in Table 2. In panel a), the analysis sample includes all observations, in particular the top .5% percentile of the respective wage outcome in treatment and control, of the respective wage outcome. In panel b), the sample is restricted to respondents in ages 18 to 65 and excludes observations in the top .5% percentile of the respective wage outcome in treatment and control. In panel c), we drop observations for respondents who have did not work in June and excludes observations in the top .5% percentile of the respective wage outcome in treatment and control. “Wage realization (Rs.)” in columns 1-4 is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” in columns 5-8 is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. The outcome is based on an a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. “BL GP Mean” is the Gram Panchayat mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.17: Predictors of differential non-response and response composition

	Missing response to				Days worked > 0	Average wage > Reservation wage
	(1) Wage realization (Rs.)	(2) Reservation wage (Rs.)	(3) Days worked private sector	(4) Days idle/unpaid	(5)	(6)
Member is female	-.0051 (.0047)	-.0032 (.017)	-.0016 (.015)	.0069 (.015)	-.022 (.021)	.0069 (.0063)
Above median hhd income	-.0047 (.0055)	.018 (.017)	.033 (.019)	.011 (.016)	.05 (.033)	-.0045 (.0094)
Hhd is ST, SC or OBC	.023 (.016)	.022 (.03)	.031 (.025)	.012 (.025)	-.0042 (.045)	-.011 (.012)
Hhd below BPL	-.012 (.012)	.024 (.033)	.045 (.031)	.022 (.029)	.091 (.043)	-.0029 (.0084)
Any hhd member can read	.024 (.011)	-.012 (.023)	.018 (.021)	-.0056 (.019)	.013 (.04)	.0069 (.017)
Head of hhd is widow	-.0017 (.0069)	.013 (.028)	.012 (.024)	.011 (.021)	-.022 (.035)	-.0071 (.014)
Carded GP	.0031 (.0036)	.0054 (.013)	.019 (.014)	.0062 (.011)	.034 (.019)	-.0038 (.0056)
Control Mean	.011	.39	.3	.33	.49	.99
Average N	7385	21349	21349	21349	14456	7255

This table reports interaction effects between household or GP characteristics and treatment status for individual non-response and strictly-positive response rates in private labor market outcomes. In columns 1-4, the outcome is a binary indicator for whether an a survey response is missing when it should not. Every cell in the regression table reports the coefficient of an interaction term (except “Carded GP”, see below) of the reported variable with the treatment indicator from a separate regression that includes the raw respective variable, the treatment indicator as well as a vector of mandal characteristics used to stratify randomization and district fixed effects as covariates. “Wage realization (Rs.)” is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. The outcome is based on an a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. “Days worked private sector” is the number of days an individual worked for somebody else in June 2012. “Days unpaid/idle” is the number of days an individual stayed idle or did unpaid work in June 2012. In columns 5-6, we look examine two types of response patterns. “Days worked private sector > 0” is an indicator for whether an individual worked in the private sector in June 2012. “Wage realization > Reservation wage” is an indicator for whether an individual’s reported average daily wage was greater than his/her Reservation wage. “Above median hhd income” is an indicator for whether an individual belongs to an household with total annualized income above the sample median. “Hhd is ST, SC or OBC” indicates household members belonging to Scheduled Castes/Tribes or Other Backward Castes - historically discriminated against section of the population - while “Hhd below BPL” indicates individuals from household living below the poverty line. “Carded GP” is a simple indicator variable (no interaction effect) for whether a household lives in a GP that has moved to Smartcard-based payment, which usually happens once 40% of beneficiaries have been issued a card. No interaction effect is included because all carded GPs are in treatment mandals (by experimental design). Finally, note that each column reports results from 7 different regressions and there is no single number of observations. This table reports the average number of observations across all regressions in a column. Standard errors clustered at the mandal level are in parentheses.

Table A.18: Baseline balance with respect to exogenous measure of neighbor's treatment

	% GPs treated within R km				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Wage realization (Rs.)	3.3 (4.1)	4.2 (5.5)	2 (6.6)	.21 (7.8)	1.3 (9.6)
Reservation wage (Rs.)	1.4 (3.3)	3.1 (4.1)	2.4 (4.8)	2.7 (5.1)	5.6 (6.1)
Days worked private sector	-.27 (.42)	-.46 (.55)	-.42 (.64)	-.21 (.76)	.026 (.89)
Days unpaid/idle	.34 (.53)	.44 (.68)	.19 (.95)	-.3 (1.5)	-.54 (1.7)
Total income (Rs.)	-377 (2872)	2653 (4168)	2177 (4826)	-2032 (5421)	-3232 (6738)

This table reports baseline balance of key outcomes with respect to \tilde{N}_p^R for GPs in treatment mandals. Each cell shows the respective coefficient from a separate regression where the outcome is given by the row header. “Wage realization (Rs.)” the average daily wage (in Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. The outcome is based on an a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. “Days worked private sector” is the number of days an individual worked for somebody else in June 2012. “Total income” is total annualized household income, where the top .5% of observations are separately trimmed in treatment and control. The “% GPs treated within R” is \tilde{N}_p^R , or the ratio of the number of GPs in treatment mandals within radius R km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 GPs are included in the denominator, and that same-mandal GPs are excluded from both the denominator and numerator. Note that each cell shows a separate regression of the outcome with \tilde{N}_p^R and district fixed effects as the only covariates. The entire GP sample used in randomization is included. Each column reports results from 5 different regressions with distinct observation counts, which are not reported. Standard errors clustered at the mandal level are in parentheses.

Table A.19: Baseline balance with respect to comprehensive measure of neighbor's treatment

	% GPs treated within R km				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Wage realization (Rs.)	-.48 (6.8)	4.2 (8.3)	1.1 (9.3)	-1.3 (10)	-1.2 (12)
Reservation wage (Rs.)	-3.2 (5.1)	-.47 (6.4)	-1.8 (6.7)	-1.2 (7)	1 (7.4)
Days worked private sector	-.68 (.64)	-.66 (.7)	-.69 (.73)	-.42 (.87)	-.32 (.98)
Days unpaid/idle	.14 (1)	.31 (1.3)	.0017 (1.6)	-.45 (2)	-.86 (2.3)
Total income (Rs.)	5133 (4572)	4115 (5022)	3683 (5992)	-917 (6638)	-3137 (7699)

This table reports baseline balance of key outcomes with respect to N_p^R for GPs in treatment mandals. Each cell shows the respective coefficient from a separate regression where the outcome is given by the row header. “Wage realization (Rs.)” the average daily wage (in Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. The outcome is based on an a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. “Days worked private sector” is the number of days an individual worked for somebody else in June 2012. “Total income” is total annualized household income. The “% GPs treated within R” is N_p^R , or the ratio of the number of GPs in treatment mandals within radius R km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 GPs are included in the denominator, and that same-mandal GPs are included in both the denominator and numerator. The entire GP sample used in randomization is included. Each cell shows a separate regression of the outcome with the “% GPs treated within R” and district fixed effects as the only covariates. Each column reports results from 5 different regressions with distinct observation counts, which are not reported. Standard errors clustered at the mandal level are in parentheses.

Table A.20: Estimating total treatment effects including spillovers (OLS)

(a) Wage										
	Wage realization (Rs.)					Reservation wage (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Total treatment effect	11 (5.6)	14 (6.9)	15 (7.5)	15 (8.5)	17 (9.8)	9.5 (3.1)	11 (4.3)	11 (5.6)	8.4 (6.2)	5.9 (6.8)
Adjusted R-squared	0.07	0.07	0.07	0.07	0.07	0.05	0.05	0.05	0.05	0.05
Control Mean	128	128	128	128	128	97	97	97	97	97
N	7269	7269	7269	7269	7269	12852	12852	12852	12852	12852

(b) Labor										
	Days worked private sector					Days idle/unpaid				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Total treatment effect	.76 (.8)	1.1 (1.1)	1.7 (1.4)	2.5 (1.6)	3.5 (1.8)	-1.5 (.78)	-2.1 (1.1)	-3 (1.4)	-4 (1.6)	-5.2 (1.8)
Adjusted R-squared	0.01	0.01	0.01	0.01	0.01	0.07	0.07	0.07	0.07	0.07
Control Mean	7.9	7.9	7.9	7.9	7.9	17	17	17	17	17
N	14441	14441	14441	14441	14441	14095	14095	14095	14095	14095

This table provides estimates for total treatment effects using the comprehensive spatial exposure measure N_p^R for (a) wage outcomes and (b) labor outcomes. Each specification contains a treatment indicator, N_p^R , and an interaction between N_p^R and treatment. Recall N_p^R is the ratio of the number of GPs in treatment mandals over the total GPs within a given radius R km. The “Total treatment effect” estimate reported in the second section of the table is the sum of the coefficients for treatment, N_p^R , and their interaction. “Wage realization (Rs.)” is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. “Days worked private sector” is the number of days an individual worked for somebody else in June 2012. “Days unpaid/idle” is the sum of days an individual did unpaid work or stayed idle in June 2012. Standard errors clustered at the mandal level are in parentheses. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization.

Table A.21: Estimating total treatment effects including spillovers (IV)

(a) Wage

	Wage realization (Rs.)					Reservation wage (Rs.)				
	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30
Treatment	-10 (9.3)	4.2 (9)	4.9 (9.2)	5.6 (9.4)	7.6 (10)	1.3 (6.7)	6.1 (6.2)	5.4 (6.3)	4.8 (6.9)	2 (7.2)
% GPs treated within R km	13 (16)	31 (17)	26 (18)	21 (15)	27 (17)	9.6 (14)	12 (12)	8.4 (13)	5 (12)	-.39 (11)
% GPs treated within R km X Treatment	16 (20)	-13 (21)	-7.2 (22)	-4.6 (21)	-9 (23)	-2.5 (16)	-8.2 (15)	-3.9 (16)	-1.1 (16)	6.2 (16)
Total treatment effect	18	23	24	22	26	8.5	9.8	9.9	8.7	7.8
SE	(5.6)	(6.6)	(7.8)	(9.1)	(11)	(4)	(4.6)	(5.4)	(6.5)	(7.4)
F-stat for % GPs treated within R km	355	205	259	340	311	344	205	267	367	321
F-stat for % GPs treated within R km X Treatment	224	156	165	171	115	226	165	186	187	118
Adj R-squared	.07	.07	.07	.07	.07	.05	.05	.05	.05	.05
Control Mean	127	128	128	128	128	97	97	97	97	97
N. of cases	6560	7049	7192	7245	7269	11614	12498	12732	12818	12852

(b) Labor

	Days worked private sector					Days idle/unpaid				
	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30
Treatment	.31 (1.6)	-.27 (1.6)	.037 (1.7)	.27 (1.7)	.48 (1.8)	.33 (1.6)	.79 (1.5)	.68 (1.5)	-.25 (1.4)	-.86 (1.5)
% GPs treated within R km	3.5 (3.1)	3.1 (2.9)	3.4 (3)	4.1 (3.1)	5.1 (3.4)	-2 (3.3)	-2.7 (3)	-2.5 (3)	-4 (2.8)	-5.8 (2.8)
% GPs treated within R km X Treatment	-2.2 (3.6)	-.53 (3.6)	-.64 (3.8)	-.82 (4)	-1.2 (4.3)	-.98 (3.7)	-1.8 (3.6)	-2.5 (3.5)	-.86 (3.4)	.48 (3.4)
Total treatment effect	1.6	2.3	2.8	3.5	4.4	-2.7	-3.7	-4.3	-5.1	-6.2
SE	(1.1)	(1.3)	(1.5)	(1.7)	(1.9)	(1)	(1.3)	(1.5)	(1.6)	(1.8)
F-stat for % GPs treated within R km	362	206	242	362	360	365	202	236	360	367
F-stat for % GPs treated within R km X Treatment	247	158	146	166	136	246	156	143	168	144
Adj R-squared	.01	.01	.01	.01	.01	.07	.06	.07	.07	.07
Control Mean	7.8	7.9	7.9	7.9	7.9	17	17	17	17	17
N	13008	13995	14300	14397	14441	12722	13689	13977	14064	14095

This table provides estimates from the total treatment effect specification (equation 5). Each structural equation contains a treatment indicator, N_p^R , and an interaction between N_p^R and treatment. The N_p^R is the ratio of the number of GPs in treatment mandals over the total GPs within a given radius R km. The instruments used in the first stage are \tilde{N}_p^R and its interaction with treatment. “% GPs treated within R km” is \tilde{N}_p^R , or the ratio of the number of GPs in treatment mandals over the total GPs within a given radius of R km. Note that wave 2 GPs are included in the denominator, and that same-mandal GPs are *excluded* in both the denominator and numerator. The “Total treatment effect” estimate reported is the sum of the coefficients for treatment, N_p^R , and their interaction. For wage and income outcomes, we censor observations in the top .5% percentile of treatment and control observations. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization.

Table A.22: Spillover effects on income at different censoring thresholds

(a) Existence of spillovers

	No censoring					Censoring top 0.5%					Censoring top 1%				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30
Control	15163 (11225)	18050 (10986)	26951 (15365)	19652 (15030)	12961 (14919)	6235 (8539)	9515 (7942)	14900 (10344)	9562 (11020)	6442 (11371)	3648 (8191)	6649 (7462)	12871 (9967)	8643 (10613)	5494 (10786)
Treatment	5762 (5373)	-506 (7688)	-384 (9790)	3565 (11596)	3492 (13988)	2179 (4710)	-2087 (6830)	-2676 (8332)	1428 (10583)	3549 (13478)	6261 (3956)	7677 (5038)	7191 (6091)	12810 (7972)	19022 (9800)
Pooled	7156 (5345)	6351 (7108)	8657 (9472)	8669 (9991)	7138 (10567)	2330 (4185)	2164 (5536)	2940 (7008)	3580 (8088)	4167 (9496)	4290 (3845)	7465 (4616)	8283 (5887)	10045 (6812)	12751 (7597)
F-test for equality	.88	2.7	3.2	.99	.29	.25	1.5	2	.33	.031	.12	.017	.28	.12	1
p-value	.35	.10	.07	.32	.59	.62	.22	.16	.57	.86	.73	.90	.60	.73	.31
N	4420	4767	4864	4892	4903	4401	4745	4840	4868	4879	4380	4722	4816	4844	4855
% of pooled sample	90	97	99	100	100	90	97	99	100	100	90	97	99	100	100

(b) Total treatment effects

	No censoring					Censoring top 0.5%					Censoring top 1%				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30
Total treatment effect	18757 (7140)	19872 (8944)	22903 (11560)	21089 (11973)	18164 (12448)	11947 (6158)	13338 (7310)	14362 (8925)	13931 (10033)	13921 (11272)	12642 (6072)	17291 (6879)	18443 (8095)	19237 (8774)	21442 (9328)
Unadjusted treatment effect	9307 (5078)	10034 (4692)	10412 (4643)	10442 (4633)	10434 (4627)	8896 (4084)	9456 (3763)	9581 (3726)	9621 (3717)	9618 (3712)	8157 (3641)	8002 (3324)	8107 (3307)	8164 (3303)	8171 (3301)
Difference	9450 (8761)	9838 (10100)	12490 (12457)	10647 (12838)	7730 (13281)	3051 (7389)	3882 (8222)	4781 (9671)	4310 (10700)	4304 (11867)	4485 (7080)	9290 (7640)	10337 (8744)	11073 (9375)	13271 (9895)
Chi-square statistic	1.2	.95	1	.69	.34	.17	.22	.24	.16	.13	.4	1.5	1.4	1.4	1.8
Control Mean	71756	72080	71935	71935	71935	68943	69255	69122	69122	69122	67133	67614	67488	67488	67488
N	4420	4767	4864	4892	4903	4401	4745	4840	4868	4879	4380	4722	4816	4844	4855

This table shows tests for the existence of spillovers and estimates for total treatment effects on total annualized household income using survey data (at different censoring thresholds of the income). For each panel, columns 1-5 show results with no censoring, columns 6-10 show results censoring top 0.5% total annual income observations by treatment and control, and columns 11-15 show results censoring top 1% total annual income observations by treatment and control Panel a) reports results using same specifications as Table 7. “% of pooled sample” refers to the % of total observations for an outcome that are used in estimation of pooled specification. Panel b) reports results using same specifications as Table 8. Note that the estimates for the unadjusted treatment effect differ slightly from those from Table 1b as the analysis sample only includes observations where the spatial exposure measures are defined.

Table A.23: Impacts on NREGS project counts & types

	Number of distinct projects				Number days spent working on					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total	Construction	Irrigation	Land development	Roads	Total	Construction	Irrigation	Land development	Roads
Treatment	-1.2 (2.9)	.11 (.44)	.099 (.31)	-1 (2.7)	.2 (.12)	61 (441)	-10 (103)	25 (247)	-119 (435)	161 (112)
BL GP Mean	.61 (.075)	.23 (.088)	.067 (.021)	1.3 (.23)	.099 (.026)	.4 (.039)	.068 (.039)	.23 (.055)	.36 (.063)	.11 (.07)
Adj. R-squared	0.24	0.17	0.11	0.20	0.13	0.35	0.30	0.47	0.24	0.11
Control Mean	32	2.8	1.8	16	.51	6539	492	1770	2606	329
N	2837	2837	2837	2837	2837	2899	2837	2837	2837	2837

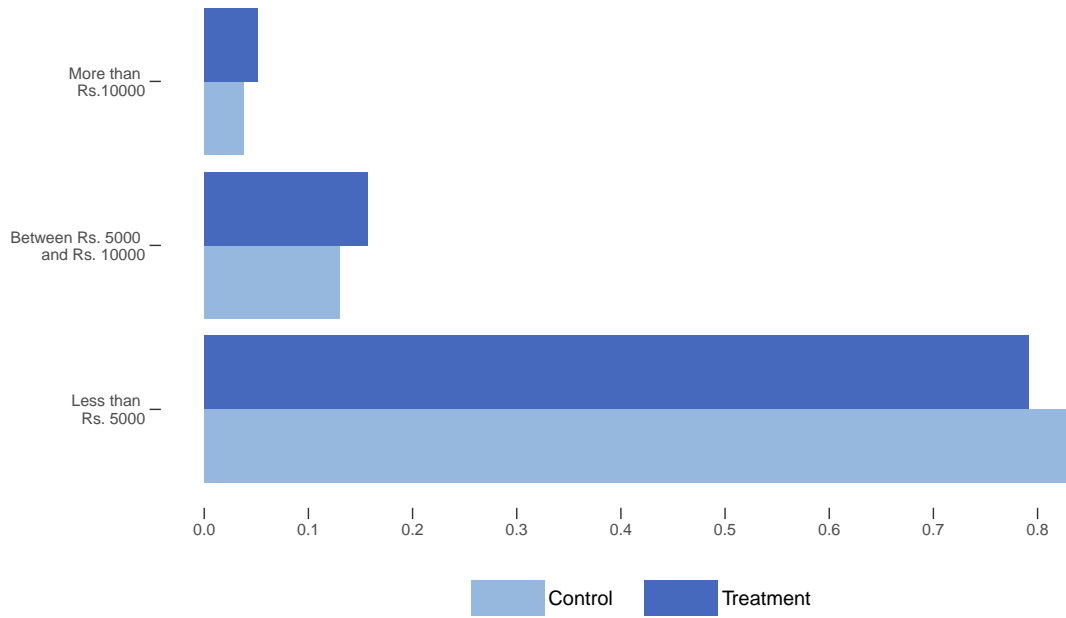
This table analyzes whether treatment impacted the creation of productivity-enhancing assets through the type of NREGS projects implemented at the GP-level using NREGS muster roll data. The outcomes in columns 1-5 are counts of unique projects in GPs as identified by their project identification numbers in the NREGS muster roll data. The relevant period is the endline study period (May 28 to July 15, 2012). The categories in columns 2-5 (and also in 6-10) are based on manual matching of project titles to any of the following categories: construction, irrigation, land development, roads, plantation work, desilting and other projects (with the latter three omitted from the table). In columns 6-10, the outcome variable is the sum of days worked within a GP in the respective category. The “BL GP Mean” is constructed in the same way with the reference being the baseline study period (May 31 to July 4, 2010). All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses.

Table A.24: Land utilization and irrigation

	Irrigated land	Total land	Total fallows	Non-agricultural use	Net area sown	Net area irrigated
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-5.6 (5.4)	-6.8 (6.1)	-.74 (1.2)	-.83 (1.3)	1.1 (1.6)	.0018 (.01)
BL GP Mean			.0074 (.0092)	.48 (.075)	.49 (.046)	.91 (.04)
Adj. R-squared	0.00	0.00	0.62	0.62	0.88	0.83
Control Mean	7.2	11	11	9.1	28	.18
N	1,828,709	1,828,708	154	154	154	154
Level	Household	Household	Mandal	Mandal	Mandal	Mandal
Data source	SECC	SECC	DSH	DSH	DSH	DSH

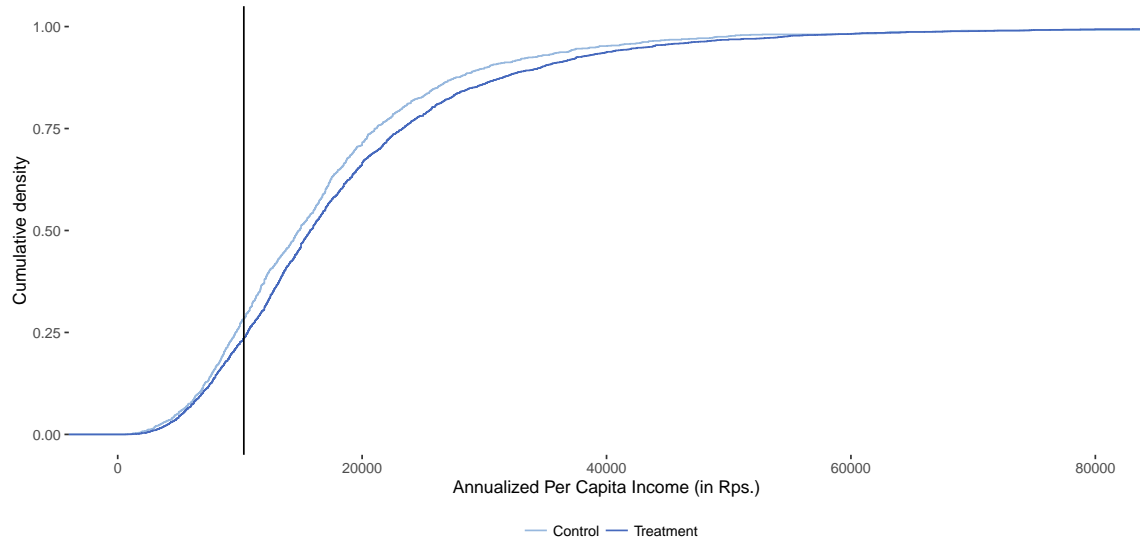
This table analyzes land ownership, land utilization and irrigation using data from the Socioeconomic and Caste Census (SECC) and the annual District Statistical Handbooks (DSH) 2012-2013 (2009-2010 for the lagged dependent variable “BL GP Mean”) for the eight study districts. “Irrigated land (ac.)” is the amount of land in acres owned with assured irrigation for two crops. “Total land (ac.)” is the total amount of land owned, including both irrigated and unirrigated land. “Total fallows” is the total area which at one point was taken up or could be taken up for cultivation but is currently left fallow. This is the sum of “current fallows” (cropped area which is kept fallow in the current year), “other fallows” (land which is has been left fallow for more than 1 year but less than 5 years) and “culturable waste” (land available which has been left fallow for the more than 5 years but would be available for cultivation). “Non-agricultural use” is the area occupied by buildings, roads, railways or under water. “Net area sown” is total area sown with crops and orchards where area that is sown more than once is counted only once. “Net area irrigated” is the total area irrigated through any source. The quantities in columns 3-6 are in percentage of total mandal area. Note that the number of observation is 154 (not 157 - the number of study mandals) due to incomplete data published in the DSHs of three mandals. In the column 1-2 regressions that use SECC data, the following control variables are included: the age of the head of HH, an indicator for whether the head of HH is illiterate, indicator for whether a HH belongs to Scheduled Castes/Tribes. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Robust standard errors are in parentheses, and are clustered at the mandal level for regressions run at the household level.

Figure A.1: Effects on income/month: SECC



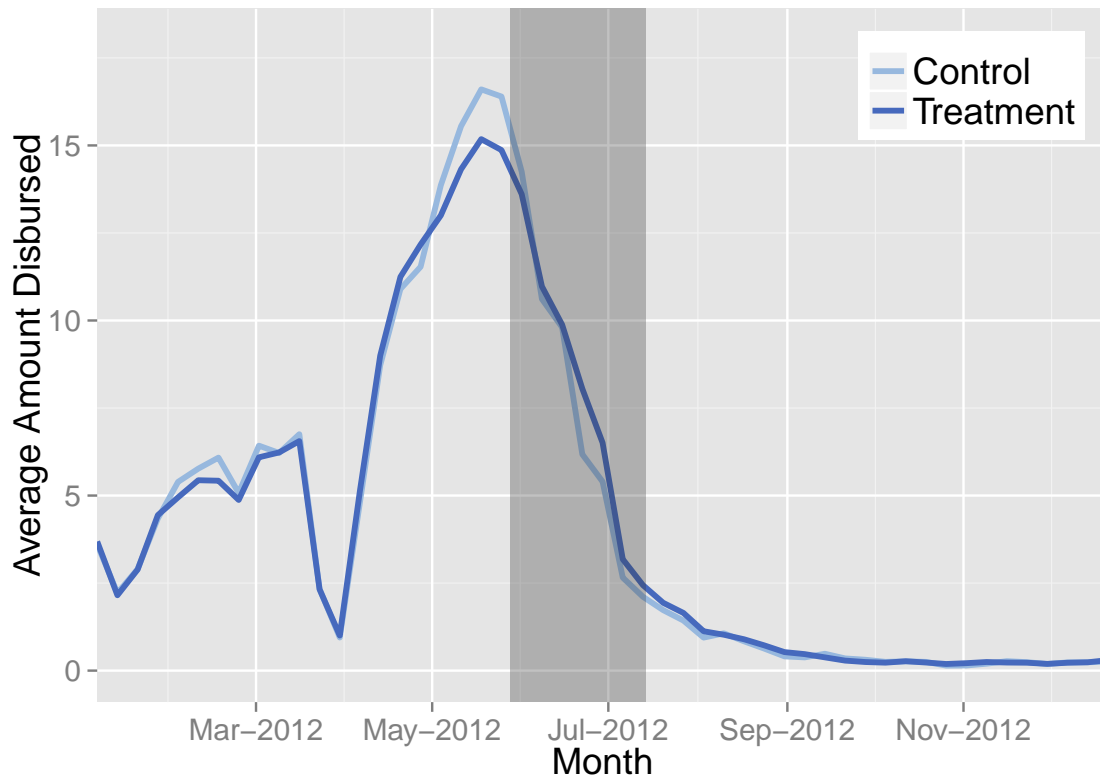
The figure shows the proportions of households in each of the three brackets of monthly income in the Socioeconomic and Caste Census (SECC) 2011 (enumeration started in late June 2011) by treatment and control households. The standard error (not included) for every category is < 0.001 .

Figure A.2: Annualized per capita income



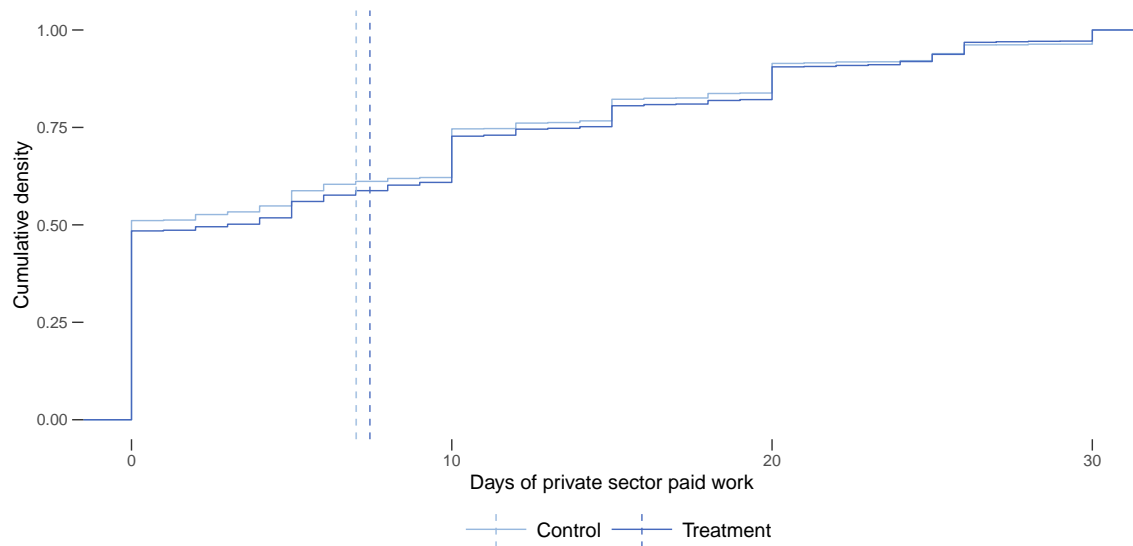
This figure shows an empirical cdf of total annualized per capita income by household for treatment and control groups using data from the endline household survey. Annualized per capita income was calculated by dividing the total annual household income by number of household members. The vertical line indicates the annualized official per capita poverty line (860 Rs. per month or 10,320 Rs. per year).

Figure A.3: Official disbursement trends in NREGS



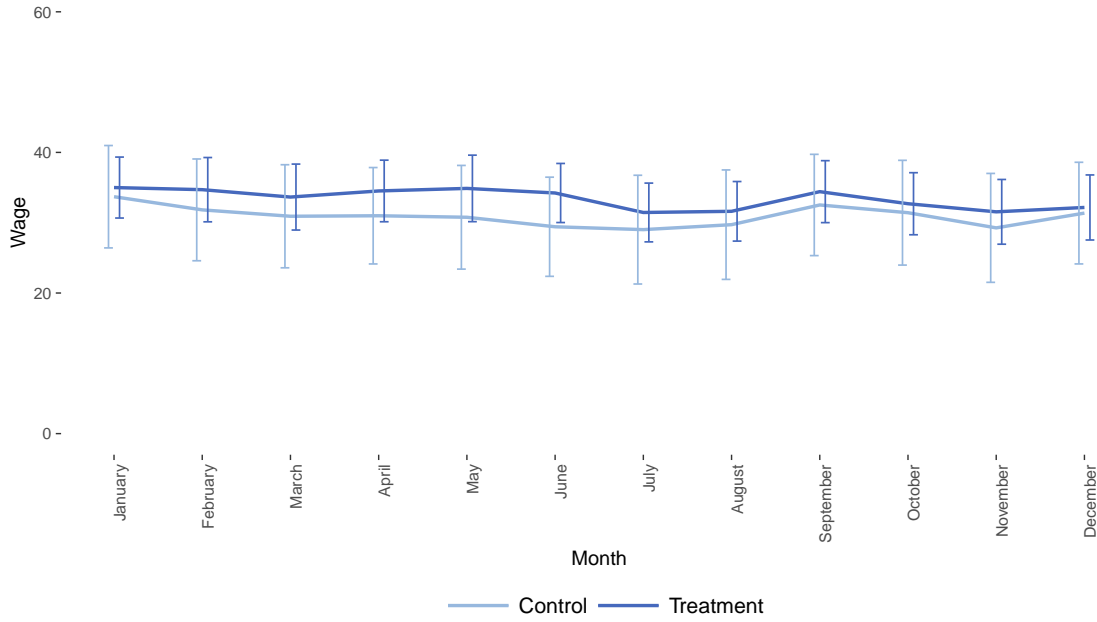
This figure (reproduced from Muralidharan et al. (2016)) shows official NREGS payments for all workers averaged at the GP-week level for treatment and control areas. The grey shaded bands denote the endline study periods on which our survey questions focus (May 28 to July 15, 2012).

Figure A.4: Private sector work in June



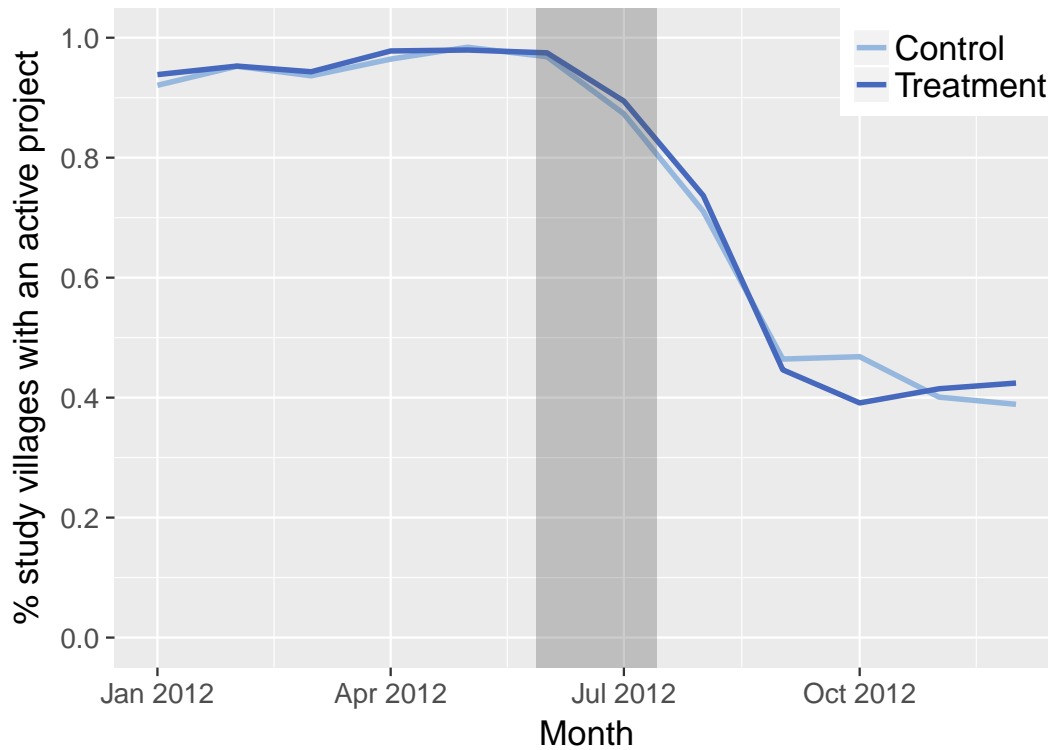
This figure shows an empirical cdf of the number of days an individual worked for someone else during June 2012, using data from the endline household survey. The dashed lines indicate in-sample means (not weighted by sampling probabilities) in treatment and control, respectively.

Figure A.5: Changes in wages by month and treatment status



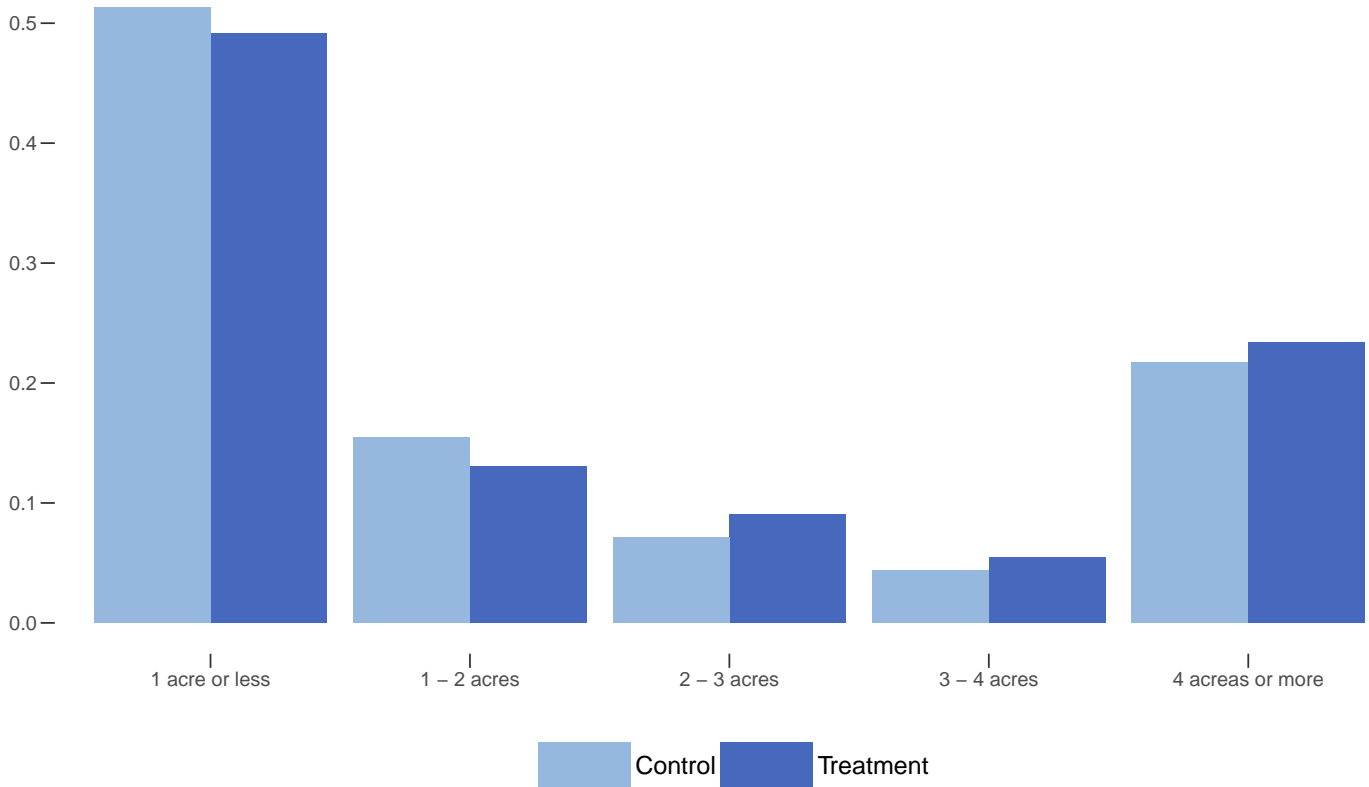
The figure shows mean changes in agricultural wages between baseline and endline, by month and treatment status, weighted by (inverse) GP sampling probability. The data, which is at the village-level, comes from surveys administered to prominent figures in each village. Standard errors are clustered at the mandal level.

Figure A.6: Availability of NREGS work in study villages by month and treatment status



The figure shows the proportion of study GPs with an active NREGS project, by month and treatment status, during 2012. The data source is official NREGS payments for all workers for treatment and control areas during 2012. A village is considering to have an active project for a given month if there is at least one official NREGS payment made to a worker during that month.

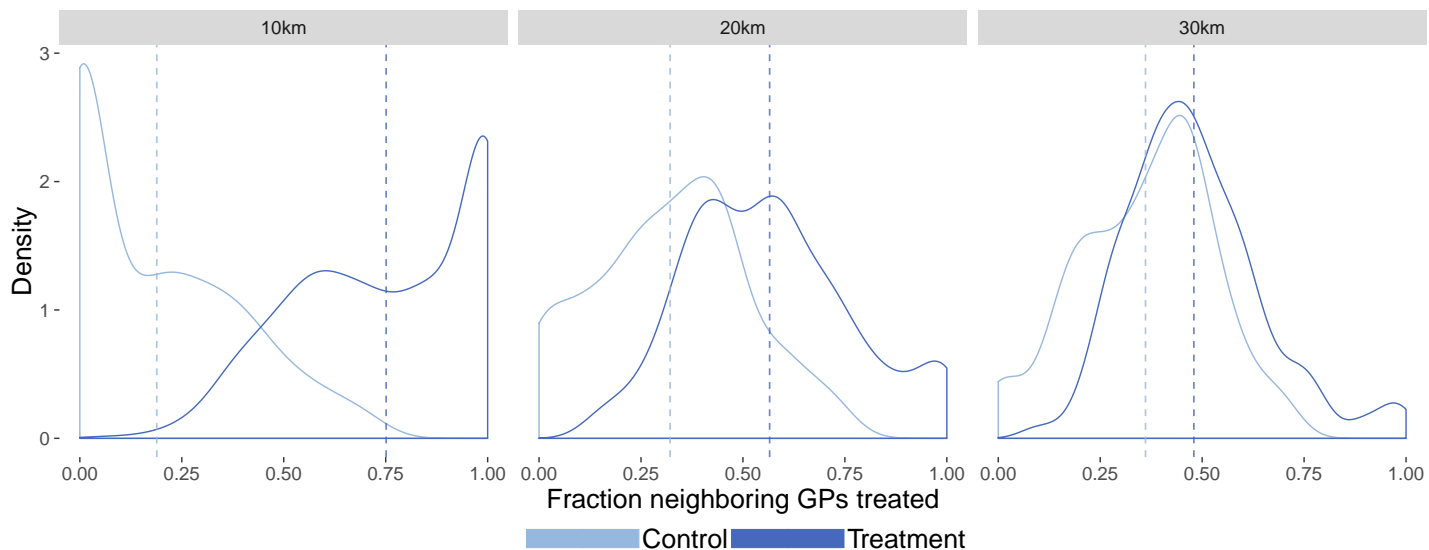
Figure A.7: Household landholdings (in acres)



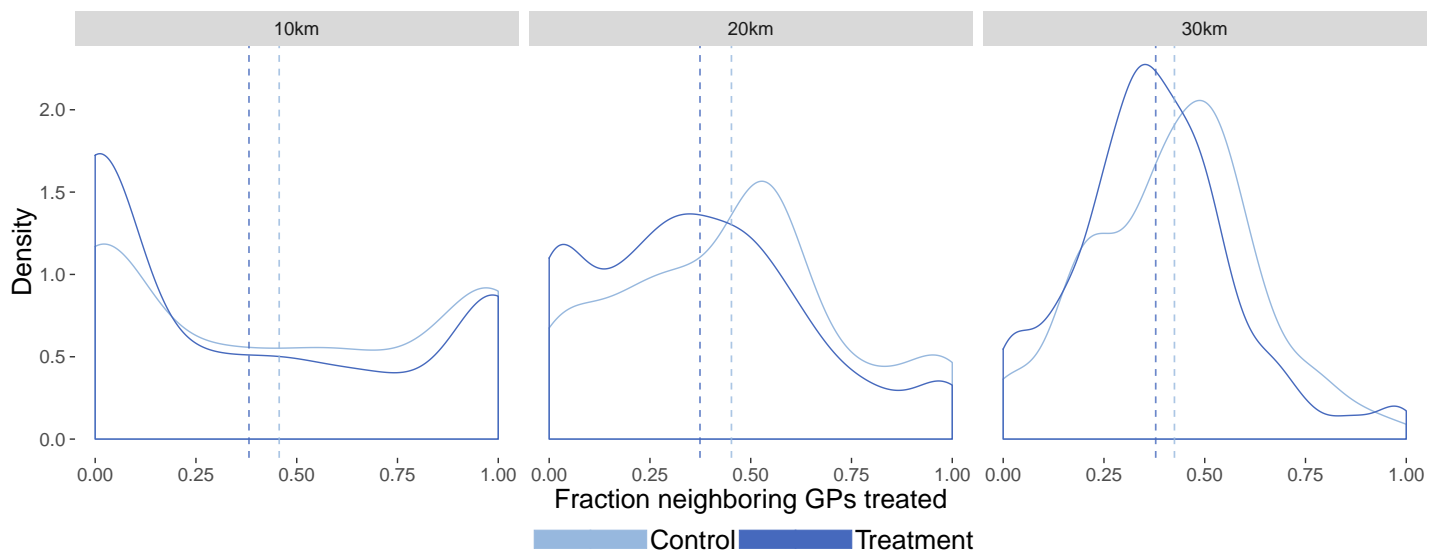
This figure shows an empirical density plot of reported total household landholdings (sum of wet and dry land owned by household in acres) in survey data by treatment and control in five bins: “1 acre or less”, “1 - 2 acres”, “2 - 3 acres”, “3 - 4 acres”, “4 acres or more”. We focus on creating bins at the lower end of the distribution to emphasize that the impact on landholdings occurs at the lower end of the landholdings distribution. Additionally, we conduct a Kolmogorov-Smirnov test of equality of distributions, and strongly reject equality, with the empirical distribution of landholding in the treatment group first-order stochastically dominating that in the control group ($p = 0.013$)

Figure A.8: Density of spatial measures of treatment exposure

(a) Exposure to Treatment: Including Same Mandal GPs



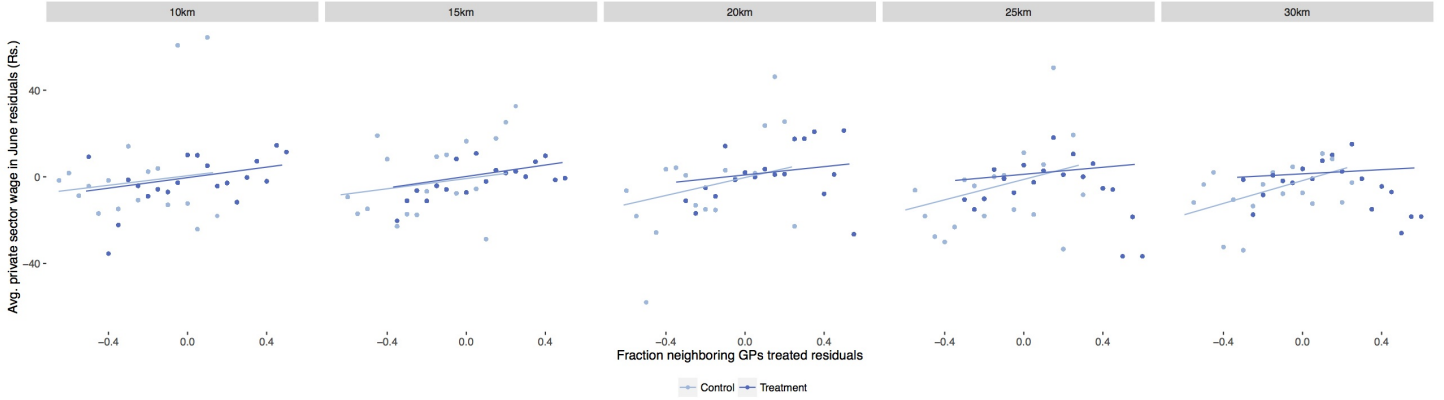
(b) Exogenous Exposure to Treatment: Excluding Same Mandal GPs



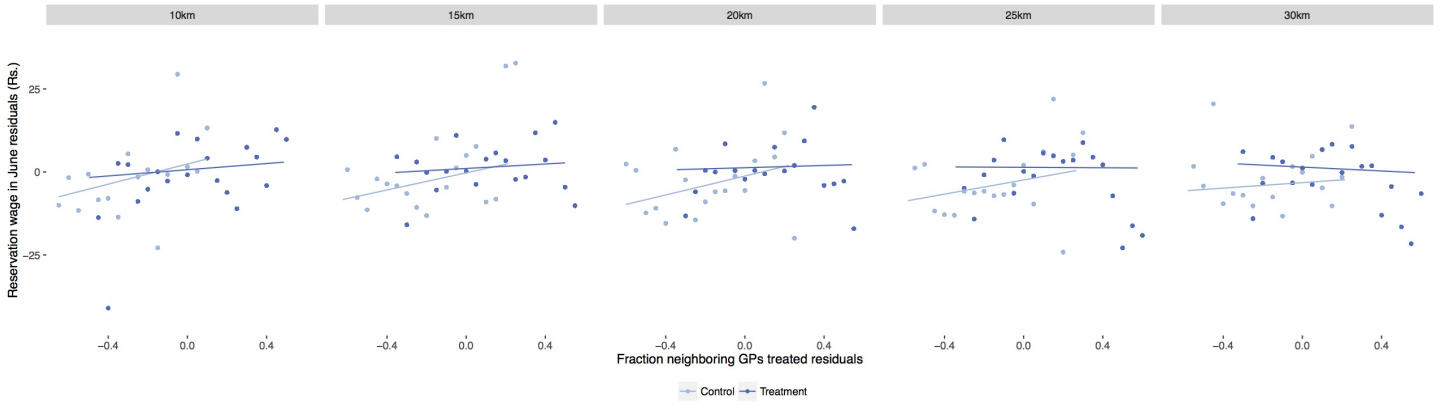
The figure shows smoothed kernel density estimates of spatial measures of treatment exposure. The only GPs included in these density calculations are surveyed GPs. Panel a) shows the distribution of exogenous spatial exposure to treatment at a given distance for survey GPs. Panel b) shows the distribution of spatial exposure to treatment at a given distance for survey GPs. The analysis was conducted at distance 10 km, 20 km, and 30 km. The spatial exposure measure is the ratio of the number of GPs in treatment mandals within radius R km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 GPs are included in the denominator, and that same-mandal GPs are *included* in both the denominator and numerator. The exogenous spatial exposure measure is the ratio of the number of GPs in treatment mandals within radius R km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 GPs are included in the denominator, and that same-mandal GPs are *excluded* in both the denominator and numerator.

Figure A.9: Relationship between Spatial Exposure and Wage/Income Outcomes

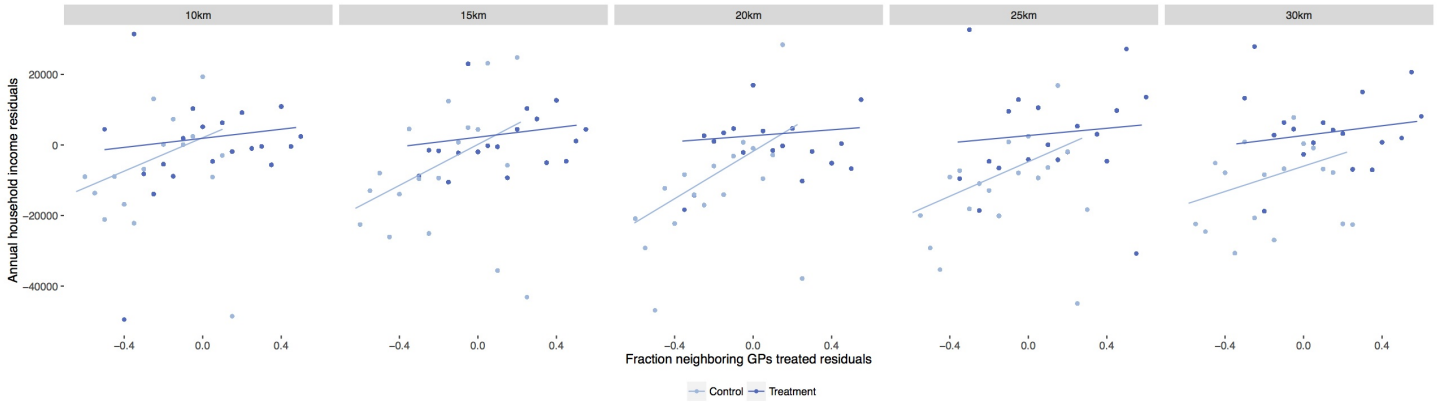
(a) Wage Realization



(b) Reservation wage



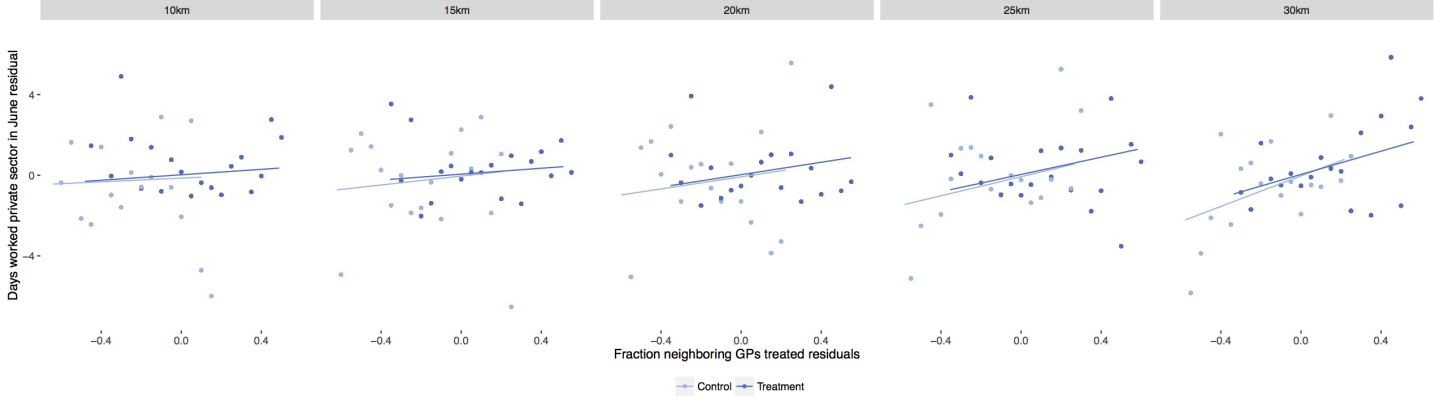
(c) Total income



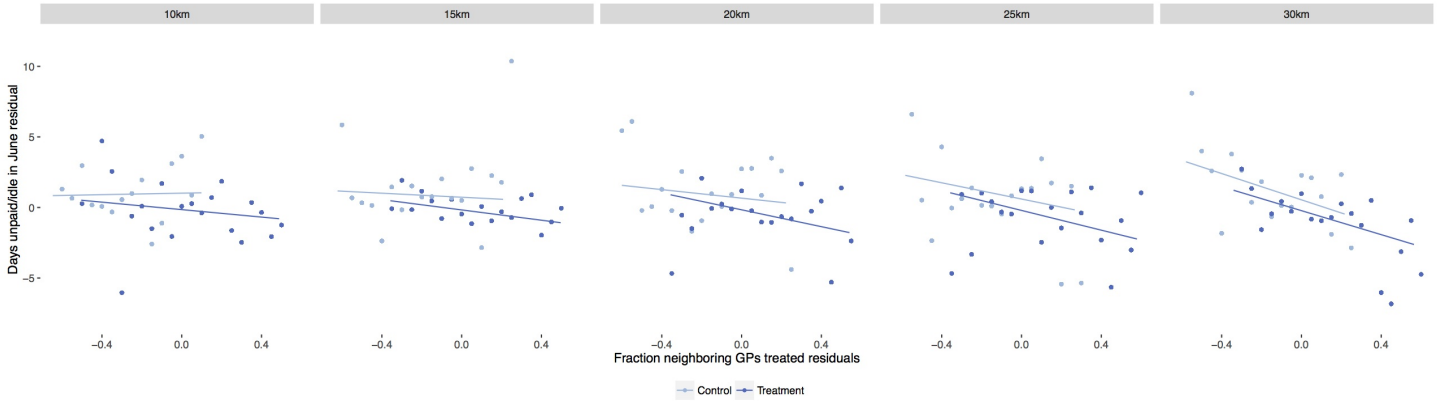
This figure shows partial residual plots for the relationship between the complete spatial exposure measure and wage/income outcomes from our survey data. For each plot, the x-axis variable is the residual from a linear regression of complete spatial exposure on district fixed effects and a first principal component of a vector of mandal characteristics that was used to stratify randomization. The y-axis variable is the residual from a linear regression of the outcome variable on district fixed effects and a first principal component of a vector of mandal characteristics that was used to stratify randomization.

Figure A.10: Relationship between Spatial Exposure and Labor Outcomes

(a) Days worked private sector

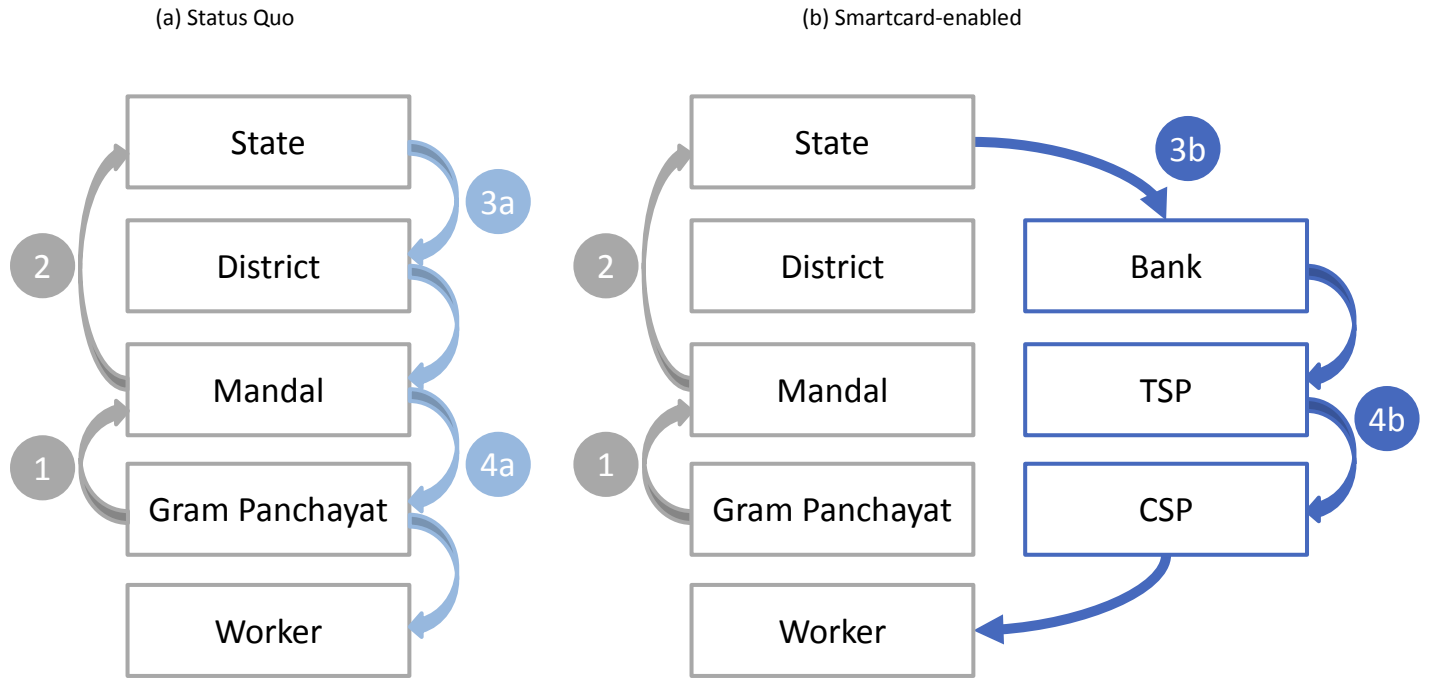


(b) Days unpaid/idle



The figure shows partial residual plots for the relationship between the complete spatial exposure measure and labor outcomes from our survey data. For each plot, the x-axis variable is the residual from a linear regression of complete spatial exposure on district fixed effects and a first principal component of a vector of mandal characteristics that was used to stratify randomization. The y-axis variable is the residual from a linear regression of the outcome variable on district fixed effects and a first principal component of a vector of mandal characteristics that was used to stratify randomization.

Figure A.11: Comparison of treatment and control payment systems



The figure (reproduced from Muralidharan et al. (2016)) shows the flow of information and funds for NREGS payments, pre- and post-Smartcards. “TSP” is a Technology Service Provider, a firm contracted by the bank to handle details of electronic transfers. “CSP” is a Customer Service Provider, from whom beneficiaries receive cash payments after authentication. The upward flow of information about work done is the same in both systems: (1) Paper muster rolls are maintained by the GP and sent to the mandal computer center, and (2) the digitized muster roll data is sent to the state financial system. However, the downward flow of funds is different. In the status quo model, (3a) the money is transferred electronically from state to district to mandal, and (4a) the paper money is delivered to the GP (typically via post office) and then to the workers. In the Smartcard-enabled system, (3b) the money is transferred electronically from the state to the bank to the TSP, and (4b) the TSP transfers cash to the CSP, who delivers the cash and receipts to beneficiaries (both with and without Smartcards). Beneficiaries with Smartcards were required to biometrically authenticate identity before getting paid. Beneficiaries without Smartcards were issued “manual payments” with status quo forms of authentication and acknowledgment of payment receipt.

The flow of information and funds for SSP payments differs in the following ways: (1) There is no weekly flow of information up from GP level to determine beneficiaries (no muster rolls etc); (2) In the status quo model, GP officials directly made payments to beneficiaries, sometimes in their homes; the post office was not involved; (3) In the Smartcard-enabled system, payments were made in the same way as for NREGS beneficiaries. In both models, SSP payments are made monthly at the beginning of the month, rather than weekly or bi-weekly like in NREGS. Note that the Bank/TSP/CSP structure for the Smartcard-based payments reflects Reserve Bank of India (RBI) regulations requiring that accounts be created only by licensed banks. Since the fixed cost of bank branches is typically too high to make it viable to profitably serve rural areas, the RBI allows banks to partner with TSPs to jointly offer and operate no-frills accounts that could be used for savings, benefits transfers, remittances, and cash withdrawals. In practice, the accounts were only used to withdraw government benefits and not to make deposits or maintain balances.