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

Karthik Muralidharan† UC San Diego
Paul Niehaus‡ UC San Diego
Sandip Sukhtankar§ University of Virginia

September 12, 2017

Abstract

Public employment programs play a large role in many developing countries' anti-poverty strategies, but their net impact on the incomes of the poor will depend on both direct program earnings as well as indirect effects through changes induced in market wages and employment. We estimate this composite effect, exploiting a large-scale randomized experiment across 157 sub-districts and 19 million people that substantially improved the implementation of India's rural employment guarantee scheme. Despite no changes in government expenditure on the program itself, the earnings of low-income households rose 13%, driven overwhelmingly by market (90%) as opposed to program earnings (10%). Low-skilled wages increased 6% and days without paid work fell 7%, while migration and prices were unaffected. Effects on wages, employment, and income also spilled over into neighboring sub-districts, and estimates of program impact that adjust for these spillovers are substantially larger, typically double the unadjusted magnitudes. These results suggest that well-implemented public works programs can be highly effective at reducing poverty. They also highlight the importance of general equilibrium effects in program evaluation, and the feasibility of studying them using large-scale experiments.

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

Keywords: public programs, general equilibrium effects, rural labor markets, NREGA, employment guarantee, India

*We thank David Atkin, Abhijit Banerjee, Prashant Bharadwaj, Gordon Dahl, Taryn Dinkelman, Roger Gordon, Gordon Hanson, Clement Imbert, Supreet Kaur, Dan Keniston, Aprajit Mahajan, Edward Miguel, Ben Moll, Dilip Mookherjee, Mark Rosenzweig and participants in various seminars for comments and suggestions. We are grateful to officials of the Government of Andhra Pradesh, including Reddy Subrahmanyam, Koppula Raju, Shamsher Singh Rawat, Raghunandan Rao, G Vijaya Laxmi, AVV Prasad, Kuberan Selvaraj, Sanju, Kalyan Rao, and Madhavi Rani; as well as Gulzar Natarajan for their continuous support of the Andhra Pradesh Smartcard Study. We are also grateful to officials of the Unique Identification Authority of India (UIDAI) including Nandan Nilekani, Ram Sevak Sharma, and R Srikar for their support. We thank Tata Consultancy Services (TCS) and Ravi Marri, Ramanna, and Shubra Dixit for their help in providing us with administrative data. This paper would not have been possible without the continuous efforts and inputs of the J-PAL/UCSD project team including Kshitij Batra, Prathap Kasina, Piali Mukhopadhyay, Michael Kaiser, Frances Lu, Raghu Kishore Nekanti, Matt Pecenco, Surili Sheth, and Pratibha Shrestha. Finally, we thank the Omidyar Network (especially Jayant Sinha, CV Madhukar, Surya Mantha, and Sonny Bardhan) and the Bill and Melinda Gates Foundation (especially Dan Radcliffe) for the financial support that made this study possible.

†UC San Diego, JPAL, NBER, and BREAD. kamurali@ucsd.edu.
‡UC San Diego, JPAL, NBER, and BREAD. pniehaus@ucsd.edu.
§University of Virginia, JPAL, and BREAD. sandip.sukhtankar@virginia.edu.

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.[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 2.1.1). 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 works programs by combining exogenous experimental variation, a demonstrable first-stage impact on implementation quality, units of randomization large enough to capture general

[1] Workfare 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 "Food-for-Work" programs across Sub-Saharan Africa and Asia (Subbarao, 2003).

[2] A practical way of differentiating partial and general equilibrium effects of an intervention (which we follow) is to define partial equilibrium effects as those estimated at constant prices, and general equilibrium effects as those which incorporate the effects of interventions on market prices.

equilibrium effects, and geocoded units of observation disaggregated enough to test 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 implementation.[4]

Evaluating the impact of improving NREGS implementation (as we do here) is not the same as evaluating the impact of rolling out the program itself. Yet, given well-documented implementation challenges in NREGS 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), a significant improvement in implementation quality is likely to result in a meaningful increase in any measure of *effective* NREGS. Further, since significant improvements in program performance were achieved without increasing the fiscal outlay on NREGS, our results are likely to reflect the *structure* of NREGS relative to other anti-poverty programs that simply transfer resources to the poor without also requiring employment on public works.

We report six main sets of results. First, we find large increases in incomes of NREGS-registered households (who comprise two-thirds of the rural population) in treated mandals two years after the Smartcards rollout began. Using our survey data, we find a 12.7% increase in household income in treated areas, which corresponds to a 17.5% reduction in an income-based measure of poverty (a 5.0 percentage point reduction on a base poverty rate of 28.5%).[5] We also find evidence of significant income gains 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.

[3]The 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.

[4]Smartcards 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 2.2.2).

[5]Putting 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.

Second, the vast majority of income gains are attributable to indirect market effects rather than direct increases in NREGS income. For NREGS-registered households, increases in program income accounted for only 10% of the total income increase, with the remaining 90% attributable to increases in private sector earnings. 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 private sector earnings are driven in large part by a significant increase in private market wages. Market wages rose by 6.1% in treated areas, with a similar 5.7% increase in reported reservation wages, suggesting that an improved NREGS increased workers' bargaining power by enhancing outside options. We find no evidence of corresponding changes in consumer goods prices, implying that the earnings and wage gains we find are real and not merely nominal.

Fourth, we find little evidence of efficiency-reducing effects on factor allocation. Despite higher wages in treated areas, we find a significant 7.1% reduction in the number of days idle or without paid work, with (insignificant) increases in the number of days of both NREGS and private sector employment. Once we adjust for spillovers (see below), we estimate that there was a significant *increase* in private sector employment. 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. 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 loans, suggesting reduced credit constraints that may have facilitated asset accumulation.

Finally, we find evidence that the labor market effects of treatment "spill over" into geographically proximate markets (including untreated mandals). We estimate spillovers by exploiting the fact that the randomization design generated variation in both a mandal's own treatment status and that of its neighbors. Spillover effects are consistent in sign with the direct effects of treatment, which both corroborates the latter and implies that they likely underestimate the "total treatment effect" a mandal would experience if *all* mandals were treated. We develop methods to estimate this effect, and find positive impacts on wages, employment, and income that are all significant and typically double the magnitude of the unadjusted estimates.

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., 2015; Sukhtankar, 2017). In addition to confirming some prior findings, like the increase in market wages (Imbert and Papp, 2015; Berg et al., 2012; 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,[6] (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* and not negative impacts 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 worker level) would have led to a ten-fold underestimate of impacts on poverty reduction. Even analyzing our own data while maintaining the assumption of no spillovers across administrative jurisdictions would lead us to meaningfully understate impacts. Read optimistically, our study demonstrates the feasibility of conducting randomized experiments with units of randomization that are large enough to capture such general equilibrium effects (Cunha et al., 2017; Muralidharan and Niehaus, forthcoming).

Third, our results contribute to the literature on wage determination in rural labor markets in developing countries (Rosenzweig, 1978; Jayachandran, 2006; Kaur, 2015), including that on the impacts of minimum wages (e.g. Dinkelman and Ranchhod (2012)). This literature relates directly to policy debates about the NREGS, whose critics have argued that it could not possibly have led to meaningful impacts on rural poverty because days worked on NREGS constitute only a small share (under 4%) of total rural employment (Bhalla, 2013). Our

[6] It is worth highlighting, for instance, that the most credible studies on NREGS to date (such as Imbert and Papp (2015)) report effects on wages but not poverty because there was no full round of the National Sample Survey in the years with adequate identifying variation in NREGS.

results suggest that this argument is incomplete. Much larger shares of rural households in AP are registered for NREGS (66%) and actively participate (32%) in the program, and our results suggest that the very existence of a well-implemented public employment program can raise wages for these workers by providing a more credible outside option (Dreze and Sen, 1991; Basu et al., 2009). We see direct evidence of this channel through the increase in reservation wages in treated areas.

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 the wage volatility induced by productivity shocks, and may be hurt by programs like NREGS that provide wage insurance to the rural poor (Jayachandran, 2006). Landlords also directly benefit from lower average wages, and 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." Our results showing that improving NREGS implementation substantially raised market wages suggest that landlords may have been made worse off by the reform. This may partly explain the widely documented resistance by landlords to NREGS (Khera, 2011), and the need for beneficiaries to mobilize politically to push governments to improve NREGS implementation (Khera, 2011; Jenkins and Manor, 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, and employment. Section 5 examines spillover effects. Section 6 discusses mechanisms, and Section 7 concludes with a discussion of policy implications.

[7] In 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 National Rural Employment Guarantee Scheme (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: 65.7% of rural households in Andhra Pradesh have at least one jobcard, which registers them for the program and entitles them to request work. Legally they can 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. The physical nature of the work is meant to induce self-targeting. NREGS projects are proposed by village-level local governance bodies (Gram Panchayats) and approved by sub-district (mandal) offices. Typical projects include public infrastructure improvement such as irrigation or water conservation works, minor road construction, and land clearance for cultivation.

The NREGS suffers from a number of known implementation issues. Although job creation is meant to be demand driven, rationing is common, and access to work is constrained both by budgetary allocations as well as local capacity to implement projects (Dutta et al., 2012; Witsoe, 2014). Corruption is also common, including over-invoicing the government to reimburse wages for work not actually done and also paying workers less than statutory wage rates for completed work (Niehaus and Sukhtankar, 2013a,b). Finally, the payment process is slow and unreliable: payments are often delayed over a month beyond the 14-day period prescribed in the law; there is considerable uncertainty over timing of payments; and payment collection is time-consuming.[8]

2.1.1 Prior evidence on NREGS impact

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, and creating useful rural infrastructure such as roads and canals, and reduced negative outcomes like distress migration (Khera, 2011). Skeptics claim that funding is largely captured by middlemen and wasted, and that the scheme could not meaningfully affect the rural economy since it accounts

[8]The cited studies mostly reflect the first five to seven years of NREGS (2005-2012), and some of these issues may of course have improved over time.

for only a small part of rural employment ("how can a small tail wag a very very large dog?" Bhalla (2013)). Even if it did increase rural wages, they argue that this would come at 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.[9]

This policy debate is constrained by the limited availability of credible causal estimates of program impact. NREGS was rolled out across districts in three phases between 2006-2008, with districts selected for earlier phases based partly on an index of deprivation, and partly on political considerations (Gupta, 2006; Chowdhury, 2014). In the absence of randomization, most empirical work on the impacts of NREGS uses either a difference-in-differences or a regression discontinuity approach for identification, with both approaches having limitations.[10] A second challenge is that NREGS implementation quality varies widely, and is typically not measured directly. Thus, differences in findings across existing studies could also reflect variation in unmeasured program implementation quality. Further, identification strategies that use the staggered phase out of NREGS, have to rely by design on the early years of the program when implementation quality was weak (Mehrotra, 2008). Thus, impacts estimated using the initial roll-out for identification may be less informative about steady state effects after implementation teething troubles are resolved.

These issues may help explain the wide variation in findings to date across different studies and for a range of outcomes. For wages specifically, the key outcome that our paper has in common with others, three studies using a difference-in-differences approach estimate a positive 4-5% effect on rural unskilled wages (Imbert and Papp, 2015; Berg et al., 2012; Azam, 2012). In contrast, one study using a regression discontinuity approach finds no impact (Zimmermann, 2015).[11]

Accounting for spillovers across program and non-program areas presents a further challenge. Most existing work uses data with geographic identifiers at the district level. Since

[9]In theory, the program should be fully funded based on demand. In practice, fiscal appropriations are made in advance and program availability is limited by these allocations. For instance, an article in 2016 reports a sharp reduction in availability of NREGS work as a result of a reduction in budgetary allocation: `http://thewire.in/75795/mnrega-centre-funds-whatsapp/`, accessed November 3, 2016.

[10]The difference-in-difference approach is limited by the fact that the parallel trends assumption often does not hold without additional controls, whereas the regression-discontinuity based approach is constrained by limited sample size and (likely) lack of power at reasonable bandwidth choices (Sukhtankar, 2017).

[11]Findings on other outcomes vary similarly. For education, Mani et al. (2014) find that educational outcomes improved as a result of NREGS, Shah and Steinberg (2015) find that they worsened, and Islam and Sivasankaran (2015) find mixed effects. For civil violence related to the leftist Naxalite or Maoist insurgency, Khanna and Zimmerman (2014) find that such violence increased after NREGS, while Dasgupta et al. (2015) find the opposite.

the identifying variation is also at the district level, this makes it difficult to test or correct for spillovers. In one recent exception, Merfeld (2017) uses ARIS/REDS data with village geo-identifiers and finds some evidence of spatial spillovers, with weaker effects on wages near borders of program/non-program districts and stronger effects away from borders. While the estimates are imprecise due to small sample size, these results suggest that ignoring spatial spillovers may bias existing estimates of the impact of NREGS.

2.2 Smartcards

To address the challenges mentioned above with leakage and the payments process, 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 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.

2.2.1 Effects on NREGS implementation quality

MNS shows using a randomized evaluation that Smartcards significantly improved NREGS implementation quality on multiple dimensions. Two years after the intervention began NREGS 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. Finally, these effects were achieved despite no increase in fiscal outlay on NREGS itself in treated areas.

These gains were also 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, suggesting broad-based gains from the move to Smartcard-based payments. Reflecting this, users were strongly in favor of

Smartcards, with 90% of households preferring it to the status quo, and only 3% opposed.

Thus, Smartcards substantially improved program implementation and brought the *effective* presence of NREGS in treated areas closer to the intentions of the program's framers. In this paper, we aim to study the impact of this improvement on rural labor markets, wages, employment, and income. One natural interpretation of our approach is to think of Smartcards as an instrumental variable for an abstract endogenous variable,"effective NREGS." However, given the many dimensions on which NREGS implementation quality can and did vary (ease of access to work, availability of work on demand, payment delays and inconvenience, and leakage) it is implausible to construct such a single-dimensional variable in practice. Our results are therefore best interpreted as the reduced form impact of improving NREGS implementation quality on multiple dimensions.

2.2.2 Impacts of Smartcards outside of NREGS

In principle, Smartcards could have affected the rural economy independent of the NREGS. The two potential channels are pensions and financial inclusion, which we discuss below.

Smartcards were also used to make payments in (and reduced leakage in) the rural social security pensions (SSP) program, raising the question whether they might have affected markets through this additional channel. It appears unlikely that these improvements in pensions would have affected rural labor markets for at least four reasons. First, the scale and scope of SSP is narrow: only 7% of rural households are eligible (whereas 66% of rural households have NREGS jobcards). Second, the benefit is modest, with a median and mode of Rs. 200 per month (about $3, or less than two days earnings for a manual laborer). Third, the improvements from the introduction of Smartcards were much less pronounced than those in NREGS: there were no significant improvements in the payments process, and 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 program was targeted to the poor who were *not able to work* and thus aimed to complement NREGS, the primary safety net for the poor who could work.[12] Thus, SSP beneficiaries are unlikely to have been able to affect or be directly affected by the labor market. As we later show, treatment generated smaller income gains among households in which a larger share of members were eligible for SSP, with *no income gains* in households where all adults were eligible for SSP (Table A.1).

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. The

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

most important reason was that GoAP was concerned about delayed payments, underpayment, and ghost accounts, and therefore did not allow undisbursed funds to remain in Smartcard accounts. They urged banks to fully disburse NREGS wages as soon as possible after work was performed to improve compliance with the 14-day statutory requirement for making payments to workers. Further, the bank accounts created had limited functionality: they were not connected to the core banking servers and instead relied on offline authentication with periodic reconciliation. Thus, accounts could only be accessed through a single Customer Service Provider and were inaccessible otherwise. 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 (about 5% of daily wage for unskilled labor).[13]

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.

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 buffer group to ensure that we could conduct endline surveys before Smartcard deployment began in control mandals, and restricted survey work to treatment

[13]See Mukhopadhyay et al. (2013), especially pp. 54-56, for a more detailed discussion on why Smartcards were not able to deliver financial inclusion. Finally, unlike the national ID program *Aadhaar*, Smartcards themselves were not considered legally valid proof of identity and were of no use outside the NREGS and SSP programs as they were not guaranteed to uniquely identify individuals (unlike *Aadhaar*, the database of Smartcard accounts created was not de-duplicated).

[14]The 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.2). Tables A.2 and A.3 that summarize the validity of the experimental design are reproduced exactly from MNS.

and control mandals. We stratified randomization by district and by a principal component of mandal socio-economic characteristics.

We examine balance in Tables A.4 and A.5. 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 well balanced, with differences significant at the 5% level in 2 out of 22 cases. The latter shows balance on the outcomes that are our primary interest in this paper, as well as key socio-economic household characteristics from our baseline survey. Here, 4 out of 34 variables are significantly different at the 10% level at least, slightly more than one might expect by chance. Where feasible, we also test for sensitivity of the results to chance imbalances by controlling for village level baseline mean values of the outcomes.

3.2 Data

3.2.1 Socio-Economic and Caste Census

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 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.

3.2.2 Original survey data

We complement the broad coverage of the SECC data with original and more detailed surveys of a smaller sample of households. We conducted surveys of a representative sample of NREGS jobcard holders during August to October of 2010 (baseline) and 2012 (endline). Surveys covered both participation in and experience with the program, and also earnings, expenditure, assets, and liabilities more generally. Within earnings, we asked detailed questions about household members' labor market participation, wages, reservation wages, and earnings during June, the period 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. In Andhra Pradesh, 65.7% of rural households have a jobcard (our calculations from the National Sample Survey (NSS) Round 68 in 2011-12). For context, in the NSS data, jobcard-holding households are generally larger and more likely to work as agricultural laborers, but own insignificantly less land, suggesting that we are likely to see a full spectrum of labor market impacts within this grouping (Table A.6).

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. 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).[15]

3.2.3 District Statistical Handbook data

We use District Statistical Handbooks (DSH) published by the Andhra Pradesh Directorate of Economics and Statistics, a branch of the Central Ministry of Agriculture and Farmers Welfare, to obtain additional data on land under cultivation and extent of irrigation and on employment in industry. DSH are published every year and land coverage data presented in the DSH are provided by the Office of the Surveyor General of India.[16]

3.2.4 National Sample Survey data

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 on a sample that is representative at the state and sector level (rural and urban). The data provide a comprehensive picture of household-level consumption and expenditure for 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 the overlap between villages in our study mandals and the NSS sample is limited to 60 villages, and we therefore use the NSS data primarily to examine price levels, for which it is the best available data source.

3.2.5 Census data

We use spatial data from the 2001 Indian Census, which contains a geocoded point location for each census village.

[15]These numbers differ from MNS, where we report the pooled sample numbers from the two independently drawn samples of NREGS jobcard holders and SSP beneficiaries.

[16]Details on data sources for the DSH are at: `http://eands.dacnet.nic.in/`, accessed March 22, 2016.

3.2.6 Livestock Census data

We use data on mandal-wise headcounts of livestock and poultry 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.

3.3 Estimation strategy

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

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

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, ordered probit). When using our survey data, we also report specifications that include the baseline GP-level mean of the dependent variable, $\overline{Y}^0_{pmd}$, when available in order to increase precision and assess sensitivity to any randomization imbalances (note that we have a village-level panel and not a household-level one):

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

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. Regressions with 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. If Smartcards have general equilibrium effects, however, then these 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. Estimating this TTE is complex enough that we defer it to Section 5, and simply 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.[17] We see that the treatment distribution first-order stochastically dominates the control, with 4.0 percentage points fewer households in the lowest category (less than Rs. 5,000), 2.7 percentage points more households in the middle category (Rs. 5,000 to 10,000), and 1.4 percentage points more in the highest category (greater than Rs. 10,000).

Table 1a reports statistical tests, conditioning on district fixed effects to obtain experimental estimates of impact. We report logistic regressions for each category individually (showing marginal effects) and also an ordered logistic regression pooling data across all three categories. We find that treatment significantly increased the log-odds ratio of being in a higher income category. We also confirm that these estimates are unaltered when we control for demographic characteristics that should not change with treatment like age of household head, caste, 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 4.0% 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 for a better sense of magnitudes of impact. 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

[17]The SECC income categories are based on the monthly income of the highest earning household member.

estimate that that treatment increased annual income by over Rs. 8,700. 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 5.0 percentage point or 17.5% reduction in poverty (Figure A.2).[18]

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.

Columns 3-8 of Table 1b report treatment effects on various income categories separately. Strikingly, effects on NREGS earnings are a small proportion of overall income gains, accounting for only 10.4% of the overall increase.[19] Thus, 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. Effects on own farm earnings (which include earnings from livestock) are positive but insignificant.

[18] Results from the SECC and from our survey are not directly comparable since (a) the SECC is a census while our survey is representative of the 66% of households that have a jobcard, and (b) the SECC measures earnings of the highest earner in the household while our survey measures total household earnings. Yet, the results are similar and consistent across both sources (for both levels and distributions of income).

[19] The observant reader may note that the (marginally) insignificant effect on NREGS earnings here appears to contrast with the significant effect in MNS. The two estimates are for two distinct measures of NREGS earnings: the measure in MNS comes from questions about a specific 6-week study period shortly before surveys were conducted and asked to the worker themselves (usually with the job-card on hand to aid recall and accuracy regarding work and earnings in each of those 6 weeks), while the measure here is from an aggregate, annual recall question posed to the head of the household, and is thus less precise. The point estimates are nevertheless economically similar.

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. This is also consistent with the patterns seen in the SECC data (Figure A.1).

Table A.1 tests for differential treatment effects in our survey data by household characteristics using a linear interaction specification.[20] 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.

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

4.4 Effects on private labor markets

We next unpack impacts on private sector earnings, examining how wages and labor quantities were affected.

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 reported by respondents who did any private-sector work. 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, with additional robustness checks in Section 4.7 below.

We estimate a significant increase of Rs. 7.8 in private sector daily wages (Table 2, Column 2). This is a large effect, equal to 6.1% of the control group mean. In fact, it is slightly

[20]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).

larger than the highest estimates of the 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) 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. 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.

Given that NREGS peaks in the late spring and early summer, one natural question is whether the wage effects we observe in June are seasonal, or persist throughout the year. While our household survey data on individual labor spells cover only the month of June, we also have data from village leaders on "going wage rates" throughout the year. With one observation per village-month, power is limited; but these data do suggest persistent wage differences between treatment and control mandals (Figure A.4). These could imply that the NREGS still provides a meaningful outside option even during peak labor demand season, or alternatively could reflect nominal wage rigidity (Kaur, 2015) and/or labor tying over the agricultural cycle (Bardhan, 1983; Mukherjee and Ray, 1995), as well as more complex macroeconomic interactions.

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 by adults (ages 18-65) into three categories: time spent working on the NREGS, time spent working in the private sector, including self-employment, and time spent idle/on leisure.

We find a significant *decrease* of 1.2 days per month in days spent idle, equal to 7% 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 0.9 days and 0.5 days per month, respectively (though these changes are not individually significant) (columns 1-4). The lack of a decline in private sector employment is not simply because there is no private sector work in June. Figure A.3 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.

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) does not distinguish domestic work and self-employment from wage employment for others. They also study impacts on a different population at a different point in time.[21]

Overall, given the persistence of wage impacts, our point estimates for wage, labor and earnings effects are internally consistent. The 11.3% increase in private sector earnings is almost exactly equal to the sum of the 6.1% increase in wages and (insignificant) 5% increase in employment.[22]

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.9 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 53 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.

The lack of impact on migration is consistent with the existence of countervailing forces that may offset each other. On one hand, increases in rural wages may make migration less attractive; on the other hand, increased rural income may make it easier to bear the transport and search costs of migration (Bryan et al., 2014; Bazzi, 2017).

[21]In particular, they study NREGS during its early years, when the program's focus was on providing employment as opposed to construction of productive NREGS assets. There is evidence that the emphasis of NREGS shifted towards creating productive public assets by the time of our study (Narayanan, 2016).

[22]Our 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.8. 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.

4.5 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):

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

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.[23]

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 restrict 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,

[23]Theoretically 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.

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, and can thus be interpreted as real wage and income gains for workers.

4.6 Balance sheet effects

To the extent that 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 5.6% percentage points, or 9.5%. 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.

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 15 different types of livestock; in Table 6 we report impacts on the 9 types for which the average control mandal has at least 100 animals. We find positive impacts on every category of livestock except one, including substantial increases in the number of buffaloes ($p < 0.001$), backyard poultry ($p = 0.093$), and fowls ($p = 0.100$). 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.[24] With that caveat in mind, Table A.10 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 10% increase in expenditure on frequently purchased items or a 16% 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 but again insignificant, and we cannot rule out a 16% increase in expenditure.

4.7 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.11). 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.12).

Our wage results are robust to alternative choices of sample. The main results include data on anyone in the household who reports wages. Restricting the sample to only those of working age (18-65) again does not qualitatively affect results (Table A.13b). Next, dropping the small number of observations who report wages but zero actual employment again does not matter (Table A.13c). Results are also largely 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, a potential concern is that the effects we estimate are driven by changes in who reports work (or wages) and not by changes in 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.14. 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).

[24]The 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.

5 Spatial spillovers

Improving NREGS implementation in one mandal may have affected outcomes in other neighboring mandals. Higher wages in one mandal, for example, might attract workers from nearby villages and thus affect labor markets in those villages. We turn now to testing for such spillover effects and to estimating "total" treatment effects that account for them.

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, and no feasible experiment could identify these functions nonparametrically. We therefore take a simple approach, modeling spillovers as a function of the fraction of neighboring GPs treated within various radii R. Specifically, we define N_p^R as the fraction of GPs within a radius R of panchayat p which were assigned to treatment. Figure 2 illustrates the construction of this measure.[25]

One might hope that the random assignment of mandals to treatment and control arms ensures that the neighborhood measure N_p^R is also "as good as" randomly assigned, but this is not the case. To see this, consider constructing the measure for GPs within a treated mandal: on average, GPs closer to the center of the mandal will have higher values of N_p^R (because more of their neighbors are from the same mandal), while those closer to the border will have lower values (because more of their neighbors are from neighboring 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 unless we are willing to make the (strong) assumption that the direct effects of treatment are unrelated to location.[26]

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, with the disadvantage that it is not defined for some GPs when R is small, as they may be more than R kilometers from the border of their mandal. We use this measure both to test for the existence of spillovers (where we are interested in testing rather than point estimation) 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 construct our measures of neighborhood treatment intensity at radii of 10, 15, 20, 25, and

[25] Note 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.

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

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 hrs. Moreover, effects can propagate much further than the distance over which any one actor is willing to arbitrage due to spatial interlinkage, with changes in one market rippling on to the next and so on (as shown recently in the UK by Manning and Petrongolo (forthcoming)).

Figure A.5 plots smoothed kernel density estimates by treatment status for several of these measures to illustrate that there is meaningful overlap in the distributions for control and treatment group. Tables A.15 and A.16 report tests showing that our outcomes of interest are balanced with respect to these measures at baseline.[27]

5.1 Testing for spillovers

To test for the existence of spillovers, we estimate

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

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 treatment status), which imposes equality of the slope coefficient 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 (Table 7, panel (c)), 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), in addition to significant direct effects of being in a treated mandal (Table 7, panel (d)).

[27] A 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).

Note that sample sizes increase as we increase the neighborhood radius R, since at larger values of R a larger share of panchayats have at least one neighbor within distance R and in another mandal; we use between 90% and 100% of the available data depending on specification. Effect sizes and t-statistics are for the most part larger, however, for larger values of R, suggesting that excluding sample is not biasing us towards rejecting the null.[28]

Overall, spillover results – estimated using a different source of variation and outcomes measured in different locations – strongly corroborate our earlier results in Section 4. In particular, finding effects on wages, employment, and earnings in *control* villages that had greater exposure to treated villages (but did not directly experience an improvement in NREGS implementation), confirms the existence of market-level general equilibrium effects from increasing NREGS presence.

5.2 Estimating total treatment effects

The conceptual distinction between the unadjusted and total treatment effects can be seen in Figure 3. 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.5). 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. 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.

To estimate this effect with our data, we first estimate

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

Here β_T captures the effect of own treatment status, β_N the effect of neighborhood treatment

[28] Alternatively, we can construct tests using 100% of the data if we are willing to make the (strong) assumption that N_p^R is exogenous. We obtain directionally similar results when doing so (Table A.17), but the estimates are less precise.

exposure, and β_{TN} any interaction between the two. The total effect of treatment is then

$$\overline{\beta} = \beta_T + \beta_N + \beta_{TN} \tag{6}$$

Since N_p^R is potentially endogenous, 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.18; 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 all of our main outcomes – wages, labor, and earnings – 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.

Table A.20 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.[29]

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 work done in the private sector is positive and significant (for all $R > 10$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.

Finally, in panel (c) we document the same pattern for total earnings: the estimated total

[29] To 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.20). 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.

treatment is larger than the conservative, unadjusted estimate and significant at the 10% level (at $R < 20$km).[30] The standard errors on the TTE for all outcomes are considerably larger than those on the unadjusted effects because estimation of the former uses all terms in Table A.20 and is less precise as a result. Yet, the TTE point estimates are always larger than the unadjusted estimates.

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.6 and A.7), 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).

6 Discussion

Like any change to a complex economy, improvements in the NREGS are likely to affect outcomes such as wages, employment, and income in several ways, with multiplier effects, feedback loops, and interactions all contributing to the overall impact. Thus, unlike in a partial equilibrium analysis, it is implausible to break down effects into a simple linear decomposition between distinct, independent channels. We can still test, however, whether there is evidence that specific mechanisms, suggested by theory, are operative.

One mechanism we are able to test directly is the hypothesis that a (better-run) employment guarantee puts competitive pressure on labor markets, driving up wages and earnings. As the results in Table 2 show, we find a significant positive impact not just on market wages, but also on reservation wages, providing direct evidence for this mechanism. Note that the channel from increased reservation wage to increased market wage does not have to involve direct bargaining between workers and employers. In practice, workers are often hired for projects (such as construction) in spot labor markets where intermediaries (labor contractors) post wages and hire workers as required. So if an improved NREGS reduces the supply of labor to the open market at any given wage, the market clearing wage will likely increase even without direct bargaining.

One piece of indirect evidence in support of this mechanism is that the difference between the unadjusted and total treatment effects in Table 8 is smaller for reservation wages (∼75-100%, not statistically significant) compared to the difference for market wages (>300%,

[30] Because the distribution of earnings is right-skewed, results for earnings are potentially more sensitive to top-censoring than those for other outcomes. In Table A.19 we examine earnings results for a range of censoring thresholds. If anything our choice of 0.5% is conservative; estimated TTE are larger and more statistically significant both when we censor more (1%) and when we censor less (0% or 0.1%).

statistically significant). In other words, changes in reservation wages appear to mainly depend 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 increased competition from NREGS even in other nearby villages (which is less likely to reflect direct bargaining).

A second possibility is that an improved NREGS created additional productive assets – roads, irrigation facilities, soil conservation structures, etc. We find no impact on the number and composition of projects *reported* as implemented in treatment areas relative to control (Table A.21), and can rule out large effects – for example, the data reject a 14% increase in the number of projects. Yet, given that there was a substantial reduction in leakage and increase in days of work reported without any change in fiscal outlay, it is conceivable that there may have been an increase in *actual* assets created proportional to the reduction in leakage (despite no change in the officially listed NREGS projects). Since we did not conduct independent audits of asset quality, we are not able to confirm or rule out this possibility.

We also tested if NREGS projects such as land improvement or minor irrigation projects led to an increase in area under cultivation or irrigated. We find no impact on the amount of land under cultivation (% area sown or % area fallow) or on the total area irrigated (Table A.22). The data rule out effect sizes larger than 16% and 10% for area sown and irrigated, respectively. Yet, it is also possible that the data from the district handbooks that we use for the calculations above only reflect irrigation projects undertaken by the Ministry of Irrigation, and not those of smaller informal projects undertaken under NREGS. We also do not have data on cropping patterns to examine whether these changed.[31] Finally, we have no access to reliable data on land prices, which would potentially pick up the effects of any asset creation that increased the marginal product of land. Thus, while we find no direct evidence that assets created under NREGS improved productivity, we also cannot rule out the possibility.

Another a priori plausible mechanism of impact is that the reform increased cash flow into treated areas, stimulating local economic activity and driving up wages and earnings (Krugman, 1991; Magruder, 2013). In practice, however, we find no effect of the intervention on the amount of money disbursed by the NREGS in administrative data, and no significant increase in the (admittedly noisy) measure of household expenditure in our survey data (Table A.10). Yet, the point estimates of expenditure impacts as measured in the NSS data are positive, and more generally our confidence intervals for expenditure include meaningful

[31] Cropping patterns might also change if improved wage insurance may have increased planting of higher yielding, but potentially riskier crops.

positive effect sizes. So, though there was no increase in fiscal outlays, we cannot rule out the possibility of an aggregate demand channel through redistribution of income from landlords and corrupt officials to the poor, who may have a higher marginal propensity to consume.[32]

We do see evidence of improved credit access, with higher borrowing in treated areas (Table 5), and also find evidence of increased asset ownership among the poor (both land and livestock). While increased land ownership among the poor likely means reduced land holdings among the rich, it is possible that such a redistribution had positive effects on productivity (as shown by Banerjee et al. (2002)). Further, increases in livestock ownership are likely to have directly raised both income as well as the marginal product of labor (which is a complement to livestock as shown in Bandiera et al. (2017)).

Finally, the fact that private sector employment did *not* fall as wages rose is also potentially informative about mechanisms. All else equal, rising wages in a competitive labor market should reduce employment; thus, the positive effects on private-sector employment suggest either that productivity improved (through any of the channels above) and increased labor demand, or that labor markets are not perfectly competitive. Indeed, there is a long-standing debate over the degree of labor market power held by rural employers (e.g. Griffin (1979)), as well as recent evidence from multiple settings that employers enjoy considerable market power in wage determination (Manning, 2011; Naidu et al., 2016). Thus, while we have no direct evidence of oligopsonistic labor markets in our setting, we cannot rule out the possibility that imperfectly competitive labor markets also contributed in part to our results.

In summary, the discussion above highlights that rural economies are complex, and that a public employment program is likely to affect them through several mechanisms that operate simultaneously and likely interact with each other over time. While we test for the existence of several possible channels and find evidence for some of these, the absence of evidence for a specific channel should *not* be interpreted as evidence of absence of that channel. Thus, our results are best interpreted as estimating the policy-relevant general equilibrium effects of improving NREGS, which is inclusive of *all channels* that may be relevant in this setting.

7 Conclusion

This paper contributes to understanding the economic impact of public employment programs in developing countries with three main sets of advances over the existing literature. These include (a) improved identification (experimental variation with units of randomiza-

[32] Santangelo (2016) finds evidence of such an aggregate demand channel in the context of the *introduction* of NREGS (with associated increase in fund-flows to rural areas), and highlights that the role of NREGS in stabilizing rural wages may also stabilize demand with positive implications for production and employment.

tion that are large enough to capture general equilibrium effects, and geo-coded units of observation that are granular enough to test and adjust for spatial spillovers), (b) measurement of actual implementation quality, which allows estimated impacts to be interpreted as the results of a demonstrable difference in the *effective* presence of the program between treatment and control areas and (c) a broader set of outcome data including reservation wages, income, and assets (with independently collected census data on the last two in addition to survey data).

Our 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, the vast majority of these impacts are attributable not to the direct income gains from the program, but to general equilibrium changes in market wages and employment induced by an increase in the effective presence of such a program. Importantly, we find that despite the increase in market wages, there was an *increase* in market employment, suggesting that well-implemented public employment programs can be efficiency enhancing.

While we estimate the effects of improving NREGS implementation, a natural question to ask is how our estimates compare to those from a hypothetical comparison between a "well-implemented NREGS" and "no NREGS." Our conjecture is that the effects would be broadly comparable, but with larger income effects. The Smartcards reform increased the labor-market appeal of the NREGS and increased participation in its projects, but did not increase the flow of funds into treated areas. In contrast, the NREGS per se clearly represents a significant transfer of funds from urban to rural areas. Thus, a fresh roll-out of a *well-implemented* NREGS would likely have larger effects than those we find, but the direction of the key impacts on wages, employment, and income is likely to be the same.

Our results speak directly to currently active policy debates on the optimal design of anti-poverty programs in developing countries. For instance, the most recent Economic Survey of India raises the question of 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 (see for example Besley and Coate (1992) and Murgai and Ravallion (2005)) have emphasized three main reasons that an employment guarantee scheme could be more cost-effective at reducing poverty than a direct transfer. First, an EGS could be self-targeting in a way that an unconditional transfer would not, as only those in greatest need would voluntarily accept hard physical labor at low wages (of course, the EGS does impose the cost of hard physical labor on the poor). Second, an EGS could create valuable public goods that markets would otherwise fail to produce – the NREGS, for example, is intended to create assets such as roads and shared irrigation infrastructure among other things. Third, an EGS could address labor market imperfections such as oligopsony power among local employers by forcing wages up and closer to their perfectly competitive level.

While a comprehensive cost-benefit analysis of the NREGS is beyond the scope of this paper, our results shed some light on these issues. Specifically, the impacts we see on the allocation of labor – significant reductions in time spent idle and significant increases (after adjusting for spillovers) in private sector employment – are consistent with either of the latter two effects. To the extent the wage gains and employment increases we see are the result of reducing labor market imperfections, they represent pure efficiency gains. If on the other hand they are largely driven by the creation of assets with public goods characteristics, then this implies the *possibility* of efficiency gains. It is not dispositive – it is possible for example that the program creates productivity-enhancing public goods, but at such great cost that net efficiency falls. But given that many people's priors are that NREGS assets are of no value at all (World Bank, 2011), even this interpretation would represent a positive update.

On net, the results considerably raise our own posterior beliefs that an EGS could be a cost-effective anti-poverty strategy relative to a direct transfer.

Finally, our results provide an illustration of the well-known idea that the costs of corruption and weak implementation of policies and programs go beyond the direct cost of diverted public resources and extend to the broader economy (Murphy et al., 1993). In practice, the empirical literature on corruption has typically relied on a forensic approach to 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 has studied the impacts of specific interventions on reducing such leakage (Olken, 2007; Muralidharan et al., 2016). However, it has been more difficult to quantify the broader economic costs of corruption with well-identified estimates. Our results enable such a quantification in this setting, and suggest that poor NREGS implementation hurts the poor much more by preventing/diluting market-level effects than by the diversion of NREGS wages themselves.[33] They also reinforce the importance of building state capacity for better service delivery in developing countries.

[33]Similarly, the cost of poor delivery of health and education services in developing countries in terms of reduced human potential is likely to far exceed the direct fiscal cost of the corruption and inefficiency that has been extensively documented in the literature (World Bank, 2003; Chaudhury et al., 2006).

References

Acemoglu, Daron, "Theory, General Equilibrium, and Political Economy in Development Economics," *Journal of Economic Perspectives*, 2010, *24* (3), 17–32.

Anderson, Siwan, Patrick Francois, and Ashok Kotwal, "Clientilism in Indian Villages," *American Economic Review*, 2015, *105* (6), 1780–1816.

Azam, Mehtabul, "The Impact of Indian Job Guarantee Scheme on Labor Market Outcomes: Evidence from a Natural Experiment," Working Paper 6548, IZA 2012.

Bandiera, Oriana, Robin Burgess, Narayan Das, Selim Gulesci, Imran Rasul, and Munshi Sulaiman, "Labor markets and poverty in village economies," *The Quarterly Journal of Economics*, 2017, *132* (2), 811–870.

Banerjee, Abhijit V, Paul J Gertler, and Maitreesh Ghatak, "Empowerment and efficiency: tenancy reform in West Bengal," *Journal of political economy*, 2002.

Bardhan, Pranab K., "Labor-Tying in a Poor Agrarian Economy: A Theoretical and Empirical Analysis," *The Quarterly Journal of Economics*, 1983, *98* (3), 501–514.

Basu, Arnab K., Nancy H. Chau, and Ravi Kanbur, "A Theory of Employment Guarantees: Contestability, Credibility and Distributional Concerns," *Journal of Public Economics*, April 2009, *93* (3-4), 482–497.

Bazzi, Samuel, "Wealth heterogeneity and the income elasticity of migration," *American Economic Journal: Applied Economics*, 2017, *9* (2), 219–255.

Beegle, Kathleen, Emanuela Galasso, and Jessica Goldberg, "Direct and Indirect Effects of Malawi's Public Works Program on Food Security," Technical Report, University of Maryland 2015.

Berg, Erlend, Sambit Bhattacharyya, Rajasekhar Durgam, and Manjula Ramachandra, "Can Rural Public Works Affect Agricultural Wages? Evidence from India," CSAE Working Paper Series 2012-05, Centre for the Study of African Economies, University of Oxford 2012.

Besley, Timothy and Stephen Coate, "Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-Alleviation Programs," *The American Economic Review*, 1992, *82* (1), 249–261.

Bhalla, Surjit, "The Unimportance of NREGA," *The Indian Express*, July 24 2013.

Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak, "Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh," *Econometrica*, 2014, *82* (5), 1671–1748.

Chaudhury, Nazmul, Jeffrey Hammer, Michael Kremer, Karthik Muralidharan, and F Halsey Rogers, "Missing in action: teacher and health worker absence in developing countries," *The Journal of Economic Perspectives*, 2006, *20* (1), 91–116.

Chowdhury, Anirvan, "Poverty Alleviation or Political Calculation? Implementing Indiaís Rural Employment Guarantee Scheme," Technical Report, Georgetown University 2014.

Cunha, Jesse, Giacomo DeGiorgi, and Seema Jayachandran, "The Price Effects of Cash Versus In-Kind Transfers," Technical Report, Northwestern University July 2017.

Dasgupta, Aditya, Kishore Gawande, and Devesh Kapur, "(When) Do Anti-poverty Programs Reduce Violence? Indiaís Rural Employment Guarantee andMaoist Conflict," Technical Report, Harvard University 2015.

Deaton, Angus and Alessandro Tarozzi, "Prices and poverty in India," 2000.

Dinkelman, Taryn and Vimal Ranchhod, "Evidence on the impact of minimum wage laws in an informal sector: Domestic workers in South Africa," *Journal of Development Economics*, 2012, *99* (1), 27 – 45.

Dreze, Jean and Amartya Sen, *Hunger and Public Action* number 9780198283652. In 'OUP Catalogue.', Oxford University Press, 1991.

Dutta, Puja, Rinku Murgai, Martin Ravallion, and Dominique van de Walle, "Does India's Employment Guarantee Scheme Guarantee Employment?," Policy Research Working Paper Series 6003, World Bank 2012.

Government of India, Ministry of Finance, "Economic Survey 2016-2017," Technical Report, http://indiabudget.nic.in/survey.asp January 2017.

Griffin, Keith, *The political economy of agrarian change: An essay on the Green Revolution.*, Springer, 1979.

Gupta, S., "Were District Choices for NFFWP Appropriate?," *Journal of Indian School of Political Economy*, 2006, *18* (4), 641–648.

Imbert, Clement and John Papp, "Estimating leakages in Indiaís employment guarantee," in Reetika Khera, ed., *The Battle for Employment Guarantee*, Oxford University Press, 2011.

_ **and** _ , "Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee," *American Economic Journal: Applied Economics*, 2015, *7* (2), 233–263.

Islam, Mahnaz and Anita Sivasankaran, "How does Child Labor respond to changes in Adult Work Opportunities? Evidence from NREGA," Technical Report, Harvard University 2015.

Jayachandran, Seema, "Selling Labor Low: Wage Responses to Productivity Shocks in Developing Countries," *Journal of Political Economy*, 2006, *114* (3), pp. 538–575.

Jenkins, R and J Manor, *Politics and the Right to Work: India's Mahatma Gandhi National Rural Employment Guarantee Act*, New Delhi: Orient BlackSwan/Hurst/Oxford University Press USA, 2017.

Kaur, Supreet, "Nominal wage rigidity in village labor markets," NBER Working Paper Series 20770, National Bureau of Economic Research, Inc 2015.

Khanna, Gaurav and Laura Zimmerman, "Guns and Butter? Fighting Violence with the Promise of Development," Technical Report, University of Michigan 2014.

Khera, Reetika, *The Battle for Employment Guarantee*, Oxford University Press, 2011.

Kramer, Howard, "The Complete Pilgrim," `http://thecompletepilgrim.com/bara-imambara/` 2015.

Krugman, Paul, "Increasing Returns and Economic Geography," *Journal of Political Economy*, June 1991, *99* (3), 483–99.

Magruder, Jeremy R., "Can minimum wages cause a big push? Evidence from Indonesia," *Journal of Development Economics*, 2013, *100* (1), 48 – 62.

Mani, Shubha, Jere Behrman, Shaikh Ghalab, and Prudhvikar Reddy, "Impact of the NREGS on Schooling and Intellectual Human Capital," Technical Report, University of Pennsylvania 2014.

Manning, Alan, "Imperfect competition in the labor market," *Handbook of labor economics*, 2011, *4*, 973– 1041.

— **and Barbara Petrongolo**, "How Local Are Labor Markets? Evidence from a Spatial Job Search Model," Technical Report, American Economic Review forthcoming.

Mehrotra, Santosh, "NREG Two Years on: Where Do We Go from Here?," *Economic and Political Weekly*, 2008, *43* (31).

Merfeld, Josh, "Spatially Heterogeneous Effects of a Public Works Program," Working Paper, University of Washington 2017.

Miguel, Edward and Michael Kremer, "Worms: identifying impacts on education and health in the presence of treatment externalities," *Econometrica*, 2004, *72* (1), 159–217.

Mukherjee, Anindita and Debraj Ray, "Labor tying," *Journal of Development Economics*, 1995, *47* (2), 207 – 239.

Mukhopadhyay, Piali, Karthik Muralidharan, Paul Niehaus, and Sandip Sukhtankar, "Implementing a Biometric Payment System: The Andhra Pradesh Experience," Technical Report, University of California, San Diego 2013.

Muralidharan, Karthik and Paul Niehaus, "Experimentation at Scale," *Journal of Economic Perspectives*, forthcoming.

— **, Jishnu Das, Alaka Holla, and Aakash Mohpal**, "The fiscal cost of weak governance: Evidence from teacher absence in India," *Journal of Public Economics*, January 2017, *145*, 116–135.

— **, Paul Niehaus, and Sandip Sukhtankar**, "Building State Capacity: Evidence from Biometric Smartcards in India," *American Economic Review*, 2016, *106* (10), 2895–2929.

Murgai, Rinku and Martin Ravallion, "Is a guaranteed living wage a good anti-poverty policy?," Policy Research Working Paper Series 3640, The World Bank June 2005.

Murphy, Kevin M, Andrei Shleifer, and Robert W Vishny, "Why Is Rent-Seeking So Costly to Growth?," *American Economic Review*, May 1993, *83* (2), 409–14.

Naidu, Suresh, Yaw Nyarko, and Shing-Yi Wang, "Monopsony power in migrant labor markets: evidence from the United Arab Emirates," *Journal of Political Economy*, 2016, *124* (6), 1735 – 1792.

Narayanan, Sudha, "MNREGA and its assets," `http://www.ideasforindia.in/article.aspx?article_id=1596` March 2016.

Niehaus, Paul and Sandip Sukhtankar, "Corruption Dynamics: The Golden Goose Effect," *American Economic Journal: Economic Policy*, 2013, *5.*

— **and** — , "The Marginal Rate of Corruption in Public Programs: Evidence from India," *Journal of Public Economics*, 2013, *104*, 52 – 64.

of India, Planning Commission Government, "Press Notes on Poverty Estimates, 2011-12," Technical Report 2013.

Olken, Benjamin A., "Monitoring Corruption: Evidence from a Field Experiment in Indonesia," *Journal of Political Economy*, April 2007, *115* (2), 200–249.

Pai, Sandeep, "Delayed NREGA payments drive workers to suicide," *Hindustan Times*, December 29 2013.

Ravallion, Martin, "Market Responses to Anti-Hunger Policies: Effects on Wages, Prices, and Employment," Technical Report November 1987. World Institute for Development Economics Research WP28.

Reinikka, Ritva and Jakob Svensson, "Local Capture: Evidence From a Central Government Transfer Program in Uganda," *The Quarterly Journal of Economics*, May 2004, *119* (2), 678–704.

Rosenzweig, Mark R., "Rural Wages, Labor Supply, and Land Reform: A Theoretical and Empirical Analysis," *The American Economic Review*, 1978, *68* (5), 847–861.

Rosenzweig, Mark R and Kenneth I Wolpin, "Credit market constraints, consumption smoothing, and the accumulation of durable production assets in low-income countries: Investments in bullocks in India," *Journal of Political Economy*, 1993, *101* (2), 223–244.

Santangelo, Gabriella, "Firms and farms: The impact of agricultural productivity on the local indian economy," Working Paper, University of Cambridge 2016.

Shah, Manisha and Bryce Millett Steinberg, "Workfare and Human Capital Investment: Evidence from India," Technical Report, University of California, Los Angeles 2015.

Subbarao, K, "Systemic Shocks and Social Protection: The Role of Public Works Programs," Technical Report, The World Bank Group 2003. Social Protection Discussion Paper Series No. 302.

Sukhtankar, Sandip, "India's National Rural Employment Guarantee Scheme: What Do We Really Know about the World's Largest Workfare Program?," *India Policy Forum*, 2017.

Witsoe, Jeffrey, "The Practice of Development: An Ethnographic Examination of the National Rural Employment Guarantee Act in Bihar," Mimeo, Union College 2014.

World Bank, "World Development Report 2004: Making Services Work for Poor People," Technical Report, World Bank 2003.

_ , "Social protection for a changing India," Technical Report, World Bank 2011.

Zimmermann, Laura, "Why Guarantee Employment? Evidence from a Large Indian Public-Works Program," Working Paper, University of Georgia April 2015.

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***	-.039***	.026**	.025**	.014**	.012**	-.041***	-.000088***
	(.014)	(.014)	(.011)	(.011)	(.0065)	(.0061)	(.014)	(.000018)
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**	8761**	914	3276**	3270**	2166	-642	528
	(3723)	(3722)	(588)	(1467)	(1305)	(2302)	(1325)	(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 using annualized data from our survey. "BL GP Mean" is the GP mean of the dependent variable at baseline. "Total income" is total annualized HH income (in Rs.). "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. "Other" is the sum of HH income not captured by the other categories. We censor observations that are in the top .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 the dependent variable. 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Wages

	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.6*	7.8**	4.9*	5.5*
	(3.6)	(3.6)	(2.9)	(2.8)
BL GP Mean		.15***		.12***
		(.053)		(.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. The outcome "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. The outcome "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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Employment

	Days worked on NREGS		Days worked private sector		Days unpaid/idle	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.95	.88	.44	.53	-1.2**	-1.2**
	(.66)	(.64)	(.57)	(.56)	(.59)	(.59)
BL GP Mean		.14***		.22***		.16***
		(.043)		(.068)		(.052)
Adj. R-squared	0.09	0.10	0.01	0.02	0.06	0.07
Control Mean	8.2	8.2	7.9	7.9	17	17
N	10504	10431	14514	14429	14163	14078

This table analyzes labor supply outcomes for June using survey data. "Days worked on NREGS" is the number of days an individual worked on NREGS during 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. "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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Note that observation count varies between columns due to differences in non-response rates between 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	.0048	-.011
	(.066)	(.025)	(.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

In this table, we use National Sample Survey (NSS) data on household consumption and prices to test for impacts on price levels. Columns 1 and 2 show analysis of a village-level price index constructed from NSS data. The outcome variable is the log of the price index. Column 3 shows analysis on observed commodity prices at the household level. The outcome is the log price. Standard errors clustered at the mandal level are in parentheses. All regressions include district fixed effects and all regressions using survey data also include the first principal component of a vector of mandal characteristics used to stratify randomization. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Savings, assets and loans

	Total savings (Rs.)		Total loans (Rs.)		Owns land (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1064	1120	11210**	11077**	.056**	.049**
	(859)	(877)	(4741)	(4801)	(.024)	(.024)
BL GP Mean		.027		.038		.21***
		(.071)		(.039)		(.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 analyzes household assets using survey data. "Total savings (Rs.)" 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.)" is the total principal of the household's five largest active loans. "Owns land (%)" 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Livestock

	Cattle	Buffaloes	Sheep	Goats	Pigs	Dogs	Fowls	Ducks	Backyard Poultry
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment	1261	4671***	-35	2473	120	244*	8150	209	8381*
	(2463)	(1776)	(4593)	(1877)	(116)	(132)	(4961)	(286)	(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 analyzes impact 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Testing for existence of spatial spillovers

(a) Control

	Wage realization (Rs.)					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
% GPs treated within R km	7.8	17**	22**	21**	24**	-.76	-1.5	-1.7	-3.4*	-5.4**
	(6.6)	(6.9)	(8.6)	(8.6)	(11)	(1.2)	(1.4)	(1.8)	(2)	(2.2)
Adj. R-squared	.06	.05	.05	.05	.05	.08	.07	.07	.07	.07
Mean	127	128	128	128	128	17	17	17	17	17
N	1850	2057	2063	2063	2063	3701	4076	4095	4095	4095
% of sample	90	100	100	100	100	90	100	100	100	100

(b) Treatment

	Wage realization (Rs.)					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
% GPs treated within R km	12***	11*	14*	14	15	-1.1	-2.3**	-3.1**	-3.5**	-4.3**
	(4.4)	(5.9)	(7.5)	(10)	(12)	(.81)	(1.1)	(1.3)	(1.5)	(1.8)
Adj. R-squared	.08	.08	.08	.08	.08	.07	.07	.07	.07	.07
Mean	133	134	134	134	134	16	16	16	16	16
N	4710	4992	5129	5182	5206	9021	9613	9882	9969	10000
% of sample	90	96	99	100	100	90	96	99	100	100

(c) Test for Equality

	Wage realization (Rs.)					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
F Statistic	0.32	0.43	0.54	0.27	0.29	0.07	0.22	0.38	0.00	0.15
p-value	0.57	0.51	0.46	0.60	0.59	0.80	0.64	0.54	0.97	0.70

(d) Pooled

	Wage realization (Rs.)					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
% GPs treated within R km	9.9***	12**	13**	13*	16*	-1.1	-2**	-2.5**	-3.3**	-4.2***
	(3.6)	(4.8)	(6.2)	(7.8)	(9.5)	(.72)	(.91)	(1.1)	(1.3)	(1.5)
Treatment	8.5**	8.3**	8**	7.4**	7.3**	-1.5**	-1.5**	-1.5**	-1.5***	-1.5***
	(3.5)	(3.5)	(3.5)	(3.6)	(3.6)	(.61)	(.58)	(.58)	(.58)	(.57)
Adj. R-squared	.07	.07	.07	.07	.07	.07	.07	.07	.07	.07
Control Mean	127	128	128	128	128	17	17	17	17	17
N	6560	7049	7192	7245	7269	12722	13689	13977	14064	14095
% of sample	90	97	99	100	100	90	97	99	100	100

This table shows the impact of $\tilde{N}_p^R$ on private wages and days unpaid/idle using survey data. Analysis was conducted separately for (a) control and (b) treatment, and (d) the pooled sample. In panel (c), we conduct an adjusted Wald test of equality between treatment and control estimates. In each panel, the outcome "Wage realization (Rs.)" is the average daily wage (Rs. = Rupees) an individual received while working for someone else in June 2012 (endline). "Days unpaid/idle" is the sum of days an individual did unpaid work or stayed idle in June 2012 (endline). "% 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 entire GP sample used in randomization is included. 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Note that the variation in observation counts is due to the construction of the spatial exposure measure.

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

(a) Wage

	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***	23***	24***	22**	26**	8.5**	9.8**	9.9*	8.7	7.8
	(5.6)	(6.6)	(7.8)	(9.1)	(11)	(4)	(4.6)	(5.4)	(6.5)	(7.4)
Unadjusted treatment effect	7.6**	7.1**	7*	6.6*	6.5*	4.3	5*	5.1*	4.9*	4.9*
	(3.5)	(3.6)	(3.6)	(3.6)	(3.6)	(3)	(2.9)	(2.9)	(2.9)	(2.9)
Difference	11	15**	17*	16	19*	4.2	4.8	4.7	3.7	3
	(6.6)	(7.5)	(8.6)	(9.8)	(11)	(5)	(5.4)	(6.1)	(7.1)	(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) Labor

	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	2.3*	2.8*	3.5**	4.4**	-2.6**	-3.7***	-4.3***	-5.1***	-6.3***
	(1.1)	(1.3)	(1.5)	(1.7)	(1.9)	(1)	(1.3)	(1.5)	(1.6)	(1.8)
Unadjusted treatment effect	.47	.45	.44	.45	.45	-1.4**	-1.3**	-1.2**	-1.3**	-1.3**
	(.59)	(.57)	(.57)	(.57)	(.57)	(.63)	(.59)	(.59)	(.59)	(.59)
Difference	1.1	1.9	2.4	3.1*	3.9**	-1.2	-2.5*	-3*	-3.9**	-5***
	(1.2)	(1.4)	(1.6)	(1.8)	(2)	(1.2)	(1.4)	(1.6)	(1.7)	(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

(c) Total income

	Total income				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Total treatment effect	11947*	13338*	14362	13931	13921
	(6158)	(7310)	(8925)	(10033)	(11272)
Unadjusted treatment effect	8896**	9456**	9581**	9621***	9618***
	(4084)	(3763)	(3726)	(3717)	(3712)
Difference	3051	3882	4781	4310	4304
	(7389)	(8222)	(9671)	(10700)	(11867)
Chi-square statistic	.17	.22	.24	.16	.13
Control Mean	68943	69255	69122	69122	69122
N	4401	4745	4840	4868	4879

In this table, we test for the equality of total treatment effect and unadjusted treatment effect estimates. Using equation-by-equation generalized method of moments estimation, we estimate our specifications for unadjusted and total treatment effects on the same analysis sample (which matches the analysis sample of Table A.20). We report the individual estimates for the unadjusted treatment effects and total treatment effects on the analysis sample. Note that the estimates for the unadjusted treatment effect differ from those from Tables 1b, 2, and 3 as the analysis sample only includes observations where the spatial exposure measures are defined. We test for equality between the unadjusted and total treatment effect estimates, with the statistical significance of this test and the numerical difference between the two estimates reported in the row marked "Difference". Standard errors clustered at the mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

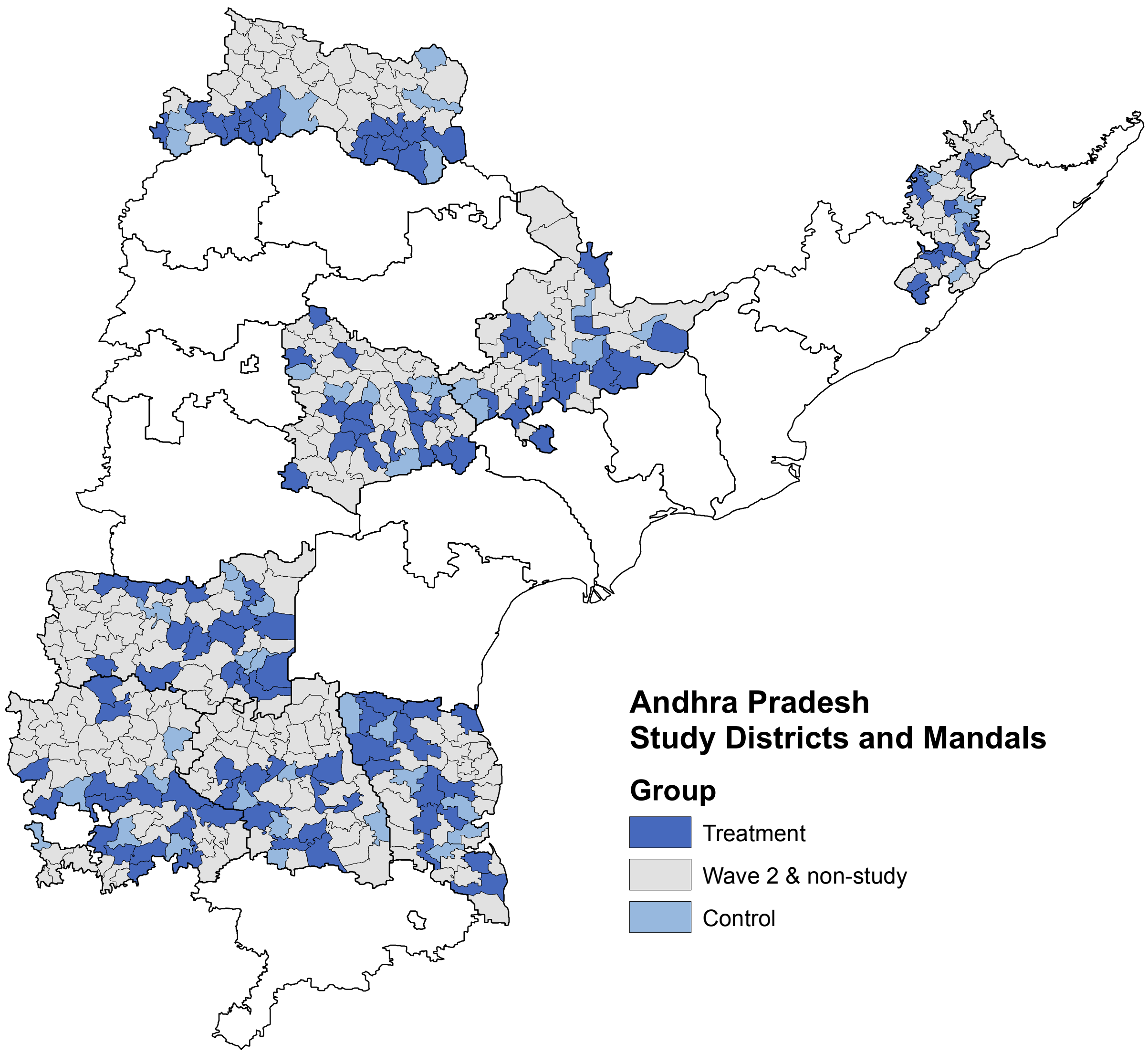
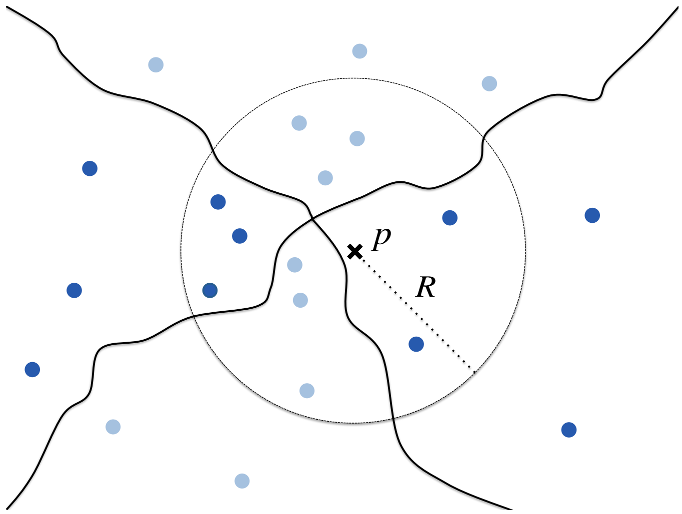


Figure 1: Study districts with treatment and control mandals

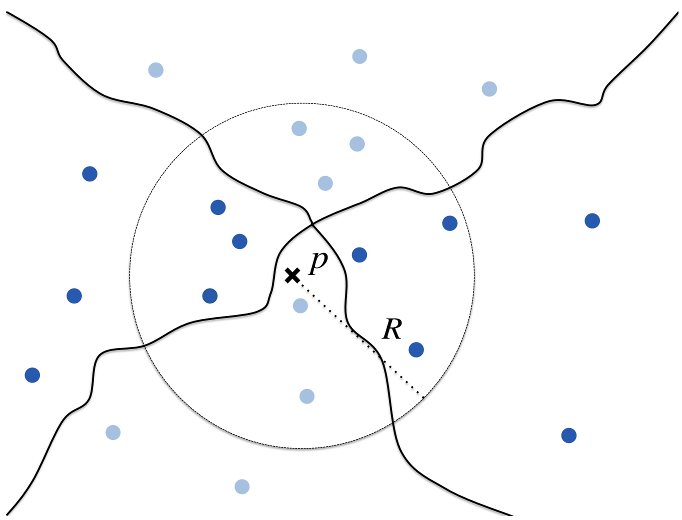
This map (reproduced from Muralidharan et al. (2016)) shows the 8 study districts - Adilabad, Anantapur, Kadapa, Khammam, Kurnool, Nalgonda, Nellore, and Vizianagaram - and the assignment of mandals (sub-districts) within those districts to our study conditions. Mandals were randomly assigned to one 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: Constructing measures of exposure to spatial spillovers

(a) Treatment

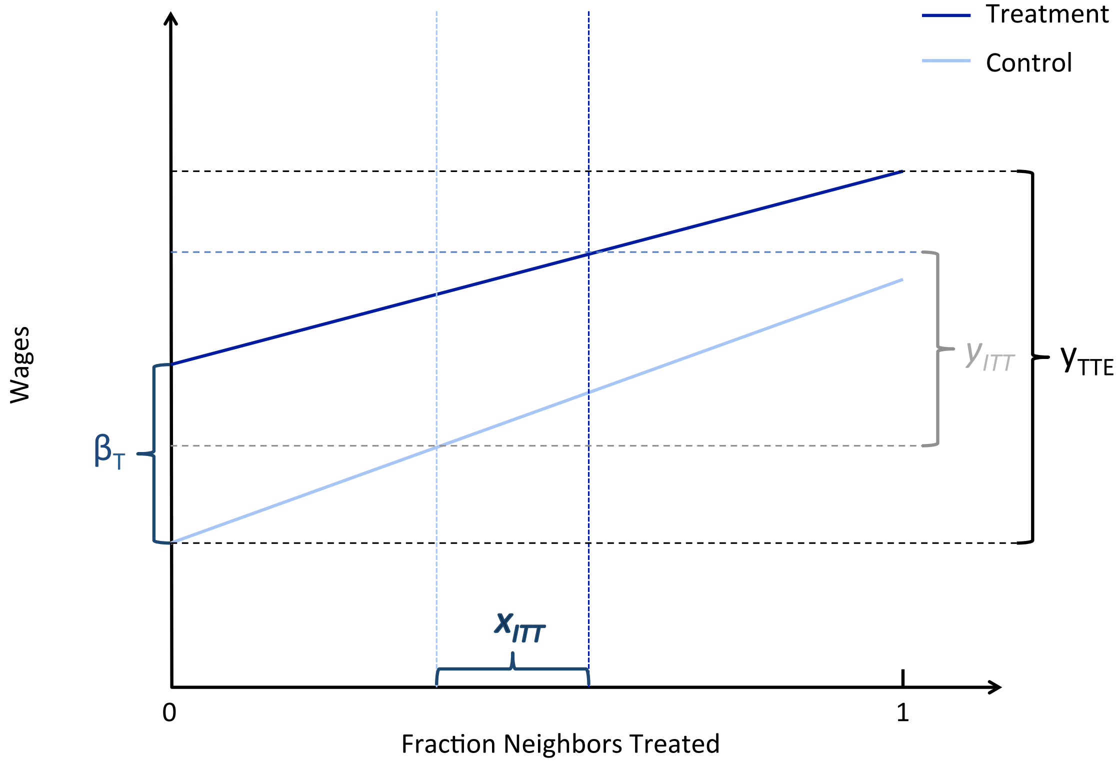


(b) Control



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) a treatment mandal and (b) a control 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 which were assigned to treatment. In Panel (a) these measures are $N_p^R = \frac{5}{11}$ and $\tilde{N}_p^R = \frac{1}{3}$, while in Panel (b) they are $N_p^R = \frac{6}{11}$ and $\tilde{N}_p^R = \frac{2}{3}$.

Figure 3: Conceptual illustration of adjusted treatment effects for spillovers



This figure shows a conceptual illustration of the total treatment effect of the intervention with and without adjusting for spillovers. The solid lines represent the theoretical relationship between the spatial exposure and an outcome, illustrated by wages. Dotted lines represent mean exposure of treatment (dark blue) and control (light blue) groups. The gray bracket range, labelled y_{ITT}, represents the unadjusted treatment effect. The black bracket range, labelled y_{TTE}, represents the total treatment effect. The dark blue bracket range on the x-axis, labelled x_{TTE}, represents the mean difference in the fraction of treated neighbors between treatment and control villages, which is positive but less than 1. The dark blue bracket range on the y-axis, labelled β_T, represents the effect of treatment when spatial exposure is zero.

Appendix

This section provides further background on the NREGS program, including the status quo payments system, as well as the 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. These schemes guarantee 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 (65.7% 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 - mostly during the slack labor seasons of April, May and June - and rarely otherwise.

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.8 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.[34] 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).

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

Table A.1: Heterogeneity in income 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*	5994	11511**	10008**	6634***	4484	8511***	7950***
	(4528)	(5642)	(4708)	(3897)	(2094)	(3479)	(2213)	(2067)
Treatment X Covariate	4821	5247	-10863	-3917	143	2509	-10606***	-10767**
	(6650)	(6202)	(8841)	(8063)	(3048)	(3337)	(3397)	(4186)
Covariate	-11703**	30106***	-39118***	-20355***	6825***	3287	-13060***	727
	(5467)	(4927)	(6707)	(6577)	(2394)	(2586)	(2711)	(3287)
Treatment + Treatment X Covariate	12676	11241	649	6091	6777	6993	-2095	-2817
Standard error	(5460)	(4120)	(6407)	(8198)	(3134)	(2040)	(3132)	(4252)
p-value	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 gains in total annualized income and labor income using our survey data. The outcome "Total income (Rs.)" is total annualized HH income. The outcome "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 Caste/Tribe. "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 constructed by multiplying the respective variable with 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.2: 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. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Indian Constitution. "Working" is defined as participating in any economically productive activity with or without compensation, wages or profit. "Main" workers are defined as those who engaged in any economically productive work for more than 183 days in a year. "Marginal" workers are those for whom the period they engaged in economically productive work does not exceed 182 days. Note that the difference in "main" and "marginal" workers only stems for different periods of work. An "agricultural laborer" is a person who works for compensation on another person's land (compensation can be paid in money, kind or share). The definitions are from the official census documentation. The second set of variables is taken from 2001 census village directory which records information about various facilities within a census village (the census level of observation). "# primary schools per village" and "Avg. village size in acres" are simple district averages - while the others are simple percentages - of the respective variable (sampling weights are not needed since all villages within a district are used). Note that we did not have this information available for the 2011 census and hence use the 2001 data. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: 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, i.e. the district's capital) is excluded since it is fully urban (hence has no NREGS). Column 3 and 4 report the point estimate and the respective p-value associated with entering the randomization pool from a simple regression of the outcome and the respective indicator variable. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Indian Constitution. "Working" is defined as the participating in any economically productive activity with or without compensation, wages or profit. "Main" workers are defined as those who engaged in any economically productive work for more than 183 days in a year. "Marginal" workers are those for whom the period they engaged in economically productive work does not exceed 182 days. Note that the difference in "main" and "marginal" workers only stems for different periods of work. An "agricultural laborer" is a person who works for compensation on another person's land (compensation can be paid in money, kind or share). The definitions are from the official census documentation. The second set of variables is taken from 2001 census village directory which records information about various facilities within a census village (the census level of observation). "# primary schools per village" and "Avg. village size in acres" are simple district averages - while the others are simple percentages - of the respective variable (sampling weights are not needed since all villages within a district are used). Note that we did not have this information available for the 2011 census and hence use the 2001 data. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: 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 regressions 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.5: 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
Owns 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
Owns 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 regressions 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: 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 estimated using NSS Round 66 data (collected in 2009-10, prior to the Smartcards intervention). Column 3 reports the estimate of a indicator of whether household owns a NREGS jobcard from simple regressions of the outcome with district fixed effects as the only controls. Column 4 reports the p-value for this estimate.

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	7418
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 > 0	.52	.49	.028	.2	14514
Wage realization ≥ 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	7101
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 > 0	.54	.52	.016	.44	13210
Wage realization ≥ 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 ought to have answered the question. "Wage realization (Rs.)" is the average daily wage (Rs. = Rupees) 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 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 ≥ 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: 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	8.4	5.8	6.5	1.4**	1.3*	.92	1	-1.7**	-1.6**
	(5.2)	(5.2)	(4)	(4)	(.69)	(.66)	(.65)	(.65)	(.68)	(.67)
Treatment X Female	-1	-3.1	-1.6	-1.9	-.81*	-.8*	-.96*	-.92	.97*	.92
	(5.3)	(5.3)	(3.4)	(3.5)	(.42)	(.42)	(.56)	(.56)	(.57)	(.59)
Female	-60***	-59***	-37***	-36***	1.3***	1.3***	-1.6***	-1.7***	1.1**	1.1**
	(4.6)	(4.6)	(2.8)	(2.9)	(.33)	(.34)	(.49)	(.49)	(.51)	(.52)
BL GP Mean		.15***		.11***		.14***		.22***		.16***
		(.048)		(.043)		(.043)		(.067)		(.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 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". The outcome "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. The outcome "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 regressions. The outcome "Days worked on NREGS" in columns 5-6 is the number of days an individual worked on NREGS during June 2012. The outcome "Days worked private sector" in columns 7-8 is the number of days an individual worked for somebody else in June 2012. The outcome "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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: Migration

	Did migrate?		Days migrated		Household size		Migration common in May?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	.024	.022	1.1	.75	.059	.054	.047	.049
	(.017)	(.018)	(4.9)	(5.1)	(.1)	(.1)	(.055)	(.038)
BL GP Mean		.093		.3		.044		
		(.09)		(.19)		(.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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Expenditure

	Short-term Expenditure (Rs. per month)		Longer-term expenditure (Rs. per year)		Monthly Per Capita Expenditure
	(1)	(2)	(3)	(4)	(5)
Treatment	-108	-428	-24	-646	71
	(1029)	(1033)	(3239)	(3227)	(122)
BL GP Mean		.051**		-.003	
		(.02)		(.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 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. The outcome in column 5 “Monthly Per Capita Expenditure” (MPCE) is measured at the household-level in the NSS data; the variable includes household expenditure as well is 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: 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***	-.039***	.025**	.024**	.013**	.012**	-.04***	-.00007***
	(.014)	(.014)	(.011)	(.011)	(.0065)	(.0061)	(.014)	(.000015)
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***	-.04***	.027**	.026**	.015**	.014*	.056***	.054***
	(.015)	(.015)	(.012)	(.012)	(.0072)	(.0071)	(.02)	(.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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: 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**	9580**	905	3675**	4471***	1738	-773	293
	(4638)	(4628)	(589)	(1485)	(1585)	(2704)	(1359)	(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 a robustness check for Table 1b - which shows treatment effects on various types of income using annualized data from our survey - by 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.13: Robustness checks for private sector wage outcomes

(a) Includes Wage Outliers

	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) Restricts 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) Excludes respondents who did not work in June

	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.4*	7.6**	4.7	5.4*
	(3.6)	(3.6)	(2.9)	(2.8)
BL GP Mean		.16***		.1***
		(.048)		(.033)
Adjusted R-squared	0.07	0.07	0.05	0.05
Control Mean	128	128	97	97
N	7256	7043	12859	12745

In this table, we perform robustness checks for Table 2. In Panel a), the analysis sample includes observations in the top .5% percentile of the respective wage outcome in treatment and control. 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.14: Predictors of differential non-response and response composition

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

This table analyzes 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 in 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.15: Baseline balance in 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	4.2	1.1	-1.3	-1.2
	(6.8)	(8.3)	(9.3)	(10)	(12)
Reservation wage (Rs.)	-3.2	-.47	-1.8	-1.2	1
	(5.1)	(6.4)	(6.7)	(7)	(7.4)
Days worked private sector	-.68	-.66	-.69	-.42	-.32
	(.64)	(.7)	(.73)	(.87)	(.98)
Days unpaid/idle	.14	.31	.0017	-.45	-.86
	(1)	(1.3)	(1.6)	(2)	(2.3)
Total income (Rs.)	5133	4115	3683	-917	-3137
	(4572)	(5022)	(5992)	(6638)	(7699)

In this table we analyze 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. Note that each cell shows a separate regression of the outcome with the "% GPs treated within R" and district fixed effects as the only covariates. Finally, note that each column reports results from 5 different regressions and there is no single number of observations. Standard errors clustered at the mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.16: Baseline balance in 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	4.2	2	.21	1.3
	(4.1)	(5.5)	(6.6)	(7.8)	(9.6)
Reservation wage (Rs.)	1.4	3.1	2.4	2.7	5.6
	(3.3)	(4.1)	(4.8)	(5.1)	(6.1)
Days worked private sector	-.27	-.46	-.42	-.21	.026
	(.42)	(.55)	(.64)	(.76)	(.89)
Days unpaid/idle	.34	.44	.19	-.3	-.54
	(.53)	(.68)	(.95)	(1.5)	(1.7)
Total income (Rs.)	-377	2653	2177	-2032	-3232
	(2872)	(4168)	(4826)	(5421)	(6738)

In this table we analyze 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. Finally, note that each column reports results from 5 different regressions and there is no single number of observations. Standard errors clustered at the mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.17: Testing for existence of spatial spillovers (comprehensive neighborhoods)

(a) Control

	Wage realization (Rs.)					Days worked private sector				
	(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
Fraction GPs treated within R km	14	18	24**	24*	26	.44	.71	1.1	2.6	4.6
	(16)	(15)	(11)	(13)	(16)	(2)	(2.1)	(2.6)	(2.6)	(2.9)
Adjusted R-squared	0.05	0.05	0.05	0.05	0.05	0.02	0.02	0.02	0.02	0.02
Mean	128	128	128	128	128	7.9	7.9	7.9	7.9	7.9
N	2063	2063	2063	2063	2063	4253	4253	4253	4253	4253

(b) Treatment

	Wage realization (Rs.)					Days worked private sector				
	(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
Percent GPs treated within R km	11	15	11	8.3	6.6	.62	.61	1.4	2.1	2.9
	(8.5)	(9.9)	(12)	(13)	(14)	(1.2)	(1.4)	(1.7)	(2)	(2.3)
Adjusted R-squared	0.08	0.08	0.08	0.08	0.08	0.02	0.02	0.02	0.02	0.02
Mean	134	134	134	134	134	8.1	8.1	8.1	8.1	8.1
N	5206	5206	5206	5206	5206	10188	10188	10188	10188	10188

(c) Test for Equality

	Wage realization (Rs.)					Days unpaid/idle				
	R = 10	R = 15	R = 20	R = 25	R = 30	R = 10	R = 15	R = 20	R = 25	R = 30
F Statistic	0.02	0.04	0.63	0.74	0.91	0.85	0.35	0.35	0.02	0.13
p-value	0.88	0.85	0.43	0.39	0.34	0.36	0.56	0.55	0.89	0.72

(d) Pooled

	Wage realization (Rs.)					Days worked private sector				
	(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
Fraction GPs treated within R km	9.5	12	10	9.1	10	.61	.83	1.5	2.2	3.2*
	(7.4)	(8.2)	(9)	(9.8)	(11)	(1.1)	(1.3)	(1.5)	(1.7)	(1.9)
Treatment	1.2	1.9	4	5	5.3	.11	.16	.11	.13	.11
	(5)	(4.4)	(4.2)	(4)	(3.8)	(.79)	(.69)	(.64)	(.59)	(.57)
Ad. R-squared	0.07	0.07	0.07	0.07	0.07	0.01	0.01	0.01	0.01	0.01
Control Mean	128	128	128	128	128	7.9	7.9	7.9	7.9	7.9
N	7269	7269	7269	7269	7269	14441	14441	14441	14441	14441

This table show the impact of comprehensive spatial exposure measure to treatment N_p^R on private wages and days unpaid/idle from the NREGS household survey. Analysis was conducted separately for (a) control and (b) treatment, and then for (d) the pooled sample. In panel (c), we conduct an adjusted Wald test of equality between treatment and control estimates. In each panel, the outcome "Wage realization (Rs.)" is the average daily wage (in Rs.) an individual received while working for someone else in June 2012 (endline). The outcome "Days unpaid/idle" is the sum of days an individual did unpaid work or stayed idle in June 2012. The "% GPs treated within R km" is the ratio of the number of GPs in treatment mandals over the total GPs within a given radius R km. 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 are included. 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.18: 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*	14**	15**	15*	17*	9.5***	11**	11*	8.4	5.9
	(5.6)	(6.9)	(7.5)	(8.5)	(9.8)	(3.1)	(4.3)	(5.6)	(6.2)	(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	1.1	1.7	2.5	3.5*	-1.5*	-2.1*	-3**	-4**	-5.2***
	(.8)	(1.1)	(1.4)	(1.6)	(1.8)	(.78)	(1.1)	(1.4)	(1.6)	(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

(c) Total income

	Total income (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Total treatment effect	14047***	16086***	14953**	13962	15344
	(4459)	(5573)	(7175)	(8649)	(9597)
Adjusted R-squared	0.04	0.04	0.04	0.04	0.04
Control Mean	79085	79085	79085	79085	79085
N	4879	4879	4879	4879	4879

This table provides estimates for total treatment effects using the comprehensive spatial exposure measure N_p^R for (a) wage outcomes, (b) labor outcomes, and (c) household income. 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. "Total income" is total annualized household income (in Rs). 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.19: Robustness of total treatment effects (total income) to censoring

(a) Top 1%

	Total income (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Total treatment effect	12642**	17291**	18443**	19237**	21442**
	(6072)	(6879)	(8095)	(8774)	(9328)
F-stat for % GPs treated within R km	333	229	263	370	367
F-stat for % GPs treated within R km X Treatment	227	178	167	184	151
Adj. R-squared	.05	.05	.05	.05	.05
Control Mean	67133	67614	67488	67488	67488
N	4380	4722	4816	4844	4855

(b) Top 0.5%

	Total income (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Total treatment effect	11947*	13338*	14362	13931	13921
	(6158)	(7310)	(8925)	(10033)	(11272)
F-stat for % GPs treated within R km	330	216	253	357	359
F-stat for % GPs treated within R km X Treatment	225	167	160	176	148
Adj. R-squared	.04	.04	.04	.04	.04
Control Mean	68943	69255	69122	69122	69122
N	4401	4745	4840	4868	4879

(c) Top 0.1%

	Total income (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Total treatment effect	14583**	15837*	17114	14748	12985
	(6531)	(8163)	(10549)	(11056)	(11987)
F-stat for % GPs treated within R km	330	216	254	360	361
F-stat for % GPs treated within R km X Treatment	225	166	158	177	150
Adj. R-squared	.03	.03	.03	.03	.03
Control Mean	71426	71780	71636	71636	71636
N	4416	4763	4860	4888	4899

(d) No Censor

	Total income (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30
Total treatment effect	18757***	19872**	22903**	21089*	18164
	(7140)	(8944)	(11560)	(11973)	(12448)
F-stat for % GPs treated within R km	330	216	255	361	361
F-stat for % GPs treated within R km X Treatment	225	166	159	177	150
Adj. R-squared	.03	.03	.03	.03	.03
Control Mean	71756	72080	71935	71935	71935
N	4420	4767	4864	4892	4903

To test to robustness of the earnings results from Table A.19 to censoring, we report results for a range of censoring thresholds. In panels (a) - (c), we restrict our analysis sample by removing observations in the top 1%, 0.5%, 0.1% from treatment and control observations separately. In panel (d), we do not censor any observations. Standard errors clustered at the mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.20: 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	4.2	4.9	5.6	7.6	1.3	6.1	5.4	4.8	2
	(9.3)	(9)	(9.2)	(9.4)	(10)	(6.7)	(6.2)	(6.3)	(6.9)	(7.2)
% GPs treated within R km	13	31*	26	21	27	9.6	12	8.4	5	-.39
	(16)	(17)	(18)	(15)	(17)	(14)	(12)	(13)	(12)	(11)
% GPs treated within R km X Treatment	16	-13	-7.2	-4.6	-9	-2.5	-8.2	-3.9	-1.1	6.2
	(20)	(21)	(22)	(21)	(23)	(16)	(15)	(16)	(16)	(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	-.27	.037	.27	.48	.33	.79	.68	-.25	-.86
	(1.6)	(1.6)	(1.7)	(1.7)	(1.8)	(1.6)	(1.5)	(1.5)	(1.4)	(1.5)
% GPs treated within R km	3.5	3.1	3.4	4.1	5.1	-2	-2.7	-2.5	-4	-5.8**
	(3.1)	(2.9)	(3)	(3.1)	(3.4)	(3.3)	(3)	(3)	(2.8)	(2.8)
% GPs treated within R km X Treatment	-2.2	-.53	-.64	-.82	-1.2	-.98	-1.8	-2.5	-.86	.48
	(3.6)	(3.6)	(3.8)	(4)	(4.3)	(3.7)	(3.6)	(3.5)	(3.4)	(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

(c) Total income

	Total Income				
	R = 10	R = 15	R = 20	R = 25	R = 30
Treatment	7797	16230	16591	9089	5005
	(10421)	(10445)	(10403)	(10038)	(10323)
% GPs treated within R km	9095	18088	18042	5445	-1219
	(22019)	(19006)	(20651)	(18079)	(15974)
% GPs treated within R km X Treatment	-4945	-20980	-20272	-603	10136
	(25292)	(22972)	(24803)	(23122)	(22528)
Total treatment effect	11947	13338	14362	13931	13921
SE	(6158)	(7310)	(8925)	(10033)	(11272)
F-stat for % GPs treated within R km	330	216	253	357	359
F-stat for % GPs treated within R km X Treatment	225	167	160	176	148
Adj. R-squared	.04	.04	.04	.04	.04
Control Mean	68943	69255	69122	69122	69122
N	4401	4745	4840	4868	4879

This table provides estimates from the total treatment effect specification (equation 5) for (a) wage, (b) labor, and (c) total income outcomes. 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 the interaction between $\tilde{N}_p^R$ and 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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.21: 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	.11	.099	-1	.2*	61	-10	25	-119	161
	(2.9)	(.44)	(.31)	(2.7)	(.12)	(441)	(103)	(247)	(435)	(112)
BL GP Mean	.61***	.23***	.067***	1.3***	.098***	.4***	.068*	.23***	.36***	.11
	(.075)	(.088)	(.021)	(.23)	(.026)	(.039)	(.039)	(.055)	(.063)	(.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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.22: 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	-6.8	-.74	-.83	1.1	.0018
	(5.4)	(6.1)	(1.2)	(1.3)	(1.6)	(.01)
BL GP Mean			.0074	.48***	.49***	.91***
			(.0092)	(.075)	(.046)	(.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. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.23: Testing for existence of spatial spillovers (days worked private sector)

(a) Control

	R = 10	R = 15	R = 20	R = 25	R = 30
	(1)	(2)	(3)	(4)	(5)
Fraction GPs treated within R km	1.2	1.3	1.8	2.9	4.3*
	(1.1)	(1.2)	(1.6)	(2)	(2.3)
Adjusted R-squared	0.02	0.02	0.02	0.02	0.02
Mean	7.8	7.9	7.9	7.9	7.9
N	3840	4234	4253	4253	4253

(b) Treatment

	R = 10	R = 15	R = 20	R = 25	R = 30
	(1)	(2)	(3)	(4)	(5)
Fraction GPs treated within R km	.42	1.3	1.8	2.5	3.3*
	(.8)	(1)	(1.3)	(1.6)	(1.9)
Adjusted R-squared	0.02	0.02	0.02	0.02	0.02
Mean	8.2	8.2	8.2	8.2	8.2
N	9168	9761	10047	10144	10188

(c) Test for Equality

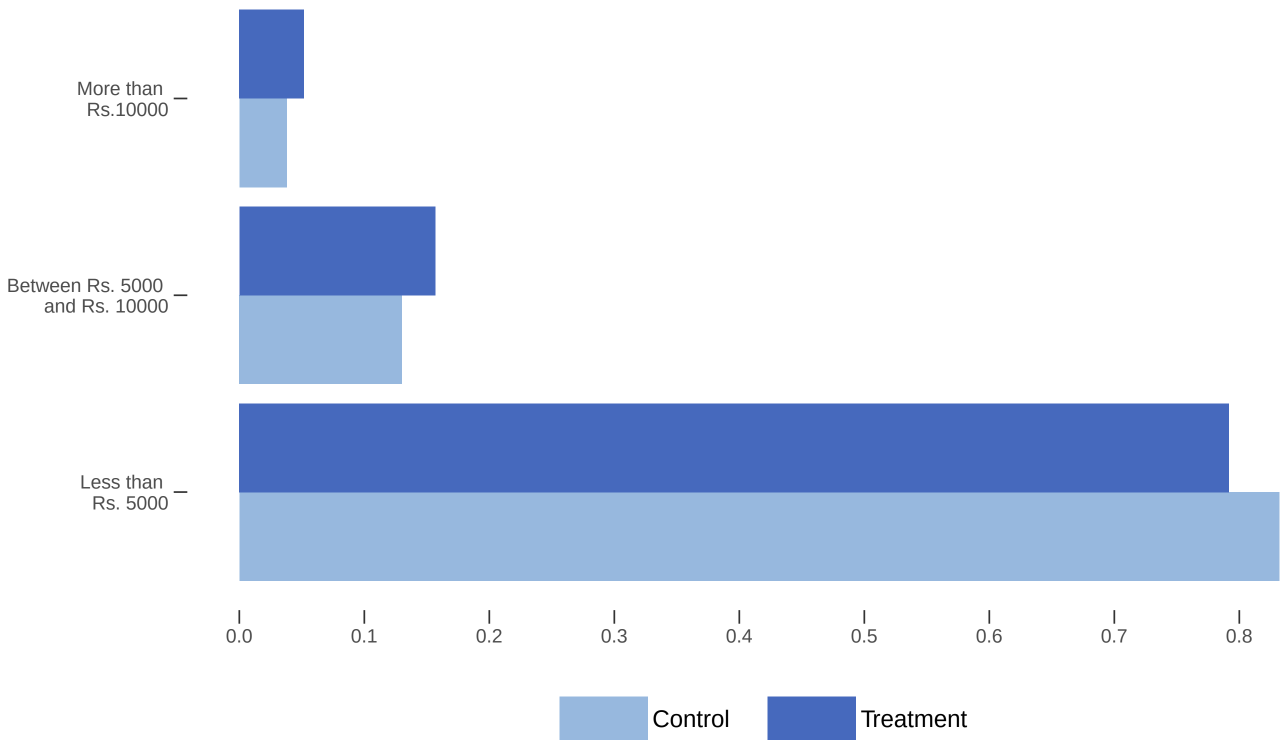
	R = 10	R = 15	R = 20	R = 25	R = 30
	(1)	(2)	(3)	(4)	(5)
F Statistic	0.33	0.00	0.00	0.03	0.13
p-value	0.57	0.98	0.99	0.87	0.72

(d) Pooled

	R = 10	R = 15	R = 20	R = 25	R = 30
	(1)	(2)	(3)	(4)	(5)
Fraction GPs treated within R km	.81	1.4*	1.8*	2.5*	3.3**
	(.71)	(.85)	(1.1)	(1.3)	(1.5)
Treatment	.56	.63	.63	.64	.65
	(.59)	(.58)	(.58)	(.58)	(.57)
Ad. R-squared	0.01	0.01	0.01	0.01	0.02
Control Mean	7.8	7.9	7.9	7.9	7.9
N	13008	13995	14300	14397	14441

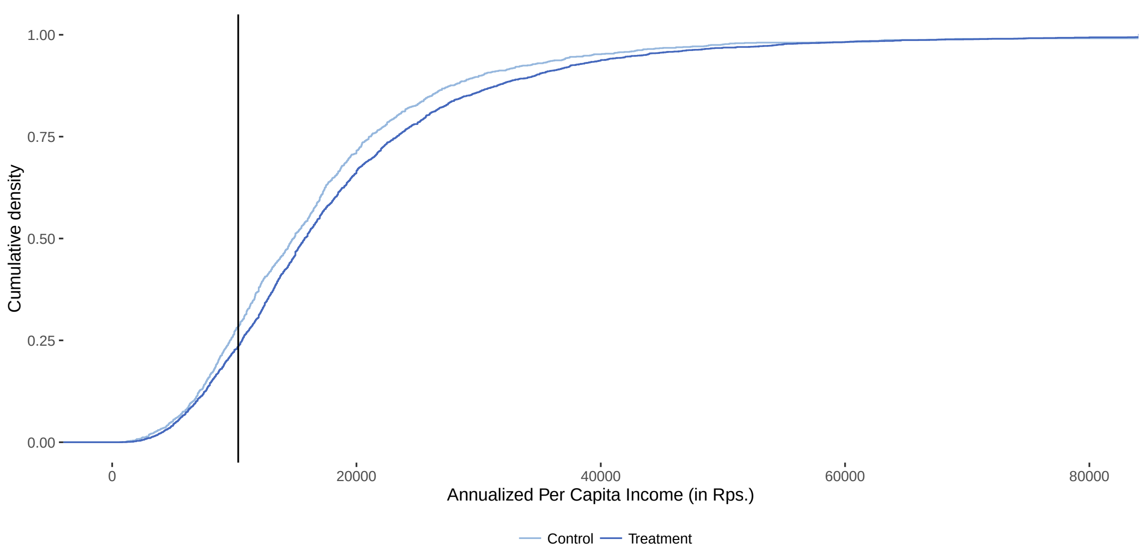
This table show the impact of $\tilde{N}_p^R$ on treatment effects for days worked in the private sector using survey data. Analysis was conducted separately for (a) control and (b) treatment, and (d) the pooled sample. In panel (c), we conduct an adjusted Wald test of equality between treatment and control estimates. In each panel, the outcome is "Days worked private sector", the number of days an individual worked for somebody else in June 2012 (endline). The "% 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 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 entire GP sample used in randomization is included. 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. Statistical significance is denoted as: $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Note that the variation in observation counts is due to the construction of the spatial exposure measure.

Figure A.1: Effects on income: SECC



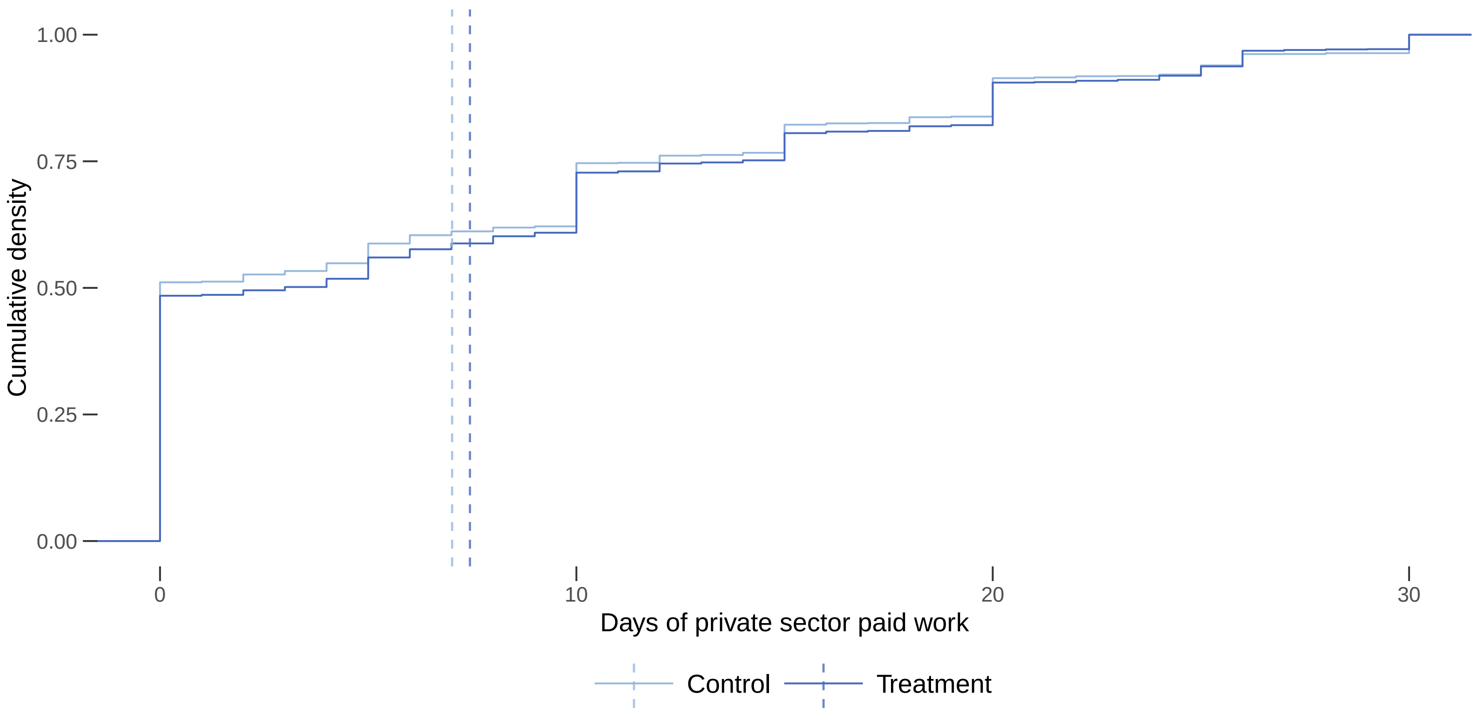
The figure shows the proportions of households in each of the three income brackets in the Socioeconomic and Caste Census (SECC) 2011 (enumeration started in late June 2011) by treatment and control households. The standard error (not included) for every category is < 0.001.

Figure A.2: Annualized per capita income



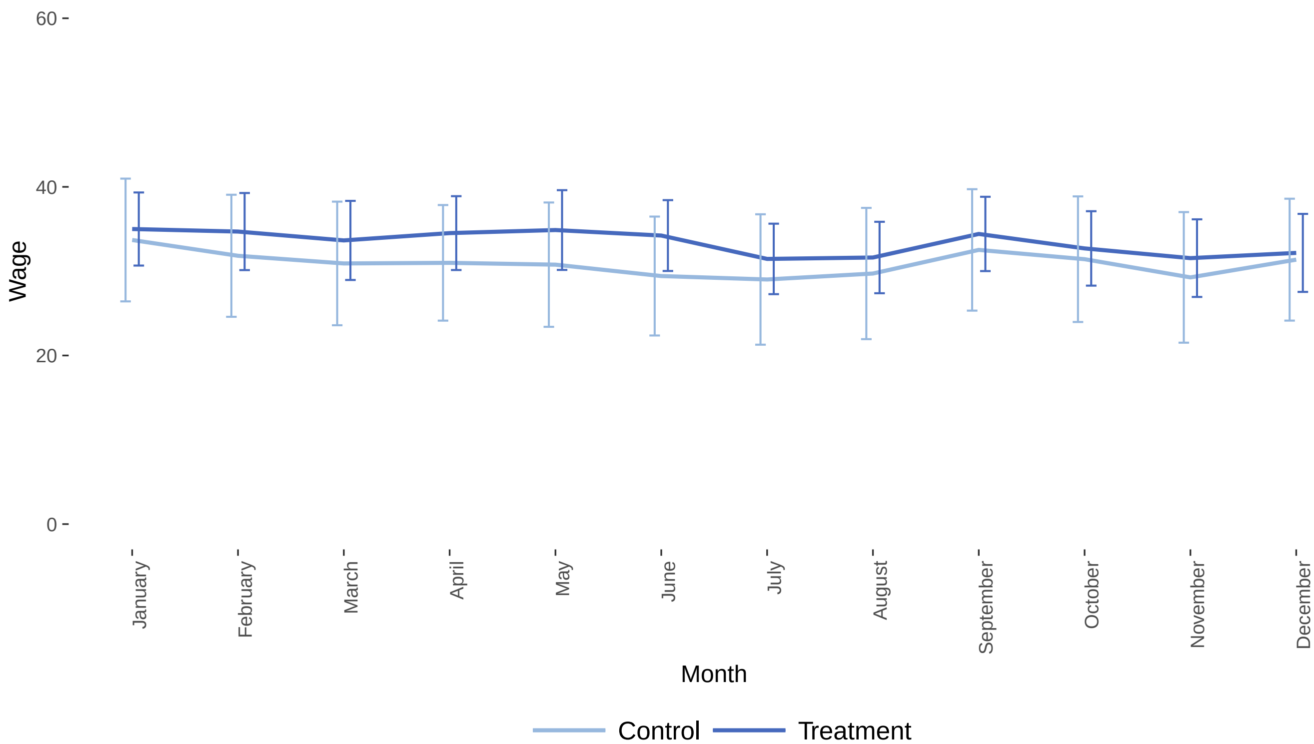
This figure shows an empirical cdf of total annualized per capita income by household for treatment and control groups using data from the endline household survey. Annualized per capita income was calculated by dividing the total annual household income by number of household members. The vertical line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

Figure A.3: 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.

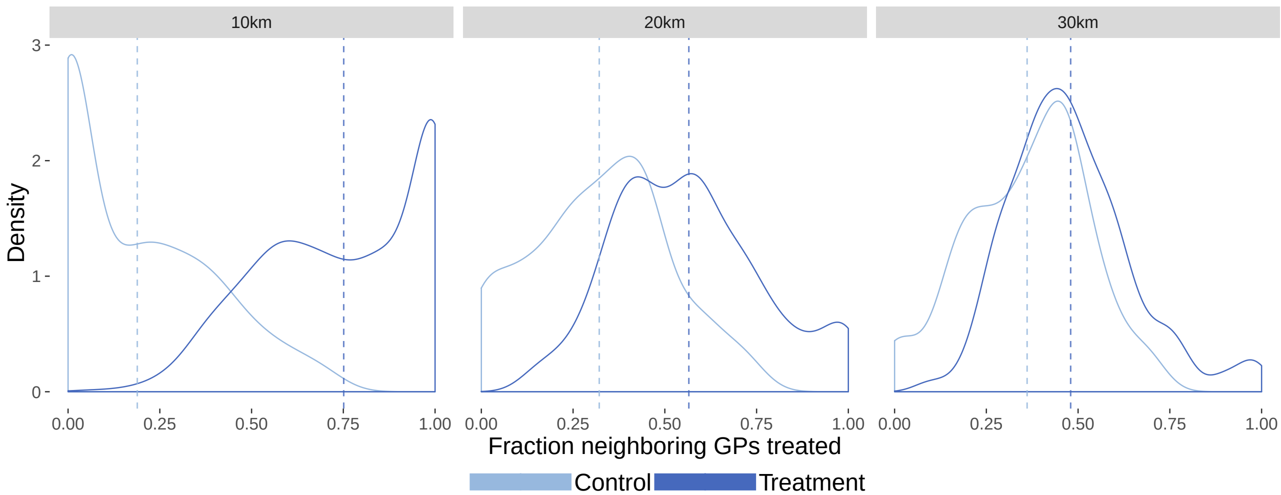
Figure A.4: Changes in wages by month and treatment status



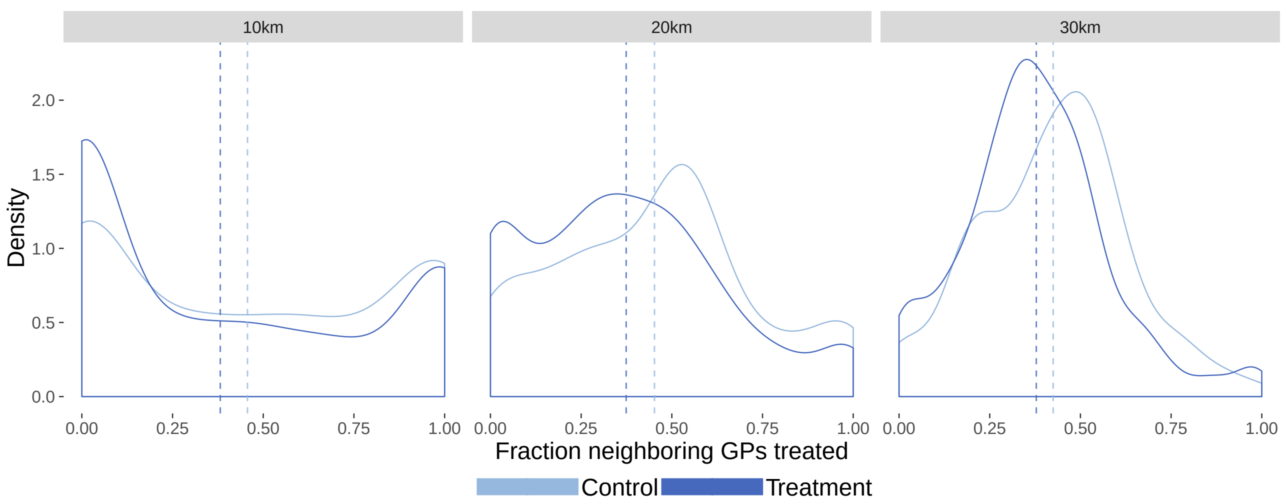
This figure shows mean changes in agricultural wages between baseline and endline, by month and treatment status, weighted by (inverse) GP sampling probability. The data, which is at the village-level, comes from surveys administered to prominent figures in each village. Standard errors are clustered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

Figure A.5: Density of spatial measures of treatment exposure

(a) Exposure to Treatment: Including Same Mandal GPs



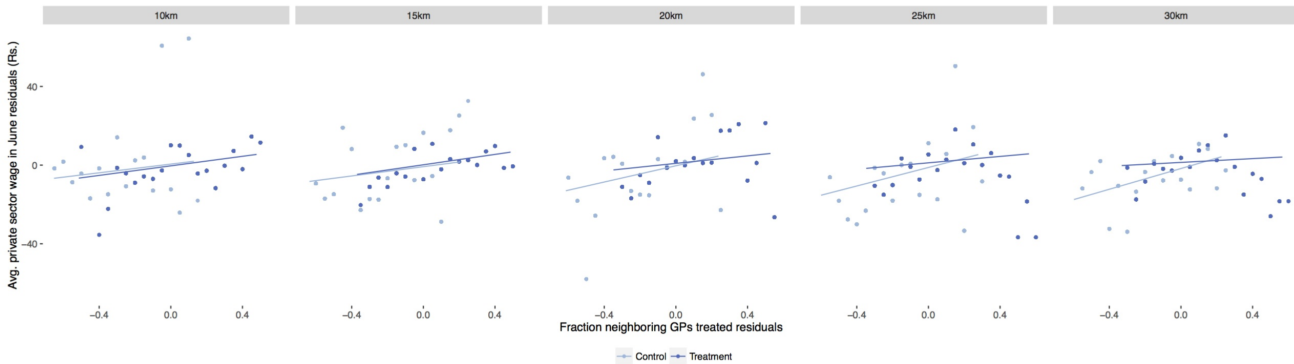
(b) Exogenous Exposure to Treatment: Excluding Same Mandal GPs



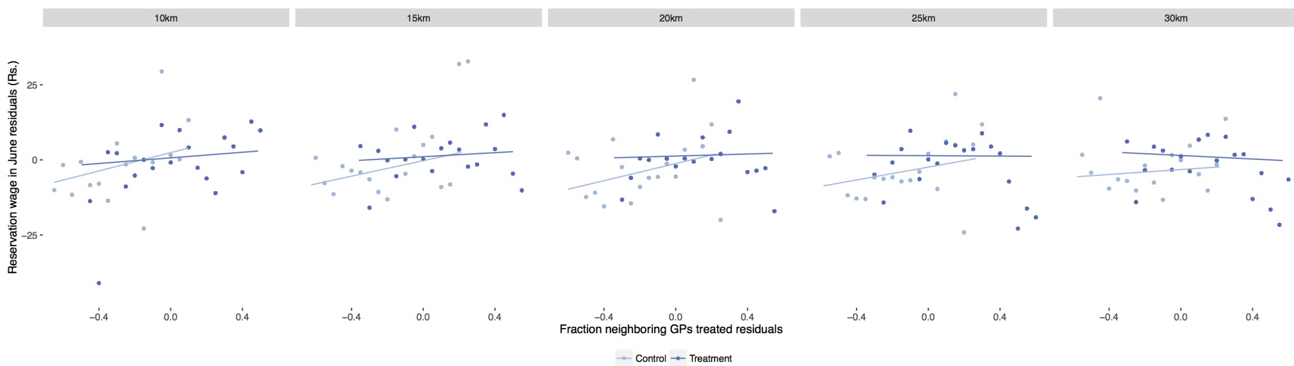
These figures show smoothed kernel density estimates of spatial measures of treatment exposure. The only GPs included in these density calculations are surveyed GPs. Panel a) shows the distribution of exogenous spatial exposure to treatment at a given distance for survey GPs. Panel b) shows the distribution 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.6: Relationship between Spatial Exposure and Wage/Income Outcomes

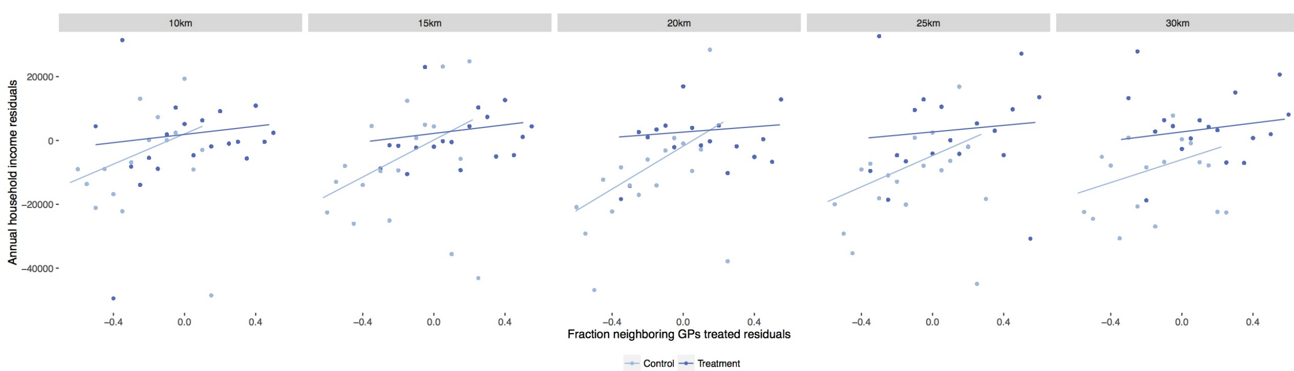
(a) Wage Realization



(b) Reservation wage



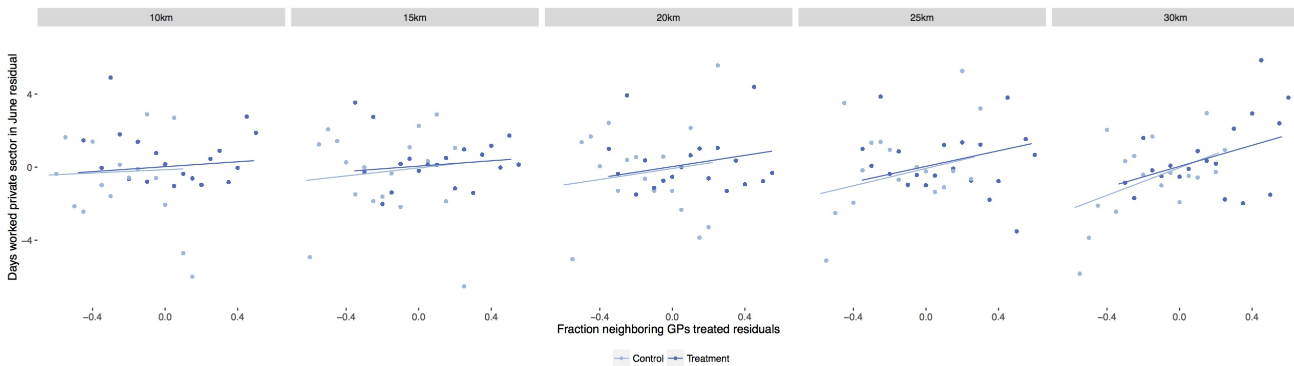
(c) Total income



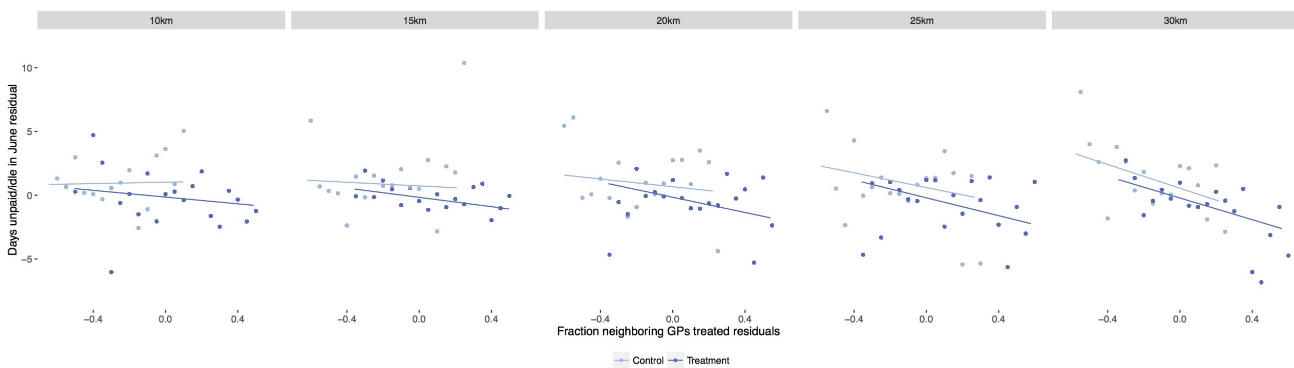
This figure shows partial residual plots for the relationship between the complete spatial exposure measure and wage/income outcomes from our survey data. For each plot, the x-axis variable is the residual from a linear regression of complete spatial exposure on district fixed effects and a first principal 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.7: Relationship between Spatial Exposure and Labor Outcomes

(a) Days worked private sector



(b) Days unpaid/idle



This 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.

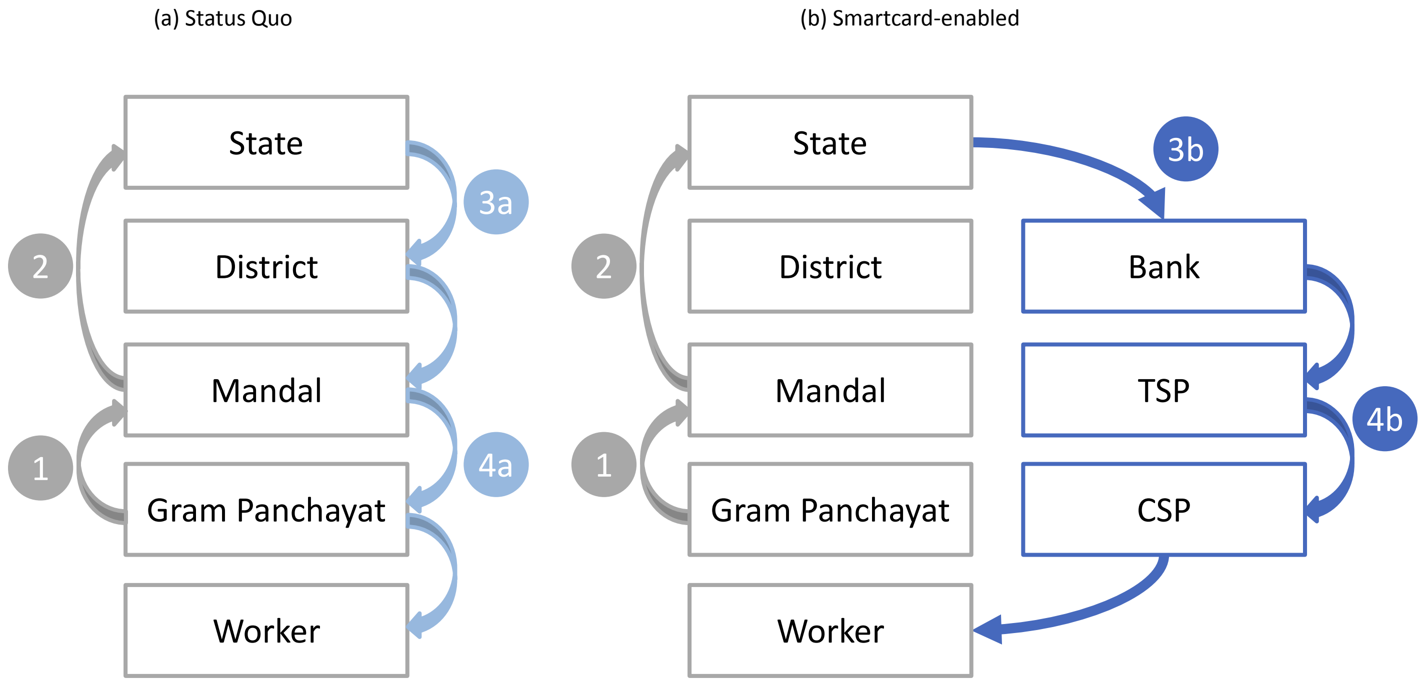


Figure A.8: Comparison of treatment and control payment systems

This 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.