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

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October 25, 2016

Abstract

Public employment programs play a major role in the anti-poverty strategy of many developing countries, but their impact on poverty reduction could be attenuated or amplified by changes they induce in private labor market wages and employment. We estimate these general equilibrium effects using a large-scale experiment that randomized the roll-out of a technological reform, which significantly improved the implementation of India's public employment scheme, across 157 sub-districts of 60,000 people each. We find that this reform increased the earnings of low-income households by 12.7%, and reduced an income-based measure of poverty by 17.2% despite no increase in fiscal outlays on the program. These income gains were overwhelmingly driven by higher private-sector earnings (90%) as opposed to earnings directly from the program (10%). We find that improving implementation of the public employment scheme led to a 6.2% increase in private market wages for rural unskilled labor, a similar increase in reservation wages, and a 7.1% reduction in days without work. We find no evidence of changes in private employment, migration, or land use. Our results highlight the importance of accounting for general equilibrium effects in evaluating programs, and also illustrate the feasibility of using large-scale experiments to study such effects.

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

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

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*We thank Gordon Dahl, Gordon Hanson, Supreet Kaur, Aprajit Mahajan, Edward Miguel, and several seminar participants 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/IPA 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 – for the financial support that made this study possible.

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1 Introduction

Public works programs, in which the government provides daily-wage jobs to those who seek them, are among the most common anti-poverty programs in developing countries. The economic rationale for such programs (over direct income support for the poor) include self-targeting through work requirements, public asset creation, and creating a more effective wage floor by having the government be an employer of last resort.[1] An important contemporary variant is the National Rural Employment Guarantee Scheme (NREGS) in India. Rolled out in 2005, the scheme is the largest workfare program in the world with over 800 million rural residents eligible to participate and a fiscal allocation of 0.8% of India's GDP.

A program of this scale and ambition raises many fundamental questions for research and policy. First, how effective is it in reducing poverty? In particular, while direct income support programs would typically reduce poverty, the general equilibrium effects of public-works programs on the larger rural economy could amplify or attenuate the direct gains in wage income for the poor.[2] Second, what is the relative contribution of direct income gains from the program and indirect income gains (or losses) outside the program? Third, what are the impacts on the broader labor market (including wages, employment, and migration), and what are the mechanisms for the indirect income effects? Finally, since these answers will depend on the underlying structure of rural labor markets, credible answers to the policy questions above can indirectly help improve understanding of questions of broader economic interest such as the extent of spatial integration of rural labor markets and the extent to which these markets are competitive.

Given the importance of NREGS, a growing literature has tried to answer the questions above, but the evidence to date has been hampered by two limitations. The first is identification, with the results being quite sensitive to the methods used and studies often reaching opposite conclusions depending on the identification strategy used (see Sukhtankar (2016) and section 2.1.2 below). Second, implementation of the scheme has proven so varied that estimating the effects of the program may be an ambiguous construct, with the more realistic approach being to assess the impacts of different degrees or qualities of program implementation. For example, the most-cited study of wage impacts finds them only in states coded (ex-post) as having implemented the program well (Imbert and Papp, 2015).

[1] Work-fare programs may also be politically more palatable to tax-payers than unconditional doles. Such programs have a long history, with recorded instances from at least the 18th century in India, the public works conducted in the US by the Work Projects Administration during the Depression-era in the 1930s, and more modern "Food-for-Work" programs across Sub-Saharan Africa and Asia.

[2] A practical way of differentiating partial and general equilibrium effects (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.

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 program implementation, and units of randomization that are large enough to capture general equilibrium effects. Specifically, we worked with the Government of the Indian state of Andhra Pradesh (GoAP), to randomize the order in which 157 sub-districts (with an average population of 60,000) introduced a new technology (biometric Smartcards) for making payments in NREGS. In prior work, we show that the new technology significantly improved the performance of NREGS on several key dimensions: it reduced leakage or diversion of funds, reduced delays between working and getting paid, reduced the time required to collect payments, and increased access to work, without changing the fiscal outlays on the program (Muralidharan et al., 2016). Thus, the Smartcard intervention brought NREGS implementation in AP closer to what its architects intended (Khera, 2011).[3]

Of course, 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 a measure of *effective* NREGS.[4] Further, since significant improvements in program performance were achieved without increasing the fiscal outlay on NREGS, our results on poverty impacts are more likely to reflect the *structure* of NREGS rather than simply reflecting additional fiscal transfers to treated areas.

We report four main sets of results. First, we find large increases in household income in areas where Smartcards were rolled out. We find this result consistently both when using our own survey data as well as when using data from the Socio-Economic and Caste Census (SECC), a census of Indian households conducted by the national government independently of our activities. The SECC collects coarse data by income categories of the highest earner in the household; we find that the Smartcards intervention made it 24.7% more likely that this earner moves out of the lowest income category. Using our survey data on income, we find a Rs. 8761 (12.7%) increase in household income in treated areas, which corresponds to

[3]The Smartcards were also used to make payments for rural social security pensions and reduced leakage here as well, but these are unlikely to have affected broader labor markets (see section 2). The original state of AP (with a population of 85 million) was divided into two states on June 2, 2014. Since this division took place after our study, we use the term AP to refer to the original undivided state.

[4]One natural interpretation is to consider the randomized roll-out of the Smartcards intervention to be an instrument for an endogenous variable that we may call 'effective NREGS.' However, given the many dimensions on which NREGS implementation quality can vary (ease of access to work, availability of work on demand, payment delays and inconvenience, and leakage) it is difficult to construct a single-dimensional summary statistic of 'effective NREGS' that can be instrumented for. Our estimates are therefore best interpreted as the reduced form impact of improving NREGS implementation on multiple dimensions.

a 17.2% reduction in an income-based measure of poverty (a 4.9 percentage point reduction on a base poverty rate of 28.4%).

Second, we find that the vast majority of income gains are attributable to indirect market effects rather than direct increases in NREGS income from the improved program implementation. For NREGS beneficiaries, increases in program income accounted for only 10% of the increases in total income, 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 its impact on poverty reduction than the direct income provided by the program.

Third, we find that improving the performance of NREGS led to a significant increase in private market wages. Market wages for NREGS workers rose by 6.2% in treated areas, with a similar 5.7% increase in reported reservation wages. We also find that market wages increased in *control* villages that had a high fraction of treated villages in their vicinity (at radii up to 20 kilometers), and find larger market wage increases in treated villages with a higher fraction of treated villages in their vicinity.

Fourth, we find no evidence of distortionary 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. We find no impacts on migration, or on measures of land use.

These results could reflect a combination of several factors including NREGS providing a de facto wage floor, monopsonistic labor markets, improved productivity through NREGS asset creation, and local aggregate demand externalities. Our experiment allows us to credibly estimate the composite effect of improving NREGS implementation on a broad set of outcomes (income, poverty, wages, employment) but is not designed to isolate mechanisms of impact. Nevertheless, two suggestive patterns regarding mechanisms emerge in our data.

First, increased labor market competition between the NREGS and private employers is likely to be a significant (though not exclusive) contributor to the increases in market wages we observe. Under this mechanism, improving the NREGS would improve workers' outside options and hence their bargaining power vis-a-vis employers. Consistent with this explanation, we observe a proportional increase in workers' reported reservation wages with actual wage realizations (correlation of 0.8). Further, our results on spatial spillovers suggest that rural labor markets are spatially integrated upto radii of around 20km. Thus, the increase in market wages in *control* villages that were exposed to a high fraction of treatment villages in their catchment area for workers (but did *not* directly see an improvement in NREGS implementation) is also consistent with private employers having to pay higher wages to compete for workers with better outside options.

Second, we find suggestive evidence that the lack of negative employment effects despite the increase in market wages may be attributable to features of the NREGS such as productivity-enhancing asset creation, or local aggregate-demand externalities from better consumption smoothing. Consistent with this interpretation, we find a *reduction* in private market employment in control villages with high exposure to treatment villages and resulting higher market wages but no direct improvements in NREGS implementation. In contrast, we find a positive (though insignificant) point estimate on private sector employment in treatment villages that saw wage increases as well as increases in NREGS activity.

Our first contribution is to the growing literature on the impact of public works programs on rural labor markets and economies (Imbert and Papp, 2015; Beegle et al., 2015; Sukhtankar, 2016). We present experimentally-identified estimates of improving the implementation of NREGS with units of randomization large enough to capture general equilibrium effects and show that doing so led to a significant increase in incomes for the poor, and reduction in poverty, even without spending additional funds on the program.

Second, our results contribute to the general literature on rural labor markets in developing countries (Rosenzweig, 1978; Jayachandran, 2006), as well as the more specific literature on the impacts of minimum wages in developing countries (e.g. Dinkelman and Ranchhod (2012)). Commentators on NREGS have argued that it could not possibly have led to meaningful impacts on rural wages and poverty because the days worked on NREGS constitute only a small share (under 4%) of total rural employment (Bhalla, 2013). Our results suggest that this argument may not be valid and demonstrate that well-implemented public works programs can raise market wages even if the number of days worked on them is not very high, because their very existence can increase workers' bargaining power over wages by providing a more credible outside option (Dreze and Sen, 1991; Basu et al., 2009).

Third, our results highlight the importance of accounting for general equilibrium effects in program evaluation (Acemoglu, 2010). Ignoring these effects would have led to a substantial underestimate of the impact of improving NREGS implementation on poverty reduction. However, on an optimistic note, 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., 2013; Muralidharan and Niehaus, 2016).

Fourth, our results highlight the importance of implementation quality as a first-order consideration in program effectiveness and in the interpretation of program evaluations (especially in developing countries). Strikingly, our estimates of the private market wage impacts of improving NREGS implementation (6%) are of a similar magnitude as reported by the most credible estimates to date of the impact of rolling out NREGS itself (Imbert and Papp, 2015). More generally, programs are not just an 'intervention', but an intervention *and* an implementation protocol and investing in improved implementation may often yield greater improvements in the *effective* presence of a program than increased fiscal outlays on the program itself.[5]

Finally, we contribute to the literature on political economy of development. (Jayachandran, 2006) shows that 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. Consistent with this, 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, and may partly explain the widely documented resistance by landlords to NREGS (Khera, 2011).

The rest of the paper is organized as follows. Section 2 describes the context, including NREGS and prior research, and the Smartcard intervention. Section 3 describes the research design, data, and estimation. Section 4 presents our main results on income, wages, and employment, spillovers to control areas, and discusses mechanisms. Section 5 discusses the implications of these results, while Section 6 concludes.

2 Context and intervention

2.1 National Rural Employment Guarantee Scheme

The NREGS is the world's largest public employment program, making any household living in rural India (i.e. 11% of the world' population) eligible for guaranteed paid employment of up to 100 days. It is one of the country's flagship social protection programs, and the Indian government spends roughly 8% of its budget ($\sim 0.75\%$ of GDP) on it. The program has broad coverage; 65.1% of rural households in Andhra Pradesh have at least one jobcard, which is the primary enrollment document for the program. Workers can theoretically demand employment at any time, and the government is obligated to provide it or pay unemployment benefits (though these are rare in practice).

Work done on the program involves manual labor compensated at statutory piece rates. The physical nature of the work is meant to induce self-targeting. NREGS projects are

[5]For instance (Niehaus and Sukhtankar, 2013b) show that an increase in the official NREGS wage and corresponding increase in fiscal outlays had no impact on the actual wages received by workers. This presents a striking contrast with our results in this paper finding significant increases in wages despite no increase in fiscal outlay. In a similar vein, (Muralidharan et al., 2014) 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.

proposed by village governance bodies (Gram Panchayat) and approved by mandal (sub-district) offices. These projects typically involve public infrastructure improvement such as irrigation or water conservation works, minor road construction, and clearance of land for agricultural use.

The NREGS suffers from a number of known implementation issues including rationing, leakage, and problems with the payment process. Although the program is meant to be demand driven, rationing is common, and work mainly takes place in the slack labor demand season (Dutta et al., 2012; Muralidharan et al., 2016). Corruption is also common, with theft from the labor budget taking the form of over-invoicing the government for work not done or paying worker less than statutory wage rates for completed work (Niehaus and Sukhtankar, 2013a,b). The payment process itself is slow and unreliable, with the norm being payment delays of over a month, uncertainty over payment dates, and lost wages as a result of time-consuming collection procedures (Muralidharan et al., 2016; Pai, 2013).

2.1.1 Potential aggregate impacts of NREGS

In theory, employment guarantee schemes such as the NREGS are expected to affect equilibrium in private labor markets (Dreze and Sen, 1991; Murgai and Ravallion, 2005). A truly guaranteed public-sector job puts upward pressure on private sector wages by improving workers' outside options. As Dutta et al. (2012) puts it,

> "...by linking the wage rate for such work to the statutory minimum wage rate, and guaranteeing work at that wage rate, [an employment guarantee] is essentially a means of enforcing that minimum wage rate on all casual work, including that not covered by the scheme. Indeed, the existence of such a program can radically alter the bargaining power of poor men and women in the labor market... by increasing the reservation wage..."

Depending on the structure of labor markets, increased wages may crowd out private sector employment, perhaps reducing efficiency.

In addition to this competitive effect, NREGS could affect the rural economy through the channels of public infrastructure, aggregate demand, and the relaxation of credit constraints. The public goods that NREGS projects create - such as irrigation canals and roads - could increase productivity, possibly mitigating negative impacts on efficiency. Given the size of the program, it could also have effects through increased aggregate demand as workers' disposable income increases, given the presence of agglomeration economies or barriers to trade (for internal barriers to trade in India see Atkin (2013)). Finally, increased income may relax credit constraints and thereby increase output.

Given the implementation issues discussed in the previous section, it is unclear whether any of these effects are actually witnessed in practice. For example, Niehaus and Sukhtankar (2013b) point out that because of corruption by officials who steal worker's wages, NREGS does not serve as an enforcement mechanism for minimum wages in the private sector, but rather functions as a price-taker.

2.1.2 Prior evidence on NREGS impact

The impact of the NREGS on labor markets, poverty, and the rural economy has been hotly debated since inception.[6] Supporters claim it has transformed the rural countryside by increasing incomes and wages; creating useful rural infrastructure such as roads and canals; and reduced distress migration Khera (2011). Detractors claim that funding is simply captured by middlemen and wasted; that it couldn't possibly affect the rural economy since it is a small part of rural employment ("how can small tail wag a very very large dog?"); or that even if it increases rural wages and reduces poverty, that this is at the cost of crowding out more efficient private employment, in rural areas and cities Bhalla (2013). The debate is still politically salient, as the current national government has been accused of attempting to let the program slide into irrelevance by slowly defunding it, while a group of prominent academics signed a letter asking it to not do so.

The problem with this heated debate is that the debate-to-evidence ratio is high, as credibly identifying causal impacts of the program is difficult. There was no evaluation prior to the program's launch or built in to program rollout, while the selection of districts for initial deployment was politicized Chowdhury (2014). The vast majority of empirical work estimating impacts of NREGS thus uses one of two empirical strategies, both relying on the fact that there was a phase-in period for program implementation: it was implemented first in a group of 200 districts starting in February 2006, followed by a second group of 130 in April 2007, while the remaining rural districts entered the program in April 2008. This allows for a difference-in-differences or regression discontinuity approach, by comparing districts in Phase I with those in Phase II and III (or those in I and II with III, etc).

These approaches are non-experimental, and as such rely on strong assumptions in order to identify causal impacts of NREGS. While this problem is well understood - and some of the studies may well satisfy the necessary assumptions - there is another less appreciated problem with these types of comparisons. Given that NREGS suffered from a number of well-documented "teething problems" - for example, two years after the start major issues with basic awareness, payment delivery, and monitoring were still to be worked out Mehrotra (2008) - any estimated impacts are less likely to be informative about steady state effects.

[6]See Sukhtankar (2016) for a review of the literature on the impacts of NREGS.

Possibly the most consistent evidence comes from estimated impacts on labor markets, with three papers Imbert and Papp (2015); Berg et al. (2012); Azam (2012) using a similar difference-in-differences approach but different datasets estimating that the NREGS rollout may have raised rural unskilled wages by as much as 4-5%. Yet these papers disagree on the timing of the effects; while Imbert and Papp (2015) suggest that wage effects are concentrated in the slack labor season, Berg et al. (2012) find that they are driven by peak labor seasons. Meanwhile Zimmermann (2015) finds no average effects on wages using a regression discontinuity approach. Effects on potential crowd-out are also inconclusive: Imbert and Papp (2015) find 1.5% decrease in private employment concentrated in the slack season, while Zimmermann (2015) finds a 3.5% reduction in private employment for men, year-round.

The estimated effects on other outcomes present an even more conflicting picture. Given the potential for labor market effects to spill over into schooling, a number of papers have examined educational outcomes. Mani et al. (2014) find that educational outcomes improved as a result of NREGS; Shah and Steinberg (2015) find that they worsened; while Islam and Sivasankaran (2015) find mixed effects. Given that the program was targeted towards underdeveloped areas suffering from civil violence related to the leftist Naxalite or Maoist insurgency, some papers have examined effects on violence. While Khanna and Zimmerman (2014) find that such violence increased, Dasgupta et al. (2015) find the opposite.

While no doubt there is variation in the samples and strategies used in these papers, as well as variation in the quality of analysis, the starkly conflicting results underline the difficulty with relying on non-experimental analysis. Meanwhile, experiments are difficult when aggregate effects are prominent, since capturing these effect would require the size of units to be large, not just the number of units. Finally, rigorous evidence of net impacts on household welfare using summary statistics such as income or consumption are missing.

2.2 Smartcards

In an attempt to address problems with implementation, the Government of Andhra Pradesh (GoAP) introduced a new payments technology based on electronic transfers of benefits to beneficiary bank accounts and biometric authentication of beneficiaries prior to benefit withdrawal. This technology - which we collectively refer to as "Smartcards" - had two major components. First, it changed the last-mile payments provider from the post office to a private Technology Service Provider / Customer Service Provider. Second, it changed the authentication technology from paper documents and ink stamps to a Smartcard and digital biometric check.

The intervention had two major goals. First, it aimed to reduce leakage from the NREGS labor budget in the form of under-payment and over-reporting. Second, it targeted improvements in the payment experience, in particular delays in NREGS wage payments. More details on the functioning of the intervention and the changes that it introduced are in Muralidharan et al. (2016); for this paper what is relevant is that the intervention dramatically improved the implementation of NREGS, which we describe briefly in section 2.3.

Note that the Smartcards intervention affected both NREGS as well as the Social Security Pension (SSP) program. In this paper, we focus on effects coming from improvements to NREGS, as it is unlikely that improvements to SSP affected the labor market or the broader rural economy for two reasons. First, the scale and scope of SSP is fairly narrow: only 7% of rural households are eligible, as it is restricted to those who are Below the Poverty Line (BPL) *and* either widowed, disabled, elderly, or had a (selected) displaced occupation. It is meant to complement NREGS for those unable to work, and the most prominent benefit level of Rs. 200 per month is small (about $3, or less than two days earnings for a manual laborer). Second, the improvements from the introduction of Smartcards were less pronounced than those in NREGS: there were no significant improvements in the payments process, while reductions in leakage only amounted to Rs. 12 per household.

2.3 Effects on program performance

In Muralidharan et al. (2016), we show that Smartcards significantly improved the functioning of NREGS in AP on multiple dimensions. Two years after the intervention began in treatment mandals, the NREGS payments process got faster (29%), less time-consuming (20%), and more predictable (39%). Additionally, households earned more through working on NREGS (24%), and there was a substantial 12.7 percentage point reduction ($\sim 41\%$) in leakage. Further, both perceived access and actual participation in the program increased (17%). Treatment distributions first order stochastically dominate control distributions for all outcomes on which there was a significant mean impact, suggesting broad-based positive impacts. Reflecting this, user preferences were strongly in favor of Smartcards, with 90% of households preferring it to the status quo, and only 3% opposed.

The improvements in implementation reflect intent-to-treat (ITT) estimates, which is important since implementation was far from complete. Logistical problems were to be expected in an intervention of this scale, and two years after implementation the proportion of payments in treated areas made using Smartcards had plateaud to 50%. It is important to note that these estimates do not reflect "teething" problems of Smartcards, since Smartcards had been implemented in other districts in AP for four years prior to their introduction in our study districts. The estimates reflect steady state, medium run impacts that are net of management, political economy, and other challenges.

A result that is important to the interpretation of general equilibrium effects of Smartcards is that there was no increase in NREGS expenditure by the government. Thus unlike the introduction of NREGS itself, no new money flowed into treatment areas. Any increases in earnings were due to a reduction in leakage corresponding to a redistribution from corrupt officials to workers. This is the main significant difference if one wishes to compare the effect of Smartcards to an idealized effect of "NREGS itself," although one could potentially use the mean level of program earnings in the control group as an indicator of what the effect of the program itself may have been on this dimension.

Other mechanisms via which the reform affected rural economies are similar to those that one might expect from the introduction of a well-implemented NREGS. First, the improvement in payments logistics such as timeliness of payments and the increase in earnings on NREGS made the program a more viable outside option to private sector employment, and thus led to an increase in competitive pressure in the labor market. Since there was additional participation in NREGS - verified by our stealth audits that counted more actual laborers on worksites - there was also an increase in the amount of rural work done and rural public goods created. The increases in NREGS earning could also have relaxed credit constraints on participants.

3 Research design

3.1 Randomization

We summarize the randomization design here, and refer the reader to Muralidharan et al. (2016) for further details. The experiment was conducted in eight districts[7] with a combined rural population of around 19 million in the erstwhile state of Andhra Pradesh (now split into two states: Andhra Pradesh and Telangana). 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 a map showing the geographical spread and size of these units. We created the buffer group to ensure that we could conduct endline surveys

[7]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 the online appendix to Muralidharan et al. (2016) for details.

before deployment began in control mandals, and restricted survey work to treatment and control mandals. We stratified randomization by district and by a principal component of mandal socio-economic characteristics.

We examine balance in Tables A.1 and A.2. The former (reproduced from Muralidharan et al. (2016)) simply shows balance on variables used as part of stratification, as well as broader mandal characteristics from the census. Treatment and control mandals are well balanced, with two out of 22 variables significant. 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 (see below). Here, four out of 34 variables are significantly different at the 10% level at least, which is slightly more than one might expect. 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 primary goal of the SECC was to enable governments to rank household by socio-economic status in order to determine which were "Below the Poverty Line" (BPL) and thereby eligible for various benefits. A secondary (and controversial) goal was to capture data on caste, which the regular decennial census does not collect. 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) and 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 during the conduct of the 2011 Census. Enumerators were assigned to cover the households listed in each block, and were also instructed to attempt to interview homeless populations. The total number of households in our SECC sample, including treatment and control mandals, is slightly more than 1.8 million.

3.2.2 Original survey data

We complement the broad coverage of the SECC data with original and much more detailed surveys of a smaller sample of households. Specifically, we conducted surveys of a representative sample of NREGS jobcard holders and SSP beneficiaries during August to October of

2010 (baseline) and 2012 (endline). Surveys covered both respondents' participation in and experience with these programs, and also their 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 the month of June (the period of peak NREGS participation in Andhra Pradesh).

Full details of the sampling procedure used are in Muralidharan et al. (2016). In brief, we drew a representative sample of SSP pension holders, and a sample of NREGS jobcard holders that over-weighted those who had recently participated in the program according to official records. We discuss corresponding weighting of estimators below. The combined frame from which we sampled covers an estimated 68% of the rural population.[8] We sampled a panel of villages and repeated cross-sections of the full concurrent NREGS and SSP sampling frames. The sample included 880 villages, with 10 households in each village (6 from NREGS frame and 4 from SSP frame). This yielded us 8,774 households at endline, of which we have survey data on survey data on 8,114 households; of the remaining, 365 were ghost households, while we were unable to survey or confirm existence of 295 (corresponding numbers for baseline are 8,572, 7,425, 102 and 1,000 respectively).

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 use and irrigation – including details by season – and on employment in industry. DSH are published every year and are not to be confused with the District Census Handbooks which contains district tables from the Census of India. Land coverage data presented in the DSH is officially provided by Office of Surveyor General of India. Ideally, land coverage data is obtained from so-called village papers prepared by village accountants. These village papers contain information on land cover that varies (area sown or fallow) while forest and mountainous areas is recorded centrally. For cases in which no village papers are maintained, "ad-hoc estimates of classification of area are derived to complete the coverage."[9]

[8] In Andhra Pradesh, 65.7% of rural households have a jobcard according to our calculations based on the National Sample Survey Round 68 in 2011-12. Since 7.6% of the population in AP received (or were due to receive) pensions and 29.5% of the SSP households in our sample do not own a jobcard, the SSP sample adds an additional 2.2% to the sample.

[9] Information from `http://eands.dacnet.nic.in/`, accessed March 22, 2016.

3.3 Estimation strategy

We report straight-forward comparisons of outcomes in treatment and control mandals throughout (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:

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

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

Regressions using the SECC data are unweighted. Regressions with NREGS samples are weighted by inverse sampling probabilities to be representative of the universe of jobcard-holders. When using survey data on financial outcomes we trim the top 0.5% of observations in both treatment and control groups to remove outliers, but our results are robust to including them.

4 Results

4.1 Effects on earnings and poverty

Figure 2 compares the distribution of SECC income categories in treatment and control mandals. The treatment distribution first-order stochastically dominates the control, with 4.1 percentage points fewer households in the lowest category (less than Rs. 5,000), 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). Overall, these estimates imply that Smartcards moved 44,319 households out of the lowest income category and into a higher one.

Table 1a reports statistical tests of these effects, using both logistic regressions on individual categories (showing marginal effects) and ordered logistic regression on the combined categories in light of the categorical nature of our income measure. The results show that treatment significantly increased the log-odds ratio of being in a higher income category, with results strongly significant at the 1% level. As a sanity check we also confirm that these estimates are unaltered when we control for arguably pre-determined measures of economic status (landholdings) or demographics (age of household head, caste, literacy). This suggests that the Smartcards randomization was indeed orthogonal to other determinants of earnings.

The SECC income measures let us test for income effects in the entire population of interest, but have two limitations when it comes to estimating magnitudes. First, much information is lost through discretization: the 4.1% reduction in the share of households in the lowest category which we observe, for example, could reflect a small earnings increase for households that were just below the Rs. 5,000 threshold, or a large impact for households that were further away from it. 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.

For a better sense of magnitudes we therefore turn to our survey data. Columns 1 and 2 of Table 1b reports estimated impacts on overall annual household income, with and without controls for the mean income in the same village at baseline. In both specifications we estimate that that treatment increased 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). It is important to bear in mind the fact that expenditure- and income-based poverty lines may well differ; the comparison is provided for illustrative purposes only. But if these lines were taken as equivalent, we see a 4.9 percentage point or 17.2% reduction in poverty: this is clearly visible in Figure 3 which plots the empirical CDF of household earnings for treatment and control groups.

Our survey data also allow us to examine distributional impacts. We see that estimated earnings impacts are weakly positive throughout the distribution, but visibly larger at the higher end, with 55% of the total earnings increase accruing to households above the 80th percentile (corresponding to annual household income of Rs. 97,000). Of course, a household earning this much remains poor by any absolute standard; this number is twice the expenditure-based poverty line, but corresponds to less than $1/day per capita (in real, not PPP exchange rates).

One potential caveat to the results above is that they show impacts on nominal, and not real, earnings. If Smartcards affected the overall level of prices in the local economy then they might under- or over-state real effects. Sufficiently disaggregated data on local prices are unfortunately not available, but our survey did measure expenditures; if local price levels rose then we should expect to see expenditures rise as well. In the data, expenditure in treated areas is not statistically significantly higher than in control areas (Table 2). This suggests both that the income effects are real, and also that households treat gains as temporary. However, the confidence intervals are wide: our survey was explicitly designed to capture earnings (given the interest in measuring NREGS leakage) rather than expenditure or consumption, and as such our data on the latter are nowhere as rich or accurately measured as, say, the National Sample Surveys (NSS) or the Living Standards Measurement Survey (LSMS).[10] Thus we treat our earnings data as more reliable than our expenditures data.

4.2 Direct versus indirect effects

Mechanically, the effects on earnings and poverty we find above must work either through increases in households' earnings from the NREGS program itself or through increases in their non-program (i.e. private sector) earnings, or both. To examine this decomposition we use our survey data, which provides more granular information than the SECC on sources of income. Specifically, we collected information separately on income from six 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 3-9 of Table 1b report treatment effects on various income categories separately. Strikingly, effects on NREGS earnings are only a small proportion of overall income gains, accounting for only 10.4% of the overall increase.[11] Instead the primary driver of increased earnings is an increase in paid labor, both in in the agricultural and non-agricultural sectors.

[10]For example, our entire expenditure module (including printed English and Telugu alternatives) was one page long and captured 26 categories for all household expenses; the NSS consumer expenditure module (Schedule 10, Round 64) in English only is over 12 pages long and has 23 options *for cereals only*.

[11]The (marginally) insignificant effect on program earnings here is not inconsistent with the estimated positive effects on program earnings in Muralidharan et al. (2016). The results in Muralidharan et al. (2016) relate to a specific study period which was just before our survey; we collected detailed data on every week of program participation and used specific methods (e.g. mentioning specific holidays/festivals) to prompt recall for each week; and we asked questions of the program beneficiary herself. The results here come from a separate section in the survey in which we collected annual income from the head of the household, with general rather than specific measures to prompt recall.

Effects on own farm earnings are positive but insignificant.

One concern about these results is potential miscategorization; households might perhaps report NREGS income as agricultural income. Given the salience of NREGS, and the fact that earnings must be collected after significant delays from local officials rather than immediately from landlords, this is unlikely. Nonetheless, the level of program earnings of approximately Rs. 5,000 is consistent with other reports of program earnings (Imbert and Papp, 2011), making large scale misreporting unlikely.

4.3 Effects on private market wages

Improved implementation appears to have reduced poverty primarily through indirect effects on private-sector labor earnings. We next examine if the increased labor earnings were driven by increased wages, or greater employment, or both.

As the SECC does not include wage data, we estimate wage effects using our survey data. We define the dependent variable as the average daily wage rate earned on private-sector work reported by respondents who did any private-sector work. We check that the results are robust to restricting the sample to adults aged 18-65 (additional robustness checks are reported in Section 4.7 below).

We find a significant increase of Rs. 7.9 in private sector wages (Table 3, Column 2). This effect size is large, equal to 6.2% of the control group mean and slightly larger than the highest estimates of the wage impacts of the rollout of the NREGS itself. In Section 4.7 below we discuss whether these effects are driven by changes in who reports wages rather than the distribution of wage offers in the market.

Thus far we have treated each mandal as an independent observation, assuming no spillovers from treated to control units. This approximation may be reasonable for NREGS outcomes, since program administration is hierarchical and unlikely to affect neighboring units, but difficult to defend in cases of market outcomes. In particular, it is unlikely that village labor markets are autarkic.

Hence we relax that assumption and explore spillovers onto geographical proximate units. We have two motives in doing so. First, our estimates above may under-state the true effects in the presence of (positive) spillovers. For example, if Smartcards drive up wages in a treated village then it seems likely that they would also raise wages to a lesser extent in nearby, untreated villages, biasing treatment effects downwards. Second, spillovers working through markets are of independent interest as they teach us about the degree of market integration. For example, the fact that we find wage effects implies that labor markets are not perfectly integrated across mandals, but does not tell us how close or far they are from autarkic.

We first visually inspect the data for the possibility of market wage spillovers by examining how market wages vary in control villages with the intensity of exposure to treatment in neighboring villages. We plot residuals from a regression of average endline private sector wages on average baseline private sector wages and district fixed effects against the fraction of treatment mandal villages that are within a radius R of the given village. We define our measure at the village level since this is the smallest unit for which we have GPS coordinates. Figure A.1 illustrates the construction of the exposure variable for a particular village. All panels of Figure 4 - presenting radii of 10, 15, 20, 25, and 30km respectively - show a clear gradient in the intensity of exposure: wage residuals are higher in control villages that have a greater proportion of neighboring villages that are in treatment mandals.[12]

To conduct inference, we present a simpler version of the above relationship by regressing the wage residual on an indicator denoting whether the fraction of neighboring villages that are in treatment mandals is greater than half. We present the results separately for control (Panel (a)) and treatment (Panel (b)) villages, with and without accounting for baseline wages. For control villages, the coefficient on intensity of exposure indicator is positive and significant for the 15 and 20km radii; wages are Rs. 8.1-9.6 higher in control mandal villages surrounded by more than 50% treatment villages in a 15 or 20km radius when compared to remaining control mandal villages. For treatment villages the indicator is smaller in magnitude ($\approx$ Rs. 5) but significant at the 5% level up to 30km. These results confirm the existence of spatial spillovers in market wages. We discuss the magnitudes and implications for the main effects of own-treatment status in Section 4.6 below.

4.4 Effects on private market employment

Next we examine the allocation of labor across sectors, using our survey data (Table 5). We classify days spent during the month of June by adults (ages 18-65) into three categories: time spent working on the NREGS (Columns 1 & 2), time spent working on the private sector, including self-employment (Columns 3 & 4), and time spent idle / on leisure (Columns 5 & 6). We find significant decreases in days spent idle, and corresponding (insignificant) increases in days spent on both NREGS work and private sector work.

One potential explanation for the latter result is that there was simply too little private sector activity in June to begin with for much to be diverted. This does not appear to be the case in the data, however, as 51% of our sample reported doing at least some private sector work in June (50% in control and 52% in treatment). Moreover, when we compare

[12] Figure A.2 shows that while somewhat sparse at the extremes, particularly in the 10km radius case, there is enough mass at across the distribution to make analysis meaningful.

the distributions of private sector days worked in the treatment and control groups, we see no evidence that upper regions of this distribution are contracting (Figure 5).

The result on private sector work is notable as it implies there is little evidence here of labor being diverted out of the private sector, despite higher wages. At first glance it also appears inconsistent with Imbert and Papp (2015), who claim a nearly 1-1 crowd out with private sector employment. However, "private sector employment" in that paper does not distinguish domestic work and self-employment from wage employment for others, as it is based on NSS data which do not make that distinction. Thus it is entirely possible that the crowd-out highlighted in Imbert and Papp (2015) is - as we find - from domestic and self-employment, rather than from working for landlords.

While we do not find evidence of crowd-out within villages, it is possible that there are spillovers across villages. To examine this possibility, we perform analysis similar to that for wages above. In the control village sample, we see that villages that are surrounded by more than 50% treatment villages see *less* employment than other control villages, suggesting that treatment villages are drawing labor from control villages. This effect is statistically significant at the 20 and 25km radii. Meanwhile, there is no detectable spillover effect in the treatment village sample. We discuss implications of these findings in section 4.5 below.

Finally, it is possible that labor income may have increased due to increases in earnings from migration. In our survey we 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 7 reports effects on each measure. We estimate a small and statistically insignificant increase in migration on both the extensive and intensive margins. This is contrary to the prevailing view that the NREGS is likely to reduce migration to cities.[13] Nonetheless, the magnitudes of the effect sizes - even accounting for confidence intervals - are small enough to discount earnings from migration as a driver of increases in income.

4.5 Channels of impact

Our experiment and data collection were not designed to tease apart mechanisms behind the income and wage impacts we report above. Nonetheless, in this section we do our best to determine what the data can tell us about the mechanism(s) through which the effects on income and wages worked. Two suggestive themes emerge from this examination: the effects of competitive pressure from NREGS, and potential productivity-enhancing impact of asset creation.

[13]Our questions do not capture permanent migration; however, we find no treatment effects on household size or population which may capture this quantity.

An improved NREGS could put competitive pressure on labor markets, driving up wages and earnings. Previous theoretical work has emphasized this mechanism (Ravallion, 1987; Basu et al., 2009), and the literature on NREGS wage impacts has taken this as motivation (e.g. Imbert and Papp (2015)).

The central prediction of the labor market competition hypothesis is that wages rise because workers demand higher wages. To examine this prediction, we elicited reservation wages in our survey, asking 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. Among respondents who worked, 98% reported reservation wages below or equal to the wages they actually earned, suggesting that they correctly understood the question (Table A.6).

Our data are consistent with workers demanding higher wages. We find significant positive effects on reservation wages, similar in magnitude to those on wage realizations (Columns 5-8 of Table 3). Treatment increased reservation wages by approximately Rs. 5.5, or 5.7% of the control group mean. This implies that better outside options must be at least part of the explanation for higher private-sector earnings. Moreover, the fact that effects on reservation wages and actual wages are nearly identical suggests that the labor market competition effect is strong enough to explain the entire wage effect. Statistically speaking, however, we cannot conclusively rule out economically meaningful differences between wage and reservation wage effects; the 90% confidence interval is Rs. (-3.57, 5.20) for the combined samples.

Since reservation wages could also rise because other outside options - not just NREGS - have improved, these results are not dispositive. However, the results on wage spillovers suggest that labor market competition must have played at least some role, since wages in control villages that are relatively more exposed to treatment grew as well without corresponding expansions in NREGS work done. This points to the tightening of labor supply from neighboring treatment villages as the driver for wage growth in exposed control villages.

While competition from NREGS may explain results on wages, the fact that we do not see declines in employment deserves further explanation. While effects on employment are not statistically significant, taking the point estimates at face value would suggest that labor markets must be sufficiently monopsonized that a higher wage can actually increase hiring (as in the much-debated case of minimum wage legislation). Alternatively, increased work on NREGS could also stimulate the creation of productivity-enhancing assets, which is consistent with both increases in wages and employment.

By rule, NREGS projects are meant to create productivity-enhancing assets such as roads, irrigation facilities, or soil conservation structure. Since Smartcards led to increased NREGS participation, they may have also have increased the creation of such assets.[14]

We do not find any direct evidence of such effects in the SECC or district handbook data. Tables 8 shows no significant effect on the amount of land under cultivation (% area sown or % area fallow) or on the total area irrigated.[15] The implied confidence intervals let us rule out effects larger than 4 percentage points in all cases. This rules out increases in labor productivity due for example to irrigation assets increasing the amount of irrigated or cultivatable land. It is also difficult to reconcile with other indirect effects on labor productivity – for example, if road construction raised the marginal revenue product of labor, one would also expect it to raise the marginal revenue product of land and thus bring more marginal land into use.

As a second test, we also examine the pattern of earnings effects in our survey data. If Smartcards generated broad productivity gains then we might expect to see these reflected in both employment and own-account earnings. For example, better market access would increase the profitability of both large plantations and small owner-farmed plots. Table 1b shows no significant impact on earnings from self-employment, however, with effects significant only for labor income categories. One limitation of this test, of course, is that assets could have been created that directly benefit only wealthy landowners and not the (typically poorer) households in our sample.

There is, however, one piece of indirect evidence that may point to increased productivity through asset creation. Recall that control villages that are surrounded by relatively more treatment villages see higher wages but *lower* employment. The fact that control villages do witness crowd-out from higher wages while treatment villages do not suggests that possibly treatment villages see a productivity boost.

Another way in which Smartcards could increase productivity is by easing credit constraints. Specifically, if a more reliable source of fallback employment makes NREGS jobcard holders a better credit risk, they may find it easier to borrow, and might then use this credit to finance productive investments. We do in fact see some evidence that treatment increased borrowing (Table 9), but do not have evidence to link this increased borrowing with increased productivity due to the lack of impact on business or farm earnings.

Improvements in NREGS increase participants' earnings from the program. A final hypothesis is that this increase in purchasing power, in the presence of transport costs and local scale economies, stimulated local economic activity and thus drove up wages and earnings (Krugman, 1991).

[14] Whether NREGS does in fact create assets of any value is much debated (Bhalla, 2013).

[15] In earlier drafts we presented evidence that the intervention increased vegetative cover in the month of May as measured based on satellite imagery using the Enhanced Vegetation Index (EVI). When we examined year-round impacts on EVI, however, we do not find a robust pattern. Results available upon request.

The data seem hard to reconcile with this view. A priori, we find no effect of the intervention on the amount of money disbursed by the NREGS (Muralidharan et al., 2016). The incremental money that beneficiaries receive from these programs is offset one-for-one by reductions in rents to implementing officials. These groups would need to exhibit sufficiently large differences in the marginal propensity to consume for redistribution between them to trigger the large wage and earnings gains we observe. Second, we do not observe a significant increase in household expenditure in our survey data. Table 2 reports an insignificant effect on treatment group expenditure, whether it be more frequently purchased consumables or infrequently purchased durable goods or other yearly expenses. Third, as noted above we see gains in employment but not in own-account earnings. On net then we see little to suggest the existence of aggregate demand effects, though we cannot rule out the possibility that they play a small role.

4.6 Adjusting estimates for spillovers

The fact that we find significant evidence of spatial spillovers naturally raises the question how best to estimate their magnitude. This is of course a general methodological issue. On the one hand, we have no strong prior reason to impose any particular structural relationship between the effects of treatment in mandal i on outcomes in mandal j; this relationship could be different for each pair (i, j). On the other hand, the number of such pairs is too large relative to the sample size for non-parametric estimation to be credible. One could imagine an intermediate approach that first reduces the dimensionality of the problem and then applies non-parametric techniques to this reduced-dimension representation: for example, we could specify that outcomes in i depend only the proportion of other mandals treated at a series of fixed distances, and then estimate outcomes as nonparametric functions of those distances as in Robinson (1988). But in our view the dimensionality-reducing arguments required to do so are inevitably so strong as to make the flexibility of the subsequent nonparametrics no comfort.

We therefore focus on a conceptually simple exercise to attempt to estimate the combined treatment effect of own-treatment status as well as spillovers by restricting the sample to treatment villages with high exposure and control villages with low exposure: in other words, we restrict the control sample to villages that are mainly surrounded by other control villages and the treatment sample to villages that are mainly surrounded by treatment villages. As is clear from Table 10, the estimated effect sizes are in general higher in magnitudes (although not statistically distinguishable) from the main treatment effects found in Tables 1b and 3.

4.7 Robustness

In this section we describe the main robustness checks of our results on income and wages. The estimated income effects in Table 1b are robust to a number of checks. Since the SECC data are categorical, we have used logit and ordered logit models for estimation. The results are robust to using probits or linear probability models instead (results available on request). Our survey data on income are top-censored to exclude outliers (the top 0.5% in treatment and control). However, including these observations does not change the results: the estimates are larger and remain significant at the 1% level (Table A.4a).

Our wage results are also robust to alternative choices of sample. As with the income data, we top-censor wages to account for outliers. Although noisier, results including these observations are similar (Table A.5a). 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.5). Next, dropping the small number of observations who report wages but zero actual employment again does not matter (Table A.5).

Given that we observe wage realizations only 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 wage offers in the market. 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.6). Second, we check that the probability of reporting any work is not significantly different between treatment and control groups (A.6). Third, we check composition and find that treatment did not affect composition of those reporting A.7. Finally, as we have showing above treatment also affected reservation wages, which we observe for nearly the entire sample (89%) of working-age adults.

5 Discussion

5.1 Magnitudes of effects on income

How plausible is the fact that increases in NREGS earnings represented only 1/9th of the increase in income, with the rest attributable to private sector sources? First, note that NREGS earnings comprise less than 7% (or less than 1/14th) of total control group income, because NREGS mainly operates during the slack labor season of April-June. Thus, a priori it is not surprising that a significant increase in NREGS annual income (19.3%) would be swamped by a modest increase in private sector sources of earnings.

However, the increase in private sector earnings implies that the wage effects continued throughout the year, not just when NREGS operated. While our household survey data on private sector wages are restricted to the month of June, we do have village-level data on wages throughout the year. Since we only have one observation per village, power is restricted, but the raw figure does substantiate persistent differences in wages in treatment and control mandals throughout the year (Figure A.3). Such persistent effects could be explained by nominal wage rigidity Kaur (2015), and/or labor tying Bardhan (1983); Mukherjee and Ray (1995). With wage increases throughout the year, and taking the coefficient on employment at face value, the total increase in non-NREGS income of 11.3% is almost exactly explained by the 6.2% increase in wages and a 5% increase in employment.

5.2 Structure of labor markets

The above discussion takes the point estimate on treatment effect on employment at face value, even though it is not statistically significant. However, employment can increase with a concurrent increase in wages only if labor markets are monopsonistic or if there was an increase in productivity.

Our point estimate is not significantly different from zero, however. To quantify precision we combine our quantity estimates here with the wage estimates reported earlier to calculate an estimate and confidence interval for the wage elasticity of labor demand, maintaining the assumption of a competitive market. We estimate a 95% confidence interval from (-0.44,0.8). This interval includes, albeit barely, the estimate of -0.31 reported by Imbert and Papp (2015). Thus we can only rule out competitive markets with a wage elasticity of labor demand greater than -0.44 in magnitude. Given the potential impacts on productivity discussed above, our data cannot rule out competitive labor markets.

We have a bit more to say on the spatial integration of labor markets. Because labor market data below the district level are largely unavailable, previous work on Indian labor markets typically treats each district as a distinct labor market (e.g. Jayachandran (2006), Imbert and Papp (2015), Kaur (2015)); little is known about the extent of within-district integration. Given that the average rural district in AP had an area of approximately 10,000 square kilometers (making a square district 100km across), our spillover effects up to 20km imply that the previous literature's assumption of the district as a unique labor market is reasonable, albeit conservative.

For context, note that 20km is a 4 hour walk at the average human walking speed of 5km/hour, and roughly 7 times the width of an average village (2.9km). NREGS rules, meanwhile, stipulate that employment should be provided within 5km of the beneficiary's home. Of course workers don't necessarily have to travel 20km for these spillover effects to be observed at that distance. Nonetheless, most workers use bicycles for transport, which makes a distance of 20km easily attainable. In our data, nearly 80% of all households own at least one bicycle.

6 Conclusion

This paper examines the impact of a major reform to a large public works program - the National Rural Employment Guarantee Scheme - in India. Such large programs often have general equilibrium impacts, which are difficult to capture non-experimentally or through experiments where the scale of the randomized unit is small. We take advantage of an unusually large-scale intervention that introduced biometric "Smartcards" to make payments to beneficiaries of the NREGS. In previous work we find that Smartcards significantly improved the implementation of NREGS. Here we examine the corresponding effects of this improvement on beneficiaries livelihoods and rural labor markets.

We find large increases in income, using not only our representative survey data but also an independent and concurrent census conducted by the government. We also find that the indirect effects of the reform are an order of magnitude larger than the direct effect on NREGS earnings. These indirect effects are driven by effects on private sector labor markets, namely increase in wages. Finally, we do not find evidence of labor market distortions related to NREGS, and also find some evidence of labor market spillovers across villages.

While we estimate the effects of improving NREGS implementation, one might also wonder 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.

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Table 1: Income

(a) SECC data

	Lowest bracket Marginal Effects		Middle bracket Marginal Effects		Highest bracket Marginal Effects		Middle bracket Predicted Probability	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.045***	-.042***	.029**	.027**	.015**	.014**	.032***	.031***
	(.016)	(.016)	(.012)	(.012)	(.0074)	(.0069)	(.011)	(.011)
Age of hhd head		-.000075***		.000027***		.000079***		.000049***
		(.000017)		(8.8e-06)		(.000018)		(.000011)
Illiterate		.091***		-.052***		-.034***		-.068***
		(.006)		(.0042)		(.003)		(.0045)
SC/ST		.052***		-.037***		-.013***		-.038***
		(.0072)		(.0054)		(.003)		(.0051)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-squared	0.01	0.03	0.01	0.02	0.02	0.04	0.01	0.02
Control Mean	.83	.83	.13	.13	.037	.037		
N. of cases	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
Data source	SECC	SECC	SECC	SECC	SECC	SECC	SECC	SECC

(b) Survey data

	Total income		NREGS	Ag. labor	Other labor	Farm	Business	Misc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	9,511**	8761**	914	3,276**	3,270**	2,166	-642	528
	(3,723)	(3,722)	(588)	(1467)	(1,305)	(2,302)	(1,325)	(2,103)
BL GP Mean		.025						
		(.071)						
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.04	.04	.01	.06	.06	.02	.01	.01
Control Mean	69,122	69,122	4,743	14,798	9,322	20,361	6,202	13,695
N. of cases	4,908	4,874	4,907	4,908	4,908	4,908	4,908	4,908
Survey	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS

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: the "Lowest bracket" corresponds to less than Rs. 5000 per month, the "Middle bracket" includes earnings between Rs. 5000 and Rs. 10000 per month, while the "Highest bracket" includes earnings in excess of Rs. 10000 per month. "Illiterate" is an indicator for whether the head of the household is illiterate and "SC/ST" is indicator for whether a household belongs to Scheduled Castes/Tribes, which are historically discriminated against sections of the population. The table reports marginal effects which are changes in the predicted probability of being in the respective income bracket (columns 1-6) resulting from i) a change in a binary indicator from 0 to 1 or ii) comparing head of households of 30 and 60 years of age (a positive number indicates a higher probability for age 60). All marginal effects are obtained by keeping all other covariates at their estimation sample mean. In columns 7-8, we show the predicted probability of being in the middle income category. The respective predicted marginal treatment probabilities for the highest income category from the ordered logit models are -.45 (0.16***) and -0.43 (0.15***). Note that households in the top .5% percentile of landholdings were excluded. Panel b) shows treatment effects on various types of income using annualized household data from the endline household survey for the NREGS sample. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. "Total Income" is total annualized household income (in Rs.). "NREGS" is the earnings from NREGS. "Ag 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. Households in the top .5% percentile based on total annualized income in treatment and control are excluded in all regressions in panel b). 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. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Expenditure

	Short-term expenditure		Longer-term expenditure	
	(1)	(2)	(3)	(4)
Treatment	53	-255	-675	-1261
	(946)	(945)	(2794)	(2780)
BL GP Mean		.053***		-.0019
		(.02)		(.0044)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.02	.03	.01	.01
Control Mean	18197	18197	36659	36659
N. of cases	4919	4885	4919	4885
Recall period	1 month	1 month	1 year	1 year
Survey	NREGA	NREGA	NREGA	NREGA

This table analyzes different categories of household expenditure using data from the NREGS sample. "Short-term expenditure" is the sum of spending on items such as produce, other food items, beverages, fuel, entertainment, personal care items or rent. The time frame for this category is one month, which is also the time period that was referred to in the survey. "Longer-term expenditure" comprises medical and social (e.g. weddings, funerals) expenses as well as tuition fees and durable goods. In the survey, people were asked to indicate their spending on these items during the last year. Note that households in the top .5% percentile of expenditure were excluded. Panel b) shows treatment effects on various types of income using annualized household data from the endline household survey for the NREGS sample. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Wages (June)

	Wage realizations (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.6*	7.9**	4.9*	5.5*
	(3.6)	(3.6)	(2.9)	(2.8)
BL GP Mean		.16***		.098***
		(.048)		(.033)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.07	.07	.05	.05
Control Mean	128	128	97	97
N. of cases	7,304	7,090	12,905	12,791
Survey	NREGS	NREGS	NREGS	NREGS

This table shows treatment effects on wage outcomes from the private labor market using data from the NREGS sample only. The outcome "Wage realizations" in columns 1-2 is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. The outcome "Reservations wages" 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 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. Observations in the top .5% percentile based on private sector wage or reservation wage in treatment and control are excluded in all regressions. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline (May 31 to July 4, 2010). All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as control variable. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Wage Spillovers

(a) Residuals in Control Villages

	Wage realizations (Rs.)					Endline Wage realizations (Rs.)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fraction GPs treated > 0.5	-0.7	8.1*	9.6**	4.0	3.0	1.4	11.1**	12.0***	6.5*	4.4
	(5.5)	(4.7)	(4.2)	(3.9)	(3.9)	(5.6)	(4.8)	(4.3)	(4.0)	(4.0)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Control	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adj R-squared	.00	.01	.02	.00	.00	.01	.02	.03	.01	.00
N. of cases	236	236	236	236	236	236	236	236	236	236
Level	GP	GP	GP	GP	GP	GP	GP	GP	GP	GP
Distance	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km

(b) Residuals in Treatment Villages

	Wage realizations (Rs.)					Endline Wage Realizations (Rs.)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fraction GPs treated > 0.5	5.9**	4.4 **	4.5**	5.6***	4.4***	4.8	4.0	4.8**	5.9***	4.7**
	(2.9)	(2.4)	(2.2)	(2.2)	(2.2)	(2.9)	(2.4)	(2.2)	(2.2)	(2.3)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Control	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adj R-squared	.01	.00	.01	.01	.00	.00	.00	.01	.01	.01
N. of cases	593	593	593	593	593	593	593	593	593	593
Level	GP	GP	GP	GP	GP	GP	GP	GP	GP	GP
Distance	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km

This table analyzes the impact of spatial exposure to treatment on private wage for treatment and control villages at radius 10, 15, 20, 25, and 30 kilometers. All analysis was conducted separately for the treatment and control samples. For Columns 1-5, we use residuals from a linear regression of average endline wages on average baseline wages using data from the NREGS household survey. The residuals were calculated by subtracting the fitted value of the regression from the mean endline wages at the village level. The outcome "Wage realizations" is the average daily wage (in Rs.) an individual received while working for someone else in June 2010 (baseline) / 2012 (endline). We then regressed these residuals on an indicator variable "Fraction GPs treated > 0.5". The "Fraction GPs treated > 0.5" is an indicator that is equal to 1 if the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals is greater than 0.5. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are included in both the denominator and numerator. The GPs included are from the entire GP sample used in randomization. For Columns 6-10, we use residuals from a linear regression of average endline wages on district fixed effects using data from the NREGS endline household survey. The residuals were calculated by subtracting the fitted value of the regression from the mean endline wages at the village level. The outcome "Endline Wage realizations" is the average daily wage (in Rs.) an individual received while working for someone else in June 2012 (endline). We then regressed these residuals on an indicator variable "Fraction GPs treated > 0.5". Observations in the top .5% percentile based on private sector wage or reservation wage in treatment and control (for both endline and baseline) are excluded in all regressions. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Employment (June)

	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)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.09	.10	.01	.02	.06	.07
Control Mean	8.2	8.2	7.9	7.9	17	17
N. of cases	10,504	10,431	14,514	14,429	14,163	14,078
Survey	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS

This table analyzes different labor supply outcomes using endline survey data from the NREGS sample. "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. Finally, "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 the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Employment Spillovers

(a) Residuals in Control Villages

	Days worked private sector					Endline Days worked private sector				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fraction GPs treated > 0.5	-0.4	-0.5	-1.2*	-1.2**	-0.2	-0.6	-0.7	-1.4**	-1.8***	-0.4
	(0.8)	(0.7)	(0.6)	(0.6)	(0.6)	(0.8)	(0.7)	(0.7)	(0.6)	(0.6)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Control	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adj R-squared	.00	.00	.01	.02	.00	.00	.00	.02	.03	.00
N. of cases	246	246	246	246	246	246	246	246	246	246
Level	GP	GP	GP	GP	GP	GP	GP	GP	GP	GP
Distance	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km

(b) Residuals in Treatment Villages

	Days worked private sector					Endline Days worked private sector				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fraction GPs treated > 0.5	-0.3	-0.2	0.2	-0.1	-0.3	-0.4	-0.2	0.2	0.0	-0.3
	(0.4)	(0.3)	(0.3)	(0.3)	(0.3)	(0.4)	(0.3)	(0.3)	(0.3)	(0.3)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Control	Yes	Yes	Yes	Yes	Yes	No	No	No	No	No
Adj R-squared	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
N. of cases	606	606	606	606	606	606	606	606	606	606
Level	GP	GP	GP	GP	GP	GP	GP	GP	GP	GP
Distance	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km

This table analyzes the impact of spatial exposure to treatment on private labor market outcomes at radius 10, 15, 20, 25, and 30 kilometers. The analysis was conducted separately for treatment and control villages. "Days worked private sector" is the number of days an individual worked for somebody else in June 2010 (baseline) / 2012 (endline). In columns 1-5, we used residuals from from a linear regression of average endline outcomes on average baseline outcomes and district fixed effects using data from the NREGS household survey. We then regressed these residuals on an indicator variable "Fraction GPs treated > 0.5". The "Fraction GPs treated > 0.5" is an indicator that is equal to 1 if the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals is greater than 0.5. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are included in both the denominator and numerator. The GPs included are from the entire GP sample used in randomization. "Endline Days worked private sector" is the number of days an individual worked for somebody else in June 2012 (endline). For Columns 6-10, we used residuals from from a linear regression of average endline outcomes on average baseline outcomes and district fixed effects using data from the NREGS endline household survey. We then regressed these residuals on an indicator variable "Fraction GPs treated > 0.5". Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Migration

	Did migrate?		Days migrated		Hhd size		Migration common in May?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	.024	.023	1.1	.75	.059	.054	.047	.049
	(.017)	(.018)	(4.9)	(5.1)	(.1)	(.1)	(.055)	(.038)
BL GP Mean		.13		.3		.044		
		(.093)		(.19)		(.048)		
Migration common prior to NREGS								.54***
								(.044)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.03	.01	.02	.02	.02	.12	.45
Control Mean	.075	.075	16	16	4.3	4.3	.21	.21
N. of cases	4,907	4,873	4,943	4,909	4,943	4,909	809	808
Level	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	GP	GP

This table illustrates treatment effects on various measures of migration using survey data from the NREGS sample as well as from 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. Last year refers to the respective time period from the point of the endline survey (May 28 to July 15, 2012). 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. All these outcomes are taken from the household survey. 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 ever since the implementation of NREGS. "Migration common prior to NREGS" 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" and "after NREGS" do not refer to the Smartcards intervention but to the rollout of the entire employment guarantee scheme. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Land utilization and irrigation

	Irrigated land (ac.)	Total land (ac.)	Total fallows (%)	Non-agri. use (%)	Net area sown (%)	Net area irrigated (%)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.043	-.12	-.74	-.83	1.1	.0018
	(.038)	(.14)	(1.2)	(1.3)	(1.6)	(.01)
Age head of hhd	.0049***	.019***				
	(.00028)	(.001)				
Illiterate	-.12***	-.38***				
	(.015)	(.051)				
SC/ST	-.16***	-.53***				
	(.018)	(.072)				
BL GP Mean			.0074	.48***	.49***	.91***
			(.0092)	(.075)	(.046)	(.04)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.02	0.04	0.62	0.62	0.88	0.83
Control Mean	.46	1.7	11	9.1	28	18.4
N. of cases	1.8 M	1.8 M	154	154	154	154
Level	Hhd	Hhd	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. "Illiterate" is an indicator for whether the head of the household is illiterate while ST/SC is an indicator for whether the household belongs to Scheduled Castes/Tribes - historically discriminated against sections of the population. "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 agri. use area" 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. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9: Savings, assets and loans

	Total savings (Rs.)		Total loans (Rs.)		Owns land (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1,064	1,120	11,210**	11,077**	.056**	.049**
	(859)	(877)	(4,741)	(4801)	(.024)	(.024)
BL GP Mean		.027		.038		.21***
		(.071)		(.039)		(.042)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.00	.00	.01	.01	.01	.03
Control Mean	2,966	2,966	68,108	68,108	.59	.59
N. of cases	4,916	4,882	4,943	4,909	4,921	4,887
Survey	NREGS	NREGS	NREGS	NREGS	NREGS	NREGS

This table analyzes household assets using endline survey data from the NREGS survey. "Total saving (Rs.)" is defined as the total amount of a household's current cash savings (in Rs), including money kept in bank accounts or Self-Help Groups. The dependent variable in columns 3-4 is the total principal of the household's five largest active loans (in Rs). "Owns land (%)" is an indicator for whether a household reports to own any land. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10: Adjusting treatment for spatial spillovers using restricted samples

(a) Control Group restricted to exposure ≤ 0.5 and Treatment Group restricted to exposure ≥ 0.5

	Wage realizations (Rs.)					Reservation wage (Rs.)					Days worked private sector					Days idle/unpaid					Total Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)
Treatment	10**	8.7*	12**	12**	9.8*	8.3***	6.7**	8.3***	7.9**	5	.41	.81	.67	.55	.73	-1.1*	-1.5**	-1.5**	-1.7**	-1.5*	10,004**	10,035**	6,749*	7,498*	7,703
	(4.4)	(4.6)	(4.7)	(5.2)	(5.3)	(3.1)	(3.1)	(3.1)	(3.4)	(4)	(.59)	(.58)	(.62)	(.69)	(.74)	(.6)	(.61)	(.65)	(.72)	(.83)	(4,079)	(4,240)	(3,996)	(4,441)	(5,027)
BL GP Mean	.16***	.21***	.21***	.19***	.24***	.099**	.14***	.11**	.12**	.13**	.22***	.25***	.24***	.19**	.24***	.1*	.079	.034	.02	.095	-.012	-.014	.039	.054	.12
	(.06)	(.057)	(.059)	(.065)	(.059)	(.048)	(.046)	(.046)	(.05)	(.056)	(.078)	(.073)	(.077)	(.08)	(.083)	(.059)	(.061)	(.06)	(.055)	(.062)	(.076)	(.081)	(.079)	(.087)	(.1)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.05	.06	.07	.06	.07	.03	.04	.04	.04	.04	.02	.02	.02	.02	.03	.07	.08	.07	.07	.09	.04	.05	.04	.04	.03
Control Mean	134	134	134	134	134	103	103	103	103	103	8.1	8.1	8.1	8.1	8.1	17	17	17	17	17	68899	67928	69508	69622	71640
N. of cases	6,008	5,354	4,763	4,167	3,624	10,771	9,535	8,316	7,194	6,316	12,182	10,777	9,498	8,261	7,305	11,873	10,544	9,271	8,043	7,115	4,107	3,647	3,204	2,779	2,445
Distance	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km

(b) Control Group restricted to exposure ≤ 0.25 and Treatment Group restricted to exposure ≥ 0.75

	Wage realizations (Rs.)					Reservation wage (Rs.)					Days worked private sector					Days idle/unpaid					Total Income				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)
Treatment	9.8*	8.2	11*	12	10	8.4***	8.1**	6.6	2.6	3.6	1.2*	.46	1.1	1.8	4*	-1.7**	-1.4	-2	-4.4**	-6.4***	11,998***	12,702**	14,551**	19,998***	28,592***
	(5.6)	(6.8)	(6.3)	(7.1)	(9)	(3.1)	(3.8)	(4.4)	(4.4)	(5.9)	(.66)	(.93)	(1.1)	(1.8)	(2.2)	(.68)	(.96)	(1.3)	(1.9)	(2.3)	(3,780)	(5,145)	(5,778)	(5,864)	(7,223)
BL GP Mean	.21***	.23***	.25***	.24***	.24***	.071	.071	.011	-.007	.044	.29***	.2*	.29*	.24	.35*	.12*	.11	.097	.065	.091	.066	.14	.11	.25*	.28
	(.068)	(.064)	(.075)	(.056)	(.058)	(.049)	(.054)	(.054)	(.051)	(.042)	(.085)	(.11)	(.15)	(.22)	(.2)	(.069)	(.076)	(.1)	(.14)	(.14)	(.081)	(.093)	(.14)	(.15)	(.18)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.08	.09	.09	.11	.09	.04	.07	.08	.07	.07	.03	.03	.04	.06	.08	.08	.08	.06	.08	.09	.05	.04	.06	.07	.13
Control Mean	134	134	134	134	134	103	103	103	103	103	8.1	8.1	8.1	8.1	8.1	17	17	17	17	17	66,559	65,844	63,557	65,189	64,768
N. of cases	3,885	2,587	1,654	1,122	918	6,947	4,592	2,919	2,017	1,617	7,827	5,195	3,219	2,186	1,752	7,658	5,060	3,120	2,139	1,728	2,666	1,730	1,080	746	599
Distance	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km	10km	15km	20km	25km	30km

This table analyzes spatial proximity effects on labor market outcomes and total annualized household income using restricted samples from the NREGS survey. For Panel a), observations were only included if they were from control villages with ≤ 0.5 spatial exposure to treatment or treatment villages with ≥ 0.5 spatial exposure to treatment at a given distance. For Panel b), observations were only included if they were from control villages with ≤ 0.25 spatial exposure to treatment or treatment villages with ≥ 0.75 spatial exposure to treatment at a given distance. The analysis was conducted at distance 10 km, 15 km, 20 km, 25 km, and 30 km. The spatial exposure measure is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are included in both the denominator and numerator. The GPs included in this calculation are from the entire sample of mandals used in randomization. "Wage Realization" is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. "Reservation Wage" 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. Finally, "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.). "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses.

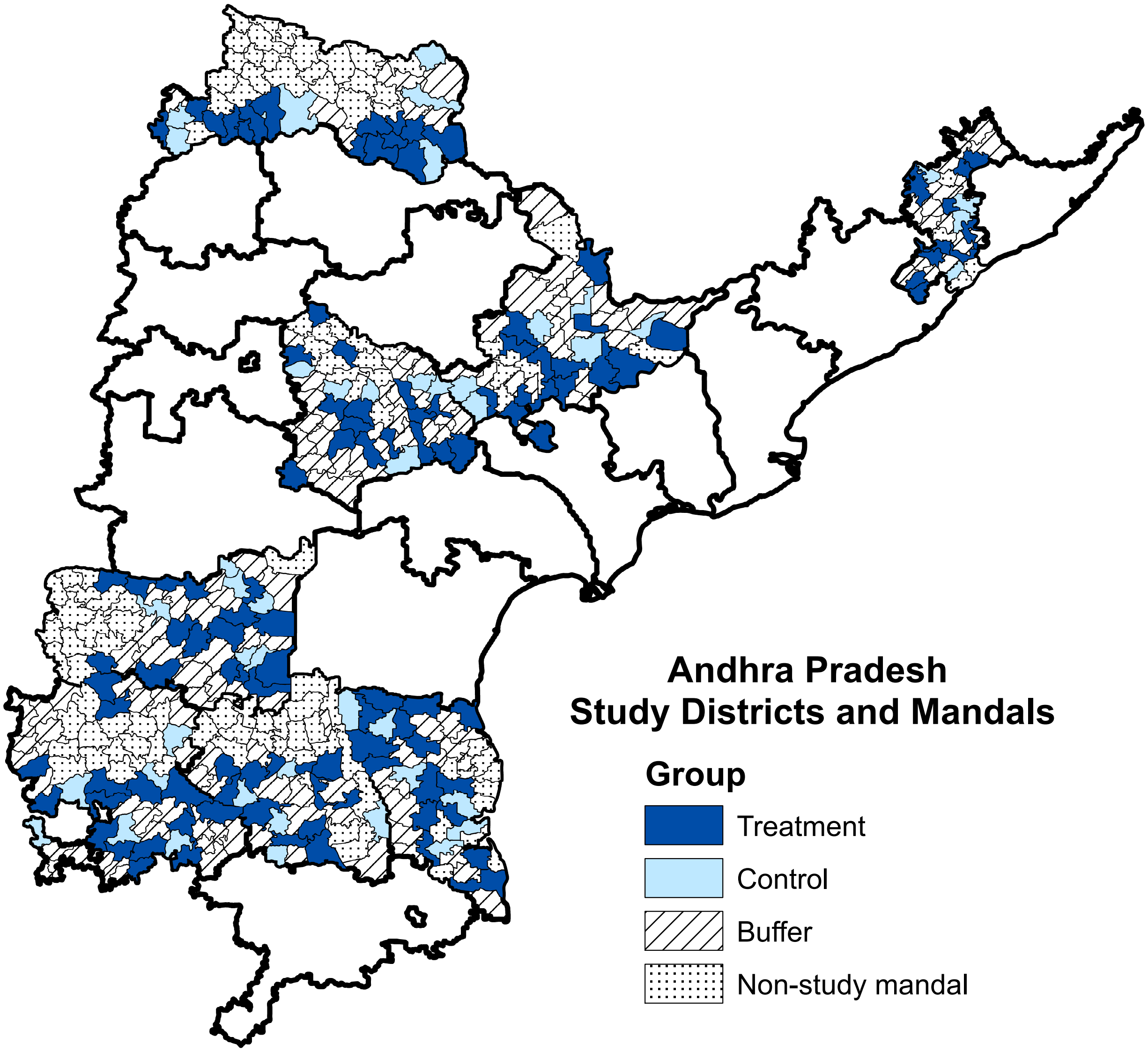
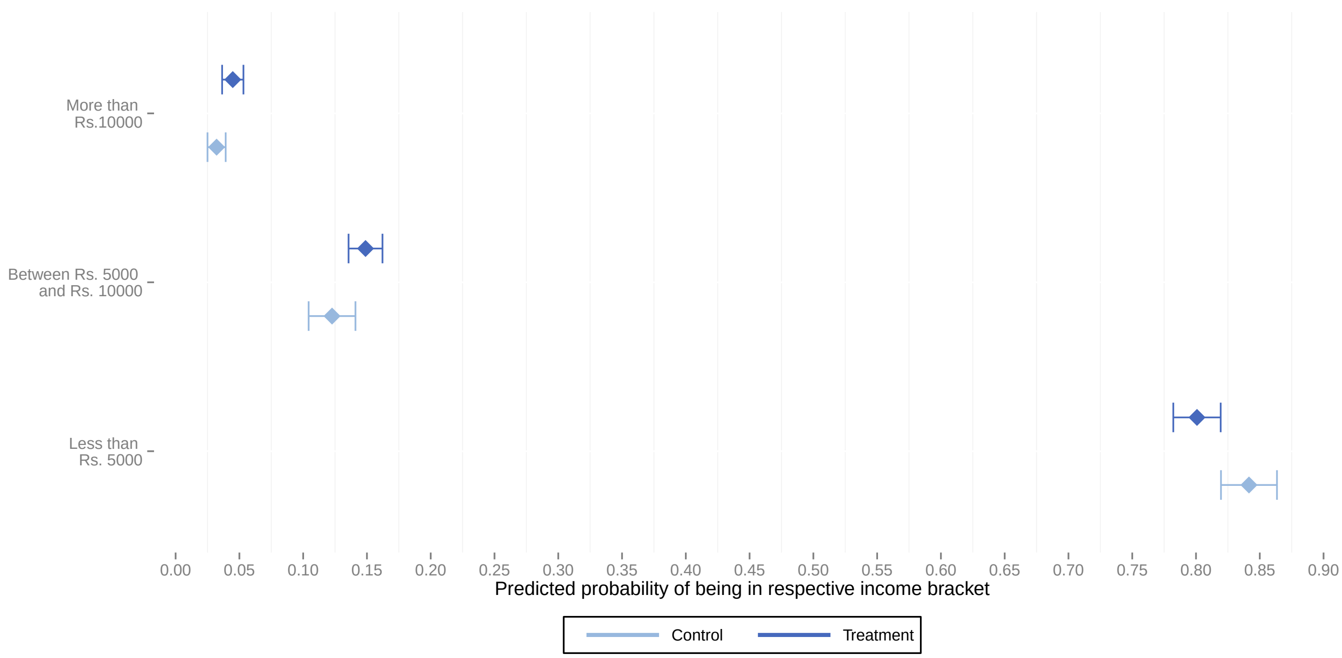


Figure 1: Study districts with treatment and control mandals

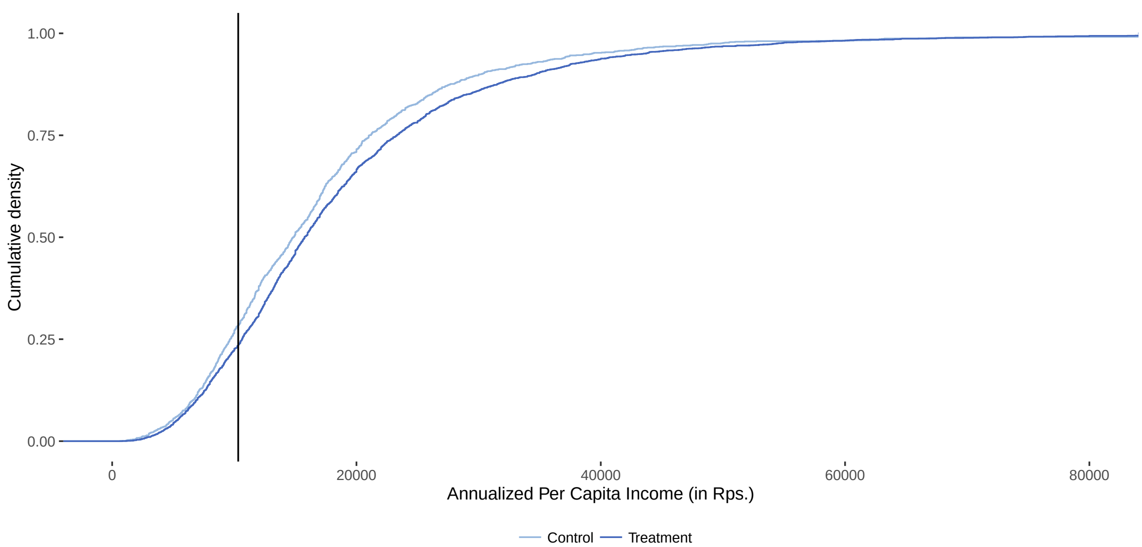
This map (reproduced from Muralidharan et al. (2016)) shows the 8 study districts - Adilabad, Anantapur, Kadapa, Khammam, Kurnool, Nalgonda, Nellore, and Vizianagaram - and the assignment of mandals (sub-districts) within those districts to one of four study conditions. Mandals were randomly assigned to one of three waves: 112 to wave 1 (treatment), 139 to wave 2, and 45 to wave 3 (control). Wave 2 was created as a buffer to maximize the time between program rollout in treatment and control waves; our study did not collect data on these mandals. A "non-study mandal" is a mandal that did not enter the randomization process because the Smartcards initiative had already started in those mandals (109 out of 405). Randomization was stratified by district and by a principal component of mandal characteristics including population, literacy, 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: Effects on income: SECC



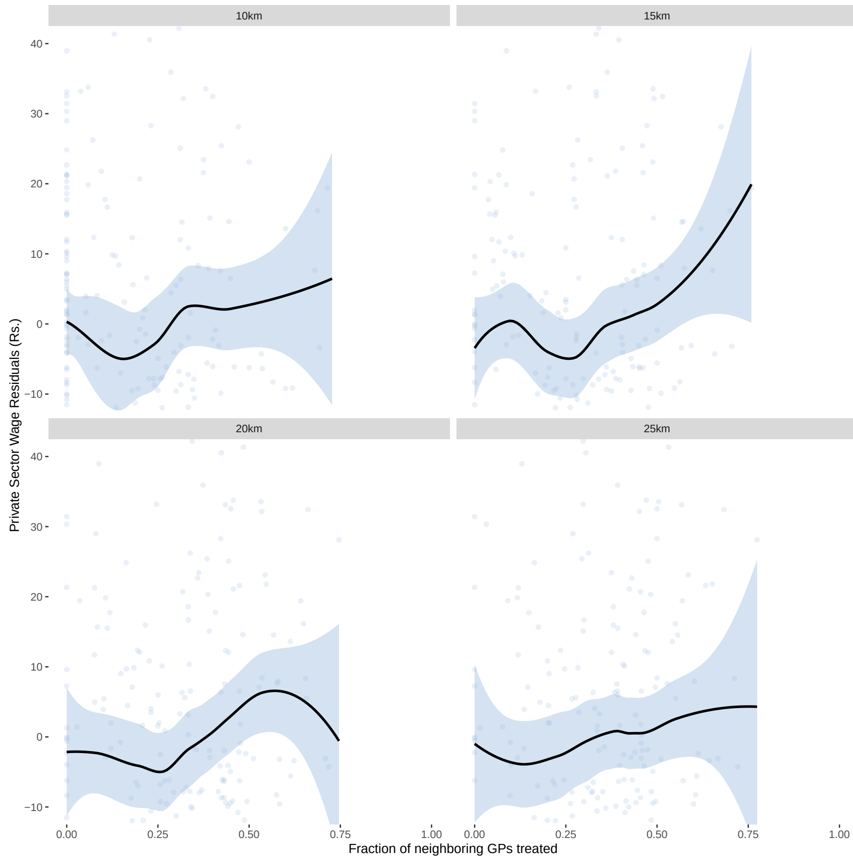
The figure shows the predicted probabilities of being in one the three income brackets in the Socioeconomic and Caste Census (SECC) 2011 (enumeration started in late June 2011) for treatment and control households. The solid rectangular shape indicates the level of the predicted probability for treatment and control respectively, holding all other covariates in the models at their estimation sample mean. The predicted probabilities are derived from the models shown in Table 1a columns 2,4 and 6, i.e., a logit model in which the outcome is a binary indicator for being in the respective income bracket. In addition to a treatment indicator, the model contains controls for the age and literacy of the head of the household as well as ST/SC status (ST/SC refers to Schedules Castes/Tribes, which are historically discriminated against section of the population). Finally, all regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable and district fixed effects. Note that the model was fit using a cluster-robust variance-covariance matrix. The error bars indicate a 95% confidence around the predicted probability.

Figure 3: Annualized Per Capita Income



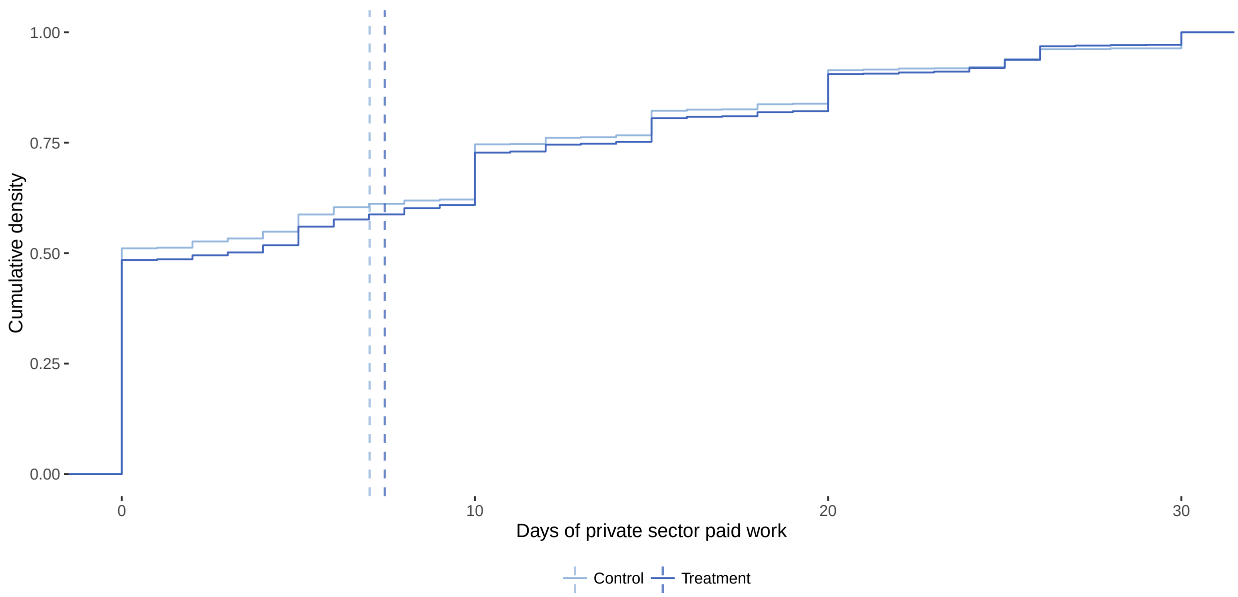
This figure shows an empirical cdf of total annualized per capita income by household for treatment and control groups using data from the endline NREGS household survey.

Figure 4: Private Sector Wage Residuals in Control Villages



This figure shows the residuals from the regression of average endline private sector wages on average baseline private sector wages as a function of spatial exposure at distances 10, 15, 20, and 25 km using the NREGS household survey. The regressions include district fixed effects. Observations in the top .5% percentile based on private sector wage or reservation wage in treatment and control are excluded in all regressions. The spatial exposure measure is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are included in both the denominator and numerator. The GPs included in this calculation are from the entire sample of mandals used in randomization. The curves are fit by a LOESS smoothing function with bootstrapped standard errors.

Figure 5: 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, based on data from the endline NREGS household survey. The dashed lines indicate in-sample means (not weighted by sampling probabilities) in treatment and control, respectively.

Table A.1: Baseline balance at Mandal Level

	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	45,580	45,758	-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	3,392	3,727	-336	.35

This table compares official data on baseline characteristics across treated and control mandals. Column 3 reports the difference in treatment and control means, while column 4 reports the p-value on the treatment indicator from simple regressions of the outcome with district fixed effects as the only controls. A "jobcard" is a household level official enrollment document for the NREGS program. "SC" ("ST") refers to Scheduled Castes (Tribes), historically discriminated-against sections of the population now accorded special status and affirmative action benefits under the Indian Constitution. "Old age", "weaver", "disabled" and "widow" are different eligibility groups within the SSP administration. "Working" is defined as the 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 definitions are from the official census documentation. The last set of variables is taken from 2001 census village directory which records information about various facilities within a census village (the census level of observation). "# primary schools per village" and "Avg. village size in acres" are simple mandal averages (while the others are simple percentages) of the respective variable. Sampling weights are not needed since all villages within a mandal are used. Note that we did not have this information available for the 2011 census and hence used 2001 census data. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.2: Baseline balance at Household Level

	Treatment	Control	Difference	p-value
	(1)	(2)	(3)	(4)
Hhd 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 Hhd	.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**
Hhd 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
# addi. days hhd wanted NREGS work	15	16	-.8	.67

This table compares NREGS household survey data on baseline characteristics across treatment and control mandals. Columns 3 reports the difference in treatment and control means, while columns 4 reports the p-value on the treatment indicator from a simple regressions of the outcome with district fixed effects as the only controls. "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.3: Income gains heterogeneity by household characteristics

	Total income		NREGS		Ag. labor		Other labor		Farm		Business		Misc	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treatment	11,653**	9,823**	331	427	4,151***	4,083***	4355***	3,862***	3,372	1,545	-458	-955	-109	1199
	(4,786)	(3,933)	(657)	(489)	(1,577)	(1,481)	(1,515)	(1,350)	(2,916)	(2,536)	(1,577)	(1,580)	(2,805)	(2,148)
Hhd fraction eligible for SSP	-36,007***		2,521**		-5,942***		-7,120***		-12,258***		-5,442***		-10,835**	
	(6531)		(1,015)		(1,977)		(2,093)		(4,542)		(1616)		(4,218)	
Head of hhd is widow		-18,375***		-111		2,709		-1,983		-16,232***		-5,311***		592
		(6,565)		(644)		(2,605)		(2,127)		(2,811)		(1,710)		(3,581)
Hhd fraction eligible for SSP x Treatment	-13,986		3,178		-4,745*		-5,860**		-6,553		-256		3346	
	(8,753)		(3,688)		(2,509)		(2,583)		(5,618)		(2,154)		(5,135)	
Head of hhd is widow x Treatment		-6,318		3,429		-6,358*		-4,402*		3,218		2,814		-2,813
		(8,102)		(2,614)		(3,307)		(2,629)		(3,788)		(2,147)		(4,177)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BL GP Mean	Yes	Yes	No	No	No	No	No	No	No	No	No	No	No	No
Adj R-squared	.083	.057	.027	.021	.076	.062	.084	.065	.027	.028	.013	.011	.017	.013
Control Mean	69,122	69,122	6,053	6,053	14,798	14,798	9,322	9,322	20,361	20,361	6,202	6,202	12,386	12,386
N. of cases	4,875	4,814	4,909	4,848	4,909	4,848	4,909	4,848	4,909	4,848	4,909	4,848	4,909	4,848

In this table we analyze whether a households's potential ability to perform manual labor affects the outcomes of the intervention. We consider various types of income using annualized household data from the endline household survey for the NREGS sample. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline (due to availability only included in columns 1-2). "NREGS" are earnings from the NREGS program. "Ag 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. In panel a), "Hhd fraction eligible for SSP" is the fraction of household members who identify as eligible for SSP, though they may not actually receive pension. "Hhd fraction eligible for SSP x Treatment" and "Head of hhd is widow x Treatment" are interaction terms constructed by multiplying the respective variable with the binary treatment indicator. In panel b), "Disabled" is an indicator for whether this sampled pensioner in this household is eligible for disabled persons pensions (Rs. 500 per month at the time of the study). "Disabled or OAP" indicates households in which the sampled pensioner is eligible for disabled persons *or* old age pensions (at the time of the study, Rs. 200 for people of age 65-79 and Rs. 500 for people above 79 per month). Again, both these variables are also included as a linear interaction with the treatment indicator. Note that pension scheme was identified from official rosters rather than from the household survey. All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Robustness check for income gains

(a) Including Total Income Outliers

	Total income		NREGS	Ag labor	Other labor	Farm income	Business	Misc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	10,308**	9,580**	905	3,675**	4,471***	1,738	-773	293
	(4,638)	(4,628)	(589)	(1,485)	(1,585)	(2,704)	(1,359)	(2,437)
BL GP Mean		.055						
		(.05)						
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.03	.02	.04	.03	.01	.01	.01
Control Mean	71,935	71,935	4,743	14,784	9,315	21,708	6,620	14,765
N. of cases	4,932	4,898	4,931	4,932	4,932	4,932	4,932	4,932

In this table, we perform robustness checks for Table 1b, which shows treatment effects on various types of income using annualized household data from the endline household survey for the NREGS sample. In Panel a), we run regressions on the NREGS data but do not removed the households in the top .5% percentile based on total annualized income in treatment and control households. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. "NREGS" is the earnings from NREGS. "Ag 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 the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Robustness checks for private sector wage outcomes

(a) Including wage outliers

	Wage realizations (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)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.05	.05	.03	.03
Control Mean	131	131	99	99
N. of cases	7326	7112	12955	12841
Survey	NREGS	NREGS	NREGS	NREGS

(b) Restricting sample to age 18-65

	Wage realizations (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.2*	7.7**	4.7	5.4*
	(3.7)	(3.7)	(3)	(2.8)
BL GP Mean		.16***		.098***
		(.048)		(.033)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.07	.07	.05	.05
Control Mean	128	128	97	97
N. of cases	7,162	7,000	12,677	12,647
Survey	NREGS	NREGS	NREGS	NREGS

(c) Excluding respondents who did not work during June

	Wage realizations (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)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.07	.07	.05	.05
Control Mean	128	128	97	97
N. of cases	7,256	7,043	12,859	12,745
Survey	NREGS	NREGS	NREGS	NREGS

In this table, we perform robustness checks for Table 3, which shows treatment effects on wage outcomes from the private labor market using data from the NREGS endline household survey . In Panel a), we include observations in the top .5% percentile based on private sector wage or reservation wage in treatment and control are included in all regressions. In Panel b), the sample is restricted to respondents in age 18 to 65 and observations in the top .5% percentile based on private sector wage or reservation wage in treatment and control are excluded in all regressions. In Panel c), we drop observations from survey respondents who have did not work in the month of June and observations in the top .5% percentile based on private sector wage or reservation wage in treatment and control are excluded in all regressions. The outcome in columns 1-4 is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. In columns 5-8, the outcome 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 (May 31 to July 4, 2010). All regressions include the first principal component of a vector of mandal characteristics used to stratify randomization as control variable. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Non-response rates by treatment status

(a) Using the full sample

	Treatment	Control	Difference	p-value	N
Wage realizations (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
Avg. wage ≥ reservation wage	.98	.99	-.0029	.57	7287

(b) People of working age (18-65)

	Treatment	Control	Difference	p-value	N
Wage realizations (Rs.)	.013	.012	.0014	.68	7102
Reservation wage (Rs.)	.099	.1	-.0035	.79	21437
Days worked private sector	.057	.056	.0015	.75	21437
Days unpaid	.066	.066	.00031	.96	21437
Days idle	.059	.06	-.00075	.88	21437
Days unpaid/idle	.057	.059	-.0013	.78	21437
Days worked > 0	.54	.52	.016	.44	13211
Avg. wage ≥ reservation wage	.98	.99	-.0025	.62	6974

This table analyzes response rates to key questions regarding labor market outcomes. Columns 1 and 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 realizations" the average daily wage (in Rs.) an individual received while working for someone else in June 2012. "Reservation wage" 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 realizations" 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. Panel b) restricts the sample to individuals of age between 18 and 65 years. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Differential predictors of non-response

	Missing response to				Days worked > 0	Avg. wage > Res. wage
	(1) Wage realizations	(2) Reservation wage	(3) Days worked	(4) Days idle/unpaid	(5)	(6)
Member is female	-.0051	-.0032	-.0016	.0069	-.021	.007
	(.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	-.0054	.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	.0031	.0031	.0031	.034*	.0031
	(.0036)	(.0036)	(.0036)	(.0036)	(.019)	(.0036)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	.011	.011	.011	.011	.49	.011
Avg. number of cases	7386	19349	19349	19349	14458	7275

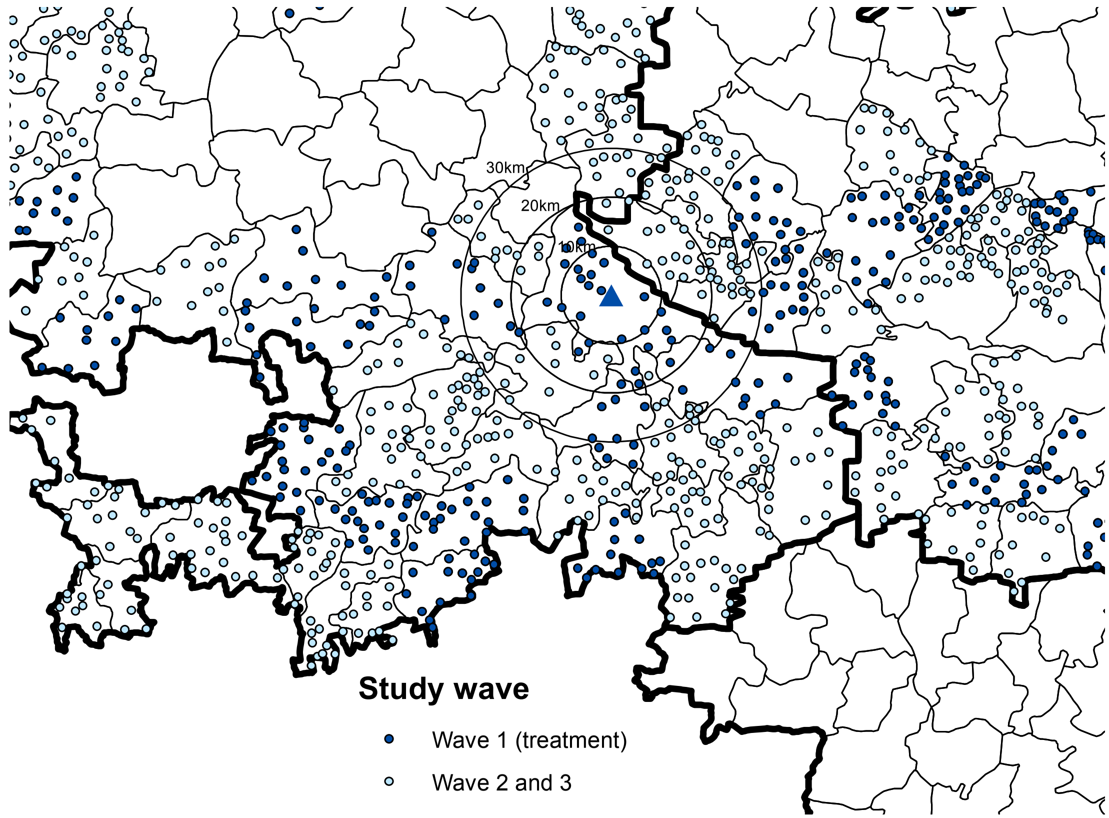
This table analyzes interaction effects between household or GP characteristics and treatment status regarding individual non-response and strictly-positive response rates for private labor market outcomes. In columns 1-4, the outcome in a binary indicator for whether an a survey response is missing when it should not. "Average wage" the average daily wage (in Rs.) an individual received while working for someone else in June 2012. "Reservation wage" 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. Note that 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. "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. that "CardedGP" 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 mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Asset creation through NREGS project types

	# of distinct projects					# days spent workin on				
	(1) Total	(2) Constr.	(3) Irrig.	(4) Land dev.	(5) Roads	(6) Total	(7) Constr.	(8) Irrig.	(9) Land dev.	(10) 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***	.099***	.4***	.068*	.23***	.36***	.11
	(.075)	(.089)	(.021)	(.23)	(.026)	(.039)	(.039)	(.055)	(.063)	(.07)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	.24	.17	.11	.2	.13	.35	.3	.47	.24	.11
Control Mean	32	2.8	1.8	16	.51	6,539	492	1,770	2,606	329
N. of cases	2,837	2,837	2,837	2,837	2,837	2,899	2,837	2,837	2,837	2,837

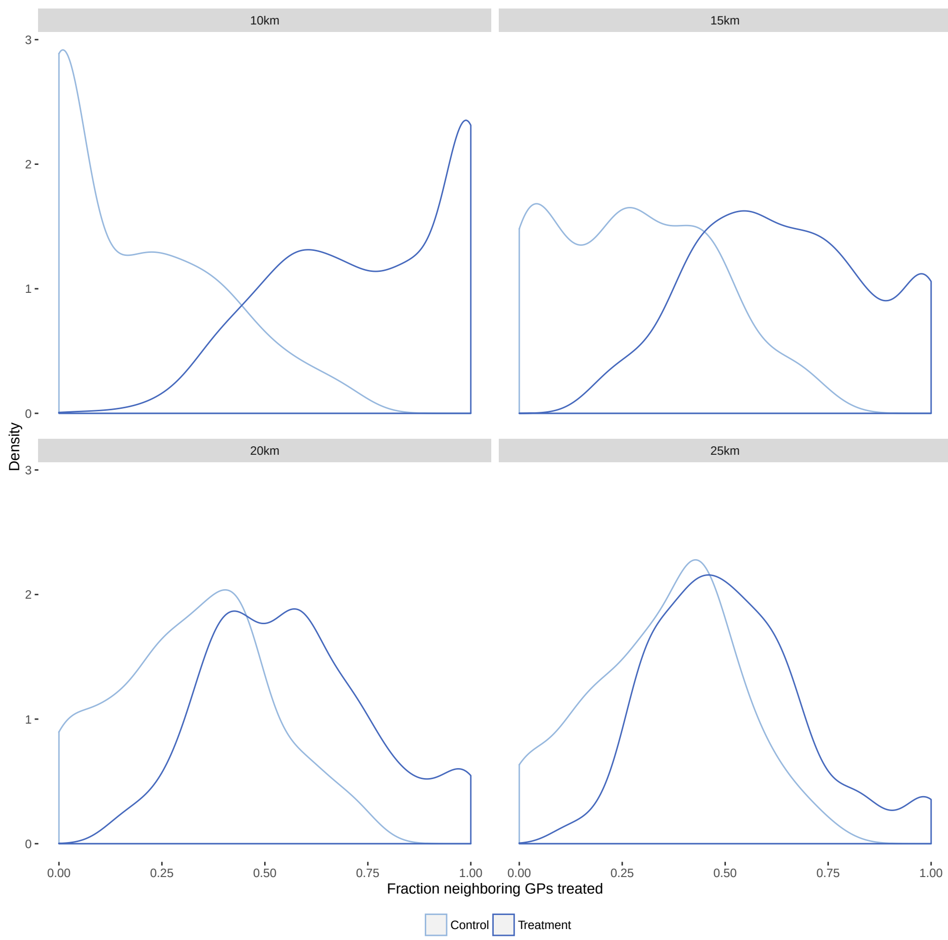
This table analyzes whether treatment helped the creation of productivity-enhancing assets through affecting the type of NREGS projects implemented at the GP-level. The outcomes in columns 1-5 are counts of unique projects in GPs as identified by their project identification numbers in the NREGS muster 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 the first principal component of a vector of mandal characteristics used to stratify randomization as a control variable. Standard errors clustered at mandal level are in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A.1: Constructing a spatial measure of exposure to treatment



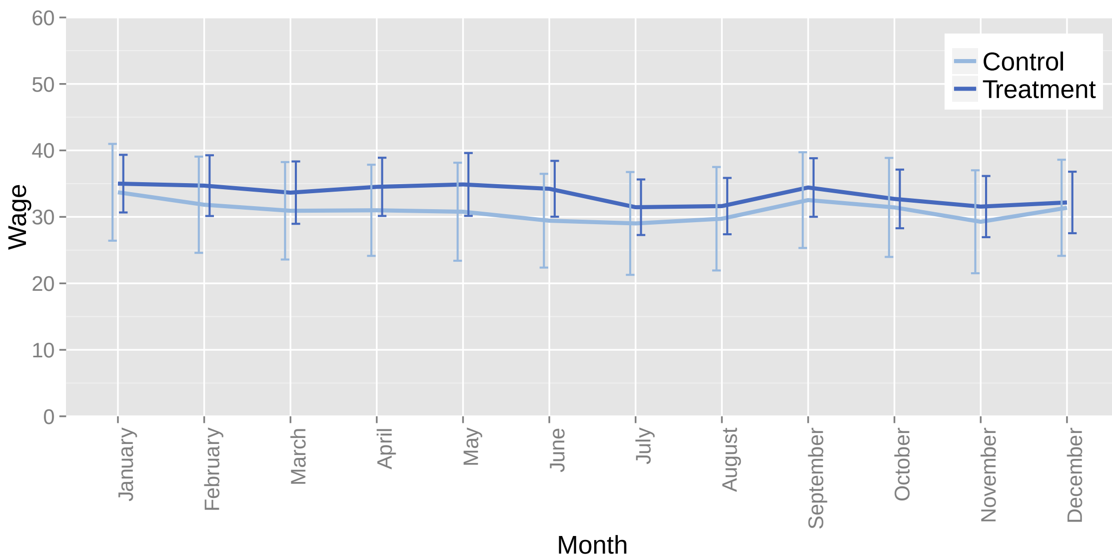
This map shows how the synthetic spatial exposure variable was constructed using the Dorigallu Gram Panchayat in treatment mandal Mudiguba in Anantapur district as an example. Thick borders indicate districts and Anantapur borders Kapada to the East (another study district) and Chittoor (a non-study district) to the South-East. Thin borders indicate mandals. Dark blue dots show the location of GPs in treatment mandals while light blue dots show GPs in wave 2 and wave 3 (control) mandals. Mandals which do not contain any dots where those which were not considered for the randomization since the Smartcard initiative had already started in them. The concentric circles around Dorigallu are of radius 10km, 20km and 30km respectively and correspond to our measures of spatial exposure to treatment used in the analysis. The spatial exposure variable is calculated as the fraction of treatment GPs to total GPs within a given radius, i.e., the number of dark blue dots over the sum of dark blue and light blue dots within a given circle. Importantly, GPs within the same mandal were included in this calculation. This can be seen in the map from the fact that no other GP in Mudiguba mandal is shown. Note that GPs in mandals which were not considered for the randomization (and not shown in this map) were not used in this calculation.

Figure A.2: Density of Spatial Measure of Exposure to Treatment



These figures show smoothed kernel density estimates of each spatial measure of exposure to treatment. Only study GPs where included in this density calculation are from the entire sample of mandals used in randomization. For Panel a), observations were included if they were from in survey GPs with spatial exposure to treatment at a given distance. For Panel b), observations were included if they were from in survey GPs with exogenous spatial exposure to treatment at a given distance. The analysis was conducted at distance 10 km, 15 km, 20 km, and 25 km. The spatial exposure measure is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are 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 x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are *excluded* in both the denominator and numerator.

Figure A.3: Time Series of Difference between Baseline and Endline Wages



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.