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

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Abstract

Public employment programs play a major role in the anti-poverty strategy of many developing countries. Besides the direct wages provided to the poor, such programs are likely to affect their welfare by changing broader labor market outcomes including wages and private employment. These general equilibrium effects may accentuate or attenuate the direct benefits of the program, but have been difficult to estimate credibly. We estimate the general equilibrium effects of a technological reform that improved the implementation quality of Indias public employment scheme on the earnings of the rural poor, using a large-scale experiment which randomized treatment across sub-districts of 60,000 people. We find that this reform had a large impact on the earnings of low-income households, and that these gains were overwhelmingly driven by higher private-sector earnings (90%) as opposed to earnings directly from the program (10%). These earnings gains reflect a 5.7% increase in market wages for rural unskilled labor, and a similar increase in reservation wages. We do not find evidence of distortions in factor allocation, including labor supply, migration, and 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.

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1 Introduction

The majority of the world's poor live and earn in rural areas, and many government anti-poverty programs are focused on the rural poor. 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. [1]

One would expect such programs to have impacts on local markets beyond their direct impacts on participating households. Such general equilibrium effects[2] could either amplify or reduce poverty-reducing effects. On one hand, public works programs may increase wages in the private sector, create productivity-enhancing public goods, have positive aggregate demand effects, and relax credit constraints; on the other, they may crowd-out and distort employment away from the private sector and thus reduce output and income.

Estimating such effects has proven challenging, however. Results from non-experimental studies can be sensitive to methods. On the other hand, experimental studies of employment opportunities have not been conducted with units of randomization large enough to plausibly generate general equilibrium effects.[3] Moreover, any implementation of a program at a scale sufficient to have general equilibrium effects is likely to be imperfect implementation, making the logical construct the effects of a program insufficient, with the more realistic approach being to estimate the effects of varying degrees of implementation quality.

In this paper we exploit an unusually large-scale experiment to examine the impacts of a significant improvement in the implementation of the public employment system in the Indian state of Andhra Pradesh. The National Rural Employment Guarantee Scheme (NREGS) is the largest workfare program in the world, covering 11% of the worlds population and costing 0.8% of Indias GDP. A growing literature has attempted to measure its impacts on a wide variety of economic and social outcomes use non-experimental methods, typically exploiting its staggered roll-out, but these estimates often conflict with each other (see section 2.1.2 below). Moreover, implementation of the scheme has proven so varied and difficult that estimating the effects of the program seems an invalid construct, with the more realistic approach being to assess the impacts of different degrees or qualities of program

[1] Such programs have a long history, with recorded instances from at least the 18th century in India (the Bada Imambara in Lucknow), the public works conducted in the US by the Works Projects Administration during the Depression-era in the 1930s, and more modern "Food-for-Work" programs across Sub-Saharan Africa and Asia.

[2] General equilibrium effects could be distinguished from partial equilibrium effects tautologically: partial equilibrium effects are those that only depend on the treatment status of a single unit, while general equilibrium effects depend on the treatment status of all units in some larger grouping. We lean towards a conceptual definition in which partial equilibrium effects hold constant prices and the "rules of the game" such as broader policies and institutions, while in general equilibrium these can adjust as well.

[3] For example, Beegle et al. (2015)) randomizes job offers at the individual level within communities.

implementation. For example, the most-cited study of wage impacts finds them only in states which it codes as having implemented the program well (Imbert and Papp, 2015).[4]

Working with the Government of the Indian state of Andhra Pradesh,[5] we randomized the order in which 157 sub-districts introduced a new technology (biometric Smartcards) for making payments in NREGS. In prior work we have found 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 measures of access to work, without changing the fiscal outlays on the program (Muralidharan et al., 2015). Thus, the Smartcard intervention brought NREGS implementation closer to what its architects intended it to do. The scale of the experiment, in the sense of large units of randomization (sub-districts with an average population of 60,000), allows us both to examine the general equilibrium market impacts, and potentially to shed light on the structure of rural labor markets in this setting.[6]

We examine how the improvement of the program affected beneficiaries' livelihoods, examining both the direct effects of program improvement as well as indirect effects through local labor markets. We find four main results.

First, we find large increases in household incomes. 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 like that this earner moves out of the lowest income category. Using our cardinal survey data on income, we find a Rs. 8500 (13.4%) increase in income in treatment group households. The results suggest that a well-implemented NREGS can have important poverty alleviation effects.

Second, we find that the vast majority of income gains are attributable to indirect effects rather than increases in program income. For NREGS beneficiaries, increases in program income accounted for only 1/9th of the increases in total income, with the rest attributable to increases in private sector earnings. This underscores the point that the general equilibrium effects of an intervention can dominate its direct effects (Acemoglu, 2010), and demonstrates

[4]This reflects the fact that that implementation issues were still being worked out even two years after NREGS was launched, with issues ranging from program awareness to payment systems to monitoring (Mehrotra, 2008).

[5]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.

[6]Of course, there are important differences between evaluating the roll-out of the program itself and evaluating the consequences of improving its implementation. We discuss this distinction and implications for interpretation briefly below and in detail in Section 2.3.

the value of experiments that are large in the sense that the unit of randomization is large enough to generate such effects. It also related directly to policy debate in India, where critics have argued that participation in NREGS is too low for it to have contributed meaningfully to recent reductions in poverty. On the contrary, if the bulk of the NREGS effects on incomes have been indirect then it may in fact the be the case that the "small tail has wagged "the very very large dog (Bhalla, 2013).

Third, we find that these indirect increases in income are driven by effects on private sector labor markets, and in particular by an increase in the private sector wage. Unskilled rural wages rose by 5.7%, as did reported reservation wages. Earnings from non-wage sources did not increase significantly. This finding is broadly consistent with most of the papers examining the effect of the rollout of NREGS on private sector wages Berg et al. (2012); Imbert and Papp (2015); Azam (2012). It is also noticeable that the wage impact we find from *improving* the NREGS is as large as the largest estimated impact of rolling the scheme out in the first place, underscoring the central importance of implementation quality for policy outcomes.

Finally, we find little evidence of distortionary effects on factor allocation. We do not find crowd-out of private sector labor, at least during the month of June; rather, private sector employment increased insignificantly. This hints at the possibility of a degree of monopsony power in the labor market, but given the precision of our estimates we cannot reject the null of perfectly competitive markets with an elasticity of labor demand smaller than -0.44. We similarly find no impacts on migration, and no effects on various measures of land use. We do find evidence that effects spillover across local labor markets, as one would expect, with effects detectable at radii up to 20 kilometers.

There are four mechanisms that could potentially contribute to producing the increase in private-sector earnings we observe. The simplest is labor market competition between the NREGS and private employers. In this mechanism, improving the NREGS improves workers' outside options and hence their bargaining power vis-a-vis employers. Consistent with this explanation, reservation wages increased 1:1 with actual wage realizations.

A second possibility is that an improved NREGS produced more or better public goods, such as irrigation structures, which then increased private sector productivity. Our data do not easily reconcile with this hypothesis, however. We find impacts on earnings only from wage employment and not from self-employment, for example, and find no effects on the amount of land under cultivation or the amount of irrigated land.

The third possibility is that a better NREGS made it easier for workers to borrow, for example because they had a more reliable source of fallback earnings, and thus relaxed credit constraints. We do see some effects on credit use, but do not see any increases in activities one would expect to be constrained by lack of credit, such as farm or business earnings or educational enrollment. The data thus seem hard to reconcile with a major role for the credit channel.

A final possibility is that the reforms triggered aggregate demand effects. Specifically, increased purchasing power in the hands of the poor could trigger increased local economic activity in the presence of scale economies and transport costs. While this mechanism seems a priori unlikely given that the reform we study did not affect the flow of funds into treatment areas, it is possible that reductions in leakage effectively redistributed money towards workers with a higher propensity to consume local goods. In the data, however, we do not find evidence of significant changes in household expenditure.[7]

Our paper makes contributions to a number of literatures. First, it contributes to a growing literature on the impact of public works programs on rural labor markets and economies (Imbert and Papp, 2015; Beegle et al., 2015). The paper presents estimates of the impact of a well-implemented NREGS that are (1) experimental, (2) around steady-state implementation, and (3) for broad outcomes, in a demographically representative but lead-implementing state. The results also suggest that the theoretical distortions that the program may have been expected to induce do not materialize perhaps because the assumption of no pre-existing market failures may not hold. The results thus also relate to the literature on rural labor markets in developing countries more generally (Rosenzweig, 1978; Jayachandran, 2006), as the more specific literature on the impacts of minimum wages in developing countries (e.g. Dinkelman and Ranchhod (2012)). It is widely thought that wage floors may be hard to enforce without an employer of last resort Dreze and Sen (1991); Basu et al. (2009).

Next, the fact that the reform substantially raised private sector wages highlights the fact that many reforms which could potentially benefit the poor also generate "losers with a vested interest in opposing them. Anderson et al. (2015) have argued, for example, 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." Finally, the paper provides an empirical example of the lack of an equity/ efficiency tradeoff in imperfect markets (Banerjee et al. (2002) being another example).

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 results on income, and discusses mechanisms. Section 5 describes results on resource allocation, while section 6 presents results on spatial spillovers. Section 7 concludes.

[7]This may be because households may view the increase in wage income as temporary, and hence do not consume it.

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 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., 2015). 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., 2015; 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. 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 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.

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 available in Muralidharan et al. (2015); 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. (2015), 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. Moreover, access did not suffer: both perceived access and actual participation in the program increased (17%). We find little evidence of heterogenous impacts, as treatment distributions first order stochastically dominate control distributions for all outcomes on which there was a significant mean impact. 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. (2015) for further details. The experiment was conducted in eight districts[8] 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). We created the buffer group to ensure that we could conduct endline surveys 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. (2015)) 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 24 variables are significantly different at conventional significant levels, which is 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 primary 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

[8]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

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. (2015). 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.[9] 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 were unable to survey or confirm existence of 295 (corresponding numbers for baseline are 8,572, 7,425, 102 and 1,000 respectively).

[9]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.

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."[10]

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:

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

where Y_{imd} is an outcome for household or individual i in mandal m and district d, and $Treated_{md}$ is an indicator for a treatment group mandal. In some cases we use non-linear analogues to this model to handle categorical data (e.g. probit, 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:

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

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. For regressions using survey data, we pool our NREGS and SSP samples without weighting by default to maximize power, and report robustness to other weighting schemes in the Appendix. When using survey data we

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

trim the top 0.5% of observations in both treatment and control groups to remove outliers in the financial data, but our results are robust to including them.

4 Effects on Earnings and Poverty

Figure 1 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. These results survive other robustness checks including using probits or linear probability models for estimation.

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 approximately Rs. 8,500. This is a large effect, equal to 13.4% of the control group mean or 17.5% 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.

The estimated income effects in Table 1b are robust to a number of checks. If we include outliers (the top 0.5% in treatment and control) the estimates are larger and remain significant at the 1% level (not reported). If we examine the NREGS and SSP samples separately, effects remain significant at the 5% level for the (larger) NREGS sample and at the 10% level for the (smaller) SSP sample (Table A.3).

Our survey data also allow us to examine distributional impacts. In Figure 2 we plot the inverse CDF of household earnings for treatment and control groups and difference. 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 as temporary. We return to the latter point below in discussing channels.

4.1 Public or private earnings gains?

Mechanically, the effects on earnings and poverty we find above must work either through increases in households earnings from the NREGS and SSP programs themselves, 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 seven categories: NREGS, SSP, 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 and SSP earnings are only a small proportion of overall income gains, accounting for a mere 5% 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. 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. 7,000 is consistent with other report of program earnings (Imbert and Papp, 2011), making large scale misreporting unlikely.

4.2 Drivers of private-sector earnings

Improved implementation appears to have reduced poverty primarily through indirect effects on private-sector earnings. In this section we examine what the data can tell us about the mechanism(s) through which this effect worked, examining three leading hypotheses in turn.

4.2.1 Labor market competition

An improved NREGS could put competitive pressure on labor markets, driving up wages. 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)).

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. We also 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%

[11]The insignificant effect on program earnings here is not inconsistent with the estimated positive effects on program earnings in Muralidharan et al. (2015). The results in Table 1b lump together NREGS and SSP beneficiaries, unlike those in the companion paper. More importantly, the results in Muralidharan et al. (2015) relate to a specific study period which was just before our survey; we collected detailed data on every week (month in the case of SSP) of program participation and used various methods to prompt recall; 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, without explicit measures to prompt recall.

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.7).

Consistent with the labor market competition hypothesis, we find a significant increase of Rs. 7.3 in private sector wages (Table 3, Columns 1 and 2). This is a large effect, equal to 5.7% of the control group mean and similar to the highest estimates of the wage impacts of the rollout of the NREGS itself. As with income results, wage results are similar when we examine the NREGS and SSP samples separately, though marginally insignificant for the NREGS sample alone.

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.7). Second, we check that the probability of reporting any work is not significantly different between treatment and control groups (A.7). Third, we check composition and find that treatment did not affect composition of those reporting A.8. Finally, we show below that treatment also affected reservation wages, which we observe for nearly the entire sample (89%) of working-age adults.

The central prediction of the labor market competition hypothesis is that wages rise because workers demand higher wages. Our data are consistent with this prediction. We find significant positive effects on reservation wages, similar in magnitude to those on wage realizations (Columns 3 and 4 of of Table 3). Treatment increased reservation wages by approximately Rs. 6, or 6% 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).

4.2.2 Increased productivity

An improved NREGS could also stimulate the creation of productivity-enhancing assets. 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.[12]

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

We do not find any direct evidence of such effects in the SECC or district handbook data. Tables 6 shows no significant effect on the amount of land under cultivation (% area sown or % area fallow) or on the total area irrigated. 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 constructed 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.

On net we find little evidence to support the view that Smartcards increased productivity through incremental asset creation.

4.2.3 Credit constraints

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 A.12), but the hypothesis that productivity increased because of these loans is no easier to reconcile with the results above than the conjecture that it increased due to NREGS asset creation.

4.2.4 Aggregate demand

Improvements in both NREGS and the SSP do increase participants' earnings from the programs. 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).

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 either the NREGS or the SSP (Muralidharan et al., 2015). 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 equal to 1.9% of the control group mean (with the caveat that a 90% confidence interval includes effects as large as a 9% increase). 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.

5 Effects on resource allocation

Given the earnings and in particular the wage effects we report above, an important question is how the intervention affected the allocation of real resources. This is particularly salient for labor: indeed, one of the main concerns about rural public works programs is that they may distort labor away from the private sector (Basu et al., 2009) and away from cities.

We first note that we see no effects on land use, the one measure of resource allocation captured by the SECC. As discussed above, the amount of land under cultivation and the amount irrigated in treatment and control areas are statistically indistinguishable (Table 6).[13]

Next we examine the allocation of labor across sectors, using our survey data (Table 4). We categorize 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 decrease 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 the distributions of private sector days worked in the treatment and control groups, we see

[13]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.

no evidence that upper regions of this distribution are contracting (Figure 3).

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. Indeed, taking the point estimate 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). 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).

Finally, we examine the allocation of labor across space. 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 5 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.[14]

6 The spatial distribution of effects

Thus far we have treated each mandal as an independent observation, assuming no spillovers from treated to control units. In this section 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.

To gauge the extent of spatial spillovers, we construct a village-level measure of exposure to treatment. Specifically, we calculate the fraction of villages that are both (i) within a radius R of the given village and (ii) located in a different mandal, that are also treated. We impose condition (ii) because the treatment status of neighboring villages in the same mandal

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

is identical to own treatment status, so that we cannot separately identify their effects. 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. We construct this measure for radii of 10, 20, and 30 kilometers (disaggregated results available on request). Note that at smaller radii we lose some observations as there are some villages that are more than R kilometers from the nearest neighboring mandal, and hence have no "neighboring villages" in the sense of our metric.

Because our exposure measures are linear functions of the treatment status of other mandals, they should be exogenous by construction. As a precaution we check in Table A.10 whether outcomes of interest are balanced with respect to them at baseline.

Overall, we find strong evidence of spillover effects at distances up to roughly 20km. Table 7) reports results for wages, reservation wages, and labor allocation. In all cases the effect of treating nearby villages has the same sign as the direct treatment effect, and it the majority of cases it is statistically significant. 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.)

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.

Our estimates of direct treatment effects also remain significant, and are larger than those which ignore spillover effects. Taking as our preferred estimates the models with $R = 20$km, which include all but 1.5% of our sample, we estimate direct effects on wages that are 19.4% larger than our earlier estimates (corresponding figures for reservation wages and days not working 11% and 30% respectively). This suggests that positive spillovers do if anything bias downwards our uncorrected estimates.

7 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 gains

(a) SECC data

	Lowest bracket		Middle bracket		Highest bracket		Income bracket 3 levels	
	(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	1,824,537	1,824,506	1,824,537	1,824,506	1,824,537	1,824,506	1,824,537	1,824,506
Estimator	Logit	Logit	Logit	Logit	Logit	Logit	Ordered logit	Ordered logit

(b) Survey data

	Total income		Program earnings	Ag. labor	Other labor	Farm	Business	Misc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	8428***	8553***	453	2426*	2815***	2488	-407	653
	(3105)	(3092)	(744)	(1298)	(1031)	(1884)	(100)	(1511)
BL GP Mean		.15**						
		(.059)						
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.03	.04	.06	.05	.01	.01	.01
Control Mean	63984	63984	6990	14105	8268	17348	5923	11351
N. of cases	8065	7852	8065	8065	8065	8065	8065	8065

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 - 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 and 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. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. "Program Earnings" is the sum of earnings from NREGS and SSP. "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 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 2: Expenditure summary

	Total expenditure		Short-term expenditure		Longer-term expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	6027	4366	362	257	-956	-1330
	(9808)	(9937)	(692)	(709)	(2115)	(2144)
BL GP Mean		.067***		.07***		.004
		(.016)		(.014)		(.0049)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.02	.03	.02	.03	.01	.01
Control Mean	229000	229000	16272	16272	32943	32943
N. of cases	8075	7862	8075	7862	8075	7859
Recall period	1 year	1 year	1 month	1 month	1 year	1 year

This table analyzes different categories of household expenditure using data from both the NREGS and SSP 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. "Total expenditure" is the sum of "Short-term expenditure" and "Longer-term expenditure" where short-term expenditure was annualized. "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 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 3: Private sector wage outcomes in June

	Wage realizations (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.4*	7.2**	5.9**	6.4**
	(3.4)	(3.4)	(2.8)	(2.7)
BL GP Mean		.16***		.096***
		(.041)		(.033)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.05	.05	.03	.03
Control Mean	126	126	96	96
N. of cases	11142	10496	19061	18493

This table shows treatment effects on wage outcomes from the private labor market using data from both, the NREGS and SSP sample. The outcome in columns 1 and 2 is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. In columns 3-4, 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 4: Labor supply

	Days worked on NREGS		Days worked private sector		Days unpaid/idle	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.95	.88	.44	.49	-1.1**	-1*
	(.66)	(.64)	(.54)	(.53)	(.54)	(.54)
BL GP Mean		.14***		.21***		.15***
		(.043)		(.052)		(.042)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.09	.10	.01	.02	.06	.07
Control Mean	8.2	8.2	8.1	8.1	17	17
N. of cases	10504	10431	21705	21216	21236	20753
Survey	NREGS	NREGS	Both	Both	Both	Both

This table analyzes different labor supply outcomes. "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. The SSP survey did not ask about NREGS work outcomes and hence columns 1 and 2 are only based on data from the NREGS sample. 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 5: Migration

	Did migrate?		Days migrated		Hhd size		Migration common in May?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	.023	.022	2.7	2.5	.035	.06	.047	.049
	(.014)	(.014)	(3.8)	(3.9)	(.083)	(.085)	(.055)	(.038)
BL GP Mean		.13*		.27*		.095***		
		(.077)		(.15)		(.033)		
Migration common prior to NREGS								.54***
								(.044)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.03	.03	.01	.01	.02	.02	.12	.45
Control Mean	.071	.071	14	14	4	4	.21	.21
N. of cases	7741	7550	8114	7901	8114	7901	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 and SSP sample as well as from a separately conducted village survey. The outcome in columns 1 and 2 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 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 6: 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	1,826,764	1,826,764	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) 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 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 7: Spatial spillovers for private labor market outcomes

(a) Wage outcomes

	Wage realizations (Rs.)			Reservation wage (Rs.)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	7.7**	8.6***	7.7**	6.1**	7.1***	6.5**
	(3.3)	(3.3)	(3.4)	(2.8)	(2.7)	(2.7)
Fraction GPs treated within 10km	11***			4.3		
	(3.4)			(3)		
Fraction GPs treated within 20km		14**			5.5	
		(6.2)			(4.9)	
Fraction GPs treated within 30km			11			2.5
			(8.5)			(6.8)
BL GP Mean	.15***	.14***	.16***	.1***	.089***	.095***
	(.037)	(.039)	(.04)	(.035)	(.033)	(.033)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.06	.06	.05	.03	.03	.03
Control Mean	126	126	126	96	96	96
N. of cases	9440	10326	10443	16620	18228	18420

(b) Labor supply outcomes

	Days worked			Days idle/unpaid		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	.67	.65	.69	-1.2**	-1.3**	-1.3**
	(.57)	(.55)	(.54)	(.56)	(.55)	(.54)
Fraction GPs treated within 10km	.66			-1		
	(.62)			(.63)		
Fraction GPs treated within 20km		1.7*			-2.2**	
		(1)			(1)	
Fraction GPs treated within 30km			3.1**			-3.8***
			(1.4)			(1.4)
BL GP Mean	.19***	.2***	.2***	.14***	.13***	.14***
	(.055)	(.051)	(.051)	(.044)	(.044)	(.043)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.02	.02	.02	.08	.08	.08
Control Mean	8.1	8.1	8.1	17	17	17
N. of cases	18980	20890	21113	18604	20465	20656

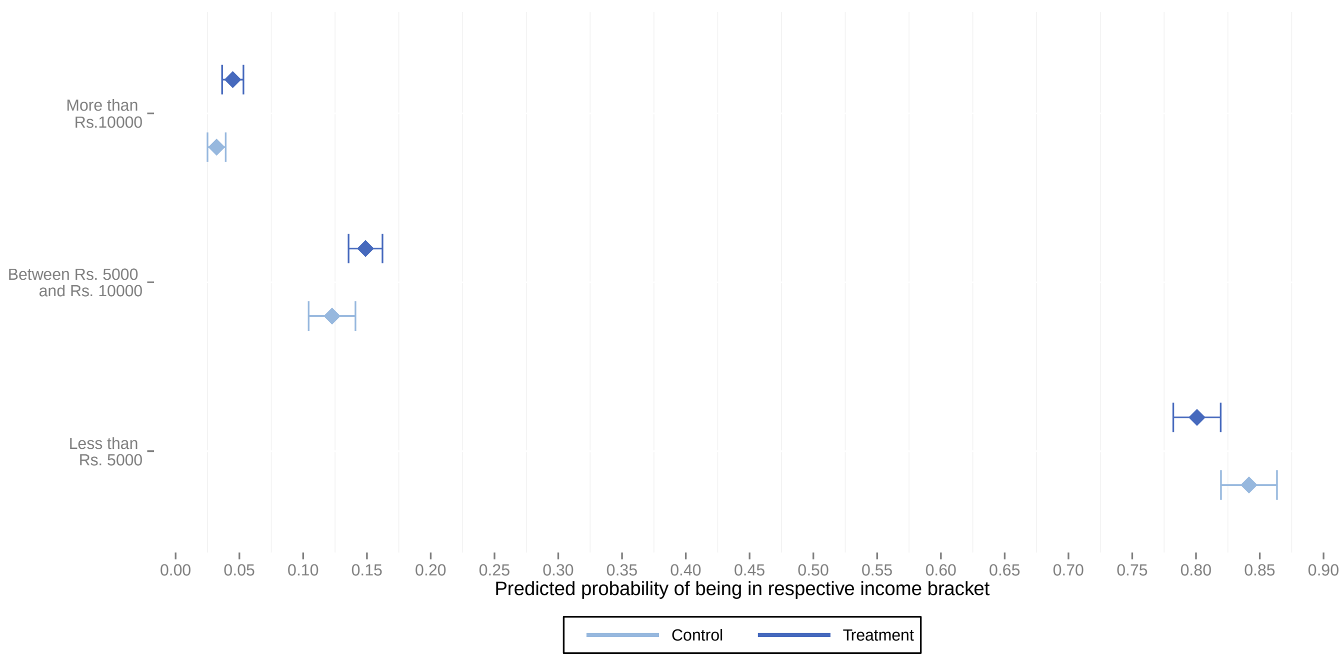
This table analysis spatial proximity effects on private sector labor market outcomes using data from both, the NREGS and SSP sample. Panel a) considers the average daily wage (in Rs.) an individual received while working for someone else in June 2012 in columns 1-3 and an individual's reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012 in columns 4-6. The latter 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. Panel b) analyzes labor supply outcomes. "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. The "Fraction GPs treated within x" is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are excluded in both the denominator and numerator. The number of observations increases in radius because some GPs simply do not have any counted neigboring GPs 10 or 20 kilometers. "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 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 8: Savings, assets and loans overview

	Total savings (Rs.)		Total loans (Rs.)		Owns land (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	396	444	9196**	9228**	.039*	.031*
	(637)	(667)	(3698)	(3709)	(.02)	(.018)
BL GP Mean		.027		.077**		.26***
		(.059)		(.036)		(.032)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.00	.00	.01	.01	.01	.04
Control Mean	3144	3144	62497	62497	.57	.57
N. of cases	8073	7860	8114	7901	8082	7865

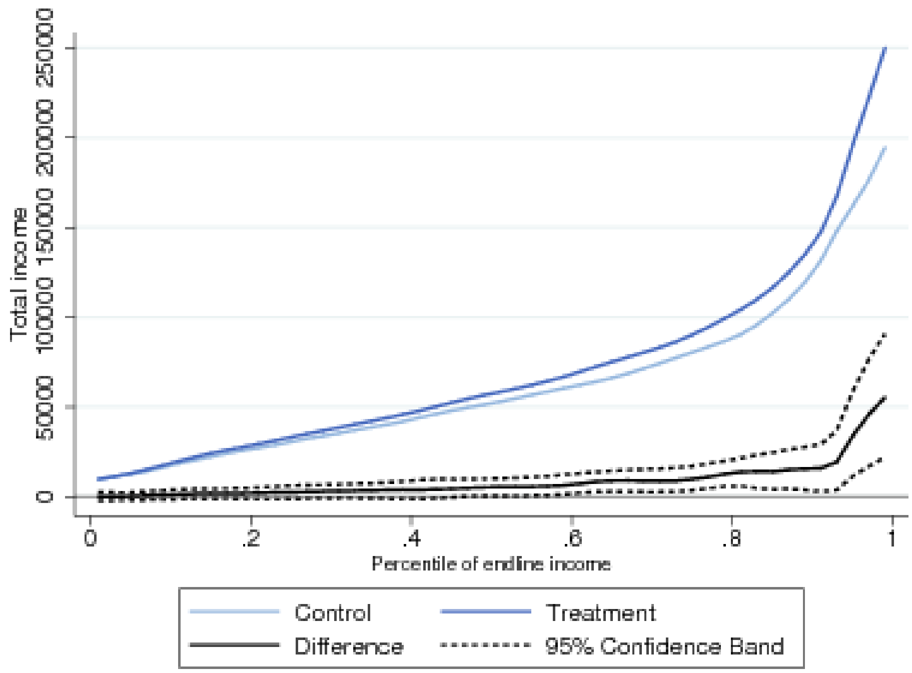
This table analyzes household assets using endline survey data from both, the NREGS and SSP survey. “Total saving (Rs.)” is defined as the total amount in Rupees of a household’s current cash savings, including money kept in bank accounts or Self-Help Groups. The dependent variable in columns 3-4 is the total principal of the five largest rupee loans a household currently has. “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 control variable. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 1: Effects on income: SECC



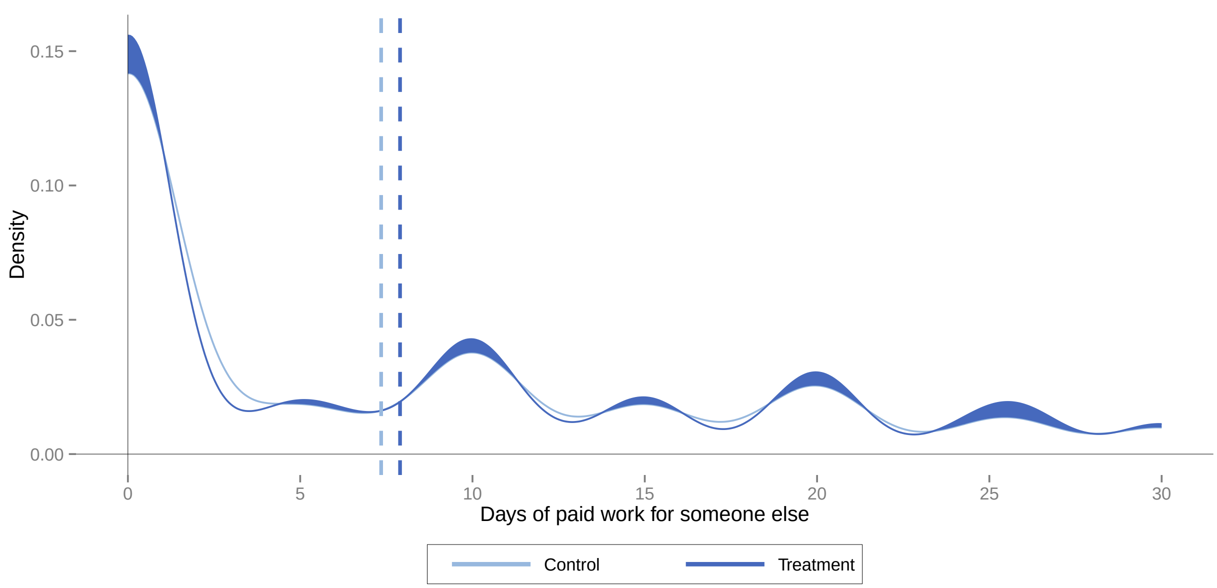
The figure shows the predicted probabilities for treatment and control for the three income brackets in the Socioeconomic and Caste Census (SECC) 2011 (enumeration started in late June 2011). The solid rectangular shape indicates the level of the predicted probability for treatment and control respectively, 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 - 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 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 2: Effects on income by quantile



This figure shows nonparametric treatment effects on total household income taken from the endline household survey. All lines are fit by a kernel-weighted local polynomial smoothing function with Epanechnikov kernel and probability weights, with bootstrapped standard errors. The dependent variable is the vector of residuals from a linear regression of the respective outcome with the first principal component of a vector of mandal characteristics used to stratify randomization and district fixed effects as regressors.

Figure 3: Private sector work in June



This figure shows a kernel density estimate of the number of days an individual worked for someone else during June 2012, based on data from the endline household survey. The densities are smoothed using a Gaussian kernel. The dashed lines indicate in-sample means (not weighted by sampling probabilities) in treatment and control, respectively. The shaded areas indicate the positive difference between the treatment and control densities.

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	45580	45758	-221	.91
% population under age 6	.11	.11	-.00075	.65
% agricultural laborers	.23	.23	-.0049	.59
% female agricultural laborers	.12	.12	-.0032	.52
% marginal agricultural laborers	.071	.063	.0081	.14
	Numbers based on 2001 census village directory			
# primary schools per village	2.9	3.2	-.28	.3
% village with medical facility	.67	.71	-.035	.37
% villages with tap water	.59	.6	-.007	.88
% villages with banking facility	.12	.16	-.034**	.021
% villages with paved road access	.8	.81	-.0082	.82
Avg. village size in acres	3392	3727	-336	.35

This table 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 use the 2001 data. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.2: Baseline balance household-level

	Treatment	Control	Difference	$p-$value
	(1)	(2)	(3)	(4)
	Results based on pooling NREGS & SSP samples			
Hhd members	4.6	4.6	-.024	.86
BPL	.98	.98	.0042	.63
Scheduled caste	.21	.24	-.03	.19
Scheduled tribe	.12	.12	-.001	.97
Literacy	.41	.41	-.00093	.95
Annual income	39,228	40,594	-1,546	.40
Short-term expenditure (1 month)	49,877	48,783	811	.68
Longer-term expenditure (1 year)	46,438	41,416	4,811	.50
Total expenditure (annualized)	644,755	626,591	14,583	.59
Average wage, June	94	97	-4	.26
Days worked priv. sector, June	4.7	5.2	-.46	.13
Reservation wage, June	69	75	-6.1^{**}	.02
Days unpaid work, June	4.4	3.4	1^{***}	.00
Days idle, June	18	19	-.68	.10
Owns land	.62	.57	$.053^{*}$	.05
Total savings	5,430	5,051	246	.69
Accessible (in 48h) savings	773	3,436	-2,778	.29
Total loans	56,659	53,606	3,345	.41
Owns business	.2	.17	.027	.15
Number of vehicles	.11	.12	-.0087	.58
	Results based on NREGS sample only			
NREGS availability	.47	.56	$-.1^{**}$	.02
Hhd doing NREGS work	.41	.41	.000024	1.00
NREGS days worked, June	8.3	8	.33	.65
NREGS hourly wage, June	13	14	-1.3	.13
# addi. days hhd wanted NREGS work	15	16	-.8	.67

This table compares NREGS and SSP 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. "Short-term expenditure" is the sum of 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 months, 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. "Total expenditure" is the sum of "Short-term expenditure" and "Longer-term expenditure" where short-term expenditure was annualized. "Average wage, June" and "Reservation wage, June" refer to work for someone else in June 2010. "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

(a) NREGS

	Total income		NREGS	Ag. labor	Other labor	Farm	Business	Misc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	9511**	8761**	914	3276**	3270**	2166	-642	528
	(3723)	(3722)	(588)	(1467)	(1305)	(2302)	(1325)	(2103)
BL GP Mean		.025						
		(.071)						
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.04	.04	.01	.06	.06	.02	.01	.01
Control Mean	69122	69122	4743	14798	9322	20361	6202	13695
N. of cases	4908	4874	4907	4908	4908	4908	4908	4908

(b) SSP

	Total income		SSP	Ag. labor	Other labor	Farm	Business	Misc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	5486	6355*	57	808	1593**	2631*	-292	682
	(3620)	(3566)	(120)	(1570)	(802)	(1561)	(1012)	(1845)
BL GP Mean		.18**						
		(.082)						
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.07	.07	.01	.12	.03	.01	.02	.04
Control Mean	53135	53135	3699	12641	6041	10986	5333	14443
N. of cases	3156	2977	3154	3156	3156	3156	3156	3156

This table shows sample-specific treatment effects on various income categories using annualized household data from the endline household survey. "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 and "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 which includes SSP (NREGS) income for households in the NREGS (SSP) sample. Households in the top .5% percentile based on total annualized income in treatment and control are excluded in all regressions. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. 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 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.4: Income gains heterogeneity by household characteristics

(a) NREGS

	Total income		SSP		Ag. labor		Other labor		Farm		Business		Misc	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treatment	11653**	9823**	331	427	4151***	4083***	4355***	3862***	3372	1545	-458	-955	-109	1199
	(4786)	(3933)	(657)	(489)	(1577)	(1481)	(1515)	(1350)	(2916)	(2536)	(1577)	(1580)	(2805)	(2148)
Hhd fraction eligible for SSP	-36007***		2521**		-5942***		-7120***		-12258***		-5442***		-10835**	
	(6531)		(1015)		(1977)		(2093)		(4542)		(1616)		(4218)	
Head of hhd is widow		-18375***		-111		2709		-1983		-16232***		-5311***		592
		(6565)		(644)		(2605)		(2127)		(2811)		(1710)		(3581)
Hhd fraction eligible for SSP X treatment	-13986		3178		-4745*		-5860**		-6553		-256		3346	
	(8753)		(3688)		(2509)		(2583)		(5618)		(2154)		(5135)	
Head of hhd is widow X treatment		-6318		3429		-6358*		-4402*		3218		2814		-2813
		(8102)		(2614)		(3307)		(2629)		(3788)		(2147)		(4177)
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	69122	69122	6053	6053	14798	14798	9322	9322	20361	20361	6202	6202	12386	12386
N. of cases	4875	4814	4909	4848	4909	4848	4909	4848	4909	4848	4909	4848	4909	4848

(b) SSP

	Total income		SSP		Ag. labor		Other labor		Farm		Business		Misc	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Treatment	6641*	2550	-75	27	603	-946	1749*	2343*	2967*	4805**	95	-1390	577	-2255
	(3722)	(5029)	(117)	(212)	(1605)	(2333)	(899)	(1296)	(1505)	(2017)	(1120)	(2087)	(1912)	(2397)
Disabled	12849***		2875***		1148		1502		3054		1984		3121	
	(4678)		(230)		(1928)		(1149)		(2691)		(2065)		(3128)	
Disabled or OAP	-3391		769**		1407		-1249		-2673		-2964		532	
	(6137)		(305)		(2252)		(1693)		(3295)		(2378)		(3910)	
Disabled X treatment		679		177		-1251		465		5357***		-2928		101
		(3717)		(256)		(1773)		(968)		(2017)		(1956)		(1893)
Disabled or OAP X treatment		5767		39		2715		-1160		-3503		1777		4470*
		(4570)		(289)		(2051)		(1471)		(2504)		(2128)		(2274)
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	.078	.075	.24	.016	.12	.12	.034	.034	.0089	.012	.015	.017	.038	.041
Control Mean	53135	53135	3699	3699	12641	12641	6041	6041	10986	10986	5333	5333	14443	14443
N. of cases	2977	2977	3154	3154	3156	3156	3156	3156	3156	3156	3156	3156	3156	3156

In this table we analyze whether a households's potential ability to perform manual labor affects how these households were affected by the intervention. Panels a) and b) consider various types of income using annualized household data from the endline household survey. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline (due to availability only included in columns 1-2). "NREGS" and "SSP" are earnings from the respective 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 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.5: Wage outcomes by survey

(a) NREGS

	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

(b) SSP

	Wage realizations (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.6*	6.2	7.8**	8.1**
	(3.9)	(3.9)	(3.1)	(3.2)
BL GP Mean		.14***		.082
		(.043)		(.05)
District FE	Yes	Yes	Yes	Yes
Adj R-squared	.05	.06	.03	.03
Control Mean	115	115	88	88
N. of cases	3816	3384	6106	5652

This table shows survey-specific treatment effects on wage outcomes from the private labor market using data. In both panels, the outcome in columns 1 and 2 is the average daily wage (in Rs.) an individual received while working for someone else in June 2012. Similarly, columns 3-4 in both panels an individual's reservation wage (in Rs.) at which he or she would have been willing to work for someone else in June 2012. The outcome is based on an a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. All regressions include 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: NREGS access labor market heterogeneity - treatment interactions

	Wage realizations (Rs.)	Reservation wage (Rs.)	Days worked	Days unpaid/idle
	(1)	(2)	(3)	(4)
No addi. work in May	6.6	-5.2	-.22	1.1
	(6)	(4.1)	(.74)	(.81)
# days addi. work in May	-.37**	-.022	.007	-.011
	(.15)	(.073)	(.019)	(.019)
No addi. work in January	7.3	2.9	.24	.34
	(5)	(3.4)	(.57)	(.66)
Anyone in village can get work	-3.8	7.1	-.59	1.7
	(8.7)	(7.4)	(1.3)	(1.6)
District FE	Yes	Yes	Yes	Yes
Control Mean	131	101	7	17
Avg. number of cases	6846	12379	13883	13540

In this table, we analyze heterogeneous labor market responses stemming from NREGS access and treatment interactions. This table displays interaction terms of the respective variable from separate regressions in each cell. All outcomes are measured at the individual level while regressors are household-level variables. Since the SSP survey did not ask about NREGS work outcomes and characteristics, the data is only available for the NREGS survey. "No addi. work in May" is an indicator for a member of a household in which no member wanted to work additional days on NREGS in May 2012 and could not get them. "No addi. work in January" is the respective indicator for January 2012. "# days addi. work in May" is the number of total number of days members of a household wanted to work on NREGS. Finally, "anyone in village can get work" is a binary indicator for whether a household believes that anybody in the village could get work on NREGS at anytime. All regressions include the GP mean of the dependent variable, district fixed effects and 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.7: Non-response rates by treatment status

(a) Using the full sample

	Treatment	Control	Difference	$p-$value	N
	(1)	(2)	(3)	(4)	(5)
Wage realizations (Rs.)	.011	.0097	.0012	.66	11173
Reservation wage (Rs.)	.41	.41	.00066	.97	32399
Days worked private sector	.34	.31	.028*	.062	32399
Days unpaid	.36	.34	.022	.13	32399
Days idle	.35	.33	.022	.11	32399
Days unpaid/idle	.35	.33	.021	.12	32399
Days worked > 0	.52	.49	.025	.24	21705

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

	Treatment	Control	Difference	$p-$value	N
	(1)	(2)	(3)	(4)	(5)
Wage realizations (Rs.)	.011	.01	.00053	.85	10322
Reservation wage (Rs.)	.11	.12	-.0092	.45	32399
Days worked private sector	.066	.066	-.00057	.92	32399
Days unpaid	.076	.078	-.0017	.81	32399
Days idle	.069	.07	-.00094	.86	32399
Days unpaid/idle	.067	.069	-.0017	.74	32399
Days worked > 0	.55	.53	.016	.43	18986

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

Table A.8: Differential predictors of non-response

	Missing response to				Days worked > 0
	Wage realizations	Reservation wage	Days worked	Days idle/unpaid	
	(1)	(2)	(3)	(4)	(5)
Member is female	.000098	.0052	.0098	.019	-.02
	(.0035)	(.014)	(.013)	(.013)	(.019)
Above median hhd income	-.0066	-.0012	.0095	.0008	.043
	(.0045)	(.014)	(.015)	(.013)	(.03)
Hhd is ST, SC or OBC	.023*	.057**	.029	.012	-.01
	(.012)	(.027)	(.019)	(.02)	(.035)
Hhd below BPL	-.0085	.013	.02	.0073	.034
	(.0074)	(.023)	(.022)	(.02)	(.04)
Any hhd member can read	.0085	-.024	.0087	-.0062	.012
	(.0064)	(.021)	(.018)	(.018)	(.032)
Head of hhd is widow	-.0031	-.0074	-.0007	.018	.005
	(.0058)	(.019)	(.017)	(.016)	(.024)
Carded GP	.0031	.0031	.0031	.0031	.034*
	(.0036)	(.0036)	(.0036)	(.0036)	(.019)
District FE	Yes	Yes	Yes	Yes	Yes
Control Mean	.0097	.0097	.0097	.0097	.49
Avg. number of cases	10594	28715	28715	28715	20604

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 unpaud/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 smple 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. The reason no interaction effect is included is because all carded GPs are in treatment mandals (by experimental design). Finally note that each column reports results from 7 different regression and hence there is no single number of observations which is why this table reports the average number of observations across all regressions in a column. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: 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)
Lagged dep. var	.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	6539	492	1770	2606	329
N. of cases	2837	2837	2837	2837	2837	2899	2837	2837	2837	2837

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 "lagged dependent variable" is constructed in the same way with the reference being the baseline study period (May 31 o 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.10: Baseline balance: spatial spillovers for survey outcomes

	Wage realizations (Rs.)	Reservation wage (Rs.)	Days worked	Days idle/unpaid	Total income
	(1)	(2)	(3)	(4)	(5)
Fraction treated within 10km	3.4	-.75	-.84**	1.1**	436
	(5.4)	(3.2)	(.38)	(.54)	(1963)
Fraction treated within 20km	3.7	-3.3	-1.6**	1.6**	2723
	(8)	(4.7)	(.64)	(.82)	(2974)
Fraction treated within 30km	10	-1.3	-1.6	1.5	4576
	(11)	(7)	(1)	(1.2)	(4458)
District FE	Yes	Yes	Yes	Yes	Yes
Control Mean	98	74	5.8	22	40379
Level	Indiv.	Indiv.	Indiv.	Indiv.	Hhd
Avg. number of cases	10007	15107	31418	31395	7044

In this table we analyze baseline balance of key outcomes with respect to spatial exposure to GPs in treatment mandals. Each cell shows the respective coefficient from a separate regression where the outcome is given by the column header. “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. “Total income” is total annualized household income, where the top .5% of observations are separately trimmed in treatment and control. The “Fraction GPs treated within x” is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are excluded in both the denominator and numerator. Note that each cell shows a separate regression of the outcome with the “Fraction treated within x” and district fixed effects as the only covariates. Finally note that since each column reports results from 7 different regression and hence there is no single number of observations which is why this table reports the average number of observations across all regressions in a column. Standard errors clustered at mandal level in parentheses. Statistical significance is denoted as: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: Spatial spillovers for household income

	Total income			Program earnings		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	8705**	10064***	9773***	284	397	550
	(4102)	(3689)	(3747)	(802)	(732)	(734)
Fraction GPs treated within 10km	5683			-296		
	(4172)			(716)		
Fraction GPs treated within 20km		5245			2.3	
		(7630)			(1412)	
Fraction GPs treated within 30km			2240			1753
			(9602)			(2564)
BL GP Mean	.2***	.19***	.19***			
	(.068)	(.066)	(.065)			
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.02	.02	.02	.03	.03	.04
Control Mean	63984	63984	63984	6990	6990	6990
N. of cases	7067	7776	7841	7259	7989	8054

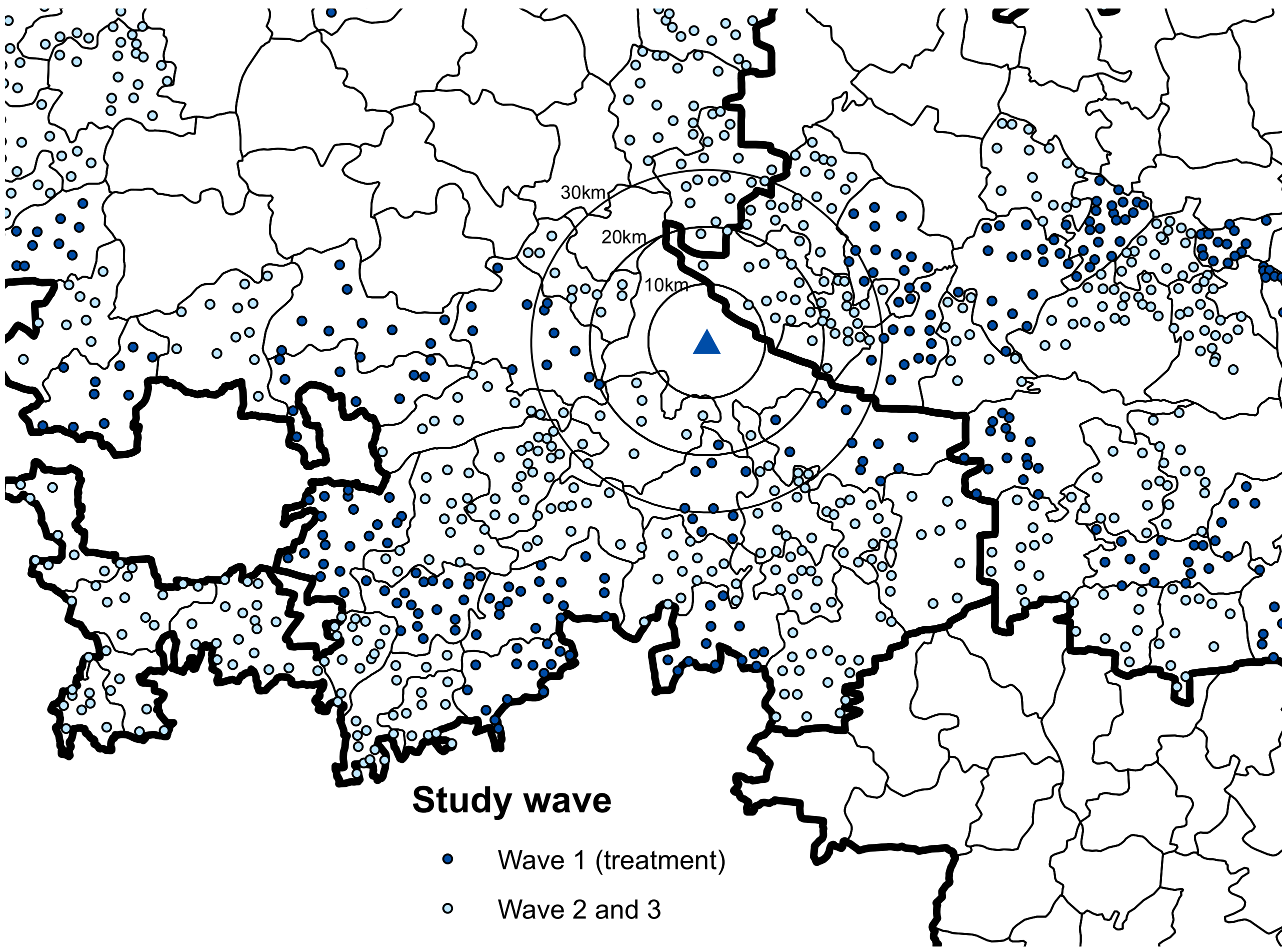
This table analyzes spatial proximity effects on household income using data from both surveys, NREGS and SSP. The outcome in columns 1-3 is total annualized household income in Rupees. In columns 4-6, we consider the combined annualized earnings a household derives from NREGS and SSP. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. The detailed measurement of program earnings is not available in the baseline data. The "Fraction GPs treated within x" is the ratio of the number of GPs in treatment mandals within radius x km over the total GPs within wave 1, 2 or 3 mandals. Note that wave 2 mandals are included in the denominator, and that same-mandal GPs are excluded in both the denominator and numerator. The number of observations increases in radius because some GPs simply do not have any neigboring GPs within 5 or 10 kilometers. "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 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.12: Loans by type of lender

	Formal		Semi-Formal		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	3218*	3533*	-543	-626	6161*	5690*
	(1834)	(1817)	(780)	(798)	(3243)	(3212)
BL GP Mean		.073		.028		.13***
		(.059)		(.032)		(.036)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj R-squared	.01	.01	.01	.01	.00	.01
Control Mean	13161	13161	5091	5091	43198	43198
N. of cases	8114	7901	8112	7899	8113	7900

This table breaks down the coefficients on total household loans reported in columns 3 and 4 of Table 8. "Formal" loans in columns 1 and 2 are defined as those loans granted by a - as mentioned in the household survey - commercial bank or a finance company. In contrast, the loans labeled "Semi-Formal" were taken out from a micro-finance institution, a self-help group, a cooperative or from a Chit fund. A Chit fund is communal savings scheme regulated by Chit Fund Act from 1982 in which members make periodical contributions to be paid out to members at a specified point in time. Finally, "informal" loans are defined as those which households borrow money from money lenders, clients, shopkeepers, friends, neighbors or family members. "BL GP Mean" is the Gram Panchayat mean of the dependent variable at baseline. All regressions include 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: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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 corresspond 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 excluded from 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.