

# General equilibrium effects of (improving) public employment programs: experimental evidence from India\*

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## Abstract

Public employment programs may affect poverty through both the income they provide and their effects on private labor markets. We estimate both effects, exploiting a large-scale experiment randomized across 157 sub-districts (with an average population of 62,500 each) that improved the implementation of India’s national rural employment guarantee scheme. The reform raised low-income households’ earnings by 13%, with 90% of this gain coming from *non-program* earnings, driven by increases in both market wages *and* private-sector employment. Workers’ reservation wages increased and their employment gains were higher in treated areas with more concentrated landholdings, consistent with monopsonistic labor markets. We also find increases in credit, private assets, and longer-term enterprise counts and non-agricultural employment, underscoring the far-reaching market impacts of the initial reform. Overall the results suggest that public employment programs can effectively reduce poverty in developing countries, and may also improve economic efficiency.

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

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

Whether and to what extent such programs raise incomes and reduce poverty is a first-order policy question. If all that mattered were the direct earnings from the program itself, we could answer this question simply using survey data on program earnings. What complicates matters is that labor-market general equilibrium (GE) effects on market wages and employment could attenuate or amplify the direct income gains from the program. For instance, program earnings might understate net income gains for the poor to the extent that public employment programs raise market wages, or overstate them to the extent that they reduce private employment. The magnitudes of these effects in turn depend on the underlying structure of the labor market.

Given the importance of the NREGS, there is a large literature on its impacts. However, the existing evidence is hampered by four factors. The first is the lack of experimental variation, with the consequence that studies often reach opposing conclusions depending on the data and identification strategy used (see Sukhtankar (2017)). Second, it is well-known that NREGS implementation quality varied considerably across time and space. This makes it difficult to interpret the wide range of estimates of NREGS impact to date, because this divergence could reflect unmeasured variation in implementation quality. Third, since labor market effects of NREGS may spill over across district boundaries, most existing estimates may be biased by not accounting for spillovers to untreated units (as in Miguel and Kremer (2004)). Finally, since the period of NREGS rollout had only a “thin” round of the National Sample Survey (NSS) that does not have representative district-level data on consumption, existing studies have not been able to study the impact on poverty.

In this paper we address these challenges by combining exogenous experimental variation, a demonstrable first-stage impact on implementation quality, units of randomization large enough to capture labor-market GE effects, and geocoded units of observation disaggregated enough to test and correct for spatial spillovers. Specifically, we worked with the Government of the Indian state of Andhra Pradesh (AP) to randomize the order in which 157 sub-districts

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

(mandals) with an average population of 62,500 each introduced a new system (biometric “Smartcards”) for making payments in NREGS.<sup>2</sup> In prior work, we show that Smartcards substantially improved the performance of NREGS on several dimensions: it reduced leakage of funds, increased program earnings, reduced payment delays and the time required to collect payments, and increased real and perceived access to work, without changing fiscal outlays on the program (Muralidharan et al. (2016), henceforth MNS). Thus, Smartcards brought NREGS implementation closer in specific, measured ways to what its architects intended.

The impacts of improving NREGS implementation are unlikely to be the same as the impacts of rolling out the program itself. Yet, given well-documented implementation challenges – including poor access to work, high rates of leakage, and long delays in receiving payments (Mehrotra, 2008; Imbert and Papp, 2011; Khera, 2011; Niehaus and Sukhtankar, 2013b) – improving implementation on these metrics is likely to meaningfully increase any measure of *effective* NREGS. Further, since improvements in the effective presence of NREGS were achieved without increasing NREGS expenditure in treated areas, our results are likely to be a lower bound on the anti-poverty impact of rolling out a well-implemented NREGS from scratch (which would also transfer incremental resources to rural areas).

We study the impact of improving NREGS implementation using detailed data on income (by source), wages, and employment from ~5,000 detailed surveys conducted in a representative sample of all households registered for NREGS, who comprise 49.5% of rural households. We measure wages and employment in the month of June (which is in the peak NREGS period), and income for the year preceding the survey. These surveys were conducted two years after the randomized roll-out of the Smartcard program, before the control group was treated. To study impacts on the full population, we supplement this survey data with three different *censuses* of income, employment, and livestock conducted by the government independently of our efforts and at around the same time.

We find, first and foremost, that this improvement in NREGS implementation led to large and widespread increases in the incomes of the rural poor. In an independently-conducted household census, the proportion of rural households in the lowest income bracket fell by 4.9% (a 4.1 percentage point reduction on a base of 83%). In our survey, which is representative of NREGS-registered households, we find mean earnings increased by 12.7%, leading to a 17.4% reduction in an income-based measure of poverty (a 4.9 percentage point reduction on a base of 28.2%). We find no evidence of corresponding changes in the prices of consumer goods, suggesting real—not merely nominal—earnings gains.

Second, the majority of these income gains are attributable to gains in earnings from

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<sup>2</sup>The original state was divided into two states on June 2, 2014. Since this division took place after our study, we use “AP” to refer to the original undivided state.

sources *other than* the NREGS. Among NREGS-registered households, increases in NREGS earnings accounted for only 10% of the total income increase, with the rest coming from private-sector earnings.<sup>3</sup> In turn, the majority of the increase in non-NREGS earnings (over 80%) comes from an increase in market labor income.

Third, this increase in earnings reflects in part the fact that market wages rose substantially: by 10.6% in treated areas during June, a month of peak NREGS activity. This increase is comprised of two effects. First, we find a significant positive ITT effect on market wages in treated mandals of 6.5%. Second, we also find strong evidence of spatial spillovers, with significantly higher market wages in *control* areas that had a greater fraction of treated villages in their neighborhood. Ignoring these spillovers would bias the treatment effects downwards. We estimate that spillovers increased wages in the control group by another 4.1%, and the adjusted increase in market wages was 10.6%.

Fourth, despite the increase in wages, we find an *increase*, not a decrease, in market employment among NREGS-registered households. Spillover-adjusted estimates of days worked in the private sector increased by 20% (1.4 days on a base of 7.1 days/month). Days worked on NREGS went up by 29% (0.9 days on a base of 3.2 days/month), and days idle or unpaid fell by 13% (2.4 days on a base of 18 days/month). The probability of migration was also higher, though not significant ( $p = 0.22$ ). Thus, to the extent that migration is primarily for employment, we also see no decrease in the likelihood of being employed outside the village.

The fact that both wages and employment increased in tandem is central to the large income gains we estimate: wage gains were not offset by reduced employment, but instead were augmented by increased employment. We consider two broad explanations. First, this could reflect an inward shift in labor supply in the context of imperfectly competitive labor markets (e.g. oligopsony). Second, this could reflect an outward shift in labor demand.

We find clear evidence that labor supply to private sector jobs shifted inwards. Reservation wages increased in line with wage realizations, suggesting that an improved NREGS increased workers' bargaining power by enhancing outside options. Moreover, changes in market employment covary systematically with measures of monopsony.<sup>4</sup> Specifically, treatment led to significantly greater increases in private-market employment in villages with greater land concentration, as measured by a normalized Herfindahl-Hirschman index (HHI). While this index likely does not capture all aspects of employer market power, we estimate that it can explain 35% of the overall increase in market employment.

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<sup>3</sup>In the control group, the mean household earned 7% of its income from NREGS and 93% from other sources. Thus, treatment increased earnings in similar proportions.

<sup>4</sup>Following usage in the literature (e.g. Manning (2003)), we use the term "monopsony" broadly to describe situations where there are "few potential employers within 'reasonable' distance of workers so that, from the perspective of a worker, the labour market appears to be 'thin'".

We find less evidence of increased labor demand within the 2-year time-frame of our study. Labor demand could rise due to the creation of productivity-increasing public assets under the NREGS, but we calculate that even under generous assumptions, the Smartcard program could have increased the total rural capital stock by at most 1%, not nearly enough to lead to our observed labor market impacts. Alternatively, the increased income from higher wages could have a secondary effect on local labor demand through an increase in demand for locally-produced products. However, we see no increase in household consumption spending that would point to this mechanism.<sup>5</sup> We also do not see increases in consumer goods prices that would transmit a demand shock to producers and ultimately to labor markets. Finally, the large increase in market wages we document are hard to reconcile with a Keynesian aggregate-demand channel driven by slack labor demand.

In the longer run, we expect the increased income among the rural poor to transmit further through the economy – including through increased demand as households recognize that the income gains are not transitory. While fully characterizing these longer-run impacts is beyond the scope of the paper, we find and report evidence of additional expansion in economic activity. First, independent census data show that holdings of livestock, and of buffaloes in particular, increased significantly, and survey data show a 12.8% increase in land ownership and a 29% increase in borrowing among NREGS-registered households. This suggests that increasing wages and incomes may have reduced credit constraints and enabled private investment. Second, using data from the Economic Census conducted a year *after* the 2-year-long experiment concluded, we find a 23% increase in the number of business establishments, and a 35% increase in employment reported by these establishments in treated mandals (an 8 percentage point increase on a base of 23%). These employment gains are concentrated in non-agricultural sectors (manufacturing, construction, and retail trade), perhaps suggestive of structural transformation (see discussion in Section 4.4.2).

Our analysis has (at least) two important limitations. First, our survey data only cover NREGS-registered households. This is appropriate, as these are the households that NREGS was designed to benefit and our focus in this paper is on labor-market GE effects on this population (who could potentially have been hurt by a reduction in market employment). But it limits our ability to fully characterize GE effects on the economy. While results using census data reassure us that overall income, employment, and assets went up, they likely still mask distributional effects. In particular, higher wages presumably hurt land-owning employers by reducing their monopsonistic rents, which we do not observe directly. Second, our labor market data are most detailed for the month of June. This is again by design, since

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<sup>5</sup>As we see below, this result is consistent with households interpreting the initial income shock as transitory and investing these income gains instead of consuming them.

this is the period of peak NREGS participation, and is thus the period where our experiment had the greatest relevance for improving NREGS implementation (and for any downstream impact on labor markets). However, this limits our ability to study the transmission of wage and employment effects to the rest of the year.

Our first contribution is to the literature and policy debate on the impact of public works programs on rural labor markets, incomes and poverty (Imbert and Papp, 2015; Beegle et al., 2017; Bertrand et al., 2017; Sukhtankar, 2017). In addition to confirming some prior findings, like the increase in market wages (Imbert and Papp, 2015; Berg et al., 2018; Azam, 2012), our data and methodology allow us to report several new results including significant gains in income and reduction in poverty, and *positive* effects on private sector employment. Taken together, our results suggest that public employment programs can both enhance economic efficiency and reduce poverty.<sup>6</sup>

Second, we contribute to the literature on rural labor markets in developing countries generally (Rosenzweig, 1978; Jayachandran, 2006; Kaur, 2019), and on the impacts of minimum wages specifically (e.g. Dinkelman and Ranchhod (2012)). Our results are consistent with evidence from the US and elsewhere pointing to a lack of large negative employment effects of raising minimum wages (see Card and Krueger (1994), Cengiz et al. (2019) and Harasztsosi and Lindner (2019), among others). They also add to a growing body of evidence on the existence of employer market power, including in labor markets that we might expect to be more competitive than those in rural India: see for example, Dube et al. (2020) for online labor markets, and (Naidu et al., 2016) for migrant labor markets.

Third, our results highlight how public options can influence private markets even when they themselves capture only modest market shares. Critics have argued that the NREGS could not have meaningfully affected market wages because NREGS work constitutes only a small share of rural employment (Bhalla, 2013). However,  $\sim 50\%$  of rural households in AP are registered for NREGS, and 32% actively participated at some point in 2011-12 (in NSS data). These facts, along with our results on increased reservation wages, suggest that what matters is not only the NREGS’s “market share”, but also its credibility as an outside option. Improving this option can in turn raise wages in the private sector, as suggested by Dreze and Sen (1991); Basu et al. (2009).<sup>7</sup>

Fourth, and related, our results highlight the importance of accounting for general equilib-

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<sup>6</sup>We provide a more extended discussion of how our results relate to existing evidence on public employment programs in India and globally in Section 5.

<sup>7</sup>In a related vein, Beaudry et al. (2012) show that changes in city-level industrial composition affect average wages by 3-4 times more than would be expected based on a simple accounting approach, likely because they affect wage bargaining, and Clemens and Gottlieb (2017) show that Medicare pricing affects private sector health care prices in the US, despite comprising less than a third of the market.

rium effects in program evaluation (Acemoglu, 2010). Ignoring these effects (say by randomizing program access at the individual level) would have led to us to sharply underestimate impacts on rural wages and poverty. Even analyzing our own data while ignoring market spillovers to control areas would meaningfully understate impacts on wages and employment. Viewed positively, our study demonstrates the feasibility of conducting experiments with units of randomization large enough to capture general equilibrium effects (Cunha et al., 2019; Muralidharan and Niehaus, 2017; Egger et al., 2019).

Fifth, our results highlight the importance of program *implementation quality* in developing countries as a first order issue in and of itself. Our estimates of the wage impacts of improving NREGS implementation, for example, are about as large as estimates of the impact of rolling out the program itself (Imbert and Papp, 2015). More generally, in settings with high corruption and inefficiency, investing in better implementation of a program could be more cost-effective than spending more on the program as is. For instance, Niehaus and Sukhtankar (2013b) find that increasing the official NREGS wage had no impact on workers’ program earnings, while we find that improving implementation *without* increasing program wages or outlays significantly increased both program and market earnings.

The rest of the paper is organized as follows. Section 1 describes the context, related literature, and Smartcard intervention. Section 2 describes the research design, data, and estimation. Section 3 presents our main results on income, wages, and employment. Section 4 explores the roles of labor supply, demand, and market structure as well as longer term impacts, Section 5 interprets our results in the context of the existing literature, and Section 6 concludes with a discussion of policy implications.

# 1 Context and intervention

## 1.1 The NREGS

The NREGS is the world’s largest public employment scheme, entitling any household living in rural India (i.e. 11% of the world’s population) to up to 100 days per year of guaranteed paid employment. It is one of the country’s flagship social protection programs, and the Indian government spent roughly 6.8% of its budget ( $\sim 0.5\%$  of GDP) on it in 2011-12.<sup>8</sup> Coverage is broad:  $\sim 50\%$  of rural households in Andhra Pradesh were registered for the program in 2011-12, meaning that they had a jobcard and were therefore legally entitled to request work at any time. NREGS jobs involve manual labor compensated at statutory

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<sup>8</sup>NREGS spending source: <https://www.indiabudget.gov.in/budget2011-2012/ub2011-12/bag/bag5.pdf>, outlays source: <https://www.indiabudget.gov.in/budget2011-2012/ub2011-12/bag/bag4.pdf>, both accessed October 1, 2019.

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

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

Debate continues over the impacts of the NREGS on a wide range of outcomes (see Sukhtankar (2017) for a review). For rural incomes, specifically, debate has hinged on the fact that the scheme accounts for only a small share of rural employment in nationally representative surveys (4% across India in 2011). Given this, skeptics argue that it cannot have played a meaningful role in rural poverty reduction: “how can a small tail wag a very very large dog?” Bhalla (2013). Others have pointed out that even if it did raise labor incomes by raising rural wages, this could come at the cost of crowding out private sector employment (Murgai and Ravallion, 2005). This debate continues to have policy significance as national and state governments can in practice decide how much to prioritize the scheme by adjusting fiscal allocations to it.<sup>9</sup>

## 1.2 Smartcards

To address the leakage and payments challenges described above, the Department of Rural Development of the Government of AP introduced a new payments system, which we refer to as “Smartcards” for short. This involved two major changes. First, the flow of funds shifted in most cases from government-run post offices to banks, who worked with local partners (called banking correspondents) to make last-mile payments in cash, typically in the village itself. Second, the protocol for authenticating when collecting payments changed from one based on paper documents and ink stamps to one based on biometric authentication. Our description here is brief; further details are available in MNS.

In MNS, we show that the Smartcards reform improved NREGS implementation quality

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<sup>9</sup>Work availability fell sharply in the second half of 2016, for example, following a cut in the NREGS budget : <http://thewire.in/75795/mnrega-centre-funds-whatsapp/>, accessed November 3, 2016.



on several dimensions. Payments in treated mandals arrived in 29% fewer days, with arrival dates 39% less varied, and took 20% less time to collect. Households earned more working on NREGS (24%), and there was a substantial 12.7 percentage point ( $\sim 41\%$ ) reduction in leakage (defined as the difference between fiscal outlays and beneficiary receipts). Program access also improved: both perceived access and actual participation in NREGS increased (17%). These gains were widely distributed; we find little evidence of heterogeneous impacts, and treatment distributions first order stochastically dominate control distributions for all outcomes on which there was a significant mean impact. Fiscal outlays, on the other hand, were unchanged. Overall, Smartcards substantially improved NREGS implementation in directly measured ways, and made it a more credible option for the rural poor. Reflecting this, users were strongly in favor of Smartcards, with 90% of households preferring it to the status quo and only 3% opposed.

### 1.3 Interpreting Smartcards' impacts on earnings

Given that Smartcards brought the *effective* presence of NREGS in treated areas closer to the intentions of the program's framers, one might think of them as an instrumental variable for an abstract, composite measure of "effective NREGS." In practice, this idea is difficult to implement because NREGS implementation quality is multi-dimensional and spans job access and availability, job receipt, wages received net of corruption, and speed and reliability of payments. Our results are therefore best interpreted as the reduced form impact of improving NREGS implementation quality on all of these dimensions.

In addition to NREGS, Smartcards were also used to make payments in the rural social security pensions (SSP) program. However, improvements in SSP implementation are unlikely to affect labor markets because the SSP program was targeted to the rural poor who were *not able to work*.<sup>10</sup> We test and show that treatment did not generate income gains in households where all adults were eligible for the SSP (see Section 3.2).

The creation of Smartcard-linked bank accounts could have also affected labor market outcomes indirectly through promoting financial inclusion. In practice, this was highly unlikely because (a) the government asked banks to fully disburse NREGS wage payments as soon as possible and not leave balances in the account, and (b) the accounts had limited

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<sup>10</sup>Specifically, pensions were restricted to those who are Below the Poverty Line (BPL) *and* either widowed, disabled, elderly, or had a displaced traditional occupation. The scale and scope of SSP is far narrower than that of the NREGS: only 7% (as opposed to 49.5%) of rural households are eligible, and the benefit is modest, with a median and mode of Rs. 200 per month ( $\sim \$3$ , or less than two days earnings for a manual laborer). Finally, the impact of Smartcards on SSP was much less than on NREGS: we found no changes in the payments process, and a small reduction of leakage from 6% to 3%, in part because payment delays and leakage rates were low to begin with.

functionality: they were not connected to the online core banking servers, relied on offline authentication, and could only be accessed through a single banking correspondent (see Appendix A.3 in MNS for more details). Reflecting these facts, only 0.3% of households in our endline survey reported having money in their account.<sup>11</sup>

Overall, the Smartcard intervention was run with the primary goal of improving the payments process and reducing leakage in the NREGS and SSP programs. It was not integrated into any other program or function either by the government or the private sector. We therefore interpret the results that follow as consequences of improving NREGS implementation.

## 2 Research design

### 2.1 Randomization

We summarize the randomization design here; MNS provides further details. The experiment was conducted in eight districts with a combined rural population of around 19 million in the erstwhile state of Andhra Pradesh (AP).<sup>12</sup> To enable an evaluation of the Smartcard program, the Govt. of AP 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 each – to treatment (112), control (45), and an intermediate “buffer” wave (139).<sup>13</sup> Figure A.1 shows the geographical spread and size of these units. We created the buffer group (Wave 2) to serve as a temporal buffer in which the government could deploy Smartcards while we conducted endline surveys, after deploying them in treated mandals and before deploying them in controls, and we did not collect any survey data in non-study mandals (see Appendix C in MNS for details). We stratified randomization by district and by a principal component of Mandal socio-economic characteristics.

Treatment and control mandals are well-balanced on stratification variables, as well as other Mandal characteristics from the census (Table A.1). Differences are significant at the

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<sup>11</sup>Further evidence that opening bank accounts, and even paying NREGS wages into these accounts, is unlikely on its own to affect labor-force participation is provided by Field et al. (2019), who find that paying NREGS wages into women’s bank accounts had no impact on their labor supply in Madhya Pradesh, unless also accompanied by training to women on how to use these accounts (which also led to an increase in balances maintained in the account). No such training was provided in our setting.

<sup>12</sup>The 8 study districts are similar to erstwhile AP’s remaining 13 non-urban districts on major socio-economic indicators, including proportion rural, scheduled caste, literate, and agricultural laborers; and represent all three historically distinct socio-cultural regions (Tables D.1 and D.2 in MNS Online Appendix).

<sup>13</sup>We dropped 109 of the total 405 mandals prior to randomization, either because the Smartcards program had already started there or because they were entirely urban and hence had no NREGS. Excluding these 109 mandals does not affect internal validity; they are also similar to the 296 randomized mandals on observables (see Table D.3 in MNS).

5% level in 2 out of 22 cases. Table A.2 shows balance on focal outcomes for this paper along with other socio-economic household characteristics from our baseline survey. None of the 34 variables are significantly different at the 5% level, and 4 out of 34 variables are significantly different at the 10% level, slightly more than one might expect by chance. In the analysis below we test the sensitivity to chance imbalances by controlling for (village means of) baseline outcome values.

## 2.2 Data

We use data on registered NREGS beneficiaries from original surveys of jobcard holders conducted during August-September of 2010 (baseline) and 2012 (endline). We designed these surveys both to understand how Smartcards affected access to NREGS work and payments, and also how this affected household’s earnings and well-being more broadly. We measured annual household earnings by category (NREGS, other agricultural labor, other labor, farm income, business income, and other miscellaneous income) based on annual recall at the time of the survey. We also asked detailed questions about household members’ labor market participation, wages, reservation wages, and earnings during June, a month of peak NREGS participation in Andhra Pradesh.<sup>14</sup>

We sampled a panel of 880 villages (technically Gram Panchayats, but the distinction is not relevant in this case) across 157 mandals in 8 districts, and a repeated cross-section of NREGS-eligible households from these villages, yielding a target sample of 5,278 households at endline. Of these we were able to survey 4,943.<sup>15</sup> We over-sampled households that were listed as having recently been paid in order to gain precision in estimating leakage in MNS.<sup>16</sup> We therefore re-weight the observations to make all estimates representative of the population of jobcard-holding households (who are the ones eligible to work on NREGS). This population made up 49.5% of rural households in Andhra Pradesh (our calculations from the NSS Round 68 in 2011-12).

We verify in MNS that Smartcards did not affect either the size or composition of the sampling frame of jobcard holders, so that treatment effects reported below are not confounded

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<sup>14</sup>The peak NREGS months are May and June during the lean season between harvesting of the winter crops and planting of the summer/monsoon crops. We focus our detailed measurement on the month of June because our surveys were conducted in August-September and June was the peak NREGS month for which respondents could best recall detailed wage, and employment details.

<sup>15</sup>Of the remaining, 200 were ghost households, while we were unable to survey or confirm the existence of 135. The corresponding figures for baseline are 5,244; 4,646; 68 and 530 respectively. Note that the totals we report in MNS differ because they also include a separate sample of pension beneficiaries.

<sup>16</sup>We sampled a panel of villages but a repeated cross-section of households because of the low auto-correlation in the household-level likelihood of working on NREGS during our survey periods two years apart.

by changes in the composition of registered households.<sup>17</sup> The sample also likely represents the entire universe of rural workers employed in agriculture.<sup>18</sup> Compared to non-holders, jobcard-holding households are larger and more likely to belong to historically disadvantaged scheduled castes (Table A.3).

Our survey data are representative of the universe of intended NREGS beneficiaries, and the main goal of this paper is to study the impact of improving NREGS on the outcomes of these beneficiaries (including private market wages and employment). Because our survey data exclude the 50.5% of rural households who are not registered for NREGS, we complement them with three different and entirely independent censuses of income, assets, and employment conducted by the government.

The first is the Socio-Economic and Caste Census (SECC), which provides basic information about income for the entire population. The SECC was a nation-wide census conducted to enable governments to determine which households were “Below the Poverty Line” (BPL) and thereby eligible for various benefits. The survey collected data on income categories for the household member with the highest income, the main source of this income, household landholdings (which we use to construct measures of concentration of land-holdings at the village level), caste, and education. The SECC was conducted in Andhra Pradesh during 2012 using the layout maps and lists of houses prepared for the regular 2011 Census. The data include approximately 1.8 million households in our study mandals.

The second is the Livestock Census of India, a nation-wide census conducted quinquennially by the Government of India, with the 19th round also conducted in Andhra Pradesh in 2012. The census reports mandal-level headcounts of 13 types of livestock including the most economically important (e.g. cattle, buffaloes) as well as a number of others. We have data at the mandal level for all 157 of our study mandals.

The third is the Economic Census of India, a nation-wide census of enterprises and employment conducted roughly quinquennially since 1977. It counts enterprises involved in all non-agricultural economic activities, as well as those agricultural enterprises that are not involved in crop production and plantation. It collects data on the industrial classification of the enterprise, number of employees, and demographic details of the owner of the enterprise. We use data from the sixth round – conducted in 2013 – one year after the experiment was

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<sup>17</sup>This likely reflects the fact that all households who may have wanted to work on NREGS were already registered for it by 2010 (five years after the launch of the program). The churn in registered households between the baseline and endline was only 2%, likely reflecting migration and new household formation. See MNS Appendix C and Tables C.4-C.9 for further details.

<sup>18</sup>In the NSS, 59% of all workers are part of households that hold a jobcard; in our sampled households, 68% of workers work primarily in agriculture. This suggests that we can account for agricultural workers representing  $59\% * 68\% = 40\%$  of the workforce, if anything slightly higher than the 2011 census figure of 33% who report primarily working in agriculture.

over, to study longer-term effects of improving NREGS implementation.

To examine consumer goods prices, we use unit cost data from Round 68 (2011-2012) of the NSS. Because the overlap between villages in our study mandals and the NSS sample is limited to 60 villages, we use the NSS data primarily to examine treatment effects on price levels, for which it is the best available data source.

To test for effects of improved NREGS on rural public goods, we use data on land under cultivation and under irrigation from the District Statistical Handbooks (DSH) published each year by the Govt. of Andhra Pradesh based on data from the Office of the Surveyor General of India.<sup>19</sup> Finally, we use geocoded point locations for each census village from the 2001 Indian Census to construct measures of spatial exposure to treated neighbors.

Figure 1 summarizes the data sources we use, the recall period that they correspond to, and the outcomes they cover.

## 2.3 Estimation strategy

Consider first a simple comparison of outcomes across treatment and control mandals (i.e. intent-to-treat estimates):

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

Here  $Y_{imd}$  is an outcome for household or individual  $i$  in mandal  $m$  and district  $d$ ,  $Treated_{md}$  is an indicator for a treatment group mandal,  $\delta_d$  are district fixed effects, and  $PC_{md}$  is the first principal component of a vector of mandal characteristics used to stratify randomization.<sup>20</sup> When using survey data, we also report specifications that include (when available) the baseline village-level mean of the dependent variable  $\bar{Y}_{pmd}^0$  to increase precision and assess sensitivity to any randomization imbalances (recall that we have a village-level panel and not a household-level one).

If outcomes for a given unit (household, village, etc.) depend only on the treatment status of the mandal in which it lives, then  $\beta$  in Equation 1 identifies a well-defined treatment effect. If improving the NREGS affects labor markets more broadly, however, there is no reason to expect these effects to be neatly confined within mandals. To the extent markets are integrated, upward pressure on wages in treated mandals (for example) might affect wages

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

<sup>20</sup>As in MNS, we include the principal component itself rather than fixed effects based on its strata as treatment status does not vary within a few strata. Hence, the latter approach implies dropping 4% of the sample. We test for robustness to including fixed effects and dropping these observations, and results are substantively unchanged. These and other robustness checks are available in an online Appendix (at Muralidharan et al. (2020)) and are referred to in the rest of the text as “available online”.

in nearby parts of control mandals. In other words, treatment effects will likely depend on both the own treatment status of a village as well as the fraction of villages around it that are treated.

Figure 2 illustrates this conceptually. The difference between the intercepts ( $\beta_T$ ) represents the effect of a village being treated when none of its neighbors are treated, and movement along the x-axis represents the additional effect of having more neighbors treated. Thus, the unadjusted intent-to-treat effects, represented by  $y_{ITT}$ , captures both the effect of a village being treated, and the mean difference in the fraction of treated neighbors between treatment and control villages ( $x_{ITT}$ ). The spillover effect, represented by  $y_{SP}$ , captures the extent to which outcomes in the control group are affected by spillovers. These also correspond to the extent to which ignoring spillovers will understate treatment effects (as shown in a different setting by Miguel and Kremer (2004)). The adjusted treatment effect, represented by  $y_{ATE}$ , corrects for this bias, and is the difference in expected outcomes between a village in a treated mandal with the mean fraction of treated neighbors and a village in a control mandal with *no* treated neighbors, which represents the counterfactual outcome had no mandals been treated.<sup>21</sup>

The practical importance of this issue is seen in Figure A.1, where we see that there are some control mandals (like  $C_{low}$ ) whose neighboring mandals are not treated, while there are others (like  $C_{high}$ ) whose neighbors are treated. We therefore need methods to (i) test for the existence of such spillover effects, and (ii) estimate the average spillover in the control areas, and (iii) adjust treatment effects for these spillovers.

As with any such spatial problem, outcomes in each village could in principle be an arbitrary function of the treatment status of all the other villages. We take a simple and transparent approach, modeling spillovers as a linear function of the fraction  $\tilde{N}_p^R$  of villages within a radius  $R$  of panchayat  $p$  and within a *different* mandal which were assigned to treatment.<sup>22</sup> We exclude villages from the same mandal as panchayat  $p$  in this calculation in order to ensure that our measure of spillovers is exogenous conditional on own-mandal treatment assignment.<sup>23</sup> Figure A.2 illustrates the construction of this measure.<sup>24</sup> Table

<sup>21</sup>It is also possible to estimate a “Total Treatment Effect” ( $y_{TTE}$  in the Figure) that extrapolates to estimate the total treatment effect if all mandals are treated compared to one where none are treated. We do not focus on  $y_{TTE}$  since it requires extrapolation beyond the coverage induced by the experiment.

<sup>22</sup>We use a linear function both because it is the most natural functional form for spillovers, and because higher-order terms are never significant.

<sup>23</sup>To see the issue, note that within a treated mandal a village closer to the center of the mandal will tend to have higher values of  $N_p^R$ , as more of their neighbors are from the same mandal and fewer from other mandals. The opposite pattern will hold in control mandals. Thus, we cannot interpret a coefficient on  $N_p^R$  as solely a measure of spillover effects without making the additional assumption that the direct effects of treatment are unrelated to location. Merfeld (2019) finds intra-district differences in wages as a function of distance to the district border, suggesting that this assumption may not hold.

<sup>24</sup>We drop non-study mandals from both the numerator and denominator in constructing the measure of

A.4 reports tests showing that our outcomes of interest are balanced with respect to these measures at baseline.

To test and adjust for spillovers, we estimate

$$Y_{ipmd} = \alpha + \beta_T T_{md} + \beta_N \tilde{N}_{pmd}^R + \delta_d + \lambda PC_{md} + \epsilon_{imd} \quad (2)$$

Here  $\beta_T$  captures the effect of being in a treated mandal and  $\beta_N$  the effect of having a higher fraction of neighboring mandals (excluding your own mandal) who are treated. The average total treatment effects experienced by treatment and control mandals respectively are

$$\bar{\beta}_T = \beta_T + \beta_N \cdot \tilde{N}_T \text{ and } \bar{\beta}_C = \beta_N \cdot \tilde{N}_C \quad (3)$$

The choice of a radius  $R$  is inherently somewhat arbitrary. We set  $R = 20$ , which is roughly the distance a worker could commute by bicycle in one hour at a speed of 20 km / hour, and then examine sensitivity to alternative radii (ranging from 10km to 30km) in Table A.7. Effects could of course propagate beyond the distance over which any single actor is willing to arbitrage as changes in one market ripple on to the next. We do not estimate the decay of spillover effects with distance as we lack the power to separately estimate effects within various bands.

For wage and employment outcomes, we present estimates for the ITT effects, spillover effects, and the adjusted treatment effects. We focus on spillover-adjusted estimates for wages and employment because data for these were collected in the month of June, which is during the peak period of NREGS (see Figure A.3) and the time of the year when the Smartcards intervention was most relevant for improving NREGS and for affecting the labor market. For outcomes based on annual recall (such as income and assets) where we are underpowered to detect spillovers, we focus on the ITT estimates in the text and present spillover-adjusted estimates in the Appendix for completeness.

For outcomes in our survey data we use standard asymptotic inference clustered at the mandal level. When we report estimates that adjust for cross-mandal spillovers, we also report (Conley, 2008) standard errors which allow for spatial autocorrelation in the error term.<sup>25</sup> For outcomes in census data we calculate standard errors using randomization

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spatial exposure (since we have no data in these mandals). We treat villages assigned to mandals in the buffer group as untreated in our default specification because treatment rolled out in these mandals much later than in the treatment group. We also test robustness to excluding the buffer group from both the numerator and denominator and results are qualitatively unchanged (available online).

<sup>25</sup>Spatial autocorrelation is unlikely to be a concern for inference on direct treatment effects as treatment is spatially negatively autocorrelated by design (as randomization is stratified geographically), but could be for inference on neighborhood measures which are positively autocorrelated.

inference.<sup>26</sup> Regressions using census data are unweighted, while those using survey samples are weighted by inverse sampling probabilities to be representative of the universe of jobcard-holders. When using survey data on wages and earnings we trim the top 0.5% of observations in both treatment and control groups to remove outliers, but all results are robust to including them (available online).

## 3 Results

### 3.1 Effects on earnings and poverty

We start by reporting impacts on income and poverty using census data. Panel (a) of Table 1 reports experimental estimates of impact on income categories in the SECC data, showing marginal effects from logistic regressions<sup>27</sup> for each category individually and an ordered logistic regression across all categories. The lowest, middle, and highest income categories correspond to households where the highest-earner reports monthly earnings below Rs. 5,000, between Rs. 5,000 and Rs. 10,000, and greater than Rs. 10,000, respectively. Treatment led to 4.1 percentage points fewer households in the lowest income category (a 4.9% reduction on a base of 83%), 2.6 percentage points more households in the middle category (a 20% increase on a base of 13%), and 1.4 percentage points more households in the highest category (a 36.8% increase on a base of 3.8%). Using an ordered logit specification, we see that treatment significantly increased the log-odds ratio of being in a higher income category. All estimates using SECC data are unaltered by controls for (arguably predetermined) demographic characteristics such as age of household head, caste, and literacy.

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

We therefore turn to our survey data, which are representative of the households registered for NREGS (comprising half the rural population), for a better sense of magnitudes of impact on the population that the program aimed to serve. Columns 1 and 2 of Panel (b)

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<sup>26</sup>Our randomization and sampling procedures were such that we cannot use randomization inference for survey outcomes. We first assigned mandals to treatment, control, and non-study groups and then sampled within treatment and control mandals. Re-randomization typically assigns to treatment or control some of the non-study mandals for which we have no survey data.

<sup>27</sup>Using probits or linear probability model makes no qualitative or significant quantitative difference to the results (available online).



of Table 1 report estimated impacts on annual household income, with and without controls for the mean income in the same village at baseline. In both specifications we estimate that treatment increased annual income by over Rs. 8,700. This is a large effect, equal to 12.7% of the control group mean or 17.9% of the national expenditure-based rural poverty line for a family of 5 in 2011-12, which was Rs. 48,960 (Government of India, Planning Commission, 2013). Of course, expenditure- and income-based poverty lines may differ and this comparison is illustrative only. But if these lines were taken as equivalent, we estimate a 4.9 percentage point or 17.4% reduction in poverty for the universe of potential NREGS beneficiaries (Figure A.4).

### 3.2 Direct versus indirect effects on earnings

In an accounting sense, the effects on earnings and poverty we find on NREGS jobcard holders in the survey data above must work through some combination of increases in households' earnings from the NREGS itself and increases in their non-program (i.e. private sector) earnings. We examine this decomposition using our survey data, which includes measures of six income categories: NREGS, agricultural labor income, other physical labor income, income from own farm, income from own business, and miscellaneous income (which includes all remaining sources, including salaried income).

Columns 3-8 of Panel (b) of Table 1 report treatment effects on various income categories separately. Earnings in most categories increase, with significant gains in wage labor – both agricultural and other. Effects on own farm earnings (which include earnings from livestock) are positive but insignificant. NREGS earnings increase modestly ( $p = 0.12$ ) and the increase in *annual* NREGS earnings is consistent with treatment effects on *weekly* NREGS earnings reported in MNS (estimated during the peak NREGS period).<sup>28</sup> Overall, the increase in NREGS income accounts for only 10% of the increase in total earnings (proportional to the share of NREGS in control group income). Nearly 90% of the income gains are attributable to non-NREGS earnings, with the primary driver being an increase in earnings from market labor, both in the agricultural and non-agricultural sectors.

Data from the control group help clarify why increases in NREGS earnings on their own

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<sup>28</sup>In MNS, we report a significant increase in weekly earnings of Rs. 35/week during the seven weeks corresponding to the peak NREGS season. Average weekly NREGS earnings per year are 49.6% of the average weekly NREGS earnings in these seven weeks (calculated using official payment records in the control mandals as shown in Figure A.3). Thus, the annualized treatment effect on NREGS earnings should be Rs.  $35 \times 52 \text{ weeks} \times 0.496$  or Rs. 903/year, which is exactly in line with the Rs. 914 measured in the annual recall data reported in Table 1. However, the results here are marginally insignificant ( $p = 0.12$ ) compared to the significant ones in MNS, likely due to the lower precision of annual recall data compared to the more precise data collected for the seven-week reference period in MNS, with job cards on hand to aid recall.

are unlikely to make a meaningful impact on poverty. NREGS earnings specifically account for just 7% of control group earnings, compared to 93% from other sources (which is broadly consistent with nationally representative statistics, in which the NREGS is a relatively small source of employment). Even a 19% increase in NREGS income as we find, would only increase total income by 1.3%. In contrast, the average household earns roughly 1/3 of its income from wage labor, primarily in agriculture, and 1/3 from self-employment activities, also primarily in agriculture. Thus, the effect of improving NREGS on market wages and employment is likely to be a much more important channel of overall impact on rural incomes (as we show below).

Figure A.4 plots the empirical CDF of household earnings for treatment and control groups in our survey data. We see income gains throughout the distribution, with the treatment income distribution in the treatment group first-order stochastically dominating that in the control group. The broad-based gains seen in the universe of NREGS jobcard holders are also seen in the SECC data representing the full population.

We test for differential treatment effects in our survey data by measures of household vulnerability using a linear interaction specification (Table A.5). We find no differential impacts by caste or education, suggesting broad-based income gains consistent with Figure A.4. We also see that the treatment effects on earnings are *not seen* for households who are less likely to work (those headed by widows or those eligible for social security pensions), thereby corroborating the point that increased labor market earnings were the key channel for the overall income gains we find. Overall, increases in earnings were broad-based across categories of households who were registered for the NREGS, but did not accrue to households whose members were unable to work.

We next examine the drivers of private sector labor earnings gains – in particular, wages and employment.

### 3.2.1 Wages

To examine wage effects we use our survey data for the month of June 2012, the peak NREGS period, for which we collected detailed data on wages and employment for all survey respondents (the SECC does not include wage information). We define the dependent variable as the average daily wage earned on private-sector work across all respondents who report a private-sector wage. We report results for the full sample of workers reporting a wage, with checks for robustness with respect to sample composition in Section 4.5 below.

The naive ITT (comparing means across treatment and control mandals) suggests a significant increase of Rs. 7.9 in daily market wages (Table 2), equal to 6.4% of the control group mean. By itself, this is slightly larger than the highest estimates of the market-wage

impacts of the rollout of the NREGS itself as reported by Imbert and Papp (2015).

In addition, we find robust evidence of spillover effects on market wages, consistent in sign with the direct effects (Column 1, Table 2). The coefficient  $\beta_N$  is statistically significant and slightly larger in magnitude than  $\beta_T$ , the impact of own treatment status.<sup>29</sup> In other words, going from having no neighbors treated to having all neighboring villages treated is as important as having your own mandal treated. These results provide independent corroboration of the evidence from the ITT specifications using a distinct source of experimental variation (i.e. variation in neighbors' as opposed to own treatment status), with the main and spillover results both rejecting the null of no effect in the same direction.

As seen in Figure 2, the spillovers to the control group will bias the naive ITT effects downwards. We estimate that the mean effect of the reform on control mandals was a significant Rs. 5.1 increase in wages (multiplying  $\beta_N$  of 11 by 0.45, which is the mean value of  $\tilde{N}_{Cp}$ ), which is 65% of the naive ITT estimate). Adjusting the naive ITT for this downward bias, we estimate an adjusted treatment effect on the treatment group of Rs. 13, or 10.5% of the control group mean. Note that standard errors for adjusted effects on this and other wage and employment outcomes are slightly smaller when we use the method of (Conley, 2008), so that the default clustered standard errors are likely conservative.

Our data on wages and employment are for the month of June, since this is a peak month of NREGS, and the period when our experiment had the greatest relevance for improving NREGS implementation. However, our income results are based on annual data and so a natural question is to ask whether the wage increases in June spilled over to the rest of the year even when NREGS activity was much lower. While we do not have detailed data on wages by month, we conducted interviews with village leaders, where we asked them to report the “going wage rate” for each month of the year. Figure A.5 plots impacts on this measure by month. The estimates are imprecise (since we have only one data point per village-month), but suggest that there were also temporal spillovers in wage gains from peak NREGS months (May and June) to the rest of the year.<sup>30</sup>

This pattern is consistent with several (non mutually exclusive) interpretations. First, while not much NREGS work appears to have been done during the end of the year (Figure A.3), almost all study villages had at least one NREGS project active for a majority of 2012, with availability dropping to a low of 40-50% of villages towards the end of the year (Figure A.5). Thus NREGS may still have been a viable outside option in many villages even outside the peak season. Second, wages may also be linked across time due to various

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<sup>29</sup>We examine in Table A.6 whether  $\beta_N$  differs for villages in treated as opposed to control mandals. We find evidence of spillovers in both cases and never reject that these are equal.

<sup>30</sup>A similar pattern is seen in Imbert and Papp (2015), who also find positive but imprecise wage impacts in the “rainy season” amounting to 61% of the dry season impact (which is the estimate they also focus on).

forms of nominal rigidity, including concerns for fairness (Kaur, 2019) and labor tying over the agricultural cycle (Bardhan, 1983; Mukherjee and Ray, 1995). The latter literature in particular suggests that landlords who provide insurance in the lean season pay lower wages in the peak season.<sup>31</sup> In these models, better NREGS availability and higher market wages in the lean season would imply a reduced need for insurance from landlords and a resulting higher wage in the peak (non-NREGS) season.

### 3.2.2 Employment and Migration

Next, we examine treatment effects on employment in June. We classify days spent during the month of June into three categories: days spent idle or doing unpaid work, days spent working on the NREGS, and days spent working in the private sector.

We find a significant *decrease* in days spent idle, with impacts coming from both own treatment status as well as spillovers (Table 2). The spillover effect is significant and larger in magnitude than the effect of the village’s own treatment status. The adjusted treatment effect is 2.4 days, or 13.3% of the control group mean.

The decrease in days spent idle appears to have been reallocated across both NREGS work<sup>32</sup> and private sector work, with significant adjusted treatment effects on both outcomes (Columns 4 and 5).<sup>33</sup> NREGS work increases by 28.8%, consistent with the increases in NREGS payments and access we find in MNS.

We also find significant *positive* effects on private sector employment, with an adjusted treatment effect of 20% of the control mean. As in the case of wages, the spillover results are directionally similar to the main effects, which provides an independent exogenous source of validation of the main results. Specifically, the ITT estimates use the exogenous variation induced by random assignment of mandals to a binary treatment or control status. However, the spillover estimates use the continuous variation induced by the (also exogenously determined) fraction of neighboring mandals that were treated. Thus, the directional similarity of the naive ITT and the spillover effects reinforce the finding that the effects of improv-

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<sup>31</sup>One common way in which this insurance is provided is through loans in the lean season, with “interest” collected in the form of lower wages in the peak season (Mukherjee and Ray, 1995).

<sup>32</sup>Note that in Table 5 of MNS, we report impacts on the extensive margin of whether a household worked on NREGS (and find a significant positive impact in treated areas) because our main concern there was with impact on *access to work*. Here we focus on decomposing total change in employment across NREGS and market labor, and hence present results on average days worked.

<sup>33</sup>Note that the number of observations for days worked on NREGS is larger: This is because we can impute zero time spent working on the NREGS in June for individuals who reported never working on NREGS. We do not impute missing values for private-sector work. Response rates for private-sector work do not differ by treatment status (Table A.13), and the results are unchanged if we restrict attention to respondents for whom we observe all three – NREGS work, private work, idle/unpaid work – outcomes (available online)

ing NREGS implementation are positive on *both* market wages and employment.<sup>34</sup> It also highlights that not correcting for spillovers can lead to incorrect estimation and inference regarding impacts on both wages and employment.<sup>35</sup>

Finally, we examine impacts on labor allocation through migration. Our survey asked two questions about migration for each family member: whether or not they spent any days working outside of the village in the last year, and if so how many such days. Table A.11 reports effects on each measure. We estimate a small and insignificant *increase* in migration on both the extensive and intensive margins. Spillover effects are also positive but generally not statistically significant. As our migration questions may fail to capture permanent migration, we also examine impacts on household size and again find no significant difference. These results are consistent with the existence of countervailing forces that may offset each other: higher rural wages may make migration less attractive (Imbert and Papp, 2019), while higher rural incomes make it easier to finance the search costs of migration (Bryan et al., 2014; Bazzi, 2017).

### 3.3 Effects on consumer goods prices

The earnings results above show impacts on nominal, and not real, earnings. Since Smart-cards affected local factor (i.e. labor) prices, they could also have affected the prices of local final goods, and thus the overall price level facing consumers, if local markets were not sufficiently well-integrated into larger product markets.

To test for impacts on consumer goods prices we use data from the 68th round of the NSS. The survey contains detailed household  $\times$  item-level data on expenditure and number of units purchased for a sample representative at the state and sector level (rural and urban). The data cover over 300 goods and services in categories including food, fuel, clothing, rent and other fees or services over mixed reference periods varying from a week to a year. We define unit costs as the ratio of expenditure to units purchased, restricting the analysis to goods that have precise measures of unit quantities (e.g. kilogram or liter) and dropping goods that likely vary a great deal in quality (e.g. clothes and shoes). We then test for price impacts in two ways. First, we define a price index  $P_{vd}$  equal to the price of purchasing

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<sup>34</sup>Our focus in this paper is on household-level economic outcomes and not on intra-household heterogeneity. For completeness, we examine heterogeneity of wage and employment effects by gender. Point estimates of the impacts on female wages are lower than those on male wages, but not significantly so. On employment, the increase in days worked is always greater for men than for women, but the differences are not always significant (available online).

<sup>35</sup>The finding that increasing the effective presence of a public employment program increased not only market wages but also market employment differs from existing evidence to date, which typically finds crowd-out of private employment by public employment. This divergence is likely explained by important differences in study design, empirical strategy, and data; we discuss these in Section 5.

the mean bundle of goods in the control group, evaluated at local village prices, following Deaton and Tarozzi (2000):

$$P_{vd} = \sum_{c=1}^n \bar{q}_{cd} \tilde{p}_{cv} \quad (4)$$

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

The set of goods for which non-zero quantities are purchased varies widely across households and, to a lesser extent, across villages. To ensure that we are picking up effects on prices (rather than compositional effects on the basket of goods purchased), we initially restrict attention to goods purchased at least once in every village in our sample. The drawback of this approach is that it excludes roughly 40% of the expenditure per village in our sample. We therefore also present a complementary set of results in which we calculate (4) using all available data. In addition, we report results using (the log of) unit cost defined at the household-commodity level as the dependent variable and including all available data. While these later specifications potentially blur together effects on prices with effects on the composition of expenditure, they do not drop any information.

Regardless of method, we find little evidence of impacts on price levels (Table 3). The point estimates are small and insignificant and, when we use the full information available, precise enough to rule out effects as large as those we found earlier for wages.<sup>36</sup> These results suggest that the treated areas are sufficiently well-integrated into product markets for the most commonly consumed goods (like food grains) that higher local wages and incomes did not affect average prices. Alternatively, they are also consistent with the finding (see below) that household consumption did not go up significantly (since they seem to have treated the income gain as temporary and saved or invested most of it), which may have limited potential upward pressure on prices. Overall, the lack of impact on prices suggests that the increases in wages and income we find are real and not just nominal.

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<sup>36</sup>The  $R^2$  values reported in Table 3 are close to 1. This is because the village-level regressions (in Columns 1 and 2) rely on just 60 observations, and district fixed effects account for a substantial extent of the variation. Similarly, most of the variation in the item  $\times$  household level regression (Column 3) is accounted for by item fixed effects.

## 4 Understanding labor market impacts

We turn next to changes in labor supply and demand that may explain the pattern of wage and employment impacts described above, along with other longer-term impacts and robustness checks.

### 4.1 Labor supply

One mechanism that could contribute to the increased wages we find is labor market competition: a (better-run) public employment guarantee may improve the outside option for workers, shifting the curve describing the supply of labor to the private sector inwards and thus driving up wages.<sup>37</sup> Theoretical models emphasize this mechanism (Ravallion, 1987; Basu et al., 2009), and it has motivated earlier work on NREGS wage impacts (e.g. Imbert and Papp (2015)), but prior work has not been able to directly test this hypothesis. Specifically, since prior work only has data on market wages, it has not been possible to distinguish shifts in labor supply and labor demand.

We are able to test for a shift in labor supply using data on reservation wages that we elicited in our survey. Specifically, we asked respondents if in the month of June they would have been “willing to work for someone else for a daily wage of Rs. X,” where X started at Rs. 20 (15% of average wage) and increased in Rs. 5 increments until the respondent agreed. One advantage of this measure is that it applies to everyone, and not only to those who actually worked. Respondents appeared to understand the question, with 98% of those who worked reporting reservation wages less than or equal to the wages they actually earned (Table A.13).

We find that treatment significantly increased workers’ reservation wages by approximately Rs. 5.5, or 5.7% of the control group mean (Table 2, columns 1-2). The increase in reservation wage in treated areas provides direct evidence that making NREGS a more appealing option would have required private employers to raise wages to attract workers.

Further, consistent with NREGS improvements from Smartcards not taking place in control mandals, we find *no* significant spillover effect on reservation wages. The coefficient on fraction of neighboring villages treated is small and not statistically significant (Table 2, column 2). However, while we find no evidence of spatial spillovers on reservation wages, we do find strong evidence of such spillovers on market wages. This suggests that reservation wages respond to NREGS improvements in the same village (which now provide a more credible

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<sup>37</sup>In the extreme, if all jobs were perfectly substitutable and the NREGS wage were constant, the supply of labor to the private sector at any lower wage would fall to zero. In this case the effect of competition with NREGS on private sector labor markets is equivalent to the effect of an enforced minimum wage.

outside option), but are not affected by the number of treated villages in the neighborhood. However, market wages face more upward pressure when a larger fraction of workers in the neighborhood are treated.

The labor supply response to the improved outside option helps explain our wage results, but on its own an inward shift in supply would clearly lead to lower employment. Below we explore explanations for why employment may have increased.

## 4.2 Employer market power

As seen above, an improved NREGS may increase workers' bargaining power with employers by providing them with a more credible outside option.<sup>38</sup> If labor markets were competitive, this should lead to a reduction in market employment. However, if labor markets in rural Andhra Pradesh were oligopsonistic to begin with, then such a wage increase could also lead to an increase in employment.

Figure 3 provides a simple sketch of this argument. When employers do not have market power and are price takers, a (binding) increase in reservation wages would lead to an increase in market wages and a reduction in employment. However, when employers do have market power, they will ration employment relative to competitive markets and keep wages below the marginal product of labor to maximize their rents. In such a setting, an improvement in the outside option measured by the reservation wage (or enforcement of a minimum wage above the monopsonistic equilibrium) will still exert upward pressure on wages but can result in an *increase* rather than a decrease in employment (see Manning (2003) for a more extended discussion).

We find evidence consistent with the existence of employer market power in our setting. Using household level data from the SECC, we construct a normalized Herfindahl-Hirschman index (HHI) of land holdings at the village level as a measure of land concentration.<sup>39</sup> We also use an alternative measure of village-level land concentration based only on concentration among households with more than 1 acre of land, since those with landholdings less than an acre are highly unlikely to employ private sector labor. On both measures, we find that villages with a higher level of land concentration have lower levels of employment, and we find a significantly greater positive effect on employment in these villages when they

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<sup>38</sup>Note that an increase in worker bargaining power does not have to lead to higher wages through direct negotiation. If their participation constraint goes up (as seen by the increased reservation wages), landlords will need to raise wages simply to get workers to show up.

<sup>39</sup>We calculate the HHI as follows:  $H_p = \sum_{i=1}^N s_i^2$ , where  $s$  is the share of the village's land owned by each household  $i$  in village  $p$ , and  $N$  is the total number of households in the village. We then normalize  $H$  to arrive at  $H_p^* = \frac{H_p - \frac{1}{N_p}}{1 - \frac{1}{N_p}}$ .



are treated (Table 4, Panel (a), Columns 3 and 6). Both facts (the negative coefficient on employment on  $H^*$  and the positive coefficient on  $T \times H^*$ ) are consistent with monopsonistic labor markets.<sup>40</sup>

To better interpret the magnitudes of these effects, we also present the results for a standardized version of  $H^*$  (normalized to mean zero, and standard deviation of one) in Panel (b). Treated villages whose land concentration is a standard deviation above the mean had 0.5 to 0.6 days of additional private-sector employment (Table 4, Panel (b)). The estimates in Panel (b) also allow us to account for the possibility that the intervention changed land concentration (which is a concern since the SECC data were collected after the intervention). We do this by standardizing the HHI separately *within* treatment and control groups for the results reported in Table 4, Panel (b). Thus, the results can be interpreted as showing the differential treatment effects on employment at the same level of relative land concentration within the treatment and control groups. Finally, we also verify that the treatment did not affect land concentration: the mean difference in HHI is 0.0001,  $p = 0.9$  (table available online).

This measure of land concentration is likely to understate the extent of employers' market power. For instance, as suggested by Anderson et al. (2015), land-holders of the same caste (*jati*) may collude quite effectively, making land-holdings more concentrated than measured by the HHI on household-level landholdings. Unfortunately, data on *jati* in the SECC has not been released by the Government of India. Yet, despite the likely understatement of market power, this measure of concentration alone can explain nearly 35% of the positive effects on employment we find. The mean HHI in our data is 0.029, and the coefficient on the interaction with treatment is 6.5 (Table 4, Panel (a), Column 6). Multiplying the 2 gives us 0.19 which is 35% of the ITT effect on employment of 0.54.<sup>41</sup>

### 4.3 Labor demand

A potentially contributing factor to increases in both wages and employment is an increase in labor demand (i.e. an outward shift of the demand curve). Since we find an increase in days worked on the NREGS in MNS, one plausible mechanism of such an increase in demand would be an increase in productivity-augmenting public assets created under NREGS (such

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<sup>40</sup>The negative coefficient on employment on  $H^*$  is only a correlation across villages, but it is consistent with employers having market power. The key test of monopsony is that an exogenous increase in the reservation wage will lead to an increase in market employment when employers have greater market power (proxied by land concentration in this case), and this is what we find evidence of. Predictions on whether the change in market wage will be higher under monopsony are less clear. Consistent with this, we do not find a differential impact on market wage as a function of land concentration (available online).

<sup>41</sup>We do not extend this analysis to include spillovers due to the additional complexity of accounting for differential concentration of land-holdings in neighboring villages.

as roads, irrigation canals, etc).

However, none of the evidence we have supports this conjecture, and some simple calculations based on an illustrative model suggest that the asset creation channel was likely to be small. First, we find no changes in the number and composition of NREGS projects *reported* across treatment and control mandals (Table A.15). While we do not have independent measures of assets created, we can directly test for productivity gains by examining changes in land utilization, which might result from land improvements or minor irrigation works. We do not see effects on the amount of land sown or on the total area irrigated (Table A.16), ruling out effect sizes larger than 16% and 10%, respectively. Finally, Appendix B shows that the assets created would have to be implausibly large to generate the wage and employment results: in short, the entire capital stock in treated areas would have to increase by 28%, while even the most generous assumptions suggest an increase on the order of 1%.<sup>42</sup>

A second plausible mechanism is an increase in aggregate demand through increased NREGS income. However, the government did not spend any more money in treated areas (MNS), and although more money made it into the hands of workers as opposed to corrupt officials, the magnitudes are too small for this channel to be important. Even, in the extreme case where the MPC from additional NREGS income for workers is 1 and that of intermediate officials is zero, the total increase in NREGS earnings in treated areas is only 1.3% of the control mean (Table 2), which is unlikely to create much additional demand.

A more plausible aggregate demand channel is that the initial increase in labor market income may have further augmented labor demand through an increase in demand for locally-produced products. However, we find no evidence of this mechanism during the 2-year horizon of our study. In particular, we see no increases in household consumption spending in our surveys that would point to this mechanism. Table A.14 shows estimated impacts on household expenditure on both frequently (Columns 1 & 2) and infrequently (Columns 3 & 4) purchased items from our survey. Both estimates are small and statistically insignificant. However, the consumption results are not very precisely estimated, because expenditure was not a focus of our household survey.<sup>43</sup> Thus, though the point estimates are close to zero, we cannot rule an 8% increase in expenditure on frequently purchased items or a 15% increase in spending on infrequently purchased items. In Column 5 we use monthly per capita

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<sup>42</sup>In brief, we (generously) assume that the 24% increase in payments received by workers we find in MNS is the increase in NREGS asset creation; we estimate that NREGS capital formation during 2010-12 is 4% of all capital stock (using NREGS budget and National Accounts data); multiplying the two gives us the total increase in capital stock due to the intervention as 0.96%.

<sup>43</sup>The entire expenditure module in our survey was a single page covering 26 categories of expenditure; for comparison, the analogous NSS consumer expenditure module is 12 pages long and covers 23 categories of *cereals alone*. The survey design reflects our focus on measuring leakage in NREGS earnings and impacts on earnings from deploying Smartcards.

expenditure as measured by the NSS, which gives us a far smaller sample but arguably a more comprehensive measure of expenditure. The estimated treatment effect is positive and insignificant, but again imprecise, and we cannot rule out a 16% increase in expenditure (consistent with a marginal propensity to consume ranging from 0 to 1, and hence not very informative).

We also do not see increases in the consumer goods prices that would transmit a demand shock to producers and ultimately to labor markets. Finally, the large increase in market wages we document are also inconsistent with a Keynesian aggregate-demand channel driven by slack labor demand or by other models of involuntary unemployment at a given market wage. In these models, increases in labor demand would typically increase employment without increasing wages, which is not what we see.

To summarize, we find no direct evidence of an increase in labor demand with most relevant point estimates being close to zero. However, given the confidence intervals, we cannot rule out the possibility that increases in labor demand had at least some role to play in increasing both wages and employment.

## 4.4 Longer-run effects

The main focus of this paper is on studying the *labor market* GE effects of improving NREGS implementation, and the overall impact of the reform on income and poverty. However, the resulting increase in incomes of the rural poor are likely to have further cascading effects on overall economic activity. In this section, we present additional results that are consistent with an overall expansion of economic activity in treated areas.

### 4.4.1 Credit and assets

Our survey collected information on two asset categories: liquid savings and land-ownership. We find positive estimated effects on both measures (Table 5a, Panel (a)), with the effect on land-ownership significant; treatment increased the share of households that owned some land by 4.9 percentage points, or 8.3%. We also see a 16% increase in total borrowing, which could reflect crowding-in of borrowing to finance asset purchases. Importantly, this is driven entirely by increases in informal borrowing, with no increase in borrowing from formal financial institutions, consistent with the fact that Smartcards were not a viable means of accessing financial services beyond NREGS and pension benefits (Table A.17).

After land, livestock is typically the most important asset category for low-income households in rural India, and a relatively easy one to adjust as a buffer stock. We test for effects on livestock holdings using data from the Government of India’s 2012 Livestock Census. The

Census reports estimated numbers of 13 different livestock categories; in Table 5b we report impacts on the 7 categories for which the average control mandal has at least 100 animals. We find positive impacts on five of the seven livestock categories, including substantial increases in the number of buffaloes ( $p = 0.035$ ). A Wald test of joint significance across the livestock categories rejects the null of no impacts ( $p = 0.04$ ). The 21% increase in buffalo holdings is especially striking since these are among the highest-returning livestock asset in rural India, but often not accessible to the poor because of the upfront costs of purchasing them (Rosenzweig and Wolpin, 1993).

Overall, we see positive impacts on holdings of arguably the two most important investment vehicles available to the poor (land and livestock). This is consistent with the view that households saved some or all of the increased earnings they received due to Smartcards, and acquired productive assets in the process.<sup>44</sup>

#### 4.4.2 Non-agricultural employment

Over time, as households see the increases in labor market income as permanent, they are likely to spend this additional income. Combined with the increases in productive assets this may, over time, result in increased economic activity overall. We use data from the 2013 Economic Census – three years after the start of the intervention, and one year after the end of the experiment – to examine this possibility. The Economic Census surveys every single nonfarm establishment, capturing data on the type of activity as well as number of employees. Since 40% of control villages had been exposed to the Smartcards intervention by 2013 (as compared to 87% of treatment), these results should be interpreted as the longer-term effects of the two additional years of exposure to Smartcards during the experimental study period (2010-2012) as well as the differential exposure in the year after the experiment (2012-13).

We find significant increases in treatment areas of 23% in the number of enterprises and 35% in the number of employees (Table 6). These results hold with and without controls for the 2005 Economic Census, as well as the total working population from the 2011 Population Census. Since the average working age population in a mandal is 29,600, and the average number of employees in the control mandals is 6,797, this represents an 8 percentage point increase in non-agricultural employment on a base of 23%.

Studying the reasons for these long-term increases in overall economic activity is beyond the scope of this paper, which aims to study the contemporaneous labor-market GE effect-

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<sup>44</sup>This result also suggests that focusing solely on consumption as a measure of welfare (as is common in household surveys) may have missed the full picture and concluded that there was no effect of improving NREGS implementation, when in fact, there was a considerable increase in income which was being invested/saved.

s of a well-implemented public employment program. Yet, these increases in longer-term non-agricultural employment are consistent with the idea that exogenous wage increases in low-income settings can promote a structural transformation. These results are for example consistent with the “big push” model of development developed by Magruder (2013) where a mandated increase in the wages of the rural poor drives coordination on a modernizing, higher-investment equilibrium. They are also consistent with higher wages inducing productivity-enhancing mechanization as shown in the case of the US by Hornbeck and Naidu (2014). Understanding this channel better is an important topic for future research.

## 4.5 Robustness

We run a number of robustness tests. These include, amongst others, (i) alternative choices of sample (e.g. restricting to only those of working age, or only NREGS workers) (ii) selection/composition effects on those who report work (or wages), including weighting wages by number of days worked and (iii) spillover-adjusted estimates for all outcome data.

The alternative sample choices do not qualitatively change any of our wage and employment results, and we can also show no significant composition/selection effects on those who report work (available online). The spillovers are almost always in the same direction as the main ITT effects, but are typically not significant for the outcomes based on annual data or for outcomes that are stocks and not flows (income results in Tables A.8 and A.9), migration (Table A.11), price (Table A.10) and savings, credit, and assets (Table A.12). This is not surprising since the experiment was most powered to detect effects on NREGS and labor markets during the peak period of NREGS itself (which is why our surveys collected data on wages and employment for June).

## 5 Discussion

Our results add to previous work on the impacts of workfare programs around the world as well as the impacts of the NREGS in particular.

Relative to work outside of India, a key contribution is the scale of the experimental variation we study. Two well-identified studies have estimated zero or negative effects on income, for example, but because they vary access to public employment at the individual as opposed to the regional level they are not able to estimate effects on private sector wages and employment and thus the total effects these interventions would have had if implemented at scale (Bertrand et al., 2017; Beegle et al., 2017). Our results suggest that these indirect effects contribute critically to the overall income effects of the NREGS, accounting for 90%

of total income gains.

Relative to work on the NREGS specifically, we make contributions in both identification and measurement. Existing work (reviewed in Sukhtankar (2017)) has sought to exploit the phased initial rollout of the program during 2006-2008 for identification in one way or another. However, both difference-in-differences and regression discontinuity approaches based on this rollout have limitations.<sup>45</sup> In addition, correcting estimates for spillovers has usually been infeasible as most outcome data are geocoded at the district level, the same level as the identifying variation.<sup>46</sup> Due in part to these challenges, existing estimates of key parameters have varied widely.<sup>47</sup> The combination of experimental variation with units of randomization and measurement appropriately-sized to test, and correct for spillovers allows us to improve identification relative to the literature to date.

Our measurement of key objects of interest including implementation quality, income, employment, reservation wages, and employer market power also allows for meaningful contributions. First, differences in findings across studies may reflect differences in *unmeasured* implementation quality. Estimates that exploit the staggered NREGS rollout are especially sensitive to this issue because implementation in the early years of the program was thought to be particularly weak (Mehrotra, 2008), and thus, may not accurately reflect impacts under higher-quality steady state program implementation. Our main contribution in this regard is to have household-level evidence (from MNS) that the treatment did improve the effective presence of NREGS on the ground on almost every metric that matters for its credibility as an outside option (including access to work, reduced corruption and leakage, and reliability and convenience of collecting payments). This allows our results on labor markets and income to be interpreted as the downstream effects of doing so.

Second, our data on incomes (from both surveys and a census) allow us to fill a gap in the current literature, where studying impacts of NREGS on income and poverty has been constrained by the fact that the NSS does not collect data on incomes, and conducted only a “thin” round on consumption in the relevant years of program rollout (that can only yield state-level estimates and not district-level ones).

Third, existing evidence on employment has also been constrained by measurement. For instance, Imbert and Papp (2015) report that the increase in public employment under

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<sup>45</sup>Specifically, the parallel trends assumption required for differences-in-differences estimation does not hold for many outcomes without additional controls, while small sample sizes limit the precision and power of regression discontinuity estimators at reasonable bandwidth choices (Sukhtankar, 2017).

<sup>46</sup>In one recent exception, Merfeld (2019) finds evidence of spillovers using ARIS/REDS data with village geo-identifiers, suggesting that ignoring spatial spillovers can substantially bias estimates.

<sup>47</sup>For wages, for example, studies using a difference-in-differences approach estimate a positive 4-5% effect on rural unskilled wages (Imbert and Papp, 2015; Berg et al., 2018; Azam, 2012) while a study using a regression discontinuity approach finds no impact (Zimmermann, 2015).

NREGS was accompanied by a 1-1 crowd-out in private employment. However, this measure of “private sector employment” (based on NSS data) includes wage employment for others *as well as* domestic work and self-employment. This is important since self-employment may represent low marginal product activities, the crowding out of which may not matter much for efficiency; indeed, Imbert and Papp (2015) themselves note that “the fall in private sector work may in part represent a fall in disguised unemployment or private sector work with close to zero productivity.” We directly measure paid market employment (unconfounded with self-employment), which is the main outcome of interest, and also correct for spatial spillovers, which turn out to be substantial. In particular, not accounting for and adjusting the spillovers would have underestimated both the wage and employment impacts.

Finally, our data on reservation wages allows us to confirm that improving NREGS did lead to an inward shift in labor supply (which cannot be inferred just from market wages), and our data on land-holdings allow us to construct measures of land concentration and test for employer market power. Overall, the evidence we find of monopsony power is both novel and central to explaining the overall pattern that wages and employment both went up together. There is a long-standing debate over the market power of rural employers, with several studies suggesting the existence of such power (e.g. Griffin (1979); Anderson et al. (2015)). However, to the best of our knowledge there has been no direct evidence of monopsony in rural labor markets to date.

A key prediction of the theory of monopsony is that an increase in the wage floor (either through a minimum wage or improved outside option) will lead to an increase in employment in the presence of employer market power, and this is exactly what we find. The interaction of the NREGS with an imperfect, frictional labor market is thus key to understanding its impacts on poverty. In this sense our results fit within the new literature on frictions in rural labor markets including (Breza et al., 2019), as well as literature finding evidence of employer market power even in labor markets that would plausibly be more competitive than rural labor markets in a developing country (Manning, 2003; Naidu et al., 2016; Dube et al., 2020).

## 6 Conclusion

This paper contributes to our understanding of the economic impact of public employment programs in developing countries. In particular, it contributes (a) improved identification: using experimental variation with units of randomization large enough to capture general equilibrium effects and units of measurement granular enough to capture spatial spillover effects; (b) measures of implementation quality: enabling us to interpret impacts as the

results of demonstrable changes in *actual* presence of the program; and (c) new outcome measures: including reservation wages, income, and assets, with independent census data on the latter two.

Overall, the results are consistent with the following broad narrative. Improving NREGS implementation improved its value as an outside option for the rural poor (as seen in increased reservation wages). This in turn forced employers to raise market wages to attract workers. Crucially for efficiency, this *raised* private employment – at least in part because of the wage increase was in the context of monopsonistic labor markets. Higher wages and income in turn induced increased credit and purchase of assets. Over time, the combination of higher wages, credit, and investment appear to have stimulated broader economic activity as seen by the increase in non-agricultural enterprises as well as employment in the Economic Census.

One natural question is how the effects of *improving* the NREGS compare to the (hypothetical) comparison between a “well-implemented NREGS” and “no NREGS.” We expect that the effects would be broadly comparable, but with larger income effects. Smartcards increased the labor-market appeal of and participation in NREGS without increasing fund flows. In contrast, the NREGS per se transfers large sums from urban to rural areas. A de novo rollout of a *well-implemented* NREGS would thus likely have effects on wages, employment and income larger than those we find here, though in the same direction.

Our results highlight the value of experimental “at scale” evaluation of large social programs. In particular, the experimental findings have led us to question, test, and reverse our own priors on NREGS. Our prior (following the default view of competitive labor markets) was that wage increases without corresponding gains in productivity would likely reduce private employment and potentially attenuate impacts on poverty. Yet, finding positive effects on employment forced us to question the default assumption of competitive labor markets, and look for credible ways to test this assumption. Our analysis and results show that employers in this setting do have market power, and that programs like NREGS can not only reduce poverty, but also be efficiency enhancing in such settings. In the absence of an at scale experiment, we may have thought that the results on income and employment were being driven by identification issues, and not probed the monopsony possibility as deeply.

Our results also highlight political economy issues in the design and implementation of anti-poverty programs in developing countries. Landlords typically benefit at the cost of workers from low wages and from the wage volatility induced by productivity shocks, and may be hurt by programs like NREGS that raise wages and/or provide wage insurance to the rural poor (Jayachandran, 2006). Anderson et al. (2015) have argued that “a primary reason... for landlords to control governance is to *thwart* implementation of centrally mandated initiatives that would raise wages at the village level.” While we do not directly observe landlord or



employer profits, our findings show that improving NREGS substantially raised market wages and employment, and likely reduced monopsonistic rents of landlords. The results underscore landlords' incentive to oppose such improvements and helps rationalize their documented resistance to the program (Khera, 2011; Misra, 2019; Mukherji and Jha, 2017).

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

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Table 1: Income  
(a) SECC data

	Lowest bracket (< Rs. 5,000)		Middle bracket (Rs. 5,000 - 10,000)		Highest bracket (> Rs. 10,000)		Income bracket 3 levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-.041*** (.014) [.005]	-.039*** (.014) [.009]	.026** (.011) [.022]	.025** (.011) [.031]	.014** (.0065) [.029]	.012** (.0061) [.05]	-.041*** (.014)	-.039*** (.014)
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
Control mean	.83	.83	.13	.13	.038	.038		
Adjusted $R^2$	.01	.028	.014	.024	.015	.041	.008	.024
Observations	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M	1.8 M
Estimator	Logit	Logit	Logit	Logit	Logit	Logit	Ordered logit	Ordered logit

(b) Survey data (Rs. per year)

	Total income		NREGS	Agricultural labor	Other labor	Farm	Business	Misc.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	8761** (3722)	9511** (3723)	914 (588)	3276** (1467)	3270** (1305)	2166 (2302)	-642 (1325)	528 (2103)
Baseline lag	Yes	No	No	No	No	No	No	No
Control mean	69122	69122	4743	14798	9322	20361	6202	13695
Adjusted $R^2$	.04	.039	.015	.059	.058	.015	.0089	.012
Observations	4874	4908	4907	4908	4908	4908	4908	4908

This table reports treatment effects on measures of household income. Panel (a) uses data from the Socio-Economic and Caste Census (SECC), which reports income categories of the highest earner in the household as indicated. Columns 1-6 report marginal effects using a logit model, and Columns 7-8 report effects on the probability of being in the lowest bracket from an ordered logit regression. Control variables, when included, are: age of the household head, an indicator for whether the head is illiterate, and an indicator for whether the household belongs to a Scheduled Caste/Tribe. Panel (b) uses data on (annualized) income, measured in Rupees, from our survey. “Total income” is total annualized household income. “NREGS” is earnings from working on the NREGS. “Agricultural labor” and “Other labor” capture income from doing agricultural and non-agricultural work for someone else. “Farm” combines income from a HH’s own land and animal husbandry, while “Business” captures income from self-employment or a HH’s own business. “Miscellaneous” is the sum of HH income not captured by the other categories. We truncate observations that are in the top 0.5% percentile of total income in treatment and control; treatment effects are higher in magnitude and remain statistically significant without truncation (available online, Table C.2). Baseline lag is the village-level mean of the dependent variable at baseline. We did not measure income sub-categories at baseline and so do not include their lags. All regressions (in both panels) include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .  $p$ -values from randomization inference on 10,000 iterations are reported in square brackets in panel (a).

Table 2: Employment and wage outcomes (June)

	Wage		Employment		
	(1) Wage realization	(2) Reservation wage	(3) Days idle or unpaid	(4) Days worked in NREGS	(5) Days worked in private sector
Adjusted TE	13*** (4.3) {3.9}	6.9** (3.2) {3}	-2.4*** (.79) {.78}	.92** (.42) {.37}	1.4* (.8) {.76}
Naive ITT	7.9** (3.6) {4.8}	5.5* (2.8) {3.9}	-1.2** (.58) {.55}	.54 (.37) {.47}	.54 (.56) {.61}
Spillover onto control	5.1* (2.9) {2.9}	1.4 (2.1) {1.9}	-1.1** (.51) {.53}	.37 (.24) {.22}	.85* (.49) {.5}
$\beta_T$	8.8** (3.6) {3.4}	5.8** (2.8) {2.6}	-1.5** (.59) {.58}	.61* (.37) {.34}	.74 (.57) {.54}
$\beta_N$	11* (6.5) {6.4}	3.1 (4.6) {4.2}	-2.5** (1.1) {1.2}	.83 (.54) {.49}	1.9* (1.1) {1.1}
Control mean	123	96	18	3.2	7.1
Adjusted $R^2$	.076	.054	.073	.04	.02
Observations	7016	12677	13951	17974	14278

This table reports treatment effects on employment and wage outcomes in the private labor market in June 2012 using survey data.  $\beta_T$  and  $\beta_N$  are the coefficients on a treatment indicator and the fraction of neighbors treated, respectively. The Adjusted TE is constructed as  $\beta_T + \beta_N \times \tilde{N}_{Tp}^R$ , where  $\tilde{N}_T^R$  is the average share of treated neighbors among treated GPs. The naive ITT estimate is the coefficient on treatment from a separate regression that excludes this spatial exposure term. The spillover onto control is constructed as  $\beta_N \times \tilde{N}_C^R$ , where  $\tilde{N}_C^R$  is the average share of treated neighbors among control GPs. We estimate effects at radius  $R$  at 20km and show in Table A.7 that results are not overly sensitive to this choice. “Wage realization” is the average daily wage in Rupees an individual received while working for someone else. “Reservation wage” is the daily wage at which an individual would have been willing to work for someone else, and is thus defined for all respondents as opposed to only those that worked. “Days unpaid/idle” is the sum of days an individual did unpaid work or stayed idle. “Days worked in NREGS” is the number of days an individual worked in NREGS. “Days worked private sector” is the number of days an individual worked for somebody else. We truncate wage observations that are in the top 0.5% percentile in treatment and control; results are not sensitive to this (Online Appendix, Table C.5). Baseline lag is the village mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . Standard errors calculated following (Conley, 2008) using a uniform kernel out to 10 km are in { brackets }. Observation counts vary across columns due to differences in response rates; Table A.13 shows that these were not differential by treatment status.

Table 3: Prices

	Log of Price Index		Log of Individual Prices
	(1) Uniform goods	(2) All goods	(3)
Treatment	.0041 (.066)	.0048 (.025)	-.011 (.011)
Item FE	No	No	Yes
Level	Village	Village	Item x Household
Control mean	11	11	
Adjusted $R^2$	.98	1	.95
Observations	60	60	18242

This table reports impacts on prices using data from the 68th Round of the National Sample Survey. Columns 1 - 2 report impacts on the log of village-level price indices constructed using Equation 4. In Column 1 we restrict attention to goods purchased at least once in every village in our sample, while in Column 2 we include all available data. Column 3 reports impacts on the log price of individual commodities using all available data. Standard errors clustered at the mandal level are in parentheses and statistical significance based on these is denoted as:  $*p < .10$ ,  $**p < .05$ ,  $***p < .01$ . All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization.



Table 4: Heterogeneous effects on employment outcomes by land concentration

(a) Absolute values of normalized Herfindahl index

	Full sample			Restricted to above 1 acre		
	(1) Days unpaid or idle	(2) Days worked in NREGS	(3) Days worked in private sector	(4) Days unpaid or idle	(5) Days worked in NREGS	(6) Days worked in private sector
Treatment	-1.1*	.48	.46	-1.1*	.51	.45
	(.62)	(.39)	(.57)	(.62)	(.39)	(.57)
$H^*$	2.3	1.1	-4.7**	2.7	1.9*	-6.2*
	(3.6)	(.84)	(2.1)	(5.1)	(1.1)	(3.2)
Treatment $\times H^*$	-3.8	.41	4.6**	-2.8	-1.2	6.5*
	(3.9)	(1.6)	(2.3)	(5.3)	(2.3)	(3.4)
Control Mean	17	3.6	7.9	17	3.6	7.9
Adjusted $R^2$	.067	.038	.019	.067	.038	.02
Observations	13482	17430	13827	13453	17378	13798

(b) Standardized values of normalized Herfindahl index

	Full sample			Restricted to above 1 acre		
	(1) Days unpaid or idle	(2) Days worked in NREGS	(3) Days worked in private sector	(4) Days unpaid or idle	(5) Days worked in NREGS	(6) Days worked in private sector
Treatment	-1.2**	.49	.6	-1.2**	.48	.6
	(.6)	(.37)	(.55)	(.6)	(.38)	(.55)
$H^*$	.28	.13	-.56**	.28	.2*	-.63*
	(.43)	(.1)	(.25)	(.52)	(.12)	(.33)
Treatment $\times H^*$	-.43	.031	.55**	-.29	-.13	.66*
	(.46)	(.18)	(.27)	(.54)	(.22)	(.34)
Control Mean	17	3.6	7.9	17	3.6	7.9
Adjusted $R^2$	.067	.038	.019	.067	.038	.02
Observations	13482	17430	13827	13453	17378	13798

This table reports treatment effects on employment outcomes in June 2012 differentiated by measures “ $H^*$ ” of land ownership concentration. In Panel (a) the measure is the normalized Herfindahl index constructed at the village level, while in Panel (b) it is the normalized Herfindahl index standardized separately for treatment and control areas. “Days unpaid/idle” is the sum of days an individual did unpaid work or stayed idle. “Days worked on NREGS” is the number of days an individual worked on NREGS. “Days worked private sector” is the number of days an individual worked for somebody else. All regressions include (the village mean of) the baseline lag, district fixed effects, and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .

Table 5: Savings, loans and assets  
(a) Savings, loans and land ownership

	Total savings (Rs.)		Total loans (Rs.)		Owns land (%)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1064 (859)	1120 (877)	11210** (4741)	11077** (4801)	.056** (.024)	.049** (.024)
Baseline lag	No	Yes	No	Yes	No	Yes
Control Mean	2966	2966	68108	68108	.59	.59
Adjusted $R^2$	.004	.004	.0095	.01	.012	.032
Observations	4916	4882	4943	4909	4921	4887

(b) Livestock assets

	Cattle	Buffaloes	Sheep	Goats	Pigs	Dogs	Backyard Poultry
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-1000 (986) [.152]	1815** (784) [.008]	-2468 (2859) [.194]	905 (1035) [.195]	58 (91) [.341]	161 (101) [.05]	2850 (2576) [.12]
Control mean	9689	8548	30527	9387	252	371	26167
Adjusted $R^2$	.41	.41	.47	.089	.019	.25	.12
Observations	157	157	157	157	157	157	157

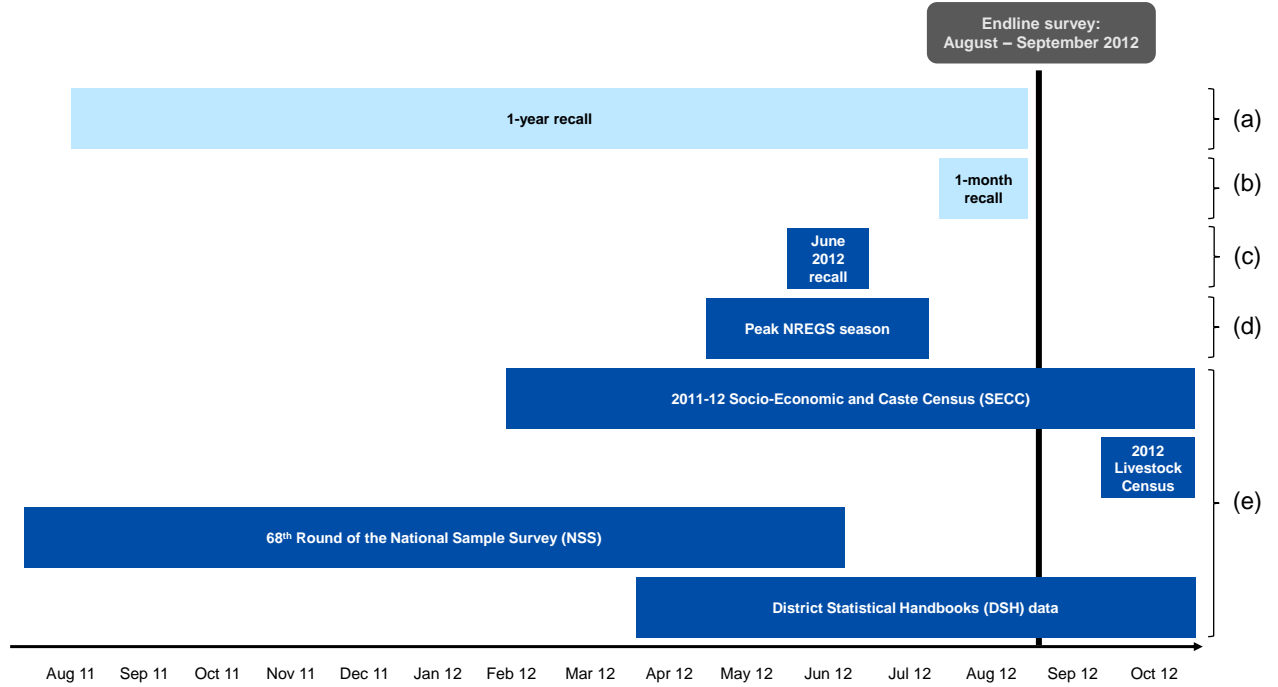
This table reports impacts on measures of assets and liabilities. Panel (a) uses data from our household survey. “Total savings (Rs.)” in columns 1-2 is defined as the total amount of a household’s current cash savings, including money kept in bank accounts or Self-Help Groups. “Total loans (Rs.)” in columns 3-4 is the total principal of the household’s five largest active loans. “Owns land (%)” in columns 5-6 is an indicator for whether a household reports owning any land. Baseline lag refers to the village mean of the dependent variable at baseline. Panel (b) uses mandal-level data from the 2012 Livestock Census, focusing on animals with headcounts of at least 100 on average in control mandals. A Wald test of joint significance rejects the null of no impacts ( $p = 0.04$ ). All regressions (in both panels) include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors in parentheses are clustered at the mandal level in Panel (a), and heteroskedasticity-robust in Panel (b); statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .  $p$ -values from randomization inference on 10,000 iterations are reported in square brackets in Panel (b).

Table 6: Number of enterprises and employees

	All sectors	Livestock	Manufacturing and construction	Wholesale and retail	Other
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Effect on number of enterprises</i>					
Treatment	883.6** (433.9) [.065]	75.8 (120.9) [.281]	214.7 (140.0) [.131]	312.6* (168.4) [.07]	280.5* (165.8) [.123]
Control mean	3816.5	1127.3	754.1	739.3	1195.7
Adjusted $R^2$	0.29	0.58	0.22	0.17	0.25
Observations	157	157	157	157	157
<i>Panel B: Effect on number of employees</i>					
Treatment	2375.7** (1131.7) [.084]	134.1 (206.3) [.295]	626.2* (321.0) [.116]	772.9* (405.2) [.087]	842.6* (449.4) [.144]
Control mean	6796.7	1711.5	1439.9	1219.2	2426.1
Adjusted $R^2$	0.17	0.52	0.17	0.12	0.12
Observations	157	157	157	157	157

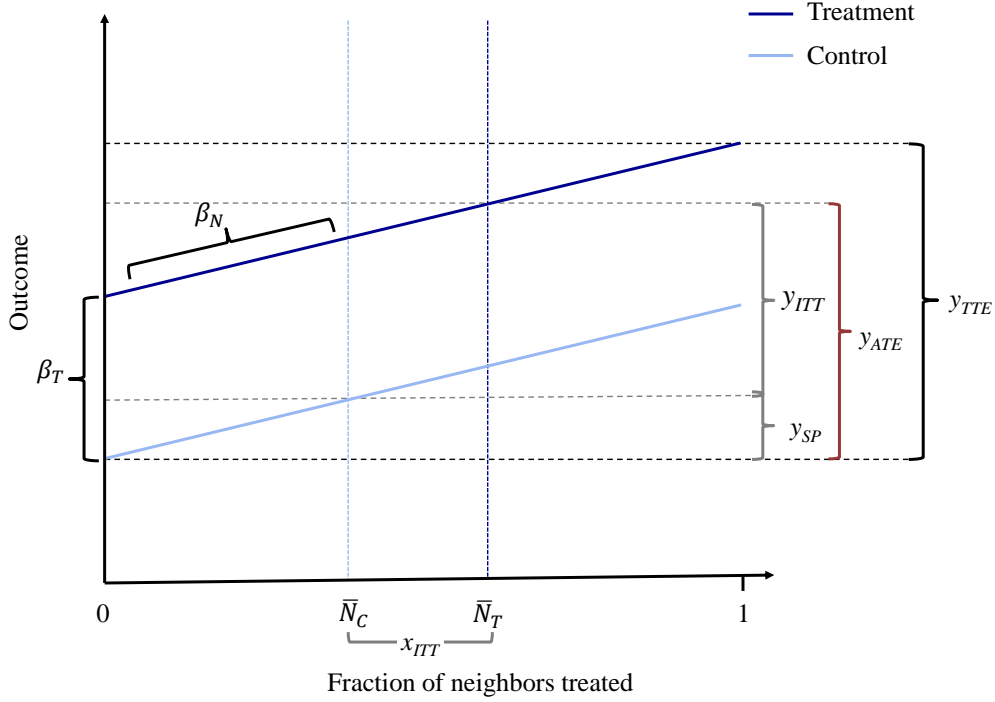
This table reports treatment effects on the number of firms (Panel A) and employees (Panel B), as reported in the Economic Census from 2012-13. Each column in each panel is a separate regression. The categories (livestock, manufacturing and construction, wholesale and retail, and other) are consolidated from industrial classifications in the census. Each regression includes aggregate baseline lags from the 2005 Economic Census (i.e. all regressions in Panel A have the total number of enterprises as controls, and analogously in Panel B), and the total working population in the mandal from the 2011 Census. Standard errors in parentheses are heteroskedasticity-robust, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .  $p$ -values from randomization inference on 10,000 iterations are reported in square brackets.

Figure 1: Timeline of endline survey reference periods



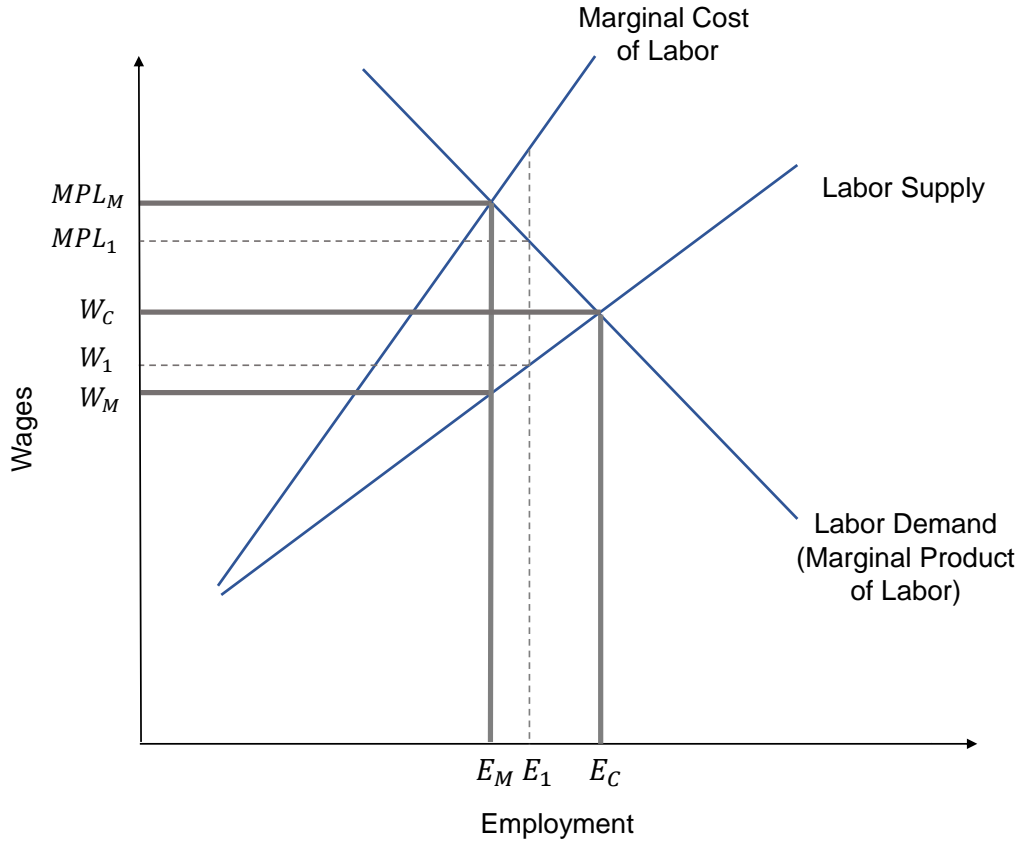
This figure illustrates the periods of time referenced by the various data sources we use. Rows labelled (a)-(c) show the recall periods used in our endline survey, which we conducted between August and September 2012. Those in dark blue are fixed with respect to the timing of the endline survey, while those in light blue are variable. These include (a) one-year recall for questions about household earnings/income and larger expenses, (b) one-month recall for questions on smaller expenses, (c) recall from June 2012 for key labor market outcomes (wages and employment), and (d) recall of the specific 7-week period from May 18 - July 14, 2012, which corresponds approximately to the typical season of peak activity on the NREGS, for questions about NREGS work and leakage and is the reference period used to calculate leakage in MNS. Rows labelled (e) show the coverage periods for independent data sources. Details on these are as follows: the 2011-2012 Socio-Economic and Caste Census was conducted in rural AP during 2012 and contains household-level data on earnings (in the month prior to the date of the interview) and land holdings (at time of the interview); the 2012 Livestock Census was conducted with October 15, 2012 as the reference date and contains data on mandal-level livestock headcounts as of that date; the 68th Round of the National Sample Survey was conducted in AP between July 2011 - June 2012 and contains data on household-level expenditure and number of units purchased for a variety of goods (in the month prior to the date of the interview); and the District Statistical Handbooks, which the Andhra Pradesh Directorate of Economics publishes annually, contain data on land utilization and irrigation during April 2012 - March 2013.

Figure 2: Conceptual framework for spillover adjustments on treatment effects



This figure illustrates how an outcome (e.g. wages) may depend on both the treatment status of one's own village as well as the fraction of treated neighbors; it corresponds to Equation (3) in the text.  $\beta_T$  represents the effect of a village being treated when none of its neighbors are treated. Moving along the x-axis corresponds to increasing the fraction of treated neighbors, and the corresponding changes in outcomes are represented by light and dark blue solid lines. Vertical dotted lines represent mean exposure of treatment (dark blue) and control (light blue) groups. The mean difference in the fraction of treated neighbors between treatment and control villages is positive but less than 1. The gray bracket range labelled  $y_{ITT}$  represents the unadjusted treatment effect. The gray bracket range labelled  $y_{SP}$  represents the spillover to control areas. The brown bracket range, labelled  $y_{ATE}$ , represents the spillover-adjusted treatment effect. The black bracket range, labelled  $y_{TTE}$ , represents the total treatment effect that a policy maker would care about.

Figure 3: Labor market equilibrium under monopsony and perfect competition



This figure illustrates the predicted effects of an increase in the value of workers' outside option (e.g. NREGS employment) on employment under competitive markets and under monopsony. In a competitive market, the equilibrium wage and employment would be  $W_C$  and  $E_C$  respectively, and a binding increase in the value of an outside option such as the NREGS would push wages above  $W_C$  and employment below  $E_C$ . However, an employer with market power faces a steeper marginal cost of labor than the labor supply curve, and will maximize profits by rationing employment levels below the competitive level, i.e.  $E_M < E_C$ , resulting in wages that are lower than the marginal product of labor, i.e.  $W_M < MPL_M$ . In such a setting, a binding increase in the reservation wages from  $W_M$  up to  $W_C$  (such as to  $W_1$ ) will be associated with an increase in wages but also with an *increase* in employment (such as from  $E_M$  to  $E_1$ ).

## Appendix

### A Background on NREGS and Smartcards

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

#### A.1 Further details on NREGS

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

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

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

#### A.2 Smartcard-introduced changes in payments

The Smartcards system was introduced in Andhra Pradesh in 2006, and while rollout in treatment areas in our study districts began in 2010. The payments system was based on electronic benefit transfers into special "no-frills" bank accounts tied to individual beneficiaries, and biometric authentication of beneficiaries before withdrawing these transfers. Figure 1 of MNS shows the status quo payment system and the changes introduced by Smartcards.

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

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

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

Compared to other documents that the household would have had (e.g. jobcard that was required in order to obtain Smartcard, voter ID card, etc), the Smartcard’s value as an identity document was limited. Unlike the national Unique ID (*Aadhaar*), Smartcards were not de-duplicated at the national level, and were therefore not legally admissible as ID for purposes other than collecting NREGS/SSP payments.<sup>48</sup> A truly “smart” card was not required or always issued: one Bank chose to issue paper cards with digital photographs and bar codes while storing biometric data in the PoS device (as opposed to on the card). Smartcards were also not portable; while Aadhaar cards are linked to a central server for authentication, Smartcards authentication was done offline. Thus while Aadhaar can be used across states and platforms (both public and private), Smartcards could only be used to make payments for NREGS and SSP beneficiaries within Andhra Pradesh.

### A.3 Impacts of Smartcards on Payments Process and Leakage

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

These improvements in the payment process were likely very important in making NREGS into

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<sup>48</sup>Meanwhile an Aadhaar card can be legally used to verify identity in airports, banks, etc.



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

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

## B Rural asset formation calculations

This Appendix examines whether increased asset formation under NREGS could plausibly generate the increases in wages and employment we observe in the data in the context of a simple model of rural production.

To this end, consider an economy that produces a single good using land  $T$ , augmentable capital  $K$ , and labor  $L$  according to  $Q = F(T, K, L) = T^{\alpha_T} K^{\alpha_K} L^{\alpha_L}$ . This good is sold at a fixed price in regional markets (i.e. we ignore the effects of localized income / demand shocks to focus on the productive effects of a capital shock). The supply of land is fixed. The supply of capital is exogenous, i.e. private capital supply is fixed and public capital is augmented by NREGS activity. Labor is supplied by atomistic workers as a function of the market wage  $w$  according to the labor supply curve  $L = L_s(w)$ . Labor demand is perfectly competitive and given by

$$F_L = \frac{w}{p} \quad (5)$$

Now suppose that increased NREGS activities raises the stock of capital  $K$  by an (unknown) proportion  $\% \Delta K$ ; we will solve for the proportion required to explain the moments we observe. We have

$$\% \Delta w = \alpha_K \cdot \% \Delta K + (\alpha_L - 1) \cdot \% \Delta L \quad (6)$$

Re-arranging this, we see that to explain the effects we see we would need an increase in the capital stock of

$$\% \Delta K = \frac{1}{\alpha_K} (\% \Delta w + (1 - \alpha_L) \% \Delta L) \quad (7)$$

Using factor share estimates of  $\alpha_L = 0.3$  and  $\alpha_K = 0.4$  from Chatterjee (2019) and our observed ITT values  $\% \Delta L = 6.7\%$  and  $\% \Delta w = 6.5\%$ , the implication is that we would need to have  $\% \Delta K = 28\%$ .<sup>49</sup>

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<sup>49</sup>Gollin (2002) has argued that factor share calculations often miss labor income of the self-employed and hence

Intuitively, it seems implausible that increased NREGS activity led to a 28% increase in the size of the *overall* capital stock. To examine this quantitatively, we assume that the rate of NREGS asset creation increased in proportion to the increase in payments received by workers (i.e. by 24%). We estimate NREGS capital stock as a proportion of total capital stock as follows:

- We obtain data on Gross Capital Formation in Agriculture and Allied sectors according to National Accounts data over the years 2002-03 through 2011-12, and apply a depreciation rate of 10% per year to the stock to obtain a total capital stock of Rs. 6.48 trillion (in 2004-05 prices).<sup>50</sup>
- Total NREGS expenditure in the years of the experiment (2010-11 and 2011-12) was Rs. 0.47 trillion (in 2004-05 prices).<sup>51</sup>
- Assuming generously that 60% of NREGS expenditure went directly to gross capital formation, this would result in 2010-12 NREGS capital formation being 4% of all capital stock.

This yields  $\% \Delta K = 24\% * 4\% \simeq 1\%$ , far short of the 28% needed to explain our results.

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underestimate labor shares. If correct, our calculations here if anything under-estimate the required increase in the capital stock.

<sup>50</sup>See [http://planningcommission.nic.in/data/datatable/data\\_2312/DatabookDec2014%2043.pdf](http://planningcommission.nic.in/data/datatable/data_2312/DatabookDec2014%2043.pdf).

<sup>51</sup>Data from Sukhtankar (2017). Note that we only count the experimental years, since anything before those years would be equal in treatment and control areas.

Table A.1: Baseline balance in administrative data

	Treatment	Control	Difference	p-value
	(1)	(2)	(3)	(4)
Numbers based on official records from GoAP in 2010				
% population working	.53	.52	.0062	.47
% male	.51	.51	.00023	.82
% literate	.45	.45	.0043	.65
% SC	.19	.19	.0025	.81
% ST	.1	.12	-.016	.42
Jobcards per capita	.54	.55	-.0098	.63
Pensions per capita	.12	.12	.0015	.69
% old age pensions	.48	.49	-.012	.11
% weaver pensions	.0088	.011	-.0018	.63
% disabled pensions	.1	.1	.0012	.72
% widow pensions	.21	.2	.013	.039
Numbers based on 2011 census rural totals				
Population	45580	45758	-221	.91
% population under age 6	.11	.11	-.00075	.65
% agricultural laborers	.23	.23	-.0049	.59
% female agricultural laborers	.12	.12	-.0032	.52
% marginal agricultural laborers	.071	.063	.0081	.14
Numbers based on 2011 census village directory				
# primary schools per village	3.2	3.6	-.4	.23
% village with medical facility	.52	.49	.028	.53
% villages with tap water	.87	.84	.033	.25
% villages with banking facility	.12	.15	-.036	.025
% villages with paved road access	.95	.94	.0086	.49
Avg. village size in acres	1374	1505	-131	.36

This table, reproduced from Muralidharan et al. (2016), compares official data on baseline characteristics across treatment and control mandals. Column 3 reports the estimate for the treatment indicator from a simple regression of the outcome with district fixed effects as the only controls; Column 4 reports the p-value for this estimate. A “jobcard” is a household level official enrollment document for the NREGS program. “SC” (“ST”) refers to Scheduled Castes (Tribes). “Old age”, “weaver”, “disabled” and “widow” are different eligibility groups within the SSP administration. “Working” is defined as the participation in any economically productive activity with or without compensation, wages or profit. “Main” workers are defined as those who engaged in any economically productive work for more than 183 days in a year. “Marginal” workers are those for whom the period they engaged in economically productive work does not exceed 182 days. The last set of variables is taken from 2011 census village directory which records information about various facilities within a census village (the census level of observation). “# primary schools per village” and “Avg. village size in acres” are simple mandal averages (others are simple percentages) of the respective variable. Sampling weights are not applicable as all villages within a mandal are used.

Table A.2: Baseline balance in survey data

	Treatment	Control	Difference	p-value
	(1)	(2)	(3)	(4)
Household members	4.8	4.8	.022	.89
BPL	.98	.98	.0042	.73
Scheduled caste	.22	.25	-.027	.35
Scheduled tribe	.12	.11	.0071	.81
Literacy	.42	.42	.0015	.93
Annual income	41,482	42,791	-1,290	.52
Total annual expenditure	687,128	657,228	26,116	.37
Short-term Expenditure	52,946	51,086	1,574	.45
Longer-term Expenditure	51,947	44,390	7,162	.45
Pay to work/enroll	.011	.0095	.00099	.82
Pay to collect	.058	.036	.023	.13
Ghost household	.012	.0096	.0019	.75
Time to collect	156	169	-7.5	.62
Owns land	.65	.6	.058	.06
Total savings	5,863	5,620	3.7	1.00
Accessible (in 48h) savings	800	898	-105	.68
Total loans	62,065	57,878	5,176	.32
Owns business	.21	.16	.048	.02
Number of vehicles	.11	.12	-.014	.49
Average payment delay	28	23	.036	.99
Payment delay deviation	11	8.8	-.52	.72
Official amount	172	162	15	.45
Survey amount	177	189	-10	.65
Leakage	-5.1	-27	25	.15
NREGS availability	.47	.56	-.1	.02
Household doing NREGS work	.43	.42	.0067	.85
NREGS days worked, June	8.3	8	.33	.65
Private sector days worked, June	4.8	5.3	-.49	.15
Days unpaid/idle, June	22	22	.29	.47
Average daily wage private sector, June	96	98	-3.7	.34
Daily reservation wage, June	70	76	-6.8	.03
NREGS hourly wage, June	13	14	-1.3	.13
NREGS overreporting	.15	.17	-.015	.55
Additional days household wanted NREGS work	15	16	-.8	.67

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

Table A.3: Household characteristics by NREGS jobcard ownership

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

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

Table A.4: Baseline balance with respect to exogeneous measure of treatment exposure ( $\tilde{N}_p^R$ )

(a) Wage outcomes										
	Wage realization (Rs.)					Reservation wage (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	10 (9.9)	5.7 (13)	-1.9 (16)	-11 (17)	-27 (19)	5.3 (6.4)	4.9 (7.1)	1.4 (7.9)	-4.2 (8.4)	-13 (9.5)
Treatment	.28 (4.1)	-.02 (4.9)	.24 (6.2)	2.6 (8.3)	9.8 (11)	.2 (3.4)	.65 (4.3)	2 (5.1)	5 (6)	12 (7.9)
Chi-sq test for equality	.88	.17	.016	.53	2.9	.5	.26	.0052	.79	4.2
p-value	.35	.68	.90	.47	.09	.48	.61	.94	.37	.04

(b) Employment outcomes										
	Days worked private sector					Days unpaid/idle				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	-.75 (.65)	-.88 (.84)	-1 (1.1)	-1 (1.3)	-.23 (1.4)	.98 (.73)	.73 (1)	.73 (1.3)	.83 (1.4)	-.11 (1.5)
Treatment	-.11 (.54)	-.34 (.71)	-.27 (.81)	-.029 (.99)	.085 (1.1)	.31 (.65)	.16 (.9)	-.53 (1.5)	-1 (2.1)	-1.4 (2.4)
Chi-sq test for equality	.57	.24	.3	.37	.032	.47	.18	.41	.57	.22
p-value	.45	.62	.58	.54	.86	.49	.67	.52	.45	.64

This table examines the baseline balance of outcomes with respect to the share of neighbors in other mandals that were treated, i.e.  $\tilde{N}_p^R$ . Note that wave 2 villages are included in the denominator for the purposes of this calculation. Each cell shows the coefficient from a separate regression; in rows labelled ‘Control’ (‘Treatment’) these are estimated within the control (treatment) group, respectively. Column group headings specify outcomes, and column headings specify the spatial radius  $R$ . “Wage realization (Rs.)” the average daily wage (in Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage (in Rs.) at which he or she would have been willing to work for someone else. “Days worked private sector” is the number of days an individual worked for somebody else. “Days unpaid/idle” is the sum of days an individual did unpaid work or stayed idle. The entire village sample used in randomization is included. All regressions include district fixed effects. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .

Table A.5: Heterogeneous effects on income

	Total income (Rs.)			
	(1) HH is ST or SC	(2) Most educated HH member has above median education	(3) HH fraction eligible for SSP	(4) Head of HH is widow
Treatment	7830* (4606)	5951* (3322)	9671** (3921)	11541** (4782)
Treatment $\times$ Covariate	2941 (6427)	4996 (6170)	-6226 (8083)	-13994 (8767)
Covariate	-9717* (5205)	28260*** (5022)	-18345*** (6564)	-36049*** (6552)
Treatment + Treatment $\times$ Covariate	10771 (5145)	10947 (5934)	3445 (8105)	-2453 (6042)
Standard error				
p-value	0.04	0.07	0.67	0.69
Control mean	69122	69122	69122	69122
Adjusted $R^2$	.043	.11	.058	.085
Observations	4853	4871	4813	4874

This table examines heterogeneous effects on income using our survey data. We test for heterogeneity along dimensions unlikely to have been affected by treatment: caste, education, and pension eligibility. Column headings define the “Covariate” variable referred to in the row headings, which differs in each column. The outcome in all columns is total annualized HH income. “HH is ST or SC” is an indicator for whether the household belongs to a Scheduled Tribe/Caste. “Most educated HH member has above median education” is an indicator for whether the most educated member of the household has an above median number of years of education. “HH fraction eligible for SSP” is the fraction of household members who identify as eligible for SSP, though they may not actually receive pension. “Head of HH is widow” is an indicator for whether the head of household is a widow. We truncate observations that are in the top .5% percentile of total income in treatment and control groups. All regressions include the village mean of the dependent variable at baseline, district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A.6: Testing for existence of spatial spillovers and equality of slopes

## (a) Wage (June)

	Wage realization (Rs.)					Reservation wage (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	5.7 (6.9)	18** (8.2)	19** (8.6)	19** (9)	25** (12)	5 (4.5)	7.2 (5)	7.6 (5.9)	5.8 (6.9)	.82 (6.7)
Treatment	14*** (4.4)	12** (6)	12 (8)	12 (9.6)	13 (12)	3.4 (3.2)	3.3 (4.3)	3.3 (5.6)	3.8 (6.9)	4.3 (8.4)
Pooled	10*** (3.7)	12** (5)	11* (6.5)	12 (7.5)	15 (9.3)	3.2 (2.9)	3.6 (3.6)	3.1 (4.6)	2.7 (5.5)	2 (6.3)
F-test for equality	1.3	.39	.36	.37	.66	.16	.83	.66	.11	.25
p-value	.25	.53	.55	.54	.42	.69	.36	.42	.74	.62
Observations	6494	6900	7016	7041	7055	11728	12475	12677	12715	12738
% of pooled sample	92	97	99	99	100	92	98	99	99	100

## (b) Employment (June)

	Days worked private sector					Days unpaid/idle				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	1.7 (1.1)	1.7 (1.2)	2.1 (1.5)	3.2* (1.8)	4.5** (2.2)	-1.2 (1.1)	-1.6 (1.4)	-1.4 (1.9)	-3.3 (2.1)	-5** (2.3)
Treatment	.73 (.79)	1.5 (1.1)	1.9 (1.3)	2.5 (1.6)	3.2* (1.9)	-1.5* (.81)	-2.7** (1.1)	-3.2** (1.3)	-3.5** (1.5)	-4.2** (1.8)
Pooled	1.1 (.69)	1.6* (.88)	1.9* (1.1)	2.5** (1.3)	3.3** (1.5)	-1.5** (.7)	-2.2** (.92)	-2.5** (1.1)	-3.3** (1.3)	-4.1*** (1.5)
F-test for equality	1.3	.031	.045	.18	.48	.1	.89	1.5	.011	.18
p-value	.25	.86	.83	.67	.49	.75	.34	.22	.92	.67
Observations	13178	14030	14278	14336	14356	12891	13716	13951	13990	14010
% of pooled sample	91	97	99	99	99	92	97	99	99	100

This table reports tests for the existence of spillovers effects on wage (Panel (a)) and employment (Panel (b)) outcomes using survey data and for various radii. Each cell reports the coefficient obtained by regressing the outcome in the column group header on the treated share of neighboring Gram Panchayats in other mandals ( $\bar{N}_p^R$ ) using the sample indicated in the row heading and the specification from Equation 2. Note that wave 2 villages are included in the denominator for the purposes of this exercise. At the bottom of each column we report the  $F$ -statistic and the  $p$ -value from an adjusted Wald test of equality between the coefficients in the control and treatment samples. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . All regressions include village means of the outcomes at baseline, district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. “% of pooled sample” refers to the % of total observations for an outcome that are used in estimation; this is less than 100% because neighborhood variables are not defined for observations that have no neighbors within the given radius.



Table A.7: Treatment effect on key outcomes over a range of spillover radii

## (a) Treatment effect on wage outcomes

	Wage realization (Rs.)					Reservation wage (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Adjusted TE	13*** (3.8)	13*** (3.9)	13*** (4.3)	13*** (4.7)	14*** (5.1)	6.5** (2.9)	7.3** (2.9)	6.9** (3.2)	6.7* (3.4)	6.3* (3.7)
Naive ITT	8.3** (3.4)	7.8** (3.6)	7.9** (3.6)	7.8** (3.6)	7.7** (3.6)	5* (2.9)	5.5* (2.8)	5.5* (2.8)	5.5* (2.8)	5.4* (2.8)
Spillover onto control	4.9*** (1.7)	5.5** (2.3)	5.1* (2.9)	5.1 (3.3)	6.2 (3.9)	1.5 (1.4)	1.7 (1.7)	1.4 (2.1)	1.2 (2.4)	.84 (2.6)
$\beta_T$	9.3*** (3.4)	9** (3.5)	8.8** (3.6)	8.5** (3.6)	8.4** (3.6)	5.3* (2.8)	5.9** (2.7)	5.8** (2.8)	5.7** (2.8)	5.6** (2.8)
$\beta_N$	10*** (3.7)	12** (5)	11* (6.5)	12 (7.5)	15 (9.3)	3.2 (2.9)	3.6 (3.6)	3.1 (4.6)	2.7 (5.5)	2 (6.3)
Control mean	123	122	123	123	122	96	96	96	96	96
Adjusted $R^2$	.079	.077	.076	.075	.075	.05	.055	.054	.054	.054
Observations	6494	6900	7016	7041	7055	11728	12475	12677	12715	12738

## (b) Treatment effect on employment outcomes

	Days worked in private sector					Days worked in NREGS					Days idle/unpaid				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30	(11) R = 10	(12) R = 15	(13) R = 20	(14) R = 25	(15) R = 30
Adjusted TE	1.1 (.69)	1.3* (.74)	1.4* (.8)	1.7* (.85)	2** (.91)	.76* (.4)	.94** (.42)	.92** (.42)	1** (.43)	1** (.45)	-2*** (.68)	-2.3*** (.71)	-2.4*** (.79)	-2.7*** (.84)	-3*** (.9)
Naive ITT	.56 (.59)	.51 (.56)	.54 (.56)	.54 (.56)	.55 (.56)	.56 (.36)	.58 (.37)	.54 (.37)	.54 (.37)	.53 (.37)	-1.2** (.61)	-1.2** (.58)	-1.2** (.58)	-1.2** (.58)	-1.2** (.58)
Spillover onto control	.52 (.33)	.73* (.41)	.85* (.49)	1.1** (.56)	1.4** (.64)	.2 (.17)	.35 (.23)	.37 (.24)	.45* (.27)	.49 (.31)	-.69** (.33)	-1** (.43)	-1.1** (.51)	-1.4** (.57)	-1.7*** (.65)
$\beta_T$	.68 (.59)	.7 (.57)	.74 (.57)	.74 (.57)	.73 (.56)	.6* (.36)	.66* (.37)	.61* (.37)	.61* (.36)	.59 (.36)	-1.4** (.6)	-1.5** (.58)	-1.5** (.59)	-1.5** (.58)	-1.4** (.57)
$\beta_N$	1.1 (.69)	1.6* (.88)	1.9* (1.1)	2.5** (1.3)	3.3** (1.5)	.43 (.36)	.76 (.49)	.83 (.54)	1* (.62)	1.2 (.75)	-1.5** (.7)	-2.2** (.92)	-2.5** (1.1)	-3.3** (1.3)	-4.1*** (1.5)
Control mean	7.4	7.2	7.1	6.8	6.5	3.4	3.2	3.2	3.1	3.1	18	18	18	19	19
Adjusted $R^2$	.019	.019	.02	.02	.021	.043	.041	.04	.04	.04	.079	.074	.073	.074	.074
Observations	13178	14030	14278	14336	14356	16672	17683	17974	18043	18075	12891	13716	13951	13990	14010

This table reports estimated effects on wage (Panel (a)) and employment (Panel (b)) outcomes for various radii, using the same specification and outcome variables as in Table 2 and the radius specified in the column header. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .

Table A.8: Spillover adjustments in treatment effects on income (SECC)

	Lowest bracket (< Rs. 5,000)		Middle bracket (Rs. 5,000 - 10,000)		Highest bracket (> Rs. 10,000)	
	(1)	(2)	(3)	(4)	(5)	(6)
Adjusted TE	-.031*	-.029*	.025*	.023	.0061	.005
	(.017)	(.017)	(.014)	(.014)	(.0062)	(.0057)
Naive ITT	-.03**	-.03**	.022**	.022*	.0083	.0081
	(.014)	(.014)	(.011)	(.011)	(.0055)	(.0054)
Spillover onto control	.003	.0044	.00066	-.00025	-.0037	-.0041
	(.012)	(.011)	(.0085)	(.0085)	(.0052)	(.005)
$\beta_T$	-.22**	-.21**	.2**	.19*	.21	.2
	(.096)	(.098)	(.099)	(.1)	(.13)	(.13)
$\beta_N$	.019	.03	.031	.023	-.15	-.16
	(.16)	(.16)	(.15)	(.15)	(.25)	(.24)
Control variables	No	Yes	No	Yes	No	Yes
Adjusted $R^2$	.014	.031	.015	.024	.013	.04
Control Mean	.83	.83	.13	.13	.038	.038
Observations	1.7 M	1.7 M	1.7 M	1.7 M	1.7 M	1.7 M

This table reports estimated effects on income measures from the SECC data as in Columns 1-6 of Panel (a) of Table 1, but adjusted for spillovers as in Table 2. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . Unlike in Table 2 we do not report standard errors calculated using the method of Conley (2008), as doing so in a sample of 1.7 million observations is excessively demanding on computation even using a supercomputer. Given clustered standard errors and Conley standard errors are similar in all other specifications we do not expect they would differ here.

Table A.9: Spillover adjustments in treatment effects on income (survey)

	Total income		NREGA	Agricultural labor	Other labor	Farm	Business	Miscellaneous
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Adjusted TE	9579** (4539) {4156}	10634** (4494) {4182}	1295 (1061) {863}	3537** (1719) {1541}	4070** (1686) {1545}	-1012 (2655) {2484}	243 (1622) {1534}	2502 (2474) {2309}
Naive ITT	8898** (3725) {4047}	9621** (3737) {4493}	941 (585) {717}	3360** (1471) {1097}	3266** (1314) {1233}	2219 (2313) {1892}	-677 (1328) {1069}	512 (2108) {2057}
Spillover onto control	663 (3199) {3060}	1000 (3184) {3054}	349 (969) {812}	175 (1004) {905}	793 (1052) {1003}	-3189 (2034) {2102}	909 (854) {798}	1964 (2047) {1958}
$\beta_T$	9030** (3670) {3271}	9804*** (3676) {3301}	1005* (584) {530}	3392** (1468) {1267}	3412** (1320) {1175}	1634 (2250) {1985}	-511 (1349) {1312}	872 (2018) {1993}
$\beta_N$	1476 (7123) {6813}	2228 (7090) {6800}	776 (2158) {1809}	389 (2236) {2014}	1767 (2342) {2233}	-7102 (4530) {4682}	2024 (1902) {1777}	4373 (4559) {4360}
Baseline lag	Yes	No	No	No	No	No	No	No
Control mean	68459.3	68121.7	4394.8	14622.9	8529.1	23550.5	5293.2	11731.5
Adjusted $R^2$	.0386	.0371	.0151	.0583	.0588	.0158	.00925	.0129
Observations	4823	4857	4856	4857	4857	4857	4857	4857

This table reports estimated effects on income measures from our survey data as in Panel (b) of Table 1, but adjusted for spillovers as in Table 2. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . Standard errors calculated using the method of (Conley, 2008) and using a uniform kernel out to 10 km are in { brackets }.

Table A.10: Spillover adjustments in treatment effects on prices

	Log of Price Index		Log of Individual Prices
	(1) Uniform goods	(2) All goods	(3)
Adjusted TE	-.055 (.13) {.11}	.0059 (.045) {.04}	-.0003 (.016) {.014}
Naive ITT	.0078 (.067) {.061}	.0078 (.067) {.061}	-.01 (.011) {.01}
Spillover onto control	-.067 (.081) {.071}	-.0019 (.027) {.023}	.0095 (.01) {.0088}
$\beta_T$	-.0072 (.079) {.072}	.0072 (.029) {.026}	-.0071 (.011) {.011}
$\beta_N$	-.14 (.17) {.15}	-.004 (.055) {.049}	.02 (.021) {.018}
Control mean	11.19	10.69	-3.098
Adjusted $R^2$	.982	.998	.951
Observations	58	58	17651
Item FE	No	No	Yes
Level	Village	Village	Item x Household

This table reports estimated impacts on prices using National Sample Survey data as in Table 3, but adjusted for spillovers as in Table 2. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . Standard errors calculated using the method of (Conley, 2008) and using a uniform kernel out to 10 km are in { brackets }.

Table A.11: Spillover adjustments in treatment effects on migration

	Did migrate?	Days migrated	Household size	Migration common in May?
	(1)	(2)	(3)	(4)
Adjusted TE	.038* (.022) {.021}	2.8 (6.9) {7}	.06 (.13) {.13}	.034 (.048) {.043}
Naive ITT	.023 (.018) {.014}	.93 (5.1) {4.5}	.051 (.1) {.17}	.051 (.037) {.034}
Spillover onto control	.015 (.012) {.012}	1.8 (2.9) {3}	.008 (.096) {.094}	-.017 (.028) {.025}
$\beta_T$	.026 (.018) {.016}	1.3 (5.4) {5.3}	.053 (.1) {.097}	.048 (.038) {.035}
$\beta_N$	.034 (.028) {.027}	4.1 (6.5) {6.6}	.018 (.21) {.21}	-.037 (.063) {.056}
Control mean	.06	14	4.2	.22
Adjusted $R^2$	.029	.016	.019	.45
Observations	4822	4858	4858	799
Level	Hhd	Hhd	Hhd	GP

This table reports estimated impacts on measures of migration, adjusted for spillovers as in Table 2. Columns 1-3 use data from our household survey, and Column 4 uses data from a separate survey of village elders. “Did migrate?” is an indicator for whether any household member stayed away from home for the purpose of work during the last year. “Days migrated” is the sum of all days any household member stayed away from home for work. “Household size” is the number of household members. “Migration common in May?” is an indicator for whether it was common for workers to migrate out of the village in search of work during the month of May since the implementation of NREGS. Baseline lag is the village mean of the dependent variable at baseline, except in the case of column (4) where it is a contemporaneously collected indicator for whether the same type of migration during the same time was common prior to the start of NREGS. All regressions include baseline lags, district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . Standard errors calculated using the method of (Conley, 2008) and using a uniform kernel out to 10 km are in { brackets }.

Table A.12: Spillover adjustments in treatment effects on savings, loans and assets

	<u>Total savings (Rs.)</u>	<u>Total loans (Rs.)</u>	<u>Owns land (%)</u>
	(1)	(2)	(3)
Adjusted TE	1664* (918) {940}	18321*** (6712) {6561}	.072** (.033) {.038}
Naive ITT	1139 (880) {880}	11145** (4836) {4836}	.053** (.024) {.024}
Spillover onto control	507 (436) {436}	6915 (4710) {4956}	.019 (.021) {.024}
$\beta_T$	1243 (866) {871}	12584** (4873) {4819}	.056** (.025) {.029}
$\beta_N$	1129 (971) {972}	15399 (10489) {11036}	.042 (.047) {.053}
Control mean	2459.0	61192.6	.572
Adjusted $R^2$	.00397	.0108	.0311
Observations	4832	4858	4836

This table reports estimated treatment effects on household assets and liabilities as in Panel (a) of Table 5a, but adjusted for spillovers as in Table 2. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ . Standard errors calculated using the method of (Conley, 2008) and using a uniform kernel out to 10 km are in { brackets }

Table A.13: Response rates by treatment status

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

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

This table compares (non-)response rates to questions regarding labor market outcomes across treatment arms and for both the full sample (Panel (a)) and for working age adults (Panel (b)). Columns 1-2 reports 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 which also includes district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization as control variables. Column 4 reports the  $p$ -value of a test that the parameter estimated in Column 3 is zero. Column 5 reports the number of individuals from whom answers were sought, which was the full sample except as noted below. “Wage realization (Rs.)” is the average daily wage (Rs.) an individual received while working for someone else in June 2012. “Reservation wage (Rs.)” is an individual’s reservation wage at which he or she would have been willing to work for someone else in June 2012. “Days worked private sector” is the number of days an individual worked for somebody else in June 2012. “Days idle” and “Days unpaid” is the number of days an individual stayed idle or did unpaid work in June 2012. “Days unpaid/idle” is the sum of the latter two variables. Responses were sought from less than the full sample in the following cases: for “Wage realization (Rs.)” we asked the set of individuals who reported a strictly positive number of days worked for someone else; for “Wage realization  $\geq$  Reservation wage” is the set of individuals that had non-missing values for both average daily wages and reservation wage. Standard errors clustered at the mandal level are in parentheses.

Table A.14: Effects on household expenditure (survey and NSS data)

	Short-term Expenditure (Rs. per month)		Longer-term expenditure (Rs. per year)		Monthly Per Capita Expenditure
	(1)	(2)	(3)	(4)	(5)
Treatment	-108 (1029)	-428 (1033)	-24 (3239)	-646 (3227)	71 (122)
Baseline lag	No	Yes	No	Yes	No
Recall period	1 month	1 month	1 year	1 year	1 month
Survey	NREGA	NREGA	NREGA	NREGA	NSS
Control mean	18915	18915	38878	38878	1894
Adjusted $R^2$	0.01	0.02	0.01	0.01	0.03
Observations	4943	4909	4943	4909	478

This table reports estimated treatment effects on household expenditure using survey data (Columns 1-4) and data from the 68th round of the NSS (Column 5). “Short-term expenditure” in Columns 1- 2 is the sum of spending on items such as on food items, fuel, entertainment, personal care items or rent in the past month. “Longer-term expenditure” in Columns 3-4 is the sum of spending on medical and social (e.g. weddings, funerals) expenses, tuition fees, and durable goods in the past year. “Monthly Per Capita Expenditure” in column 5 is a measure of all household expenditure as well as the imputed value of household production. Baseline lag is the village mean of the dependent variable at baseline. All regressions include district fixed effects and those from our survey data include the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as:  $*p < .10$ ,  $**p < .05$ ,  $***p < .01$ .



Table A.15: Effects on NREGS project counts &amp; types

	Number of distinct projects						Number days spent working on					
	(1) Total	(2) Construction	(3) Irrigation	(4) Land development	(5) Roads	(6) Other	(7) Total	(8) Construction	(9) Irrigation	(10) Land development	(11) Roads	(12) Other
Treatment	-1.2 (2.9)	.11 (.44)	.1 (.31)	-1 (2.7)	.2* (.12)	-.95 (1.4)	61 (441)	-10 (103)	25 (247)	-119 (435)	161 (112)	9.1 (198)
Baseline lag	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	32	2.8	1.8	16	.51	11	6539	492	1770	2606	329	1409
Adjusted $R^2$	.24	.17	.11	.2	.13	.11	.35	.3	.47	.24	.11	.073
Observations	2837	2837	2837	2837	2837	2837	2899	2837	2837	2837	2837	2837

This table reports estimated treatment effects on NREGS activity by project type using NREGS muster roll data. The outcomes in Columns 1-5 are counts by Gram panchayat of unique project identification numbers for which some work was reported during the endline study period (May 28 to July 15, 2012) in total (Column 1) and by type (Columns 2-6). In Columns 7-12 the outcome variable is the number of days worked by Gram panchayat during the endline study period and on projects of the given type. The baseline lag variables are constructed analogously but for the baseline study period (May 31 to July 4, 2010). All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level are in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .

Table A.16: Effects on land utilization and irrigation

	Irrigated land	Total land	Total fallows	Non-agricultural use	Net area sown	Net area irrigated
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-.035 (.037)	-.13 (.14)	-.74 (1.2)	-.83 (1.3)	1.1 (1.6)	.0018 (.01)
Baseline lag	No	No	Yes	Yes	Yes	Yes
Control mean	7.2	11	11	9.1	28	.18
Adjusted $R^2$	0.02	0.04	0.62	0.62	0.88	0.83
Observations	1,820,629	1,820,303	154	154	154	154
Level	Household	Household	Mandal	Mandal	Mandal	Mandal
Data source	SECC	SECC	DSH	DSH	DSH	DSH

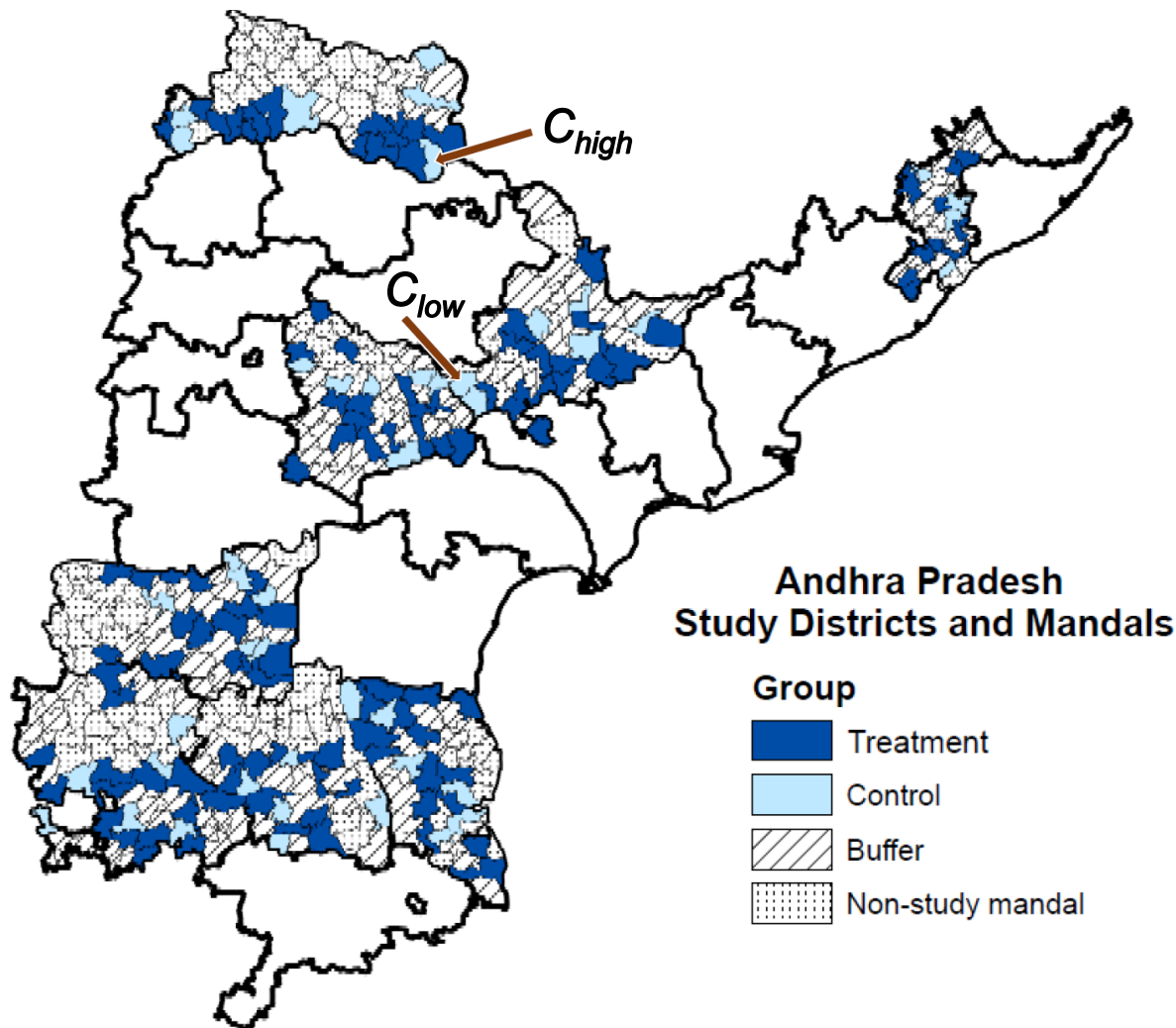
This table reports estimated treatment effects on measures of land utilization using data from the Socioeconomic and Caste Census (Columns 1-2) and from the annual District Statistical Handbooks (DSH) 2012-2013 (Columns 3-6). The DSH contains incomplete data for three mandals, leaving us with 154 out of our 157 study mandals. In Columns 1-2 the units are acres of land; “Irrigated land” is the amount in acres of land owned with assured irrigation for two crops, and “Total land” is the total amount of land owned, including both irrigated and unirrigated land. In Columns 3-6 the units are percentage of total mandal area. “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, and is equal to the sum of “current fallows” (cropped area which is kept fallow in the current year), “other fallows” (land which has been left fallow for more than 1 year but less than 5 years) and “culturable waste” (land available which has been left fallow for the more than 5 years but would be available for cultivation). “Non-agricultural use” is land occupied by buildings, roads, railways or under water. “Net area sown” is total area sown with crops and orchards. “Net area irrigated” is the total area irrigated through any source. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Columns 1-2 also include household-level control variables (age of the household head, an indicator for whether the head is illiterate, and an indicator for whether the household belongs to a Scheduled Caste or Tribe), and Columns 3-6 also include the lag of the dependant variables from the 2009-2010 DSH. Standard errors in parentheses are heteroskedasticity-robust and in Columns 1-2 are clustered at the mandal level. Statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .

Table A.17: Effects on loans, by type of lender

	Total		Formal		Semi-Formal		Informal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	11210* (4741)	11077* (4801)	2606 (2398)	2982 (2358)	-457 (1001)	-534 (1027)	8562* (4144)	7710 (4157)
Constant	49155*** (7419)	47618*** (7647)	5734* (2335)	5187* (2352)	3053** (1035)	3037** (1062)	39564*** (6416)	37556*** (6611)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline lag	No	Yes	No	Yes	No	Yes	No	Yes
Control mean	68108	68108	15358	15358	4970	4970	46832	46832
Adjusted $R^2$	0.01	0.01	0.01	0.02	0.01	0.01	0.01	0.01
Observations	4943	4909	4943	4909	4942	4908	4942	4908

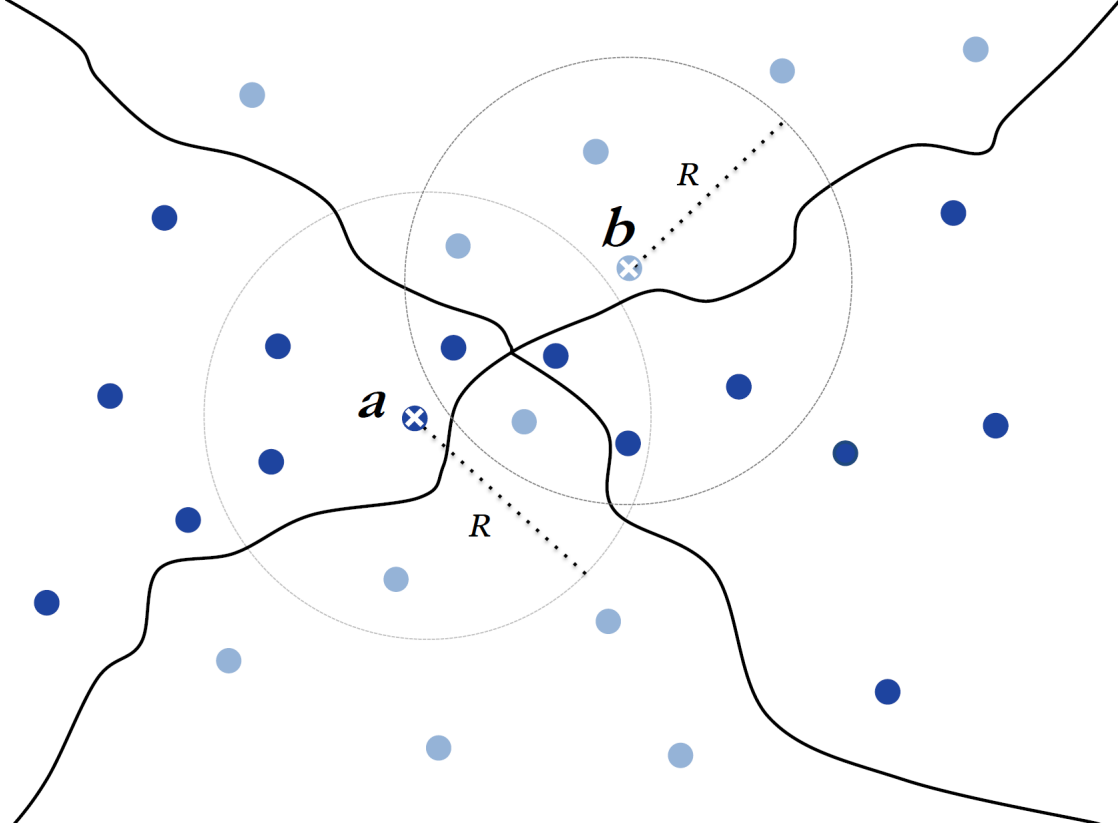
This table reports impacts on borrowing as in Columns 3 and 4 of Panel (a) of Table 5a, but broken down by lender. “Formal” loans are those issued by a commercial bank or a finance company. “Semi-Formal” loans are those issued by a micro-finance institution, a self-help group, a cooperative or a “chit fund,” which is a communal savings scheme regulated by the Chit Fund Act of 1982 in which members make periodic contributions to be paid out on a rotating basis at specified points in time. Finally, “informal” loans are those issued by money lenders, clients, shopkeepers, friends, neighbors or family members. The baseline lag is the village mean of the dependent variable at baseline. All regressions include district fixed effects and the first principal component of a vector of mandal characteristics used to stratify randomization. Standard errors clustered at the mandal level in parentheses, and statistical significance based on these is denoted as: \* $p < .10$ , \*\* $p < .05$ , \*\*\* $p < .01$ .

Figure A.1: Study districts with treatment and control mandals



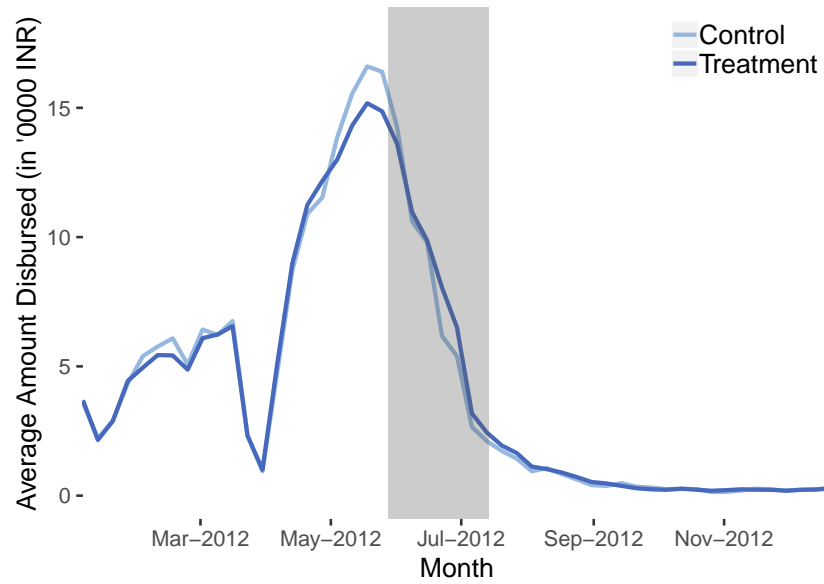
This map shows the 8 study districts - Adilabad, Anantapur, Kadapa, Khammam, Kurnool, Nalgonda, Nellore, and Vizianagaram - and the assignment of mandals (sub-districts) within those districts to study arms. Mandals were randomly assigned to one of three waves: 112 to wave 1 (treatment), 139 to wave 2, and 45 to wave 3 (control). Wave 2 was created as a buffer to maximize the time between program rollout in treatment and control waves; we did not collect survey data in these mandals. We did not assign “non-study” mandals to an arm because the Smartcards initiative had already started in those mandals or in some cases (109 out of 405) because they were entirely urban and thus had no NREGS activity. Randomization was stratified by district and by a principal component of mandal characteristics including population, literacy, proportion of Scheduled Caste and Tribe, NREGS jobcards, NREGS peak employment rate, proportion of SSP disability recipients, and proportion of other SSP pension recipients. The two mandals marked are examples of those which by had chance had a high (low) proportion of their neighbors treated.

Figure A.2: Constructing measures of exposure to spatial spillovers



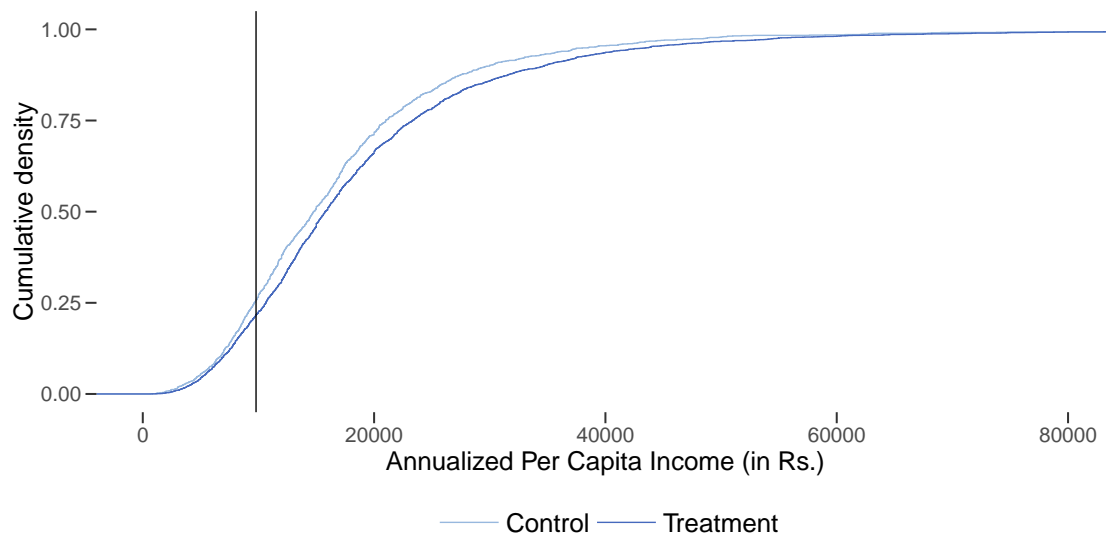
This figure illustrates the construction of measures of spatial exposure to treatment within radius  $R$ . Dark (light) blue dots represent treatment (control) villages; black lines represent mandal borders. An illustrative pair of villages in treatment ( $a$ ) and control ( $b$ ) mandals are denoted with the white X symbol.  $\tilde{N}_p^R$  is the fraction of villages within a radius  $R$  of a given village  $p$  and *within a different mandal (excluding villages in the same mandal from both the numerator and denominator)* which were assigned to treatment. In the figure above, these measures are  $\tilde{N}_a^R = \frac{2}{5}$  and  $\tilde{N}_b^R = \frac{4}{5}$ .

Figure A.3: Official disbursement trends in NREGS



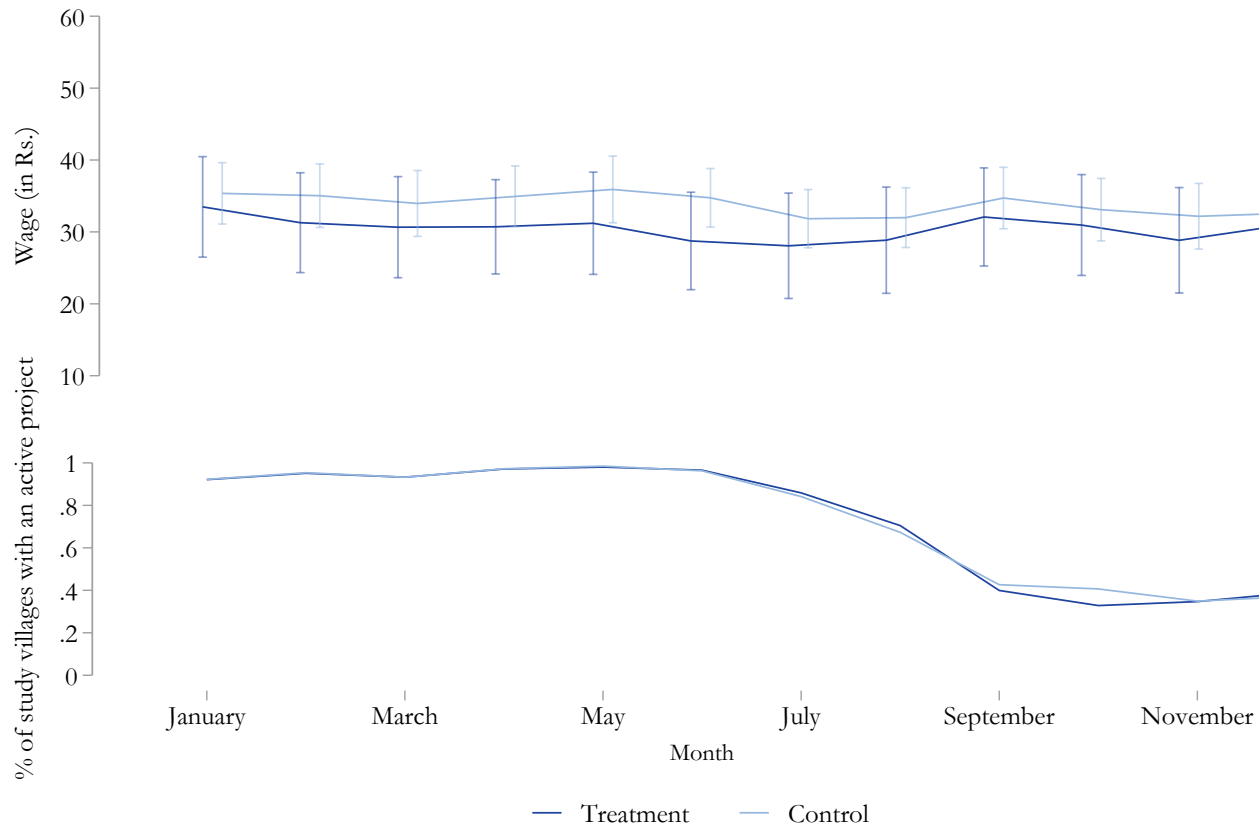
This figure (reproduced from Muralidharan et al. (2016)) shows official NREGS payments for all workers averaged at the village-week level for treatment and control areas. The grey band denotes the endline study period, May 28 to July 15, 2012, on which our survey questions focus.

Figure A.4: Annualized per capita income



This figure plots the empirical cumulative distribution function of total annualized per capita income by household from the endline survey separately for households in the treatment (dark blue) and control (light blue) groups. The vertical line indicates the annualized official per capita poverty line (Rs. 860 per person per month or Rs. 10,320 per person per year).

Figure A.5: Changes in wages and availability of NREGS work by month and treatment status



The figure plots the average change in agricultural wages between baseline and endline (top series) and the proportion of study villages with at least one active NREGS project (bottom series), both by month during 2012 and by treatment status. We use measures of (changes in) agricultural wages from surveys of prominent figures in each village and weight these by (inverse) village sampling probabilities. Confidence intervals are based on standard errors clustered at the mandal level. We measure NREGS project activity using muster roll data from 2012 and define a village as having an active project if any work was reported in that village during that month.