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

Karthik Muralidharan† UC San Diego

Paul Niehaus‡ UC San Diego

Sandip Sukhtankar§ University of Virginia

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**Abstract**

Public employment programs may affect poverty both directly through the income they provide and indirectly through general-equilibrium effects. We estimate both effects, exploiting a reform that improved the implementation of India's National Rural Employment Guarantee Scheme (NREGS) and whose rollout was randomized at a large (sub-district) scale. The reform raised beneficiary households' earnings by 14%, and reduced poverty by 26%. Importantly, 86% of income gains came from non-program earnings, driven by higher private-sector (real) wages and employment. This pattern appears to reflect imperfectly competitive labor markets more than productivity gains: worker's reservation wages increased, land returns fell, and employment gains were higher in villages with more concentrated landholdings. Non-agricultural enterprise counts and employment grew rapidly despite higher wages, consistent with a role for local demand in structural transformation. These 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

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

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†UC San Diego, JPAL, NBER, and BREAD. kamurali@ucsd.edu.

‡UC San Diego, JPAL, NBER, and BREAD. pniehaus@ucsd.edu.

§University of Virginia, JPAL, and BREAD. sandip.sukhtankar@virginia.edu.

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. Economic rationales for such programs include self-targeting through work requirements, public asset creation, and enforcing wage floors in informal labor markets by making the government an employer of last resort.[1] The world's largest such program is the National Rural Employment Guarantee Scheme (NREGS) in India, with over 600 million rural residents eligible to participate and a fiscal allocation of 0.5% of India's GDP.

Whether and to what extent such a program raises incomes and reduces poverty is a first-order policy question. It is also a subtle one, as the gains participants obtain directly from program earnings themselves (which are relatively easy to observe) may be attenuated or amplified by partial- and general-equilibrium (GE) effects. For instance, gross program earnings may overstate net income gains for the poor to the extent that they substitute out of private employment (Bertrand et al. 2021), or understate them to the extent that market wages increase, with the magnitude of these effects depending in turn on labor market structure. Answering this question requires credible identification of impacts at scales large enough to move markets, accounting for the spatial spillovers this may involve, and measuring income from various sources comprehensively. For the NREGS (and for public employment schemes in general) this has proven challenging.[2]

In this paper we address these challenges by combining experimental variation in NREGS implementation quality, units of randomization large enough to capture labor-market GE effects, units of observation geocoded finely enough to adjust estimates for spatial spillovers, and a wide range of both survey and census data. Specifically, we worked with the Government of the Indian state of (erstwhile) Andhra Pradesh (AP) to randomize the order in which 157 sub-districts (mandals) with an average population of 62,500 each introduced a new system (biometric "Smartcards") for making payments in NREGS during 2010-2012. In prior work, we show that Smartcards substantially improved NREGS performance on several dimensions: it reduced leakage of funds, increased program earnings, reduced pay-

1. Workfare programs may also be politically more palatable to taxpayers than unconditional "doles." Such programs have a long history, with recorded instances from as early as the 18th century in India, 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).

2. With respect to the NREGS there have been four main issues. First, experimental variation has not been available, with the consequence that studies often reach opposing conclusions depending on the data and identification strategy used (see Sukhtankar 2017). Second, many data sources do not permit geolocation of affected households at levels finer than the identifying variation (typically the district), which is needed to identify spatial spillovers. Third, a key data source (the National Sample Survey) does not collect data on income, and did not collect representative district-level data on consumption (used for poverty estimates) during NREGS rollout. Finally, NREGS implementation quality varied considerably across time and space, making it difficult even to define in precise terms the intervention whose effects are measured.

ment delays and the time required to collect payments, and increased real and perceived access to work, without changing fiscal outlays on the program (Muralidharan, Niehaus, and Sukhtankar (2016), henceforth MNS).

In short, Smartcards brought NREGS implementation closer in specific, measured ways to what its architects intended. This paper studies the downstream effects of a better-implemented NREGS on income, poverty, and labor markets. These effects are both intrinsically important, and also informative about—though not necessarily the same as—the effects of rolling out NREGS itself (a comparison we explore further in the conclusion).[3]

We study these effects using data from an original survey of $\sim$5,000 households, sampled to be representative of the 49.5% of rural households registered for NREGS. These surveys were conducted two years after the randomized rollout of the Smartcard program, before the control group was treated. We have detailed data on income for the full year preceding the survey, and on wages and employment for the month of June. To study impacts on the full population, we supplement these data with three distinct *censuses*—of households, non-agricultural employment and enterprises, and livestock—conducted by the government independently of our efforts and at around the same time. We also use National Sample Survey (NSS) data for consumer price information, and administrative data on land use and irrigation. Each data source has limitations, but collectively they allow us to paint a reasonably comprehensive picture of the reform's economic consequences.[4]

We find, first and foremost, that improving NREGS implementation substantially increased real incomes of the rural poor. Mean earnings among NREGS-registered households increased by 13.9%, leading to a 25.8% (7.4 percentage points) reduction in an income-based measure of poverty, while consumer goods prices did not change significantly. Strikingly these income gains came primarily from sources *other than* the NREGS. Program earnings accounted for only 14% of income gains, whereas the majority (80%) of the total income increase came from private labor-market earnings.

This increase in labor market earnings in turn reflects the fact that both market wages and employment rose in tandem. Wages rose substantially, by 10.1% in treated areas during June, consistent with the expectation that competitive pressure from the NREGS would influence

3. The centrality of implementation quality in evaluating the effects of NREGS is well recognized. For instance, Imbert and Papp (2015) focus their analysis on the "star" states that implemented the program well during the initial rollout. Our paper follows in this same tradition.

4. Two limitations are especially noteworthy. First, our survey measured wages and employment in June, because this is near the peak season of NREGS activity and hence most relevant for studying the impacts of Smartcards on NREGS implementation. However, this limits our ability to study the transmission of wage and employment effects to the rest of the year. Second, our survey sampling frame of households registered for the NREGS (jobcard holders) likely excludes large landholders who may have been made worse off by the reform. We therefore study distributional effects using both survey and census data (see Section 3.3).

private-sector wages. Yet despite higher wages, we see an *increase*, not a decrease, in market employment among NREGS-registered households, which rose by 20% (1.4 days/month), while days self-employed or not working fell by 13% (2.4 days/month). For each of these outcomes we document significant spatial spillover effects which independently validate the thesis that the reform impacted private labor markets—and which would lead us to substantially understate total effects if not accounted for. These results are internally consistent in the sense that the wage and employment gains we observe in June would, if persistent through the year, explain the increases in annual labor earnings.

Independent census data corroborate the finding that the reform reduced poverty, and also had large economic effects on the *overall* population. In a census of rural households, the proportion in the lowest income bracket fell by 3.4% (2.8 percentage points). We also document substantial increases in non-agricultural employment, paralleling the employment gains in our survey data. Employment in non-agricultural establishments in the Economic Census of firms rose 48.6%. The number of these establishments increased by 29% in parallel, with increases concentrated among small owner-operated enterprises. To put this magnitude in perspective, this employment increase within 3 years of the reform was 15.7% of the working-age population; whereas the share of the Indian workforce engaged in non-agricultural employment increased by 8% in the entire preceding decade (2000-10).

We analyze the distributional consequences of the reform by combining census and survey data. Our calculations suggest that the income gains in our survey of NREGS-registered households fully account for the gains we see in the census of all households. This suggests that poverty reductions were concentrated among NREGS-registered households, as one might expect given they tend to be relatively more dependent on labor as opposed to land income. While we do not directly observe the profits of large landholders, we find that farm earnings per acre fell by 18% and land prices fell by an insignificant 6% among those NREGS-registered households that did own some land, which is consistent with the idea that higher labor costs reduced the returns to land ownership. Combining estimated treatment effects on wage income and land profits with census data on labor and land endowments, we estimate that net incomes increased for the bottom 92.5% of households by landholdings, and that net income decreased only for large landowners holding over 7 acres of land.

The fact that both wages *and* employment increased is central to the large income gains we estimate: wage gains were not offset by reduced employment, but instead were amplified by increased employment. We consider three broad sets of (non mutually exclusive) mechanisms for the increase in wages and employment: (1) an increase in labor productivity; (2) an inward shift in labor supply in the context of imperfectly competitive labor markets (e.g. oligopsony); (3) increases in aggregate demand for locally-produced goods and services. To help interpret the relative importance of these channels, we combine estimated treatment effects with a canonical theoretical model of production, labor supply, and demand.

Our calculations suggest that direct increases in rural labor productivity through augmentation of either physical or human capital from a better-implemented NREGS were likely second order. This interpretation is corroborated by our finding negative treatment effects on land profits and prices (which should be weakly increasing in productivity).

In contrast, we find clear evidence that labor supply to private sector jobs shifted inwards. Reservation wages increased significantly in treated areas, suggesting that an improved NREGS increased workers' bargaining power by enhancing outside options. Moreover, changes in market employment covary systematically with proxies for employer market power. 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 account for 23% of the overall increase in market employment. We estimate an upper bound on the markdown of wages relative to marginal product of 25%. This is within, but at the higher end of, the range of estimates in a global evidence review (Sokolova and Sorensen 2021), suggesting that employers may have considerable market power in this setting.

Finally, we find several pieces of evidence suggesting meaningful increases in local demand from the increased income. First, savings increased by only 3% of the estimated income gain. Second, both borrowing and asset holdings of NREGS beneficiaries increased in treated areas. Together, these two facts suggest that most of the income gains were either consumed or used to acquire assets (but not saved), which would boost local demand.[5] Finally, the large increase in the number of non-agricultural firms, and in employment in these firms suggest that the net benefits of increased demand exceeded the cost of higher wages.

Our first contribution is to the literature and policy debate on the impact of public works programs on labor markets, incomes and poverty (Imbert and Papp 2015; Beegle, Galasso, and Goldberg 2017; Sukhtankar 2017; Bertrand et al. 2021). We confirm some prior findings, like the increase in market wages (Imbert and Papp 2015; Berg et al. 2018), while providing estimates that account for spatial spillovers—in some cases consequentially. We also report a number of new results, including gains in income and reductions in poverty, balance sheet

5. We focused on measuring income rather than consumption, so our measures of consumption are rudimentary. Hence, estimated effects on consumption are very imprecise with a 95% CI of the marginal propensity to consume the additional income of [-92%, +100%]. We therefore infer increases in local demand from the much more precisely estimated (small) treatment effects on savings. Other work has found no overall effect on consumption across India (Klonner and Oldiges 2022), although work focused on AP finds significant increases in consumption (Ravi and Engler 2015; Deininger and Liu 2019)

effects, reductions in land returns, and *positive* effects on private sector employment in two independent data sources (see Section 3.4 for a detailed comparison of our results with existing studies). Taken together, our results suggest that public employment programs can both reduce poverty and enhance economic efficiency.

Second, we contribute to the literature on rural labor markets in developing countries (Jayachandran 2006; Kaur 2019). The employment gains we document, and particularly the mediating role of concentrated landholdings, add to the growing body of evidence of employer market power in a wide range of markets.[6] Our employment results are also broadly consistent with evidence pointing to the absence of large negative employment effects of minimum wages both in developing countries (e.g. Dinkelman and Ranchhod 2012) and elsewhere.[7] Further, our finding large gains in non-agricultural employment adds experimental support to the idea posited by Magruder (2013), Emerick (2018) and Santangelo (2019) (among others) that positive earnings shocks in more "traditional" sectors can drive structural transformation (through boosting aggregate demand), even while raising wages.

Third, our results highlight the influence public options can exert on 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). A similar premise holds in our data: only 7% of income earned by our control group came from the NREGS. Yet 32% of households actively participated in the NREGS at *some* point in 2011-12 (in NSS data), and the reform sharply increased their reservation wages. This underscores that the NREGS's impact depends not only on its "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) and Basu, Chau, and Kanbur (2009).[8]

Fourth, and related, our results highlight the importance of accounting for general equilibrium effects in program evaluation (Acemoglu 2010). Ignoring these effects (say by randomizing program access at 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 (Muralidharan and Niehaus 2017; Cunha, DeGiorgi, and Jayachandran 2019; Egger et al. 2020).

6. E.g. online (Dube et al. 2020) and migrant labor markets (Naidu, Nyarko, and Wang 2016).

7. See Card and Krueger (1994), Cengiz et al. (2019) and Harasztosi and Lindner (2019), among others.

8. Relatedly, Beaudry, Green, and Sand (2012) show that changes in city-level industrial composition affect average wages and wage bargaining, and Clemens and Gottlieb (2017) show that Medicare pricing affects private sector health care prices in the US.

Fifth, our results highlight program *implementation quality* in developing countries as a first order issue in and of itself. It is striking that the estimated effects on market wages of improving NREGS implementation are about as large as the effects of the initial rollout itself (Imbert and Papp 2015), and much larger than the effects of simply increasing official wages without reforming implementation (which Niehaus and Sukhtankar (2013b) find had *no* impact on workers' earnings). This suggests that more generally in settings with high corruption and inefficiency it may be more cost-effective to invest in better implementation of a program than to simply increase program budgets.

# 1 Context & 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 India's flagship social protection programs, and the Indian government spent roughly 6.8% of its budget (∼0.5% of GDP) on it in 2011-12.[9] Coverage is broad: ∼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 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.

As of 2010, NREGS implementation suffered from several known issues. Rationing was common (even though *de jure* jobs were meant to 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 was widespread, and occurred both 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; 2013b). 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 (Khera 2011; Banerjee et al. 2020).

9. 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.

## 1.2 Smartcards

To help address these issues, 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 cash payments 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 using Smartcards.

The Smartcards reform improved NREGS implementation quality on several dimensions, as we show in MNS. Payments in treated mandals arrived 29% faster, 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 (∼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.

Given that Smartcards brought the *effective* presence of NREGS closer to the intentions of its framers, one might in principle think of them as an instrumental variable for a measure of "effective NREGS." In practice defining such a measure is difficult since implementation quality is multi-dimensional, spanning job access and availability, job receipt, wages received net of corruption, speed and reliability of payments, and so on. Past work has typically used administrative records on the number of days of employment provided as a proxy for implementation quality, but this measure may be overstated due to corruption, and also reflect labor market conditions unrelated to implementation quality.[10] One alternative metric which may better quantify the value of changes in NREGS implementation as assessed by workers themselves is the change in their reservation wages, i.e. the amount an employer would have to pay to attract a worker away from the NREGS (or simply not working). Below we estimate that Smartcards increased reservation wages by Rs. 6.9 per day on average (7.1% of the control mean); one might interpret this as the magnitude of the "first stage" effect.

10. Reported employment is higher during drought years, for instance Santangelo (2019), and in our setting does not respond to treatment (Table F.1) even though actual participation does.

In addition to NREGS, Smartcards were 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*.[11] We test and show that treatment did not generate income gains in households where all adults were eligible for the SSP (see Table F.4). In principle, 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 as (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 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.[12]

Overall, the Smartcards reform 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

Our study uses the same experimental design as in MNS. The experiment was originally designed to study the effects of Smartcards on NREGS implementation quality, and our primary data collection reflected this goal. This paper aims to study the downstream effects of improving NREGS implementation on the overall rural economy, and uses both our survey data as well as several additional data sources to capture impacts on the overall economy.

11. Specifically, pensions were restricted to those Below the Poverty Line (BPL) *and* who were 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 (∼\$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.

12. Consistent with this view, Field et al. (2021) find that opening bank accounts and paying NREGS wages into them in Madhya Pradesh had no effect on women's labor supply unless accompanied by training on how to use the accounts. No such training was provided along with Smartcards.

## 2.1 Randomization

The experiment was conducted in eight districts with a combined rural population of around 19 million in the erstwhile state of Andhra Pradesh (AP).[13] The Govt. of AP agreed to randomize the order in which mandals (sub-districts with an average population of ∼62,500) within these districts received Smartcards. We randomly assigned 296 mandals to treatment waves, stratifying by district and by a principal component of mandal socio-economic characteristics.[14] We assigned mandals to treatment (112), "buffer" (139), and control (45) groups, to be treated in that order; we chose this design to give us time to conduct endline surveys in treatment and control mandals while the government deployed Smartcards in buffer mandals. Figure F.1 shows the geographical spread and size of these units. We only collected survey data in treatment and control mandals (study mandals), and did not do so in the "buffer" or other mandals outside the study. As reported in MNS, treatment and control mandals are generally well-balanced on stratification variables, and other census variables (Table F.2), and also on household characteristics from our survey (Table F.3).[15]

## 2.2 Data

We next summarize our main data sources; Appendix A provides further detail, including the construction of every outcome (see Table A.1).

We drew our survey sample from administrative data on the universe of registered NREGS beneficiaries. Our sample is based on a panel of 880 Gram Panchayats ("GPs," groups of villages) across the 157 study mandals, and a repeated cross-section of households within these GPs, yielding a target sample of 5,278 households at endline.[16] We over-sampled households that were listed as having recently been paid in order to gain precision in estimating leakage in MNS. 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) and likely represents the entire universe of rural workers

13. The original state was divided into two states on June 2, 2014. Since this division took place after our experiment (conducted in 2010-12), we use "AP" to refer to the undivided state. Study districts are similar to AP's remaining 13 non-urban districts on major socioeconomic indicators, including proportion rural, scheduled caste, literate, and agricultural laborers; and represent all three historically distinct socio-cultural regions (MNS Online Appendix, Tables D.1 and D.2).

14. We dropped 109 of the 405 mandals in study districts prior to randomization, either because the Smartcards program had already started there or because they were entirely urban and hence had no NREGS. These non-study mandals are similar on observables to the 296 randomized mandals (see MNS, Table D.3).

15. An independent replication by 3ie has also found the results in MNS to be robust (Atanda 2019).

16. We drew a repeated cross-section of households as opposed to a panel due to considerable variation over time on whether specific households report having worked on NREGS.

employed in agriculture.[17]

As our endline sample was drawn from the register of beneficiaries as of endline, one potential concern is that the intervention might have affected this register itself. In Appendix H we examine this issue, showing that treatment is not associated with differential rates of entry into, exit from, or net change in the beneficiary register. This likely reflects the fact that most households who wanted to work on NREGS would have registered for it by 2010, five years after the program was launched.

We conducted baseline and endline surveys during August-September of 2010 and 2012, respectively. We successfully surveyed 4,943 households at endline, or 94% of our target sample, with no differences in either the rate or the composition of follow up across treatment and control groups (see Appendix H). We asked detailed questions about household members' labor market participation, wages, reservation wages, and earnings during the month of June (the peak period of NREGS participation in AP). We also measured annual household earnings by source on an annual recall basis, as well as stocks of savings, debt, and landholdings. When using survey data on monetary outcomes we truncate the top 0.5% of observations in both treatment and control groups to remove outliers.[18]

The timing of measurement is important in our context given seasonal patterns of labor market activity. NREGS activity usually peaks in April-June, as for example is evident in wage disbursements (Figure 1, Panel A), and then drops with the onset of the monsoon rains and the main agricultural planting season later in the summer. Crops are typically harvested in September-October and in January. That said, the *overall* rate of work (including both wage employment and self-employment) appears to hold fairly steady throughout the year (Figure 1, Panel D, plotting data from the National Sample Survey). Figure F.2 summarizes the recall periods covered by our survey data as as well as by other data sources we use. In particular, our most detailed labor market outcomes are for June 2012 (as our survey was timed to capture NREGS activity near its peak), a point we discuss further below.

To examine how the Smartcards reform affected the economy more broadly, beyond the 49.5% of households registered for NREGS, we supplement our survey data with three distinct and entirely independent censuses of income, employment, and livestock assets conducted by the government. The first and most important is the Socio-Economic and Caste Census (SECC), which provides basic information about income for the entire population.

17. In the NSS, 59% of all workers are part of households that hold a jobcard; in our sampled households, 65% of workers work primarily in agriculture. This suggests that we can account for agricultural workers representing $59\% * 65\% = 38\%$ of the workforce, if anything slightly higher than the 2011 census figure of 33% who report primarily working in agriculture.

18. Results are generally robust to including these (Table G.1); we discuss exceptions in the text below, and discuss sensitivity to outliers and to recall issues more generally in Appendices G and J, respectively.

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 highest-earning household member, the main source of this income, landholdings, 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 1.8 million households in our study mandals. In addition to measuring impacts on income in the entire population and the distribution of these effects, the SECC also allows us to construct measures of landholding concentration.

The second 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, gathering 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 over. The third is the Livestock Census of India, a nation-wide census conducted quinquennially by the Government of India, with the 19th round conducted in Andhra Pradesh in 2012.

We also draw on several additional sources. We use expenditure and unit cost data from Round 68 (2011-2012) of the NSS; while overlap between NSS villages and our study mandals is limited to 60 villages, this source uniquely affords consumer goods price measures. We use data on agricultural land use 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. Finally, we use geocoded point locations for each census village from the 2001 Indian Census to construct measures of spatial exposure to treated neighbors.

## 2.3 Estimation strategy

In settings where spillover effects across units are unlikely it is normal to simply regress outcomes on a binary indicator for assignment to treatment and interpret the resulting coefficient estimate $\hat{\beta}$ as an estimate of the total effect of treatment on treated units. In our setting, however, we cannot rule out spillovers a priori. If improving the NREGS has general equilibrium effects, there is no reason to expect these to be confined within mandals.[19] We therefore estimate the total treatment-on-treated effect using an augmented model:

$$Y_{ipmd} = \alpha + \beta_T T_{md} + \beta_N N^R_{pmd} + \gamma \overline{Y}^0_{pmd} + \delta_d + \lambda PC_{md} + \epsilon_{ipmd} \quad (1)$$

19. Merfeld (2019) finds intra-district differences in wage effects of NREGS as a function of distance to the border, suggesting that SUTVA may not hold.

Here $Y_{ipmd}$ is an outcome for household or individual $i$ in gram panchayat $p$ in mandal $m$ and district $d$. The direct effects of treatment are captured in this specification by the coefficient $\beta_T$ on an indicator $T_{md}$ for assignment to the treatment group, while any indirect effects are captured by the coefficient $\beta_N$ on a measure $N^R_{pmd}$ of the intensity with which the area in the neighborhood of a given gram panchayat (but outside its own mandal) was treated. Controls include the GP mean $\overline{Y}^0_{pmd}$ of the baseline value of that outcome (when available),[20] district fixed effects $\delta_d$, and the first principal component $PC_{md}$ of a vector of mandal characteristics used to stratify randomization.[21] We then define our adjusted estimate of the total effect on the treated as

$$\text{Adjusted TE} = \hat{\beta}_T + \hat{\beta}_N \cdot \bar{N}_T \tag{2}$$

where $\bar{N}_T = 36\%$ is the mean of $N^R_{pmd}$ among gram panchayats in treated mandals. We will refer to this quantity as the AdjTE for short to distinguish it from the Average Treatment Effect (ATE). We also report the components $\hat{\beta}_T$ (main effect) and $\hat{\beta}_N \cdot \bar{N}_T$ (neighborhood effect) separately in all tables.[22]

To the extent that the unadjusted estimate $\hat{\beta}$ is biased by spillover effects, this adjusted estimator will help to correct that bias. If on the other hand effects do *not* spill over across mandal boundaries then $N^R_{pmd}$ can be interpreted as an irrelevant control variable, and $\hat{\beta}_T$ and $\hat{\beta}$ are asymptotically equivalent. However, this correction comes at the cost of precision, as we must estimate the contributions of two distinct terms. Which approach is a priori preferable thus depends on one's priors about spillovers and on the relative importance one places on point estimation as opposed to hypothesis testing. Given this we present the AdjTE for main effects on all outcomes, but also provide a parallel set of unadjusted estimates in Appendix K.[23] When testing for heterogeneity we focus on unadjusted effects, since it is not easy to interpret heterogeneity in spillovers, and including spillovers would double the number of parameters to be estimated (including all interactions) and further reduce precision.

20. We control for the baseline mean rather than household $i$'s baseline outcomes as our data come from a repeated cross-section. Omitting this control generally does not substantively change our results (Table G.2); we discuss any exceptions below.

21. 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, so that fixed effects require dropping 4% of the sample. Results using strata fixed effects are substantively unchanged.

22. One can also predict the "total treatment effect" of a universally scaled up program to be $\hat{\beta}_T + \hat{\beta}_N$ (since all neighbors would be treated), but we do not present or discuss this estimate given that it involves extrapolating well beyond the average neighborhood exposure to treatment induced by our experiment.

23. This approach may also be relevant for future experimental studies that may want to test for spatial spillovers. Note that it is important to present both quantities (unadjusted and AdjTE) for transparency. Showing only the unadjusted estimate after testing and finding that spillovers are insignificant would be problematic for inference due to the implied data-driven model selection.

We define our measure $N^R_{pmd}$ of neighborhood treatment intensity as the proportion of GPs located within 20km of GP $p$ and in *other* mandals which were assigned to treatment. This is a simple and intuitive way to capture spillover effects as a linear combination of treatment indicators for nearby gram panchayats, consistent for example with the approach taken in Miguel and Kremer (2004) and Egger et al. (2020). That said, it necessarily involves a number of judgment calls. In Appendix B we discuss the reasoning for these and show that our main conclusions are generally robust to a wide range of alternatives, including interpreting "buffer" mandals as partially treated, using an alternative radius, using a smooth rather than a discrete kernel, including same-mandal gram panchayats in the calculation, and modelling spillovers as a higher-order polynomial function of neighborhood treatment intensity. For outcomes observed at the mandal level, we replace $N^R_{pmd}$ with its average $N^R_{md}$ over gram panchayats in given mandal $m$. This is a natural generalization, but necessarily less well-powered as we average over much of the GP-level variation within a mandal.[24]

We conduct inference for most outcomes using standard errors clustered at the mandal level (the unit of randomization). For outcomes geolocated at the GP level we also report standard errors computed using the method of Conley (2008) which allows for spatial autocorrelation in the error term; these are typically somewhat smaller than clustered standard errors, so we report significance levels conservatively based on the latter.[25] For SECC outcomes the Conley procedure is computationally infeasible (as we have 1.8M observations) but we are able to calculate alternative $p$-values using randomization inference as a robustness check.[26] 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.[27]

# 3 Results

We first present results using our survey data for NREGS-registered households (henceforth, "beneficiaries"), who make up 49.5% of all rural households and are the households the

24. For SECC GPs that we cannot geolocate by matching to the census we replace $N^R_{pmd}$ by its mean and include a dummy variable indicating this.

25. 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.

26. We cannot use randomization inference for survey outcomes because our randomization procedure assigned some mandals to the "buffer" wave in which we did not conduct surveys. Re-running this procedure yields a new assignment in which some of the mandals actually assigned to the buffer are re-assigned to treatment or control waves. To estimate a pseudo-treatment effect under this replicated assignment we would need to observe data for these mandals, which we do not.

27. We do not follow a pre-registered analysis plan as data collection for this project was complete and a first paper written before widespread use of the AEA RCT registry began in July 2014.

NREGS was designed to benefit. We then turn to examining impacts on the overall economy using the various censuses. Finally, we use a combination of survey and census data to characterize the distributional effects of the reform. Our primary focus here is on treatment effects on income and its proximate components—wages and employment. We discuss what people *do* with their additional income in Section 4.2 when we consider mechanisms through which this demand may feed back into wages, employment, and incomes.

## 3.1 Impacts on beneficiaries

### 3.1.1 Earnings

Our first core finding is that the Smartcards reform substantially raised the annual incomes of beneficiaries (Table 1, Column 1). The estimated AdjTE is over Rs. 9,500 per household per year, driven largely by the main effect of treatment with a smaller, statistically insignificant contribution from the neighborhood effect. The AdjTE is equal to 13.9% of the control group mean or 19.6% of the national expenditure-based rural poverty line for a family of 5 in 2011-12, which was Rs. 48,960. Expenditure- and income-based poverty lines may of course differ, so the comparison is only illustrative. That said, if we examine how many households' *incomes* moved across the *expenditure* poverty line we estimate a 7.4 percentage point (25.8%) reduction in poverty among beneficiaries. Figure F.3 illustrates this, and more broadly the fact that gains were broad-based throughout the income distribution, with the treatment distribution first-order stochastically dominating that in the control group (with the caveat that this Figure captures main but not adjusted treatment effects).

Next, these income gains were driven primarily by increases in private sector wage labor earnings rather than NREGS earnings (Table 1, Columns 2-5). The latter accounted for an estimated 14% of the earnings gain (with a marginally significant main effect and a larger but less precise AdjTE). Meanwhile, market wage earnings rose substantially and significantly, and accounted for 80% of the AdjTE on total income. Income from other sources did not change significantly: self-employment income (encompassing farm and non-farm earnings) fell slightly (95% CI of $[-7073, 5536]$) and miscellaneous income (encompassing private and government employment earnings, along with pensions, gifts and any other income) increased slightly (95% CI of $[-2385, 7388]$).

This pattern of effects aligns with the structure of control group earnings, where NREGS earnings account for just 7% of the total while wage labor earnings account for 35%. This in turn is broadly consistent with nationally representative statistics, in which the NREGS is a relatively small source of employment. Even a proportionately large increase in NREGS earnings such as the one we see here can thus contribute only modestly to overall income growth, while indirect effects through labor market earnings can potentially be much larger.

The importance of labor market earnings is also seen in our analysis of heterogeneity. We do not see substantial differences in income effects along demographic characteristics such as caste or education, though these splits are not estimated precisely (Table F.4, Columns 1 & 2). We do, however, see substantially smaller and insignificant effects for households headed by a widow and those with one or more members eligible for a pension (Columns 3 & 4), with the differential effect marginally significant in the latter case ($p = 0.098$). Thus, income gains accrued primarily to those who are likely to have participated in the labor market, and not to those who did not work.

### 3.1.2 Wages and Employment

To better understand the drivers of these wage earnings, we turn to more detailed wage and employment data for June 2012.[28] In addition to data on market wages, we also collected data on reservation wages for all respondents.[29]

We find that treatment significantly increased respondents' reservation wages by approximately Rs. 6.9, or 7.1% of the control group mean (Table 2, Column 1), with a significant main effect and a small and insignificant spillover effect. The increase in reservation wage provides direct evidence that the reform made NREGS a more appealing outside option. We find no evidence of spatial spillovers in reservation wages, which is consistent with the legal requirement that jobcard holders could only do NREGS work in their own villages.[30]

Consistent with the increase in reservation wages in treated areas, we also see large increases in market wages (Table 2, Column 2). The AdjTE is a Rs. 13 increase on a base of Rs. 128 per day, or 10.2%. Of this effect, roughly 2/3 is the main effect, and the remaining 1/3 is a (statistically significant) spillover effect. The interpretation is that having 36% of ones' neighbors treated (the average neighborhood exposure induced by the experiment) led to half as big an effect as that of receiving treatment oneself. Equivalently, failing to adjust for these spillovers would bias our estimated wage effects downwards by 33%. Results are

28. We report results for all adults, including those who identify primarily as workers but also some who identify as primarily students, houseworkers, or retirees, as the latter do report positive amounts of work. Control means are thus an average across individuals working full- and part-time (see Table I.1 for a cross-tabulation). Full-time adult male workers reported working 22 days out of the previous 30, or slightly more than 5 days per week..

29. We asked respondents whether they would have been "willing to work for someone else for a daily wage of Rs. X" in June, 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 H.1).

30. This result is also consistent with our finding no significant spillovers in measures of program implementation quality from the Smartcards intervention (Tables E5-E7 of MNS).

robust to re-weighting wage data by days worked (Column 3)[31]

The spillover results independently corroborate the hypothesis test based on the main effect, rejecting the null in the same direction, using a different source of exogenous variation—the fraction of neighbors *outside* a GP's own mandal that were randomly assigned to treatment.[32] They also suggest that rural labor markets in this setting are spatially integrated beyond individual GPs or even mandals. Specifically, the results show that there was greater upward pressure on market wages when a larger share of nearby workers were treated, and consequently had higher reservation wages.[33]

These wage increases only partly explain increases in labor market earnings; we also see an *increase* of 1.4 days per month (18%) in private-sector employment (Column 5, $p = 0.08$). Here the main effect and neighborhood effect are quantitatively similar, the latter marginally significant. Days worked in the NREGS also increased significantly by 1.3 days per month (29%) (Column 4, $p = 0.02$), consistent with the findings reported in MNS, and the NREGS earnings gains in Table 1.[34] These days replace time spent in self-employment or not working, which fell by 2.4 days per month (14%) (Column 3, $p = 0.003$). In Appendix I we show that this reduction was driven primarily by a significant reduction in days not working, with a smaller reduction in days of self-employment (Table I.2).

Taken together, the wage and employment point estimates in Table 2 imply a 29% increase in June labor market earnings, which aligns well with the 32% increase in *annual* labor market earnings in Table 1.[35] We do not observe the same detailed individual employment and earnings data year-round to make this comparison exact. We did, however, ask village leaders to report the "going wage rate" in their communities for each month of the year. Estimated impacts on this measure are imprecise (as we have only a maximum of three data points per village-month), but suggest that wage gains were sustained to some extent

31. We also show in Appendix I that treatment did not significantly alter workforce composition on a large set of characteristics, and that the (insignificant) changes we do see cannot explain the wage effects.

32. We also reject the joint null of no spillovers for *any* of the outcomes in Table 2 ($p < 0.01$) (even though a priori we would not expect spillovers in reservation wages). This corroborates our interpretation of treatment effects as being driven by the labor market effects of improving NREGS, as channels such as improved pensions and financial inclusion should only matter within treated areas and not beyond.

33. Note that an increase in worker bargaining power can raise wages without direct negotiation taking place; higher reservation wages imply that employers must raise wages simply to get workers to participate.

34. We use here the sample of workers who reported their private-sector work. For the NREGS outcome we can also expand our sample if we impute zeros for individuals who reported (earlier in the survey) that they had *never* worked on NREGS; doing so yields similar results (Table G.3).

35. That is, $[(128 + 13)/128] \times [(7.9 + 1.4)/7.9] - 1 = 29\%$. Since Table 1 has annual outcomes at the household level, and Table 2 has June outcomes at the individual level, it is easier to test for consistency across the tables by comparing percentage changes. The magnitudes across the tables are comparable if we multiply quantities in Table 2 by 12 (to go from month to year), and by 2 (approximate number of working members per household).

through the year (Figure 1, Panel C).[36] This is consistent with the fact that almost all study villages had at least one NREGS project active through most of the year, with availability dropping to a low of 40-50% of villages toward the end of the year (Figure 1, Panel B), so that the NREGS remained a credible outside option if not a major employer through much of the year. It may also reflect mechanisms through which wages are linked across time.[37]

Another potentially important influence on households' earnings is migration patterns. Here, however, we do not see strong or consistent evidence of an effect (Table F.5). Across five indicators, the signs of the estimated effects suggest less migration in two cases (non-response due to migration and household size) and more migration in the other three (whether any member migrated, total days of individual migration, and whether local leaders reported migration was common in May); only the indicator for whether any member migrated is significant ($p = 0.08$). Overall this mixed pattern of results may reflect the offsetting price and income effects that higher rural wages would be expected to have, reducing the incentive while increasing the ability to migrate (Bryan, Chowdhury, and Mobarak 2014; Bazzi 2017).[38]

### 3.1.3 Prices and Land Returns

Given that the reform impacted wages, it is also possible that it affected the local prices of final consumer goods, and thus the overall price level facing consumers. In this case the nominal earnings effects we reported above would overstate the real gains to beneficiaries.

We examine price effects using data from the 68th round of the NSS, collected in 2011-12. 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 covers 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 construct an index equal to the price of purchasing the mean bundle of goods in the control group

36. Imbert and Papp (2015) report a similar pattern, with positive but imprecise wage impacts in the "rainy season" amounting to 61% of the dry season impact they focus on.

37. These include nominal wage rigidity (Kaur 2019) and labor tying over the agricultural cycle (Bardhan 1983; Mukherjee and Ray 1995; Anderson, Francois, and Kotwal 2015). The latter literature in particular suggests that landlords who provide insurance in the lean season pay lower wages in the peak season. In these models, better NREGS availability and higher market wages in the lean season would imply a reduced need for insurance from landlords and a resulting higher wage in the peak (non-NREGS) season.

38. Imbert and Papp (2019) find evidence that the NREGS rollout reduced short-term migration. However, their results are driven by effects in traditional migrant-sending states such as Bihar, Uttar Pradesh, and Rajasthan, and they report that our study districts have among the lowest seasonal migration rates in India (see Figure 4 in their paper), suggesting the contexts are quite different.

following Deaton and Tarozzi (2000):

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

Here $\bar{q}_{cd}$ is the average number of units of commodity $c$ purchased 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 measure the compensating variation required to enable households to continue purchasing their old bundle of goods at the (potentially) new prices.

In our preferred approach we restrict attention to goods and services purchased at least once in *every* village in our sample. This ensures that we are not picking up compositional effects on the basket of goods purchased, but has the drawback of excluding roughly 40% of the expenditure per village in our sample. We therefore complement it with a version applying Equation (3) to all available data, and also report effects on the log of unit costs defined at the household-commodity level and including all available data. These later specifications may capture some compositional effects, but do not drop any information.

Regardless of method, we do not see evidence of price appreciation (Table 3). The point estimate using our preferred method actually suggests a *decrease* of 6 log points, though it is not very precisely estimated (95% CI of $[-0.32, 0.20]$). Using either of our alternative methods we obtain estimates very close to zero and precise enough to reject effects as large as the 14% increase in earnings we observe.[39]

One implication of these results is that the costs of higher unskilled wages were absorbed by employers, rather than passed through to consumers. In the agricultural sector in particular (where the majority of beneficiaries worked) this implies that the profits of landowners should have fallen. While our sample of NREGS jobcard-holders likely does not include the larger landholders and other employers we would expect to be hurt by higher wages, our data does include holders of *some* land; 24.6% of households own 3 or more acres of land (38.3% among those with a positive amount of land). We can therefore examine how the reform affected the returns on this land. We do this using a variant of Equation 1 augmented with the (log) of landholdings as a control variable, to capture the fact that land earnings and prices are roughly log-linear in acres held with a slope substantially less than one (Figure F.4).

We find that net farm earnings *fell* significantly by 19 log points, or 21% (Table 3, Column 4). This is consistent with the increase in wages, regardless of households' net labor position: even if firms of this scale do not hire much labor in, higher wages would also raise

39. The adjusted $R^2$ values in Table 3 are close to 1 because a large share of the variation is explained by either district fixed effects (in the village-level specifications in Columns 1 and 2) or item fixed effects (in the item $\times$ household level specification in Column 3).

the opportunity costs of family farm labor that could be hired out. The result indicates that a better-implemented NREGS substantially shifted the returns to factor ownership, reducing the returns to land even as it increased the returns to labor. In line with this result, landowners self-report valuations of their land that are lower by 6 log points, or 6.2% lower (Table 3, Column 5), though this difference is not significant ($p = 0.65$). Together these results point to meaningful economic losses for landholders, in contrast to the large gains experienced by workers. We return to this point in Section 3.3.

## 3.2 Impacts on the broader economy

We turn next to evidence from government censuses on income and employment. These complement our survey data in three ways. First, the census data include *all* households and not just jobcard holders, and allow us to understand impacts on population-level outcomes. Second, they provide an independent source of validation of our main survey-data results of higher incomes and employment and lower poverty as a result of the reform. Third, in conjunction with our survey results, they allow us to examine the distribution of impacts.

We begin as above with earnings. The Socio-Economic Caste Census (SECC) classifies households into low, middle, and high-income categories based on whether their highest-earning member reported monthly earnings below Rs. 5,000, between Rs. 5,000 and Rs. 10,000, and greater than Rs. 10,000, respectively. By way of comparison, the average control group *household* in our survey data reported monthly earnings of Rs. 5,800, and so was likely near or below the threshold between the bottom and middle SECC tiers.

We find that the reform shifted a significant share of households from the lowest income category to higher categories (Table 4). This is clearest when we look at the main effects of treatment, which we estimate most precisely, but the same pattern is also evident and significant at the 10% level in the adjusted treatment effects. We also strongly reject the null of no effect for all three earnings categories when we estimate a simpler unadjusted specification without spillover effects (Table K.4). Quantitatively, the estimates imply that roughly 3% of households moved out of the bottom bracket, primarily into the middle one.[40] Overall, these results validate using entirely independent census data that the reform had substantial impacts on population-level earnings and poverty.

We turn next to employment. Our survey data arguably provides good coverage of agricultural employment: the majority (65%) of workers report working primarily in agriculture, and collectively they can account for most of the agricultural workforce in our study area (see Footnote 17). The Economic Census provides a complementary picture of employment

40. We report logit marginal effects; results from linear probability models are essentially the same.

in the non-agricultural sector one year after our survey.

Employment in the non-agricultural sector increased substantially (Table 5, Panel A). The overall AdjTE of 3,307 additional workers per mandal is a large 48.6% increase relative to the control group mean of 6,797. Looking across subsectors, effects were uniformly positive but largest in manufacturing and construction, wholesale and retail, and "other," with smaller effects in livestock-related firms. Paralleling this employment expansion, the number of non-agricultural firms also increased significantly (Table 5, Panel B). The AdjTE of an additional 1,095 additional firms per mandal represents 29% of the control group mean. Notice that this is the opposite of what we would expect to see due solely to upward wage pressure: regardless of market structure, higher wages should reduce firm profits and thus induce exit, not entry. The fact that firm count increased and that estimated effects on firm count and employment are positive at all firm sizes (Table F.6) suggests a positive shock to demand for locally produced non-agricultural goods and services, an idea we return to in Section 4.2.

## 3.3 Distributional Impacts

The positive effects on overall income and employment in independent censuses complement the results from our survey data and suggest that the overall effects of the reform were positive. We turn next to its distributional impacts, combining the SECC and survey data to examine variation across households along two dimensions: status as an NREGS beneficiary, and the relative importance of labor and land endowments.

The SECC includes non-beneficiaries but does not let us isolate impacts on them directly, as it does not record which households have jobcards. We can, however, infer impacts on non-beneficiaries using a simple decomposition of the overall average treatment effect $\beta^{All}$. Since beneficiaries make up 49.5% of the population and the conditional variance of treatment is the same among both groups (due to random assignment), we can write

$$\beta^{All} = (49.5\%) \times \beta^{B} + (50.5\%) \times \beta^{NB} \tag{4}$$

where $\beta^{B}$ and $\beta^{NB}$ are average effects on beneficiaries and non-beneficiaries, respectively.[41]

Combined with a consistent estimate of $\beta^{B}$ from our survey data, this allows us in principle to consistently estimate $\beta^{NB}$. To implement this idea we need to harmonize the distinct income measures reported in the SECC and in our survey. We do so by constructing outcomes from our continuous survey measure that are comparable to the categorical SECC measures,

41. See for example Angrist (1998). Strictly speaking this argument has been demonstrated in a linear regression setting; while we present results here from our default logit specification, results from a linear probability model analogue are essentially identical.

calibrating the share of total household income accruing to the top earner at 65% to match that from a separate survey conducted in our 8 study districts in 2014.[42]

The results (Table F.7) suggest that the earnings gains we observe in the SECC accrued primarily to beneficiaries. Focusing on the 65% top earner share as our central scenario, the point estimates for beneficiaries are close to double those we see in the SECC overall, which (since beneficiaries are roughly half the population) implies that they can account for the full effect. The inferred estimates for non-beneficiaries are consequently close to zero, and we can reject large effects (e.g. larger than a 0.3% decrease in share in the lowest earning category). Results are not particularly sensitive to alternative nearby assumptions about the top earner share. Thus, while the reform triggered substantial general equilibrium effects, the pattern of these was such that gains still accrued primarily to the population the NREGS was intended to benefit (jobcard holders).

We next consider distributional impacts by households' endowments of land and labor from the SECC data. We summarize results here, with full details of the analysis in Appendix D. Figure 2 sorts and bins households by landholdings and then for each bin estimates the gains from labor income, losses from land income, and resulting net change in total income. Panel A presents gains from labor income (the bar plot), calculated by multiplying the average number of working-age adults (aged 18-65) in the household in the SECC data (the line plot) by the estimated treatment effects on labor income per working-age adult in the survey data.

Panel B plots losses from land income (bar plots), calculated as average landholdings (line plot) multiplied by the estimated impacts on profits. We consider two possible values for profit reduction: the 6.2% reduction in land prices estimated in Table 3, Column 5,[43] and a 2.5% reduction obtained from a calibrated Cobb-Douglas production function (see Appendix C.3.2). To obtain absolute reductions we multiply these figures by the Rs 10,200/acre average profit in the NSS agriculture survey of 2012.

Panel C adds these two effects, revealing that the estimated net impact of the reform was positive for almost all citizens. Even using the larger 6.2% estimate of profit reduction we see that the bottom 92.5% of households (ordered by landholdings) were better off after the reform, with only large landholders—holding over 7 acres—worse off. Using the model-based 2.5% estimate of profit reduction net effects are positive even for this top group. Net effects are also positive at all points of the distribution if we order households not by landholding

42. We use the Center for the Monitoring of the Indian Economy's (CMIE) household panel survey, which attributes income to specific household members. In the CMIE panel approximately 65% of household earnings are attributable to the top earner. We also examine sensitivity to values in $[55\%, 75\%]$

43. We use the estimated 6.2% reduction in land prices instead of the 19% reduction in land profits reported in Table 3 because the latter likely also reflect reallocation of labor from respondents own farms to wage labor, and survey responses are unlikely to have imputed the cost of their own labor.

but by percentiles of imputed consumption using the SECC data (Figure D.1; see Appendix D for details). Overall, this analysis suggests that in addition to causing large gains in average household income, the reform generated net gains for over 92% of the population, with net losses seen only for large landowners.[44]

## 3.4 Comparison with other studies

We now turn to comparing our results with the existing literature on NREGS, especially Imbert and Papp (2015) (henceforth IP) who provide the best-identified estimates of impact to date. At the outset, we note that there is no a priori reason for the magnitudes to be strictly comparable given that the studies differ in terms of (a) the exact intervention in question (improving implementation versus the initial program rollout), (b) the setting (AP versus all-India or a subset of "star" states that implemented NREGS well), (c) the time period (in 2010-2012 at a more mature stage of implementation versus 2005-2008 during a period of significant implementation challenges), (d) the survey sampling frame (jobcard holders as opposed to all households), and (e) the outcomes measured. At the same time, comparing the patterns of results across studies is helpful for drawing broader lessons regarding the impact of increasing the *effective* presence of NREGS on the ground—which happens both when the program is rolled out, and when its implementation is improved.

With respect to wages, the 10.1% increase we observe is similar to the 9% increase that IP estimate for "star" states (of which Andhra Pradesh was one), and over double their nation-wide estimate of 4.7% (which includes states that initially did not implement the program well). This underscores the importance of the quality of program *implementation* as well as its *presence*, and highlights that improving implementation quality may be as first order for achieving intended impacts as rolling out the program itself.[45]

The main difference between our results and IP's is with respect to employment, as IP estimate a modest negative effect (with an elasticity of $-0.38$) in the star-states while we estimate a positive one, and can reject the null of the elasticity they estimate ($p = 0.07$).[46] One important explanation, among others, may be differences in the way employment is

44. Figure 2 can also explain why net program gains in the SECC data are concentrated among jobcard holders. Non-jobcard holders are likely to own more land, and the wage gains in this group are likely offset by reduction in land profits.

45. Work by Ravallion, Datt, and Chaudhuri (1993) on a precursor EGS in Maharashtra, on the other hand, finds no impact of a higher EGS wage on private-sector wages. The contrast illustrates the importance of implementation: they show that the higher EGS wage led de facto to the rationing of EGS jobs, so that they were not a credible outside option for workers seeking private-sector employment.

46. Meanwhile Zimmermann (2020), also using NSS data, finds that NREGS did not crowd-out private sector employment but instead led to an increase in family employment. These differences are likely due to differences in design (regression discontinuity) and sample (comparison of different roll-out phases).

measured. Specifically, the NSS does not distinguish between self-employment and market employment in its categorization of private employment. Thus, as IP note, their estimated reduction in overall employment in the NSS data may reflect in part or whole a reduction in low marginal product self-employment, as opposed to higher marginal product market wage labor. Our data identify these categories separately, and indeed we find a reduction in self-employment (consistent with IP) but an increase in paid private-sector employment.[47] Importantly, results from the independent Economic Census corroborate the latter result.

Turning to income, our results do not have exact analogues in the existing literature because we measure income directly in our surveys and observe income categories in the SECC, whereas the NSS does not do so. The large effects on income we find in both our survey data and in the SECC thus establish a key new result in the NREGS literature. It is also consistent with recent evidence from Cook and Shah (2022) who find that the introduction of NREGS led to an increase in overall economic activity as measured by bank deposits and night-time lights, which they use as proxies for economic activity.

By way of comparison, it is instructive to relate our direct estimates to those which IP construct indirectly from estimated effects on wages and employment. Expressed as a proportion of NSS household per capita expenditure (PCE), they obtain all-India estimates ranging from 5.8% in the lowest quintile of the PCE distribution to -0.2% in the highest (where higher wages reduce the earnings of net employers). This progressive pattern is consistent with our distributional estimates in Figure 2. However, we estimate a larger overall effect, with estimated *average* earnings gains of 10.9% of mean PCE among jobcard-holding households in AP. We show in Appendix E that this difference is fully explained by the differences in estimated employment effects: using comparable wage changes and IP's estimated employment elasticity of -0.38 instead of ours, we would have seen an estimated income increase of 3.3% of mean PCE.

Taken together, our results are consistent with IP's for wages as well as (broadly) for the distributional effects of increasing the (effective) presence of the NREGS. The main points of difference are the employment gains we see in both survey and census data and the relatively large effects we see on income. Note that these two differences are closely related since IP do not directly observe income, but indirectly infer it from the wage and employment results.

47. Our estimates also adjust for spillovers, which the NSS data does not allow IP to do. While research designs (such as those of IP) that exploit the district-level rollout of the NREGS should capture some of the spillovers that we detect here at the sub-district (mandal) level, in practice both units are fairly "small" relative to the radius over which we estimate spillovers. In our data, while 99.7% of GPs are within 20km of their mandal border, a substantial 83% of GPs are located within 20km of their district border and would therefore be exposed to cross-district spillovers. Of course, ignoring spillovers should not change the sign of the measured impact (which is better explained by how the NSS measures employment). But our estimated effects on market employment would be smaller and insignificant if we did not correct for spillovers.

# 4 Mechanisms & Discussion

We now turn to a discussion of the economic mechanisms that could explain our experimental results. We focus first on mechanisms through which the initial "impulse" of an improved NREGS could have affected rural labor markets. In doing so we put special emphasis on understanding the result that both wages *and* employment increased in tandem. This pattern is key for assessing the overall policy impacts of a more effective NREGS (as it implies an increase in aggregate economic activity), as well as for explaining the large increases in labor income we observe (as the wage increase alone explains less than half the income gains holding employment fixed). We then discuss ways in which these wage and income increases could in turn have affected other parts of the economy. Our goal in this section is not to conclusively rule out any specific channel, but to provide a sense of the relative importance of various mechanisms.

## 4.1 Direct impacts on Rural Labor Markets

We interpret treatment effects through the lens of a canonical theoretical framework (presented in Appendix C). In the model, workers supply labor to the market if the wage offered exceeds their reservation wage, which depends on the quality of NREGS implementation. We characterize the impacts of improving NREGS quality on wages and employment under various combinations of labor supply and demand conditions, including both perfect and imperfect competition among employers. In particular, the model formalizes the idea that an increase in employment must reflect some combination of an outward shift in labor demand driven by higher labor (revenue) productivity, and an upward shift in labor supply in the context of monopsonistic labor markets. We consider both channels in turn.

### 4.1.1 Labor Productivity

An improved NREGS could boost labor demand in three ways. First, it could increase the marginal product of labor by augmenting the stock of complementary physical capital. Second, it could directly make workers more productive through human capital channels such as improved nutrition or skills. Third, increased income from NREGS could boost demand for goods produced using local labor. We examine the first two (productivity) channels here, and the third (demand) channel in Section 4.2.

With respect to complementary capital inputs, an increase in NREGS participation could have increased the quantity or quality of public assets such as roads, ponds, and canals created under it. Such improvements could make labor more productive. However, calculations based on our model in Appendix C.2 suggest that this asset creation channel was likely to be small. Using a Cobb-Douglas production function with factor shares estimated from the NSS 2012 cost of cultivation survey, we calculate that we would need a 59.9% increase in the rural capital stock to fully account for the increases in wage and employment we find. In contrast, we estimate that *all* NREGS capital formation during 2010-12 represented 4.4% of the total rural capital stock. Even if the 28.9% increase in NREGS days worked (Table 2) led to a proportionate increase in NREGS assets, this would imply a total increase in capital stock due to the intervention of just 1.3% (4.4% x 28.9%), and thus could account for at most 2.4% of the 59.9% increase in rural capital stock needed to explain our results. We also find no significant effects on land use measures that one might expect to respond to rural infrastructure, such as the amount of land under cultivation or on the total area irrigated (Table F.8), ruling out effect sizes larger than 16% and 10%, respectively.

With respect to human capital, our survey contains several helpful indicators. One captures whether all members of the household ate at least 3 full meals every day in the last month. The control mean is 97.4% and the adjusted treatment effect is 1% and not significant, suggesting that a nutrition-based efficiency wage mechanism (Dasgupta and Ray 1986) is unlikely to apply here. We also observe the skill level of the work performed by on the NREGS. Consistent with the NREGS requirement that projects use unskilled labor, 97.2% of survey respondents report doing unskilled manual labor, the others serving as a field assistant (or "mate") recording attendance and the quantity of work done (e.g. volume of earth removed). Low rates of skilled work, and the absence of treatment effects on this rate (Table F.9, Column 2), suggest that the scope for skill acquisition was quite limited. More generally, profits from land should be (weakly) increasing in labor productivity (see Appendix C). Therefore, the *negative* effects we observe on profits from land suggest that increases in labor productivity were unlikely to have been first order.

### 4.1.2 Labor Supply and Employer Market Power

An improved NREGS could also provide a better outside option for workers, shifting the labor supply curve to the private sector inwards and thus driving up wages (see Appendix C for a formal derivation). This possibility has been widely conjectured (see for instance, Basu, Chau, and Kanbur (2009)), but has been difficult to establish empirically because existing work typically observes only the market wage, which can reflect changes in either labor supply or demand. Our data on reservation wages allows us to test this hypothesis, and the increase in reservation wage in treated areas of Rs. 6.9, or 7.1% of the control group mean (Table 2, Column 1), provides direct evidence that labor supply shifted inwards.[48]

The effects of higher reservation wages on employment are ambiguous and depend on market structure. If labor markets were competitive, it should lead to a reduction in market employment—especially in the absence of meaningful increases in labor productivity, as shown in the previous section. However, if employers have market power to set wages, this need not be true. This is easiest to see in the case where NREGS and private sector jobs are perfect substitutes, in which case the NREGS wage acts as a binding minimum wage, but as we illustrate in Appendix C the result also holds more generally.

We have already seen one piece of suggestive evidence in Table 2 pointing to imperfectly competitive labor markets in the fact that market wages increased by considerably more (Rs. 13) than the increase in reservation wages (Rs. 6.9). This is consistent with the fact that employment rose (for which the market wage increase would need to more than compensate for the reservation wage increase) but not with competitive labor markets, in which case the market wage should go up by no more than reservation wages.

To test for this possibility more formally we construct a measure of employer concentration, a commonly-used proxy for market power. Specifically, we use household level data on landholdings from the SECC to construct a normalized Herfindahl-Hirschman index ($H^*$) of landholding concentration at the village level.[49] We then test for the existence of employer market power by examining if treatment effects on employment vary with $H^*$.

We find that villages with greater land concentration had both lower levels of employment, and also significantly larger positive treatment effects on employment (Table 6, Column 1). Both facts are consistent with monopsonistic labor markets.[50] To better interpret the

48. This is consistent with descriptive evidence that the private sector competes for labor with the NREGS. We provide several additional pieces of evidence to support this view. First, there is substantial overlap in the distribution of earnings per day in the two sectors (Figure F.5)—suggesting that these are substitutable sources of income (keeping in mind that many non-wage job attributes also vary across the sectors). Second, the *same* people often work in both sectors: in June, 64% of workers who did some private sector work also did some NREGS work, and 51% of those who did some NREGS work also did some private sector work. Third, when we asked individuals who had done NREGS work what they would have done if they had been unable to get it, only 6.5% reported that their alternative would at any point have been to not work; the remaining 93.5% reported that their alternative would always have been to work in the private sector.

49. We calculate the $H^*$ 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 $H_p^* = \frac{H_p - \frac{1}{N_p}}{1 - \frac{1}{N_p}}$. We also calculate an alternate measure of $H^*$ using only households with more than 1 acre of land, since those with less than an acre of land are less likely to use hired labor. Since the SECC was conducted after the treatment, we verify that the treatment did not affect land concentration: the mean difference in standardized $H^*$ is 0.0005, $p = 0.9$, Table F.10, Column 2.

50. As shown in Appendix C, employment should fall if wages rise in competitive markets (with no productivity gains), whereas it can increase in the presence of employer market power, making the differential effects on employment by $H^*$ the sharp test of employer market power. Differential predictions for effects on wages are not sharp, depending for example on the cross-sectional covariation between market wages, reservation wages, and land concentration. For completeness we nevertheless report the corresponding results (Table

magnitudes of these effects, we also present the results for a standardized version of $H^*$ ($\mu = 0$, $\sigma = 1$) in Column 3. Treated villages whose land concentration is $1\sigma$ above the mean had 0.55 days of additional private-sector employment (Table 6, Column 3). These results also hold when we construct $H^*$ using only landholdings above 1 acre (Columns 2 and 4).

The $H^*$ we compute is likely to understate the concentration of effective wage-setting power. For instance, as suggested by Anderson, Francois, and Kotwal (2015), landholders of the same sub-caste (*jati*) may collude, making effective land concentration higher than that measured by the $H^*$ of household-level landholdings. Unfortunately, data on *jati* in the SECC has not been released by the Government of India. Yet, even this imperfect measure of concentration can account for 23.2% of the positive effects on employment we find.[51]

To quantify employer market power economically the appropriate measure is the wedge between wages and the marginal product of labor, determined by the elasticity of labor supply facing individual employers. While we do not observe the employer-level data necessary to estimate these individual elasticities, we show in Appendix C that we can identify the aggregate labor supply elasticity using the estimated moments in our data (in particular, exploiting the fact that we observe reservation wages). Using this approach we estimate an aggregate elasticity of 3.07 (albeit imprecisely).[52] Since employer-specific elasticities are presumably greater than the aggregate elasticity, this in turn bounds the market power of any individual employer, implying that workers receive at least 75% of their marginal product.

By way of comparison, Sokolova and Sorensen's (2021) review of studies across a range of labor markets finds an average firm-level elasticity among "best-practice" studies of 7.1, implying that workers receive 88% of their marginal product, with a 95% confidence interval from 64% to 93%. Our estimate of the aggregate elasticity lies towards the lower end of this range, meaning that our results are consistent with a relatively substantial degree of employer market power. These results are also consistent with those in Soundararajan (2019), who finds suggestive evidence of monopsonistic labor markets in India: better enforcement of minimum wage laws increased both market wages and employment.

## 4.2 Effects on the Broader Economy

The initial impacts of the reform on rural labor markets and incomes likely had downstream effects on the broader economy, which could also generate feedback to rural labor markets. To the extent households spent their additional income on locally-produced goods and services

F.11). We cannot draw any strong conclusions, as interaction terms are estimated fairly imprecisely and signs vary depending on the measure of land concentration we use.

51. The mean $H^*$ in our data is 0.021, and the coefficient on $H^* \times T$ is 6.5 (Table 6, Column 2). Multiplying the 2 gives us 0.1365 which is 23.2% of the total effect on employment of 0.5865 days (0.45 + 0.1365).

52. A 95% CI derived via the delta method is [-3.89, 10.02]; see Appendix C.

(whether for consumption or asset purchases) as opposed to purchasing imported goods or saving through financial intermediaries, this would tend to stimulate economic activity. Higher wages, on the other hand, could dampen employment and economic activity in more competitive sectors of the labor market.

We examine treatment effects on consumption, savings, and assets in our survey data in Table 7. Our survey focused on measuring income as opposed to consumption, and hence contained a very abbreviated single-page expenditure module. Using this data we estimate a small increase equivalent to 4% of the estimated income gain, but also cannot reject changes ranging from -92% to +100% of the income gain at the 95% confidence level (Column 1). The NSS, on the other hand, contains a far more detailed consumption module, but for a much smaller sample of NREGS beneficiaries. Using the NSS we estimate a increase that is *larger* than the estimated earnings gain, but again not precisely estimated (Column 2), though we do marginally reject the null that the main effect of treatment was zero ($p = 0.07$). Together these two data points suggest a positive marginal propensity to consume, but are consistent with a wide range of possibilities.

In terms of balance sheet outcomes, we see an insignificant increase in liquid savings by Rs. 260, or 9% of the control mean. We see more substantial changes in total borrowing (by Rs. 20,400 or 30%) and the probability of owning land (by 7.2 percentage points or 12%) (Columns 3-5). Our survey did not cover holdings of livestock, but analysis of data from the 2012 livestock census suggests a shift in holdings from cattle to (more lucrative) buffaloes (Table F.12). However, the estimated treatment effect on the total value of livestock is small (5.2%) and reasonably precise with a 95% CI of [-9.1%, 19.7%].[53]

Two features of the borrowing increase are worth highlighting. First, it is driven by informal borrowing, as opposed to borrowing from formal financial institutions (Table F.13, Columns 2-4). Second, it is driven by borrowing to offset negative shocks (e.g. unemployment) or cover the costs of major events (e.g. weddings), not to invest in productive assets, consumer durables, or refinancing (Columns 5-9). Increased household earnings thus did not "crowd in" investment using outside capital, but may have increased borrower and/or lender confidence that informal loans could be repaid.[54]

However, the key point to note is that both the level of financial savings and the treatment effect on savings are very low. Mean household savings *stocks* are less than 5% of annual household expenditure *flows*, and mean outstanding loans are over twenty times greater than mean savings (Table 7). Similarly, the increase in savings (Rs. 260) is less than 3%

53. We value livestock at district prices, so that results reflect treatment effects on price-weighted animal counts, but not on prices themselves. See notes to Table F.12 for further details.

54. Both results are also inconsistent with a direct financial inclusion explanation for the other results.

of treatment effects on income (Rs. 9579), and this marginal propensity to save is precisely estimated with a 95% CI of [-3.6%, +9.0%]. This implies that most of the increased income was either consumed (though we measure this imprecisely) or spent on tangible assets.[55] Either use of funds would increase local demand relative to increasing deposits in the financial system and being deployed elsewhere.

While we do not observe how this spending was allocated across the agricultural and non-agricultural sectors, there are several reasons to think that it was concentrated on the latter. First, it is well-established that the income elasticity of demand for food crops (the primary agricultural output in AP) is low.[56] Second, and consistent with this view, the reduction in farm profits suggests that the net effect of any increased productivity *and* any increase in demand was not enough to offset the cost of higher wages. Third, and in contrast, the significant increase in the number of *non-agricultural* firms and employment in these firms documented in the Economic Census suggests that the benefits of increased demand significantly exceeded the cost of higher wages. Finally, the increase in new enterprises is concentrated among small single-proprietor businesses—who are more likely to depend on local demand than to sell further away (Table F.6).

This increase in demand could also explain some of the income gains in our survey data. While we do not observe the breakdown of employment across agricultural and non-agricultural sectors, we do so for income and see that a little over half of the increase in labor income came from non-farm earnings (Table F.14). This is consistent with an increase in demand for labor outside agriculture (note that the economic census of non-agricultural employment could include jobs done by members of NREGS jobcard owning households).[57]

These results are consistent with growing evidence from other studies of meaningful local demand multipliers from increasing the incomes of the poor in developing countries. For instance, Egger et al. (2020) estimate a fiscal multiplier of 2.4 using a randomized community-level experiment of income transfers in rural Kenya, with this expansion concentrated in the non-agricultural sector. In India, Santangelo (2019) finds that positive (rainfall) shocks to agriculture raise rural wages, but also lead to an expansion of non-agricultural employment. This suggests that the positive effects of greater aggregate demand outweighed the potential

55. The increase in savings is larger (Rs. 1664) and marginally significant if we do not truncate the outcome (Table G.1). However, this would still only amount to 17.3% [-0.01%, 35%] of the increase in income.

56. For instance, the income elasticity of food expenditure in India was 0.75 and declining with income even in the 1980s (Subramanian and Deaton 1996). This figure is likely to be much lower 25 years later and in our setting, where 97% of households report eating 3 full meals a day every day in the past month.

57. Some of the increase in non-agricultural employment could possibly be driven by higher wages and imperfect competition *outside* agriculture. While we cannot test this conjecture directly, it may be less plausible than the demand channel given that most of the new enterprises were single-employee firms (Table F.6). Similarly, while it is likely second-order in terms of magnitude (for reasons discussed in Section 1.2), reduced leakage in old-age pensions enabled by Smartcards may have contributed to the demand channel.

negative effects of higher wages—a result that is directly relevant to our setting.

These empirical results also echo a well-established theoretical literature in development economics highlighting the possibility of positive feedback mechanisms from higher wages due to demand externalities. Such externalities, it is thought, can accelerate structural transformation and even potentially give rise to multiple equilibria (Rosenstein-Rodan 1943; Murphy, Shleifer, and Vishny 1989). Magruder (2013) finds that formal employment in non-traded industries rose after an increase in the minimum wage in Indonesia and interprets it similarly, arguing that the coordinated boost to local incomes generated aggregate demand externalities and facilitated a "big push" towards greater formal sector employment.[58]

The magnitude of the effects here suggest that similar mechanisms may be at play. The overall AdjTE of 3,307 additional workers per mandal is a nearly 50% increase relative to the control group mean (Table 5), and equivalent to 15.7% of the entire working-age population (29,600) of the average mandal. To put this figure in perspective, the share of the Indian workforce engaged in non-agricultural employment increased by roughly 8% from 2000 to 2010 (World Bank 2021). Thus, the effects we see within 3 years of the reform are nearly double the change seen in the preceding decade of endogenous structural change.

# 5 Conclusion

This paper contributes to understanding the impact of increasing the effective presence of public employment programs in developing countries, in the context of the largest such program in the world—India's NREGS. Relative to the existing literature, 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) ground-level measures of implementation quality: enabling us to interpret impacts as the results of demonstrable changes in *actual* presence of the program; (c) new outcome measures: including reservation wages, income, and market employment; with independent census data on the latter two; (d) a more thorough examination of plausible mechanisms of impact including productivity, imperfectly-competitive labor markets, and aggregate demand.

Overall, our results are consistent with the following broad narrative. Improving NREGS implementation raised its value as an outside option for the rural poor (as seen in higher reservation wages). This in turn put pressure on employers to raise market wages to attract

58. Higher wages might also promote structural transformation by increasing employer incentives to mechanize production, as for example Hornbeck and Naidu (2014) find for the historical US. There is some evidence of NREGS-driven increases in mechanization in India (Deininger, Nagarajan, and Singh 2016), but given estimated effects on farm profits here are negative this channel may not yet have materialized.

workers. Crucially for efficiency, this *raised* private employment—in part, arguably, because the wage increase was in a context of imperfectly-competitive labor markets. Positive employment effects appear to have amplified positive wage effects to generate large increases in the incomes of the rural poor. Since very little of this income was saved via financial intermediaries, it likely boosted local demand for goods, services, and assets, contributing to large increases in both the number of non-agricultural firms and employment in these firms.

These results directly contribute to the ongoing debate over the impacts of the NREGS and how much funding it should receive.[59] Debate has centered on whether the NREGS can have had a meaningful impact on rural incomes and poverty given that it accounts for only a small share of rural employment (4% across India in 2011). Skeptics such as Bhalla (2013) ask, "how can a small tail wag a very very large dog?" And even if the NREGS did indirectly raise rural wages, this effect could be offset by crowding-out of private sector employment (Murgai and Ravallion 2005). Our results show that the NREGS can indeed have large impacts on market wages, and in doing so can raise rather than reduce private-sector employment, leading to large net income gains.

One natural question is how our results on the effects of *improving* NREGS implementation speak to policymakers in other settings who are considering whether or not to *introduce* a public employment program from scratch. While the specific impacts will depend on context, program design, and especially (as our paper demonstrates) implementation quality, we see the main implication of our results as follows:

Many economists—including ourselves—were initially skeptical about the likely impact of NREGS on rural poverty. 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. Our findings have reversed these priors. In particular, our finding positive effects on wages, *employment*, and incomes, and finding evidence consistent with employer market power, suggest that programs like NREGS can not only reduce poverty, but also be efficiency enhancing.[60] Since governments often consider public employment programs as a policy response to high unemployment, they are likely to use them in conditions where employers have more market power than job seekers. In such settings, a public employment program may have positive effects not only on wages, but also on employment and income. Our results also suggest that such a boost to the wages and incomes of the poor may have positive demand multiplier effects on

59. While NREGS is a legislated right under an Act of Parliament, in practice, work availability is constrained by budgetary allocations. For instance, work availability fell sharply in 2016, following a budget cut: http://thewire.in/75795/mnrega-centre-funds-whatsapp/, accessed November 3, 2016.

60. In this sense, our results echo those of Banerjee, Gertler, and Ghatak (2002) who find that strengthening property rights (and bargaining power) of tenant farmers in West Bengal improved both equity and efficiency.

employment and broader economic activity, as also seen recently in Egger et al. (2020).

Our results also highlight political economy issues in the design and implementation of anti-poverty programs in developing countries. Landlords and employers 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, Francois, and Kotwal (2015) have argued that "a primary reason... for landlords to control governance is to *thwart* implementation of centrally mandated initiatives that would raise wages at the village level." Our distributional analysis shows that the reforms generated broad-based benefits, but also likely hurt a small but politically influential group of large landowners. This may help explain such landowners' documented opposition to NREGS (Anderson, Francois, and Kotwal 2015; Khera 2011).

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. Empirical work on corruption has made great strides in quantifying leakage as the difference between fiscal outlays and actual receipts by beneficiaries (e.g. Reinikka and Svensson 2004; Muralidharan et al. 2017) and studying the impacts of reforms on these measures (Olken 2007; Muralidharan, Niehaus, and Sukhtankar 2016). Yet the broader economic costs of corruption have been harder to detect. Our results suggest that weak NREGS implementation may hurt the poor much more through diluting its general equilibrium effects than through the diversion of wages per se. Consequently they also underscore the importance of building state capacity for better implementation of social programs in developing countries.

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Table 1: Earnings

| | Total | NREGA | Wage labor | Self employment | Misc. |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Adjusted TE | 9579** | 1295 | 7607*** | -769 | 2502 |
| ($\beta_T + 0.36 * \beta_N$) | (4539) | (1061) | (2720) | (3192) | (2474) |
| | {4548} | {1154} | {2968} | {3131} | {2488} |
| Main effect | 9030** | 1005* | 6804*** | 1123 | 872 |
| ($\beta_T$) | (3670) | (584) | (2130) | (2681) | (2018) |
| | {3483} | {619} | {2228} | {2602} | {1959} |
| Nbhd effect | 550 | 289 | 803 | -1892 | 1629 |
| ($0.36 * \beta_N$) | (2654) | (804) | (1099) | (1791) | (1699) |
| | {2081} | {827} | {1133} | {1650} | {1277} |
| Baseline | Yes | No | No | No | No |
| Control mean | 69,122.1 | 4,743.4 | 24,120.2 | 26,563.1 | 13,695.4 |
| Adjusted $R^2$ | .039 | .015 | .053 | .015 | .013 |
| Observations | 4,823 | 4,856 | 4,857 | 4,857 | 4,857 |

The unit of analysis is a household. All outcomes are in Rs. per year. "Total" sums all other categories; "Wage labor" includes agricultural and non-agricultural labor; "Self-employment" includes farm and non-farm business; "Misc." sums all other income (private and government employment earnings, pensions, gifts, and "other"). Estimation is as described in Section 2.3. Appendices J and G discuss recall and sensitivity to outliers. Standard errors in parentheses are clustered by mandal; those in brackets are spatial as in Conley (2008). Significance based on the former is denoted: $^{*}p < .10, ^{**} p < .05, ^{***} p < .01$.

Table 2: Employment and wages in June

| | Wage | | | Employment | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Reservation wage | Wage realization | Wage realization (weighted) | Days self-employed or not working | Days worked in NREGS | Days worked in private sector |
| Adjusted TE | 6.9** | 13*** | 10** | -2.4*** | 1.3** | 1.4* |
| ($\beta_T + 0.36 * \beta_N$) | (3.2) | (4.3) | (5) | (.79) | (.55) | (.8) |
| | {3.5} | {4.6} | {5.2} | {.81} | {.56} | {.78} |
| Main effect | 5.8** | 8.8** | 7.9* | -1.5** | .89* | .74 |
| ($\beta_T$) | (2.8) | (3.6) | (4.1) | (.59) | (.47) | (.57) |
| | {2.9} | {3.6} | {4.1} | {.6} | {.51} | {.57} |
| Nbhd effect | 1.1 | 4.3* | 2.5 | -.95** | .39 | .71* |
| ($0.36 * \beta_N$) | (1.7) | (2.4) | (3) | (.42) | (.27) | (.4) |
| | {1.7} | {2.6} | {3.1} | {.41} | {.24} | {.38} |
| Control mean | 97.2 | 127.9 | 128 | 17.3 | 4.5 | 7.9 |
| Adjusted $R^2$ | .054 | .076 | .058 | .073 | .076 | .020 |
| Observations | 12,677 | 7,016 | 6969 | 13,951 | 14,009 | 14,278 |

The unit of analysis is an adult. "Wage realization" is the average daily wage, in Rs. per day, received by adults who worked (for "weighted," we weight by days worked). "Reservation wage" is the wage at which an individual would have been willing to work for someone else. The outcome in Columns 4-6 is the number of days out of the past 30 spent in the respective occupations (including partial days). Estimation is as described in Section 2.3. Appendices J and G discuss recall and sensitivity to outliers in more detail. Standard errors in parentheses are clustered by mandal; those in brackets are spatial as in Conley (2008). Significance based on the former is denoted: $^{*}p < .10, ^{**} p < .05, ^{***} p < .01$.

Table 3: Prices

| | Consumer goods | | | Prices and rates of return | |
|---|---|---|---|---|---|
| | (1) Index: uniform goods | (2) Index: all goods | (3) Individual goods | (4) Logged own-land profits | (5) Logged value per acre |
| Adjusted TE | -.055 | .0059 | -.0003 | -.19** | -.06 |
| ($\beta_T + 0.36 * \beta_N$) | (.13) | (.045) | (.016) | (.08) | (.13) |
| | {.13} | {.051} | {.015} | {.076} | {.15} |
| Main effect | -.0072 | .0072 | -.0071 | -.09 | -.061 |
| ($\beta_T$) | (.079) | (.029) | (.011) | (.075) | (.11) |
| | {.082} | {.032} | {.011} | {.065} | {.11} |
| Nbhd effect | -.048 | -.0014 | .0068 | -.1** | .0018 |
| ($0.36 * \beta_N$) | (.057) | (.019) | (.0073) | (.042) | (.053) |
| | {.059} | {.023} | {.0075} | {.042} | {.059} |
| Item FE | No | No | Yes | No | No |
| Unit of analysis | Village | Village | Item x Household | Household | Household |
| Control mean | 11.1 | 10.7 | -3.1 | 10.0 | 11.7 |
| Adjusted $R^2$ | .982 | .998 | .951 | .261 | .173 |
| Observations | 58 | 58 | 17,651 | 2,487 | 3,053 |

The outcome in Columns 1 & 2 is the log of the village-level price indices constructed using Equation 3; Column 1 restricts the sample to goods purchased at least once in every village. The outcome in Column 3 is the log of the individual commodity price. "Own-land profits" is the log of the household's income from their owned land. "Value per acre" is the log value per acre of a household's landholdings. Estimation is as described in Section 2.3. Appendices J and G discuss recall and sensitivity to outliers in more detail. Standard errors in parentheses are clustered by mandal; those in brackets are spatial as in Conley (2008). Significance based on the former is denoted: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 4: Income categories (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) |
| Adjusted TE | -.028* | .025* | .0034 | -.026 |
| ($\beta_T + 0.36 * \beta_N$) | (.017) | (.014) | (.0069) | (.017) |
| | [.084] | [.03] | [.42] | |
| Main effect | -.032** | .024** | .0078 | -.031** |
| ($\beta_T$) | (.014) | (.011) | (.0055) | (.014) |
| | [.02] | [0] | [.29] | |
| Nbhd effect | .0038 | .0019 | -.0051 | .0053 |
| ($0.36 * \beta_N$) | (.0087) | (.0064) | (.0043) | (.009) |
| | [.4] | [0] | [.34] | |
| Control Mean | .8 | .1 | .0 | . |
| Adjusted $R^2$ | .016 | .016 | .030 | .013 |
| Observations | 1.8 M | 1.8 M | 1.8 M | 1.8 M |

The unit of analysis is a household. The outcome in Columns 1-3 is the probability of having a top earner in the indicated income bracket. Estimation is via logit with marginal effects reported (Columns 1-3) and ordered logit with marginal effects on the lowest income category reported (Column 4). Standard errors in parentheses are clustered by mandal. Significance is denoted: $^{*}p < .10,^{**} p < .05,^{***} p < .01$. p-values from randomization inference on 10,000 iterations are reported in square brackets.

Table 5: Non-agricultural enterprises and employees

| | All sectors | Livestock | Manufacturing and construction | Wholesale and retail | Other |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Number of employees* | | | | | |
| Adjusted TE | 3307** | 294 | 909* | 836 | 1268** |
| ($\beta_T + 0.36 * \beta_N$) | (1554) | (246) | (465) | (554) | (616) |
| | [.089] | [.19] | [.13] | [.15] | [.12] |
| Main effect | 2251** | 113 | 588* | 764* | 786* |
| ($\beta_T$) | (1101) | (212) | (313) | (398) | (435) |
| | [.1] | [.33] | [.14] | [.1] | [.17] |
| Nbhd effect | 1056 | 182 | 320 | 71 | 483 |
| ($0.36 * \beta_N$) | (826) | (191) | (280) | (317) | (339) |
| | [.2] | [.16] | [.22] | [.41] | [.2] |
| Control mean | 6796.7 | 1711.5 | 1439.9 | 1219.2 | 2426.1 |
| Adjusted $R^2$ | 0.165 | 0.518 | 0.164 | 0.115 | 0.122 |
| Observations | 157 | 157 | 157 | 157 | 157 |
| *Panel B: Number of enterprises* | | | | | |
| Adjusted TE | 1095* | 177 | 167 | 327 | 423** |
| ($\beta_T + 0.36 * \beta_N$) | (575) | (134) | (176) | (227) | (214) |
| | [.085] | [.18] | [.28] | [.13] | [.093] |
| Main effect | 856** | 62 | 221 | 311* | 262 |
| ($\beta_T$) | (427) | (126) | (141) | (165) | (163) |
| | [.078] | [.32] | [.14] | [.074] | [.14] |
| Nbhd effect | 239 | 115 | -54 | 16 | 162 |
| ($0.36 * \beta_N$) | (311) | (108) | (115) | (126) | (120) |
| | [.27] | [.14] | [.58] | [.43] | [.17] |
| Control mean | 3816.5 | 1127.3 | 754.1 | 739.3 | 1195.7 |
| Adjusted $R^2$ | 0.285 | 0.579 | 0.211 | 0.163 | 0.245 |
| Observations | 157 | 157 | 157 | 157 | 157 |

The unit of analysis is a mandal. Outcomes are the number of employees (Panel A) and number of firms (Panel B) reported in the respective categories in the Economic 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.

Table 6: Heterogeneous effects on days worked by land concentration

| | Raw HHI (full sample) | Raw HHI (above 1 acre) | Standardized (full sample) | Standardized (above 1 acre) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treatment | .46 | .45 | .6 | .6 |
| | (.57) | (.57) | (.55) | (.55) |
| | {.58} | {.58} | {.55} | {.56} |
| $H^*$ | -4.7** | -6.2* | -.56** | -.63* |
| | (2.1) | (3.2) | (.25) | (.33) |
| | {2.6} | {2.9} | {.3} | {.3} |
| Treatment × $H^*$ | 4.6** | 6.5* | .55** | .66* |
| | (2.3) | (3.4) | (.27) | (.34) |
| | {3} | {3.2} | {.35} | {.33} |
| Control Mean | 7.9 | 7.9 | 7.9 | 7.9 |
| Adjusted $R^2$ | .019 | .020 | .019 | .020 |
| Observations | 13,827 | 13,798 | 13,827 | 13,798 |

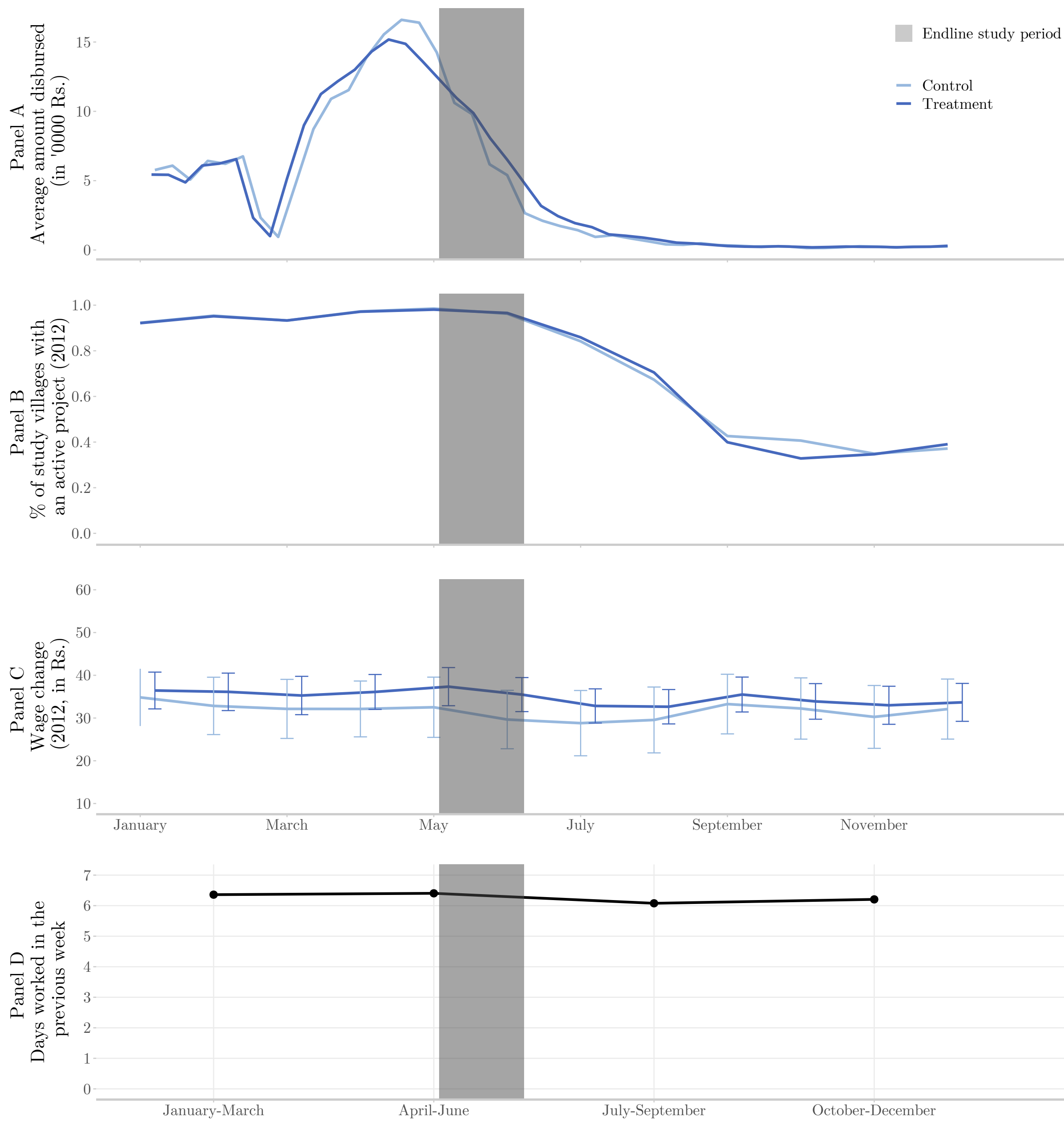
The unit of analysis is an adult. The outcome variable is the same as Column 5 of Table 2. "$H^*$" is the Herfindahl index of land ownership in the village, and each column represents a different measure of the index; for both the full sample and a restricted sample of those who own above 1 acre, both normalized (raw) and standardized separately for treatment and control areas. Estimation is as described in Section 2.3. Standard errors in parentheses are clustered by mandal; those in brackets are spatial as in Conley (2008). Significance based on the former is denoted: ${}^{*}p < .10, {}^{**} p < .05, {}^{***} p < .01$.

Table 7: Assets, liabilities, and expenditure

| | Annualized expenditure (Rs. per year) | | Total savings (Rs.) | Total loans (Rs.) | Owns land (%) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Adjusted TE | 389 | 18105 | 260 | 20400*** | .072** |
| ($\beta_T + 0.36 * \beta_N$) | (4676) | (13106) | (322) | (6403) | (.033) |
| | {4820} | {12360} | {370} | {6356} | {.033} |
| Main effect | -1028 | 16417* | 41 | 11237** | .056** |
| ($\beta_T$) | (3893) | (8866) | (279) | (4912) | (.025) |
| | {3692} | {9532} | {303} | {4656} | {.024} |
| Nbhd effect | 1416 | 1687 | 219 | 9163*** | .016 |
| ($0.36 * \beta_N$) | (2642) | (7599) | (157) | (3308) | (.018) |
| | {2646} | {6735} | {183} | {3441} | {.018} |
| Survey | NREGA | NSS | NREGA | NREGA | NREGA |
| Control mean | 85,030.7 | 58,779.1 | 2,966.1 | 68,107.7 | .6 |
| Adjusted $R^2$ | .014 | .080 | .018 | .013 | .031 |
| Observations | 4,827 | 222 | 4,808 | 4,840 | 4,836 |

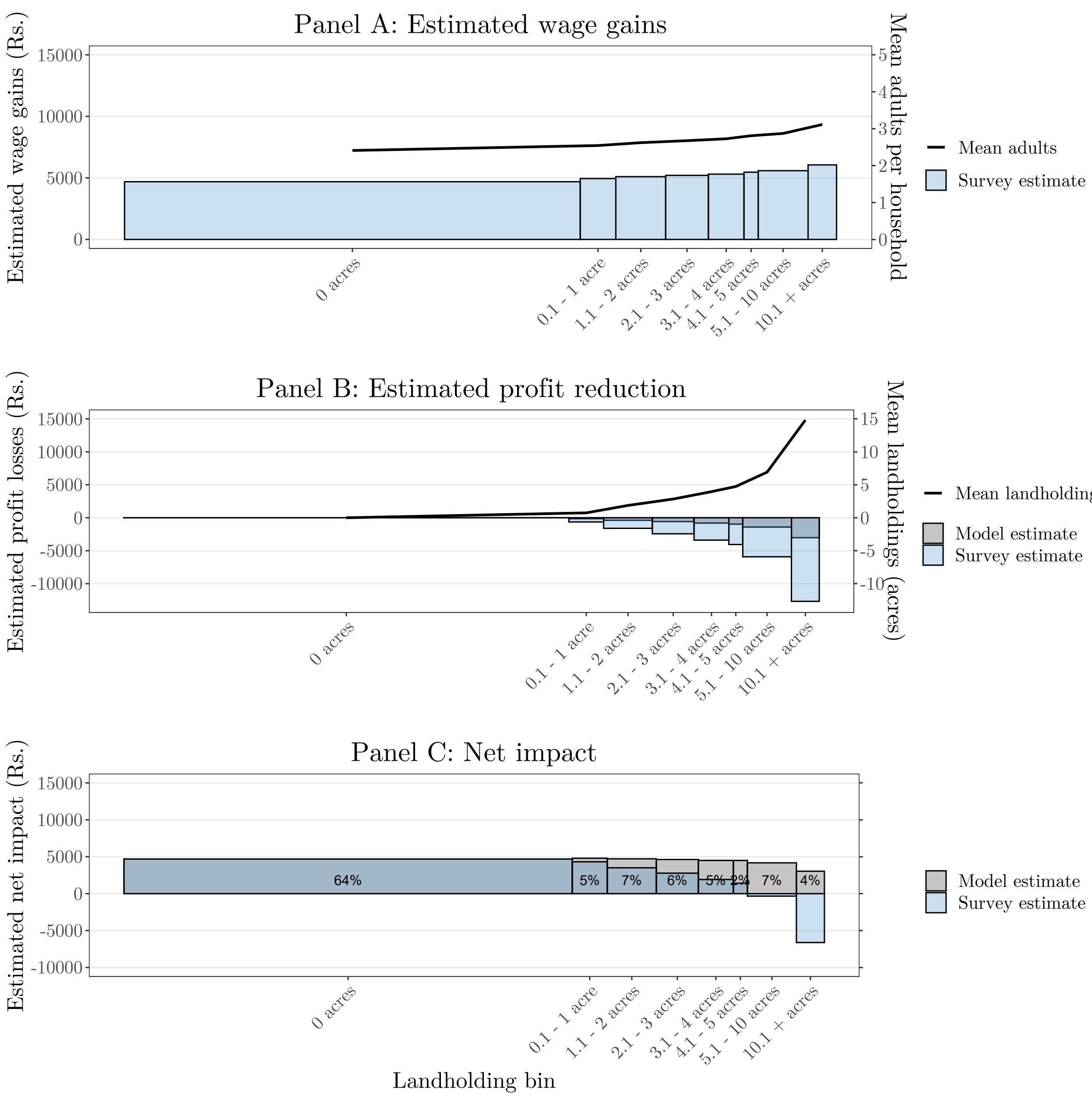
The unit of analysis is a household. "Total savings" is current cash savings. "Total loans" is the total outstanding principal of the household's five largest active loans. "Owns land (%)" is an indicator for whether a household owns any land. "Annualized Expenditure (Rs. per year)" is the household's estimated annual expenditure. Estimation is as described in Section 2.3. Appendices J and G discuss recall and sensitivity to outliers in more detail. Standard errors in parentheses are clustered by mandal; those in brackets are spatial as in Conley (2008). Significance based on the former is denoted: ${}^{*}p < .10, {}^{**} p < .05, {}^{***} p < .01$.

Figure 1: Seasonality in NREGS and labor market outcomes




Panel A (reproduced from Muralidharan, Niehaus, and Sukhtankar (2016)) shows official NREGS payments for all workers averaged at the village-week level for treatment and control areas. Panel B plots the proportion of study villages with at least one active NREGS project. 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. Panel C plots the average change in agricultural wages between baseline and endline. We plot the adjusted treatment effect on (changes in) agricultural wages using 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. The grey band denotes the endline study period, June 2012, on which our survey questions focus. Each of the first three panels are disaggregated by month and treatment status. Panel D captures seasonal (i.e. pooled three-month periods) variation in the average number of days worked either through wage employment or self-employment (excluding NREGA work) in the previous week using data pooled from the 66th (2009-2010) and 68th (2011-2012) rounds of the National Sample Survey (NSS).

Figure 2: Estimated wage and profit effects by landholding




Panel A uses SECC microdata to show the number of laborers per household for each discrete interval across the landholding distribution (black line, right-hand axis). The blue bars and left-hand axis show the estimated wage gains when we apply the treatment effect estimated in our survey data to the distribution of laborers. Panel B shows both the mean landholding size in acres for each bin plus two estimates of profit losses (derived from our survey estimates and model-based estimates). Panel 3 shows the net impact, calculated by summing the estimated wage gains with the two estimated profit losses. Percentages indicate the share of the population in each landholding bin. A full description of the methods used is in Appendix D.