Do Labor Market Interventions Incentivize Technology Adoption? Unexpected Effects of the World’s Largest Rural Poverty Program

Anil K. Bhargava

Abstract

The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) is the largest poverty program ever undertaken. It aims to augment the purchasing power of the rural poor by guaranteeing employment during droughts and slack agricultural production periods. Given the scale of such programs, they have the potential to generate additional impacts throughout the economy. This paper investigates whether higher unskilled agricultural wages attributed to this labor market intervention incentivize farm owners to adopt labor-saving agricultural technologies. Using a novel regression discontinuity design and new agricultural census data, I find 10–30 percentage point shifts away from labor-intensive technologies towards labor-saving ones, driven by India’s smallest farmers. Such unintended impacts have the potential to shape rural landscapes in unexpected ways. JEL: H53, J43, O13, O33, Q12

Keywords: Technology Adoption, Labor Markets, Poverty, India, NREGA

1 anilbhar@umich.edu. This research received financial support from the International Food Policy Research Institute and benefited from comments by Travis Lybbert, Michael Carter, Hilary Hoynes, two anonymous reviewers, and audiences at PacDev (UCLA 2014), NEUDC (Harvard 2013), AAEA (Washington DC 2013), the University of San Francisco, and PEGNet. Any errors are my own.
1. Introduction

Landless agricultural laborers and small farmers constitute much of the world’s poor. In many countries, as populations continue to grow and more people enter an expanding rural labor force, they must eke out a living in the rural sector or add to the growing pressure on urban areas. Meanwhile, rural work can be scarce and wages for the poor are often persistently below official subsistence levels.

Rural development programs and social safety nets can help address these issues. In many cases, they provide direct benefits to households in the form of income transfers, education and health, financial services, agricultural inputs, rural infrastructure, and employment. These can be crucial in generating household resources needed to stay out of poverty traps and address social priorities. But, in addition to measuring success in these intended outcomes, it is equally important to examine potential unexpected impacts of such policies.

Spillovers and general equilibrium effects are as likely to affect short- and long-run rural poverty as direct effects of a program. In some cases, these effects may persist longer into the future in shaping the rural economy, once the program has concluded. Researchers have begun to shift to longer view theoretical and empirical assessments of development programs. Thome et al. [1] have modeled economy-wide spillovers of targeted development programs directly into randomized control trial evaluations to estimate social cash transfer program impacts on agricultural outcomes in Sub-Saharan Africa. Mobarak and Rosenzweig [2] use a general equilibrium model to estimate effects of an index insurance program on labor market outcomes in India. Hornbeck and Naidu [3] found far-reaching unintended impacts of worker outmigration in the United States South on agricultural technology adoption that replaces unskilled labor. As is the case with many of these extended view studies, results are often surprising as they go beyond a simple confirmation of whether intended outcomes were achieved. This latter study, for example, shows that the welfare of landless laborers often suffers disproportionately under status quo agricultural insurance programs, as well as certain index insurance schemes.

This paper adds to such assessments of unintended program impacts by showing how a large scale rural employment generation program can lead to the adoption of labor-saving agricultural technology. I use evidence from India’s Mahatma Gandhi
National Rural Employment Guarantee Act (NREGA). Many of the studies on this topic have documented unskilled agricultural wage increases of 3–8 percent due to the program [4, 5, 6]. This increase in public sector jobs and rural wages have generally lead to higher overall rural labor force participation rates. Findings are unclear, however, on whether there is a crowding out of private-sector jobs, such as those in agriculture.

Since many farm owners depend on the unskilled labor targeted by NREGA, a change in worker wages is likely to affect the input price ratio faced by farmers and increase the use of technology that replaces unskilled workers. Farm owners in India have demonstrated uncertainty season to season about whether they can continue to hire workers for the same real wages they paid before NREGA. Laborers acknowledge receipt of higher wages after NREGA for some farm tasks and nonexistence of previously available work for others. Together, these reports suggest that labor-saving technology adoption may be occurring due to NREGA’s impact on agricultural wages or expectations of wages and favors some agricultural production tasks over others.

I model this by incorporating rising unskilled worker opportunity costs into a farm size threshold model of technology adoption for a variety of technologies. The model predicts that unskilled agricultural wage increases will cause farmers, previously highly-dependent on labor-intensive farm operations, to adopt labor-saving technologies. In other words, NREGA lowers the farm size threshold for certain agricultural technologies. For example, a farmer may replace unskilled workers, previously hired to hand hoe a field, with an animal-drawn wooden plow or power tiller, which requires fewer, more highly-skilled workers. However, it does not incentivize farmers to adopt technologies that do not replace unskilled agricultural labor: the farmer faces unchanged incentives in replacing, say, a power thresher with a combine harvester, both of which rely little on unskilled labor.

Empirically, I test the predictions of the model using a regression discontinuity (RD) design that is novel to the NREGA literature. Whereas most studies use district-level panel data from India’s National Sample Survey to assess impacts of the program, only one study has used a state-level RD to assess program impacts [7]. I use a national RD design that takes advantage of the progressive phased roll-
out of the program to the poorest districts of the country first. India’s Planning Commission ranked 447 districts from poorest to richest prior to the program and implemented NREGA in the first 200 of these during Phase I from 2006–2007. The next 130 were included in Phase II starting in mid-2007, and the rest of the country became eligible in the mid-2008. I use districts on both sides of this arbitrary Phase I cutoff as treatment and control groups and attribute differences in technology adoption levels to NREGA implementation.

The main data comes from the Indian Ministry of Rural Development’s Agricultural Census Input Survey (ACIS). ACIS data was last recorded in 2007 and allows for estimation of program impacts among relatively poorer districts at the Phase I cutoff. In contrast, the more-widely used National Sample Survey (NSS) data spans a period ending in 2008, which restricts analysis to wealthier districts around the Phase II cutoff. I compile data for over 50 agricultural technologies recorded in the ACIS dataset and then compare my results to difference-in-differences estimates using an additional panel of data from the 2004 India Human Development Survey (IHDS).

Results show an overall 10 percentage point increase in the use of labor-saving technologies for farms in NREGA districts. The smallest farmers see impacts of up to 18.5 percentage points, and some technologies are used 30 points more. Most movement occurs within hand- and animal-operated technology groups, suggesting a decrease in the farm size threshold of adoption for low-intensity labor-saving agricultural technologies and a surprisingly high elasticity of small farmer technology demand with respect to unskilled wages. This is in part aided by prevalent custom-hire technology markets in many parts of the country, which I document here.

I also address how infrastructure and income effects of the program could lead to credit and expenditure channels that may potentially affect technology use. I find that credit for agricultural purposes is not affected by NREGA and argue that infrastructure impacts are less likely to have an immediate impact on technology adoption of Phase I NREGA districts. Moreover, the seasonality of labor-saving technology adoption coincides with the targeted seasonality of NREGA employment during slack agricultural production periods. This provides further evidence
on NREGA’s impact on technology adoption through labor markets and increased agricultural wages. I discuss this more later in the paper.

In the short run, workers are likely better off as they now have multiple rural labor options and a bidding up of the wages between them. Although, empirically, agricultural labor supply effects of this program are unclear in the literature, I conduct a simple exercise using historically-documented elasticities of labor supply to find that NREGA may be causing modest average savings to farmers in labor hired and associated expenditures. Farmers are likely to be the only ones who suffer in the short-run as their new technology and input bundle has likely increased in cost, assuming imperfect degrees of substitutability between capital and labor.

In the long run, farmers and workers can both gain as a result of these intended and unintended effects of NREGA, especially if increases in farm productivity combine with newly-created NREGA infrastructure to increase agricultural production and demand for labor. However, skill levels of workers would need to rise to match this increased demand for labor at a higher value marginal product. In this case, agricultural labor levels could not only return to pre-NREGA quantities but surpass them, leading to higher wages and increased employment. Farm owners would recoup any welfare loss stemming from these higher wages with increased output and productivity. On the other hand, if poor-quality infrastructure and low levels of skill development prevail, both farmers and laborers may be worse off in a post-NREGA era, as labor-saving technology is adopted and neither public works nor agricultural jobs are available.2 Policymakers may want to consider these technology adoption farmer incentives and the role of unskilled labor vis-à-vis their rural development priorities as they move forward with new components of NREGA and other national poverty programs that complement it.

This rest of this paper is structured as follows. Section 2 provides background on NREGA and reviews the literature related to the employment guarantee and labor and technology markets. Section 3 develops the farm size threshold model of technology adoption that uses increases in the opportunity cost of agricultural labor

---

2 Even in this worst-case scenario, the prevalence of custom-hire technology markets increases the chance farmers disadopt technology in response to potentially deflating agricultural wage levels. Technology use and markets could then conceivably return to pre-NREGA equilibria.
to generate adoption of labor-saving agricultural technology. Section 4 discusses potential empirical strategies, including panel methods and RD. Sections 5 and 6 detail the data and results, respectively, while section 7 concludes.

2. Background

This section starts by describing the motivation and goals of NREGA. The relevant literature is then divided into two broad categories for discussion. The first covers agricultural wage responses to an employment guarantee, including a review of theoretical studies on the 1970s state-level employment guarantee in Maharashtra, as well as recent empirical studies on NREGA’s agricultural wage impacts. The second summarizes recent trends on determinants of technology adoption, focusing particularly on studies that pertain to labor-saving technology.

2.1. National Rural Employment Guarantee Act

NREGA guarantees any rural Indian household up to 100 days of employment per year for labor-intensive public works projects within 15 kilometers of one’s residence and paid at state-level minimum wages, typically $2 USD per day. Half of all works are water-related, and 20 percent focus on rural connectivity. Nearly half of all workers have been women—far surpassing the 25 percent quota set by the government at the outset of the program. Through this program, NREGA aims to directly impact India’s rural economy in at least three ways.

The first is to enhance the purchasing power of poor laborers during slack agricultural production periods and droughts. Drèze and Sen [8] closely study a government response to the severe drought in Maharashtra from 1970 to 1973, known as the Employment Guarantee Scheme (EGS), and conclude that diminishing purchasing power by the poor in the face of famine was of larger concern at the time than the limitations in food availability due to market imperfections. In a review of India’s history of famines, Drèze cites a 19th-century report that notes “the first effect of drought is to diminish greatly, and at last to stop, all field labor, and to throw out of employment the great mass of people who live on the wages of such labor” (1991, 17). He continues, “even today it is clear that the high level of market integration in India would be of little consolation for agricultural laborers if government intervention did not also protect their market command over food during lean years” (1991, 25). Thus, NREGA provides a work guarantee aimed primarily at
laborers who either lose their seasonal work in bad years or cannot make ends meet during slack agricultural production periods when work is low. NREGA guarantees officially-stated minimum wages to ensure that the poor maintain their purchasing power in bad years.

Second, NREGA tries to incorporate methods from the Maharashtra EGS to deal with targeting and selection issues of the program. The EGS was able to target those most vulnerable to drought-related income collapses by locating offices in rural areas and requiring regular worker attendance. This way officials could be sure that those with the lowest opportunity costs would select themselves into the treatment, ensuring the objectives of both getting aid to those who were at highest risk of starvation and avoiding elite capture. The structure of NREGA reflects the successes of the Maharashtra EGS in the types of labor-intensive works undertaken and the method of implementing the program. Liu and Barrett [9] evaluate participation profiles and household expenditures from NSS data and find that NREGA is well-targeted to its intended poor beneficiaries, while Jha et al. [10] show some variation across states in how size of landholdings predicts participation.

Finally, NREGA aims to increase local village infrastructure as a public good. Half of the projects relate to public water availability for agriculture and another 20% target rural connectivity, such as roads. These are intended to be a means for landless laborers to more easily enter into farming in the medium and long runs. With workers employed to physically develop their own villages through NREGA, the idea is to pave the way for economic growth and poverty reduction at home.

2.2. Employment Guarantee and Agricultural Labor Markets
After the Maharashtra EGS of the 1970s, several studies emerged on theoretical impacts of employment guarantees on labor markets in India. Narayana et al. [11] stylize the Indian agricultural labor market by separating demand into peak and lean seasons. Because both the EGS and NREGA are intended for lean seasons or droughts, there may not necessarily be a theoretical effect of the program on agricultural employment or wages since there can be sufficient days in the lean season to meet both agricultural and public labor demands. In this case, workers would gain from public works employment at high public wages, compared to low-wage or nonexistent temporal agricultural work in such low-production seasons.
Osmani [12] argues that farm workers collectively determine the equilibrium wage via repeated wage-setting games and implicit cooperation. The cooperative wage must be at least as much as one would make outside of agriculture but not so high that a worker would be willing to incur the penalty for a chance at receiving it. An employment guarantee with relatively high wages would serve as a boost in this opportunity income, thus shifting up the wage interval and, potentially, the cooperative wage within it. Basu [13] goes further to develop a model that predicts impacts on both labor and output markets. He incorporates labor demand seasonality, and the theoretical results show that institutionalizing a public works wage will result in a decrease in agricultural labor and increase in the casual wage rate. This study briefly discusses how technological improvement can increase the casual wage rate, but does not develop this hypothesis further. These points will be important to keep in mind during discussion of the long-run impacts of technology adoption on unskilled labor and agricultural output at the end of this paper.

Empirically, most analyses of NREGA’s effects in the labor market find positive impacts on agricultural wages. Most prominently, Imbert and Papp [4] find both a 5.5 percent increase in agricultural wages and a crowding out of private-sector employment. Berg et al. [5] estimate a 3 percent increase in agricultural wages with a 6–11 month lag for the impact to materialize on farms using casual labor. Azam [6] finds an 8 percent increase in female agricultural wages but only 1 percent for men. All these studies use difference-in-differences techniques to estimate wage impacts, finding effects on private-sector labor levels only during the dry season and gender neutrality in the impact distribution. Zimmermann [7] uses a state-level regression discontinuity design and finds private sector wage impacts only for women. Shah [14] shows a 6.5 percent increase in agricultural wages and demonstrates that a one standard deviation increase in infrastructure development due to NREGA led to a 30 percent reduction in wage sensitivity to production shocks.

NREGA papers focusing on topics besides labor market effects broadly fall into three categories: income, consumption, and expenditure; human capital and nutrition; and political economy and implementation. The latter has been discussed above briefly with regards to targeting and elite capture, but this group of research also includes numerous discussions and analyses of accountability and inclusive-
ness. Dutta et al. [15] find that significant unmet demand occurs in most states, especially the poorer ones, while Ambasta et al. [16] highlight pre-existing bureaucratic inefficiencies and institutional shortcomings that shaped the early years of NREGA implementation.

Deininger and Liu [17] use a three-panel dataset for over 4,000 households to show that NREGA leads to significant increases in consumption, such as protein and energy intake in the short run and nonfinancial assets in the medium run. These are most pronounced for the casual laborers that are the intended beneficiaries of the program. Drèze and Khera [18] stress that NREGA is a lifeline for the poor and generates empowerment from the “dignified” work that some NREGA laborers had never experienced. This research focuses on the surprisingly high rates of women participants across the country and may help partially explain modest second-order impacts of NREGA on education and nutrition expenditures [19, 20].

2.3. Labor Markets and Technology Adoption

The focus of technology adoption literature has changed rapidly in recent years. Since the green revolution of the 1970s, it has moved away from labor markets, macroeconomic policy, and farm characteristics as primary determinants of individual adoption to the quality of local markets, behavioral processes, and social networks. I discuss some surveys below that trace this literature over the recent past, highlighting in particular studies pertaining to labor-saving technologies.

One of the earlier reviews of technology adoption models discusses the role of land tenure, farm size, uncertainty, and information on adoption [21]. The authors caution against what they see as a trend in the literature of “nonexistence of government policies in most adoption models” (p. 288), which can strongly affect relative input and output prices and, thereby, technology choices. This warning harkens back to an earlier era where research on technology choices focused more on the government policies that shape incentives relative to other factors.

Besley and Case [22] center on empirical results from technology adoption studies. In comparing and contrasting time-series models, they suggest such models are too broad in nature and, thus, less useful than other methods for determining individual adoption practices. At the same time, however, the authors argue that cross-sectional studies ignore adoption dynamics and focus only on correlation between
farmer characteristics and adoption. Ultimately, they suggest a more balanced approach and highlight dynamic optimization methods that model state dependence between periods and testing for adoption practices using panel data.

Foster and Rosenzweig [23] highlight in their more recent work several additional adoption constraints, including credit, insurance, information, economies of scale, risk preferences, and behavioral processes. They conclude with an acknowledgment of the gap in the literature on documenting actual returns to technologies that are seemingly underutilized. While this study of NREGA’s impact on technology adoption cannot generate estimates of financial returns to technology use, it helps fill part of this gap in the literature by showing how a ripple effect of NREGA on agricultural wages can alter farmer incentives just enough to push farmers over the adoption threshold, holding markets and institutions constant.

Most of the studies captured within these surveys do not explicitly address the role of labor availability in technology adoption decisions. Part of the reason may be the challenging empirical task of identification. The most recent studies that thoroughly discuss the role of this specific factor date back several decades, again around the time of the Green Revolution. Hicks and Johnson [24] and Harriss [25] examine the effect of high and low rural labor supplies, respectively, on the adoption of labor-intensive technologies. Adoption of such technologies in the rice fields of colonial Taiwan is cited as a reason for rapid population and labor force growth in that area. However, lessons from these studies have not been extrapolated to the case of labor-saving technology.

Feder et al. [21] cite some empirical evidence demonstrating that uncertainty in the availability of labor can lead to the adoption of labor-saving technologies, and Spencer and Byerlee [26] examine technical change and labor use in a farming area of Sierra Leone characterized by large quantities of land and small amounts of labor. The latter argue that labor supply constraints may be overcome by adoption of mechanical production techniques in rice-growing areas. But these geographical areas feature a land-labor ratio that is the opposite of South Asia, with very few available workers per hectare of land.

A recent study by Hornbeck and Naidu [3] found evidence of substitution of unskilled labor with agricultural technology in the US South during the early 20th
century due to outmigration of workers and a consequent rise in average labor costs. This is potentially quite relevant to India and NREGA. As with Hicks and Johnson [24], it is a pre-industrialization study of the agricultural sector in the US and potentially has many similarities with modern-day India.

In summary, the role of labor availability was an important topic in earlier studies of technology adoption, but this discussion has moved away from labor towards other influences. The empirical work has followed and abstracted from more difficult to measure changes in informal labor markets. The present research aims to fill this gap and complement existing research by re-examining and re-modeling the role of labor availability in technology adoption.

3. Model

The conceptual framework illustrates how a rural works program that raises agricultural wages can impact farmer decisions on technology. The decision to adopt is captured in a technology-specific minimum farm size at which adoption begins to be profitable. The model includes a number of determinants predicting this farm size, while also illustrating the distribution of potential impacts of NREGA across both farmer and technology types. As discussed earlier, NREGA can have an overall impact on labor-saving technology adoption and very specific impacts on technologies directly related to the agricultural tasks most affected by the program.

I adapt the farm size threshold model of Sunding and Zilberman [27] to isolate one particular channel through which agricultural technology adoption can occur: unskilled agricultural wages. Though this model was originally intended to describe technology diffusion over time, it can also capture farmer heterogeneity of technology adoption at a given point in time. Equation (1) states that adoption will take place on farms above a certain cutoff size, $H^c_j$, depending on fixed costs, $F_j$, and the difference in profit, $\Delta \pi_j$, between a new technology $j$ and its incumbent:

$$H^c_j = \frac{F_j}{\Delta \pi_j}.$$  

The relationship between the cutoff size and technology $j$ is illustrated in a stylized depiction in Figure 1. The pre-NREGA farm size threshold curve increases in $F$, keeping $\Delta \pi$ constant for all technologies $j$ along the x-axis. Here, fixed
costs represent information and learning that increase with the complexity of the technology, as other types of costs are often small or negligible when custom hire markets are prevalent. Thus, $F$ starts out very small (near the origin) for very labor-intensive technologies, such as hand-transplanted rice and hand-operated implements, and then increases with more complex technologies, such as animal- and machine-operated implements. Farm sizes along the $y$-axis are broken into acre groupings following India’s official categorization.

I explicitly incorporate the opportunity cost of unskilled agricultural labor, $w^A$, into this equation and expand:

$$H_j^c = F_j / [\pi_j^1(p, Q, w^A, L, r, K) - \pi_j^0(p, Q, w^A, L, r, K)].$$  \hspace{2cm} (2)

Profits now depend explicitly on input and output levels and prices, including the variable, $w^A$, that is primarily being affected by NREGA. This captures the main factor altering $\Delta \pi_j$, where $\pi_j^1$ is farmer profit using technology $j$ and $\pi_j^0$ is profit under the incumbent technology associated with technology $j$.

Figure 1: Theoretical change in technology adoption due to NREGA: a stylized depiction of NREGA’s impact on adoption using the farm size threshold model for technologies ordered on the x-axis from most labor-intensive to most labor-saving. Source: Author.
As an example, consider \( j = \text{animal plow} \). In this case, \( \pi_1^j \) is farm profit when adopting an animal-draw wooden plow during the land preparation phase of production, while \( \pi_0^j \) is the profit using several workers operating hand hoes, wheel hoes, and blade hoes. Since profits under these labor-intensive technologies are highly dependent on unskilled agricultural wages, an increase in \( w^A \) will have a relatively large impact on \( \pi_0^j \) compared to \( \pi_1^j \). This change in \( \Delta \pi_j \) increases the denominator of equation (2) and reduces the minimum farm size needed to adopt an animal plow while remaining profitable. For all such technologies where \( \pi_0^j \) decreases relative to \( \pi_1^j \) as a result of NREGA, the curve in Figure 1 falls. Similarly, NREGA will increase the farm size threshold for the opposite case, or labor-intensive technologies.

It is important to note that this model does not predict NREGA will increase adoption of technologies that do not directly replace unskilled labor. In other words, switching from an animal-drawn wooden plow to a tractor-drawn plow will have little to no impact on \( \Delta \pi_j \), as \( j \) in this case represents a tractor-drawn plow and the incumbent technology equals the animal-drawn plow. Since neither technology depends on unskilled wages, \( w^A \), there will be no change in \( \Delta \pi_j \). Thus, the largest changes in \( \Delta \pi_j \) (and, therefore, on \( H_j^c \)) will occur for labor-saving technologies closer to the center of the x-axis in Figure 1, although in some cases farmers may leap over such intermediary technologies and directly replace labor with higher technology machines.

One benefit of this threshold model in which farm size is the cutoff for adoption is that it is flexible enough to describe both large and small farm areas, an important variable in the Indian context, where the vast majority of farms are small and technology adoption studies are often done in a large farm context only. However, one empirical challenge with this model is the inability to precisely know the incumbent technologies associated with each farmer’s technology choice. While it is not always simple to identify incumbent technologies or impose specific choices between technologies for all farmers, these can be generalized depending on observable technologies being used on farms and evidence from previous studies on technology availability and farmer choices.

Binswanger and Ruttan [28] and Pingali et al. [29], for example, shed some light on this in the Indian context. They highlight the complexity of identifying technol-
ogy incumbents in the case of machine-powered implements, such as tractor-drawn seeders and levelers, that may achieve intermediate products and yields that are unattainable by labor, such as deeper tillage or higher precision. In this case, there is not a clear substitution between the two production methods. Threshing is generally not mechanized where wages are low and harvested volumes are small, and the authors report that weeding, interculture, and harvesting continue to be done by hand in land-scarce economies where non agricultural demand for labor is low. Such studies, though several decades old, show that while some technology contexts in India do not lead to an easy fit within the simple binary choice model presented here, the roles of labor demand and wages play a crucial role in shaping these decisions, as type and availability of technologies evolve over time. NREGA is likely to be a major factor in this setting.

4. Empirical Strategy

There are several approaches one could use to estimate NREGA’s impact on technology adoption. To understand the advantages and disadvantages of each, it is important to know more about the implementation of the program.

NREGA was implemented by district throughout the country over three phases. The Indian Planning Commission’s Backwardness Index (BI) ranked the 447 poorest districts in India using mid-1990s data on wages, productivity, and scheduled castes/scheduled tribe populations. The first 200 districts in the index make up Phase I and were eligible for NREGA funds in 2006. The next 130 were part of Phase II and began implementing the program a year and half later. By mid-2008, the remaining rural districts in the country became eligible. The main data used in this study was collected just before the start of Phase II. Figure 2 organizes some of these key dates into a timeline.
I start with a discussion of ordinary least squares (OLS) in generating a statistical relationship between NREGA and technology adoption. Problems with endogeneity of adoption decisions with early NREGA eligibility lead to exploration of time series and panel data methods. Nearly all studies on wage impacts have relied on difference-in-differences (DD) and panel methods to identify causal impacts, requiring the ability to identify and control for trends that may vary between treatment and control groups due to the nonrandom assignment of the program.

I estimate impacts using OLS and panel methods but ultimately argue for use of regression discontinuity design (RD) because of its ability to take advantage the progressive rollout of NREGA from poorest to richest district. It is also preferable because the pseudo-randomization of the treatment around the phased cutoff districts means only on a cross-section of data is needed to estimate impacts. Finally, RD does not need to rely on common trends assumptions between treatment and control groups over time nor does it require full non-manipulabilitly of treatment. Because this is the RD approach is novel to the NREGA literature, I discuss this method in detail below after presenting the OLS and DD approaches.

4.1. Ordinary Least Squares

In order to obtain a baseline estimate relating NREGA to technology adoption, I first consider a pooled OLS model with district-level controls,

$$TA_{it} = \alpha + \beta NREGA_{it} + \gamma X_{it} + \epsilon_{it},$$

where $TA$ is the percentage of farms in district $i$ using any labor-saving technology in year $t$, $NREGA$ is a binary indicator of whether district $i$ was eligible for NREGA in year $t$, and $X$ is a vector of district-level controls. This model will only
capture the effect of the NREGA program on technology adoption in district \( i \) if the expected value of the error term is zero, or \( E(\varepsilon_{it} | X_{it}) = 0 \). This is unlikely to be the case if districts more likely to adopt technology are also less likely to be poor and, therefore, less likely to be eligible for early phases of NREGA. That is, Phase I NREGA eligibility may be correlated with unobservable variables that also jointly determine technology adoption levels.

Without being able to rule out such endogeneity, OLS estimates of NREGA’s impacts will likely be biased downwards, since factors in the error term that affect adoption decisions, such as individual ability, may be negatively correlated with early-phase NREGA eligibility. Nevertheless, it is helpful to generate this estimate as a baseline to compare to those of other methods. To address omitted variable bias, I next consider difference-in-differences and panel methods.

4.2. Difference-in-Differences and Panel Fixed Effects

The difference-in-differences approach compares districts that participated in the first phase of NREGA (the treatment) to those that did not (the control) before and after Phase I of the program. The econometric specification is

\[
TA_{it} = \alpha + \beta NREGA_{it} + \gamma post_{it} + \delta NREGA_{it}post_{it} + \varepsilon_{it},
\]  

(4)

where \( TA \) is again the percentage of farms using labor-saving technology in district \( i \) and year \( t \), \( NREGA \) is a dummy variable equaling one if district \( i \) has implemented NREGA in year \( t \), and \( post \) is a dummy variable taking on the value of one for observations recorded after the first phase of the program.

With panel data, equation (4) can include district fixed effects to become

\[
TA_{it} = \beta_i + \gamma + \delta NREGA_{it}post_{it} + \varepsilon_{it},
\]  

(5)

where now \( \beta \) is the district fixed effect, \( \gamma \) is the time fixed effect, and the main coefficient of interest is \( \delta \), or the within estimator, which gives the treatment effect of NREGA on technology adoption net of an assumed common time trend and time-invariant district characteristics.

The estimate of \( \delta \) from equation (5) addresses some endogeneity concerns associated with OLS and pooled cross-sectional difference-in-differences specifications,
since selection into NREGA is biased. The panel fixed effects approach controls for unobservable time-invariant characteristics that may affect technology adoption of the 200 poorest districts in India differently than other districts. However, it still cannot control for any time-varying characteristics that may affect the two groups differently.

Previous NREGA studies have explored many potential common trends issues. These explorations include placebo tests, cubic and quartic time trends, and including additional controls. This paper estimates a naive fixed effects model that assumes common trends in order to compare these results to those of regression discontinuity. This acts as a validity check on accuracy of typically-used panel methods and associated common trends assumptions. If the fixed effects model above generates estimates similar to those of RD, additional controls may not be necessary when performing fixed effects estimation of Phase I impacts.³

4.3. Regression Discontinuity Design

The main advantage of an RD approach is that it does not require exogeneity of the treatment variable with respect to the outcome. It solves the identification challenge by relying on local random assignment, that is, that districts around a treatment threshold are the same on average in all characteristics except for the treatment.

The methodology follows Lee and Lemieux [31], who argue that “in many contexts the RD design may have more in common with randomized experiments (or circumstances when an instrument is truly randomized)—in terms of their ‘internal validity’ and how to implement them in practice—than with regression control or matching methods, instrumental variables, or panel data approaches” (p. 292).

In the case of NREGA, district rank in the neighborhood of the Phase I cutoff acts as a randomized variable that is highly correlated with treatment but not the outcome of interest. The RD equation takes the form

\[ TA_i = \alpha + \beta NREGA_i + \gamma \text{rank}_i + \delta \text{rank}^2_i + \eta NREGA_i \text{rank}_i + \lambda NREGA_i \text{rank}^2_i + \varepsilon_i, \quad (6) \]

where the dependent variable is the technology adoption rate in district \( i \) after NREGA has been implemented in Phase I, \( \alpha = TA_0 \) is the technology adoption

³ Since many NREGA papers estimate Phase II impacts, this validity check around the Phase I cutoff may not necessarily apply.
rate in the absence of the program, $\beta = TA_1 - TA_0$ is the treatment effect of interest, and rank is what determines the cutoffs for each phase based on the BI (the running or forcing variable).

The interaction terms in equation (6) allow the function to differ on either side of the Phase I cutoff, while the squared terms allow a flexible form to be used instead of imposing linearity. Use of RD usually requires that either observations closest to the cutoff be weighted more heavily or that the window of observations be restricted to the districts that make the most natural treatment and control groups. Here I will combine these methods, weighting observations away from the cutoff using a triangle kernel and then experimenting with several windows around the threshold in which to estimate coefficients.

Although RD does not require that the variation in the treatment variable be exogenous to the outcome of interest, it is important that the running variable be non-manipulable by the treatment beneficiaries. Since the Planning Commission uses data from the 1990s to determine a district’s rank in the BI, it would be difficult for a district to change its rank in response to the program. However, compliance with treatment assignment stemming from BI rank is not completely one-to-one, suggesting that something other than national rank is partially determining NREGA treatment. I use a fuzzy RD design that only requires treatment not be fully manipulable and discuss potential explanations of imperfect compliance in Section 6.

Use of a sharp RD design would require that participation in the program be solely determinant on the running variable, or $NREGA_i = f(rank_i)$. When this is not the case, use of the fuzzy RD is more appropriate. I define the fuzzy RD approach by the set of equations

$$TA_i = \alpha + \beta NREGA_i + \gamma rank_i + \delta rank_i^2 + \eta NREGA_i rank_i + \lambda NREGA_i rank_i^2 + \varepsilon_i, \quad (7)$$

$$NREGA_i = \xi + \rho \mathbb{1}[rank_i \leq c] + \phi rank_i + \chi rank_i^2 + \psi NREGA_i rank_i + \omega NREGA_i rank_i^2 + \nu_i, \quad (8)$$

where $c$ is the cutoff value officially representing NREGA treatment. In reduced form, these two equations yield

$$TA_i = \alpha_r + \beta_r NREGA_i + \gamma_r rank_i + \delta_r rank_i^2 + \eta_r NREGA_i rank_i + \lambda_r NREGA_i rank_i^2 + \varepsilon_{ir}, \quad (9)$$

where the true treatment effect is $\beta = \frac{\beta_r}{\rho}$, or the reduced form coefficient divided
by the probability of being treated at the threshold when ranked below the official
cutoff. In Section 6, I discuss each of these components and the overall treatment
effect for various windows around the threshold.

5. Data

The data for this study comes primarily from the Ministry of Rural Development’s
Agricultural Census Input Survey (ACIS). ACIS data exists for the years 1997, 2002
and 2007, though some survey questions, such as those on agricultural technology
use, are not comparable between the years.

The survey process is conducted over three stages. In the first stage, the number
of farm holdings in each district is recorded and tabulated by size, gender, and
social group. Then there is random selection of tehsils—blocks or administrative
units at the sub-district level that consist of several villages. Each tehsil had 20
percent of its villages randomly selected in the third stage (100 percent of villages
for small states), and the survey is conducted at this time for farms on the final list.
Enumerators ensure that each village have at least four farms within each of the five
official farm size groups: marginal, small, semi-medium, medium, and large. Farm
input data is collected almost one year after starting the household-farm selection
process resulting in data corresponding to mid-2007. Figure 2 shows when this final
stage of the data collection happened in relation to NREGA’s implementation.

Many NREGA studies use the 2009 National Sample Survey (NSS) data. Be-
cause of the timing of this data collection within the NREGA rollout, its use restricts
analysis to comparisons between Phase II and III districts, since the Phase II cutoff
was mid-2008. Technology adoption impacts are most likely to occur at smaller
farm levels, where shifts away from labor-intensive farm operations have yet to
occur, so use of this new ACIS data is preferable as it allows for comparisons at
the cutoff between Phase I and Phase II when more small farms will be receiving
treatment. NSS data collected from 2008 onwards essentially restricts analysis to
impacts on Phase II districts with Phase III districts as controls. In the case where
Phase I and II districts are pooled together as a single treatment group, some studies
ignore the fact that Phase I districts had been implementing NREGA for a longer
period than Phase II districts or Phase I districts are dropped entirely. This study, in
contrast, uses only a portion of Phase I and Phase II districts, excluding Phase III.
For the panel estimations, I also use the 2004–2005 India Human Development Survey (IHDS) as baseline technology adoption data. The IHDS has been used extensively by sociologists interested in nutrition and intrahousehold decision making in India. Because it is representative at the district level, I match technology use data to the ACIS by district. This enables pooled OLS, difference-in-differences, and panel fixed effects estimations. The RD method only requires a cross-section of data, but using baseline data to subtract off previous values of variables may potentially reduce the estimator’s variability [31]. I conduct RD estimations with and without this baseline control.

The main challenge with this constructed panel is that measurements of technology use in IHDS in 2004 may differ in approach and quality from that of the ACIS in 2007, since they were conducted by different agencies. This would produce bias if measurement differed systematically for Phase I and Phase II districts in either dataset, particularly in the neighborhood of the district 200 cutoff. There is no indication to suggest this would be the case, but comparisons of RD estimates with and without this baseline data can help determine the extent to which this might be a problem. Ultimately, use of baseline data for RD estimations serves as a robustness check of the cross-sectional estimates.

Table 1 shows characteristics of the farms that make up the study. I categorize these by the official farm size groups of the Indian government and include a 2002 round of ACIS data on farm characteristics to illustrate how the number, size, and makeup of farms has changed over a five-year period. Of the 559 districts in 32 states and union territories that ACIS covers, 411 districts in 29 states and union territories are covered in both years. The average number of marginal and small farms increased in the country over the five-year span by 6 and 3 percent, respectively, while acres cultivated in these groups rose by 10 and 5 percent. This is a mirror image of the drop in the total number of medium and large farms and de-

---

4 For 2007 alone, there are 521 districts in 31 states and union territories, with increases in coverage from 2002 to include the states of Bihar, Jharkand, and Meghalaya and additional districts in Arunachal Pradesh, Assam, Gujarat, Haryana, Jammu & Kashmir, Karnataka, Madhya Pradesh, Nagaland, Pondicherry, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh, and West Bengal. Observations decreased slightly in Maharashtra between 2002 and 2007.
crease in acres cultivated within those. Average plot sizes also change dramatically by group in 2007.

Table 1: Total farms and area farmed in India in 2002 and 2007 per district. Source: Ministry of Agriculture [32].

<table>
<thead>
<tr>
<th>Farm Group</th>
<th># of Farms per District</th>
<th># of Acres per District</th>
<th>Average Plot Size</th>
<th>Plots per Farm</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2002</td>
<td>2007</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Marginal</td>
<td>124,060</td>
<td>131,005</td>
<td>6%</td>
<td>50,051</td>
</tr>
<tr>
<td>(155,005)</td>
<td>(141,057)</td>
<td>(149,440)</td>
<td>(53,419)</td>
<td>(0.41)</td>
</tr>
<tr>
<td>Small</td>
<td>36,898</td>
<td>38,119</td>
<td>3%</td>
<td>51,572</td>
</tr>
<tr>
<td>(31,626)</td>
<td>(35,565)</td>
<td>(45,134)</td>
<td>(48,159)</td>
<td>(0.94)</td>
</tr>
<tr>
<td>Semi-Medium</td>
<td>22,917</td>
<td>22,695</td>
<td>-1%</td>
<td>61,318</td>
</tr>
<tr>
<td>(21,490)</td>
<td>(22,265)</td>
<td>(59,278)</td>
<td>(61,540)</td>
<td>(1.79)</td>
</tr>
<tr>
<td>Medium</td>
<td>11,372</td>
<td>10,764</td>
<td>-5%</td>
<td>65,091</td>
</tr>
<tr>
<td>(13,821)</td>
<td>(13,904)</td>
<td>(84,595)</td>
<td>(86,082)</td>
<td>(3.48)</td>
</tr>
<tr>
<td>Large</td>
<td>2,293</td>
<td>2,039</td>
<td>-11%</td>
<td>35,689</td>
</tr>
<tr>
<td>(6,428)</td>
<td>(6,056)</td>
<td>(118,692)</td>
<td>(107,643)</td>
<td>(11.01)</td>
</tr>
<tr>
<td>All</td>
<td>197,474</td>
<td>204,621</td>
<td>4%</td>
<td>264,289</td>
</tr>
<tr>
<td></td>
<td>(162,186)</td>
<td>(169,569)</td>
<td>(258,592)</td>
<td>(257,688)</td>
</tr>
</tbody>
</table>

*Standard deviations in parentheses. N=411 districts in 29 states and union territories.

There is no information on the composition of these changes, such as whether larger farms were perhaps broken into the smaller ones that increase in number. If this were the case, a potentially confounding factor could arise from a shift of technologies on large farms to their corresponding smaller plots of land. This alone would bring down the farm size threshold for adoption. For this to bias estimation results in this study, however, such a shift would have to be more plausible for NREGA districts around the first-phase cutoff and for labor-saving technologies only. I assume these conditions do not all hold and that any change in farm size composition is not an outcome of NREGA and not correlated with unobservables that may cause a farmer to use labor-saving technology.

Figure 3 illustrates how technology use on farms varies by farm size and technology type. I highlight the relevant technologies from the ACIS study, grouped by hand-operated, animal-operated, machine-operated, and water-related implements. In all but one case, marginal farms make the least use of technology. Hand hoes and animal-drawn wooden plows are most widely-used in India’s farming sector, covering roughly 55 and 45 percent of all farms, respectively. Medium and large farms dominate the use of machines and some animal-drawn implements, such as bullock
carts and disc harrows, but there is more parity for other animal- and hand-operated implements.

Most of this technology is custom hired. While the ACIS data does not distinguish between technologies that are owned versus hired, the IHDS shows that nearly 67 percent of all technology expenditure goes towards hiring of animals and equipment, whereas only 12 percent goes to own purchases. The rest is spent on maintenance, loan installments, interest, transportation, associated diesel and electricity expenditures, etc., which are not clearly associated with either hired or own technology.
Figure 3: Percentage of farms using agricultural technologies in 2007, by farm size. Source: Ministry of Agriculture [32].

Figure 4 shows an estimate of the farm size threshold for each technology and how this differs for NREGA versus non-NREGA districts. This is analogous to the theoretical depiction in Figure 1, except here I estimate the farm size threshold as the share of marginal farms using each technology. Because the ACIS data only has farm size resolution by five groups, I use the marginal group since it represents the smallest farms adopting the technology and, therefore, can provide a proxy for the
farm size threshold. I then rank all the technologies in the data in decreasing order of the share used by non-NREGA marginal farms.

Figure 4: Estimated minimum farm size associated with each technology, by NREGA participation. Source: Ministry of Agriculture [32].

One can see that the order of these technologies by share of marginal farms using them is similar to what is predicted by the theoretical model in equation (2) and Figure 1. With a few exceptions, hand-operated implements dominate the left-hand side of the x-axis, representing the largest share of marginal farms using. Machine-powered technologies are far to the right. Animal-operated technologies generally take up the middle, with some overlap on either side. I add “hand,” “animal,” and “power” to the beginning of each technology title in this figure for ease of exposition. I then estimate the same marginal shares for all districts that participated in Phase I of NREGA. One sees larger variation around the non-NREGA curve at the lower end of the technology spectrum compared to higher end. This is consistent with a rising farm size threshold of adoption for technologies that replace unskilled labor, at the cost of hand-operations that make use of unskilled labor. However, it is not clear from this simple difference in means how NREGA may be influencing
such differences or what the causal pathways are. Thus, in the next section, I show results for statistical tests of NREGA’s impact for these thresholds.
6. Results

Following the estimation approach detailed in Section 4, Table 2 compares regression results for OLS, difference-in-differences, and panel approaches. For each, the specification was done two ways. In the first, I aggregate technology adoption at the district level and use observations from 2004 and 2007 \((n = 848)\). In the second, I split the data by five farm size categories over the two years \((n = 3,661)\), clustering standard errors at the district level and including farm size dummies. I combine all animal- and machine-operated technologies into a single binary labor-saving technology use variable. While this does not illustrate the rich heterogeneity in adoption impacts at technology-specific levels, it can provide a useful overall program effect to begin the analysis. Later, I analyze technology-specific impacts.

Table 2: Estimated impacts of NREGA participation on the percentage of farms adopting labor-saving technology using OLS, difference-in-differences & panel regression methods. Source: Author.

<table>
<thead>
<tr>
<th>Use of Any Labor-Saving Technology</th>
<th>OLS (1)</th>
<th>OLS (2)</th>
<th>DD (3)</th>
<th>DD (4)</th>
<th>Panel FE (5)</th>
<th>Panel FE (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>NREGA</td>
<td>0.0664***</td>
<td>0.0673***</td>
<td>0.00462</td>
<td>0.0171</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0189)</td>
<td>(0.0177)</td>
<td>(0.0296)</td>
<td>(0.0262)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post</td>
<td>-0.122***</td>
<td>-0.0744***</td>
<td>-0.125***</td>
<td>-0.0585**</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0229)</td>
<td>(0.0209)</td>
<td>(0.0289)</td>
<td>(0.0255)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>NREGA*Post</td>
<td>0.103***</td>
<td>0.0727***</td>
<td>0.149***</td>
<td>0.0997***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0383)</td>
<td>(0.0345)</td>
<td>(0.0534)</td>
<td>(0.0422)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>0.645***</td>
<td>0.618***</td>
<td>0.718***</td>
<td>0.657***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0113)</td>
<td>(0.0118)</td>
<td>(0.0133)</td>
<td>(0.0145)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Farm Size Dummies</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>848</td>
<td>3,661</td>
<td>848</td>
<td>3,661</td>
<td>848</td>
<td>3,661</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.012</td>
<td>0.038</td>
<td>0.048</td>
<td>0.048</td>
<td>0.749</td>
<td>0.597</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses. *** p<0.01. ** p<0.05. * p<0.1. Columns (2), (4), and (6) clustered at district level.

As discussed earlier, OLS results are likely to be biased because they do not account for endogeneity between adoption and participation in the NREGA program. Thus, the roughly 6.7 percentage point impact shown in Table 2 is likely to underrepresent the true effect. Indeed, columns (3) and (4) show larger impacts of 7-10
percentage points using the difference-in-differences specification, which accounts for some of this endogeneity. Using the panel approach, the fixed effects method controls for time-invariant, district-level unobservables, increasing estimates to 10-15 percentage points.

These magnitudes are quite large. The final column of Table 2 says that a district whose initial adoption of labor-saving technology was 65.7 percent before NREGA—the 2004 average—will now see 75.7 percent of its farms using labor-saving technology due to the program. Considering impacts of the program on unskilled agricultural wages are relatively small—around 5%—this suggests a large elasticity of technology demand with respect to unskilled agricultural wages.

Table 3 estimates the fixed effects model separately for each farm size category. This helps shed light on the distribution of NREGA’s impacts across farm size categories. From columns (1) and (2), it is clear that the within estimates of impacts on the marginal and small farmers are driving the aggregate results, with increases of 18.5 and 12.2 percentage points, respectively. As farm sizes get larger, the effect becomes smaller and less significant, even with high numbers of observations for larger farm groups. Panel methods generate the largest R-squared of the methods discussed so far, but again the validity of all these estimates is questionable primarily because of the parallel trends assumption between treatment and control groups.

Regression discontinuity methods can help address this. As discussed earlier, it can be thought of as a pseudo-randomization of NREGA assignment around the district 200 cutoff. Figure 5 illustrates this by plotting the Planning Commission’s BI index value against the associated ranking used to determine NREGA participation. The figure shows that, near the origin, each one-unit increase in BI rank stems from relatively large increases in BI value. In other words, there are large marginal increases in a district’s development status at very low BI ranks. This is also true for districts at BI rankings above 400, where a one-unit change in district ranking indicates much higher development as measured by the index value than the next-lowest ranked district. An RD cutoff in these extreme regions would generate poorly-balanced treatment and control groups, especially as the window of analysis increases. However, between the ranks of 100 and 300, changes in BI index value
Table 3: Panel fixed effects regression estimates of NREGA participation on the percentage of farms adopting labor-saving technology, by farm size. Source: Author.

<table>
<thead>
<tr>
<th>Use of Any Labor-Saving Technology</th>
<th>Marginal (1)</th>
<th>Small (2)</th>
<th>Semi-Median Medium (3)</th>
<th>Large (4)</th>
<th>Overall (5)</th>
<th>Overall (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>-0.176***</td>
<td>-0.0818**</td>
<td>-0.0104</td>
<td>0.0282</td>
<td>-0.00257</td>
<td>-0.125***</td>
</tr>
<tr>
<td></td>
<td>(0.0315)</td>
<td>(0.0359)</td>
<td>(0.0395)</td>
<td>(0.0603)</td>
<td>(0.148)</td>
<td>(0.0289)</td>
</tr>
<tr>
<td>NREGA*Post</td>
<td>0.185***</td>
<td>0.122*</td>
<td>0.111</td>
<td>0.0414</td>
<td>-0.138</td>
<td>0.149***</td>
</tr>
<tr>
<td></td>
<td>(0.0594)</td>
<td>(0.0643)</td>
<td>(0.0703)</td>
<td>(0.0872)</td>
<td>(0.204)</td>
<td>(0.0534)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.715***</td>
<td>0.731***</td>
<td>0.715***</td>
<td>0.720***</td>
<td>0.807***</td>
<td>0.714***</td>
</tr>
<tr>
<td></td>
<td>(0.0185)</td>
<td>(0.0212)</td>
<td>(0.0235)</td>
<td>(0.0370)</td>
<td>(0.0986)</td>
<td>(0.0168)</td>
</tr>
<tr>
<td>Observations</td>
<td>828</td>
<td>798</td>
<td>777</td>
<td>703</td>
<td>555</td>
<td>848</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.750</td>
<td>0.759</td>
<td>0.758</td>
<td>0.756</td>
<td>0.848</td>
<td>0.749</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 5: Rank of India’s districts according to the Planning Commission’s Backwardness Index compared to index values. Source: Zimmermann [7].
per ranking increase are small. This is shown by the relatively flat line on either side of the Phase I cutoff. Thus, districts just to the left and right of the Phase I cutoff make especially good comparison groups.

Because of the abrupt change in NREGA treatment at this 200-district cutoff, the outcome variable may also show an abrupt change in adoption levels at the same cutoff value. Figure 6 depicts two local polynomial curves on each side of the normalized NREGA cutoff, fitted using a triangle kernel. The vertical axis measures the change in the percent of farms adopting any labor-saving technology between 2004 to 2007. As described earlier, the binary variable used here is district-level information aggregated across all labor-saving technologies. Because the 2004 data may not necessarily compare well to 2007, the intercepts of these curves at the cutoff may not fully represent the change over time during these three years. However, the measure of the difference in this change between NREGA and non-NREGA districts should not be affected. To show this, Figure 7 uses 2007 data only and shows a similar magnitude jump between these groups when measuring the percent of farms using any labor-saving technology in that year alone.

Figure 6: Fitted curves to the left and right of the NREGA district cutoff of 200 measuring the change in percent of farms per district using any labor-saving technology from 2004 to 2007. Source: Author.
Although the first 200 districts of the BI were eligible for NREGA, Figure 8 shows that this was not fully consistent with uptake of the program. The density functions of BI rank for both NREGA and non-NREGA districts show that, while most districts within the first 200 implemented NREGA, there are tails in the treated group on the non-treated side and in the non-treated group on the treated side, the latter of which is slightly bigger indicating more districts ranked below 200 that did not receive NREGA in the first phase. Use of RD only requires there to not be full manipulation at the cutoff value of the running variable, but an alternative NREGA assignment algorithm that more precisely corresponds to actual implementation is one where each state receives at least one NREGA district in Phase I, even if that state does not have any districts ranked below 200. The choice of district within state is then based directly on national BI ranking after taking state-level participation into account [7].
Figure 9 shows how the national BI rank of districts compares to the normalized state rank, which is based on whether they actually participated in the program in Phase I. I generate a state-level NREGA rank of $-1$ for the highest ranked district of each state that is included in the first phase of implementation. A state rank of zero or above indicates no actual NREGA treatment. Thus, quadrants I and III show complier districts. Quadrant I contains districts that were ranked above 200 in the BI and did not receive any NREGA treatment (i.e., their rank within state was zero or above). Quadrant III contains districts that were ranked below 200 nationally and actually implemented NREGA. This latter group contains the poorest districts in India, such as those in Orissa, and the former group some of the richest, such as those in western Uttar Pradesh, Punjab, and Haryana.

Quadrant II of Figure 9, on the other hand, shows the districts that received NREGA treatment despite having a BI rank above the national cutoff of 200. This includes districts in states such as Kerala that would be too developed to be officially eligible for Phase I of NREGA but, based on the unofficial criterion that each state receive at least one NREGA district, included at least their poorest district. In the case of Kerala, this poorest district has a national rank of over 400. Similarly, quadrant IV shows districts that didn’t receive NREGA treatment even though they
ranked below 200, such as those of Maharashtra. Even though NREGA implementation does not perfectly follow national BI rankings, actual implementation is not fully-manipulable and a state-by-state normalized ranking system shows a more objective method of final implementation.

![State Rank of BI As Determinant of Participation](image)

Figure 9: District rank in the BI nationally (normalized to Phase I eligibility) versus district rank in the BI by state (normalized to actual Phase I participation). Source: Author.

This could lead one to choose a sharp RD design using state rank, as in Zimmermann [7], but this raises concerns of treatment and control group balance, as each state’s treatment groups may be quite different in development status (see Kerala case above, for example). Instead, I generate fuzzy RD estimates at the national level and do this by calculating the Wald estimates corresponding to equation (9), subject to the probability estimates in equation (8). As shown in Figure 10, with 40–100 district windows on either side of the cutoff, the jump in any labor-saving tech-

---

5 Technically, estimates use the specified number of observations on the right-hand side of the cutoff and a number of observations on the left-hand side that is close to the specified value.
nology adoption for districts ranked below 200 in the BI is 9.9 percentage points.\textsuperscript{6} Note that this estimate is the same as the panel fixed effects estimate in the final specification of Table 2. This may give some additional validity to estimates from other difference-in-differences studies on NREGA.\textsuperscript{7}

Figure 10: Regression discontinuity estimates of NREGA’s impact on the percentage of farms per district using any labor-saving technology, using 2007 cross-sectional data and district bandwidths between 40-100. Source: Author.

In order to address the high variance in estimates using the aggregated left hand side variable, I consider technologies on an individual basis as discussed in Section 3. Table 4 shows these impacts by labor-intensive (Panel A) and labor-saving (Panel

\textsuperscript{6} As presented here, it is a negative jump of 10 percentage points for districts ranked above 200. The sharp jump in labor-saving technology adoption for districts that were ranked below 200 in the BI is 5.3 percentage points, corresponding to equation 7. The jump in treatment probability for those districts is 53.5 percent, yielding a final estimate of a 9.9 percentage point increase in adoption using the 40-district window. The larger the district window around the cutoff, the lower the estimated sharp impact of NREGA, as districts further away from each other become part of the treatment and control groups. The treatment probability for the districts in these larger windows to the left of the cutoff also decreases, generating similar Wald estimates for each estimation.

\textsuperscript{7} In those studies, however, the treatment focus is around the Phase II cutoff (around district 350 in the BI). Thus, others typically attempt to control or test for trends among a different sets of districts than the ones studied here. The robustness of the overall difference-in-difference estimates in light of these RD results suggests that a common trends assumption may hold for districts around the Phase I cutoff but it is not clear if this holds for estimations around Phase II.
B) technologies. As in Figure 10, the coefficients represent the jump in technology use at the 200 district cutoff for the first district not to receive NREGA, using a fuzzy RD design. Districts just missing the NREGA cutoff employ hand-operated technologies, such as seed/fertilizer drills, sprayer/dusters, hand hoes, and blade hoes 20–26 percentage points more than districts that just made the NREGA Phase I cutoff. Maize shellers, wheel hoes, and paddy weeders were used just slightly less (3-5 percentage points less) in NREGA Phase I districts.

Animal-drawn wooden plows, levelers, and soil scoops appear to be substituting for some of these hand-operated land preparation technologies. Levelers and wooden plows are used 25–30 percentage points more in NREGA districts and soil scoops just under 15 percentage points more. Certain machine-operated technologies that are likely to directly replace hand-operated technologies, such as power sprayer/dusters and power tillers, are used more in NREGA districts. However, as discussed above, this may not be the case when machine-operated technologies instead replace animal-operated technologies that are already labor-saving in that they do not rely on unskilled labor. The statistically and economically insignificant impact on the power cane crusher, for example, illustrates that NREGA shifted some of these operations from labor-intensive to animal-powered, as shown in the fourth column of Panel B, but not necessarily straight to machine-powered. This sort of multi-stage movement along the labor-intensity curve has similar implications for tractor technologies and combine harvesters that would replace animal-operations that are already labor-saving.

The appendix graphically shows estimates for 40-100 district bandwidths corresponding to these and other technologies in the ACIS dataset. Figure 11 shows all hand-operated implements. As discussed above, hoe implements are the most widely-used of any technology. They are also perhaps the most dependent on unskilled agricultural labor and vulnerable to direct labor-saving substitutes, such as plows, soil scoops, and power tillers. All three types of hoes captured in this data are used less due to NREGA. The paddy weeder and maize sheller are also used less, though the change in the use of the hand-operated chaff cutter is not statistically significantly different from zero. Hand-operated fodder choppers are not documented in this dataset but may be an important substitute for chaff cutters.
Table 4: Regression discontinuity results of NREGA’s impact on technology adoption for labor-intensive and labor-saving technologies for bandwidths of 40-100 districts.

### A. Hand-Operated Technology Regression Discontinuity Estimates

<table>
<thead>
<tr>
<th></th>
<th>Seed/Fertilizer Drill</th>
<th>Maize Sheller</th>
<th>Sprayer/Duster</th>
<th>Hand Hoe</th>
<th>Wheel Hoe</th>
<th>Blade Hoe</th>
<th>Paddy Weeder</th>
</tr>
</thead>
<tbody>
<tr>
<td>40 districts</td>
<td>20.5 (7.2)</td>
<td>2.33</td>
<td>6.9 (2.2)</td>
<td>2.26</td>
<td>26.4 (11.2)</td>
<td>2.55</td>
<td>20.8 (16.1)</td>
</tr>
<tr>
<td>50 districts</td>
<td>25.3 (7.7)</td>
<td>3.30</td>
<td>6.2 (2.4)</td>
<td>2.55</td>
<td>25.1 (10.6)</td>
<td>2.37</td>
<td>32.0 (17.3)</td>
</tr>
<tr>
<td>60 districts</td>
<td>24.9 (7.3)</td>
<td>3.39</td>
<td>6.4 (2.4)</td>
<td>2.66</td>
<td>25.7 (10.3)</td>
<td>2.49</td>
<td>28.4 (16.7)</td>
</tr>
<tr>
<td>70 districts</td>
<td>22.5 (7.1)</td>
<td>3.18</td>
<td>6.5 (2.5)</td>
<td>2.58</td>
<td>26.6 (10.7)</td>
<td>2.47</td>
<td>26.7 (15.9)</td>
</tr>
<tr>
<td>80 districts</td>
<td>21.5 (7.0)</td>
<td>3.06</td>
<td>6.5 (2.6)</td>
<td>2.52</td>
<td>31.3 (11.6)</td>
<td>2.69</td>
<td>20.5 (16.6)</td>
</tr>
<tr>
<td>90 districts</td>
<td>19.9 (6.8)</td>
<td>2.91</td>
<td>6.0 (2.5)</td>
<td>2.35</td>
<td>34.4 (22.2)</td>
<td>2.83</td>
<td>17.8 (25.3)</td>
</tr>
<tr>
<td>100 districts</td>
<td>16.6 (6.4)</td>
<td>2.59</td>
<td>4.9 (2.4)</td>
<td>2.01</td>
<td>32.8 (11.9)</td>
<td>2.75</td>
<td>20.6 (16.2)</td>
</tr>
</tbody>
</table>

### B. Animal- and Machine-Operated Technology Regression Discontinuity Estimates

<table>
<thead>
<tr>
<th></th>
<th>Wooden Plow</th>
<th>Leveled</th>
<th>Soil Scoop</th>
<th>Animal Cane Crusher</th>
<th>Power Sprayer/Duster</th>
<th>Power Tiller</th>
<th>Power Cane Crusher</th>
</tr>
</thead>
<tbody>
<tr>
<td>40 districts</td>
<td>-29.8 (10.1)</td>
<td>-2.97</td>
<td>-26.7 (9.7)</td>
<td>-2.75</td>
<td>-13.4 (5.2)</td>
<td>-2.55</td>
<td>-1.6 (5.4)</td>
</tr>
<tr>
<td>50 districts</td>
<td>-26.1 (9.7)</td>
<td>-2.68</td>
<td>-24.8 (9.5)</td>
<td>-2.62</td>
<td>-12.8 (5.3)</td>
<td>-2.42</td>
<td>-2.8 (5.2)</td>
</tr>
<tr>
<td>60 districts</td>
<td>-14.8 (10.1)</td>
<td>-1.47</td>
<td>-14.1 (9.6)</td>
<td>-1.47</td>
<td>-11.8 (5.1)</td>
<td>-2.33</td>
<td>-2.9 (5.1)</td>
</tr>
<tr>
<td>70 districts</td>
<td>-10.1 (10.7)</td>
<td>-0.90</td>
<td>-6.5 (10.3)</td>
<td>-0.63</td>
<td>-12.8 (5.1)</td>
<td>-2.64</td>
<td>-3.1 (1.0)</td>
</tr>
<tr>
<td>80 districts</td>
<td>-7.6 (11.0)</td>
<td>-0.69</td>
<td>0.5 (11.0)</td>
<td>0.05</td>
<td>-12.5 (3.9)</td>
<td>-2.38</td>
<td>-3.3 (1.1)</td>
</tr>
<tr>
<td>90 districts</td>
<td>-5.9 (11.0)</td>
<td>-0.53</td>
<td>4.8 (11.3)</td>
<td>0.42</td>
<td>-13.4 (5.3)</td>
<td>-2.52</td>
<td>-3.4 (1.1)</td>
</tr>
<tr>
<td>100 districts</td>
<td>-4.4 (11.0)</td>
<td>-0.40</td>
<td>5.2 (11.3)</td>
<td>0.55</td>
<td>-14.0 (5.4)</td>
<td>-2.65</td>
<td>-3.5 (1.1)</td>
</tr>
</tbody>
</table>

Notes: Bandwidths are measured as number of districts to the right of the cutoff. Jump in the adoption estimates change in percent of farms adopting labor-saving technology at NREGA cutoff, where NREGA districts are on the left of threshold and non-NREGA districts on the right. Jump in treatment probability represents the change in probability of treatment at NREGA cutoff. The treatment effect shown here is the quotient of these two local Wald estimates, measured in percentage point changes between groups.
Animal-operated implements are used more in NREGA districts, suggesting substitution away from the above-mentioned labor-intensive farm operations towards these labor-saving ones. There are shifts to wooden plows, levelers, and soil scoops, as well as smaller impacts on sugar cane crushers and potato/groundnut diggers. These latter shifts might be expected if there are fewer hand-operated substitutes available. Animal-drawn bullock carts do not show any significant impacts because, like tractors, the use of these implements is highly multi-purpose, making it unclear whether substitutes for these technologies are labor-intensive or also labor-saving. NREGA also increases the use of power tillers in NREGA districts, suggesting that some farmers substitute labor-intensive land preparation techniques (e.g. use of hoes) with machines, leaping over animal-drawn plows. But there is ambiguity in other machine-operated implements, such as cane crushers and combine harvesters. Animal-drawn technologies have likely already substituted for their labor-intensive counterparts.

It is interesting to note the seasonality of this labor-saving technology adoption. As mentioned earlier, NREGA was intended to supplement peak-season agricultural work with slack agricultural period public works opportunities. And while some studies have shown seasonality in NREGA’s wage effects, the overall increase in unskilled agricultural wages for a variety of tasks means that there may be some spillover on shadow wages for peak period tasks.

The results shown here suggest that there is also some spillover of labor-saving technology adoption between seasons, but it is mostly concentrated in the slack production period. While I cannot observe the timing of NREGA workdays associated with adoption, I do note that for slack production period agricultural tasks, such as weeding, chemical application, and irrigation, the results show a movement away from hand-operated paddy weeders and sprayer/dusters to power sprayer/dusters and animal irrigation, for example. There are also movements towards labor-saving technologies during the land preparation and planting periods. Hand seeding/fertilizer drills and tilling have given way to wooden ploughs, tractor cage wheels, soil scoops, and power tillers. Harvest period production technologies, on the other hand, show less clarity in shifting technology patterns. Though hand-operated maize shellers are down and mechanized reapers, diggers, and cane crushers are up,
there are not shifts from pedal-operated threshing to Olpad or combine-harvester threshers. This suggests that the harvest season is more “peak” than the land preparation and planting period. That NREGA’s impacts on technology adoption are muted during this peak production period is further evidence that labor markets likely condition when and whether farmers use new labor-saving technologies in response to the program.

As mentioned above, there are a number of alternative channels through which NREGA may be simultaneously influencing technology adoption. The primary competing theory is that technology adoption rises due to income provided by NREGA, which would allow farmers to either 1) overcome credit constraints in the purchase or hire of technologies or 2) send their kids to school, requiring technological replacements for working-age children. I aim to untangle this first mechanism from the labor market channel in Table 5. Using the same regression discontinuity empirical framework as above, I estimate equations (7)-(9) using district-level credit uptake as the outcome variable, replacing labor-saving technology adoption. A significant difference between NREGA and non-NREGA districts would suggest an alternate channel through which the program may be affecting farmers. As with earlier RD regressions, the jump in treatment probability from equation (8) is significant and increases as the bandwidth around the NREGA cutoff decreases. However, we cannot reject with any acceptable statistical level of confidence that differences in credit uptake differ by NREGA treatment. The z-values on the local Wald estimates range from -0.06 to 0.65.

Table 5: Test of credit effects attributed to NREGA using RD bandwidths of 40-100 districts.

<table>
<thead>
<tr>
<th>Bandwidth:</th>
<th>Jump in Credit Uptake</th>
<th>Jump in Treatment Probability</th>
<th>Treatment Effect on Credit</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coef.</td>
<td>SE</td>
<td>z</td>
</tr>
<tr>
<td>40 districts</td>
<td>12.9</td>
<td>16.2</td>
<td>0.79</td>
</tr>
<tr>
<td>50 districts</td>
<td>8.9</td>
<td>14.0</td>
<td>0.64</td>
</tr>
<tr>
<td>60 districts</td>
<td>5.0</td>
<td>12.3</td>
<td>0.41</td>
</tr>
<tr>
<td>70 districts</td>
<td>2.5</td>
<td>11.0</td>
<td>0.23</td>
</tr>
<tr>
<td>80 districts</td>
<td>1.5</td>
<td>10.1</td>
<td>0.15</td>
</tr>
<tr>
<td>90 districts</td>
<td>1.1</td>
<td>9.4</td>
<td>0.12</td>
</tr>
<tr>
<td>100 districts</td>
<td>-0.5</td>
<td>8.9</td>
<td>-0.06</td>
</tr>
</tbody>
</table>

Notes: Bandwidths are measured in number of districts to the right of the cutoff. Jump in the credit uptake estimates equal the change in percent of farms taking any credit at NREGA cutoff, where NREGA districts are on the left of threshold and non-NREGA districts on the right. Jump in treatment probability represents the change in probability of treatment at NREGA cutoff. The treatment effect is the quotient of these, or local Wald estimate, measured in percentage point changes between groups.
Other potential channels through which NREGA may be affecting technology adoption are schooling and infrastructure. At least in the context of labor-saving technology adoption, sending one’s kids to school due to NREGA is similar to replacing a child, adult family member, or hired worker who has chosen to participate partially or fully in NREGA. Both represent second-order effects of the program through the labor market. As for infrastructure, technology adoption could increase due to reduced transaction costs associated with NREGA or increased access to water. Indeed, as roads and water-related projects represent 20 and 50 percent of works, respectively, during the first years of NREGA, these would be the primary infrastructure channels. Yet, whereas labor market effects are more likely to show up relatively quickly upon program implementation, roads and irrigation facilities take a wide-ranging amount time to physically be installed. Anecdotally, it seems to be the case that NREGA was particularly slower to accomplish the completion of public works during the first year and a half of the program, or Phase I, as compared to post-2007. This may be due to the time it takes to both physically carry out the activity and create new supply chains needed for associated materials, especially during Phase I when the poorest and least-developed districts were participating.

Even if infrastructure were thought to be generally in place in Phase I villages by the time of this data collection, there is still the question of whether or not there would have been sufficient to implement a private technology on a farm to take advantage of this and whether that technology would be labor-saving. For example, water-related improvements may lead to an increase in labor-saving irrigation-related activities (if diesel pumps are labor-saving, for example) or increase in labor-intensive water-related activities (bund control and weeding, for example). In order to disentangle the infrastructure and labor market channels, I single out all of the water-related technologies that could be affected by a potential increase in water infrastructure. The evidence is mixed. The first three technologies displayed—bund formers, Persian wheels, and cage wheels—can be thought

---

8 The 20 percent of infrastructure activity related to roads is both relatively small in amount and difficult to attribute to any subset of agricultural technologies. I, therefore, do not do an analysis of these infrastructure activities similar to those of water.
of as the most labor-saving water technologies in the ACIS dataset. Bund formers are implements that create and shape bunds around plots in order to retain water, decrease runoff, and sometimes provide an elevated canal to channel water from some source. Bunds are traditionally built manually with stones and dirt and can be very labor-intensive to make. The Persian wheel is an animal-drawn device that uses a series of bucket, jars, or scoops attached to points on a large wheel to lift water from open wells. And a cage wheel is simply an attachment to tractors that increases traction in wet fields for a variety of mechanized operations, such as tilling and puddling for paddy. The Persian wheel in particular is most likely to be affected by NREGA because not only is it labor-saving but it is also primarily used during slack agricultural production periods for irrigation. Indeed, the effect of NREGA on Persian wheel use is statistically significant and 1–2 percentage points higher in NREGA areas. Neither of the other two technologies are statistically significant.

Table 6: Infrastructure-related technology adoption estimates due to NREGA, using RD bandwidths of 40–100 districts.

|                | Coef. | SE    | z     | Coef. | SE    | z     | Coef. | SE    | z     | Coef. | SE    | z     | Coef. | SE    | z     |
|----------------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|-------|
|                | Bundle Former (2.5%) | Persian Wheel (0.5%) | Cage Wheel (0.8%) | Hydraulic Ram |
| 40 districts   | -0.2  | (3.4) | 0.07  | -0.7  | (0.7) | 1.02  | -0.6  | (1.7) | 0.36  | 0.0004 | 0.0003 | 1.10  |
| 50 districts   | -1.4  | (3.2) | 0.44  | -1.3  | (0.7) | 1.90  | -0.9  | (1.6) | 0.56  | 0.0004 | 0.0004 | 1.02  |
| 60 districts   | -2.4  | (3.0) | 0.80  | -1.3  | (0.7) | 1.95  | -1.3  | (1.6) | 0.85  | 0.0004 | 0.0004 | 1.02  |
| 70 districts   | -1.9  | (5.0) | 1.29  | -1.6  | (0.8) | 1.21  | -1.8  | (1.6) | 1.13  | 0.0003 | 0.0004 | 0.91  |
| 80 districts   | -6.6  | (5.0) | -1.54 | -2.0  | (0.8) | -2.37 | -1.8  | (1.6) | -1.14 | 0.0003 | 0.0004 | 0.85  |
| 90 districts   | -1.6  | (2.9) | -1.90 | -2.4  | (0.9) | -2.53 | -1.6  | (1.5) | -1.08 | 0.0003 | 0.0004 | 0.92  |
| 100 districts  | -6.1  | (2.9) | -2.16 | -2.7  | (1.0) | -2.83 | -2.5  | (1.5) | -1.65 | 0.0004 | 0.0003 | 1.02  |

Table: Infrastructure-related technology adoption estimates due to NREGA, using RD bandwidths of 40–100 districts.

<table>
<thead>
<tr>
<th></th>
<th>Coef.</th>
<th>SE</th>
<th>z</th>
<th>Coef.</th>
<th>SE</th>
<th>z</th>
<th>Coef.</th>
<th>SE</th>
<th>z</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Sprinkler Set</td>
<td>Drip Irrigation</td>
<td>Diesel Pump</td>
<td>Electric Pump</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>40 districts</td>
<td>-0.0068</td>
<td>0.0024</td>
<td>-0.59</td>
<td>-0.0008</td>
<td>0.0010</td>
<td>-0.02</td>
<td>46.9</td>
<td>(12.8)</td>
<td>3.67</td>
</tr>
<tr>
<td>50 districts</td>
<td>-0.0015</td>
<td>0.0024</td>
<td>-0.61</td>
<td>-0.0011</td>
<td>0.0009</td>
<td>-1.23</td>
<td>46.9</td>
<td>(12.7)</td>
<td>3.68</td>
</tr>
<tr>
<td>60 districts</td>
<td>-0.0006</td>
<td>0.0022</td>
<td>-0.28</td>
<td>-0.0016</td>
<td>0.0010</td>
<td>-1.63</td>
<td>43.2</td>
<td>(12.9)</td>
<td>2.53</td>
</tr>
<tr>
<td>70 districts</td>
<td>0.0004</td>
<td>0.0013</td>
<td>0.31</td>
<td>0.0018</td>
<td>0.0011</td>
<td>-1.61</td>
<td>42.5</td>
<td>(12.5)</td>
<td>3.39</td>
</tr>
<tr>
<td>80 districts</td>
<td>0.0010</td>
<td>0.0012</td>
<td>0.88</td>
<td>0.0017</td>
<td>0.0011</td>
<td>-1.52</td>
<td>37.6</td>
<td>(12.1)</td>
<td>2.10</td>
</tr>
<tr>
<td>90 districts</td>
<td>0.0018</td>
<td>0.0015</td>
<td>1.19</td>
<td>-0.0015</td>
<td>0.0011</td>
<td>-1.39</td>
<td>30.7</td>
<td>(11.4)</td>
<td>2.69</td>
</tr>
<tr>
<td>100 districts</td>
<td>0.0025</td>
<td>0.0020</td>
<td>1.30</td>
<td>-0.0013</td>
<td>0.0011</td>
<td>-1.20</td>
<td>26.6</td>
<td>(10.9)</td>
<td>2.44</td>
</tr>
</tbody>
</table>

Notes: Bandwidths are measured in number of districts to the right of the cutoff. The coefficient shown here is the local linear estimate measured in percentage point changes between treatment and control groups.

The next water-related technology shown is a hydraulic ram, or hydropowered water pump used to bring water from low elevations to higher ones. It might be considered a water-intensive technology in that its use would increase when more surface water is available, such as with ponds that are common in NREGA. That it does not significantly increase due to NREGA may suggest that water infras-
tructure is generally still low at the time of this analysis, thus reducing the chance that water-related infrastructure confounds the labor market channel in labor-saving technology adoption. On the other hand, more water availability would likely lead to a reduction in the use of water-conserving technologies, like sprinkler sets and drip irrigation. Estimates for these are negative but extremely small and not significant. The positive and significant estimates for diesel and electric pumps are a bit puzzling. While adoption of these technologies does not exactly fit the story of a water-infrastructure channel of technology adoption, since additional public water facilities would reduce the need for private water extraction methods, the increase in adoption could be evidence of some sort of income or credit effect where ownership or use of diesel or electric pump may be thought of as a boost in household and farm assets and a place where general income or wealth is invested. Another possibility is that this is a result of higher water tables that make drilling for diesel pumps more profitable, since boreholes do not need to go as far down.

All of the irrigation technologies are not as clear cut in how much labor, especially unskilled labor, they are actually replacing, especially compared to bund formers and tractor cage wheels. Thus, one might conclude from this table that NREGA has increased labor-saving water technologies, as evidenced by negative estimates on the first three technologies, while not responding much to other labor-neutral water-intensive or water-conserving technologies. But these estimates are all quite imprecise to interpret generally. Note also that most of the technologies in Table 6 are used by a very small portion of farmers overall. On average, 12–13 percent of farmers per district use diesel or electric pumps, 3 percent use a bund former, and just under 1 percent each use a Persian and cage wheel. A much smaller proportion use the others.

If it is true that labor-saving technologies are being adopted due to NREGA, the question emerges as to how much labor is being saved. Arriving at such estimates can be extremely difficult for several reasons. Empirically, the data required to control for all combinations of complementary and substitute technologies under a variety of crop choices, household factors, and agroecological conditions in estimating any single technology’s impact on both family and hired labor is generally not available. Instead, datasets typically either have detailed information on
dozens of technologies and agricultural inputs but little on household time use (e.g. ACIS) or detailed information on household labor, time use and socioeconomic information but only a few of the most prominent technologies, usually machines like tractors and combines (e.g. IHDS). In the former case, no estimate of labor and time use can be generated and, in the latter case, an inability to comprehensively control for all other technologies renders any estimate likely quite biased. Furthermore, in both cases, distinguishing labor between agricultural tasks is rare, let alone explicitly differentiating between formal/informal, permanent/casual, or skilled/unskilled labor, which this and other studies highlight as a distinction of the type of labor most affected by NREGA.

Nevertheless, some studies have attempted to calculate tradeoffs between labor and mechanization. Assuming constant returns to scale, Binswanger [33] found a partial elasticity of substitution between labor and machinery of 0.851 in mid-1900s United States. He uses a relatively crude measure of machinery expenditure, estimated as 15 percent of the value of farm machinery and equipment for interest and depreciation, as well as operation and repair. He also generates a machinery price elasticity of labor demand between 0.1256–0.1475, depending on functional form assumptions. Jayasuriya and Shand [34], on the other hand, look at differences in labor use per hectare with and without technical change in south and southeast Asia, but they restrict “technical change” to new rice varieties, as did many labor-technology studies of the era (as I discuss above).

Peterson and Kislev [35] perform an interesting study on the competing hypotheses on the direction of causality between technology adoption and increased agricultural wages. They find that, upon adoption of the cotton harvester in the US between 1930–1980, 79 percent of the reduction in cotton labor was due to increases nonfarm wages, with the rest attributed to decreasing costs of technology. Specifically, the elasticity of labor supply with respect to manufacturing wages is -2.5 percent. This suggests that NREGA’s nonfarm wages have more potential to decrease agricultural labor through technology adoption than through improving technology markets themselves. On the other hand, the authors focus on a major harvest technology only, which my study shows not to be affected by NREGA. Thus, it is not clear if 2.5 percent reductions in labor would apply to other technolo-
gies, as well.

A crude method of applying this rate to NREGA in India would be to measure
the percentage contribution of NREGA wages to off-farm income, use the num-
ber of mandays hired per year as a proxy for farm labor, and apply one of these
historically-documented hypothetical rates. Omitting family labor would bias this
estimate downward but applying a 2.5 percent elasticity to all technologies adopting
may at the same time bias the result upward.

Table 7: NREGA income, off-farm income, and hired agricultural labor, 2011.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Obs</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>NREGA annual income</td>
<td>42,144</td>
<td>854</td>
<td>2,856</td>
<td>0</td>
<td>100,000</td>
</tr>
<tr>
<td>Non-NREGA non-agricultural income</td>
<td>42,144</td>
<td>15,829</td>
<td>36,467</td>
<td>0</td>
<td>736,000</td>
</tr>
<tr>
<td>Total non-agricultural income</td>
<td>42,144</td>
<td>16,771</td>
<td>36,599</td>
<td>0</td>
<td>736,000</td>
</tr>
<tr>
<td>Hired labor days</td>
<td>14,746</td>
<td>37</td>
<td>186</td>
<td>0</td>
<td>8,880</td>
</tr>
<tr>
<td>Hired labor expenditure</td>
<td>15,517</td>
<td>6,171</td>
<td>20,302</td>
<td>0</td>
<td>500,000</td>
</tr>
<tr>
<td>Hired daily agricultural wage</td>
<td>14,024</td>
<td>102</td>
<td>238</td>
<td>0</td>
<td>12,000</td>
</tr>
<tr>
<td>Technology expenditure</td>
<td>15,944</td>
<td>7,294</td>
<td>29,126</td>
<td>0</td>
<td>924,000</td>
</tr>
</tbody>
</table>

Hired Labor Saved | Expenditure Saved
0.51 | 52
0.93 | 95
4.63 | 472

Note: all monetary figures in 2011 INR. Hired labor days are allowed to be zero even if hired labor expenditure is
positive (and vice versa). Hired daily agricultural wage is equal to expenditure divided by days, though cases
where expenditure is positive and days is zero show up as missing values. Technology expenditure equals
purchased and hired animals and equipment, plus loan, interest, and maintenance payments. *Ehris (1974) estimate
technology expenditure response to labor expenditure. *Paterson (1996) estimate of percent changes
in mandays as a result increases in off-farm wage income.

In Table 7, I experiment with a range of elasticities applied to the various tech-
nologies in this study to estimate potential labor savings. On average, NREGA
provided INR853 to households per year in 2011, or USD16–18. This represents
roughly 5 percent of total non-agricultural wage income. About one third of house-
holds hire agricultural labor for 37 mandays per year at a rate of just over INR100
per day or about USD2. Technology expenditure, as measured in the 2011 IHDS,
captures the amount of money used to purchase or hire equipment and animals,
loan and interest payments, and maintenance and transportation of technologies. At
an average of INR7,294 (USD160), a technology elasticity of labor expenditure of
0.851 would mean each additional INR729 (USD16) spent on agricultural technolo-
gies would cause a drop of INR52 (USD1.17) in labor expenditure, representing a
decrease of roughly 0.5 mandays. Using the Peterson and Kislev [35] off-farm income elasticity of labor supply of 2.5 percent instead, an INR168 (USD3.70) increase in off-farm wage income leads to almost a full day less of labor use. In the case of NREGA, the average additional income of INR853 (USD18–19) represents 5 percent of total off-farm wage income. The labor saved to farm owners is then 4.6 mandays and INR472 (USD10).

As it is not clear in this case how many households adopt more than one labor-saving technology as a result of the program, if one can assume additive separability of the impacts of multiple technologies, and whether the labor supply elasticity associated with a peak-season harvest technology can be applied to the primarily slack agricultural production period and animal-drawn technologies discussed in this study, this coarse illustration of potential labor-saving impacts from NREGA’s effect on technology adoption can be tested with targeted field work or existing high-resolution agricultural data from the early period of NREGA.

7. Conclusion

NREGA is one of the largest development programs ever implemented and, consequently, its direct and indirect effects are likely to be large and far-reaching. In addition to providing rural laborers with an important source of income and much-needed infrastructure in the poorest villages in the country, it can also alter short- and long-run incentives and equilibria in other areas of the rural economy.

This study theoretically models how incentives for agricultural technology adoption change due to NREGA’s impact on the opportunity cost of unskilled agricultural labor. This applies not only to hired labor but family labor drawn away from the farm due to NREGA. I test this hypothesis using a variety of empirical methods. With data collected just after the first phased rollout of the program in 2007, I use a national regression discontinuity design to estimate changes in labor-saving technology adoption and confirm threshold model predictions of a reduction in the cutoff farm size associated with basic labor-saving technologies. This reduction is roughly 10 percentage points overall and up to 18.5 for the smallest farmers. The biggest shifts occur between hand- and animal-operated implements and occur primarily for slack agricultural production period tasks.

I address several other channels through which technology adoption may simul-
taneously be changing as a consequence of NREGA. The participation in NREGA by small farming households can create income and credit effects that directly boost the use of agricultural technology, and NREGA’s impact on infrastructure could also alter a farmer’s technology portfolio. In additional analysis, I show that credit effects due to NREGA are insignificant but that in the case of diesel and electric pump purchase may be a plausible explanation. Infrastructure effects are not likely to occur in the short time frame between when NREGA began and when the data was collected. Indeed, over 50 percent of projects were water-related in early years but had little impact on water-related technologies of NREGA households, except in the case where those technologies were labor-saving.

While infrastructure impacts take time to materialize, it is quite plausible that both labor market and technology adoption effects show up immediately. NREGA started employing people right away, which caused shifts in the labor market. Farmers in India are generally able to respond quickly because of well-developed custom hire markets for technologies in villages, which reduce the need for farm households to generate high levels of savings or credit. I find that, at a minimum, two-thirds of expenditures on agricultural technologies in 2004 were for hired animals and equipment, as opposed to purchase.

More intuitively, the pattern of technology adoption, which favors the labor-saving variety over labor-intensive, suggests that labor markets must be in play to some extent. All of the other channels discussed here are more likely to show up as shifters in a household-farm budget constraint, boosting expenditure on all technology and input bundles uniformly. In other words, they would shift the pre-NREGA curve in Figure 1 down for all implements and not necessarily boost use of technology in the direction of labor-saving implements at the expense of labor-intensive ones.

This short-run result may lead to a range of potential outcomes in the long run. The quality of infrastructure may be the key determinant for the ultimate welfare of workers and farmers. There is evidence of a positive link between investment decisions of governments, financial institutions, and farmers in India and agricultural output and productivity growth [36]. Roads, schools, and electrification generate the most positive effects, whereas private investment effects on tractors, fertilizers,
pumps, and animal purchases by farmers are more mixed. Together, however, these investments are significant in increasing agricultural input use, output levels, and private investment.

Increases in farm productivity due to private technology adoption can also serve to recoup welfare losses to farm owners [37, 38] from increasingly-costly labor inputs. Modest output and productivity impacts have been recorded from irrigation and soil and water conservation [39], but NREGA water investments are unique and merit further study in the context of decreasing water supplies and poor surface water reliability in the face of climate change.

The adoption of agricultural technology stemming from increased agricultural wages may interact with higher rural incomes and more public infrastructure investment in the long run. If this increases agricultural output, farmers may expand use of all inputs, including labor. However, the type of labor may tend to favor the higher-skilled workers needed to operate new technologies and match the increasing marginal value products of labor. A focus on education and skill-development for rural populations to accompany NREGA may best prepare workers for this potential long-run scenario. Future research can expand the discussion and evidence of general equilibrium effects for this and other rural poverty and social safety net programs.


[21] G. Feder, R. E. Just, D. Zilberman, Adoption of Agricultural Innovations in Developing Countries: A Survey, Economic Development and


[27] D. Sunding, D. Zilberman, The agricultural innovation process: Re-


Appendix A

Figure 11: Regression discontinuity estimates of NREGA’s impact on the percentage of farms per district using hand-operated technologies, using 2007 cross-sectional data and district bandwidths between 40-100. Source: Author.
Figure 12: Regression discontinuity estimates of NREGA’s impact on the percentage of farms per district using animal-operated technologies, using 2007 cross-sectional data and district bandwidths between 40-100. Source: Author.
Figure 13: Regression discontinuity estimates of NREGA’s impact on the percentage of farms per district using machine-operated technologies, using 2007 cross-sectional data and district bandwidths between 40-100. Source: Author.