The Impact of India's Rural Employment Guarantee on Demand for Agricultural Technology

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Abstract

The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) is approaching eight years of implementation. Since 2006, it has offered up to 100 days per year of guaranteed public works employment to tens of millions of rural Indian households. It is intended to augment the purchasing power of the rural poor during droughts and slack agricultural production periods. Given its scale, it has the potential to generate additional ripples throughout the rural economy. Recent working papers have explored NREGA's effect of higher agricultural wages. In this paper, I ask whether this increase in the opportunity cost of agricultural labor incentivizes farm owners to adopt labor-saving agricultural technology. Using a regression discontinuity design and new Indian agricultural census data, I find that NREGA causes a roughly 20 percentage point shift away from labor-intensive technologies towards labor-saving ones, particularly for small farmers and low-powered technologies. This short-run result can lead to a variety of long-run outcomes in technology use, labor markets, and food security. A focus on education, skill development, and quality infrastructure alongside NREGA would augment the chances that the most positive long-run scenario occurs.

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1 Introduction

Landless agricultural laborers and small farmers constitute the majority of India's poor. As the rural population continues to grow and more people enter the country's 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 is scarce and wages for the poorest have been persistently below official subsistence levels. The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) aims to alleviate some of these concerns by providing yearly public works employment to rural households at minimum wages.

Passed into law in 2005 and first implemented in 2006, NREGA guarantees any Indian household up to 100 days per year of rural public works employment within 5 kilometers of residence and 15 days of application. Remuneration depends on state-specific minimum wages, usually about $2 per day. The law is modeled after the Maharashtra Employment Guarantee Scheme of the 1970-80's and seeks to increase the purchasing power of the poor during droughts and slack agricultural production periods, when unskilled workers work fewer days and face higher food prices. NREGA projects have focused primarily on water and road infrastructure, and nearly half of all workers have been women–far surpassing the 25% quota set by the government at the outset of the program.

Recent working papers have used district-level panel data from India's National Sample Survey and Ministry of Agriculture to find difference-in-differences estimates of 3-5% unskilled agricultural wage increases across the country due to NREGA (Imbert and Papp, 2013; Berg et al., 2012; Azam, 2012). Shah (2012) further finds a 30% reduction in wage sensitivity to farm production shocks for every one standard deviation increase in the NREGA infrastructure that workers must build. Wage increases are biased towards women and lead to higher overall rural labor force participation rates (Azam, 2012; Zimmermann, 2012), though it is not clear whether there is crowding out of private sector jobs. These studies, as well as Liu and Barrett (2013), indicate that the program is well-targeted to poor laborers.

This paper extends the analysis within the agricultural sector by focusing on how NREGA's effects on rural labor markets alter technology adoption decisions by farm owners. Since farm owners depend on the unskilled labor targeted by NREGA, a change in a worker's wage may affect the input price ratio faced by the farmer and increase the use of technology that replaces unskilled work. During informal focus groups in small farming areas of eastern Uttar Pradesh in late 2011, I found that some farm owners were uncertain about whether they could hire workers on their fields in the next season for the same wages they paid before NREGA. Laborers, on the other hand, explained that they receive higher wages for some farm tasks while others were simply no longer available. This suggests labor-saving technology adoption may favor some agricultural production tasks over others.

I incorporate rising unskilled worker opportunity costs into a farm size threshold technology adoption model, which predicts that, for relatively small wage increases such as these, the smallest farms will be the most likely to adjust their technology inputs in response to the program. I analyze outcomes for a range of agricultural technologies and find that the switch is most likely to occur for farms initially moving from labor-intensive technologies to low-powered labor-saving ones but not for movements to more labor-saving technologies. For example, NREGA may drive up the cost of hiring unskilled workers hand-plowing a field. In response, the farmer adopts a low-powered, animal-drawn wooden plough that requires fewer–and more skilled–workers. However, the model does not predict changes between two labor-saving technologies, such as a power tiller and tractor-drawn plough, both of which rely less on unskilled labor.

To test adoption empirically, I use a regression discontinuity design that takes advantage of the progressive rollout of NREGA to the poorest districts of the country first. India's Planning Commission ranked 447 districts in its "Backwardness Index" and implemented the program in the first 200 of these during Phase I in 2006-07. The next 150 were included as part of Phase II in mid-2007 and the rest of the country in mid-2008. I use this arbitrary Phase I cutoff to argue that the districts on either side of the 200th rank are similar in observable and unobservable aspects aside from NREGA eligibility. I use a fuzzy design because some districts did not end up in the treatment and control groups according to their ranking.

I use data from the Indian Agricultural Census Input Survey (ACIS), which records farm technology use for hand-, animal- and machine-powered implements. ACIS data was collected in mid-2007 when NREGA's first phase was drawing to a close, so I am able to use as my treatment and control groups the districts on either side of the Phase 1 cutoff. This contrasts with the more widely-used National Sample Survey released in 2009. Because this contains data collected around the Phase II cutoff, it can only allow for comparisons of richer districts. Since I expect technology adoption to occur at the smallest and poorest farm levels, use of the ACIS in 2007 at the first phase cutoff is preferable.

My findings show that NREGA causes a reduction in the percent of farms using labor-intensive technologies by roughly 20 points, whereas animal-drawn technologies are used 15-25 percentage points more. This implies a decrease in the threshold cutoff farm size for basic labor-saving technology adoption due to NREGA. While it is possible that participation in NREGA by small farming households creates income and credit effects that directly boost use of agricultural technology, the labor-saving nature of adoption suggests that at least some of NREGA's impact on technology is channeled through the increased opportunity cost of unskilled labor.

In the long run, continued reverberations between labor, technology, and NREGA in the rural economy can result in a wide range of outcomes. In the best-case scenario, NREGA's positive impact on adoption creates a win-win for farm owners and laborers to the extent that the technologies adopted increase farm productivity and the newly created NREGA infrastructure increases market access. In this case, demand for agricultural labor could not only return to pre-NREGA levels but shift out further, leading to higher wages and increased employment (at higher skill levels). With poor quality infrastructure and low levels of education and skill development, however, laborers could be worse off in a post-NREGA era as labor-saving technology is adopted and neither public works nor agricultural jobs are available. Even in this worst-case scenario, though, the prevalence of custom-hire technology markets increases the chances that farmers can disadopt technology and labor and technology markets return to pre-NREGA equilibria. With new data, I plan to test these competing long-run scenarios.

Finally, in additional analysis here, I explore whether NREGA's focus on water-related public infrastructure impacts adoption of water-saving technologies. I show that NREGA not only significantly reduces the use of private diesel pumps–one of the most popular methods of agricultural water extraction–but also reduces the use of water-conserving technologies, such as sprinkler and drip irrigation systems. Thus, NREGA has an important, albeit indirect, role in influencing both labor-saving and water-conserving agricultural technology adoption through its wage payments and choice of public works. Policymakers may want to consider these technology adoption incentives vis-à-vis their rural development priorities as they move forward with changes to NREGA and creation of other rural poverty programs inside and outside of India.

The rest of this paper is structured as follows. Section 2 provides the motivation and structure of NREGA and reviews the literature related to the employment guarantee's impact on labor and technology markets. Section 3 develops a farm-size threshold model of adoption that ties together increases in the opportunity cost of agricultural labor with the adoption of labor-saving technology. Section 4 discusses empirical methodologies, and Sections 5 and 6 detail the data and results, respectively. Section 7 concludes.

2 Background

In this section I first describe in more detail the motivation behind NREGA and its specific poverty-related goals. I then look more closely at the literature related to agricultural wage responses to an employment guarantee, including an earlier set of studies revolving around a 1970s state-level employment guarantee in Maharashtra, as well as recent studies on NREGA's agricultural wage effects. In Section 2.3, I discuss the state of the literature on determinants of technology adoption, specifically those pertaining to labor-saving technologies. In general, recent studies have not focused on the role of labor market changes in determining labor-saving technology adoption. Finally, I review the literature on how both the quantity and quality of village infrastructure investment affect labor and technology markets in helping determine long run outcomes.

2.1 NREGA

NREGA offers local wage employment for public village development projects, guaranteeing every unskilled laborer 100 days of public works employment in their own village at a wage of at least Rs. 100 per day. This employment guarantee is not the first program of such a scale to take place. Conditional cash transfers (CCT), such as *Bolsa Família* and *Oportunidades*, as well as the Public Distribution System (PDS) have taken place in Brazil, Mexico, and India, respectively. Utility theory suggests that in-kind transfers are less efficient in raising the utility of the poor than direct cash transfer programs, which let the targets of the programs decide how to spend all of their income. However, there have been concerns about the long-term outcomes of program beneficiaries, especially in the areas of health and education. Programs like *Oportunidades* combine a cash transfer with in-kind assistance by directly transferring money to beneficiaries and attaching conditionalities to the transfer, such as attendance at school or regular family health checkups.

Although NREGA is a public works employment program, it can also be thought of as a sort of CCT that transfers money directly to laborers conditional on fulfillment of a requirement. Whereas in *Oportunidades* the requirement is school attendance, health clinic visits and nutritional support, a NREGA unskilled laborer must work on infrastructure development projects in their own village. In the same way that CCTs like *Oportunidades* aim to shape specific long-term outcomes such as education and health through cash transfers, NREGA focuses on improving village infrastructure as a public good. Workers are able to physically develop their own villages and pave the way for economic growth and poverty reduction at home. Several studies have discussed the impacts that infrastructure development can make on the economies of marginalized villages (de Janvry, Fafchamps, and Sadoulet 1991; Binswanger, Khandker, and Rosenzweig 1993; Fan, Hazell, and Thorat 2000; Narayana, Parikh, and Srinivasan 1988).

Besides rural infrastructure development, NREGA directly aims to achieve three broader goals in rural areas. The first, and according to the government the most important, is to enhance the purchasing power of poor laborers. Drèze studied closely a government response to the severe drought in Maharashtra in 1970-73 known as the Employment Guarantee Scheme (EGS) (Dreze, 1990). He concluded that diminishing purchasing power by the poor in the face of famine was of larger concern than actual limitations in food availability due to market imperfections. In a review of the history of famines in India, Drèze cites a 19th century report noting "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" (p 17). And "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" (p 25). NREGA guarantees work to laborers who either lose their seasonal work in bad years or who simply cannot make ends meet during typical slack agricultural production periods, when work is low. Thus, in addition to guaranteeing a job, NREGA also pays minimum wages to ensure that the poor maintain their purchasing power in bad seasons.

A second goal of NREGA is the enforcement of minimum wages in rural areas. The Indian Minimum Wages Act of 1948 was created to ensure a subsistence wage for workers, with each state of India determining their own minimum amount of income needed to stay out of poverty. The legal wage is increased at least every five years to keep up with subsistence requirements in real terms. In rural India the structure does not exist to ensure or enforce the payment of minimum wages, especially on farms. Moreover, with an economic environment that can change quickly along with increasing volatility in food prices, the minimum wages themselves are often not updated frequently enough. NREGA incentivizes the minimum wage payment by covering the wages of unskilled workers using the federal budget while putting the onus on local governments to cover unemployment benefits for those in their constituency. Local governments have a financial incentive to implement NREGA and keep unemployment low in their villages.[1]

Finally, NREGA tried to incorporate from the Maharashtra EGS methods to deal with targeting and selection issues in this transfer program. The EGS was able to target those most vulnerable to drought-related income collapses by locating offices in rural areas and requiring regular attendance. This way, officials could be sure that those with the lowest opportunity costs would select

[1]Wage seekers have the right to unemployment allowance from their local government in case NREGA employment is not provided within 15 days of submitting the application or from the date when NREGA work is sought.

themselves into the treatment, ensuring both the objectives of getting aid to those who are of highest risk of starvation and also avoiding elite capture.[2] Thus, the structure of NREGA reflects the successes and lessons of the Maharashtra EGS, particularly in the types of works undertaken and the method of implementing the program.

2.2 Employment Guarantee and Agricultural Labor Markets

Though the theoretical literature on guaranteed employment and rural labor impacts are scarce, ongoing empirical analyses of NREGA's effects in the labor market have shown mixed results, with most studies estimating positive impacts on agricultural wages due to NREGA. For example,Imbert and Papp (2013) find both a 5.5% increase in agricultural wages and crowding out of private sector employment. Berg et al. (2012) find roughly 3% increases in agricultural wages with about 6-11 months for this impact to manifest itself on farms that hire casual labor. Azam (2012) saw an 8% increase in female agricultural wages but only 1% for men.

All these studies used difference-in-differences estimation to find increases in agricultural wages of between 3-5%, while highlighting private sector impacts only during the dry season and gender-neutrality in impact distribution. Shah (2012) estimated a 6.5% increase in agricultural wages and additionally found that a one standard deviation increase in infrastructure development due to NREGA leads to a 30% reduction in wage sensitivity to production shocks. Zimmermann (2012) uses a regression discontinuity design and finds agricultural wage increases for women only during the main agricultural season and no effect on private employment so no change in labor force makeup.

Most of these studies do not develop theoretical models explaining how an employment guarantee should impact agricultural wages. Of those that do, Imbert and Papp (2013) draw heavily from earlier models showing the distributional effects of price changes on consumption goods by simply replacing the latter with labor markets. Zimmermann (2012) uses a very simple minimum

[2] Narayana, Parikh, and Srinivasan (1988) also discusses the topic of elite capture in the EGS and show that a program carried out efficiently, targeted effectively and financed properly is effective in alleviating poverty in India.

wage model and adds labor rationing to generate the hypothesis of increased agricultural wages.

During India's original employment guarantee in Maharashtra in the 1980s, most studies of the effects were theoretical and not empirical. Narayana, Parikh, and Srinivasan (1988) stylized the Indian agricultural labor market by separating demand into peak and lean season. They then show how the EGS changes the market. This is shown in Figure 1. The amount of labor up until point L is the labor supply available to work at the going lean season wage, w_L. Before the EGS, the only demand for rural labor is assumed to be for agricultural purposes. With the lean season labor demand curve, D_L, workers are only hired until point L, leaving $L - L_L$ excess labor in the lean season (and full employment at L_P in the peak season). With a limited employment guarantee, total lean season labor demand now shifts out to, $D_{L'}$, putting the total lean season labor equilibrium at L_T. One can see that, in this analysis, it is inconclusive and depends on the magnitude of the shift in D_L whether or not agricultural wages increase. As long as L_T is less than L, i.e., excess labor is not totally exhausted by the public works program, there will be no effect on agricultural employment (still at L_L) or workers' agricultural income ($L_L \times w_L$). But workers will now be gaining $(L_T - L_L) \times w_P$, where w_P is the officially set public works wage. The peak season equilibrium, (L_P, w_P) is also unaffected.[3]

Osmani (1990) sees the agricultural wage determination process in India differently. He argues that farm workers collectively determine the equilibrium wage via repeated wage-setting games. The equilibrium wage becomes higher than the competitive wage due this "implicit cooperation." Workers ask for a wage above their opportunity cost and employ a "trigger strategy" that penalizes any worker who undercuts theme by accepting a lower wage. The success of this strategy and the value of the initially requested wage depends on the opportunity income of the worker. A requested wage must at least be higher than what one would make outside of agriculture but not so high that a worker would be willing to incur the penalty of the trigger strategy. In the Osmani model, an employment guarantee would serve as a boost in opportunity income or increase in c_1 to c_2 (see Figure 2). This pushes up Osmani's equilibrium wage interval, which has c as its lower

[3]Even in the case where $w_P \leqslant w_N$, there should still be no affect on the peak agricultural labor market because both EGS and NREGA intend employment to only be offered during the lean agricultural season.

bound. But it is not clear if this changes e. The equilibrium wage is characterized either by an interior solution within the wage interval or the maximum interval value, m. If the original wage is an interior solution to (c_1, m_1), such as e'', then a boost in the opportunity income to c_2 does not necessarily have an effect on the equilibrium wage. If the original solution was e', however, the agricultural wage will get pushed up from e' to at least c_2. A third scenario is if the equilibrium wage is initially the maximum value of the interval, m_1, and then can either stay there or move to m_2 with the change in opportunity income. Osmani cites several factors that determine this interval and where exactly the equilibrium wage falls that include a worker's time discount factor and subjective probability of employment.

Basu (2011) develops a theoretical model of an employment guarantee that predicts impacts on output and labor markets. His model features a mutually exclusive choice by laborers to work either in a year-round permanent contract with a landlord or as both a public works employee during the lean season and casual agricultural laborer during the peak season. He finds that 1) an increase in the public works wage results in a decrease in agricultural labor and increase in the casual wage rate, if certain public and private productivity levels are met, and 2) a technological improvement can also increase the casual wage rate. Although Basu was able to conclude that agricultural wages increase due to an employment guarantee, the results are highly dependent on a highly stylized specification of the Indian labor market. The existence of permanent labor is important in the model, but it is not necessarily applicable to all rural Indian contexts, especially the poorest ones. The author also assumes that workers cannot perform lean season agricultural work and public work at the same time.

Nevertheless, Basu does use his model to consider the impact of an EGS on agricultural employment and wages under different labor market specifications. For example, he shows that a landlord who is confronted with a minimum wage, $\bar{w}$, but simply wants to pay workers their reservation wage, w_r, will result in a game theoretic problem between two types of workers, high-wage and low-wage, both of whom are represented by separate labor unions that can contest agricultural wages against the other group in a non-cooperative way. This is an extension of Osmani's implicit cooperation model. But again it is highly stylized: the existence of labor unions was more specific to the Kerala case at that time and not generalizable to the Indian context as a whole, especially poorer states. The results of the game theoretic extension results in upward pressure on agricultural wages. When there exists an additional permanent versus casual labor distinction, Basu builds on previous tied-labor literature to argue that an EGS wage that offers more than the lean-season casual labor wage would induce more permanent labor contracts, which would be beneficial to those who get the contract. This is because the EGS increases the cost to the landlord of hiring casual workers during the lean and peak seasons as needed and makes the purchasing of permanent worker contracts across an entire year more attractive. This would mean less employment for some of the poorest workers in the economy who are casual but better employment in terms of permanent contracts for others.

2.3 Technology Adoption

The literature on determinants of technology adoption has evolved substantially over the last few decades. Three survey studies capture the transition. Feder, Just, and Zilberman (1985) reviews technology adoption models that discuss the role of land tenure, farm size, uncertainty, and information. The authors caution against a trend in the literature at the time of "nonexistence of government policies in most adoption models" (p 288), which can affect relative input and output prices and, therefore, technology choices. Besley and Case (1993) critique time-series adoption models for being too broad in nature and less useful for determining individual adoption practices. But they also note that most cross-section empirical studies ignore adoption dynamics and focus only on the correlation between farmer characteristics and final adoption. The authors suggest a more a balanced approach and highlight dynamic optimization studies that model state dependence between periods and test adoption practices using panel data. They conclude that most of the previous studies do not account well for factors such as information and access to credit. Finally, Foster and Rosenzweig (2010) highlight in their more recent survey on technology adoption other important adoption constraints, including credit, insurance, information, economies of scale, risk preferences, and behavioral processes.

Most of these surveys and studies do not explicitly address the role of labor availability in technology adoption. Hicks and Johnson (1979) and Harriss (1972) examine the effect of high and low rural labor supplies, respectively, on the adoption of labor-intensive technologies, but the effect of either of these on labor-saving technologies has not been rigorously studied with data. Empirical evidence cited by Feder, Just, and Zilberman (1985) demonstrates that uncertainty in the availability of labor does indeed lead to the adoption of labor-saving technologies. And Spencer and Byerlee (1976) examine technical change and labor use in a farming area of Sierra Leone that is characterized by large quantities of land and small amounts of labor. Labor supply constraints are shown to be overcome by adoption of mechanical production techniques in rice-growing areas. But it is not clear if the opposite conclusion can be made for the other end of the land-labor ratio spectrum, which is more characteristic of countries like India.

It is clear that the role of labor availability was a topic in much earlier studies of technology adoption. But the discussion of determinants has moved away from this towards previously lesser known issues, such as finance, information and risk. Empirical work on technology adoption has thus shifted towards changes in these explanatory variables and consequently found interesting results with many policy implications. This research fills a gap in recent literature by re-examining and re-modeling the role of labor availability in technology adoption. I begin with threshold models developed by Sunding and Zilberman (2001) and Just and Zilberman (1988) that use changes in (expected) profits as triggers for adoption. These profits are thought of abstractly in these studies with discussion often alluding to changes or uncertainty in output prices or learning. I develop the threshold model to explicitly account for changes in labor markets and restrict the outcome to labor-saving technologies in order to capture the theoretical effects of NREGA.

2.4 Infrastructure Investment

Finally, I review some of the literature on infrastructure investment and discuss how this relates to a public works employment guarantee's effect on both agricultural labor markets and technology adoption in the long run.

Binswanger, Khandker, and Rosenzweig (1993) look at links between investment decisions of governments, financial institutions and farmers in 85 districts across 17 states in India. They measure both the impact of investment by these entities on infrastructure development and the joint impact of all investment on agricultural output and productivity using district-level, time-series data. Addressing the simultaneity of infrastructure improvements, financial investment and agro-climatic variables, the authors use fixed effects to identify the impacts of roads, primary schools and electrification on agricultural output growth, which were shown to have significant positive effects of 7, 8 and 2 percent, respectively. Private investment, such as on tractors, fertilizers, pumps, and animal purchases by farmers show mixed effects. The use of tractors by farmers increased 6% due to canal irrigation, whereas roads improved agricultural output 6.7%. These were both significant in affecting both agricultural input use and output levels, as well as encouraging private investment. Fan, Hazell, and Thorat (2000) show that rural roads and agricultural research have the highest per Rupee impact on poverty and productivity growth in India, with only modest impacts of irrigation, soil and water conservation, health, and rural and community development.

de Janvry, Fafchamps, and Sadoulet (1991) focus on the transaction cost wedge of rural villages and show pathways through which physical rural development can benefit the poor. These authors address the seeming paradox that peasant farm households do not respond to price changes in a way that is consistent with traditional economic theory and argue that it is the lack of infrastructure that keeps transaction costs high prevents price changes from reaching the most marginalized villagers. With a reduction in these transaction costs through infrastructure development, rural households will be more responsive to changes in their economic environment.

Narayana, Parikh, and Srinivasan (1988) released a study around the same time as Dreze's post-Maharashtra EGS analysis that looks at the potential of rural works programs in India that are similar to those of NREGA in that they provide work opportunities in roads, irrigation, and school building to unskilled labor during slack agricultural seasons. The authors show, using a sequential general equilibrium model, that these programs do not necessarily jeopardize long-term growth and can be effective in alleviating poverty. In addition to creating "demand for perhaps the only endowment the rural poor have, namely, unskilled labor," they claim that rural works programs "also improve rural infrastructure, thereby increasing productivity of land.

3 Model

This section brings labor and technology markets together to determine the theoretical short-run effects of NREGA. The model shows how a rural works program that raises agricultural wages impacts farm owner decisions in the technology sector by reducing the minimum farm size needed to cross the adoption threshold.

3.1 Technology Adoption

Emerging empirical evidence shows wages have increased due to NREGA. The farm owner may now reconsider previous labor-saving technology decisions. In Figure 3, a rural economy begins at point A, where the agricultural wage is equal to those in other rural labor sectors in the village, or $w^A = w^P = w^*$(for simplicity, I assume just two work opportunities: agriculture, A, and public work, P).

Due to NREGA payments, which far exceed the prevailing agricultural wages, the public works wage increases to w^N. Whereas before L^P workers would have accepted w^P (point B), now L^N laborers earn w^N (point C). More public works projects are undertaken in the village, and the agricultural labor supply curve shifts in. If, in the extreme scenario, NREGA's 100 days of employment can cover a worker's entire income for the year and if the worker is indifferent between public works and agricultural labor, then the agricultural labor supply curve shifts all the way to $S_A^{''}$ and results in a new agricultural equilibrium at Point D. The worker in this case must be paid at least w^N to work on the farm. However, NREGA work alone is not likely to satisfy a rural laborer's desire for employment. Thus, the new agricultural supply curve is likely to instead shift only to $S_A^{'}$, resulting in an equilibrium of Point E. This corresponds to an aggregate labor supply increase to $L^{'}$ and equilibrium wage of $w^{'}$ (Point F).

The quantity $L^A - \underline{L}$ is the notional excess demand for agricultural labor, defined as the difference between "the amount...that people would want to buy...if they ignored any constraints on the quantity of other goods they were able to buy" (DeLong, 2010) and the amount they are actually able to buy given the constraints. As Muellbauer and Portes (1978) point out, "an agent who is rationed as a buyer or seller on one market and cannot transact his notional excess demand there will in general alter his behavior on other markets" (p. 789). This is depicted at the bottom of Figure 3 where the demand for labor-saving agricultural technology shifts out until the marginal value products of labor and technology are equal at the new agricultural labor allocation. If farm owners cannot satisfy their excess notional demand for agricultural labor, this affects their activity on the technology market.

3.1.1 Threshold Model

The farm size technology adoption threshold of Sunding and Zilberman (2001) shows one channel through which this activity may occur, linking NREGA, agricultural wages and technology adoption. Though the threshold model is intended to describe diffusion over time, it can also capture farmer heterogeneity of technology adoption at a given point in time. Adoption takes place above a certain cutoff farm size, H_j^c, which depends on fixed costs, F_j, and the difference in profit, $\triangle\pi_j$, for technology j compared to the incumbent technology:

$$H_j^c = F_j/\triangle\pi_j. \tag{1}$$

Figure 4 shows a pre-NREGA farm size threshold curve that increases in F, keeping $\triangle\pi$ constant across all technologies j for simplicity. The technologies that increase with F are categorized on the x-axis as hand-, animal- and machine-powered technologies. Due partly to the active custom-hire technology markets in India, much of the fixed cost of technology adoption captures information and learning. Thus, there is little or no fixed cost near the origin where farmers use no technology or most basic hand-powered implements, while machine-powered implements, such as combine harvesters or direct-seeded rice, require the most information and learning.

In the denominator of equation 1, I explicitly incorporate the opportunity cost of agricultural labor, w^A, to obtain

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

Assuming F is does not change due to NREGA, then the effect of the program will show up via w^A and, consequently, through $\triangle\pi_j$, where π_j^1 is the profit when adopting technology j and π_j^0 is profit from using the incumbent technology associated with technology j. The largest changes in $\triangle\pi_j$ (and, therefore, on H_j^c) will occur for technologies closer to the origin of Figure 4, that is, when a farmer switches either from no technology to a labor-intensive one or from a labor-intensive technology to a labor-saving one. This is because in these cases π_j^1 and π_j^0 will provide the most separation from each other as w^A changes.

As an example, for a farmer considering using many workers equipped with hand hoe technology to turn over soil on a field, an increase in w^A due to NREGA causes $\pi^1_{hand\,hoe}$ to decrease, since profits under a labor-intensive technology are highly dependent on agricultural wages, and $\pi^0_{hand\,hoe}$ to be unaffected, since agricultural wages are not being paid for a fallow plot. Thus, $\triangle\pi_{hand\,hoe}$ decreases by the change in $\pi^1_{hand\,hoe}$ from period before NREGA to the period after. A farmer already employing workers with hand hoes and considering a switch to a labor-saving animal-drawn wooden plough will observe a slight decrease in $\pi^1_{wooden\,plough}$ since labor-saving technology is relatively less dependent on agricultural wages, and a large decrease in $\pi^0_{wooden\,plough}$, or the profit under the wooden plough's incumbent technology, hand hoes, due to higher unskilled wages resulting from NREGA. As the farmer moves further along the x-axis, the relative changes in profits from new technologies will decrease as the technologies under consideration become less dependent on unskilled agricultural wages.

The effect on the farm size threshold for various technologies is shown in the post-NREGA curve in Figure 4. For hand-operated implements, the farm size threshold increases making adoption more difficult for small farmers. For animal-powered implements, the farm size threshold decreases because higher wages make labor-saving technology more profitable and labor-intensive operations more expensive. The change in profits when adopting machine-powered implements to replace animal-powered ones is likely to be the smallest when NREGA's impact is channeled only through agricultural wages. While using a tractor to plough one's field is arguably more profitable than using oxen, this difference in profit does not likely change due to higher agricultural wages, as compared to choosing oxen over a field full of workers with hand hoes. So the change in the farm size threshold for machine-operated implements due to NREGA's impact on wages is not likely to be very high.

One benefit of the 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 many technology adoption studies are done in the large farm context only. However, since fixed costs are mostly held constant in this analysis, it is possible to show a similar result on small farm technology adoption without them, such as in a labor-cost supervision model. The next section discusses the empirical approach for testing these theoretical implications.

4 Empirical Strategy

There are several approaches one could use in estimating NREGA's effect on technology adoption. I first consider ordinary least squares (OLS) but argue that endogeneity of technology adoption decisions with NREGA treatment will lead to biased results, since the poorest districts received the program in the first phase. Most NREGA studies have relied on difference-in-differences (DD) to identify causal impacts on other outcomes, such as agricultural wages and nutrition. I consider both a general DD specification and fixed effects model. I discuss the validity of these estimates given the non-random assignment of NREGA across districts. Finally, I present a regression discontinuity design that, in contrast to OLS and DD methods, takes advantage of the progressive rollout of the program by evaluating differences in outcomes at the arbitrary Phase I treatment cutoff.

4.1 OLS

In order to obtain a first rough estimate of the impact of NREGA on technology adoption, I consider a simple OLS model with district-level controls:

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

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 received NREGA in year t, and X is a vector of district-level controls. This will capture the effect the NREGA program has on technology adoption in district i if the expected value of the error term is zero, or $E(\varepsilon_{it} \mid X_{it}) = 0$. However, this is unlikely to be the case if districts that are more likely to adopt the technology are also less likely to be poor and, therefore, also less likely to be a first-phase NREGA village. The econometric concern is endogeneity where technology levels in the district also determine whether the village is likely to receive NREGA treatment. There is also a high chance of serial correlation in outcomes over the years before and after implementation of NREGA.

OLS estimates of the effect of NREGA participation on technology adoption ultimately will be biased. To address this, I employ two econometric techniques: difference-in-differences (DD) and regression discontinuity design (RD), the second of which relies on changes in adoption rates in the districts that were above and below the cutoff index value that determined the dispersal of NREGA funds during the initial rollout. Estimates from these two approaches will be compared to each other and the OLS approach.

4.2 Difference-in-Differences & 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) both before and after the program takes place. The specification is

$$TA_{it} = \alpha + \beta NREGA_{it} \cdot post_t + \gamma post_t + \delta NREGA_{it} + \varepsilon_{it},$$

where TA is the percent of farms using labor-saving technology in district i and year t, $NREGA$ is a dummy variable equaling 1 if the district has implemented NREGA in year t, and $post$ is a dummy variable equaling 1 for observations after the beginning of the program. Covering the number of farms using technology into a percent controls for differing numbers of farms in different districts, while right hand side specification accounts for both varied initial levels of technology use in districts and general trends over time.

Equation (**??**) can be improved upon with panel data by including district fixed effects. The panel fixed effects equation is

$$TA_{it} = \beta NREGA_{it} \cdot post + \gamma_t + \delta_i + \varepsilon_{it}, \tag{3}$$

where now γ is a post-NREGA dummy representing the time fixed effect and δ is a district-level fixed effect for each district i. The main coefficient of interest in Equation 3 is β, which gives the treatment effect of NREGA on technology adoption net of time trends and time-invariant district characteristics.

I use this within estimator to counter endogeneity concerns of both OLS and a general difference-in-differences specification since selection into NREGA is not random. The 200 poorest districts that first got NREGA may have unobservable time-invariant characteristics that affect their technology adoption practices. However, there may be time-varying characteristics that do affect groups differently. All previous NREGA studies have found evidence for common trends between the two groups, using placebo tests, cubic and quartic time trends, and a variety of controls. I do not test for parallel trends in this study, opting instead for a regression discontinuity approach that does not require the common trends assumption.

4.3 Regression Discontinuity Design

The regression discontinuity (RD) method does not require exogeneity of the treatment variable with the outcome. RD solves this identification challenge by assuming that villages around a treatment threshold are the same in all characteristics except for a certain exogenous factor which assigns the treatment to some and not to others. Lee and Lemieux (2009) 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."

The RD equation takes the form

$$TA_i = \alpha + \beta NREGA_i + \gamma rank_i + \delta rank_i^2 + \eta NREGA_i rank_i + \lambda NREGA_i rank_i^2 + \varepsilon_i, \tag{4}$$

where the dependent variable is the technology adoption rate in district i after NREGA has been implemented, and $\alpha = TA_0$ is the estimated percent of non-NREGA farms adopting labor-saving technology at district 200 cutoff. $\beta = TA_1 - TA_0$ is the treatment effect of interest, and *rank* is what determines the cutoffs for each phase based on the BI. In Section 6, I will subtract 2004 baseline technology adoption rates from the dependent variable in some estimations because of the potential reduction in the estimator's sampling variability that can occur with the inclusion of pre random assignment observations on the dependent variable (Lee and Lemieux 2009).

The interaction terms in equation (4) allow the pooled regression function to differ on both sides of the NREGA 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 threshold are appropriately weighted or the window of observations is restricted to the districts that make more natural treatment-control groups, due to similarity in characteristics before the program. In this study, I will weight observations away from the cutoff using a triangle kernel and also consider several windows around the threshold.

RD does not require that the variation in the treatment variable be exogenous to the outcome of interest. It is important, however, that the threshold variable of a RD specification be non-manipulable by the beneficiaries of the treatment. This can happen in the case of government healthcare for low-income individuals, for example, where employers may pay individuals slightly less in order to avoid private healthcare costs, thus contaminating the the treatment and control groups for comparison on either side of the threshold level of income. In the case of NREGA, the threshold is the Planning Commission's Backwardness Index (BI), which ranks the 447 poorest districts in India using wages, productivity and SC/ST[4] population percentage from the early and mid-1990s. The first 200 districts in the BI received NREGA funds in 2006, while next 130 began the program in almost two years later (see Figure 5). Because the government used measures from the 1990s to determine whether villages received NREGA treatment in 2006, this threshold variable does not appear manipulable. Without any knowledge that NREGA would exist a decade later, it would not have been possible for district governments to manipulate their development indicators in the 1990s in anticipation of the program.

I use a fuzzy RD design because, although districts theoretically become part of NREGA in a deterministic way solely dependent on their rank, i.e., $NREGA_i = f(rank_i)$ and they cannot manipulate the threshold variable, the correlation between ranks under 200 and NREGA participation is not one-to-one. This is most likely a consequence of many states having been politically assured NREGA participation to their poorest districts, regardless of whether those districts were below the cutoff. I discuss this in more detail below using graphical depictions.

[4]Scheduled Caste/Scheduled Tribe

5 Data

The data for this study comes from the Ministry of Rural Development's Agricultural Census Input Survey (ACIS). Figure 5 indicates when the data was collected. While NREGA was being rolled out in three phases, the ACIS data was collected in three periods of its own in 2006-2007. In the first period, the number of farm holdings in each district was recorded and tabulated by size, gender and social group. At that point in each district the block-level (or *tehsil*) was randomly selected. A *tehsil* is an administrative unit at the sub-district level consisting of many villages. Each *tehsil* then had 20% of its villages randomly selected (100% of villages for small states), and, finally, the input survey itself was conducted for the farms within the final list of villages, ensuring that each village had at least four farms for each of the five farm size groups: marginal, small, semi-medium, medium, and large. Enumerators enacted this final data collection phase after almost one year of starting the process in 2006, placing the actual data collected at early- to mid-2007.

Previous studies mostly use 2009 National Sample Survey (NSS) data, which restricts analysis to comparisons between Phases 2 and 3. Because my theoretical model predicts impacts at the poorest and smallest farm levels, comparisons at the Phase 1 and Phase 2 cutoff is preferable for this analysis. Furthermore, Phase 3 districts are likely not the best controls for the poorest districts in the country (those in Phase1) and because pooling Phase 1 and 2 districts together ignores the fact that Phase 1 districts received NREGA longer than Phase2 districts (see Figure 5). I use treatment and control groups consisting of Phase 1 and Phase 2 districts in a regression discontinuity framework, which this allows me to first trim the richest (and absolute poorest) districts in India before estimating impacts at lower levels of development.

I also make brief use of the 2004-2005 the India Human Development Survey (IHDS). This data has been used extensively, particularly by sociologists interested in nutrition and intra-household decision making in India. This gives me another panel for testing short-run technology adoption decisions using difference-in-differences and district fixed effects methods as comparisons to regression discontinuity results. Both IHDS and ACIS will soon be releasing their next rounds of data allowing me to test long-run implications of results generated here.

Table 1 shows ACIS data broken down by farm size. Each district in the sample has on average 123,000 marginal farmers, whose total acreage equals 2.5 or less. Despite making up 64% of all farms in the district, marginal farmers only cultivate 21% of total area. Conversely, the largest farmers in each district make up just 1% of farmers but cultivate 12% of all land. The average farm in this study is 4.2 acres, which is divided into just over two plots.

Figure 7 shows how technology use varies by farm size and technology type. As might be expected, marginal farms use all technologies the least compared to the rest of the farm size groups. For animal-operated implements, the difference in technology use by farm size is less clear for farmers not in the marginal group, i.e., cultivating over 2.5 acres. This may be the first evidence of a farm size threshold effect for animal-powered technology, where small to large farmers use roughly the same amount and marginal farmers lag behind. Machine-operated implements have a much clearer distinction between all farm size groups. Nearly half of all large farmers use tractors compared to about a third for semi-medium farmers and a quarter of all small farms. This suggests a potentially much higher farm size threshold for machines, which likely incur higher fixed costs and a greater scale on which to operate.

Overall, animal-drawn wooden ploughs are found in 45% of farms, whereas levelers and bullock carts are used in about a quarter of farms. The number of machine-powered implements are generally used less. Diesel and electric pumpsets are found in 12-13% of farms. As discussed in more detail below, water-related technologies adopted as a result of NREGA's heavy emphasis on water infrastructure can have a significant impact on labor use, which in turn can alter labor-saving technology adoption decisions. Both water- and energy-related technologies show a pattern of adoption across farm size similar to that of machine-operated technology.

6 Results

Table 2 compares the initial OLS, difference-in-differences, and panel fixed effects results. As discussed earlier, OLS results are biased because they do not account for endogeneity between technology adoption and participating in the NREGA program. Columns (3) and (4) contain results from the difference-in-differences specification. The first uses overall percentages of farms using labor-saving technology in each district in 2004 and 2007 (N=848) as the dependent variable. This approach yields a 10.3 percentage point increase in overall technology adoption in NREGA districts. This means that a district whose initial labor-saving technology adoption rate was 71.9%–the 2004 average rate–will now see 82.2% of its farms adopt labor-saving technology when NREGA is implemented. In column (4), the observations are disaggregated by the five farm size categories and clustered at the district level, yielding a 7.27 percentage point increase in farms adopting technology. When district fixed effects are included in Columns 5 and 6, the impact on aggregate district-year data increases to 14.9 percentage points and, with farm size controls, decreases to nearly 10. These are all much higher than the naive OLS estimates in columns (1) and (2).

I then estimate equation (3) separately for each farm size category in Table (3). The marginal and small farmer groups see higher impacts on labor-saving technology adoption of 18.5 and 12.2 percentage point increases, respectively. As farm sizes get larger, the effect becomes smaller and less significant. For the largest size group, however, the number of observations drops dramatically, as there are not many farms in the sample over 25 acres.

Before conducting the RD estimations, I look at two graphs that can help further describe the data. The first (Figure 8) shows the how the Planning Commission's Backwardness Index (BI) varies with the ranking assigned to each district in the country. This figure reveals that many of the most developed districts were not ranked in the BI. This does not matter as much when comparing Phase 1 districts to those in Phase 2 and 3 versus comparing Phase 1 and 2 districts to those in Phase 3, as would be required with NSS data. Clearly, districts ranked 400 and higher no longer become good comparisons for any group.

The top panel of Figure 9 shows density functions of BI rank for both NREGA and non-NREGA districts. While most of the districts fall within the first 200 if they are in NREGA and above 200 if not, there are tails for each group that overlap. This is due to imperfect assignment of NREGA according to rank. Kerala, for example, does not have any districts poor enough to rank below 200. When the poorest Kerala district receives NREGA, then districts just below the cutoff move to above the cutoff, for example, Gujarati districts that are more likely to fall under 200. Zimmerman (2012) discusses a potential alternate NREGA assignment algorithm that gives each state at least one NREGA district by first considering the district's rank within state. Here, I show how being nationally ranked in the first 200 (bottom half of graph) corresponds to one's normalized state rank, where the last district in each state to receive NREGA is assigned a state rank of minus one. State ranks of 0 and above indicate no NREGA treatment. Quadrants II and IV show compatibility with a district's national and state ranks. Quadrant I shows the districts that received NREGA treatment even though their rank was above the official cutoff. Similarly, quadrant III shows that the districts who didn't receive NREGA treatment even though the had rank below 200 are even more numerous. It may helpful to think of the long tail in quadrant II as districts in highly-developed Kerala, almost all of which were above zero, and the group of districts closest to the origin as Uttar Pradesh, a state with over 20 districts receiving NREGA treatment.

Figure 10 shows estimates of equation (4) for bandwidths between 40 and 90 districts. The selection of bandwidth is what determines the districts used in the analysis. Larger bandwidths include more districts away from the threshold, thus affecting the calculated probabilities of treatment, i.e., more districts are included in the calculation of the local linear regression but with triangle kernel weights that drop more gradually as observations get farther away from the cutoff. Smaller bandwidths mean fewer districts are included in the calculation of the estimated local linear regression with weights dropping more rapidly for points away from the cutoff.

Since, as discussed above, a fuzzy RD design will require a larger bandwidth than a sharp design in order to calculate probabilities of treatment at the threshold, regressions at bandwidths of 30 and lower are not able to generate predictions of treatment at the cutoff. The first bandwidth where the power is high enough is 40 districts, and I stop at 90 districts in accordance with the highly curved tails observed in Figure 8. Figure 11 graphically depicts two fitted curves on either side of the normalized NREGA cutoff using a 40-district bandwidth and a dependent variable of the change in percent of farms adopting labor-saving technology from 2004 to 2007. This picture stays consistent when considering the jump at the cutoff in 2007 alone.

Table 4 shows estimates of the jump at the cutoff for these different bandwidths. In this specification, I allowed 2004 adoption to be a right-hand side variable in order to not restrict the coefficient on it to one. The numerator for each of these bandwidths is the jump in the outcome variable at the cutoff, which is what would be the final estimate if the RD design was sharp. However, in the fuzzy design, the jump in the probability of treatment at the cutoff is used as the denominator of the final Wald estimate. Here, the results are negative and the "treatment" is switched to not receiving NREGA. So with the tightest possible bandwidth that allows for estimation of the treatment effect, one sees an 11-percentage point decrease in labor-saving technologies adopted by non-NREGA districts compared to NREGA districts. As in the case of the panel fixed effects estimates, the variation increases when more of the sample is included. However, here it renders the results insignificant at each bandwidth.

To combat this high variance problem, I take technologies on an individual basis to compute estimates of jumps. In order to determine which technologies to consider and what result to expect from NREGA, I consult Binswanger (1978) and Pingali, Bigot and Binswanger (1987). Both discuss how labor-saving agricultural technologies relate to mechanization and farming intensity, but the former is specific to India. In fact, Binswanger warns that much of it is specific to the agroeconomic conditions in Punjab.

The adoption of tractors and tractor-related machinery, including seeders and levelers, are perfectly labor-saving when the substitution view of Binswanger (1978) holds. That is, the only reason for adoption of this equipment is factor prices or factor scarcity. On the other extreme, this sort of mechanization would not be labor-displacing and would be considered net contributing in that it achieves intermediate products and yields that are unattainable by labor, such as deeper tillage or higher precision. Net contributing technologies could also increase the speed of operations, allowing for a greater range of potential cropping patterns. This latter sort of technology might even lead to additional labor usage for any farm operations not performed by machines, such as land preparation, planting, weeding, chemical spraying, fertilization, harvesting (if not already mechanized), threshing, marketing, and transportation.

Tractor-powered machines used for tillage, irrigation, threshing, sowing, and transport are most likely (Pingali, Bigot and Binswanger 1987). However, the order of mechanization for land-scarce areas would first intensify water use by upgrading to diesel and electric pumpsets, which are labor-saving holding land amounts fixed but could be labor-intensive if farmers expand into marginal lands because of better irrigation. Mechanical mills, tillage and transport equipment follow, but threshing is generally not mechanized where wages are low and harvested volumes are small. Weeding, interculture and harvesting continue to be done by hand in land-scarce economies where nonagricultural demand for labor is low. One would expect NREGA to increase mechanization for these technologies on the margin.

To look at individual technologies, I must use 2007 data only since the 2004 data is less-specific on the exact technologies being used. Figure 12 shows estimates versus bandwidths for select technologies. The top row shows hand-operated implements which one would expect to be more abundant on farms not affected by NREGA where labor is more abundant. For hand-operated seed drills, chemical sprayers and weeders, positive jumps are all observed. Changes in hand hoes for land preparation are mostly nonzero for NREGA districts, as they are for wheel hoes and blade hoes (not pictured) at various bandwidths.

Most of the key labor-saving animal-powered implements are adopted less in those districts not receiving NREGA. The first three graphs of the second row show wooden ploughs, traditional levelers and soil scooping all were adopted more in NREGA districts. Bullock carts, however, do not show a significant impact. This may be because bullocks had already been counted as those that pull ploughs and levelers.

Machine-powered implements show an interesting pattern. Almost all seem to be associated with non-NREGA districts indicating complementarity with labor-abundance. This may be a sign of increases on the intensive and extensive margin by farms and a net contributor view of labor-saving technology. Finally, it is interesting to note that more pumpsets and sprinkler irrigation are adopted as a result of NREGA. This could be due to the public investment in irrigation and water infrastructure in NREGA villages, as well as the abundance of labor needed to intensify farming as a result of improved irrigation.

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 building much-needed infrastructure in and around the poorest villages in the country, it can also alter short- and long-run equilibria in other areas of the rural economy, such as labor, technology, and agricultural output. This study theoretically models how incentives for agricultural technology adoption change due to NREGA's impact on the opportunity cost of agricultural labor and tests these implications empirically.

Using data collected during the phased rollout of the program, I use a regression discontinuity design to estimate changes in labor-saving technology adoption of around 20 percentage points, confirming the threshold model predictions of a reduction in the cutoff farm size associated with basic labor-saving technology adoption when agricultural wages increase. I find that this reduction occurs within the marginal and small farmer groups, and, while it is possible that participation in NREGA by small farming households creates income and credit effects that directly boost use of agricultural technology, the labor-saving nature of adoption suggests that at least some of NREGA's impact on technology is channeled through the increased opportunity cost of unskilled labor.

This research brings the analysis of NREGA closer to determining the long-run impacts on the poor. There is evidence so far that the rural poor's incomes are increasing, village infrastructure is improving, and agricultural wages are going up. I find that labor- and water-saving technology are also being affected. What remains to be seen is what the net impact of this will be on poor farmers and laborers in the long run. Continued reverberations between labor, technology and NREGA in the rural economy can result in a win-win for farm owners and laborers to the extent that the technologies adopted increase farm productivity and newly created NREGA infrastructure increases market access. However, with poor quality infrastructure and low levels of education and skill development, laborers could be worse off in a post-NREGA era. A focus on education, skill development, and quality infrastructure may augment the chances that the former scenario plays out.

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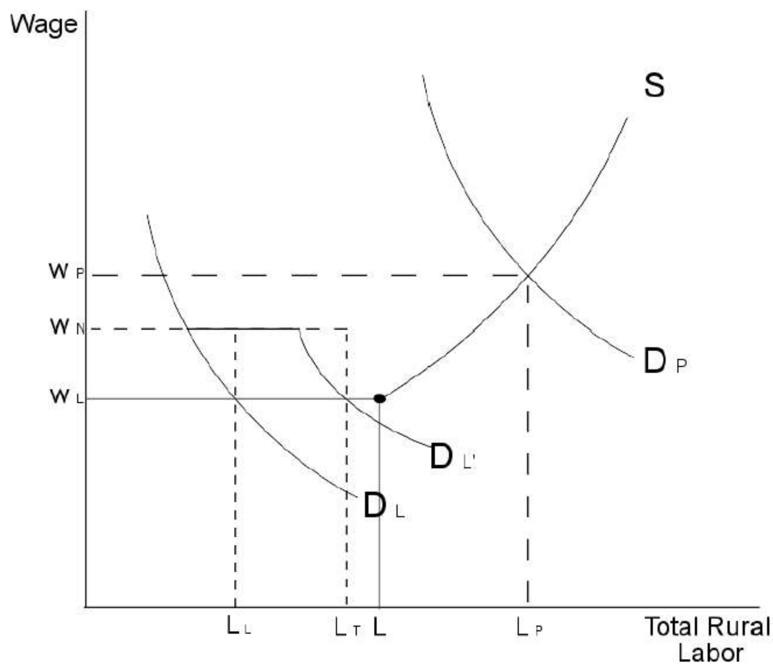

Figure 1: Agricultural and NREGA Labor Supply with Peak and Lean Season Demand (Narayana, Parikh, and Srinivasan, 1988)

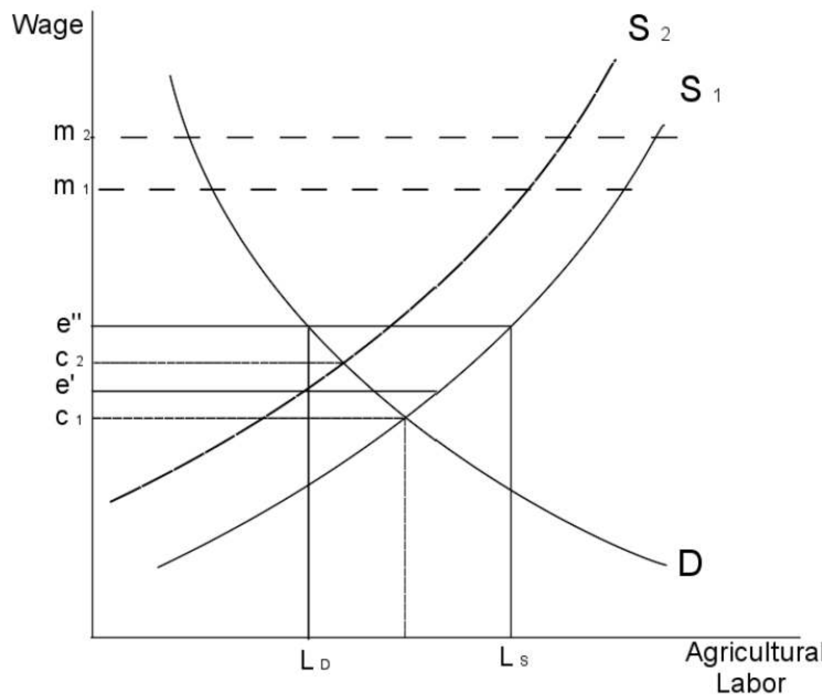

Figure 2: Implicit Cooperation Amongst Workers Leads to Equilibrium Wage Above Competitive Wage (Osmani, 1990)

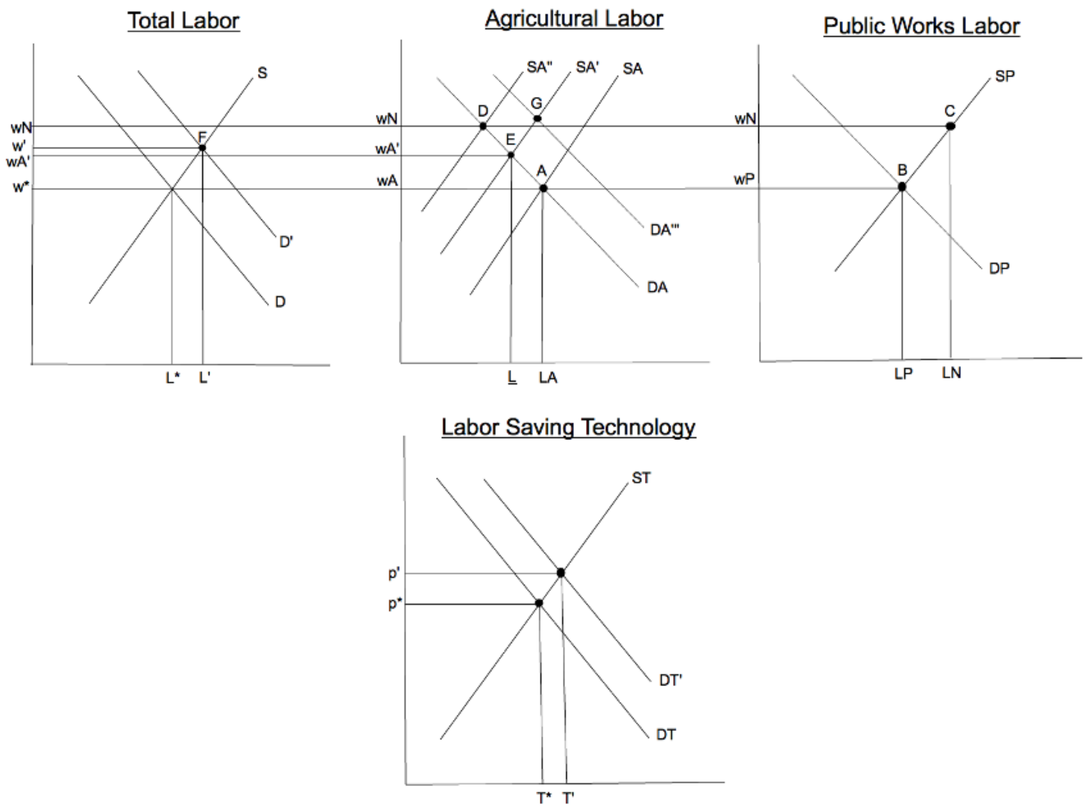

Figure 3: Short and Long Run Effects of NREGA on Rural Labor and Technology Markets

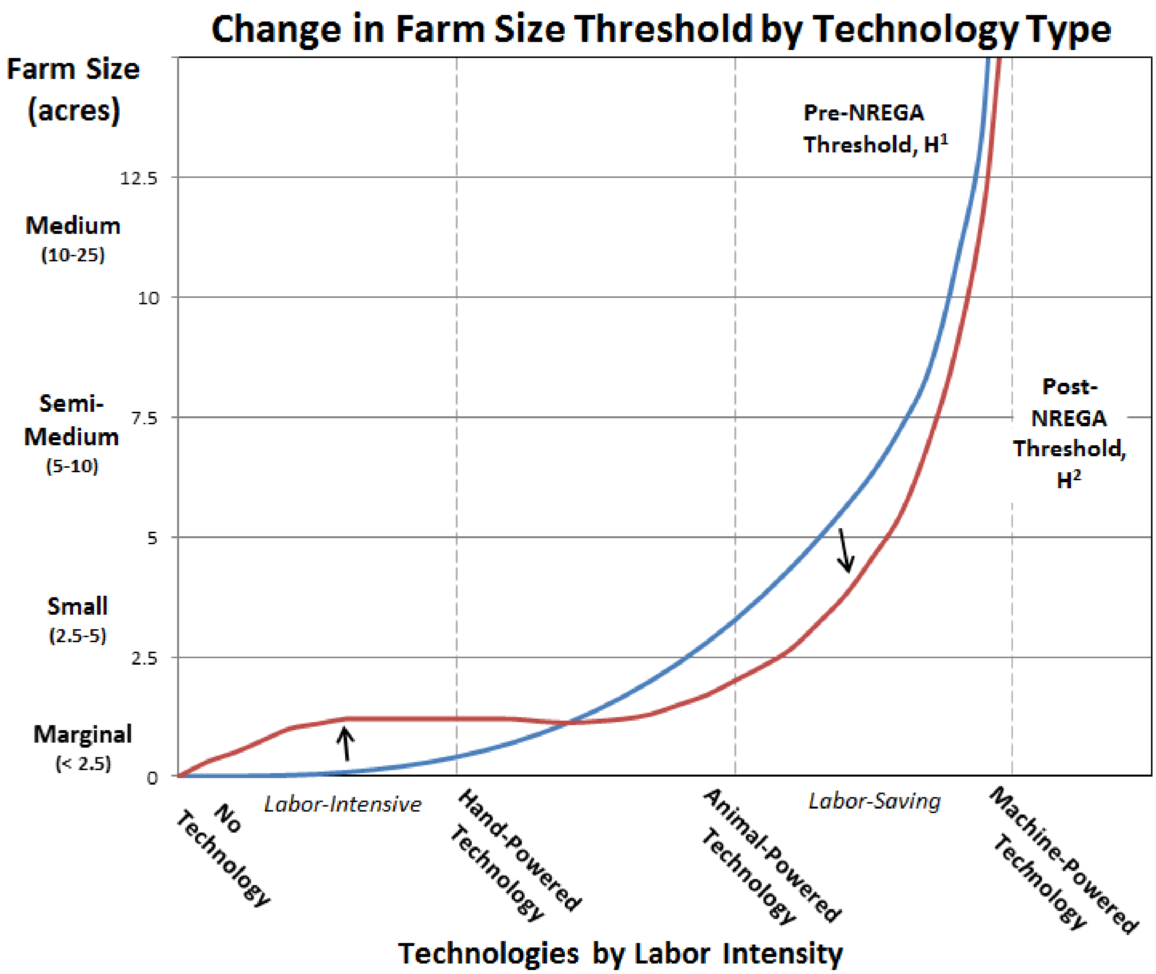

Figure 4: The technology adoption threshold increases for labor-intensive technologies and decreases for labor-saving technologies due to NREGA

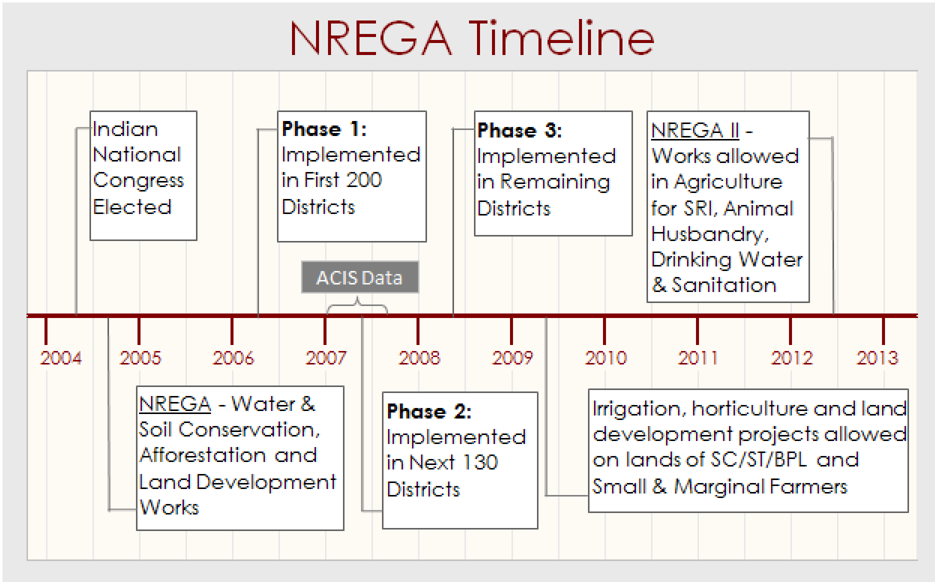

Figure 5: Evolution of NREGA. The Indian National Congress party was elected in May 2004 and passed NREGA by the end of the year. The first districts implemented NREGA in February 2006, one and a half years before Phase 2 districts. The primary data used for this study is from 2007. NREGA included mostly just public water- and land-related projects until 2009.

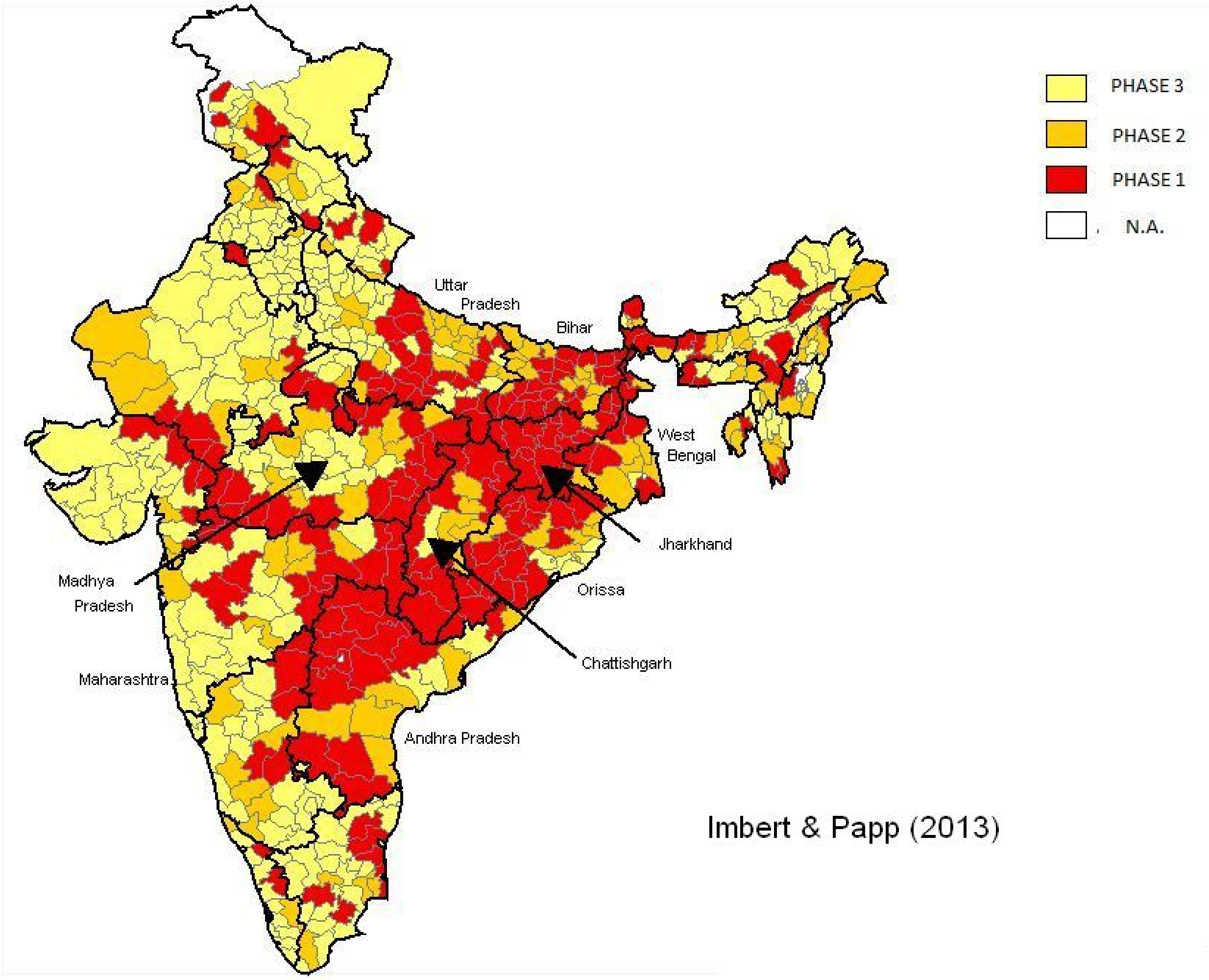

Figure 6: The Phased Rollout of NREGA Across India

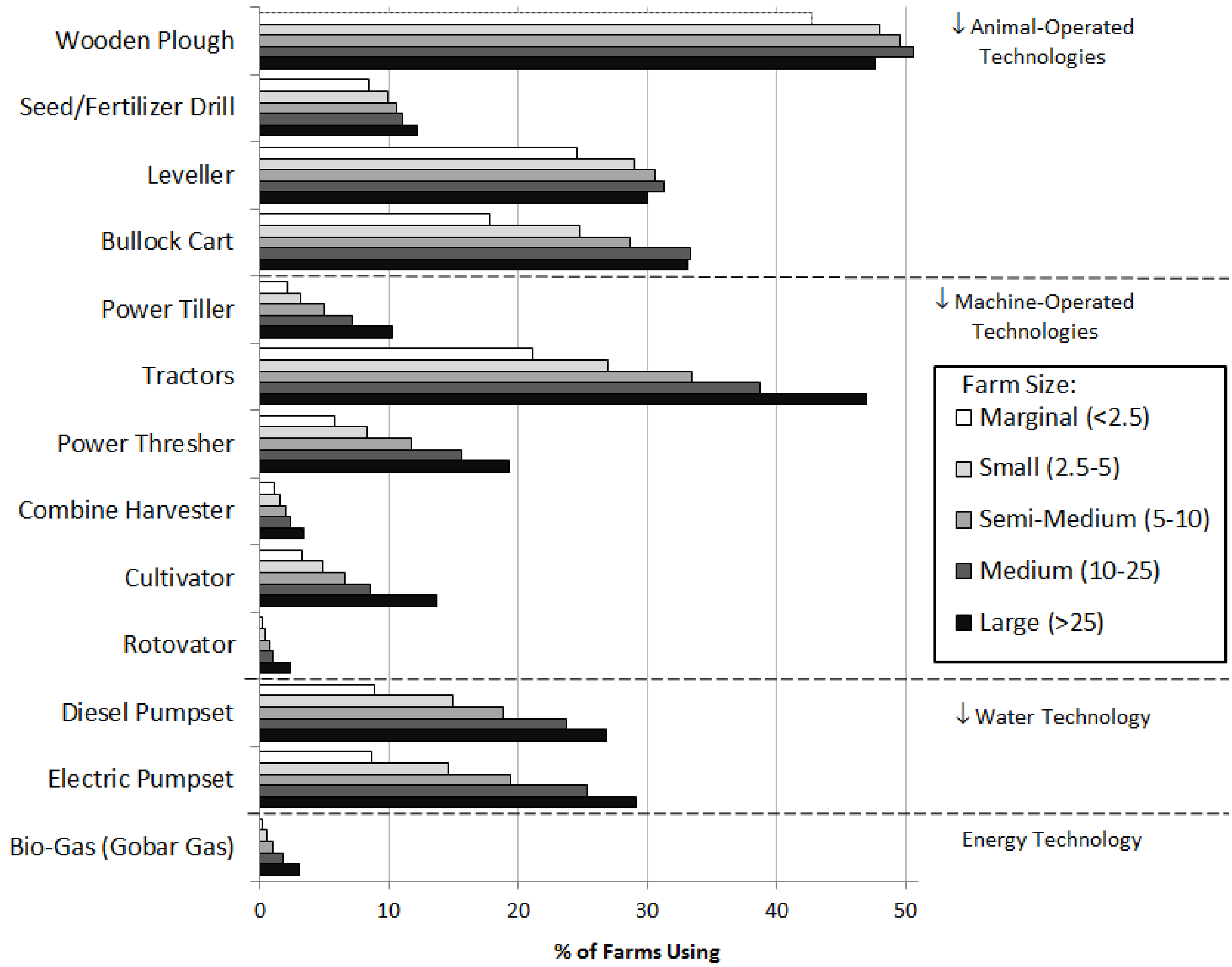

Figure 7: Differences in percentage of farms using specific technologies across farm size

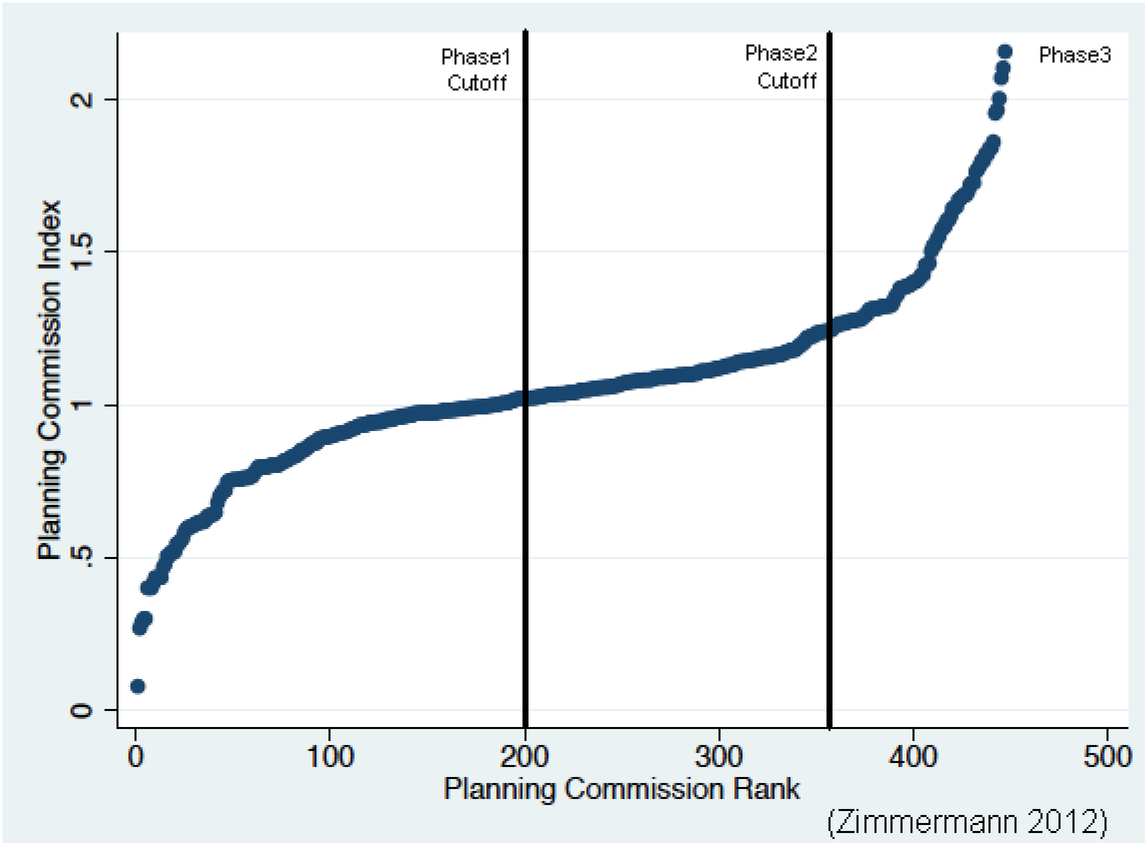

Figure 8: Distribution of Index over Ranks with Official Phase Cutoffs for Implementation

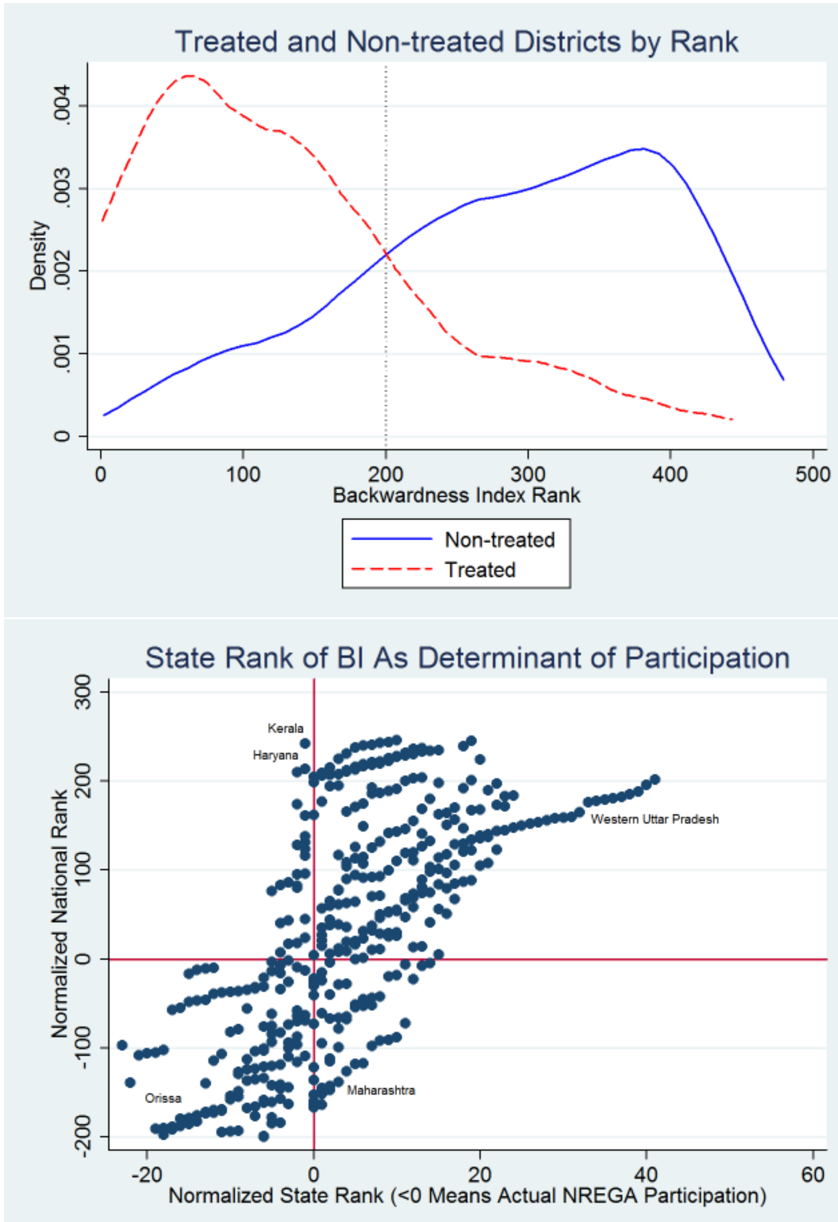

Figure 9: Fuzzy RD Design. Top panel: density of running variable (BI rank) for groups that received treatment versus those who did not. Some districts above the 200 district cutoff implemented NREGA. Bottom panel: the normalized national rank against state rank normalized to actual participation. Most states implemented NREGA in at least one district even if all ranks were above official cutoff (quadrant II).

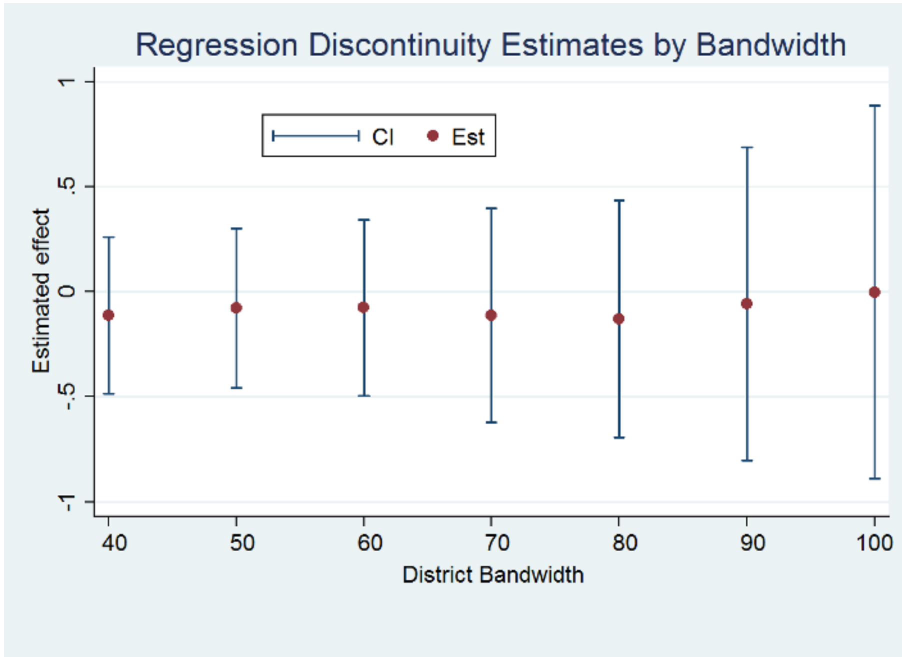

Figure 10: Overall estimates of NREGA effect on labor-saving technology using regression discontinuity design at bandwidths between 40-100 with confidence intervals

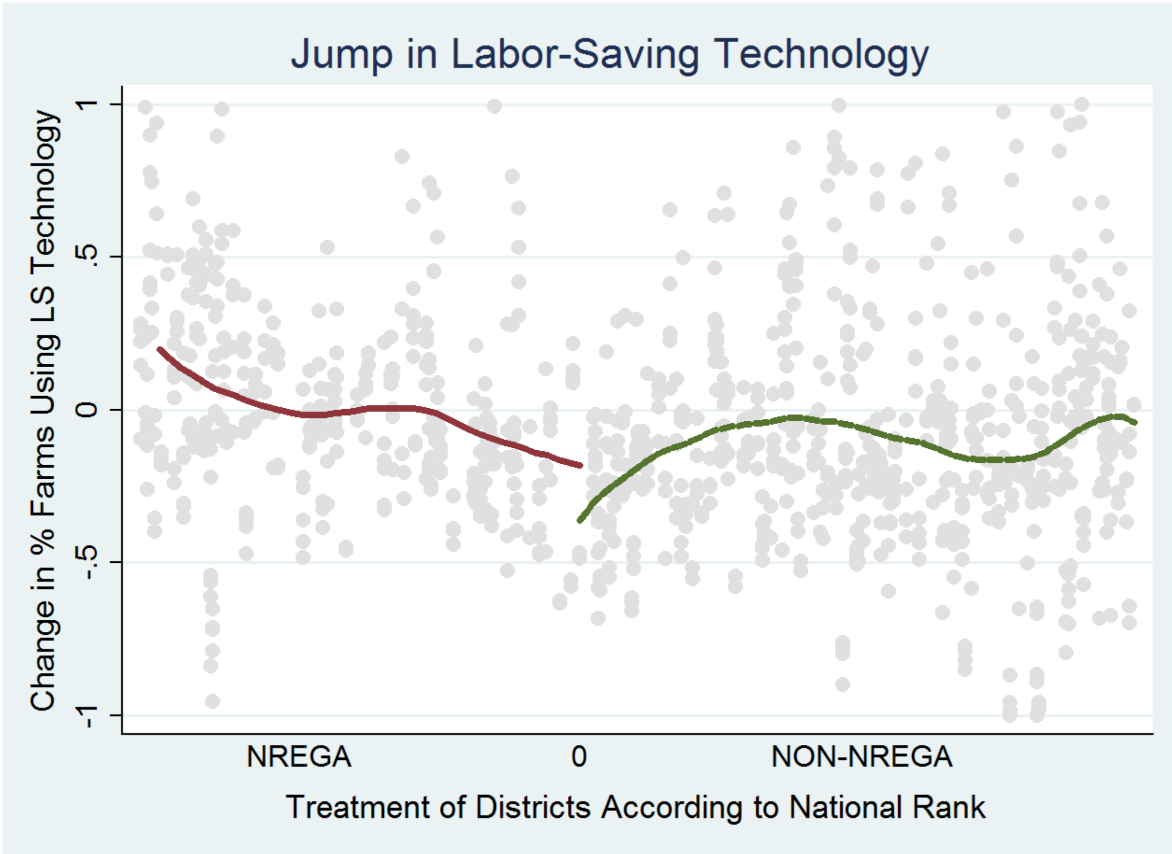

Figure 11: Curves fit to the left and right of the normalized NREGA cutoff. Y-axis measures change in percent of farms adopting labor-saving technology between 2004 and 2007.

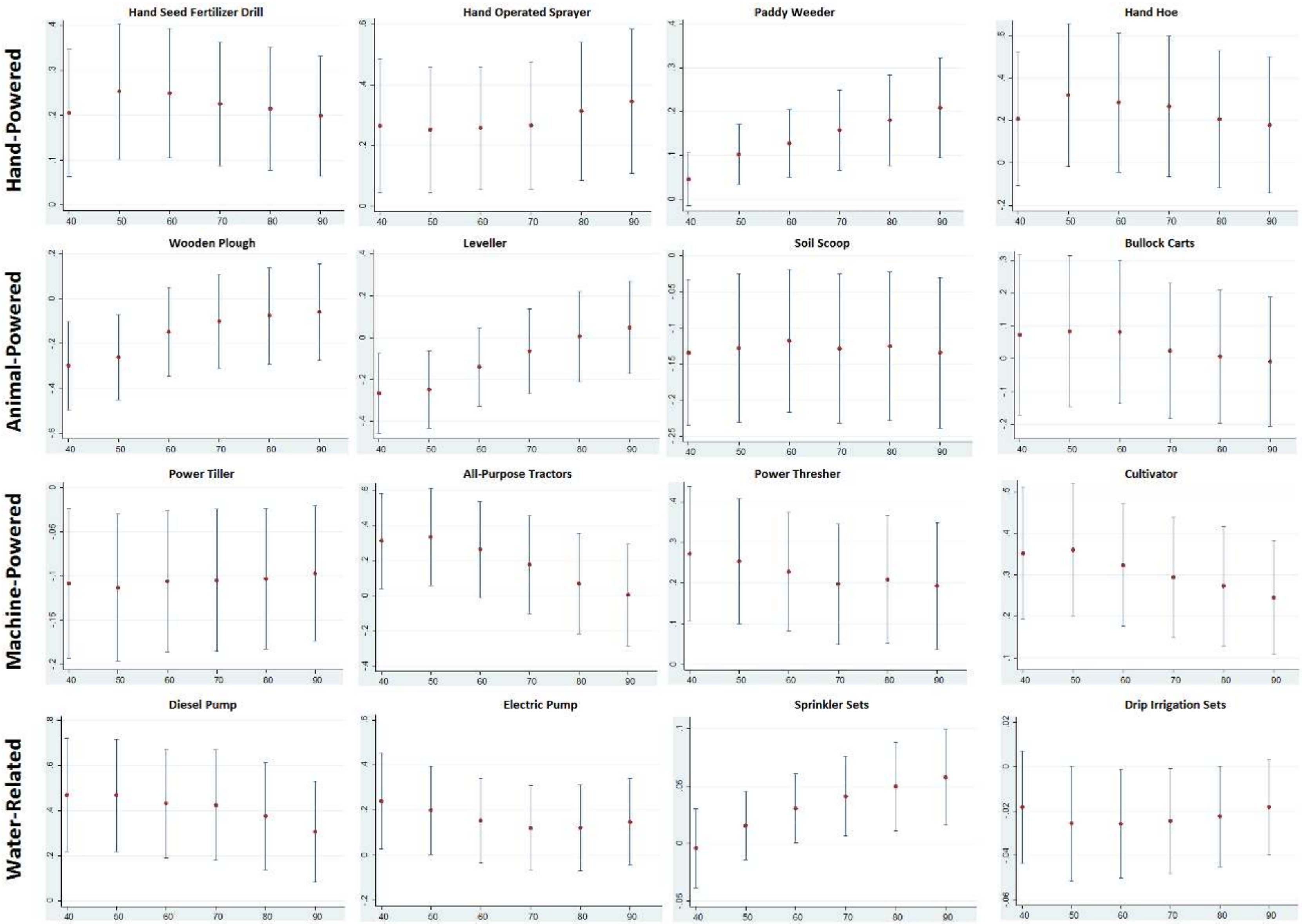

Figure 12: Estimates of jumps in technology use at NREGA cutoff by technology type

	Avg # of Farms per District	% of Total	Avg Acres Farmed per District	% of Total	Average Farm Size	Average Plot Size	Plots per Farm
Marginal (below 2.5)	123	0.64	128	0.21	1.21	0.86	1.67
	(139)		(130)		(0.30)	(0.41)	(0.92)
Small (2.5 - 5)	36	0.19	126	0.20	3.45	1.78	2.75
	(33)		(118)		(0.24)	(0.94)	(2.22)
Semi-Medium (5 - 10)	21	0.11	144	0.23	6.57	2.75	3.88
	(22)		(150)		(0.49)	(1.79)	(3.54)
Medium (10 - 25)	10	0.05	147	0.24	13.44	4.60	5.14
	(14)		(207)		(1.47)	(3.48)	(4.57)
Large (25 and above)	2	0.01	74	0.12	34.78	10.09	6.42
	(6)		(256)		(24.97)	(11.01)	(7.60)
All	192		619		4.16	2.29	2.33
	(169)		(628)		(3.90)	(2.46)	(1.45)

*Standard deviations in parentheses. Columns 2 and 4 are measured in thousands. N=371.

Table 1: Total farms and area farmed in India in 2007

	OLS		DD		Panel FE	
	(1)	(2)	(3)	(4)	(5)	(6)
NREGA	0.0664***	0.0673***	0.00462	0.0171		
	(0.0189)	(0.0177)	(0.0296)	(0.0262)		
Post			-0.122***	-0.0744***	-0.125***	-0.0585**
			(0.0229)	(0.0200)	(0.0289)	(0.0255)
NREGA*Post			0.103***	0.0727**	0.149***	0.0997**
			(0.0383)	(0.0345)	(0.0534)	(0.0422)
Constant	0.645***	0.618***	0.718***	0.665***	0.714***	0.657***
	(0.0113)	(0.0118)	(0.0183)	(0.0159)	(0.0168)	(0.0145)
Farm Size Dummies	No	Yes	No	Yes	No	Yes
Observations	848	3,661	848	3,661	848	3,661
R-squared	0.012	0.038	0.048	0.048	0.749	0.597

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

Table 2: OLS, DD & Panel Regression Results

	Marginal	Small	Semi-Medium	Medium	Large	Overall
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.178***	-0.0818**	-0.0104	0.0282	-0.00267	-0.125***
	(0.0315)	(0.0359)	(0.0395)	(0.0603)	(0.148)	(0.0289)
NREGA*Post	0.185***	0.122*	0.111	0.0414	-0.138	0.149***
	(0.0584)	(0.0643)	(0.0700)	(0.0972)	(0.204)	(0.0534)
Constant	0.715***	0.731***	0.715***	0.720***	0.807***	0.714***
	(0.0185)	(0.0212)	(0.0235)	(0.0370)	(0.0988)	(0.0168)
Observations	828	798	777	703	555	848
R-squared	0.760	0.759	0.758	0.766	0.848	0.749

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

Table 3: Panel Fixed Effect Regressions by Farm Size

	Jump in Adoption Rates		Jump in Treatment Probability		Treatment Effect	
	Coef.	SE	Coef.	SE	Coef.	SE
40 districts	-6.8	13.8	60.8	32.9	-11.2	19.0
50 districts	-4.2	12.3	55.1	32.0	-7.7	19.4
60 districts	-3.4	11.4	46.3	32.4	-7.4	21.4
70 districts	-3.5	10.0	31.3	30.6	-11.3	26.0
80 districts	-3.3	9.1	25.5	28.3	-12.9	28.9
90 districts	-1.1	8.5	20.2	27.0	-5.6	38.1
100 districts	0.0	8.0	17.6	26.0	-0.1	45.3

Notes: Bandwidths are measured in number of districts to the left and right of cutoff. Jump in the adoption rates 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 is the quotient of the two, or local Wald estimate, measured in percentage points.

Table 4: Overall Regression Discontinuity Results with Treatment Effect Equal to Jump in Adoption Rates over Jump in Treatment Probability

Do One-time Input Subsidies Lead to Sustained Adoption of Improved Agricultural Technologies? Evidence from a Randomized Controlled Trial in Mozambique

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Abstract

We report the results of a randomized experiment testing impacts of fertilizer subsidy vouchers on farming households in rural Mozambique. Winning a lottery for a one-time fertilizer voucher leads to substantial increases in fertilizer utilization, and positive impacts persist through two additional subsequent seasons. Voucher receipt also leads to persistent increases in household per capita consumption, assets, durable goods ownership, and housing improvements. Our results are consistent with a set of theoretical models that predict persistence of one-time subsidies, and inconsistent with others that do not have such a persistence feature.

JEL Codes: XXXX

Keywords: agriculture, fertilizer, vouchers

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1 Introduction

Over the past several decades, farmers in a wide variety of developing countries have enjoyed substantial gains in agricultural productivity due to the Green Revolution, which involved introduction of improved seeds and modern fertilizers. In this context, Sub-Saharan Africa has proved to be a stark exception: from 1960 to 2000, it experienced the smallest increase in agricultural yields across regions of the world (Evenson and Gollin (2003)). In 2009, fertilizer utilization in sub-Saharan Africa average only 13 kilograms per hectare; by contrast, in other developing countries the average was 94 kilograms per hectare. Motivated by this disparity in fertilizer utilization, many African countries, with international donor support, have implemented large-scale fertilizer subsidy programs. It is therefore surprising that such a widespread policy has not been evaluated using the gold standard method for establishing causal impacts: randomized controlled trials. In this paper we seek to fill this evidence gap, by reporting the results of a randomized controlled trial that we implemented. Among farmers within villages in rural Mozambique, we implemented a random lottery that awarded winners with a one-time subsidy voucher for a modern agricultural input package (fertilizer and improved maize seeds.) We estimate impacts of voucher winning on utilization of modern inputs, agricultural output, and on a variety of other important household outcomes. Another important feature of our study is that we follow study participants for three annual agricultural seasons, which allows us to examine the persistence of the one-time subsidy beyond the agricultural season in which it was offered. We find that voucher winning causes increased utilization of fertilizer and of improved seeds in the agricultural season for which the voucher was offered (2010-11), as well as increased agricultural output in that season. Strikingly, positive impacts on fertilizer and on agricultural output persists into the next two agricultural seasons (2011-12 and 2012-13), when no subsidies were offered. These are, in themselves, important and new findings. In addition, we find that voucher winning leads to substantial improvements in important household outcomes, such as per-capita consumption, asset and durable goods ownership, and housing investments. These impacts do not occur immediately after the season of voucher usage, but occur in the following two seasons. These broad-ranging impacts of fertilizer subsidies are also new, not having been documented in previous research.

In addition to being of substantial importance for policy, our results also provide support for some classes of theoretical models of agricultural households, and evidence against others. Our results are consistent with a class of models in which a one-time subsidy leads to persistent changes in technology adoption, such as models with fixed costs of adoption or that involve learning. However, persistence of adoption in response to a one-time subsidy is possible even without fixed costs or learning, as long as there are liquidity constraints and the fertilizer-output relationship has a certain form. We provide such a model in Section 2 of the paper. Our results rule out models where a one-time subsidy does not lead to persistent technology adoption. For example, a simple Ramsey-style model without capital market imperfections and an optimal steady-state level of input utilization would predict that a one-time subsidy would have only a temporary effect, and that utilization would rapidly return to the steady state. Our results also are contrary to the prediction of a behavioral model a la Dufflo et al. (2011), in which partially naïve farmers who face stochastic temptation shocks systematically delay fertilizer purchases, so that some farmers wait too long and run out of liquidity right before planting time and thus have lower utilization than optimal. In such a setting (and in their experiment, in practice), a one-time nudge or subsidy raises adoption only in the current season, and is not persistent. This paper is organized as follows. In Section 2 we outline a simple theory that generates persistence of adoption in response to a one-time subsidy. We outline the study setting and experimental design in Section 3. Section 4 provides a description of the sample, balance tests, and attrition. In Section 5 we present the empirical results, and we provide concluding thoughts in Section 6.

2 A Model of the Impact of One-time Input Subsidies on Technology Adoption

This section puts forward a model of technology adoption by a risk averse agricultural household that lacks access to capital markets and is unable to borrow to finance the adoption of an improved agricultural technology (hybrid seeds and chemical fertilizers). To cut down on verbiage, we will simply refer to this technology as fertilizer in discussing the model. Assuming that the technology is profitable in expectation, we show the following:

1. Absent an input subsidy, a non-adoption equilibrium can emerge if initial living standards are low, households are risk averse and if households have an unbiased, but flat/diffuse prior about the returns to the new technology.
2. A one-time subsidy on the price of the new technology can move otherwise non-adopting households to adopt the new technology.
3. If the subsidy-induced adoption does not have any learning effects, then technology adoption will be unlikely to 'stick' for most households who will return to the traditional technology.
4. If the subsidy-induced adoption has learning effects that lead to a mean-preserving squeeze of prior beliefs, then the subsidy is more likely to induce adoption that sticks and maintains itself over time.

2.1 Model Structure and Core Assumptions

- Traditional technology yields a fixed/non-stochastic output, $\bar{x}$; [this does not matter much, and keeps things simple; at appropriate place can mention how results change if z is stochastic with varying degrees of correlation with y.]

- An improved technology that utilizes fertilizer f and produces output, $z+yf$, where y is the random return per unit fertilizer and we assume that it is distributed over the closed interval $[y_-, y^+]$ with $E[y] = \bar{y}$. Denote the true probability distribution function for y as $\phi^T(y)$.

- We justify this constant marginal impact of fertilizer via an 'efficiency wage theory' of plant growth such that a given an amount of fertilizer is applied to an optimal area/number of plants, yielding a constant/liner (expected) output increment per-unit fertilizer.[1] Spreading this amount of fertilizer across a larger area will decrease yields. Note that this perspective is consistent with standard fertilizer practice which is to concentrate fertilizer in a small area, rather than spreading it out so that each plant gets only some tiny amount. Importantly, this production specification means that marginal returns to fertilizer are always finite, even at low levels of use.

- Normalizing the price of the agricultural output to 1 and denoting the market price of fertilizer as p_f, we assume that the technology is profitable in expectation, i.e., $\bar{y} > p_f$.

- Individuals have subjective expectations over the distribution of the returns to fertilizer, y. Denote these subjective beliefs as $\phi^e(y)$, where e denotes the individual's years of experience using fertilizer. We will assume that subjective beliefs are unbiased (i.e., $E_{\phi^e}(y) = \bar{y}\ \forall e$), but that increments of experience make subjective beliefs less diffuse and "squeeze in" the subjective probability distribution. Put differently, ϕ^j is a mean preserving spread of ϕ^k, $\forall j < k$. The character of our results are unchanged if we assume more pessimistic, downwardly biased low experience priors.

- Households risk averse and liquidity constrained in the sense that cannot borrow and must self-finance

2.2 Technology Adoption without Learning

We first consider the no learning case in which the pdf is fixed over time and no learning takes place. To denote this, we will write all expectation operators with a sub-script ϕ^0 to indicate that expectations are taken over a baseline, zero experience prior probability distribution for the impact of fertilizer on yields. The next section will consider what happens when households update their priors about returns to fertilizers.

Consider a 3-period model of an agricultural household that produces and consumes the agricultural commodity. We assume that the household is offered a once-off input subsidy in in period 1 that reduces the cost of fertilizer from p_f to $p_f - v$. After period 1, the voucher expires and the price of fertilizer returns

[1] Specifically we assume that plant yields are unresponsive at low levels of fertilizer or plant nutrition, and then have an increasing returns portion followed by a diminishing returns portion. As in the nutrition-based efficiency wage theory, this relationship will pin down a unique level of fertilizer that maximizes returns. Spreading a given amount of nutrition out across more plants will lower total yields across all plants.

to its fixed market price of p_f. To explore the impact of this temporary fertilizer voucher subsidy, we consider the following model of an agricultural household that maximizes expected utility conditional on its subjective beliefs about ϕ (note here to what happens if we assume something else ...):

$$\begin{array}{l} \max_{f_1,f_2} \; u(c_1) + E_{\phi_0}\left[u(c_2) + u(c_3)\right] \\ \qquad\qquad subject\ to: \\ \qquad\qquad c_1 \leq z_0 - (p_f - v) f_1 \\ \qquad\qquad c_2 \leq \bar{x} + y_1 f_1 - p_f f_2 \\ \qquad\qquad c_3 \leq \bar{x} + y_2 f_2 \\ \qquad\qquad f_1, f_2 \geq 0 \end{array} \tag{1}$$

where z_0 is initial cash on hand for the household and y_1 and y_2 represent the realized returns to fertilizer in production periods 1 and 2, respectively. Note that his model assumes no savings ... [but if not ...]. Also period 3 is the end of the line and eat everything ... . While restrictive, this 3-period set-up allows us to explore the key economic consideration that .. .

2.2.1 Second Period Problem

Working backwards, we begin by examining second period choice conditional on realizations from the first year crop yield. To simplify the notation, define second period cash-on-hand as $z_2 = \bar{x} + y_1 f_1$. Note that cash on hand only depends on period 1 decisions and realizations. We can write the conditional second-period value function as:

$$\begin{array}{l} V_2^*(z_2) \equiv \max_{f_2} \; u(c_2) + E_{\phi_0}\left[u(c_3)\right] \\ \qquad\qquad subject\ to: \\ \qquad\qquad\quad c_2 \leq z_2 - p_f f_2 \\ \qquad\qquad\quad c_3 \leq \bar{x} + y_2 f_2 \\ \qquad\qquad\quad f_2 \geq 0 \end{array} \tag{2}$$

The Kuhn-Tucker conditions for this problem are:

$$\begin{aligned} \frac{dV}{df_2} &= E_{\phi_0}[y_2 u_3'] - u_2' p_f \leq 0 \\ f_2 \frac{dV}{df_2} &= 0 \end{aligned} \tag{3}$$

As can be seen from these conditions, key comparison determining fertilizer use is the comparison of the expected benefits ($E_{\phi_0}[y_2 u_3']$) and the shadow price of liquidity ($u_2' p_f$). Note that ... .

The corner solution of no fertilizer use will occur when benefits are strictly smaller the the shadow price of liquidity when evaluated at $f = 0$. Because second period cash-on-hand influences only the shadow price of liquidity, other things equal we can define a critical level of cash on hand, $\tilde{z}_2$, such that the individual is indifferent between adopting and not adopting the improved technology. For values $z_2 > \tilde{z}_2$, the individual will adopt whereas no adoption will occur if the opposite inequality holds. In addition, it is straightforward to show that:

1. $\tilde{z}_2$ is strictly increasing in risk aversion; and,
2. $\tilde{z}_2$ is strictly decreasing as subjective beliefs about the returns to fertilizer become less diffuse via a mean preserving squeeze.

Finally, note that for any given level of f_1 we can define the yield level necessary to give cash on hand of $\tilde{z}$ as $\tilde{y}(f) = (\tilde{z}_2 - \bar{x})/f_1$. In other words, $\tilde{y}(f)$ is the minimum first period returns to fertilizer that must be realized in order or the household to sustain the adoption of the new technology in period 2. Note that this new term also depends on risk aversion and subjective beliefs. We will assume that $\tilde{z} > \bar{x}$.

2.2.2 First Period Problem

We can now examine the first period problem as:

$$\begin{aligned} \max_{f_1} \;\; & u(c_1) + E_{\phi_0}[V_2^*(z_2)] \\ & \textit{subject to}: \\ & c_1 \leq z_0 - (p_f - v)f_1 \\ & z_2 = \bar{x} + y_1 f_1 \\ & f_1 \geq 0 \end{aligned}$$

In general form, we can write the first order condition to the first period problem as:

$$\frac{dE_{\phi_0}\left[V_2^*\right]}{df_1} - u_1'(p_f - v) \leq 0.$$

As is apparent from this condition, the first impact of the voucher subsidy is to lower the shadow price of liquidity, making adoption of an interior solution more likely.

Because the second period problem has an economically significant corner solution, it is useful to break apart the second component of the maximand in (2) above into two pieces. Defining $\Phi(f_1) = Prob(y_1 < \tilde{y}(f_1))$, we can rewrite the second component of the maximand as:

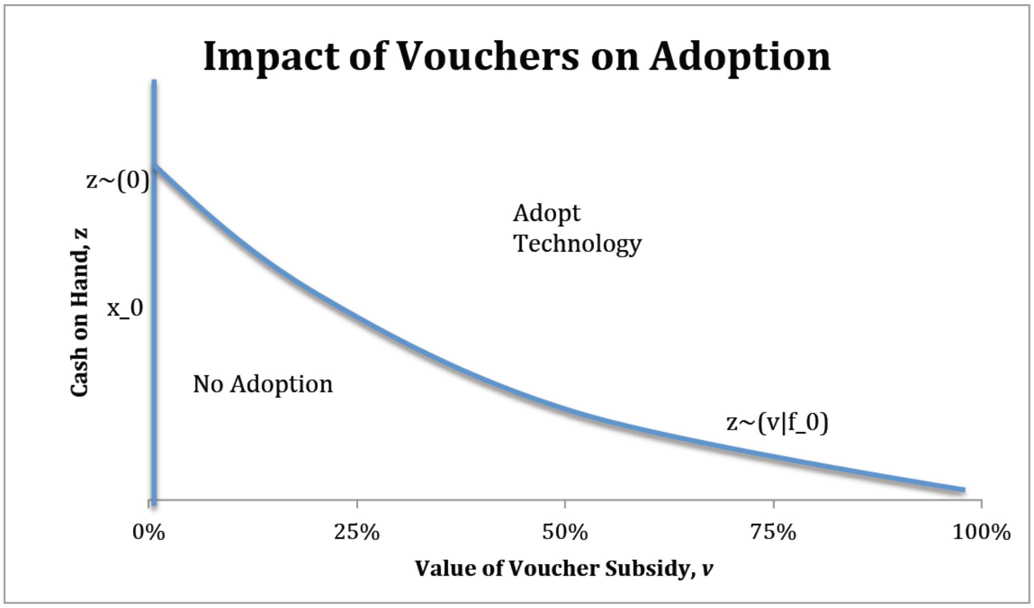
$$E\left[V_2^*(z_2)\right] = \Phi\left\{E\left[V_2^*(z_2) \mid y_1 < \tilde{y}(f_1)\right]\right\} + (1-\Phi)\left\{E\left[V_2^*(z_2) \mid y_1 > \tilde{y}(f_1)\right]\right\},$$

where we have dropped the notation indicating the conditioning of the probability Φ on f_1 in order to eliminate clutter. Denote the first term in curly brackets as A and the second term in curly brackets as B.

Next note that the derivatives of the two conditional expectation terms have different forms. When the realization of y_1 keeps second period cash-on-hand below the critical value $\tilde{z}$, we write the derivative as:

$$\frac{dE_{\phi}\left[V_2^*\right]}{df_1} = E\left[y_1 u_2'\right] \equiv A'.$$

Figure 1: Impact of Vouchers on Technology Adoption



For more buoyant yield realizations, the derivative of the conditional expectation becomes:

$$\frac{dE_{\phi}\left[V_2^*\right]}{df_1} = E\left[y\left(u_2'\frac{dc_2^*}{dz_2} + E\left(\frac{df_2^*}{dz_2}y_2 u_3'\right)\right)\right] \equiv B'.$$

In this case we see that fertilizer can indirectly relax the liquidity constraint in period 2 if they sufficiently increase cash-on-hand. **Also note that B'>A' and that A' is basically the condition from the second period problem.

The full first order condition for the period 1 problem can now be written as:

$$\left\{\Phi A' + \frac{d\Phi}{df_1}A\right\} + \left\{(1-\Phi)B' - \frac{d\Phi}{df_1}B\right\} - u_1'(p_f - v) \leq 0.$$

This problem can of course admit a corner solution ... . As with the second period problem, there will be a critical minimum amount of cash-on-hand below which adoption does not occur. Note that this minimum level decreases with the magnitude of the subsidy and denote it as $\tilde{z}_0(v)$. Can say more here about pessimism and how it causes $\tilde{z}_0(v)$ to increase. Note also that a large value of Φ is equivalent to a gloominess prior (i.e., nothing will ever get better). Figure 1 shows the relationship between this critical minimum initial cash on hand and adoption of the new technology as a function of the amount of the subsidy.

We are now in a position to see the impacts of a once-off voucher subsidy in a model without learning. To focus the analysis and accommodate the stylized facts of large areas of sub-saharan Africa like our Mozambique study area, we make the following assumptions:

- $\bar{x} < \tilde{z}_0(v = 0)$: This assumption means that adoption will not occur in the absence of subsidy. Adoption failure is a combination of both risk and liquidity constraints.[2]

We can now consider the possible impacts of the introduction of a once-off fertilizer voucher of size v:

1. Case 1: nothing happens if $\bar{x} < \tilde{z}_0(v)$;
2. Case 2: Adoption followed by period 2 disadoption if $\bar{x} > \tilde{z}_0(v)$ and $y_1 < \tilde{y}(f_1)$.
3. Case 3: Adoption-stochastic stickiness: So what we can see here is if realization of y_1 is big enough ($> \tilde{y}(f)$), then could have at least the lucky continue to adopt fertilizers in period 2.

In summary, absent learning, adoption of the improved technology will only stick if the initial adoption is sufficiently successful to bump second period cash on hand beyond a critical minimum level.

2.3 Technology Adoption in the Presence of Learning

While there are various ways to model learning, we here assume that learning is naive or unanticipated. Under this assumption, first period choice is exactly as modeled above. However, unanticipated learning will make second period or sustained adoption more likely under our assumption that experience operates as a mean preserving squeeze, making $\phi^1(y)$ less diffuse than the prior $\phi^0(y)$. As shown above, this shift in subjective beliefs will lower $\tilde{z}_2$, making expanding the set of individuals who will sustain adoption of the new technology.

3 Project Description

3.1 Agriculture in Mozambique and Input Subsidies in the Region

Following its independence in 1975, Mozambique went through 15 years of civil war, from 1977 to 1992. Despite an annual GDP growth of 8% on average between 1994 and 2006, it remains one of the poorest countries in the world. In 2011, its Human Development Index was ranked 184th out of 187 countries rated. More than 75% of the Mozambican population works in small-scale agriculture, with little to no use of tractors, ploughs, fertilizer, pesticides, irrigation and other agro-inputs. The most common crops include maize, cassava, sweet potatoes, cotton, tobacco, sesame and groundnuts. The use of mineral fertilizer among small households is primarily limited to cash crops and scarce on cereal

[2]Note that it is a bit difficult to separate the risk and liquidity constraints, as less risk (or risk aversion) will lower the criticial amoutn of cash on hand needed to spur adoption. This perspective is somewhat in contrast to the Karlan et al. (2012) paper that portrays risk and liquidity constraints as separable constraints that can be tested in an RCT horse race.

crops, leading to low yield, generally below one ton per hectare for maize production (compared to up to 8 tons per hectare in the most productive developing countries). The nascent input market is small and its network unsubstantial. Between 1996 and 2003, agricultural production grew by an average of 6% per year, leading to a decrease in the rural poverty headcount, from 69% to 54% during the same period. However, Nankani et al. (2006) note that this growth mainly resulted from the expansion of area cultivated and labor due to the return of migrants, while technological improvements have been modest and yields almost stagnant, which threatens the sustainability of agricultural growth in the absence of future technological progress.

In the 1970's and early 80's in a majority of African countries, fertilizer was subsidized and sold through state-owned enterprises in order to address the under provision of fertilizer by the market. However, blamed for being costly, inefficient, overwhelmingly beneficial to large farmers, and detrimental to the private sector, most of the public monopolies of agro-inputs were eliminated during the structural adjustments of the late 80's. Yet in the late 90's, agro-input subsidies have re-emerged under what is now called "smart subsidies". Typically, vouchers are distributed to poor farmers, giving them access to an agro-input package, which will be provided by the private sector at a subsidized price (the providers then trade the vouchers against the amount of the subsidy, at an intermediary bank or agency). This scheme has been claimed to offer the previously-mentioned advantages of traditional fertilizer subsidy while stimulating rather than undermining the private sector, and targeting the poor more effectively. On the other hand, some agencies have indicated failures to target the poor, and the low cost effectiveness of the intervention (Minot and Benson, 2009). Hence the debate on agro-input subsidy remains very active.

The use of voucher subsidy in Mozambique was inspired by neighboring Malawi's agro-input subsidy program. The Starter Pack Scheme (SPS), implemented in 1998, followed by the Target Input Program (TIP) in 2000 and the Farmer Input Subsidy Program (FISP) in 2005, were large scale input subsidies, targeting mostly maize but also tobacco production in Malawi. While these subsidies are thought to have contributed to an increase in fertilizer use and maize production, helping achieve both national and household food self-sufficiency, their potential for long-term growth and poverty reduction remains unclear. Levy (2003) finds evidence that the households most dependent on maize production were most affected by the 2001-2003 crisis, substantiating worries that the reduction of diversification caused by the subsidy has increased the vulnerability of the beneficiaries. Chibwana et al. (2010) find that the FISP failed to target the most vulnerable members of the communities (i.e. asset poor households and households with female heads) because the selection of the coupon recipients was affected by political factors (Christiaensen et al (2012) also finds that the vouchers were largely captures by local elites in Tanzania). Whether the subsidy programs strengthened or weakened the private provision of agro-inputs remains unknown, and the claim that the subsidized learning of farmers will stimulate commercial demand has not yet been confirmed empirically. At its peak in 2008/09, subsidy costs accounted for around 16% of Malawi's national budget and 74% of its agricultural budget (Dorward and Chirwa, 2011). Given that the cost of a large scale voucher subsidy threatens its viability, the case for the subsidy depends on whether it can generate long lasting benefits for recipients, which we investigate using a field experiment in Mozambique. Furthemore the credibility of the existing evidence is undermined by the potential bias in the selection of the beneficiaries of the voucher, and the difficulty to find a good instrument that affects the probability of being a beneficiary but not the production. The random assignment of the vouchers in our experiment addresses this issue.

3.2 Project Overview and Research Desig

Unlike many of its neighbors that launched nationwide fertilizer subsidy programs, Mozambique piloted a limited, two-year fertilizer subsidy program funded by the European Union, implemented by Mozambique's Ministry of Agriculture, the Food and Agriculture Organization (FAO) and the International Fertilizer Development Center (IFDC). The limited scope of this program allowed the research team, in cooperation with the Ministry of Agriculture in Mozambique and the IFDC (International Fertilizer Development Center), to design and implement a randomized controlled trial of the voucher coupon system. Over the 2009-10 and 2010-11 crop years, the pilot targeted 25,000 farmers, of which 15,000 received a subsidy for maize production, and the remaining 10,000 received a subsidy for rice production. Among the recipients fo the subsidy for maize production, 5000 were in the Mancia province, where the study was implemented which is in the center west of the country and shares a border with Zimbabwe

For maize, the voucher coupon underwrote the purchase of a technology package designed for a half hectare of improved maize production: 12.5 kg of improved seeds (either OPV or hybrid) and 100 kg of fertilizer (50 kg of urea and 50 kg of NPK 12-24-12). The market value of this package was MZN 3,163 or about USD 117, with farmers required to co-pay about USD 32, or 27% of the total cost. Individuals were deemed eligible for a voucher coupon if they met the standard program criteria:

- Farming between 0.5 hectare and 5 hectares of maize;
- Being a "progressive farmer," defined as a producer interested in modernization of their production methods and commercial farming;
- Having access to agricultural extension and access to input and output markets;
- Being able and willing to pay for the remaining 27% of the package cost.

Given the absence of prior data on maize cultivated and other necessary information, lists of eligible farmers were created jointly by agricultural extension, local leaders, and agro-input retailers, under the supervision of the IFDC. Farmers were asked to register only if they had the money to complete the subsidy.

Only one person per household was allowed to register. The farmers were informed that a lottery would occur and only half of those on the list would win a voucher. After official approval of the Provincial Service of Agriculture of Manica, the lists of possible participants were used to randomly assign vouchers to 50% of the households in the list of each village.

The Voucher Treatment was combined with a financial intervention, hence the access to the financial institution Banco Oportunidade was a criteria for the selection of villages. All the villages that combined access to Banco Oportunidade and participation to the voucher program were included in the study, which spreads over three districts of the Manica province: Barue, Manica district, and Sussundenga.

Besides the voucher intervention, the financial intervention randomly assigned one third of the villages to a control group, one third to a savings treatment (ST) and one third to the Matched Savings treatment (MST). The ST group was encouraged to open savings accounts through easier access and financial education. The MST group received the same encouragement to save as well as a "bonus" of 50% of the savings left in the account between the harvest and the time to purchase fertilizer (from August 1st to October 31st, 2011), with a maximum match of MZN 1500 per individual (approximately USD 56). The financial reward aimed to assist farmers in developing a habit of savings in order to carry forward the benefits of the agro-input subsidy from year to year and sustainably self-finance their inputs for maize production. This paper focuses on the short term and medium term impact of the voucher intervention, on input use, production and other welfare indicators. This in is a new contribition to the literature, and requires specific attention to the possible economic mechanism. We thus conduct the analysis of this paper narrowing the sample to the 514 households in the control group of the financial intervention, spread over 41 villages. In each one of these villages, half of the households were randomly selected to receive the voucher. A companion paper [##REFERENCE##] analyzes the effect of the financial intervention and its interaction with the voucher intervention.

4 Sample, balance tests, and attrition

Table 1 provides summary statistics on the study participants, and tests for balance on these variables across treatment and control farmers. Sample household heads are 85% male and 78% are literate. By comparison, in rural Manica province, only 66% of household heads are male and 45% are literate, an indication that the targeted households are relatively less vulnerable and more educated compared to the rest of the region. This is not a surprise given the initial intention of targeting progressive farmers. Study households own an average 10.3 hectares of land owned (the median is 5 hectares). Eleven percent of households have electricity at home, and 19% used fertilizer on at least one of their maize fields during the preintervention 2009–2010 season. While better off than some, the study population is dominated by relatively poor small-scale farmers with limited experience with modern agro-inputs.

The "baseline" survey was implemented after the distribution of vouchers and planting by the farmers, but before harvest. Hence to check the balance between treatment and control groups, we look at variables that are not expected to vary in the short run (for example education of the household head), or variables related to the 2009-10 agricultural season, which preceeded the season with the randomized distribution of vouchers. The sample is balanced on all of these variables: in not one case is the difference in means across treatment and control farmers different from zero at conventional significance levels.

We look at attrition from the study, and whether such attrition could lead to biased treatment effect estimates. We investigate attrition in Appendix Table 1. We attempted to survey everyone in the initial sample at each subsequent survey round (in other words, attrition was not cumulative), so all attrition rates reported are vis-à-vis that initial sample. Attrition is relatively low for a field study of this type: 8.6% in the first (2011) follow-up survey, 10.0% in the second (2012) round, and 7.6% in the final (2013) round. The regressions of the table regress the dummy for treatment (winning the lottery) on attrition, controlling for village fixed effects. In no case is attrition large or statistically significantly different from zero. Attrition bias is therefore not likely to be an issue in for our treatment effect estimates.

5 Empirical results

5.1 Estimation

Random treatment assignment allows us to estimate the causal impact of the voucher receipt on a variety of outcomes. The main results in this paper are estimated using the following equation:

$$Y_{iv} = a_0 + a_1 Z_{iv} + \theta_v + \epsilon_{iv} \tag{4}$$

where i indexes the study participant household, and v indexes the study participant's village. Z_{iv} is a dummy for whether individual i (located in village v) was selected to receive a voucher or not. The regression variables do not have time subscripts: we run this regression separately for outcomes in each of the three annual post-treatment follow-up surveys that we implemented. This allows examination of changes in the coefficient on the treatment dummy over time. Our estimates will be intent to treat (ITT) effects of voucher winning on the outcomes of interest. θ_v are stratification cell fixed effects representing the village of the study participant. We report Huber/White heteroskedasticity-consistent standard errors. Y_{iv} is the outcome of interest, which can be the use of input, the maize production or other welfare indicators. It can be expressed either in level or as the inverse hyperbolic sine transformation (IHST)[3], which

[3]The inverse hyperbolic sine (IHST) of the horizon, proposed by Johnson (1949), which is given by IHST(x) = log(x + (x2 + 1)1/2). As with the logarithmic transformation, the slopes' coefficients can be interpreted as elasticities, and it has been shown by Burbidge et al.

presents the advantage of being less sensitive to outliers and more sensitive to variations that can be sizeable in relative terms for a small producer, but less sizeable in absolute term. a_1 provides an unbiased estimator of the Intention To Treat (ITT) of being randomly selected to receive a voucher.

5.2 Take-up and input use

Before we consider how receipt of the voucher may have affected behavior, we first examine the take up of the voucher and how that take up differs by treatment group. Farmers in the treatment group were entitled to receive a voucher distributed by the public agricultural extension. Under the supervision of our team in IFDC, in each village, the extension invited all the winners of the lottery in order to distribute the vouchers. The beneficiaries were asked to pickup their voucher only if they have the money to complete the subsidy and are planning to use it themselves. Among the households in the study, winning the lottery increased their chances of receiving at least one voucher[4] from 13% to 49%, and it increased the chances of obtaining a voucher and using it for maize production from 10% to 29% (regressions 2A.5 and 2A.6). Hence the use of the package among those who were entitled to receive the voucher is alarmingly low. When entitled to receive a voucher, only half of beneficiaries received it and, conditional on receiving the voucher, only 57% redeemed it and actually used the content of the package for their maize production. We have not observed any case of farmer who won the lottery but was denied his voucher, hence not receiving the voucher always results from the farmer's decision.

At the same time, some of the participants in the control group tried to negotiate for an exception to the rule with the government's extension service. Additionally, the agricultural extension, in charge of the distribution of the vouchers, was also facing pressures from the organizations implementing the program who wanted all vouchers to be used; every voucher not picked up by a beneficiary had to be redistributed to another household. In this case, we pushed for a redistribution of the spare vouchers outside of the area of the study in order not to contaminate our sample. However, despite our efforts, the result of the lottery was not perfectly enforced, and 13% of the control group managed to obtain a voucher. The limited compliance rate[5] indicates the difficulties of implementing a randomized control trial in a real life setting with multiple stakeholders. But it is also largely driven by difficulties inherent to voucher programs: lack of money from the farmers to complete the amount of the subsidy and a distribution of vouchers and availability of inputs in the

(1988) to be a better way to handle outliers and non positive values than adding a constant to the log or dropping the non-positive values.

[4]Regression 2A.5 uses a dummy equal to one if the household used at least one voucher. Out of the 154 households who received at least one voucher, 8 received two vouchers, and none received more than two.

[5]If we define using the package for maize production as the treatment then the compliance rate is equal to 16%. It is given by the difference between the percentage of individuals who used the package for their maize production in the treatment group compared to the same percentage in the control group.

shop that was completed in early December, after some farmers had already planted. Despite the fact that the program preselected the progressive farmers, only 28% of those who were offered a voucher received it, redeemed it and used the package for their maize production.

5.3 Impacts

On input utilization:

Table 3 presents impacts of treatment (voucher receipt) on adoption of fertilizer and improved seeds. Treatment had a positive impact on fertilizer in the first year, whether expressed as total kilograms or in kilograms per hectare, and also when expressed as the IHST. These impacts are statistically significantly different from zero at conventional levels, ranging from the 10% to the 1% levels across outcomes and specifications. Given that the voucher offered a subsidy for 100 kg of fertilizer, the increase in fertilizer use only represents 17% of what it could have been would the voucher have fully been used for an increase in input use for maize production. The considerable difference result from farmers who did not use their voucher (or not for maize production), those who used it only partially, and those who used the inputs of the voucher to substitutes similar inputs that they would have purchased had they not received the voucher. This shows a large margin for improvement through better targeting and ensuring that the subsidized package is widely used for its predertmined purpose.

Strikingly, impacts on fertilizer use presist in the 2nd and 3rd follow-up surveys. The impact on fertilizer use is smaller in magnitude in 2012 compared to the (2011) year of the subsidy, but does not seem to decrease between 2012 and 2013. Average impacts (2012 and 2013) are significant at the 10% level in kilograms and kg/ha, and at the 1% level for the IHST of these variables. By contrast, impacts on improved seeds utilization are only statistically significantly different from zero in the first year for the IHST specification of these variables. Hence the increase in fertilizer use was persistent, but not the one of the use of improved maize seeds. In the season prior to the intervention, 22% of the households were using fertilizer for maize cultivation compared to 53% for improved seeds, which are familiar to a larger number of farmers (and also more affordable).

On maize output:

Table 3 presents impacts of the treatment on maize output. The treatment has positive impacts on maize production and yield in the first follow-up survey. Impacts on yield are statistically significantly different from zero in the kg/ha and IHST kg/ha specifications. These impacts are persistent through the 2nd and 3rd follow-up surveys: average (2012 and 2013) impacts are statistically significantly different from zero at the 5% level in the kg and kg/ha specifications, as well as for the IHST kg/ha specification. Given that average maize production at baseline is 2,165 kg, an average increase of 336 kg in the average (2012 and 2013) production is economically very meaninful for the beneficiaries[6]. The results show no evidence of a reduction of the impact of the subsidy on production and yield after the year of the subsidy (In most specifications the impact is higher in the subsequent year although the difference is not significant). Column 3 shows little evidence of a change in the maize area cultivated. Surpisingly the quantity of maize sold (in kg) is not significantly affected, however the IHST regression shows a 53% increase in the average (2012 and 2013) maize sales significant at the 5% level. This increase in sales being more significant in relative term than in absolute term indicates a change that is more prominent among farmers who were selling small quantities (since IHST regressions put more weight on variations among small values of the outcome of interest). Indeed column 3A.5 shows the average (2012-2013) proportion of farmers who sell maize increased by 8.4 percentage points (statistically significantly different from zero at the 5% level). The transformation of subsitence farmers into commercial farmers is one of the major objectives announced before the implementation of the voucher subsidy.

Table 4 shows IV regressions where the fertilizer use (in kg or kg/ha) is instrumented by the result of the voucher lottery, in order to provide some estimates of benefit cost ratio[7]. Column 4A.2 shows that in 2011 a one kg/ha increase in fertilizer use resulted in a 14.2 kg/ha increase in maize production. Given the market price of the package, and a kg of maize valued at 5 MZN, this corresponds to a benefit cost ratio of 2.24 (a 124% return over the season). While economically substantial, it is common among agronomists to consider that in agriculture a benefit cost ratio of at least 2 is required for an agricultural invesment to be worthwhile in order to comensate for the risk, time, etc.

Following years indicate benefits-cost ratios that are consistently above 3 (though not always significant), but these ratios should be interpreted with a lot of caution given that with time the reallocation of additional inputs becomes more likely and the restiction hypothesis less credible. Possible explanations for the increase in the benefit-cost ratio include the late distribution of vouchers in 2011, progressive learning on how to use fertilizer, the accumulation of nutrients in the soil from multiple years of fertilizer use, or the adjustment of other inputs in the long run (the last two reasons being are examples of violations of the exlusion restriction).

On assets, durable goods, and consumption

Table 5 shows impacts of the treatment on consumption, assets, and durable goods. There are no impacts on any of these outcomes in the first follow-up survey (immediately following the season for which the voucher was provided). In the subsequent two years, positive impacts emerge. Average (2012 and 2013) impacts across the latter two years are positive and statistically significantly

[6]The impact of actually using the voucher for maize production is likely to be higher, however we focus on the reduced form (the impact of being assigned to receive a voucher on economic outcomes) first because this question is of central interest for policy recommendations, and second because it does not require the exclusion restriction.

[7]Given that the package provided both fertilizer and improved seeds, the coefficients should be interpreted as the impact of increasing fertilizer joiny with the corresponding use of improved seeds, at least for the 2011 season where the voucher increased the use of improved seeds.

different from zero for several outcomes measured in meticais: per capita daily consumption, formal savings, informal savings, total savings, and crop stocks. In regressions for the IHST specification Column 5B.8 shows a progressive incease in the treatment group (compared to the control group) throught the years in order to reach a 26% increase in total assets and savings by 2013 (significant at the 10% level). This increase results from an increase in total savings, livestock and cropstock which all indicate an increase in the livelihood of the household, which is confirmed 9% increase in the average (2012-2013) consumption per day of the beneficiary household (significant at the 5% level).

On housing variables:

Table 6 presents impacts of the treatment on variables related to housing quality. The first dependent variable is a dummy that is equal to one if there is any housing improvement (across the categories in the table). There is no impact in the first year, but on average over the subsequent two years there is a 4 percentage point increase in the likelihood of making any housing improvement that is statistically significant at the 5% level, providing additional evidence of long term improvement in the living conditions of the beneficiaries. The other columns of the table show that the specific areas of housing improvement are in walls and floors.

On other agricultural activities:

Aside from impacts on maize production (which was the object of the input voucher program), table 7 ivestigates whether the treatment had impacts on other farming activities. There are no large or statistically significant impacts in the first year, but impacts do emerge in the latter years. On average across the latter two years, there are positive impacts on all the outcomes in the table: animal sales, fertilizer on other crops, production of other crops, self-consumption of other crops, and sales of other crops. (In two regressions, the estimated effects are not statistically significant at conventional levels: for fertilizer on other crops measured in kg., and for the IHST of animal sales. Quite surprisingly the increase in the sales of other crops is more substantial than the increase in maize sales (analyzed in table 3). This potentially indicates that such crops provide returns to fertilizr that are even higher than the return of using it on maize.

6 Conclusion

We report the results of a randomized experiment testing impacts of fertilizer subsidies on input utilization, agricultural output, and other household outcomes. We find substantial and persistent impact (over three years following the one-time subsidy) on all these outcomes. Our results are consistent with a set of theoretical models that predict persistence of one-time subsidies, and inconsistent with others that do not have such a persistence feature. An important avenue for future research would be to mediate between competing models that predict such persistence.

Table 1 Basic Statistics for each Treatment Group and Verification of Randomization

	Verification of Randomization			
	All groups	Lost voucher lottery	Won voucher lottery	p-value of difference
Hh head educ (yrs)	4.73	4.77	4.7	0.8188
	[3.17]	[3.32]	[3.01]	
Hh head male (%)	0.85	0.85	0.85	0.9311
	[0.36]	[0.36]	[0.36]	
Hh head age	46.12	45.82	46.43	0.6275
	[13.92]	[14.09]	[13.76]	
Hh head lit (%)	0.78	0.79	0.76	0.4262
	[0.42]	[0.41]	[0.43]	
Area (ha) t0*	3.28	3.37	3.18	0.4900
	[3.03]	[2.98]	[3.07]	
Fertilizer (kg) t0*	25.04	27.05	22.9	0.4290
	[59.44]	[63.54]	[54.76]	
Fertilizer (kg/ha) t0*	14.07	15.17	12.88	0.5421
	[42.33]	[44.37]	[40.08]	
fert use t0(%)	0.22	0.22	0.21	0.7230
	[0.41]	[0.42]	[0.41]	
Improved seeds (kg) t0*	21.66	21.31	22.03	0.8197
	[35.45]	[35.27]	[35.70]	
Improved seeds (kg/ha) t(	9.44	9.23	9.66	0.7395
	[14.59]	[14.82]	[14.36]	
Production (kg) t0*	2164.75	2208.08	2117.97	0.6912
	[2512.79]	[2377.05]	[2655.78]	
Yield (kg/ha) t0*	947.48	979.45	913.08	0.4886
	[1066.55]	[1114.46]	[1013.70]	
Maize sold t0 (kg)*	510.6	454.0	571.2	0.3047
	[1248.19]	[1056.79]	[1424.98]	
Maize sold t0 (%)	0.49	0.50	0.48	0.7439
	[0.50]	[0.50]	[0.50]	
Irrigation t0(%)	0.05	0.05	0.05	0.7083
	[0.22]	[0.21]	[0.22]	
Fert experience	1.03	1.05	1.00	0.7811
(yrs in last 9 years)	[2.16]	[2.19]	[2.13]	
Number of observations	514	267	247	

Standard deviations in brackets
Agricultural data are about agricultural campaign 2009-2010, prior to assignment to any treatment
* The top 1% values have been replaced by 99th percentile to avoid means being affected
by unrealistic values.

Table 2: Impact of Voucher on Inputs

2A REGRESSIONS IN LEVEL

		2A.1	2A.2	2A.3	2A.4	2A.5	2A.6
	Explained variable:	**Fertilizer (kg)**	**Fertilizer (kg/ha)**	**Improved seeds (kg)**	**Improved seeds (kg/ha)**	**Voucher received**	**Voucher Used**
2011	**Won lottery**	17.16***	12.28*	3.57	3.15	0.37***	0.20***
		[5.12]	[6.94]	[3.40]	[2.12]	[0.04]	[0.04]
	Observations	510	505	496	491	510	514
2012	**Won lottery**	6.37*	13.36	-3.92*	1.25		
		[3.40]	[9.03]	[2.10]	[2.68]		
	Observations	457	449	454	447		
2013	**Won lottery**	7.50	5.76*	3.13	0.92		
		[5.48]	[3.23]	[2.52]	[1.20]		
	Observations	473	471	466	464		
Avg 2012-2013	**Won lottery**	8.65*	11.82*	-0.77	1.41		
		[4.34]	[6.21]	[1.52]	[1.58]		
	Observations	495	493	494	492		

The top 1% values have been replaced by 99th percentile

2B INVERSE HYPERBOLIC SINE TRANSFORMATION (IHST)

		2B.1	2B.2	2B.3	2B.4
	Explained variable:	**Fertilizer (kg)**	**Fertilizer (kg/ha)**	**Improved seeds (kg)**	**Improved seeds (kg/ha)**
2011	**Won lottery**	0.76***	0.67***	0.49***	0.44***
		[0.19]	[0.20]	[0.17]	[0.14]
	Observations	510	505	496	491
2012	**Won lottery**	0.32**	0.31**	0.01	0.10
		[0.13]	[0.12]	[0.14]	[0.14]
	Observations	457	449	454	447
2013	**Won lottery**	0.31**	0.26**	0.19	0.16
		[0.13]	[0.11]	[0.16]	[0.13]
	Observations	473	471	466	464
Avg 2012-2013	**Won lottery**	0.36***	0.34***	0.10	0.14
		[0.12]	[0.10]	[0.12]	[0.10]
	Observations	495	493	494	492

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1
All regressions include village fixed effects and no additional control variable

Table 3: Impact of Maize Production, Yield and Sales

3A REGRESSIONS IN LEVEL

		3A.1	3A.2	3A.3	3A.4	3A.5
	Explained variable:	**Maize Production (kg)**	**Yield (kg/ha)**	**AREA (ha)**	**Qtty Sold (kg)**	**Dummy sell maize**
2011	**Won lottery**	204.7	192.2**	-0.19	125.8	-0.031
		[158.1]	[87.3]	[0.22]	[84.3]	[0.040]
	Observations	460	457	507	449	468
2012	**Won lottery**	208.8	288.1*	-0.49*	21.5	0.064
		[225.4]	[149.2]	[0.26]	[115.1]	[0.044]
	Observations	442	436	449	454	458
2013	**Won lottery**	440.7**	167.2	0.29	5.9	0.072*
		[211.4]	[105.0]	[0.21]	[59.8]	[0.036]
	Observations	468	466	471	466	470
Avg 2012-2013	**Won lottery**	335.7**	248.7**	-0.10	44.5	0.084**
		[161.1]	[107.6]	[0.19]	[56.7]	[0.039]
	Observations	492	491	493	495	495

The top 1% values have been replaced by 99th percentile

3B INVERSE HYPERBOLIC SINE TRANSFORMATION (IHST)

		3B.1	3B.2	3B.3	3B.4
	Explained variable:	**Maize Production (kg)**	**Yield (kg/ha)**	**AREA (ha)**	**Qtty Sold (kg)**
2011	**Won lottery**	0.05	0.23**	-0.06	-0.22
		[0.09]	[0.09]	[0.05]	[0.21]
	Observations	460	457	507	449
2012	**Won lottery**	0.09	0.25**	-0.11*	0.37
		[0.09]	[0.12]	[0.06]	[0.28]
	Observations	442	436	449	454
2013	**Won lottery**	0.13	0.14*	0.01	0.49**
		[0.09]	[0.08]	[0.05]	[0.22]
	Observations	468	466	471	466
Avg 2012-2013	**Won lottery**	0.11	0.19**	-0.06	0.53**
		[0.08]	[0.08]	[0.04]	[0.23]
	Observations	492	491	493	495

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1
All regressions include village fixed effects and no additional control variable

Table 4: IV Regressions

		4A REGRESSIONS IN LEVEL		4.B INVERSE HYPERBOLIC SINE TRANSFORMATION (IHST)	
		4A.1	**4A.2**	**4A.3**	**4A.4**
		Maize Production (kg)	**Yield (kg/ha)**	**Maize Production (kg)**	**Yield (kg/ha)**
2011	**Fertilizer (kg)**	11.8		0.07	
		[11.8]		[0.12]	
	Fertilizer (kg/ha)		14.2*		0.33**
			[8.1]		[0.15]
	Observations	450	447	450	447
2012	**Fertilizer (kg)**	30.3		0.30	
		[39.5]		[0.32]	
	Fertilizer (kg/ha)		21.3*		0.85*
			[12.8]		[0.48]
	Observations	434	429	434	429
2013	**Fertilizer (kg)**	58.8		0.41	
		[45.1]		[0.30]	
	Fertilizer (kg/ha)		28.4		0.51
			[22.3]		[0.40]
	Observations	462	460	462	460
Avg 2012-2013	**Fertilizer (kg)**	36.1		0.31	
		[24.5]		[0.21]	
	Fertilizer (kg/ha)		21.1**		0.57**
			[9.4]		[0.27]
	Observations	485	485	485	485

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1
The top 1% values have been replaced by 99th percentile

For each year, Fertilizer (kg) and Fertilizer (kg/ha) are instumented by a dummy equal to one if the household won the voucher lottery.

All regressions include village fixed effects and no additional control variable

Table 5: Impact of Voucher on Consumption, Saving and Assets

5A REGRESSIONS IN LEVEL

		5A.1	5A.2	5A.3	5A.4	5A.5	5A.6	5A.7	5A.8
		Cons/day	Formal Savings	Informal Savings	Total savings	Durable Goods	Livestock	Crop Stock	Assets and Savings
août-11	Won lottery	0.78	313.5	760.7	868.7	3,134.9	4,456.9	-41.4	9,318.2
		[3.65]	[432.5]	[766.5]	[1,047.5]	[2,286.8]	[4,465.0]	[742.9]	[7,097.0]
	Observations	469	470	453	470	470	470	470	470
2012	Won lottery	14.03***	498.6	1,143.4**	1,855.8***	3,743.3	1,056.9	1,916.4	7,016.3
		[4.58]	[443.1]	[493.4]	[619.4]	[2,573.7]	[3,996.8]	[1,163.9]	[6,295.0]
	Observations	462	462	431	462	462	462	462	462
2013	Won lottery	6.81	1,333.0**	1,424.8*	2,759.5***	5,165.6	5,493.7	1,888.0*	14,956.0
		[4.85]	[577.4]	[777.0]	[995.0]	[3,435.4]	[4,765.9]	[944.0]	[10,181.9]
	Observations	475	475	458	475	475	475	475	475
Avg 2012-2013	Won lottery	10.59**	819.3**	1,144.5**	2,122.7***	4,696.3	4,303.3	2,042.5***	11,502.9
		[4.11]	[396.0]	[426.9]	[583.1]	[2,882.8]	[3,962.3]	[694.7]	[7,818.4]
	Observations	496	496	494	496	496	496	496	496

The top 1% values have been replaced by 99th percentile

5B INVERSE HYPERBOLIC SINE TRANSFORMATION (IHST)

		5B.1	5B.2	5B.3	5B.4	5B.5	5B.6	5B.7	5B.8
		Cons/day	Formal Savings	Informal Savings	Total savings	Durable Goods	Livestock	Crop Stock	Assets and Savings
août-11	Won lottery	0.01	0.05	0.24	0.20	0.33	-0.02	0.10	0.12
		[0.04]	[0.27]	[0.28]	[0.25]	[0.25]	[0.23]	[0.20]	[0.13]
	Observations	469	470	453	470	470	470	470	470
2012	Won lottery	0.14***	0.57**	0.32	0.66**	0.10	0.44	0.34	0.17
		[0.04]	[0.26]	[0.37]	[0.27]	[0.19]	[0.27]	[0.25]	[0.12]
	Observations	462	462	431	462	462	462	462	462
2013	Won lottery	0.05	0.39	0.32	0.43	0.10	0.70*	0.22	0.26**
		[0.05]	[0.32]	[0.23]	[0.27]	[0.23]	[0.35]	[0.14]	[0.12]
	Observations	475	475	458	475	475	475	475	475
Avg 2012-2013	Won lottery	0.09**	0.41	0.31	0.51**	0.12	0.60**	0.30*	0.22*
		[0.04]	[0.25]	[0.22]	[0.20]	[0.19]	[0.29]	[0.15]	[0.11]
	Observations	496	496	494	496	496	496	496	496

Robust standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

All regressions include village fixed effects and no additional control variable

Table 6: Impact of Voucher on Housing Investments

Dummies for whether that part of housing conditions was changed (OLS Regressions)

		6.1	6.2	6.3	6.4	6.5	6.6	6.7
	VARIABLES	**Housing Improve-ment (any)**	**Walls**	**Ceiling**	**Floor**	**Latrine**	**Energy for cooking**	**Energy for light**
2011	Won lottery	-0.04	0.00	0.01	-0.04**	-0.01	-0.01	-0.01
		[0.03]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.01]
	Observations	470	470	470	470	470	470	470
2012	Won lottery	0.04	0.02	-0.01	0.03	-0.02	-0.00	0.02
		[0.04]	[0.03]	[0.02]	[0.03]	[0.02]	[0.01]	[0.03]
	Observations	462	462	462	462	462	462	462
2013	Won lottery	0.04	0.08***	0.05*	0.05*	0.04	0.02	0.03
		[0.04]	[0.03]	[0.03]	[0.03]	[0.02]	[0.02]	[0.02]
	Observations	475	475	475	475	475	475	475
Avg 2012-2013	Won lottery	0.04**	0.06***	0.03	0.04*	0.02	0.01	0.02
		[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.02]
	Observations	496	496	496	496	496	496	496

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1
All regressions include village fixed effects and no additional control variable

Table 7: Other Agricultural Production

7.A REGRESSIONS IN LEVEL

	VARIABLES	7A.1 Animal Sales	7A.2 Fertilizer other crops	7A.3 Production other crops (MZN)	7A.4 Self-consumption other crops (MZN)	7A.5 Sales other crops (MZN)
2011	Won lottery	-	3.3	452.5	-24.0	-328.5
		-	[6.5]	[836.4]	[53.3]	[458.6]
	Observations	-	504	470	470	470
2012	Won lottery	1,211.4	14.5**	2,973.7	-6.8	1,047.2*
		[723.7]	[6.9]	[2,032.3]	[97.8]	[522.0]
	Observations	462	456	462	462	462
2013	Won lottery	95.1	3.2	1,713.8	196.5***	1,609.1**
		[559.6]	[6.2]	[1,062.6]	[64.6]	[770.1]
	Observations	475	472	475	475	475
Avg 2012-2013	Won lottery	998.0*	9.1	2,148.8*	97.3*	1,281.2**
		[537.9]	[5.5]	[1,085.6]	[52.7]	[553.6]
	Observations	496	496	496	496	496

The top 1% values have been replaced by 99th percentile

7.B INVERSE HYPERBOLIC SINE TRANSFORMATION (IHST)

	VARIABLES	7B.1 Animal Sales	7B.2 Fertilizer other crops	7B.3 Production other crops (MZN)	7B.4 Self-consumption other crops (MZN)	7B.5 Sales other crops (MSN)
2011	Won lottery	-	0.04	-0.26	-0.29	-0.06
		-	[0.15]	[0.28]	[0.28]	[0.26]
	Observations	-	504	470	470	470
2012	Won lottery	0.10	0.38**	0.81***	0.28	0.59*
		[0.29]	[0.15]	[0.30]	[0.29]	[0.33]
	Observations	462	456	462	462	462
2013	Won lottery	0.41	0.18	0.45**	0.32	0.54
		[0.28]	[0.15]	[0.22]	[0.20]	[0.39]
	Observations	475	472	475	475	475
Avg 2012-2013	Won lottery	0.32	0.32**	0.62***	0.31*	0.66**
		[0.23]	[0.12]	[0.19]	[0.17]	[0.27]
	Observations	496	496	496	496	496

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1
All regressions include village fixed effects and no additional control variable

Appendix Table 1: attrition conditional on Voucher treatment

VARIABLES	First Follow-up	Second Follow-up	Third Follow-up
Won lottery	-0.016	0.048	0.005
	[0.023]	[0.035]	[0.024]
Constant	0.114***	0.096***	0.090***
	[0.021]	[0.025]	[0.020]
Average attrition rate:	**8.6%**	**10.0%**	**7.6%**
(vis-à-vis initial sample)			
Observations	514	514	514

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1
For each round of the 3 follow-ups, we use a dummy = 1 if the household was not surveyed at the round in question.
All regressions include village fixed effects and no additional control variable

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