Labor Market Effects of Agricultural Mechanization: Experimental Evidence from India

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Abstract

Agricultural mechanization plays an important role in countries' productivity growth. Through labor markets, mechanization's impacts extend beyond mechanizing farmers themselves. This study randomized a program at the labor market level that reduced farmer barriers to mechanization. By combining government farmer training infrastructure with rentals, the intervention tripled the uptake of a technology that replaced manual rice transplanting. At the farmer level, labor costs decreased with no change in yields or other expenses, leading to an 8% increase in profits. At the labor market level, mechanization led to a 7% decrease in days of labor demanded for transplanting, which translated to a 6% decrease in piece-rate transplanting wages. Beyond the first-order distributional impacts, the wage effects reduce the relative profitability of further mechanization. This is illustrated in a quantitative model calibrated to the experimental results. The model shows how the same reduction in the fixed cost of adopting mechanization under a shallower labor supply curve results in larger increases in productivity with more income for workers. The study shows how labor market frictions shape how mechanization impacts workers, farmers, and economies.

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1 Introduction

No country has gotten rich without also experiencing an increase in agricultural labor productivity. Developing countries' agriculture sectors are laggards in the race for growth. The productivity gap between the wealthiest 5% and the poorest 5% of countries is fifteen times greater in agriculture than in non-agricultural sectors (Restuccia et al., 2008). Mechanization significantly contributes to the agriculture labor productivity gap across countries, accounting for up to 37% of the difference (Caunedo and Keller, 2020). A key barrier to the adoption of mechanization is the small scale of farms in developing countries, which makes buying most machines unprofitable (Foster and Rosenzweig, 2022). One potential solution is rental markets, but these are often absent for new technologies.

In addition to boosting productivity, mechanization also often has distributional consequences. In particular, replacing labor with capital mechanization is likely to affect rural labor markets. However, how wages evolve in response to mechanization is theoretically ambiguous. If workers can compete for jobs across the economy, higher productivity per worker should translate to higher wages, but if workers' only option is the mechanized task itself, wages will fall in response to decreased labor demand. The degree to which workers are willing to continue to supply labor regardless of wage for the mechanized task, the labor supply elasticity, determines how far wages fall. The distributional impacts when wages fall create a tension between improving productivity and protecting workers.

Falling wage's impact extends beyond workers, feeding back into the pace of mechanization, which is often slow. Tractor adoption in the US took over fifty years, in large part because rural wages took that long to rise to a point where tractors were universally profitable (Manuelli and Seshadri, 2014). Any decrease in manual labor costs reduces the potential profit from mechanizing for farmers still operating manually. Thus, early adopters of mechanization pushing down wages can extend the wait for capital to be cheap enough relative to labor for additional farmers to mechanize.

This paper studies mechanization and its consequences in the context of rice farming in the

Indian State of Telangana. Drum seeders are a relatively low-cost device that significantly enhances the labor productivity of rice planting by avoiding transplanting, which accounts for 23% of the cost of rice cultivation. To adopt drum seeders, farmers need access to both the devices themselves and the skills to use them. While active private rental markets exist for well-known technologies like tractors, none exist in the study context for drum seeders. Entrepreneurs are not incentivized to provide training as it is a costly public good. Governments provide agricultural training as a public good through a system of agricultural trainers, extensionists. However, extensionists offer skills, not capital. By working with the Government of Telangana to coordinate extension with village government drum seeder rentals, the study facilitates drum seeder adoption.

A key advantage of working with the government is the ability to deploy this program at scale randomized across villages. Rice transplanting in Telangana is a setting where randomizing at the village level makes studying labor market effects possible. Transplanting is done by locally hired female labor. While men travel between villages to work, women do not, which keeps the labor market segmented at the village level. Only 2.5% of women worked outside their village during the primary wet season, which is the focus of the results. In addition to being segmented, village labor markets are observable. Unlike settings dominated by family labor, even small farmers in Telangana hire transplanting labor.

The study uses primary data collected from surveys of approximately 2,200 farmers in 342 villages sampled from the administrative register of rice farmers. The primary outcome data were collected after the main monsoon rice season and during the planting period for the secondary dry rice season. This sample exhibits similar demographics to the overall rural population for the study districts, as a wide range of farmers grow rice in this region. The survey encompasses data on all farming costs and revenue, with meticulous collection of task-specific wages. The survey also included detailed data on farmer training and the government workers delivering it in the villages. The second major data source is phone surveys of female workers, which provide insights into work locations and government job scheme utilization during the rice transplanting period.

This study presents three main sets of results. The impact on drum seeder adoption, the impact on farmers, and finally the distributional consequences.

First, the program led to a nearly 300% increase in drum-seeded acreage in the primary rice season (an increase from 0.04 to 0.15 acres per farmer [1]. In addition to using the rentals, farmers in treatment villages reported receiving more training on drum seeders as designed. Further, the uptake effects appear durable, persisting into the secondary wet season.

Second, drum seeders improved farmers' labor productivity. Farmers in treatment villages demanded 7% less labor per acre. They did not use any more family labor or spend significantly more on other inputs, as compared to control farmers. Despite the reduced inputs, farmers in treatment villages experienced similar or even slightly higher yields than control farmers at the end of the season. With equivalent revenue and lower total costs, including the rental fee, farmers in treatment villages achieved profits per acre of rice that were 8% higher than in control villages.

Third, the reduction in labor demand had distributional consequences by leading wages for transplanting to fall. Overall, farmers in treatment villages spent 10% less on hired labor. Farmers who did not adopt drum seeders reported 6% lower piece rate wages for transplanting in treatment villages. While the identity of farmers who transplant rice in treatment villages is endogenous, the wage is an equilibrium object at the village level. Farmers competitively bid against each other within the village to attract transplanters on a given day. The wage decreases, in part, reflect women having few other job options outside these transplanting labor markets. There is suggestive evidence from the laborer survey that workers did not find other jobs in the short term. Workers who were transplanters reported a greater unmet demand for days of paid manual work from a government job scheme, workfare, during the transplanting season. The laborer survey found no evidence that the transplanters immediately found jobs in other villages or sectors.

The labor market effects mean even farmers who did not adopt drum seeders benefited from lower labor costs. This is a direct implication of the lower wages transplanting farmers reported in treatment villages. There is still a significant negative treatment effect on labor costs and positive

[1] The adoption of mechanization is notoriously slow. The modest increases are expected as primary crops mechanize slowly over many seasons (Gulati and Juneja, 2020)) as farmers learn from their neighbors (Foster and Rosenzweig, 1995). Tractors took many years to spread in the US (Manuelli and Seshadri, 2014)

treatment effects on profits when the sample is restricted to only transplanting farmers. [2]. The wage reductions mean that drum seeding became relatively less profitable in treatment villages. This effect, in which mechanization reduces the profitability of further mechanization, is driven by the slope of the labor supply curve.

To explore the interaction between labor markets and mechanization further, the results inform a quantitative model. This model calculates how lowering barriers to mechanization impacts productivity and wages under varying labor market conditions. Crucially, the labor supply curve's slope dictates the extent to which reduced mechanization costs reduce wages and boost technology adoption. The model is calibrated using the estimated labor supply elasticity from this experiment as a baseline. The first scenario simulates implementing higher reservation wages alongside mechanization subsidies, which represents the effect of better workfare implementation during the transplanting season. The second scenario simulates reduced wage sensitivity to labor demand shocks, reflecting easier worker transitions to alternative jobs. This would correspond with government policy interventions to increase labor market opportunities for women. Both policy counterfactuals illustrate how labor market policy can not only blunt the distributional contexts of mechanization but also increase the effectiveness of policies designed to encourage mechanization.

The study's first contribution is demonstrating a way for governments to promote mechanization without large capital subsidies. The extensive literature on agricultural technology adoption focuses on technologies where the primary barrier is either skill (Conley and Udry, 2010) or capital (Caunedo and Kala, 2021) alone, but few papers address both barriers simultaneously. Prominent social enterprises leverage donor funds to combine training and input access (Deutschmann et al., 2019), but this study explores a model augmenting existing government training infrastructure with self-financing rentals. The drum seeders' purchase cost was equivalent to 22 days of rentals at the study's price, at most two seasons. Other research emphasizes the importance of rental markets for improving access to capital goods (Bassi et al., 2022) but does not explore how these markets form. A private sector rental market had not emerged in the control arm of the study, suggesting

[2] While these regressions are not causal since who transplants in the treatment group is endogenous, the implied TOT effects if there was no treatment effect on non-drum seeding farmers are unreasonably large

government intervention was required to kick-start adoption [3]. The rental aspect of the scheme was fiscally sustainable. Integrating rental markets offers a promising avenue for governments to fully capitalize on existing agricultural training investments [4].

Second, the study contributes to the literature on the impact of mechanization on agricultural labor markets by using a market-level experiment to show how mechanization can affect wages and labor demand. National-level studies (Bustos et al., 2016) are limited to the long-run effects of labor displacement and cannot analyze mechanization's impact on non-adopting farmers. On the other hand, household-level randomization of mechanization subsidies (Caunedo and Kala, 2021) can only identify farm household labor supply decisions rather than changes in the labor market. My study bridges the national and household level literature to identify the labor market spillovers of mechanization. Other evidence on technology adoption and labor markets demonstrates how labor shortages constrain the adoption of labor-complementing technologies like irrigation (Jones et al., 2022). This paper illustrates how mechanization can reduce labor shortages, highlighting how labor market structure mediates the profitability of different technologies along the path of agricultural transformation. The slow pace of rural wage increases partially determined the slow pace of tractor adoption in the US (Manuelli and Seshadri, 2014). Parts of the American South with exogenously tighter labor markets also mechanized faster (Hornbeck and Naidu, 2014). Therefore, the observed wage effects likely influence the long-term pace of mechanization and agricultural transformation.

Third, the study also contributes to the literature on rural labor markets. The results reinforce the finding that rural labor markets only experience seasonal slack (Breza et al., 2021). Further, in contrast to the overall downward rigidity in response to seasonal reductions in agricultural activity (Kaur, 2019), peak agricultural wages are very sensitive to changes in labor demand. The results in Muralidharan et al. (2023) highlight how rural wages increase with more credible outside options. This study shows when mechanization removes job options wages decrease.

Fourth, this study contributes to an emerging literature on low female labor force participation,

[3] I briefly attempted to partner with an NGO that had identified village entrepreneurs interested in renting drum seeders, but the entrepreneurs were not willing to take a risk on buying the devices.

[4] A neighboring state, Andra Pradesh, has combined agricultural equipment rentals with extension at integrated farmer services hubs called Rythu Bharosa Kendras (Reddy et al., 2023)

particularly in India. This study partially traces the downward-sloping part of the U-shaped curve relating development and female labor force participation. Mechanization also displaces women at a national level, districts in India more agronomically suited to mechanization have lower female labor force participation but no reductions in male labor (Afridi et al., 2023). This study reinforces the results that Indian women are more impacted by agriculture labor demand shocks, such as droughts, than men due to limited outside options (Afridi et al., 2022).

Finally, this study provides a clear illustration of the broader tension countries face between increasing productivity and protecting workers [5]. Recent theoretical work emphasizes the need to consider mechanization separately from other kinds of technical change in general equilibrium (Acemoglu et al., 2024). As the quantitative model illustrates, strengthening the social protection system can lead to more mechanization while reducing negative impacts on workers. Governments can potentially play a role in helping displaced workers find other jobs, which affects not just their own welfare, but also the pace of mechanization overall. Recent evidence suggests retraining programs in the US are modestly successful (Card et al., 2010) (Hyman, 2018). This approach of intervening in the labor market contrasts with politically popular[6] but economically harmful attempts to save jobs by slowing mechanization.

2 Background

All developed countries have transitioned away from manually transplanting rice as part of their structural transformation process. In the US, farmers seed rice from airplanes. Research in Italy studying the historical transition from manual transplanting in the 1950s suggests that in the long run, rice regions had higher female labor force participation and female political participation (Bagnato et al., 2023). The authors argue that the high level of organization among rice transplanting groups had long-term benefits long after the task was mechanized away.

Interest in drum seeders in Telangana has been driven by changes in wages rather than the sud-

[5] This tension is at the core of industrial policy debates in India and around the world (Muralidharan, 2024)

[6] Politicians from the Luddites to Gandhi have long highlighted that workers can pay a price for productivity and thus opposed mechanization.

den invention of the technology. In fact, drum seeders were first promoted by the International Rice Research Institute (IRRI) in India over a decade ago. These simple plastic devices are now manufactured locally in India. They are simple enough that at least one farmer made one before the study. However, the government and farmers' interest in technology has only emerged recently. Two big shifts drive this. First, transplanting labor marketized with even small farmers paying each other to working in each other's fields. Second, these wages increased dramatically in the past 10 years, nearly 45% in real terms. During piloting, village leaders consistently cited a "labor shortage" as the most pressing concern facing farmers. Of course, what these farmers really meant was a labor shortage at the prevailing wage. I hope to explore the drivers of these changes in future work, but irrigation expansion leading to expanded rice cultivation seems to be an important driver of the wage increases[7]. Overall increases in household wealth associated with the rapid development of Hyderabad, the major metropolis 2-4 hours from the study villages, may have also allowed more families to afford to not engage in agricultural labor. Transplanting, the task drum seeders replace, is particularly unpleasant as it involves wading crouched over in mud for hours at a time [8]. A willingness to pay study found females in Indian farming households had a higher willingness to pay for drum seeders than males (Khan et al., 2016). Women, especially those who actually transplant, also place a higher value on mechanical transplanting machines than their husbands (Gulati et al., 2024). There are health risks associated with spending extended periods in flooded rice fields, including greater exposure to water-born diseases (Vent et al., 2016). Thus, while the labor displacement in my study might reduce wage income, its welfare impact is more complex.

In the absence of mechanization, farmers had to pay what village leaders called "unreasonable" wages to ensure their fields were transplanted, pulling in new workers at high wages. We can directly observe these "unreasonable" wages by comparing the distribution of manual weeding, an optional time-flexible task, to transplanting wages. While both distributions have the same medians, there is a large mass of high transplanting wages that do not exist for weeding (Figure 5). Farmers

[7]Irrigated area has increased by 117% and rice production by 342% in Telangana from 2015 to 2022 (Planning Department, 2023)

[8]Transplanting, when offered as a luxury activity at resorts is coupled with spa treatments 6

report trying to stagger their transplanting to avoid these peak wages, but once the rice seedlings are matured in their nurseries farmers have a relatively short window to transplant them into the main field. In villages with canal or rain-based irrigation, it is also important to ensure the rice is transplanted while there is still ample water to flood the fields. Leveraging remote sensing to better understand the environmental determinants of observed transplanting wages is an area of ongoing analysis.

As has been found in other contexts (Aker and Jack, 2023), farmers struggle to adapt to changing climates, economic and meteorological, without training. Drum seeding itself simply involves rolling a drum seeder along a leveled and damp rice paddy to place the rice seeds in neat rows along the field. However, farmers must make other adjustments to their farming practices to ensure they drum seed successfully. Farmers must adjust their land preparation, irrigation, and weed management when using drum seeders. The farmers must ensure the field is well leveled so that there are no deep puddles of water where the rice won't germinate [9].

A major concern farmers have with drum seeding is weeds. While the relative weed susceptibility of drum-seed rice is debated by agronomists, farmers feel the method is more weed-prone. Weeds were the number one reason farmers who had tried drum seeding at some point in the past reported no longer using drum seeders (Figure 1). However, a field can become weed infested at any time so part of the complaints could be driven by farmers associating weed infestations with drum seeding. Very few farmers practice the recommended weed management regimen: spraying a limited amount of herbicide before weeds emerge in the early season, followed by manual weeding in the late season. Most spray only after weeds emerge, and many skip manual weeding altogether. The strongest correlate of yields among drum seeding farmers was weed management knowledge: specifically to flood the field again after germination as a form of weed control (Table 10). Thus, access to training across the season and not just at the time of rental is important.

Given the knowledge required to successfully use drum seeders across the season, it is clear farmers require both extension and access to make the best use of drum seeders. This was especially

[9] A surprising cause of germination failure is birds eating the exposed seeds, which can be partially addressed by leaving excess seeds in piles near the fields for birds to eat

true in villages without any historical drum seeder use.

3 Intervention

The Telangana Department of Agriculture has been promoting drum seeding as one of its priority technologies. In 2020 the state government invested massively in agriculture extension infrastructure: ensuring every 5,000 acres of cultivated land had one farmer meeting hall staffed by a college educated government agriculture extension officer. These officers managed agriculture subsidy schemes and provided training. During field visits, village leaders consistently flagged transplanting labor shortages as farmers' top issue and expressed interest in drum seeders. Interestingly, while leaders recognized earnings from transplanting wages were important for many of their constituents, they felt promoting drum seeders would not hurt transplanters since there was a "labor shortage." Extension agents complained about an inability to mobilize farmers to attend meetings and the difficulty of promoting devices they could not directly provide. I worked with the Telangana Department of Agriculture to develop a pilot program that would address extensionists' and village governments' concerns.

The primary intervention was supplying village governments with drum seeders and allowing them to rent the drum seeders to their citizens. The devices were rented for 250 INR per day. Farmers were allowed to share the rental as one machine could cover up to 4 acres in a day. The income from the rentals was deposited into the village government's bank account. Village governments were told the money could be spent on village agricultural development following the norms for other village spending. The actual government order formalizing the spending guidelines did not pass until after the experiment.

The role of the extension agent in the rentals was randomized, with some extension agents directly managing the rentals on behalf of the village governments and other extension agents focusing solely on promotion activities, with the village government completely managing the rentals. The goal of this sub-experiment was to better understand the role village elected governments can play in extension, but in practice, local elected leaders involved themselves in both arms (Table 8) (Table 7). Thus, I will focus on the pooled treatment for the remainder of the paper.

4 Data

The project collected data from two main overlapping sampling frames. The farmers were surveyed from a database of farmers and cultivated land maintained by the agriculture department. Several aspects of this dataset build its credibility. First, inclusion in this database is necessary for farmers to get a sizeable cash transfer called Rythu Bandhu or PM-Kisan. Thus, the department devotes considerable time and resources to ensuring these records are complete. The data is also linked to land records which further reduces the risk any farmed area is excluded. The sampling frame was restricted to farmers recorded as cultivating rice. I also drew two small supplemental samples of drum-seeding farmers discussed in Appendix A. These supplemental samples were used for non-causal regressions looking at predictors of yield for drum seeding farmers.

The second sampling frame is female laborers from the workfare, National Rural Employment Guarantee Scheme (NREGS), rolls [10] These are women from households that wanted the opportunity to take part in India's primary rural welfare scheme, NREGS. 80% of households in my farming sample also had NREGS cards. While not all women who appear on the NREGS rolls are transplanters, nearly all transplanters take advantage of NREGS work during periods when other agricultural work is not available. See Figure 3 for a summary of the sampling frames.

While neither sampling frame is perfectly representative of the population of my study villages, they capture a wide swath of the relevant population. Thus, my data can be used to illustrate the heterogeneous impacts of easing access to drum seeders, which is the study's primary goal.

Similarly, the village sample is not fully representative due to the government's requirement that the village elected leaders opt-in to participate in the study. This means the results might not generalize to the 33% of villages where the village leader did not consent. Often, these were villages

[10] I scraped job cards from the NREGS website and then asked village-level officials to track the women on the job cards. Note that with mandatory linking of biometrics, fake job cards have largely disappeared from Telangana.

with less paddy cultivation, but they were also less likely to have any drum seeders and had slightly lower transplanting wages on average. Nonetheless, the selected villages broadly cover the selected districts geographically (Figure 7) and represent a wide range of previous drum seeder exposure and prevailing transplanting wages. Also, a state government operating a similar program at scale would have to rely on village leaders' consent, which might provide a helpful layer of self-targeting.

Strata were grouped into four categories based on how the variables used for randomization predicted administrative drum seeder uptake. Based on predicted uptake, the sample size targets ranged from 4 to 12 to help ensure that drum-seeding farmers were included. Thus, the primary results represent a hypothetical set of villages that were slightly better targeted. A scaled-up program could simply use the results of this study to screen villages better based on the data collected to form strata.

Notably, the final farmer sample matches the 2019 demographic and health survey (DHS) sample for the study districts relatively closely. The comparison between the sample and the DHS, designed to be representative at the district level, is complicated by the amorphous definition of rural in the Indian context. The DHS sample includes both urban and rural areas, but many areas classified as rural are, in fact, small towns with limited farming activity. Thus, I present a comparison to the full sample for the six districts (Table 2), the sample of individuals owning agricultural land (Table 3), and finally, individuals residing in areas classified as rural (Table 4). Notably, the three variables most important for drum seeder uptake do not have significant differences between my sample and the DHS sample: acres owned, household head education, and irrigation pump ownership. Wealth proxied by televisions, pressure cookers, and livestock is similar across samples.

5 Timeline

Telangana has two rice growing seasons: a winter dry season (November to March) and a summer wet season (June to October). While drum seeding during the wet season can make water management more challenging, there is also more rice cultivation during the wet season overall. The project began halfway through the planting period of dry season 2022-2023. I considered this a pilot season. The primary season discussed in this paper is the wet season of 2023. Before the start of this season, we surveyed village leaders and agricultural extensionists and identified workers from the NREGS rolls for the laborer survey. After planting but before harvest, we conducted the first farmer survey and asked detailed household and agronomic questions. Telangana then entered its state election period, which meant the state government was far less involved in monitoring the drum seeder program. Thus, the treatment effects in this second season can be seen as evidence of the durability of the uptake. However, increasing uptake driven by learning from others will likely not occur until the subsequent dry and wet seasons since farmers consider them agronomically separate. The primary survey with data on full wet season yields, costs, and wages took place after the state elections but before the national elections, from February to March of 2024. A follow-up phone survey of transplanters took place in March and April, and a final phone survey to capture dry season yields for farmers took place in May and June 2024. See figure 4 for a full timeline.

6 Randomization

This study was randomized at the village level, which allowed me to study village-level effects but limited the scope for stratification.

As mentioned, villages were screened before randomization based on the village elected leaders' willingness to participate. Part of this screening included a limited questionnaire for village elected leaders, which formed the basis for the randomization since the timeline did not accommodate a baseline. Before randomization, one village was selected per extension agent to ensure no agent served both a treatment and control village. Among the villages served by an extensionist willing to participate in the study, I included the village closest to the extension agent's office in the randomization. The randomization was stratified by initial transplanting wage and drum seeder uptake as well as the district. I randomized 342 villages. Randomizing only villages served by different extensionists led to greater spacing between treatment and control villages. The distance helped to limit the risk of spillovers driven by the 3% of female transplanters who report doing some work outside their village during the transplanting season. Since treatment and control villages were seldom neighbors (Figure 7), the likelihood that control villages pulled labor from treatment villages is very low.

The resulting randomization led to a sample of farmers that is balanced between treatment and control villages (Table 1). Unfortunately, the 2022 administrative rice cultivation data was unavailable for some farmers due to an inconsistency in IDs between different rounds of data shared by the government. Household characteristics are also missing for the 3% households that were only surveyed in the second round of data collection due to logistical issues in the first survey round.

7 Empirical Strategy

The main specification for the following results is the standard approach for cluster randomized controlled trials.

$$Y_{ivse} = \beta_1 T_v + \chi_e + \gamma_s + \varepsilon_{vi}$$

Y_{ivse} is the outcome variable. T_v is the treatment dummy for village v indicating whether drum seeders were rented. χ_e are enumerator fixed effects. γ_s are strata fixed effects. Finally, ε_{iv} errors clustered by village. In this specification, β_1 is the treatment effect.

Note that I can't use the standard approach of estimating treatment on the treated regressions to isolate the effects of drum seeding alone. This is because there are substantial spillovers from farmers up-taking drum seeding to those not up-taking drum seeding driven by the wage effects. This violates the exclusion restriction required to use village treatment assignment as an instrument for drum-seeding as in the standard treatment on the treated approach. The treatment affects non-compliers, e.g., farmers who do not drum seed, by lowering their labor costs. This means dividing the profit treatment effect by the fraction of drum-seeding farmers would overestimate the profitability of drum seeding. Thus, in the results section, I focus on the overall intent to treat effects, which compares outcomes for all farmers sampled in treatment villages to all farmers sampled in control villages. The only identifying assumption required for these results is that the randomization is independent of any confounding variables.

8 Results

The project successfully increased farmers' awareness of drum seeders and drum seeder farming practices (Table 5). It also successfully got local elected leaders to engage in promoting drum seeders (Table 8). However, the actual number of farmers experimenting with drum seeders was relatively small: 10 farmers per village based on an average of 315 farmers in a village (Table 7). There are several possible reasons for the low absolute uptake. First, farmers do not generally make dramatic changes to how they cultivate their primary crop quickly. There is a prisoner's dilemma created by farmers waiting to see the success of earlier adopters before making their own adoption decisions. Second, qualitative data showed farmers placed outsized signaling value on early adopters' yields. One negative or positive experience can shape an entire village's attitude toward the technology. For example, in some villages, farmers were convinced drum seeders led to higher yields because the early adopters had used drum seeding as an opportunity to try more expensive seeds. In a neighboring village, a farmer tried a different new seed along with the drum seeder, which the farmer blamed for the poor yields. However, the narrative in the village was focused on the drum seeder. In aggregate, I don't find statistically significant treatment effects on other input expenditures beyond labor (Table 12). The importance of farmers learning from others partially explains why uptake was not uniformly spread across study villages (Figure 2). The majority of treatment villages had no drum-seeding farmers in the sample, but in other villages, large numbers of farmers switched to drum-seeding. These high uptake villages tended to have some previous experience with drum seeders which likely primed farmers' interest in trying the technology.

For the labor market effects, the acreage converted to drum seeding matters more than the number of farmers adopting the technology. Importantly, larger farmers were much more likely to adopt drum seeding (Table 9) than small farmers. The average village in the sample had 315 paddy farmers which means the treatment effect would translate to approximately 35 acres per village. An important caveat is that a few very large farmers decided to adopt drum seeders in treatment villages according to extension agent reports in a handful of villages. These farmers drum seeded 20 to 30 acres by themselves, but were not sampled in the survey. Thus the acreage estimate is likely a conservative estimate of the overall effect. The smaller treatment effect on dry season acreage is likely driven by the overall reduction in paddy cultivation in that season. Only fields with a reliable water supply can be cultivated during the dry season. Thus, long-term adoption dynamics will require following these villages for many more seasons [11]

Drum seeder adoption had the expected decrease in hired female labor days (Table 11). This variable reflects females hired labor across all tasks, but since harvesting is mechanized, transplanting is the only major agricultural task requiring large numbers of women in this context. The intent to treat effect of .73 fewer hired female workers per acre roughly corresponds with the 10 fewer hired laborers per transplanted acre expected when the fraction of drum-seeding farmers is taken into account. Since these are intent to treat effects, any additional hiring by non-drum seeding farmers induced by the lower wages is taken into account in this estimate. To reconcile these results with the uptake results, there would have to be minimal additional hiring by non-drum seeding farmers. Since the transplanting operation takes a fixed amount of labor per land area no additional hiring makes sense. Thus, this result suggests that, aside from the choice of drum seeding, labor demand for female hired labor is inelastic.

This decrease in labor demand translated to decreases in labor expenditure in treatment villages (Table 12). There were no major compensating changes in family labor or other expenditures. The different numbers of observations in these regressions reflect both zeros as the variables are in logs and the exclusion of farmers who felt they couldn't accurately report expenditures despite surveyors' efforts to break up expenditures into sub-categories.

While it seems clear drum seeders saved farmers money, that would be meaningless if it came

[11] I am currently partnering with remote sensing experts in an effort to track long-run uptake at the village level using remotely sensed flooding patterns, which can provide a good proxy as to whether a field was transplanted or not.

at the expense of revenues. However, I see some evidence that farmers in treatment villages were able to obtain higher revenues than those in control villages (Table 13). At the time of the survey, a number of farmers were not able to recall the price they sold their rice for the previous season. However, the distribution of prices is quite narrow so it is possible to impute using district median prices. The modest increases in revenue could be driven by a few factors. One possibility is that by avoiding large transplanting labor expenses, farmers were able to make small additional non-labor expenditures that, while not statistically significant individually, mattered for revenue. For example, decisions by individual farmers to buy slightly more expensive seeds, fertilizers, additives, or herbicides could end up having an aggregate impact on yields, which also appear to rise modestly (Table 14). Another possibility is that farmers paid more attention to land leveling because they were planning on using drum seeders. Other studies have shown that land leveling is important for rice yields overall since it ensures the even distribution of water and other inputs. (Ali et al., 2018) (Magnan et al., 2015).

Overall, the revenue and cost effects translate into a positive and significant treatment effect on winsorized and unwinsorized profits using the imputed revenues (Table 15). Note that not all of these profit effects come from the drum-seeding farmers alone. Since wages for transplanting decrease in treatment villages even farmers who don't adopt drum seeding benefit from lower labor costs. As previously mentioned some of these lower labor costs could lead to small re-optimizations that improve yield and profits as well. Assuming labor demand for transplanting is perfectly inelastic, since it takes a fixed number of workers to do the task on a given farmer's land, the wage effects alone would drive a 5 percent decrease in labor costs as compared to the 10 percent observed.

Farmers pay transplanters in two ways: Wages per acre transplanted and wages per day. I find wages decrease for both types of wages individually (Table 16) as well as when they are pooled (Table 17). The smaller sample sizes are because some farmers just reported total labor costs. This data is included in the total labor cost per acre regression, which is significantly negative for transplanting farmers even though the total days hired is not (Table 18).

The wage per acre, although the rarer type of wage, is the preferred result since it is a piece rate wage, so it should be unaffected by productivity differences. The primary driver of wage differences seems to be a steep labor supply curve in periods of peak labor demand. The treatment villages have fewer peak wages associated with labor market congestion, as seen in this histogram comparing daily wages in treatment and control areas (Figure 5).

The fact that transplanting wages went down means that while the treatment appears to have been a success focusing on the farmer sample, not everyone benefited. Transplanters in the laborer sample reported 2 more days of unmet demand for workfare days in treatment villages in the preceding two weeks compared to the control villages. This suggests that NREGS was not responding as it should have to cushion transplanters against the labor demand shock. Although noisier, I find no evidence of more NREGS workfare days offered in treatment villages using the program's administrative data.

In the longer run, the goal would be for the displaced transplanters to find jobs in other sectors. Increasing the participation of women outside the agricultural sector is a major policy goal in India. In the second laborer phone survey I asked whether workers' husbands would let them work outside their villages. Interestingly, the overall effect appears to be husbands getting more restrictive, but husbands of women who do transplanting work each season become less restrictive. The likely mechanism is that husbands who are benefiting from greater farming profits in treatment villages can afford to become more restrictive as has been the general trend in India (Afridi et al., 2023), but husbands who primarily experience treatment as negative wage effects become more willing for their wives to search for work in other locations. There are no significant treatment effects on women working outside their home villages, but it would likely take women multiple seasons to identify new job opportunities. Further, there is still a substantial amount of transplanting work in treatment villages. There is no treatment effect on the extensive margin of whether women did any transplanting work (Table 19).

9 Model

In this section, I develop a model of agricultural labor markets. The model's primary purpose is to illustrate how differences in labor market structure would have led to different wage and technology adoption outcomes from the experiment. In particular, the experiment is conceptualized as a reduction in the fixed cost of adopting drum seeders. The experiment is used to estimate the baseline calibration of the model. Then, the model is used to assess how the same reduction in the fixed cost of adoption implied by the experiment would impact the uptake of mechanization and wages under different labor market conditions. These results help illustrate how labor market and mechanization policies interact.

9.1 Model Environment

The core of the model is a technology and labor supply decision made by households endowed with land L_i and a worker. Households decide whether to adopt drum seeding $a_i = 1$ or continue to transplant, conditional on their landholdings and the equilibrium wage for transplanting w_T^*. They also decide whether the worker should work as transplanters or not t_i. Equilibrium is reached when the labor market for transplanting clears. The final share of land drum seeded, and the final transplanting wages are endogenous objects in the model, which I target for calibration. The treatment is modeled as a decrease in the fixed cost of accessing a drum seeder. The model is static since I only observe the short-run equilibrium in the experiment. The model is illustrated schematically in figure 8.

Household Technology Adoption Problem

Each household i maximizes its agricultural profits, which are conditional on the equilibrium w_T^* and land endowment L_i. The household decision variable is a_i, which is a binary variable indicating whether or not they adopt drum seeding. I do not allow for partial adoption since a given plot can only be planted in one uniform way for agronomic reasons. Also, partial adoption is only rational for farmers concerned about the risk of technology adoption. This model abstracts from risk in the technology adoption process to focus attention on labor market interactions. I also assume that individual farmers are small relative to the labor market and take the equilibrium wage w_T^* as given when making their technology choice. The profit maximization function is:

$$\max_{a_i} \Pi(a_i, w_T^*, L_i) = pf(a_i, L_i) - (1 - a_i) w_T^* h(L_i) - a_i \alpha \tag{1}$$

$$\text{s.t. } a_i \in \{0, 1\}$$

Farmer's revenues are a function of the land they cultivate and the exogenous price received for the output p. Due to widespread government procurement and centralized milling, rice output markets are relatively integrated, and there is no evidence of output price effects in the experiment's setting. If farmers do not adopt drum seeders they pay $w_T^* h(L_i)$ where w_T^* is the equilibrium wage and $h(L_i)$ is the labor required to transplant their land L_i. Note that the labor demand is modeled as purely a function of land since practically farmers plant the entirety of a rice plot. Other inputs, such as water and fertilizer, would be wasted if the entire plot is not planted. Thus, labor demand can be thought of as inelastic to wages conditional on the farmer's choice of planting technology. If farmers adopt drum seeders, they pay a fixed cost α to acquire the device but do not have to pay the variable cost of hiring transplanting workers [12].

For the empirical simulation, I chose linear production and labor demand functions. The labor demand is linear because the task is the same per unit area regardless of the amount of land planted. While yields vary with plot size, the exact nature of this variation is highly context-dependent, and assuming a constant yield is the most conservative assumption. Given these linear functional forms profits per acre are increasing in L if a farmer drum-seeds ($a_i = 1$), but not changing in L if farmers transplant $a_i = 0$. This matches the empirical result that farmers with large landholdings are more likely to adopt drum seeders than farmers with small landholdings.

Reformulating 1 assuming $a_i = 1$, $f(a_i, L_i) = \beta L_i$

[12] In practice, there are small variable costs associated with transplanting, but these tend to be done by male workers or family members who would expend a similar amount of effort overseeing a hired transplanting team

$$\frac{\Pi_i}{L_i} = p\beta - \frac{\alpha}{L_i}$$

$$\frac{d\frac{\Pi_i}{L_i}}{dL_i} = \frac{\alpha}{L_i^2} > 0$$

Reformulating assuming $a_i = 0, h(L_i) = \gamma L_i$

$$\frac{\Pi_i}{L_i} = p\beta - w_T^* \gamma$$

$$\frac{d\frac{\Pi_i}{L_i}}{dL_i} = 0$$

Household Labor Problem

Alongside the technology decision, households make a labor supply decision, which is taken independently of the agriculture technology decision. The assumption is that providing transplanting labor to a household's own farm is no different than providing it to another farmer in the village. An alternative motivation would be a scenario where labor is supplied by households who do not have any land and simply decide whether to transplant or take on other tasks based on wages. The household's labor supply is modeled as the standard labor leisure decision with days of transplanting work t_i considered separately from days of other work o_i. The time constraint T is the total number of working days available during the transplanting period. y_o is the income of the household. To the extent drum seeding improves overall household wealth through increased agricultural profits, it could increase y_o, further decreasing labor supply [13].

$$\max U_i(y_{0i}, t_i, o_i) = u(y_0 + w_t t_i + w_o o_i) + v(T - t_i - o_i) \tag{2}$$

See the Appendix C for a derivation of the labor supply elasticity assuming specific functional forms for $u()$ and $v()$

[13] In the empirical simulations, this effect could be thought of as simply a steeper labor supply curve

Market Clearing

The demand for transplanting labor must equal the amount of transplanting labor supplied for the transplanting labor market to clear. This market-clearing condition pins down the transplanting wage.

$$LD = \sum_i (1 - a_i)h(L_i) = \sum_i t_i = LS$$

Solving

Since the experiment only estimates the labor supply elasticity, the quantitative model uses a constant elasticity labor supply curve. The labor supply elasticity is defined as the percent change in labor supplied over the percent change in wage. Mathematically:

$$\frac{d \ln LS}{d \ln w_T} = \eta_T$$

where η_T is the labor supply elasticity

This implies an inverse labor supply equation of the form

$$w_T = (LS)^{\frac{1}{\eta_T}} + R$$

where R mathematically is the integration constant, and R economically is the reservation wage.

To simplify the problem, I assume farmers predict the adoption decisions of other farmers and solve their adoption problem based on a correct expectation of the equilibrium wage. Thus, the farmers solve backward, correctly guessing adoption and thus labor demand and wage before solving their own technology adoption problem. Qualitatively, farmers do report anticipating fewer wage spikes due to drum seeder adoption, but they can also learn about adoption and wages from segments of the village that plant and transplant relatively early.

Simulation and Calibration

First, some values are exogenous. The yield of 2000 kg per acre ($f(L_i) = 2000L_i$), the labor demand of 10 days per acre $h(L_i) = 10L_i$, and the rice output price of 2000 INR per 100 kg (p) are median values for the setting. There are 700 farmers with land sizes ranging from 1 to 8 acres in steps of .01 acres. The linear land distribution ensures the effect of wages on mechanization is consistent across different wage levels. The elasticity η_T I take directly from the experiment dividing the wage effect by the labor demand effect. The assumption is that labor demand is inelastic conditional on the farmers' choice of technology so the experiment traces the labor supply curve. The other parameters, reservation wage R, initial α_0, and final α_1, are calibrated by matching moments so that the baseline wage and uptake and the changes match values observed in the experiment.

The model is solved algorithmically by guessing wages and then iterating until wages reach a steady state. First, each farmer's drum seeder adoption decision is calculated based on a wage guess. Second, based on drum seeder adoption, calculate the total labor demand. Third, update the wages based on the calculated labor demand. Finally, iterate until a steady state is reached.

To simulate the experiment, solve the model for a higher drum seeder adoption cost α_0. Then, solve the model again with a lower α_1 using the equilibrium wages in the first step as the initial guess.

Policy Simulation Results

I simulate the same reduction in adoption fixed costs under four different labor market conditions in Table 22. I chose values to ensure the simulations were centered around similar baseline wages and uptake levels so the changes can be directly compared. The first simulation is an approximation of the experiment. The goal is to match the wage and uptake effects. The second simulation illustrates how increasing the reservation wage increases adoption and wages but simply shifts the wage effects to a similar decrease between higher wage levels. This simulation approximates a policy where the government strengthened workfare alongside reducing barriers to adoption.

The next two simulations emphasize the importance of the slope of the labor supply curve. This is analogous to expanding the effective labor market for women by reducing search, transport, and cultural frictions. In this world, labor supplied to transplanting would reduce much more rapidly in response to small changes in wages. When the labor supply curve is flat, the same decrease in adoption costs leads to greater increases in adoption and no wage effects. If the labor supply curve is even slightly steeper than the baseline scenario, the same decrease in adoption costs leads to a smaller increase in adoption and more severe wage effects.

The simulations illustrate the importance of shifting labor supply curves alongside mechanization policy and targeting labor markets with low labor supply elasticities. They also illustrate the feedback loop between mechanization adoption and wages. The simulations show that across the same adoption cost decreases, different labor market responses to mechanization can lead to very different equilibrium wages and adoption.

10 Conclusion

By leveraging the agricultural extension system of a major rice-growing state in India, the study tests intervention that reduces barriers to mechanization. The study leverages local governments to improve farmers' access to drum seeders, which allows farmers to avoid the laborious and costly process of transplanting. While only a modest number of farmers adopt the technology in the initial seasons covered by my study, these farmers save on transplanting costs and earn higher profits. Crucially, even farmers who do not drum seed earn higher profits in treatment villages.

The reason non-adopters also profited is that wages went down in treatment villages. The wage effect suggests a relatively steep labor supply curve for female transplanting labor. This steepness is likely driven by the high wages farmers pay during congested periods. There is no evidence of women transitioning into other industries in the short run, but there is some evidence of women attempting to access, without success, more work from India's workfare scheme. This shows how the labor market in the short-run was not able to absorb the labor displaced by mechanization. The resulting wage decreases make further mechanization less profitable relative to the now cheaper manual transplanting.

The period of this paper is too short to know whether the displaced women will eventually find potentially higher-paying jobs in other sectors, but clearly structural transformation would happen faster if transplanting wages didn't fall. I hope to study potential interventions to facilitate women finding new jobs in neighboring villages in future research.

Understanding the overall welfare effects of this shift is difficult, however. Many transplanting women are in households that benefit from greater farming profits and transplanting itself is a deeply unpleasant task. While increasing female labor force participation is an important policy goal, the types of jobs policymakers envision are not rice transplanting. A more relevant policy question raised by the experiment is how can women stay in the labor force as they transition out of agriculture. Evidence from my experiment suggests husbands' opinions on where women can work shift along with mechanization and that women do look for new opportunities at the time of mechanization. Thus pairing programs which target female labor force participation with programs targeting mechanization could be a promising solution. In the shorter run, governments can also ensure their workfare schemes continue to operate during peak agriculture seasons where they were historically less relevant.

In general, the study highlights how labor market frictions can sabotage productivity growth. When labor shortages result in steep labor supply curves, the wage effects of mechanization will be more pronounced. These wage effects hurt both equity and efficiency, reducing worker incomes and reducing the relative profitability of mechanization.

For developing countries trying to spur structural transformation, combining efforts to reduce labor market frictions and barriers to technology adoption might be a promising path forward. If displaced workers cannot find new jobs, the mechanization process will simply resolve the labor shortage, which will make mechanization profitable in the first place. Sustained productivity growth through mechanization requires the labor market to absorb the displaced labor. This study highlights how mechanizing one of the least productive tasks in Indian agriculture, rice transplanting, is slowed by mechanization's own effect on wages.

Appendix

A Additional Sample

In addition to the primary sample, there are two supplemental samples. The first supplemental sample is 145 farmers listed on drum seeder rental records as a method for validating that administrative data. However, the comparison between implementation arms is not the focus of this paper.

The second supplemental sample is 186 farmers who were individually randomized to receive further cash incentives to encourage drum seeder adoption. Unfortunately, due to issues processing the payments this experiment did not have a large enough first stage to yield meaningful analysis.

However, both of these samples have more drum-seeding farmers than the main representative sample, so they are used in some auxiliary regressions exploring determinants of drum-seeding yield and profits.

B Pooled Wage Specification

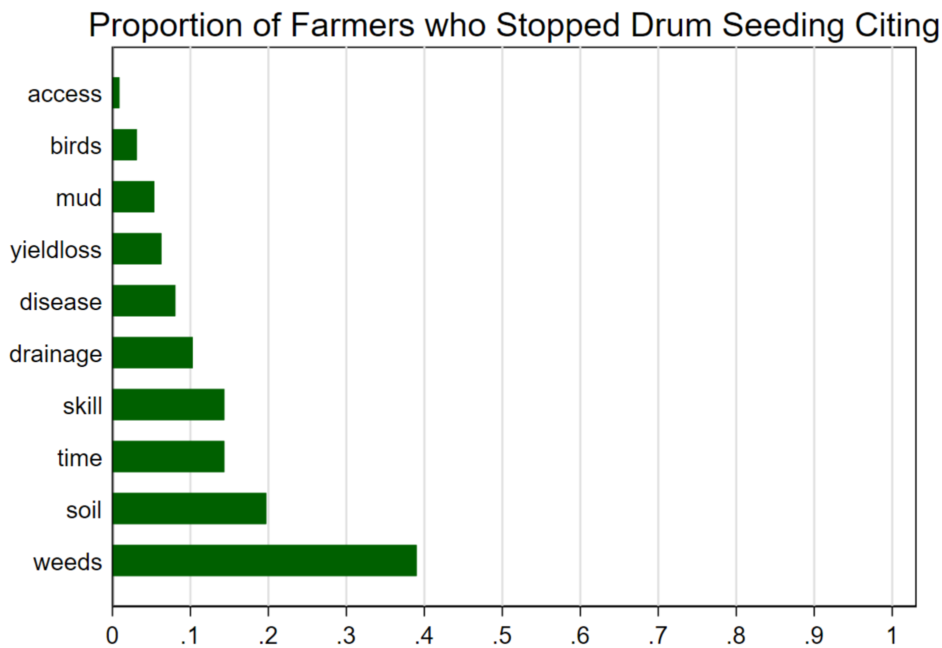
To pool wages across reporting types I use the following specification:

$$\ln(Wage_{ivse}) = \beta_1 T_g + \beta_2 D_i + \beta_3 L_i + \chi_e + \gamma_s + \varepsilon_v$$

$Wage_{ivse}$ wage as either cost per acre or cost per worker per day to transplant a field standardized by their standard deviation to ensure both variables have a similar scale. T_v treatment dummy for village v. D_i indicator for how wage is reported for individual i. L_i acres of land owned by individual i. This is the best predictor of wage type with larger farmers paying by acre. χ_e enumerator fixed effects. γ_s strata fixed effects. ε_{vi} are errors clustered by village. In this specification, β_1 is the percent change in wage induced by the treatment.

11 Figures

Figure 1: Reasons Farmers Stop Drum Seeding



Farmers who reported having drum seeded in the past, but did not drum seed the season they were surveyed.

Figure 2: Distribution of Uptake Across Villages

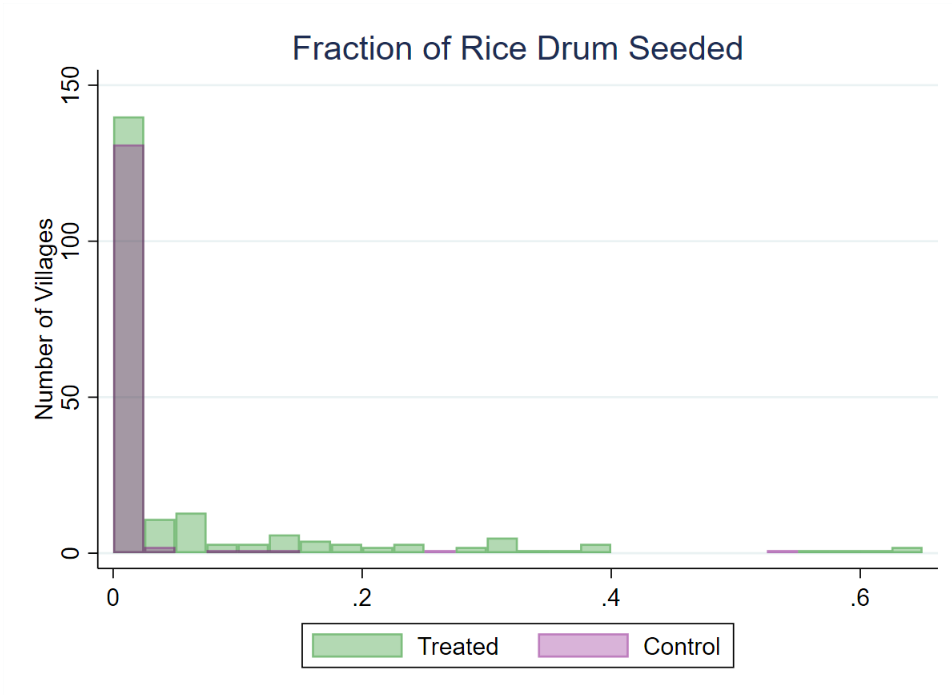


Figure 3: Main Samples

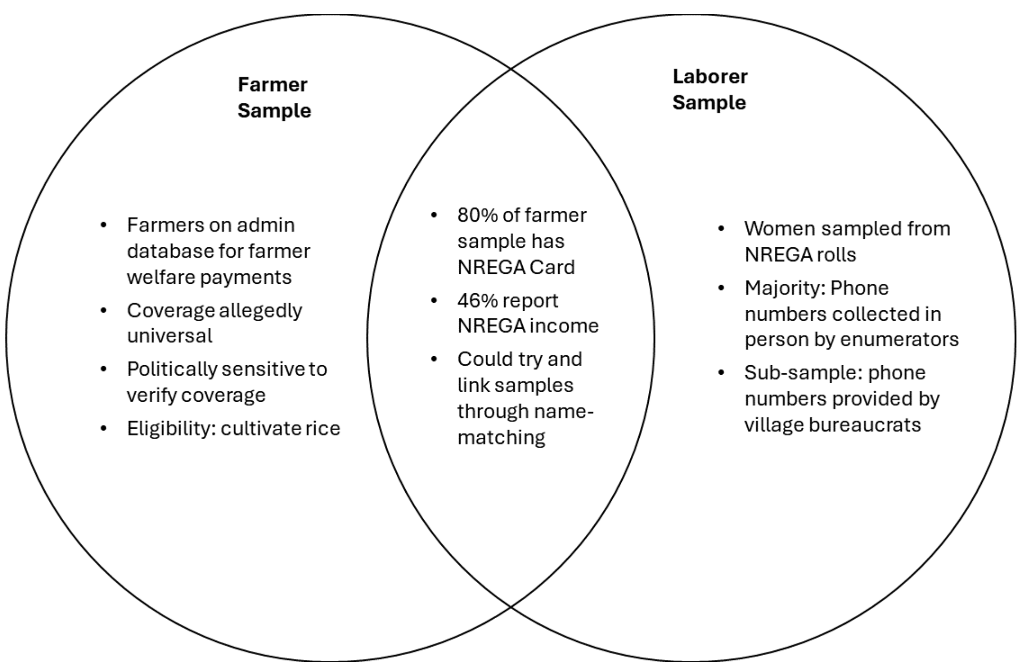


Figure 4: Timeline of data collection

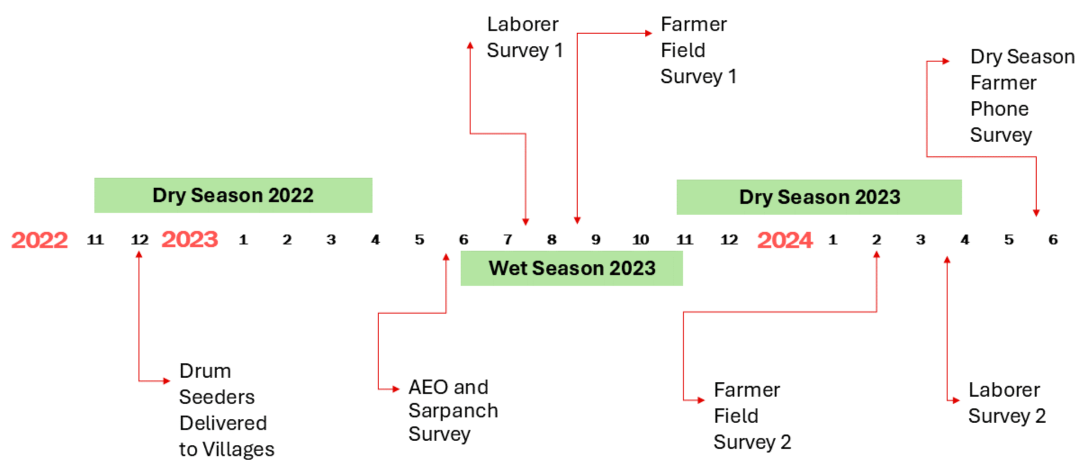


Figure 5: Comparing Wage Distribution of Different Tasks

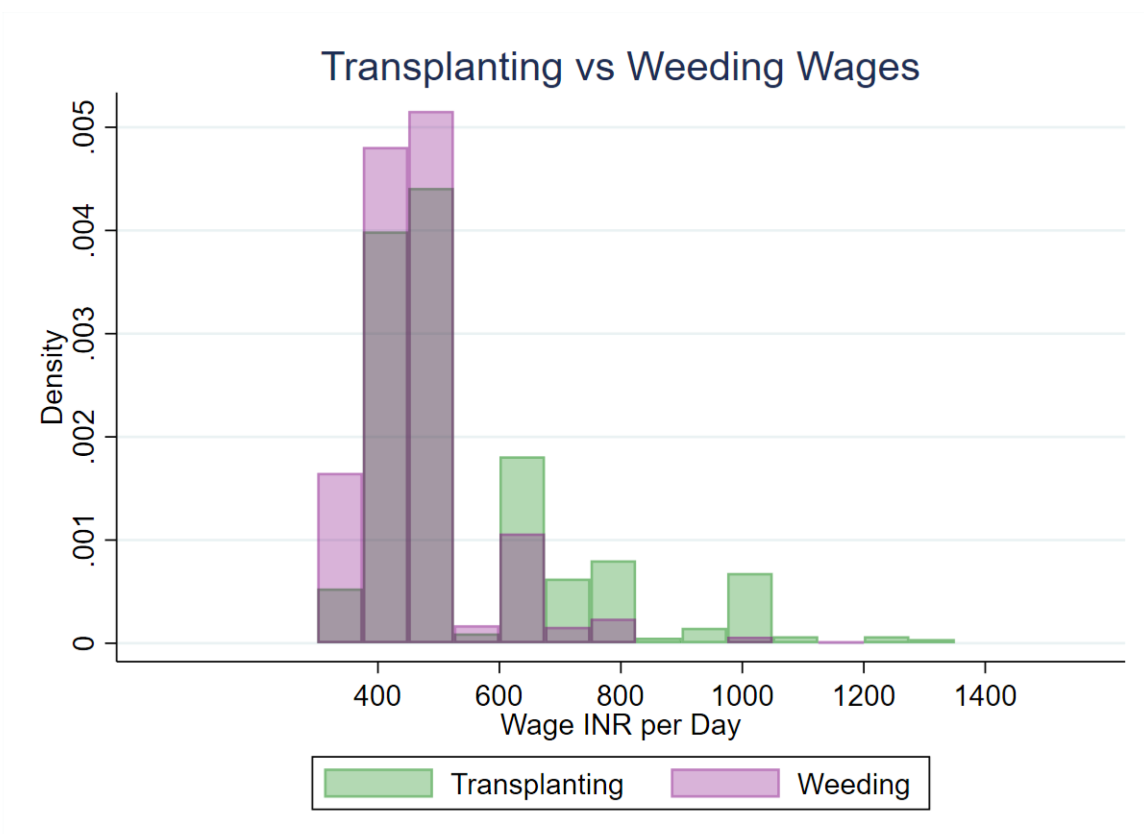
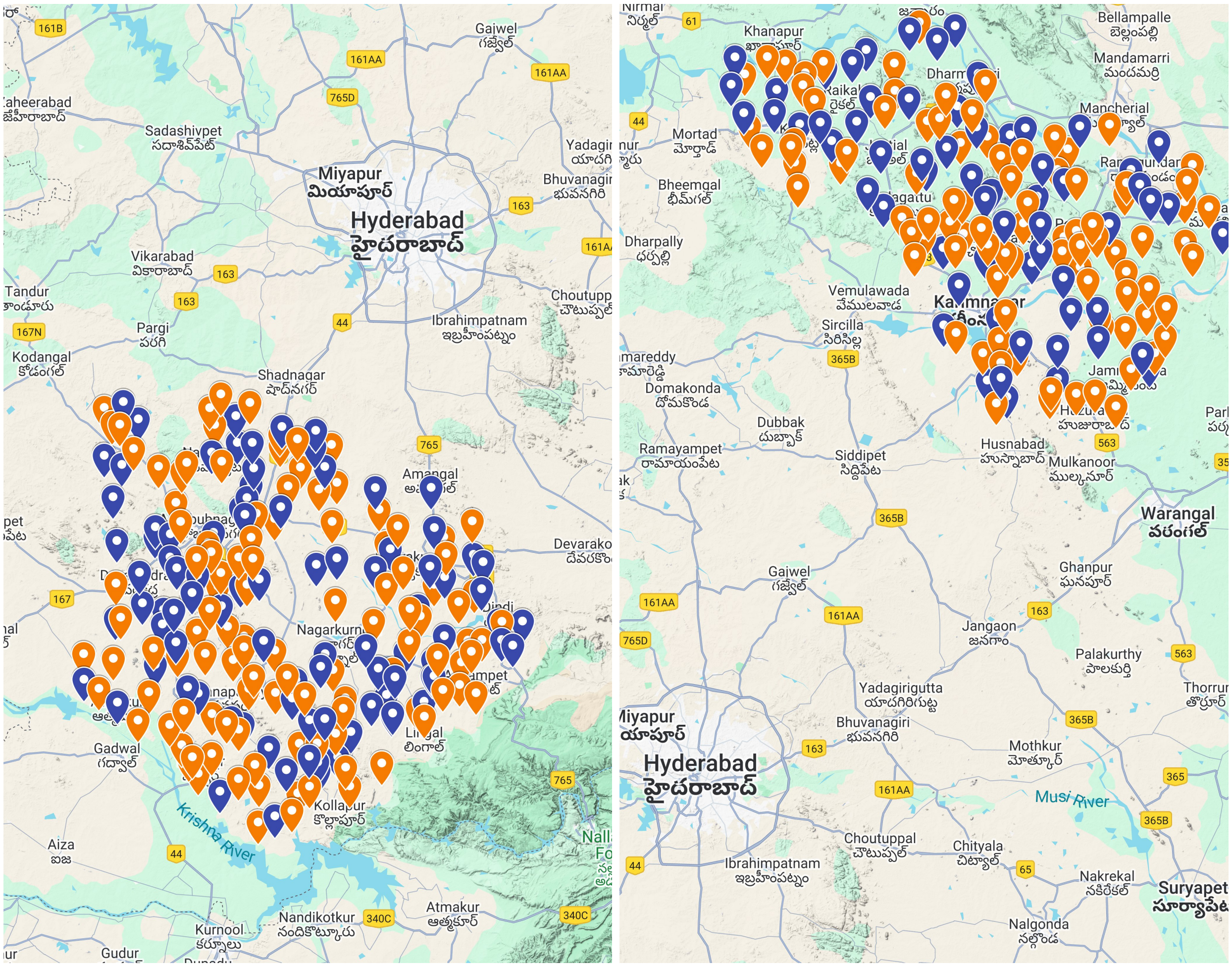


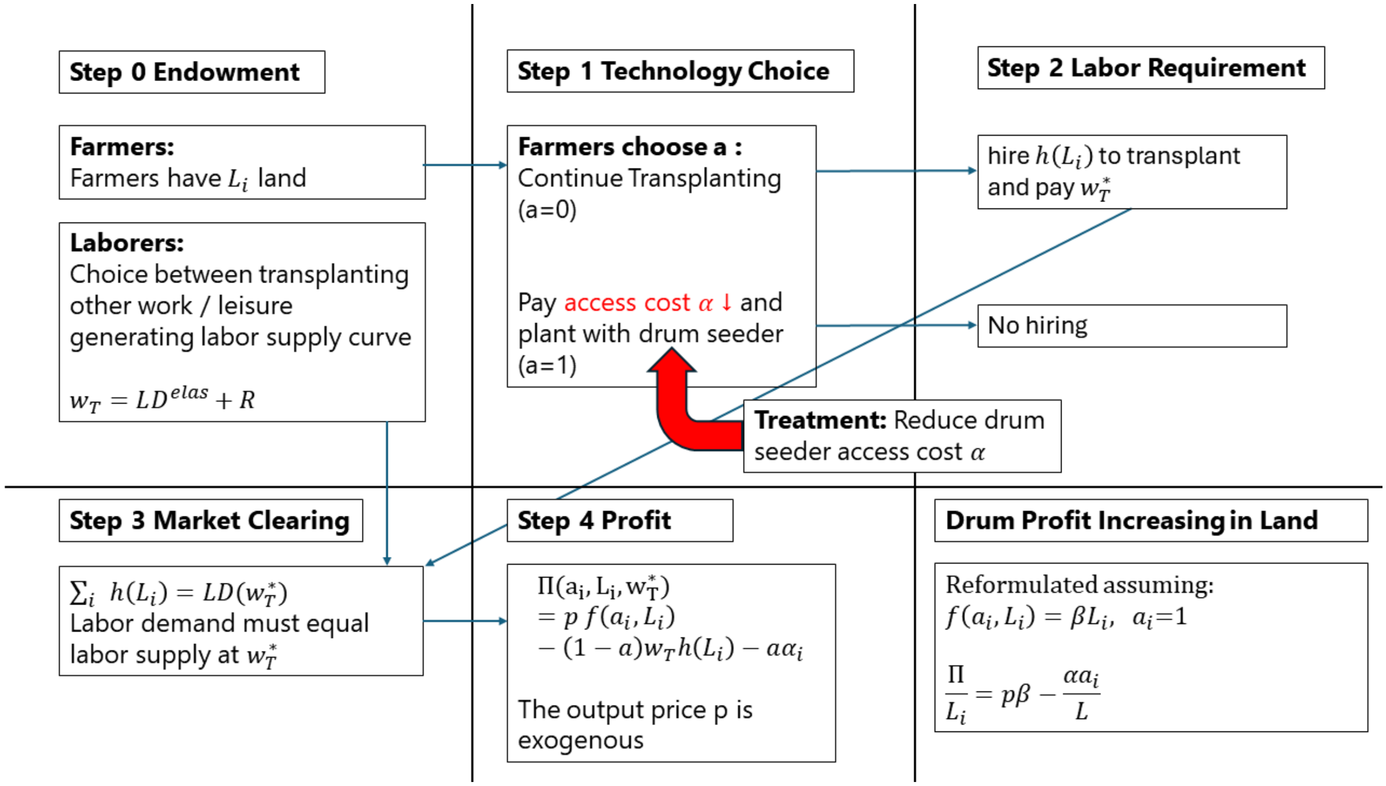
Figure 6: Rice Transplanting as a Luxury Activity

Figure 7: Project Villages



Orange pins represent treatment and blue represent control

Figure 8: Model Schematic



12 Tables

Table 1: Balance Table

	(1)		(2)			(2)-(1)
	0		1			Pairwise t-test
Variable	N/Clusters	Mean/(SE)	N/Clusters	Mean/(SE)	N/Clusters	Mean difference
Total Admin 2022 Cult	796	1.751	1065	1.819	1861	0.068
	134	(0.065)	200	(0.060)	334	
Total Land Owned	877	4.977	1147	4.698	2024	-0.278
	138	(0.221)	204	(0.168)	342	
Household Size	850	3.922	1108	3.961	1958	0.039
	135	(0.072)	200	(0.063)	335	
Well Irrigation	877	0.800	1147	0.776	2024	-0.025
	138	(0.024)	204	(0.023)	342	
HH Head Secondary and Above	850	0.376	1108	0.366	1958	-0.011
	135	(0.018)	200	(0.016)	335	
F-test of joint significance (F-stat)						0.460
F-test, number of observations						1813
F-test, number of clusters						327

Fixed effect used in pairwise and f-test regressions: [strata_id]. Significance: ***=.01, **=.05, *=.1. Errors are clustered at variable: [panchayatid]. Note that with the exception of the administrative data on 2022 cultivation all variables come from the initial survey which took place after the start of some implementation

Table 2: Sample NFHS/DHS Comparison

	(1) Farmer Sample		(2) DHS Sample in Study Districts		(2)-(1) Pairwise t-test	
Variable	N/Clusters	Mean/(SE)	N/Clusters	Mean/(SE)	N/Clusters	Mean difference
Acres Owned	2024	4.819	2219	4.733	4243	-0.086
	342	(0.135)	249	(0.441)	591	
HH Head Secondary and Above	1958	0.370	4114	0.431	6072	0.060***
	335	(0.012)	252	(0.014)	587	
Irrigation Pump	1958	0.477	4115	0.466	6073	-0.011
	335	(0.018)	252	(0.017)	587	
Pressure Cooker	1958	0.511	4115	0.561	6073	0.050**
	335	(0.016)	252	(0.013)	587	
Television	1958	0.877	4115	0.882	6073	0.005
	335	(0.008)	252	(0.006)	587	
Has Mobile	2024	0.954	4115	0.968	6139	0.014
	342	(0.012)	252	(0.004)	594	
Has Cow or Buffalo	2024	0.398	4115	0.212	6139	-0.186***
	342	(0.016)	252	(0.011)	594	
F-test of joint significance (F-stat)						3.199***
F-test, number of observations						4176
F-test, number of clusters						584

. Significance: ***=.01, **=.05, *=.1. Errors are clustered at variable: [panchayatid].

Table 3: Sample NFHS/DHS Comparison Only Farmers

	(1) Farmer Sample		(2) DHS Ag Land Owners in Study Districts		(2)-(1) Pairwise t-test	
Variable	N/Clusters	Mean/(SE)	N/Clusters	Mean/(SE)	N/Clusters	Mean difference
Acres Owned	2024	4.819	2219	4.733	4243	-0.086
	342	(0.135)	249	(0.441)	591	
HH Head Secondary and Above	1958	0.370	2218	0.349	4176	-0.022
	335	(0.012)	249	(0.013)	584	
Irrigation Pump	1958	0.477	2219	0.458	4177	-0.019
	335	(0.018)	249	(0.020)	584	
Pressure Cooker	1958	0.511	2219	0.531	4177	0.021
	335	(0.016)	249	(0.014)	584	
Television	1958	0.877	2219	0.890	4177	0.013
	335	(0.008)	249	(0.008)	584	
Has Mobile	2024	0.954	2219	0.972	4243	0.018
	342	(0.012)	249	(0.005)	591	
Has Cow or Buffalo	2024	0.398	2219	0.347	4243	-0.050**
	342	(0.016)	249	(0.014)	591	
F-test of joint significance (F-stat)						3.199***
F-test, number of observations						4176
F-test, number of clusters						584

Significance: ***=.01, **=.05, *=.1. Errors are clustered at variable: [panchayatid].

Table 4: Sample NFHS/DHS Comparison Only Rural

	(1) Farmer Sample		(2) DHS Rural Sample in Study Districts		(2)-(1) Pairwise t-test	
Variable	N/Clusters	Mean/(SE)	N/Clusters	Mean/(SE)	N/Clusters	Mean difference
Acres Owned	2024	4.819	1971	4.443	3995	-0.376
	342	(0.135)	194	(0.395)	536	
HH Head Secondary and Above	1958	0.370	1970	0.314	3928	-0.056***
	335	(0.012)	194	(0.012)	529	
Irrigation Pump	1958	0.477	1971	0.449	3929	-0.029
	335	(0.018)	194	(0.022)	529	
Pressure Cooker	1958	0.511	1971	0.510	3929	-0.001
	335	(0.016)	194	(0.014)	529	
Television	1958	0.877	1971	0.884	3929	0.006
	335	(0.008)	194	(0.008)	529	
Has Mobile	2024	0.954	1971	0.970	3995	0.016
	342	(0.012)	194	(0.006)	536	
Has Cow or Buffalo	2024	0.398	1971	0.377	3995	-0.021
	342	(0.016)	194	(0.014)	536	
F-test of joint significance (F-stat)						3.385***
F-test, number of observations						3928
F-test, number of clusters						529

Significance: ***=.01, **=.05, *=.1. Errors are clustered at variable: [panchayatid].

Table 5: Treatment Effect on Awareness

	Ag Officer Info	Attend Demo	Heard Promotion
Extensionist Arm	0.06**	0.10***	0.16***
	(0.03)	(0.03)	(0.03)
Leader Arm	0.05	0.07**	0.17***
	(0.03)	(0.03)	(0.03)
Control Mean	0.43	0.49	0.32
R-squared	0.27	0.12	0.13
Observations	2205	1946	1893
Difference p-value	0.622	0.402	0.801

*** p<.01, ** p<.05, * p<.1 Standard errors clustered at the village level. Includes fixed effects for randomization strata.

Table 6: Treatment Effect on Uptake

	(1) Drum Rainy	(2) Drum Dry	(3) Acres Drum Rainy	(4) Acres Dry
Leader Arm	0.03***	0.02**	0.10**	0.05
	(0.01)	(0.01)	(0.04)	(0.04)
Extensionist Arm	0.04***	0.03***	0.13***	0.07*
	(0.01)	(0.01)	(0.04)	(0.04)
Observations	2,023	2,023	2,023	2,023
Ctrl Average	0.01	0.01	0.04	0.05
Controls	N	N	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Treatment Effect on Uptake

	(1) Drum Rainy	(2) Drum Dry	(3) Acres Drum Rainy	(4) Acres Dry
Treatment	0.04^{***}	0.03^{***}	0.11^{***}	0.06^{**}
	(0.01)	(0.01)	(0.03)	(0.03)
Observations	2,023	2,023	2,023	2,023
Ctrl Average	0.01	0.01	0.04	0.05
Controls	N	N	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Similar involvement of elected leaders across treatment arms

	Leader Gives Ag Advice	Leader Refers to Extensionist	Drum Seeder in Village Meeting
Extensionist Arm	0.08***	0.14***	0.11***
	(0.03)	(0.03)	(0.04)
Leader Arm	0.10***	0.11***	0.17***
	(0.03)	(0.03)	(0.04)
Control Mean	0.38	0.48	0.59
R-squared	0.11	0.12	0.22
Observations	1882	1943	787
Difference p-value	0.640	0.413	0.186

Standard errors in parentheses
Standard errors clustered at the community level. Strata FE
Don't know coded as missing
Meeting variable conditional on having had a meeting
Leader refers to expansionist means farmers reported village leaders telling them to reach out to extension agents
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Who Adopts Drum Seeders in Wet Season

	(1) Acres Drum Wet	(2) Acres Drum Wet
Total Cultivation	0.05***	0.06***
	(0.01)	(0.01)
Saline Soil	-0.16	-0.24**
	(0.11)	(0.10)
HH Head Secondary	0.14**	0.14**
	(0.06)	(0.06)
HH Head Higher Secondary	0.14*	0.17**
	(0.07)	(0.07)
Well Irrigation	0.13**	0.05
	(0.06)	(0.06)
Observations	1,926	1,926
Ctrl Average	0.04	0.04
Fixed Effects	Village	Strata

Standard errors in parentheses

Standard errors clustered at the community level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: What Predicts Yield for Drum Seeding Farmers

	Drum Seeder Yield
Knows to Delay Irrigation Post Planting	3.76
	(2.56)
Knows to Reflood field to control weeds	3.56**
	(1.66)
Clay Soil	-5.50**
	(2.44)
Saline Soil	-1.45
	(4.20)
total land	0.17
	(0.26)
HH Head Secondary	2.75
	(2.24)
HH Head Higher Secondary	0.82
	(2.66)
Well Irrigation	-3.75
	(2.53)
Observations	125
Ctrl Average	19.47

Standard errors in parentheses

Standard errors clustered at the community level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Workers Hired per Acre

	(1)
	Total Hired per Area
Treatment	-0.72**
	(0.32)
Observations	2,020
Ctrl Average	9.63
Controls	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Treatment Effect on Costs per Acre

	(1) ln labor exp	(2) total family days	(3) ln total non-labor exp.
Treatment	-0.10***	-0.02	0.03
	(0.03)	(0.07)	(0.03)
Observations	1,967	2,023	1,789
Ctrl Average	8.9	1.5	9.4
Controls	N	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table reports treatment effector on log total labor expenditure, total family work days, log total nonlabor expenditure, and log total expenditure. The different numbers of observations in these regressions reflect both 0s as the variables are in logs and the exclusion of farmers who felt they could not accurately report expenditures despite surveyors' efforts to break up expenditures into sub-categories.

Table 13: Treatment Effect on Ln Revenue Per Acre

	(1) Paddy Rev.	(2) Total Rev.	(3) Impute Paddy Rev.	(4) Impute Total Rev.
Treatment	0.02	0.01	0.03*	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	1,697	1,715	1,974	1,923
Ctrl Average	10.7	10.6	10.6	10.5
Controls	N	N	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

Actual paddy revenue is reported sales while imputed paddy revenue is production times district median price

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Treatment Effect on Paddy Yields and Acres

	(1) Paddy Yield	(2) Acres Paddy	(3) Acres Not Paddy	(4) Any Not Paddy
Treatment	0.59	0.12	-0.07	-0.03*
	(0.38)	(0.14)	(0.06)	(0.01)
Observations	2,023	2,023	2,023	2,023
Ctrl Average	18.3	3.3	0.6	0.2
Controls	N	N	N	N

Standard errors in parentheses

Standard errors clustered at the community level.

Enumerator and Strata FE. Yields and acres are for all cultivation types

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Treatment Effect on Profit

	(1) Paddy	(2) Total	(3) Winsor Paddy	(4) Winsor Total
Treatment	1,663.33**	1,242.45*	1,451.65**	1,086.72*
	(757.78)	(727.80)	(671.29)	(640.57)
Observations	1,844	1,895	1,844	1,895
Ctrl Average	17,720.9	20,252.0	17,701.4	20,152.7
Controls	N	N	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

Winsorization Done at 5th and 95th Percentiles.

Total is across all crops and paddy is just rice

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Main Treatment Effect on Transplanting Wages

	(1) Wage per Day	(2) Wage per Acre
Treatment	-17.05**	-335.84**
	(8.54)	(137.32)
Observations	895	418
Ctrl Average	557.43	5,776.75
Controls	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Treatment Pooled Wages

	(1) Reported Daily	(2) Log Wage
Treatment	0.03	-0.03**
	(0.02)	(0.01)
Reported Wage Day		-2.18***
		(0.03)
Observations	1,332	1,332
Ctrl Average	0.64	7.12

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Labor Costs for Transplanters

	(1) Ln Labor Cost Trans Only	(2) Ln Days Hired Trans Only
Treatment	-0.07**	-0.03
	(0.03)	(0.04)
Observations	1,919	1,841
Ctrl Average	8.94	1.92

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Treatment Effect on Husband Allows to Work Outside

	(1) Trans at Baseline	(2) Work Outside OK
Treatment	0.00	-0.10***
	(0.03)	(0.03)
Transplant at Baseline		0.06*
		(0.04)
Interaction		0.09*
		(0.05)
Observations	1,338	1,338
Ctrl Average	0.3	0.5
High Pred. Uptake Strata	N	N

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 20: Treatment Effect on Husband Attitudes in Higher Predicted Uptake Strata

	(1) Trans at Baseline	(2) Work Outside OK
Treatment	-0.01	-0.14***
	(0.03)	(0.04)
Transplant at Baseline		0.03
		(0.05)
Interaction		0.19***
		(0.07)
Observations	879	879
Ctrl Average	0.3	0.5
High Pred. Uptake Strata	Y	Y

Standard errors in parentheses

Standard errors clustered at the community level. Enumerator and Strata FE.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 21: Transplanters in Treatment Villages Have Greater Unmet Demand for NREGA

	(1) Transplanted at Baseline	(2) Days Demand Minus Offered
Treatment	-0.01	-1.12
	(0.02)	(0.85)
Transplanter X Treatment		1.93**
		(0.91)
Observations	1,230	553
Ctrl Average		4.6

Standard errors in parentheses

Data from phone survey of workers on NREGA rolls

Standard errors clustered at the community level. Enumerator and Strata FE.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 22: Model Simulations

	Baseline	Higher Reservation	Low Elas	High Elas
Fixed Cost Initial α	41.00	41.00	41.00	41.00
Fixed Cost Final α	36.00	36.00	36.00	36.00
Elasticity η	0.71	0.71	0.14	0.79
Initial Reservation Wage	230	230	520	27
Final Reservation Wage	230	260	520	27
Initial Adoption	3.85%	3.85%	2.43%	6.13%
Final Adoption	11.55%	14.98%	15.98%	11.55%
Initial Wage	530	530	523	541
Final Wage	500	517	523	500

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