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 presents results from a randomized experiment that studies the impact of reducing farmer barriers to mechanization at the village level. By combining government farmer training infrastructure with rentals, the intervention tripled the uptake of a technology that replaced manual rice transplanting. For farmers, labor costs decreased with no change in yields or other expenses, leading to a 6% 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 grown rich without also experiencing an increase in agricultural labor productivity. Developing countries' agriculture sectors are laggards in the quest for growth. The productivity gap between the wealthiest 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 agricultural 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 solution is rental markets, but these are often absent for new technologies.

While boosting productivity, mechanization also often has distributional consequences. Acemoglu et al. (2024) highlights how mechanization, defined as replacing labor with capital for a specific task, has a theoretically ambiguous effect on wages. 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 create a tension between improving productivity and protecting workers. The impact on workers feeds back into the equilibrium effect on productivity. Wages for the mechanized task determine the relative profitability of mechanization for later adopters (Hornbeck and Naidu, 2014; Manuelli and Seshadri, 2014). The wage for the mechanized task also partially reflects displaced workers' productivity in their new jobs, which determines how mechanization translates to changes in aggregate productivity driven by the new output displaced workers generate in their new jobs.

This paper studies mechanization and its consequences in the context of rice farming in the Indian state of Telangana. Drum seeders are low-cost devices that increase the labor productivity of rice planting by eliminating transplanting, which comprises a fifth of the cost of rice cultivation. However, adoption requires access to both the devices and the knowledge to use the devices successfully. Although private rental markets exist for well-known technologies like tractors, they are largely absent for drum seeders. The need for skilled staff to train large groups of farmers clashes with the individualized nature of equipment rentals, making training a low-profit endeavor for rental entrepreneurs.[1] Governments provide agricultural training through a system of agricultural trainers and extensionists who offer skills but not capital. I worked with the Government of Telangana to help design a program to coordinate extension with village government drum seeder rentals facilitating drum seeder adoption. Beyond the interest in improving aggregate productivity, governments and carbon credit providers have additional interest in drum seeders due to the technology's potential to reduce water use and methane emissions from rice cultivation.[2]

Working with the government enabled deploying this program at scale across 205 randomly selected villages with over 70,000 farmers. Rice transplanting in Telangana is a setting where comparing villages closely approximates comparing separate rice transplanting labor markets. 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. 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 and phone surveys of 1,400 female workers. The rice farmer sample includes all types of rice farmers and reflects the demographics of the broader rural population in the study districts. The in-person survey encompasses data on all farming costs and revenues, with meticulous collection of task-specific wages. The survey also included detailed data on farmer training and extension. The worker phone survey included questions on work locations and government job scheme utilization during the rice transplanting period.

[1] A related issue is that farmers might not trust entrepreneurs who are trying to profit from a new device to impartially explain its costs and benefits

[2] The Good Rice Alliance carbon project has enrolled enough farmers in India to target 100,000 tonnes equivalent in methane emission reductions from direct seeded rice and alternate wetting drying, which drum seeders facilitate (NDTV, 2024). Rice is responsible for 6-11% of global methane emissions (Smith et al., 2021). Crop burning is another potential environmental externality. However, in this study only 28 farmers self-reported rice stubble burning one of which drum seeded.

This study presents three main sets of empirical results: the impact on drum seeder adoption, the impact on farm productivity and profits, and finally the labor market consequences.

First, the program led to a nearly 300% increase in drum-seeded acreage in the primary rice season, an increase from 14 to 50 acres per village or 20% average utilization of the village's drum seeders.[3] Mirroring a natural uptake process, more educated farmers with more land were more likely to adopt drum seeding. On the extensive margin, the fraction of farmers using drum seeders at all increased from 1% to 4% (about 12 additional farmers per village). Adoption was highly uneven across villages, with many villages with no adoption and others with up to 60%. 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 employed 7% less hired female 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 labor 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. This large increase in profits relative to takeup is an intent-to-treat effect that includes impacts on both drum seeding and transplanting farmers in treatment villages.

Overall, farmers in treatment villages spent 10% less on hired labor. Even farmers who did not adopt drum seeders spent 7% less on hired labor.[4] These savings came from transplanting farmers paying 6% lower piece-rate wages 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 likely partially reflect women having few other job options

[3] 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)

[4] While this regression is not causal since who transplants in the treatment group is endogenous, the implied treatment on the treated effects, if there was no treatment effect on non-drum seeding farmers, are more than 100%.

outside these transplanting labor markets. There is suggestive evidence from the laborer survey that workers did not find other jobs in the short term. Unlike men, only 2.5% of women worked outside their village during the primary wet season, which is the focus of the results. 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.

To explore the interplay between labor markets and mechanization, I developed a quantitative model examining how reducing barriers to mechanization affects productivity and wages under different labor market conditions. Through the model, I explore how, through wages, mechanization can sow the seeds of its own demise. In the model, the labor supply curve's slope determines how far wages fall in response to the mechanization labor demand shock, which in turn affects how many farmers stand to profit from mechanization. In the model, the equilibrium wage also serves as a proxy for the productivity of displaced workers in new jobs. Since agricultural output in the model is constrained by land, as is true in many developing country settings, productivity growth from mechanization is entirely driven by the output displaced workers achieve in their new sectors. Calibrating the model to the experiment allows me to quantify the role of wages in either amplifying or mitigating the effects of reduced mechanization fixed costs on equilibrium mechanization and productivity.

I explore how the experiments' impact on adoption and wages would change if the reduction in fixed adoption costs remained the same, but the labor market changed. The first scenario simulates offering government jobs alongside mechanization subsidies. Absorbing half the jobs lost due to mechanization in a government jobs scheme does increase mechanization by 30% and reduce the wage decrease from 6.3% to 5.5%. However, this comes at a cost to productivity since the government jobs scheme is assumed to produce no output. The second scenario simulates improving the set of jobs women can access, thus reducing wage sensitivity to labor demand shocks.[5] Making the labor supply 20% less elastic nearly triples mechanization in equilibrium from 3.42% to 9.13%

[5]This would correspond with government policy interventions to increase labor market opportunities for women, such as free access to inter-village buses.

and more than triples the aggregate productivity increases associated with reduced fixed cost of adoption from 5% to 19% since more workers move out of the agricultural sector and are more productive in their new jobs. The model illustrates the quantitative importance of labor market structure in shaping the impact of agricultural mechanization interventions in settings similar to the experiment.

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, such as One Acre Fund, combine training and input access (Deutschmann et al., 2019), but they often need donor funding to scale. The rental aspect of this study is self-financing. 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 government intervention contributed to kick-starting rentals.[6] Integrating rental markets offers a promising avenue for governments to fully capitalize on existing agricultural training investments.[7]

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. The study bridges the national and household level literature to identify the impacts of mechanization by identifying spillovers from mechanization at the labor market level. Other evidence on

[6] 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.

[7] 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).

technology adoption and labor markets demonstrates how labor shortages constrain the adoption of labor-complementing technologies like irrigation (Jones et al., 2022) and how higher rural wages can spur mechanization, in the long run (Hornbeck and Naidu, 2014; Manuelli and Seshadri, 2014). Given this evidence, the lower wages caused by mechanization in this paper will likely slow mechanization until workers adjust or until the next labor-complementing shock, such as an irrigation expansion, occurs.

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. (2023a) highlight how rural wages increase with more credible outside options. This study shows that when mechanization removes job options wages decrease.

Fourth, this study contributes to an emerging literature on low female labor force participation, particularly in India. This study provides micro-level experimental evidence consistent with a macro trend of declining female labor participation. The study reinforces the finding that districts in India more agronomically suited to mechanization have lower female labor force participation but no reductions in male labor (Afridi et al., 2023). The study also provides another example of wages for Indian women decreasing in response to rural labor demand shocks (Afridi et al., 2022).

Finally, this study provides a clear illustration of the broader tension countries face between increasing productivity and protecting workers. This paper provides an empirical example of a mechanization event where an entire task is replaced, which is the focus of recent theoretical and empirical work highlighting the distributional consequences of mechanization (Acemoglu et al., 2024). These distributional consequences sometimes can create political push-back.[8]. The quantitative model suggests that social protection and labor mobility policies can address political concerns by making mechanization more equitable and, in equilibrium, more effective as well.

[8]From British Luddites to Gandhi, opposition to mechanization on behalf of workers has been a prominent political position. In India, politicians often conflate policies supporting inefficient firms with protecting workers (Muralidharan, 2024)

2 Background

Manual transplanting (Figure 7) remains the dominant method for growing rice in Telangana for two reasons. First, high transplanting wages and even the existence of wages are recent phenomena. The transplanting labor market is now tight. Second, switching rice cultivation to drum seeding is deceptively complex and requires substantial training.

This paper is not about the adoption of a newly invented complex 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 before the study created one from scratch inspired by a YouTube video. The reason farmers resorted to making drum seeders themselves is that they are only available for purchase in district headquarters and not available for rent in study districts' agricultural rental businesses. These rental businesses, custom hiring centers, provide easy access to tractor and other device rentals. However, they do not provide any training.

Two big shifts in transplanting labor markets likely explain farmers' newfound interest in drum seeders. First, farmers shifted from exchanging transplanting labor to working in their neighbor's fields for wages. 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. The irrigated area has increased by 117% and rice production by 342% in Telangana from 2015 to 2022 (Planning Department, 2023).

Wages also likely reflect a compensating differential for the unpleasantness of crouching in mud for hours at a time. Willingness to pay studies find Indian wives have a higher willingness to pay than their husbands for drum seeders (Khan et al., 2016) and mechanical transplanting machines (Gulati et al., 2024). There are also health risks associated with spending extended periods in flooded rice fields, including greater exposure to water-born diseases, mosquitoes, and snakes (Vent et al., 2016). Thus, both men and women in farming households have substantial incentives to adopt drum seeders.

As has been found for other seemingly simple agricultural technologies (Aker and Jack, 2023), training is a major barrier to drum seeder adoption. Drum seeding simply involves rolling a drum seeder along a leveled and damp rice paddy to place the rice seeds in rows along the field. However, farmers must make other adjustments to ensure they drum seed successfully. Importantly, these adjustments take place at other points during the season before and after farmers use drum seeders. This makes a service provider model where service providers drum seed on behalf of farmers unable to fully address the training barrier to drum seeder adoption.

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 that stop the rice from germinating.[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: re-flooding after planting, 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 11). Thus, access to training across the season and not just when farmers use the device is important.

Given the knowledge required to successfully drum seed, farmers clearly require both extension and access to make the best use of drum seeders. This was especially true in villages without any

[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

historical drum seeder use.

3 Intervention

The intervention's origin stems from the government of Telangana's interest in consolidating its investment in decentralization by strengthening village governments with its investment in agricultural training by hiring one college-educated extension officer for every 5,000 acres. Locally elected governments can mobilize citizens and wield fiscal autonomy but are short-staffed with one skilled bureaucrat saddled with a dizzying array of responsibilities (Brownstone et al. Forthcoming). On the other hand, extensionists have extensive agronomic knowledge but struggle to interest farmers in hypothetical technologies requiring upfront investment. I worked with the Telangana Department of Agriculture to develop a pilot program that would address extensionists' and village governments' concerns. Local governments could leverage their fiscal autonomy to rent agricultural technology and leverage their popularity to mobilize farmers to receive training from extensionists on the same technology. Extensionists would be able to reach more farmers and provide training on a technology farmers could easily access.

Of the handful of priority technologies extensionists promoted, drum seeders interested village elected leaders, sarpanches, the most. During pilot field visits, village leaders consistently flagged transplanting labor shortages as farmers' top issue. 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."[10] Extension agents complained about an inability to mobilize farmers to attend meetings and the difficulty of promoting devices they could not directly provide.

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 which was the median of rental prices in other districts with higher adoption. This price also ensured that

[10]Surprisingly, there was no correlation between the fraction of support elected leaders estimated came from laborers and the leaders' expectation of gaining electorally as a result of the program.

after 22 days of rentals, approximately 2 seasons, the device's cost would be covered. Farmers were allowed to share the rental as one machine could cover up to 4 acres daily. 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 cross-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 drum seeder promotion in both arms (Table 5) leading to similar levels of drum seeder adoption (Table 8). 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 strengthen 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 descriptive 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. [11] These are women from households that wanted the oppor-

[11] I scraped job cards from the NREGS website and then asked village-level officials to track the women on the job

tunity to take part in India's primary rural welfare scheme, NREGS. Eighty percent of households in the 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 unavailable. See Figure 3 for a summary of the sampling frames.

While neither sampling frame perfectly represents the population of the study villages, it captures a wide swath of the relevant populations. Thus, the data can illustrate the heterogeneous impacts of easing access to drum seeders, which is the study's primary goal.

Due to the autonomy of the local governments, village elected leaders, sarpanches, had to opt-in to participate in the study. Of those contacted, 33% of villages had leader was not interested in the program. Often, these were villages 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 9) and represent a wide range of previous drum seeder exposure and prevailing transplanting wages. Importantly, any scaled-up version of this program in India would have to operate on a similar opt-in basis. Some degree of self-targeting is likely optimal since local leaders are best informed about their community's agricultural needs.

For survey sampling, the randomization 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. In the lowest predicted uptake strata, only one of the two control villages was sampled. 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

cards. Note that with mandatory linking of biometrics, fake job cards have largely disappeared from Telangana.

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 are not significantly different between the study's 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.

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 social learning will likely not occur until the subsequent dry and wet seasons since farmers consider the wet and dry seasons agronomically distinct. 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.

5 Randomization

This study was randomized at the village level, which is important for capturing labor market effects.

As mentioned above, villages were screened before randomization based on the village elected leader's 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. 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 2.5% of female transplanters who report doing some work outside their village during the transplanting season. Since treatment and control villages were seldom neighbors (Figure 9), 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.

6 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. The limited control variables available only improve precision marginally, [12] so results are reported without control variables for simplicity.

Note that estimating the treatment on the treated regressions likely will not isolate the effects of drum seeding in this study. This is because there are substantial spillovers from farmers drum seeding to those not 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.

7 Results

The project successfully increased farmersáwareness of drum seeders (Table 6). It also improved farmers knowledge of drum seeding practices correlated with yields (Table 7). It also successfully got local elected leaders to engage in promoting drum seeders (Table 5). However, as previously noted, there were no differences between the cross-randomized arms in leadersínvolvement or uptake (Table 8).

The treatment did successfully lead to a large percentage increase in drum seeder cultivation in both the wet and dry seasons (Table 9). However, the actual number of farmers experimenting

[12]The most useful control variable is administrative data on rice cultivation prior to the experiment. There are also variables on historical wages, soil types, and educational attainment, which I assume are time-invariant and unaffected by the treatment.

with drum seeders was relatively small. The treatment effects translate to approximately 10 acres in control villages as compared to 50 in treatment villages. However, 40 acres is still 400 days worth of transplanting work. 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. Farmers wait 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 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. 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.

The acreage converted to drum seeding matters more than the number of farmers adopting the technology for driving labor market effects. Importantly, larger farmers were much more likely to adopt drum seeding (Table 10) than small farmers.[13] 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.[14]

Drum seeder adoption led to the expected decrease in hired female labor days (Table 12). This variable reflects women hired across all tasks, but since harvesting is mechanized, transplanting is

[13] In the administrative data, a handful of very large farmers adopted drum seeders. 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.

[14] 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.

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 considered. 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, seeing 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 hired translated to decreases in labor expenditure in treatment villages (Table 13). 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 at the expense of revenues. However, there is some evidence that farmers in treatment villages were able to obtain higher revenues than those in control villages (Table 14). 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 15). Another possibility is that farmers paid more attention to land leveling because they planned 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 16). The effect is 9% for rice profits and 6% for total profits. 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 17) as well as when they are pooled (Table 18). The smaller sample sizes are because some farmers just reported total labor costs which can encompass multiple tasks. 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 19). 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 between transplanters hired. This wage is 6% lower in treatment villages than control villages.

Wages are responsive because the transplanting labor market appears to be congested. Congestion in the transplanting market is evident when comparing the distribution of manual weeding, an optional time-flexible task done by the same women in the same fields, 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 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. Hydrology limits farmers' ability to stagger planting. [15] The treatment villages have fewer peak wages associated with labor market congestion,

[15] Leveraging remote sensing to better understand the environmental determinants of observed transplanting wages is an area of ongoing analysis.

as seen in this histogram comparing daily wages in treatment and control areas (Figure 6).

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. The exact distribution of benefits depends on the joint distribution of labor supply and paddy cultivation. The population representative national health and family survey has data on both agricultural land ownership and women's primary occupation. Importantly, the survey distinguishes cultivation, which is household agricultural work, and agricultural labor. Comparing the cumulative distribution functions of land ownership for laborers and non-labors shows that more than half of laborers come from land-owning households and a large fraction of laborers come from households with substantial amounts of land (Figure 10). Small landholders are marginally more likely to work as laborers, and households with very large land holdings are unlikely to work as laborers. Taking these insights and the experimental results, I estimate the overall change in household income driven by the experiment factoring in changing agricultural profits, fewer days of work, and lower wages (11). As expected, the impacts are asymmetric, with the bottom 20th percentile of households negatively affected and the largest benefits acquiring the largest landowning households.

There is evidence from the laborer survey that displaced workers were underemployed. 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 4 days of unmet demand in the control villages (Table 20). The workfare program known as the National Rural Employment Guarantee Scheme (NREGS) offers manual labor jobs for a daily wage. The outside option it provides plays an important role in rural labor markets (Muralidharan et al., 2023b). However, while jobs are meant to be offered year-round, the scheme is often unavailable during periods of peak agricultural activity. Although noisier, I find no evidence of more NREGS workfare days offered in treatment villages using the program's administrative data (Table 21).

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 (40% approving in treatment compared to 50% in control), but husbands of women who do transplanting work each season become 9 percentage points relatively less restrictive (Table 22). The pattern grows stronger when the sample is restricted to only strata with high predicted uptake (Table 23). 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 the 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. Note that there is no treatment effect on the extensive margin of whether women did any transplanting work (Table 22).

8 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, technology adoption, and productivity outcomes from a policy shock similar to the experiment. In particular, the experiment is conceptualized as a reduction in the fixed cost of adopting mechanization. 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.

8.1 Model Environment

The core of the model is a technology and labor supply decision made by farmers endowed with land L_i. Farmers 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^*. Workers characterized by a representative household decide how much transplanting labor to supply 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 12.

Farmer Technology Adoption Problem

Each farmer 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. 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. [16]

For the remainder of this section, I assume 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.

Proposition 1: With linear production $f(a_i, L_i) = \beta L_i$ and labor demand $h(L_i) = \gamma L_i$, profits per acre $\frac{\Pi_i}{L_i}$ are increasing in land for mechanized farmers $a_i = 1$ and unchanged for unmechanized farmers $a_i = 0$.

Proof:

Case 1: $a_i = 1$

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

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

Case 2: $a_i = 0$

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

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

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.

Proposition 2 The lower wages decrease the acreage under mechanization.

Proof: The marginal farmer who adopts will be the farmer whose cost of cultivation equals the fixed cost of adoption e.g. $w_T \gamma L_i^* = \alpha$. The mechanized acreage L_m is:

[16] 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

$$\int_{\frac{\alpha}{w_T\gamma}}^{\bar{T}} L(i)di = L_m$$

Since $L(i)$ is always positive L_m is increasing in the interval of integration I

$$I = \bar{T} - \frac{\alpha}{w_T\gamma}$$

$$\frac{\partial I}{\partial w_T} = \bar{T} + \frac{\alpha}{w_T^2\gamma} > 0 \Rightarrow \frac{\partial L_m}{\partial w_T} > 0$$

Household Labor Problem

The labor supply decision is taken by a representative household. 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. The time constraint T is the total number of working days available during the transplanting period.

$$\max_{t_i} U_i(w_t, t_i) = u(w_t t_i) + v(T - t_i) \tag{2}$$

See Appendix C for a derivation of the labor supply elasticity assuming specific functional forms for $u()$ and $v()$ to match the labor supply equation of the form $\log w_t = \frac{1}{\eta}\log t + \log\alpha$ where α is a constant.

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.

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

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 a constant inverse labor supply equation of the form

$$\alpha LS^{\frac{1}{\eta}} = w_t$$

where $\log \alpha$ is a constant corresponding to to the lowest log wage where any labor would be supplied. This can be seen by rearranging the above equation and taking logs

$$\frac{1}{\eta} \log LS = \log w_t - \log \alpha$$

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. To calibrate α, I use the fact that for the marginal farmer $alpha = w_T 10 L_m$ which implies $L_m = \frac{\alpha}{10 w_T}$. If the are N total farmers then adoption is $\frac{(N-m)}{N}$. Conditional on the values of α, I get an estimated labor supply at w_t which I can plug back into the labor supply curve $w_t = LS^{\frac{1}{\eta_T}} + R$ to find R.

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. When calibrated, the model matches the experiment closely (Table 24).

Policy Simulation Results

I simulate the same reduction in adoption fixed costs under two alternate labor market conditions in Table 25. The row agriculture productivity is the total output from agriculture divided by the labor in agriculture. The row aggregate productivity is the total output from agriculture and the output from the displaced labor at the equilibrium wage, assuming the wage is equivalent to the marginal product of labor for workers' outside option, which could include work within the household. The first column in the table simply replicates the experiment in the model. The second simulation provides a benchmark for the counterfactual where labor supply is so elastic there is no wage effect.

The third simulation explores what would happen if the labor supply was such that the wages were less responsive to the labor demand shock. This is analogous to expanding the effective labor market for women by reducing search, transport, and cultural frictions. Alternatively, this simulation can be interpreted as the government introducing a rental market for a technology that replaces male labor, which other research suggests has much more muted responses to labor demand shocks in the Indian context (Afridi et al., 2022). In this world, labor supplied to agriculture would reduce much more rapidly in response to small wage changes. When the labor supply curve is flat, the same decrease in adoption costs leads to greater increases in adoption and limited wage effects. In equilibrium, mechanization is profitable for more farmers since wages remain high, more workers are displaced, and those displaced workers are more productive since their outside wages are higher.

The forth simulation explores if labor supply was even less elastic. This would result in further wage declines, less technology adoption, and lower productivity.

The simulations illustrate the importance of considering labor supply alongside mechanization policy and targeting labor markets with low labor supply elasticities. Importantly, for the government to achieve its goal of improving aggregate productivity, workers need to be able to move into other productive jobs. Otherwise, in equilibrium, farmers will produce the same output with less labor, but the freed-up labor will not produce anything new.

9 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 few farmers adopt the technology in the initial seasons covered by the study, these farmers save on transplanting costs and earn higher profits. Even farmers who do not drum seed earn higher profits in treatment villages.

The reason non-adopters also profited is that wages were lower in treatment villages. The wage effect suggests a relatively steep labor supply curve for female transplanting labor. Because rice production is restricted by farmers' land and water access and labor requirements for transplanting are fixed, the shift in employment can be interpreted as a shift in labor demand. 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 wage evidence, alongside workers' greater unmet demand for jobs, suggests that workers struggled to transition to other productive opportunities.

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 in the short run, the rental scheme appears to have primarily translated into farmer profits rather than aggregate productivity growth. In future research, I aim to study potential interventions to facilitate women's finding new jobs in neighboring villages. 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).

Political backlash to mechanization has been common throughout history. Mechanizing gendered agricultural tasks is a unique case where the winners and losers from mechanization are often in the same household. In my sample, only 22% of women in farming households did not engage in agricultural work. While increasing female labor force participation is an important policy goal, the types of jobs policymakers envision are not rice transplanting. The experiment highlights the importance of identifying ways for women to stay in the labor force as they transition out of agriculture. Evidence from the 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 that 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 when they were historically less relevant.

In general, the study highlights how labor market frictions can sabotage productivity growth. When workers are trapped in a task, labor supply curves will be steeper, and the wage effects of mechanization will be more pronounced. These wage effects hurt both equity and efficiency, reducing worker incomes, the extent of mechanization, and the potential increase in aggregate productivity.

Social protection programs can blunt the wage and distributional effects of mechanization, but workers' job opportunities need to improve to fully realize the potential of agricultural mechanization to increase a country's aggregate productivity.

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 will become less profitable and slower. 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, displaced workers and reduced the profitability of further mechanization in the short run. Government intervention to give transplanters more employment options would not only benefit those vulnerable women but also accelerate productivity growth and structural transformation.

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.

C Utility Functional Forms

Recall the basic labor supply problem where w_t is the transplanting wage and n is hours worked:

$$\max_n U_i(c, n) = u(c) - v(n) \tag{3}$$

Now we choose a functional form and rewrite this as:

$$U_i(c, n) = c - R\frac{n^{1+\frac{1}{\eta}}}{1+\frac{1}{\eta}}$$

subject to $c = w_t n$

The first-order conditions are

$$1 = \lambda$$

$$Rn^{\frac{1}{\eta}} = \lambda w_t$$

The labor supply is:

$$Rn^{\frac{1}{\eta}} = w_t$$

$$n^{\frac{1}{\eta}} = \frac{w_t}{R}$$

Taking logs, we get

$$\frac{1}{\eta}\log n = \log\frac{w_t}{R}$$

$$\frac{1}{\eta}\log n = \log w_t - \log R$$

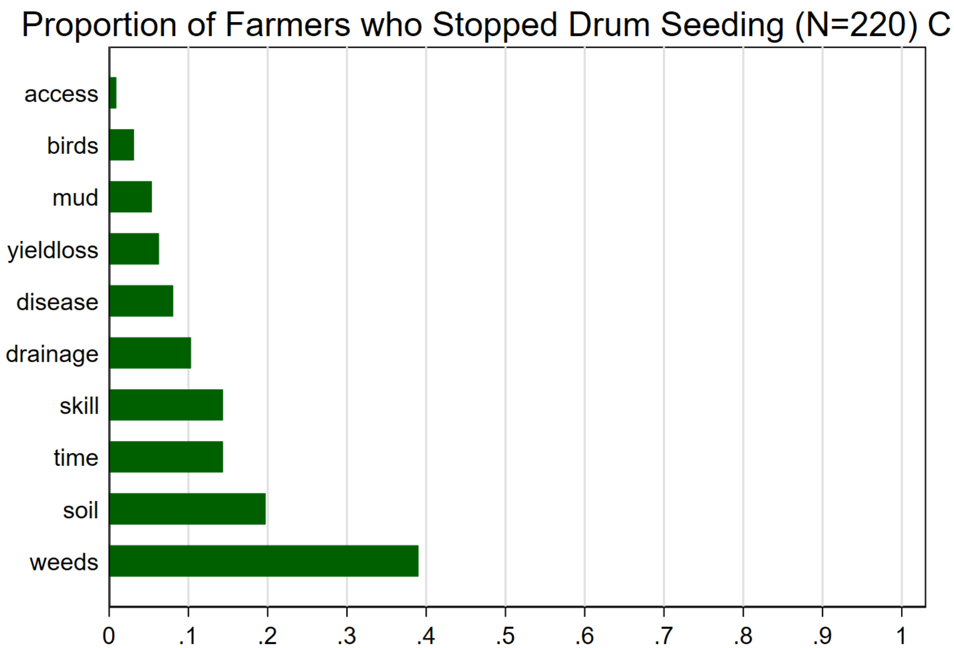
$$\log n = \eta\log w_t - \eta\log R$$

Now we can directly find the elasticity:

$$\frac{d\ln n}{d\ln w_T} = \eta$$

10 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

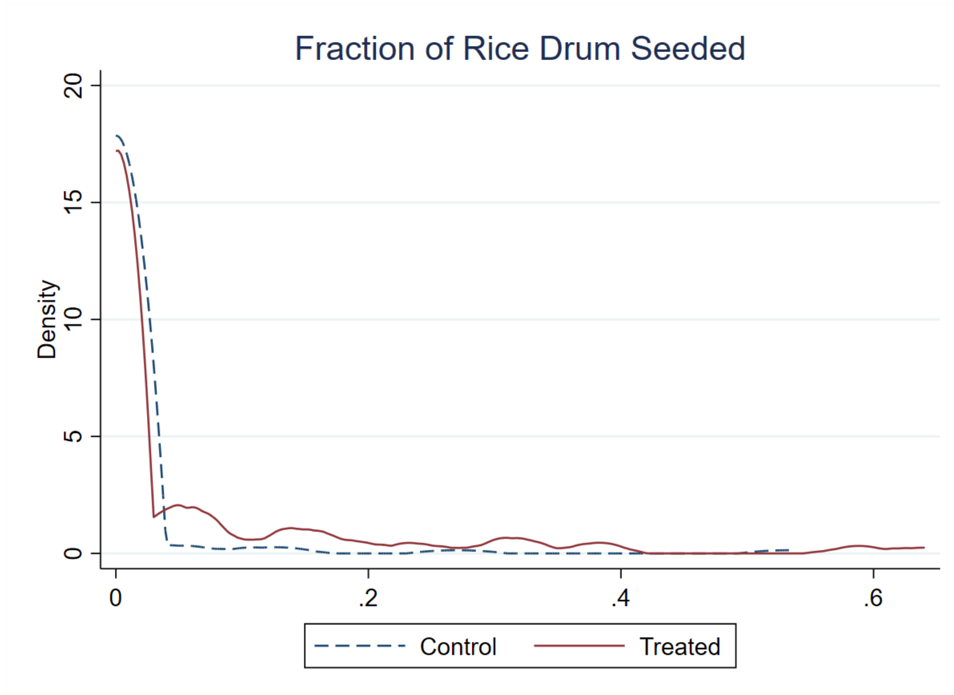


Figure 3: Main Samples

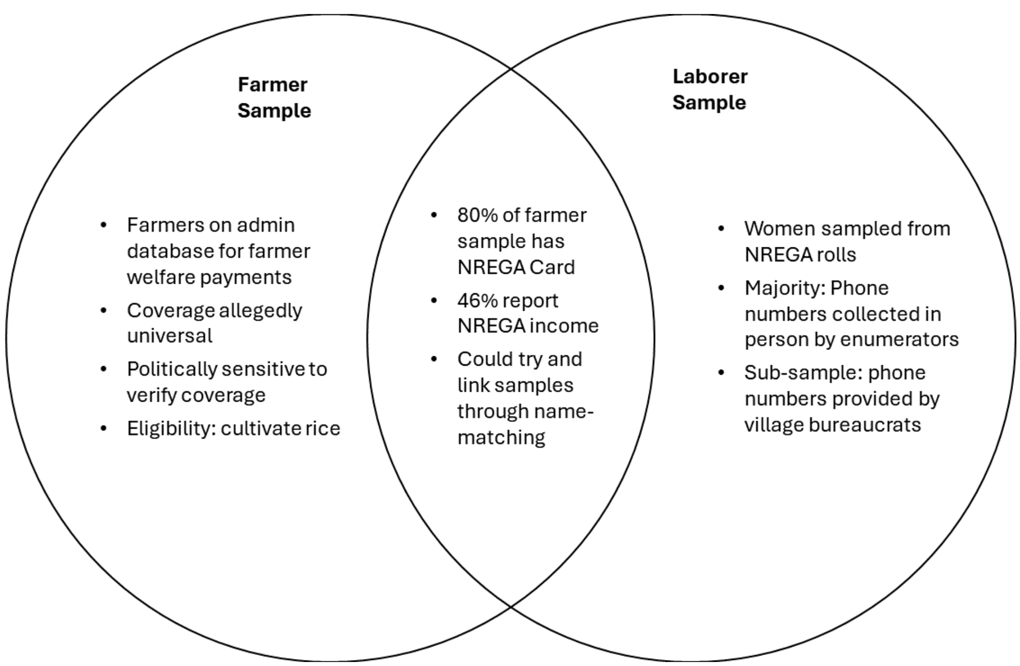


Figure 4: Timeline of data collection

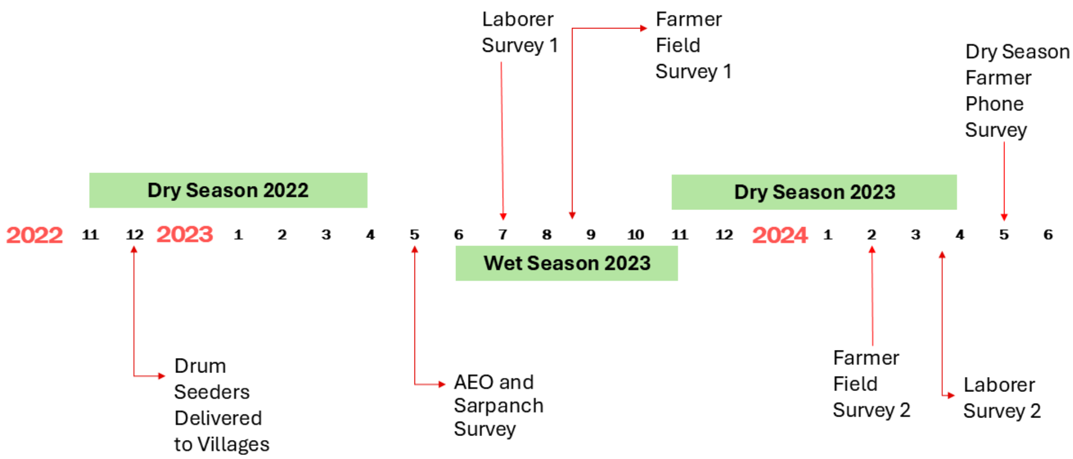


Figure 5: Comparing Wage Distribution of Different Tasks

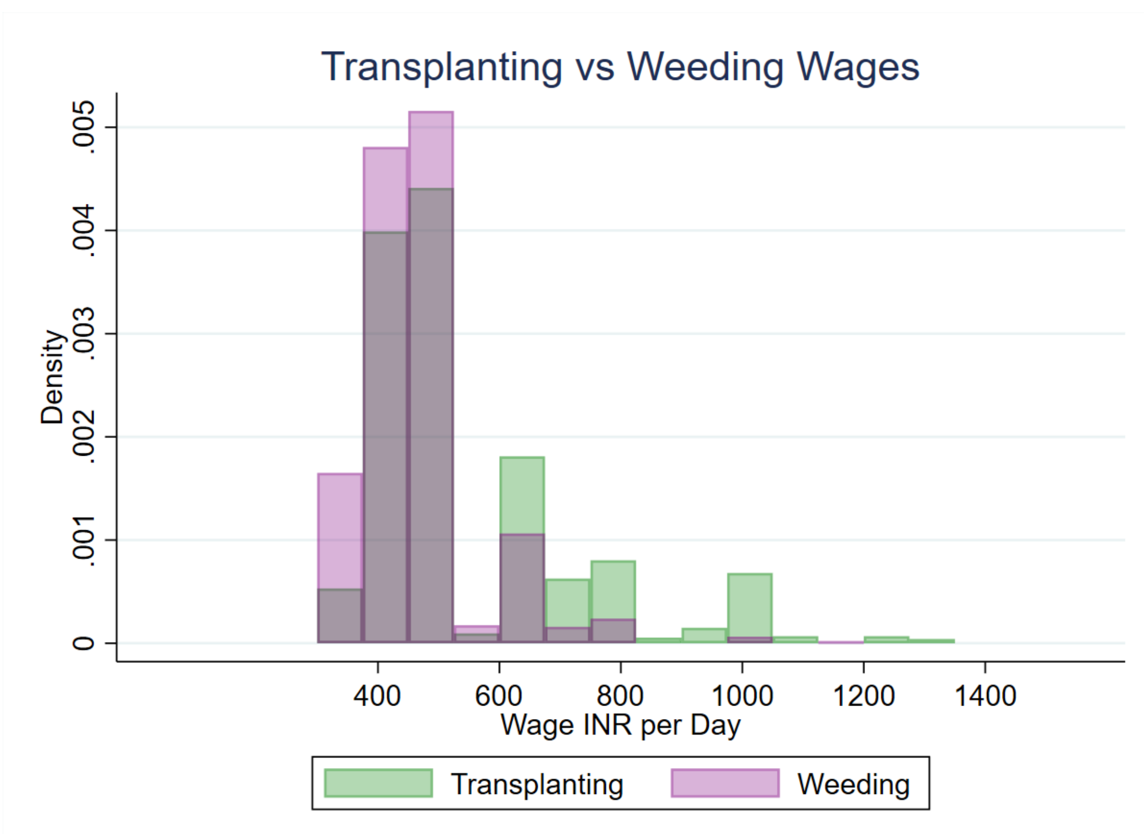


Figure 6: Comparing Wage Distribution of Different Tasks

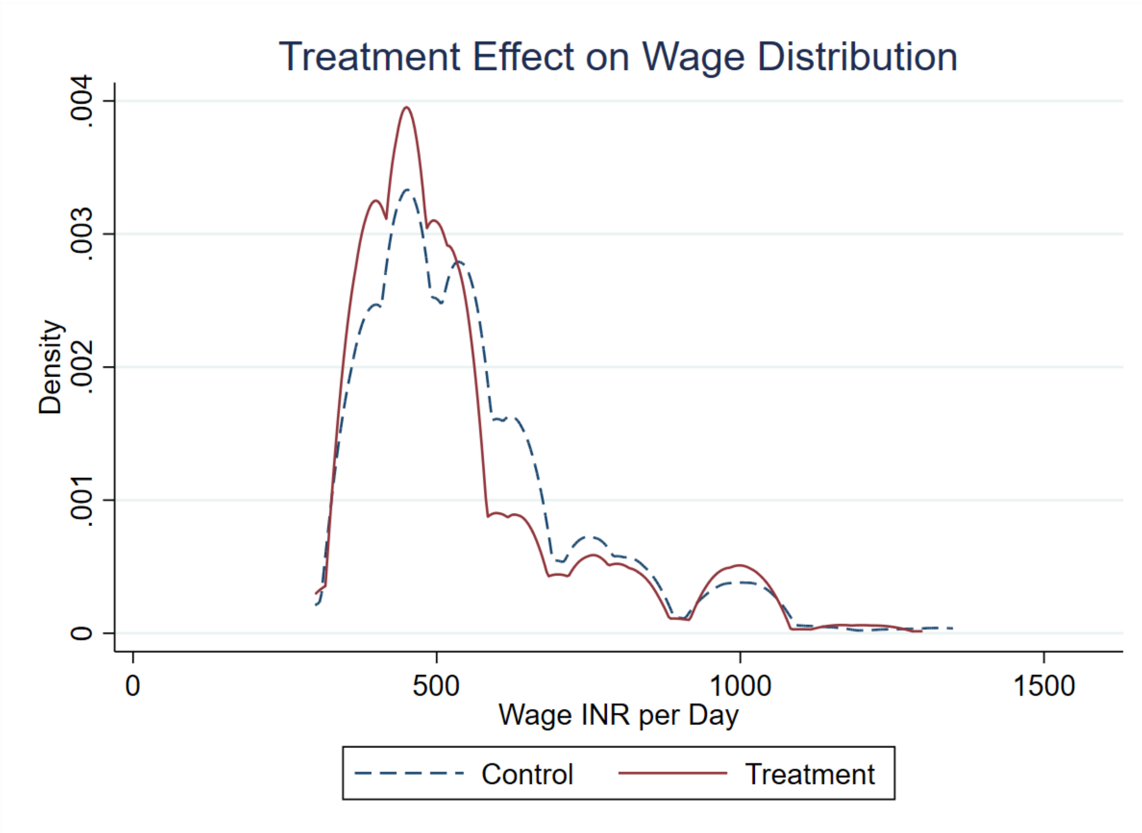
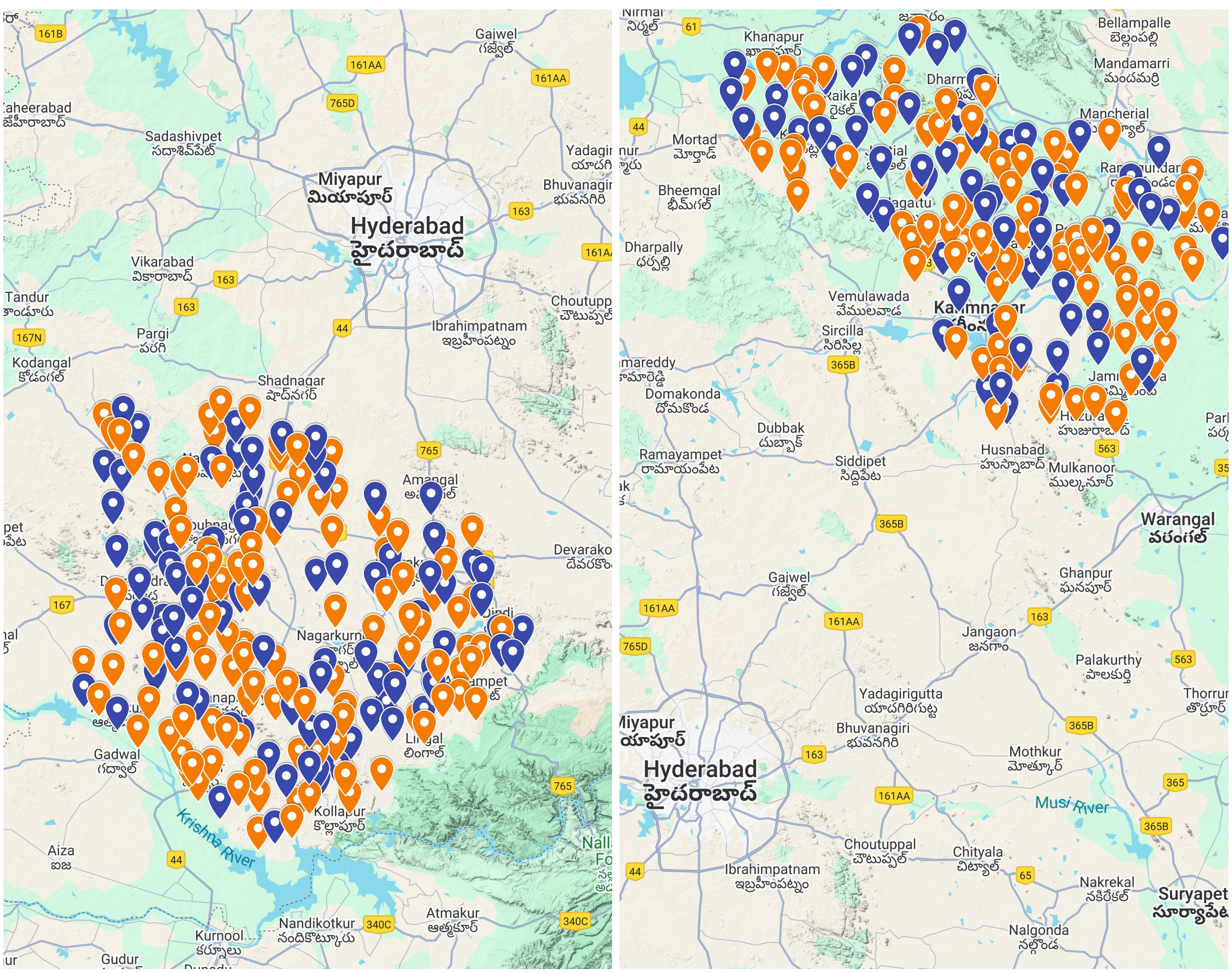


Figure 7: Transplanting in Telangana

Figure 8: Transplanting Through Time

Japan 1900 Italy 1948 India Now

Figure 9: Project Villages



Orange pins represent treatment and blue represent control

Figure 10: CDF of Rural Household's Agricultural Land Owned by Women's Primary Occupation in DHS Survey in Study Districts

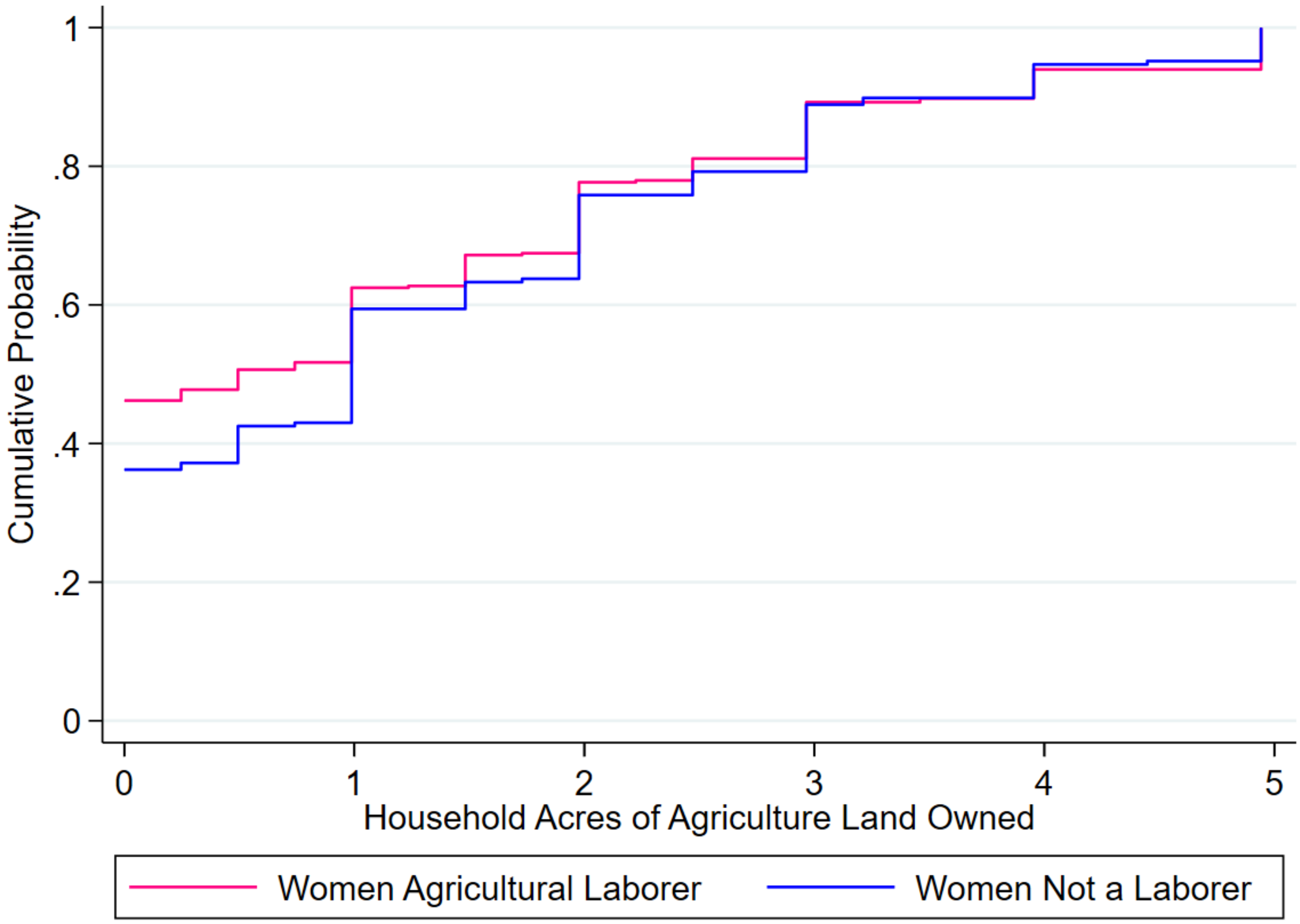


Figure 11: Distribution of Household Income Effects

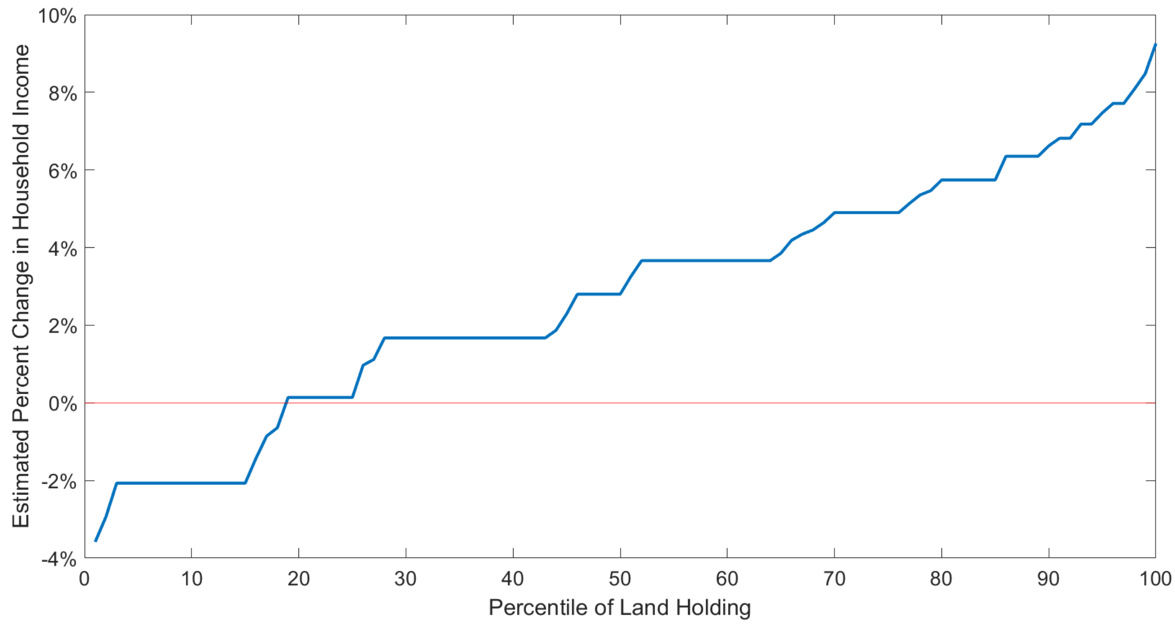
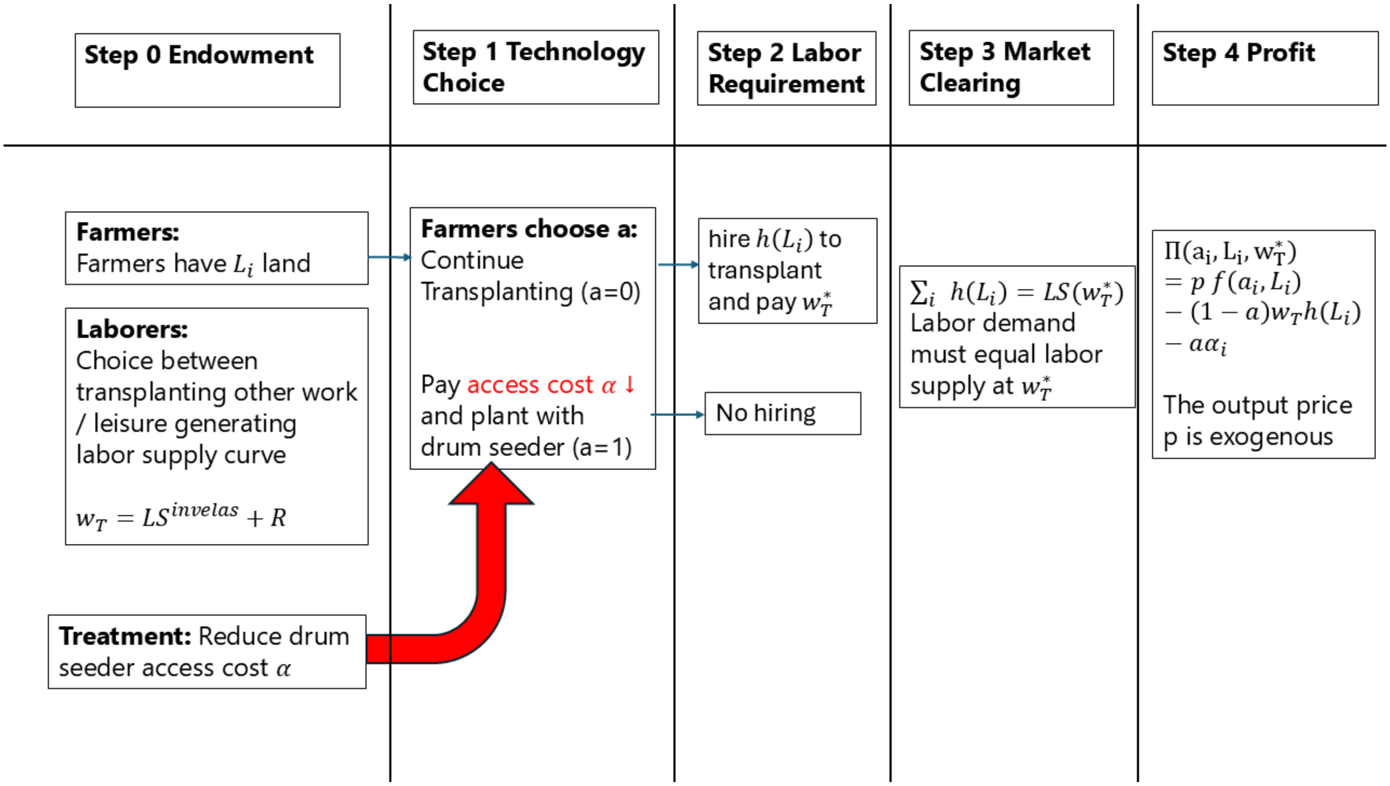


Figure 12: Model Schematic



11 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)		(2)		(2)-(1)	
	Farmer Sample		DHS Sample in Study Districts		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 Village Engagement

	(1) Leader Gives Ag Advice	(2) Leader Refers to Extensionist	(3) Drum Seeder in Village Meeting
Treatment	0.05**	0.07***	0.16***
	(0.02)	(0.02)	(0.02)
Observations	1,879	1,939	1,890
Ctrl Average	0.38	0.49	0.19

Standard errors in parentheses. Strata and enumerator fixed effects. Standard errors clustered at the village level. Column (1) is farmer report village elected leader discussed agriculture. Column (2) is farmers reported village leaders telling them to contact extension agents. Column (3) is drum seeders mentioned at the official village meeting

Table 6: 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. Ag Officer information means extensionist listed as source of agricultural advice in last month. Attend demo means the farmer ever attended a demonstration of drum seeders. Heard promotion means the farmer heard drum seeders promoted in their village. Don't know coded as missing.

Table 7: Farmer Drum Seeder Specific Knowledge

	(1) Irrig Info	(2) Weed Info
Treatment	0.08***	0.04***
	(0.02)	(0.01)
Observations	2,023	2,023
Ctrl Average	0.19	0.06
Controls	N	N

Standard errors clustered at the community level. Enumerator and Strata FE. Column 1 is knows to delay irrigation post planting. Column 2 is knows to reflood to control weeds

Table 8: Treatment Effect on Uptake

	(1) Drum Wet	(2) Drum Dry	(3) Acres Drum Wet	(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

Columns (1-2) are indicator variables for any drum seeding. Columns (3-4) are acres drum seeded. Standard errors in parentheses. Strata and enumerator fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Treatment Effect on Uptake

	(1) Drum Wet	(2) Drum Dry	(3) Acres Drum Wet	(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

Columns (1-2) are indicator variables for any drum seeding. Columns (3-4) are acres drum seeded. Standard errors in parentheses. Strata and enumerator fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: 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

Column (1) focuses on variation within village and Column (2) looks at variation across villages but wtihin strata. HH Head means household head's total completed education and is coded as a binary variable. Total cultivation is administrative baseline data on rice cultivation. Standard errors in parentheses. Standard errors clustered at the village level. sym* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: 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 village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data comes from only drum seeding farmers and includes a supplemental sample not used in the primary regressions.

Table 12: Farmers Hiring Decisions Normalized by Area Farmed

	(1) Workers Hired per Acre	(2) Workers Hired per Acre
Treatment	-0.72**	-0.72**
	(0.31)	(0.32)
Observations	1,995	2,020
Ctrl Average	9.63	9.63
Controls	Y	N

Standard errors clustered at the community level. Enumerator and Strata FE. Total female workers hired per acre cultivated across all tasks. Excludes family work. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Treatment Effect on Costs per Acre

	(1) log total labor expenditure	(2) total family days worked	(3) log total non-labor expenditure
Treatment	**-0.10***	-0.02	0.03
	(0.03)	(0.07)	(0.03)
Observations	1,967	2,023	1,789
Control Mean	8.9	1.5	9.4
Controls	N	N	N

Don't know coded as missing for expenditure variables

Table 14: 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

Actual revenue is reported sales (1-2) while imputed paddy revenue is production times district median price. The small increases are likely driven by the effects of small investments enabled by drum seeders, such as slightly better seeds. Strata and enumerator fixed effects. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: 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 village level. Enumerator and Strata FE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Yields and acres aggregate across all types of rice if farmer planted rice in multiple ways. Acres and any not paddy refer to any crop that is not rice

Table 16: Treatment Effect on Profit per Acre in USD

	(1) Paddy Prof	(2) Total Prof	(3) Winsor Paddy Prof	(4) Winsor Total Prof
Treatment	20.28**	15.15*	17.70**	13.25*
	(9.24)	(8.88)	(8.19)	(7.81)
Observations	1,844	1,895	1,844	1,895
Control Mean	216.11	246.98	215.87	245.76

Standard errors in parentheses. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Strata and enumerator fixed effects. Winsorized results winsorized at 5th and 95th Percentiles. Total aggregates across all crops and paddy is just rice. Imputed revenues used to calculate the profits.

Table 17: 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

Wage data from farmers hiring transplanting labor. Column (1) are wages based on time and column (2) are piece rate wages based on productivity. There is no treatment effect on wage type, but farmers with more land tend to pay piece rate wages. Standard errors in parentheses. Standard errors clustered at the village level. Enumerator and Strata FE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 18: 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 village level. Enumerator and Strata FE.

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

Table 19: Labor Costs for Farmers who Transplant

	(1) Ln Labor Cost Trans Only	(2) Days Hired Trans Only
Treatment	-0.07**	-0.42
	(0.03)	(0.33)
Observations	1,919	1,965
Ctrl Average	8.94	9.63

Standard errors in parentheses

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

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

Table 20: Transplanters in Treatment Villages Have Greater Unmet Demand for NREGA

	(1) Transplanted at Baseline	(2) Unmet Demand
Treatment	0.01	-1.12
	(0.04)	(0.85)
Transplanted X Treatment		1.93**
		(0.91)
Observations	553	553
Ctrl Average	0.7	4.6

Standard errors in parentheses. Data from phone survey of workers on NREGA rolls. Standard errors clustered at the community level. Column (1) is a dummy for whether the worker engages in transplanting every season at baseline since many workers only transplant a few days. Column (2) is the number of worker days the worker wanted minus the number of days they worked. Unmet demand is preferred since demand can appear to spike if the government exogenously starts a new project not considering local labor market conditions

Table 21: No Effect on Administrative NREGA Days Offered

	(1) Total NREGA Person Days July-October	(2) Person Days August
Treatment	116.09	-4.56
	(83.81)	(26.61)
Lagged Dry Season NREGS	0.27***	0.06***
	(0.06)	(0.02)
Lagged Wet Season	0.43***	0.08***
	(0.06)	(0.02)
Double Lagged Wet Season	0.10**	0.03**
	(0.04)	(0.01)
Observations	361	361
Control Mean	1867	466

Standard errors in parentheses. Standard errors clustered at the village level. Strata Fixed Effects.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Data source is the NREGA program website

Table 22: 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

Standard errors in parentheses. Data from phone survey of workers on NREGA rolls. Standard errors clustered at the community level. Enumerator and Strata FE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) is a dummy for whether the worker engages in transplanting every season at baseline since many workers only transplant a few days. Column (2) is whether women report that their husband allows them to work outside.

Table 23: 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 24: Calibration Comparison

	Baseline Calibration	Experimental Data
Inverse Wage Elasticity η	0.86	0.86
Initial Adoption	1.14%	1.00%
Final Adoption	4.56%	4.00%
Initial Wage	557	557
Final Wage	525	523

Table 25: Model Simulation Results

	Baseline	No Wage Channel	Low Elas	High Elas
Change in Fixed Cost α	-8.59%	-8.59%	-8.59%	-8.59%
Elasticity η	0.86	0.07	0.43	1.30
Initial Adoption	1.14%	1.14%	1.14%	1.14%
Final Adoption	4.56%	9.56%	5.99%	3.71%
Wage Change	-5.77%	-1.28%	-4.42%	-6.41%
Ag Prod Change	6.06%	14.56%	8.53%	4.56%
Total Prod Delta	7.79%	19.62%	11.12%	5.83%

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