The Economic Consequences of Knowledge Hoarding*

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Abstract

Social learning is an important source of knowledge diffusion in low-income countries. However, the highly localized character of many labor markets could inhibit social learning by giving rise to incentives for individuals to hoard their knowledge. This paper studies the impact of knowledge hoarding on the diffusion of profitable skills and technologies in rural Burundi and measures its aggregate and distributional consequences for the village economy. In a field experiment covering 223 villages (labor markets), we encourage workers skilled in high-return agricultural technologies to share their knowledge with unskilled individuals. We randomize at the local labor market level whether the unskilled worker is a competitor (i.e., someone from the same labor market) and whether the training is about a technology with rivalrous rents (modern planting practices, which commands a wage premium in the labor market). We first establish that knowledge hoarding indeed reduces social learning. When incumbents are matched with an individual from the same labor market, knowledge transmission occurs only 3% of the time, but this figure reaches 43% if the unskilled worker is not a competitor. In contrast, transmission of a technology with nonrivalrous rents (composting) is high regardless of the unskilled worker's identity. Next, we show that knowledge hoarding creates winners and losers: By hoarding knowledge, incumbents earn 6% more, and the skilled equilibrium wage is 3% higher. In contrast, unskilled workers' earnings and farm output are 7% and 20% lower, respectively. Overall, knowledge hoarding reduces technology adoption by over 20%, suggesting substantial yield losses. Finally, our results suggest that fear of social sanction is a mechanism that sustains knowledge hoarding among the incumbents.

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If one nation or class has the knowledge which enables it to achieve high productivity, why is not the other acquiring that information?

Kenneth J. Arrow (1969)

1 Introduction

Adoption of new technologies is crucial for economic growth (Romer, 1990b). These technologies often take the form of information and skills (Rosenberg, 1972). For example, increasing agricultural output in low-income countries (LICs) requires adopting higher-value crops and techniques, but individuals lack the knowledge and skills to use such modern agricultural practices successfully. When knowledge diffusion plays a central role in technology adoption, this creates scope for social learning to affect the process of growth and development.

Social connections can be particularly important in the transmission of knowledge and skills in LICs, which display certain features—such as strong social ties, repeated interactions in localized markets, and homogenous occupations such as farming—viewed as conducive to the social transmission of knowledge (e.g., Rogers, 1983; Foster and Rosenzweig, 2001; Jackson et al., 2012). However, these very same features of poor countries could, under some circumstances, also *inhibit* information diffusion: Because developing country markets are often highly localized, individuals with social ties may compete more directly for the same economic rents.[1] This, in turn, can create incentives to inhibit knowledge diffusion to preserve some strategic advantage.

In this paper, we test whether the potential for such pecuniary externalities can create incentives to strategically withhold, or "hoard," knowledge of certain technologies or skills from others, and quantify its aggregate and distributional effects in the village economy.[2,3] Moreover, we argue that the strength of social ties among some individuals might enable the enforcement of collective norms among against information sharing with other individuals (Breza et al., 2019)—preventing diffusion even in communities where individuals are in principle atomistic.

We test for the presence of knowledge hoarding within the context of high-return agricultural technologies in Burundi. In this setting, product markets are fairly well integrated, so that modest increases in the output of one's covillagers does not affect one's own crop prices or revenues. However, as is the case in many developing country settings, agricultural labor markets in rural areas are highly segmented—with the village constituting the local labor market (Fink et al.,

[1]For example, among workers in the same village, the number of employers is largely fixed, and relatively modest changes in the number of skilled workers could affect wages.

[2]An early sociology literature documents the prevalence of the "image of limited good" in pre-industrial society, i.e., the idea that there is only a finite amount of good in a society over which individuals compete, and posits that this may hinder development (see, e.g., Foster, 1965).

[3]Incentives to hoard information from socially connected individuals may arise even in nondevelopment contexts, such as firms or classrooms. For instance, a literature in organizational behavior documents the hoarding of resources and knowledge within firms (e.g., Gagné, 2009).

2020; Breza et al., 2021).[4,5] This segmentation suggests that incumbents may be likelier to hoard knowledge of skills whose returns are realized in the labor market as a wage premium rather than in the output market.

To motivate our hypothesis, we collect data on the diffusion of various agricultural technologies recently introduced by a large international nongovernment organization (NGO) in Burundian villages, where only a subset of farmers in each village were trained. Among technologies that increase farm productivity but for which no hiring occurs in the labor market, such as compost production or application of antimold products, we document wide levels of diffusion: Within five years, over 50% of households not directly trained adopted these technologies. In contrast, the "Tubura practices"—a bundle of techniques including plant spacing, construction of seedbeds and application of complementary inputs—a technology for which farmers often hire in the labor market and which commands a wage premium, diffused to less than 10% of the households that were not directly trained. Consistent with our hypothesis, incumbent skilled workers express fear that diffusion of the Tubura practices could reduce their employment rates and wages.

We design two large-scale field experiments to test for knowledge hoarding and its implications. In both experiments, we create an opportunity for individuals who know a given modern agricultural practice to transmit their knowledge to those who do not—via group training events, the normal format of the agricultural extension activities undertaken by the large agricultural NGO with which we partner. In these events, each skilled worker is paired with an unskilled one and given a plot of land along with the material necessary for the training. The training itself is unsupervised, and individuals have the option to engage in leisure activities, so that skilled workers have discretion in whether to and how well they provide training. Within this setup, we induce variation in whether the skilled workers have an incentive to hoard knowledge.

Specifically, in the first experiment, we induce two cross-randomized sources of variation in knowledge-hoarding motives. First, we vary whether the skilled and unskilled workers compete in the same labor market—by randomizing whether they are from the same or a different village. Second, we cross-randomize whether the agricultural task itself is one for which there are labor market returns (Tubura planting practices) or placebo technologies for which there is no hiring in the labor market (compost production and post-harvest storage techniques). In this 2×2 design, we expect knowledge-hoarding motives to be particularly pronounced when skilled workers train others from their own village in the planting practices but not under any of the other three conditions. We conduct this experiment with 1073 pairs of individuals across 102 villages where these technologies

[4] For instance, in our context, we document that 80% of workers travel less than 20 minutes to reach employers, with transport costs to neighboring villages often exceed 50% of daily wages. These statistics match, for example, Breza et al. (2021), which documents that 70% of agricultural days of labor occur within the village, and 97% percent within 5 kilometers of the village.

[5] Contrarily to labor markets, output markets for agricultural output tend to span multiple villages. Farmers can either sell in markets that attract vendors from many villages, or to local traders that buy at farm-gate.

were only recently introduced (and so have not yet had a chance to diffuse).

We find, consistent with our hypothesis, starkly different levels of skill transmission based on whether knowledge-hoarding motives are present. When the members of a skilled–unskilled pair do not compete in the same labor market (i.e., are from different villages), 38.2% of the unskilled workers are successfully trained in the planting practices (as measured by a practice proficiency test). However, we find that, when the unskilled workers do compete in the same labor market, less than 3% of them learn the technique. In contrast, the level of training for the placebo activities is approximately 90%—regardless of whether the pair is from the same or a different village (p=.771).

The difference in learning outcomes in the planting practice does not appear to be fully explained by the time that the incumbents spend training acoss the two arms: Unskilled workers paired with skilled workers from a different village receive only 27.1 more minutes of training. Rather, there is a stark difference in the *quality* of the training: Incumbents paired with non-competitors are 25.8% likelier to provide feedback or corrections during training. These findings demonstrate both incumbents' ability to transmit knowledge to others and their reluctance to do so among those with whom they compete for jobs.

Heterogeneity analysis suggests that incumbents are more likely to hoard their knowledge when they depend more on the labor market for their livelihood. We use several proxies to identify individuals who have greater stakes in the labor market, such as having above-median labor market earnings or working more days in the planting practice compared to the other workers in the village at baseline: unskilled workers who learn the planting practice from incumbents from their own village with above median earning (days of employment performing the plantint practices) are 14 (17) percentage points less likely to pass the test (p = .004 and .040). No such heterogeneity appears when unskilled workers are paired with incumbents from their village to learn the placebo skill. Moreover, in learning the placebo technology, we confirm the predictions of social learning theory: unskilled workers learn more when they have social ties with the incumbent.

To further test whether skilled workers behave strategically to limit diffusion, we conduct a supplementary exercise. At baseline, we document that the unskilled workers greatly overestimate how difficult it is to learn the planting practice: The amount of time that they believe it takes to learn to row plant is 50% longer than the corresponding belief among skilled workers. To understand the source of these divergent beliefs, we organize focus groups in which skilled workers are invited to discuss several aspects of modern agricultural practices and are told that the goal is to inform researchers about the benefits and obstacles the workers face. The discussion includes asking the skilled workers how long it takes to learn to row plant. In a random half of the focus groups, we also invite the unskilled workers to attend. When unskilled workers are present, the time estimate given by the skilled workers to learn the planting practice is more than *twice* longer than the time given when unskilled workers are absent—with the latter estimate being more closely aligned with the truth. This suggests that skilled workers intentionally attempt to curb unskilled workers' demand for learning by inflating their beliefs about its costs.

In our second experiment, we build on the 2×2 design to measure the economic costs of knowledge hoarding and its distributional implications. In 121 additional villages, we invite 30% of the unskilled labor force to a training event—again randomizing at the village level whether the workers are paired with a skilled worker from their own village or from a different village. This enables us to compare the outcomes under the status quo (where skilled and unskilled workers are from the same village) with those in a counterfactual world where knowledge hoarding motives are substantially lower (where skilled and unskilled workers are from different villages). We also introduce a *Control* arm where unskilled workers are invited to an event but receive no training.[6] We collect data on labor market outcomes, adoption of the planting practice, and farm output for over 6,500 farmers across the 121 villages through two survey rounds several months after the training events.

We have four main findings, which together demonstrate that reducing knowledge hoarding increases aggregate productivity, albeit at a cost for incumbent skilled workers. First, we replicate the result in our first experiment that unskilled workers learn substantially more when paired with a skilled worker from another village than when paired with one from their own village (market). Importantly, knowledge hoarding is even more prevalent in smaller markets, as suggested by the fact that the learning gap between unskilled–skilled pairs from the same village as opposed to different villages is much larger when the incumbent comes from a village with a smaller labor force. Second, the difference in learning has substantial consequences for the labor market earnings of the unskilled workers who learn from incumbents from a different village: Their earnings increase 7% more than those of the unskilled workers in the *Competitor villages* arm ($p = .035$). This increase in earnings comes from an increase in days of row-planting work: Unskilled workers paired with incumbents from a different labor market spend 140% more days working with the more productive row-planting technology than those paired with same-village incumbents ($p < 0.001$).

Third, we look at the incumbents and find that the gains for the unskilled workers come at the expense of the skilled workers operating in their labor market: Skilled workers in villages where the unskilled learn how to row plant from outsiders earn approximately 6% less than *Competitor village* skilled workers ($p = .041$) and earn a lower average daily wage ($p = .083$). Importantly, this result suggests that the incumbents correctly anticipate that diffusion would cause them economic losses. Finally, at the village level, we observe a 19% increase in the number of days worked in the planting practice ($p = .034$) and a 23% increase in the plots planted with this technology ($p = .025$),

[6] We do not introduce the placebo technology variation here because, in the villages where the second experiment occurred, the NGO has been operating for some time and so the technologies have already diffused widely. This feature of the setting enables us to examine impacts of knowledge hoarding in villages where the level of diffusion has plateaued.

suggesting an increase in productivity. When we look at the equilibrium wage, we find that the average daily wage for the row-planting task falls by 3% (p= .068).

Next, we ask which costs motivate hoarding in the first place. We find evidence for two main factors. First, we document that the vast majority of skilled workers (78%, N=3470) believe that sharing knowledge of the planting practice with one unskilled individual would lead to much broader diffusion that, in turn, could affect their earnings. Second, we find evidence that incumbents expect that sharing knowledge with an unskilled worker in their village would lead to social sanction by other incumbents. In a supplementary survey of 374 skilled workers, we find that 77% report that other skilled workers would impose some form of social sanction if a skilled worker were to train another laborer from the same village, with more than 65% citing work-related consequences (e.g., exclusion from future job opportunities or being negatively spoken about by employers) and approximately 50% mentioning social exclusion or gossip. Furthermore, social sanctioning appears to be stronger in smaller communities, where it is perhaps easier for incumbents to coordinate: Incumbents from smaller villages are 25% *likelier* to mention any kind of sanction than those from larger villages $p = .002$. This differential prevalence of social sanction can explain our previous finding that knowledge hoarding is more prevalent in smaller markets.

We contribute to four strands of literature. First, we contribute to the literature on the social diffusion of technologies in LICs and in particular in the agricultural domain.[7] A common assumption in this literature is that there are limited strategic incentives to hide information about productive technologies (Foster and Rosenzweig, 1995; Conley and Udry, 2010; Beaman et al., 2021). More recent work points out that heterogeneity in the costs of sharing and acquiring information based on the senders' and receivers' characteristics also matters for diffusion of new technologies, but this work does not discuss strategic motives (BenYishay and Mobarak, 2018; Beaman and Dillon, 2018; Chandrasekhar et al., 2022; Bandiera et al., 2023).[8] We contribute to this literature by showing that the fear of losing rents from technology diffusion is a strong deterrent of knowledge-sharing in social networks, and we highlight conditions—namely, the rivalrousness of returns—likely to lead to knowledge hoarding.

Second, this work speaks to the literature on the diffusion of rivalrous information in the field.[9]

[7]A large and longstanding literature documents the central role of social learning and learning externalities in the diffusion of new technologies, in both rich and poor countries, dating back to, e.g., Griliches (1957). In the context of LICs, Foster and Rosenzweig (1995), Bandiera and Rasul (2006) and Conley and Udry (2010) are some foundational contributions. Since then, there is a large experimental literature in Economics that documented social learning of agricultural technologies (e.g., Feder and Savastano, 2006; Behaghel et al., 2020; Emerick and Dar, 2021; Fafchamps et al., 2021). However, the literature has also found that sometimes social learning is ineffective, pointing to a variety of reasons why this is may be the case (e.g. Marenya and Barrett, 2009; Suri, 2011; Kondylis et al., 2017; Tjernström, 2017; Beaman and Dillon, 2018; Dzanku et al., 2022)

[8]In particular, BenYishay and Mobarak (2018) endogenize the senders' decision to share their knowledge based on communication costs that depend on both sender and receiver characteristics.

[9]Some theoretical contributions on this topic include Immorlica et al. (2014) and Persson et al. (2021). Both consider agents' decision to share information that loses value as more people acquire it, where they trade off

Most of the empirical literature has studied this issue in the context of job referrals (Chiplunkar et al., 2024) or participation in experimental games (Banerjee et al., 2012; Vilela, 2019). Two exceptions are Hardy and McCasland (2021) and Cai and Szeidl (2018), which explore the possibility that interfirm competition may hinder knowlege diffusion.[10] Our contribution to this empirical literature is twofold. First, our unique experimental setting allows us to measure the passthrough of strategic sharing to economic outcomes and measure the aggregate costs and equilibrium effects of knowledge hoarding. Second, our experiment shows that incumbents' concerns about reduced rents are justified. It also highlights that the diffusion of technology—which is a non-rival good (Romer, 1990b)—can generate rival returns in markets with inelastic demand.

Third, this work contributes to a nascent empirical literature on the role of collective behavior in the functioning of markets in LICs. A long-standing, mostly theoretical, literature discusses how communities can self-regulate behavior and sustain cooperation through informal sanctions and reputation (e.g., Osmani, 1990; Kandori, 1992; Greif, 1993; Fudenberg et al., 1994). A recent literature (e.g., Banerjee et al., 2022; Breza et al., 2019) provides compelling documentation of the presence of implicit coordination among groups of individuals but do not provide evidence on whether such coordination is consequential for market equilibria. Our paper furthers this literature by showing that such cooperation among decentralized agents can actually change equilibrium outcomes in markets (e.g., via prices) and consequently aggregate output.

Finally, this work relates to a literature in organizational behavior on knowledge hoarding within organizations (e.g., Gagné, 2009; Wang and Noe, 2010; Connelly et al., 2012; Connelly and Zweig, 2015; Bolino and Grant, 2016; Stenius et al., 2017; Gagné et al., 2019).[11] While this literature documents that knowledge hoarding is pervasive in modern organizations, and suggests that can have meaningful costs (e.g., Babcock, 2004), we contribute by highlighting that knowledge hoarding can be relevant well beyond the boundaries of the firm and, under certain circumstances, can even affect market equilibria.

2 Context & Motivating Evidence

We conduct our field experiment with farming households in Muramvya, Gitega and Mwaro provinces, Burundi. Despite a favorable climate for production, agricultural yields in this re-

strategic considerations with other-regarding preferences (e.g., altruism, reciprocity).

[10] In particular, Hardy and McCasland (2021) cross-randomize training in a new weaving technique among small firms in Ghana and one-off orders that require workers to use the technique. Their main finding is that firms receiving experimental demand but not training learn the new technique from firms that receive both training and the experimental order but not from firms that receive the training alone. Cai and Szeidl (2018), instead, form business associations for firm owners and find that information about rivalrous financial products introduced by the researchers diffuses less in groups where the business owners are direct competitors.

[11] An important distinction between knowledge hoarding and lack of information transmission is that the former has some elements of strategic behavior. See also Bilginoğlu (2019) for a review of the literature on knowledge hoarding.

gion are relatively low.[12] We collaborate with a large international NGO that introduced several agricultural technologies with the aim of increasing farm yields in Burundi. We describe some of these technologies in sections 2.1 and 2.2. Next, in section 2.3, we describe the agricultural labor market in Burundian villages, where laborers who can row plant are in high demand. Finally, in section 2.4, we document how these different agricultural technologies diffuse heterogeneously in Burundian villages.

2.1 "Tubura" Planting practices: A profitable but time-sensitive technology

Main characteristics. We study the diffusion of the Tubura planting practices, a bundle of agricultural techniques promoted by the NGO One Acre Fund (locally known as Tubura) that requires farmers to i) till the land and construct well-ordered seedbeds, ii) sow in parallel lines spaced by the same, specified distance throughout the field, and iii) microdose fertilizer by applying precise amounts in a specified order, along with other complementary inputs such as compost, in the seedbeds as they row plant.[13]The alternative to the practices is broadcasting, a traditional method in which Burundian farmers scatter seeds and other inputs semi-randomly across their fields—a practice that most still use today.

Adopting these practices take longer and demands greater physical effort than broadcasting. This has important implications in settings—such as Burundi—where agriculture is rain-fed: Planting must be completed during a short window of approximately one to two weeks after the onset of rains, making labor a key input in the production function.[14]

Returns to the planting practices: Own-farm adoption and labor market. Agronomic studies estimate that optimal spacing increases crop yields by 30–100% (Vandercasteelen et al., 2020; Jeyabalasingh and Bayissa, 2018; Mihretie et al., 2021). In Rwanda, a neighboring country with similar agro-ecological conditions, studies find that improved spacing alone increases yields by 30–70% (Dusabumuremyi et al., 2014), while many other agronomic and less controlled evaluations estimate positive returns to adoption (Blackshaw et al., 1999; Alemu et al., 2014; Fentie and Beyene,

[12]In 2018, average maize and bean yields in Burundi were equal to approximately 1.53 tonnes and 0.66 tonnes per hectare, among the lowest yields in the world for these two crops (Ritchie et al., 2022). We do not have disaggregate data by province.

[13]The introduction of high-yielding, more labor-intensive technologies in agriculture is common in LICs. For instance, Emerick et al. (2016) study how the adoption of new flood-resistant seeds boosts diffusion among rice cultivators in India of manual transplantation methods for planting the seedlings as opposed to broadcasting. Aker and Jack (2023) study the adoption of rainwater harvesting techniques that prevent soil degradation and require considerable upfront labor cost investments. Jones et al. (2022) discuss how labor costs prevent the adoption of irrigation-fostered horticultural practices.

[14]The widespread notion that delays to planting decrease yields is consistent with a long agronomic literature (Howard et al., 2003; Kruger, 2016). In Burundi, the government reinforces this urgency by providing geography-specific planting windows to guide farmers on optimal timing (IWACU, 2022; SOS Médias Burundi, 2024).

2019; Martey et al., 2020). The reasons for the yield gains from optimal spacing are fourfold: First, it reduces plant competition for water and nutrients; second, it increases germination rates and chances of survival post-germination; third, it increases the crop's yield response to other inputs, such as fertilizer; and, finally, it reduces weeding requirements later in the agricultural season (Mansingh J and Deressa Bayissa, 2018; Vandercasteelen et al., 2020). The main additional cost of practices adoption is the extra labor required at planting, though this may be offset by lower seed costs and reduced weeding labor. Cefalà et al. (2024) find that adoption of the Tubura planting practices is profitable in this setting.[15]

In addition, knowledge of the Tubura planting practices yields labor market returns for farmers who work in the local casual labor market. Farmers who master this technology can earn up to a 20% wage premium for performing the row-planting task when hired by other farmers, as we document in a representative sample of the labor force in over 120 Burundian villages in our baseline data (Appendix Table A5). This wage premium results from the relative scarcity of workers who know how to row plant in the village (see section 2.3), further exacerbated by the fact that the practices requires approximately twice the number of man-days to plant the same area as traditional methods.[16]

Note, however, that labor contracts in this setting mean that returns in the labor market require workers to be able to plant quickly and accurately. Many contracts are structured as a "task", in which workers must complete planting a certain area of land before being paid. Moreover, contracts that are paid as daily wages tend to involve an implicit agreement about the amount of area that the worker must finish in a day. Because of this, the wage is diluted in cases where the worker is slow, or inconsistent in their work. Because of this, learning how to work quickly and accurately is a key element required to achieve labor market returns.

Mastering the Tubura planting practices: Key skills and sources of learning. Learning the practices requires both technical and practical knowledge. Some technical aspects, such as the exact spacing of rows and seedbeds, may be observed in other fields, but replicating them precisely is difficult without tools like ropes or poles.[17] Other key aspects—such as seedbed depth and the correct application of inputs (e.g., fertilizer and compost)—are unobservable once fields are planted, making them harder to learn through observation alone.

[15]Cefalà et al. (2024) randomize incentives for employers in some labor markets to train casual laborers and find that laborers invited to be trained in villages treated with financial incentives adopt the practices in an additional 1.3 fields, increasing farm profitability by 14%.

[16]Skilled labor is essential for achieving the benefits of adoption: If the spacing and density guidelines are applied incorrectly (Morris et al., 1999), their adoption can *lower* yields.

[17]This difficulty may be partly explained by low educational attainment: 40% of non-adopters never attended school, and only 10% studied beyond primary school (see Table A5). This is consistent with previous studies showing that a farmer level education correlates positively with technology adoption in agriculture (e.g. Knight et al., 2003)

Beyond technical knowledge, mastering the planting practices also requires hands-on experience, which is even more essential on uneven or sloping terrains—the majority of farms in Burundi. While farmers might eventually be learn through practice, doing so without guidance is difficult. Most experienced farmers report that they initially learned these techniques through direct NGO training. Novice farmers need real-time corrections and repetition to develop proficiency, a process made easier with teaching aids such as ropes to ensure consistent row spacing. Finally, the short planting window demands both speed and accuracy, posing a significant challenge for those without proper training and experience.

NGO training is the primary source of learning among the farmers in our sample. However, the NGO's training is typically limited to its members because of space constraints. As is typical of many NGOs offering agricultural or credit services, the NGO imposes some barriers for nonmembers in the form of enrollment fees, limits on the number of accepted clients per village, and joint liability requirements for receipt of credit services.

We also find that two other sources of learning—training by government extension agents or by employers—are not effective in this context. First, the government extension agents (*moniteurs agricoles*)—tasked with disseminating modern agricultural technologies in Burundi—appear to have limited coverage, and as is common in other sub-Saharan contexts, few farmers at baseline report having learned from them.[18] We are also unaware of other training centers. Second, Cefalà et al. (2024) find that on-the-job training by employers is rare, partly because they share limited social ties with workers. Overall, these factors imply that learning the practices from other socially connected farmers or from coworkers is the most viable avenue for learning among many unskilled farmers who are not NGO members.

2.2 Other agricultural technologies

Main characteristics. We contrast with row-planting the diffusion of two other technologies introduced by the same NGO: modern compost production and post-harvest storage techniques. Composting follows a three-step process: i) selection of suitable materials such as manure and crop residues, ii) creation of a compost heap through layering of green and brown materials, and iii) proper monitoring and stirring to ensure even decomposition. During training, farmers learn how to build a proper composting heap and how to recognize when the compost is ready for utilization, a key element distinguishing this method from traditional compost production, whereby farmers do not wait for proper decomposition.

Likewise, adoption of post-harvest storage technologies involves three steps: i) an initial reduction of grain moisture through adequate drying, ii) sorting of spoiled or pest-infected grains to prevent

[18] This is consistent with findings from other studies in the region. For instance, BenYishay and Mobarak (2018) document that each extension agent in their sample serves almost 2500 farmers.

contamination of the healthy grains, and iii) storing of the dried and healthy grains in appropriate containers and in areas with adequate ventilation. Important aspects of the training involve identification of best practices for drying and storing the grains and of signs of pests through regular monitoring.

Returns to technologies: Own adoption but not in the labor market. Adoption of the above technologies is beneficial for farmers. Post-harvest storage techniques reduce spoilage and increase the harvest shelf-life by reducing the likelihood of the appearance of pests or rotting due to excessive humidity. Modern compost production provides farmers with an affordable source of organic fertilizer, which improves the soil's mineral content and, thus, its fertility.[19] It also enhances water retention, leading to healthier crops and higher yields.

While we do not have experimental evidence on the returns to these technologies in Burundi, randomized trials document positive effects from adoption in other contexts. Basu and Wong (2015) find that adoption of similar storing practices among Indonesian farmers decreased households' likelihood of reporting food shortages, while BenYishay and Mobarak (2018) estimate a 50–100% average increase in yields from adoption of composting.

A key distinction between these technologies and the planting practices is that knowledge of composting and post-harvest storage is not associated with labor market returns: In our sample, we do not observe any hiring to perform these tasks. A major reason for this difference is that neither compost production nor storage are time-sensitive tasks: They are performed during the offseason, when farmers have limited additional commitments, and there is no strict requirement to complete them within a certain time window.

Although there are no labor market returns to knowledge of these technologies, farmers may believe their diffusion will result in lower harvest returns by decreasing the price in the output market. However, we expect price effects to be unlikely because most unskilled farmers have limited engagement in the market selling output: 20% of unskilled farmers report having sold any harvest at baseline and those who do sell only a small fraction of it. Second, output markets tend to be more integrated than labor markets because the former typically aggregate output from several villages and there are middlemen who buy harvest directly from farmers.

Key differences in learning requirements. Post-harvest storage and composting technologies were introduced concurrently with the planting practices to NGO members in the villages, and just like the practices, they require some training to learn: unobservable elements make it impossible to learn through observation.

[19] Sub-Saharan African countries have experienced a decline in soil content of key nutrients such as nitrogens, calcium and magnesium due to overuse, deforestation, and unsustainable farming practices (Batjes, 2008; Montanarella et al., 2015).

2.3 Agricultural labor markets

Labor market characteristics. Burundian villages have active markets for agricultural labor during the peak agricultural seasons. Labor is hired from decentralized and informal labor markets, similar to casual rural labor markets in other low- and middle-income countries (e.g., Fink et al., 2020; Breza et al., 2021).

A village (*sous-colline*) defines a local labor market in this setting.[20] The logic of this definition is that the villages are isolated and transport costs prohibitive, a finding consistent with the characteristics of other African settings (Fink et al., 2020). On average, the labor force in the villages in our sample comprises 89 workers, of which 42% are skilled (see Table A4).

Contracting is arranged bilaterally between employers and laborers, often with the employers visiting the households of various laborers or with laborers visiting employers requesting jobs. In the vast majority of cases, employers attempt to contact laborers in person 1–2 days prior to requiring their labor and contract labor for just a few days. This style of search offers scope for workers to signal their skills to prospective employers either by demonstrating their technique in their own fields near their house or by showing how fields close to their households have been planted (if sufficient time since the onset of rains has passed and the fields were planted sufficiently quickly). After initial contact, employers and employees appear to bargain over wages, which depend on a variety of features including the task assigned and size of the land to be prepared (Fink et al., 2020).

Household participation in the labor market. Households can participate in the labor market as both buyers and suppliers of labor. Table A1 shows some of the household characteristics based on labor market participation from a household census conducted in a sample of 35 villages in the *Control* group. On average, 25% of households in a village only hire labor, 9% both hire and supply labor, and 49% only supply labor. Column (3) in the same table shows that the households that only supply labor tend to be poorer: they are 15 times as likely to be in the bottom quartile of land ownership in the village, and only 9% belong to the top quartile. In contrast, households that hire workers tend to be concentrated in the top quartile of the land distribution in the village (column 1) and have three times more savings than households that only supply labor.

Focusing on households that only supply labor (columns 3 and 4 in Table A2), we see that those that are not NGO members tend to be better off: They are 15 percentage points (p.p.) less likely to be in the bottom quartile of the income distribution with respect to nonmember households and are much likelier to have adopted the planting practices on their own farm (88% compared to 7%). Furthermore, skilled workers are almost exclusively NGO members.

[20]In a full household census we conducted in 35 control villages, we find that the average number of households is 175.

2.4 Heterogeneity in technology diffusion

Heterogeneous diffusion of different technologies beyond original seeds. We begin by documenting significant heterogeneity in the diffusion of agricultural technologies beyond the individuals who were originally seeded (i.e., received the information about the technology) by the NGO in the first place. To do so, we conducted a full household census in a sample of 35 *Control* villages from the market effect experiment and other nonstudy villages.[21] As Figure 1 shows, adoption rates among the NGO members are high for all the technologies: 90% of member households report having adopted the planting practices in at least one field, 57% produce compost, and 56% triage their grains according to the modern practices described in section 2.2.

The extent to which technologies diffuse beyond the seeds, however, is technology dependent. Post-harvest storage and composting techniques spread widely beyond NGO members: 58% report adopting antimold products and 28% modern composting, and 32% report triaging their harvest. This suggests that some members are willing to incur some costs (e.g., time, hassle) to teach others.[22] However, fewer than 10% of the nonmember households report adopting the planting practices or having knowledge of how to properly implement it according to optimal spacing rules. This is true even among agricultural casual laborers, who have strong incentives to acquire knowledge of the practices because of the wage premium that they would obtain from it in the labor market.[23]

Social network and diffusion. We document the presence of social capital among skilled and unskilled workers through a network survey in the 35 villages where we conducted the household census and find evidence inconsistent with the hypothesis that social capital may induce exchange of rivalrous information between the two groups. For example, more than half of the NGO members report friendships with nonmembers, and many engage in lending or borrowing with them, indicating participation in shared informal insurance networks. However, work-related social capital is weaker: Less than 30% of nonmembers exchange job referrals or discuss employer-related issues with members, suggesting that the unwilligness to share is specific to the employment domain.

[21]We conducted this census with the help of local authorities. Our field staff went house by house and spoke with either the household head or another adult. The survey comprises basic demographic questions about household members, technology adoption and labor supply.

[22]We verify that the adoption did not predate the NGO's arrival by comparing these adoption rates with those of the participants of the knowledge-sharing experiment (Table A3).

[23]Our focus on agricultural workers helps rule out that nonadoption simply reflects heterogeneity in on-farm returns (Griliches, 1957; Suri, 2011; Magnan et al., 2015), although the possibility of heterogeneity in the labor market returns to the practices remains open.

3 Experiment

3.1 Experimental design

We begin by describing the experimental variation that we introduce, then discuss the protocol of the training events, and conclude by summarizing the sampling and treatment randomization.

Conceptual overview. The ideal treatment would vary an individual's willingness to share information about a technology with another person, holding constant the identity of the receiver and all features of the technology *except* whether the returns to the sharer from her knowledge of the technology depend on the proportion of others who also know it. A key difference with respect to the prior literature studying diffusion of rivalrous information—which typically introduces rivalrousness in the form of a finite good created by the experimenter—is that, in our context, the rivalrousness arises from market incentives. Hence, to change only the returns to sharing, we would need to somehow guarantee the participants that demand for labor will be completely elastic in perpetuity to absorb the additional skilled labor supply. Given the size of the village labor markets in our context and the costs associated with doing so, this is, of course, not feasible. Instead, similarly to the approach of Cai and Szeidl (2018), our experiment looks at an incumbent's willingness to share knowledge of a technology with another person and varies the *identity* of the nonadopter and *type* of technology to test whether the incumbent hoards the knowledge from individuals perceived as competitive—but only knowledge of technologies whose diffusion would affect market returns.

Training event overview. We design *training events*, which we create as opportunities for the skilled workers to share—or not—their knowledge of technologies with unskilled laborers. We model these events on situations in which two workers might naturally interact—for instance, if they were to meet in someone's field or were working together for the same employer. The skilled and unskilled workers share social connections, and it is very common for the skilled workers to have unskilled workers as neighbors: At baseline, the skilled workers report being neighbors with a median number of 3 unskilled workers.

We also create the training events to offer the skilled laborers "wiggle room" to *avoid* training the unskilled workers if they wish. From focus group discussions, we found that skilled workers typically avoid giving training when requested to do so by employing two strategies: either i) by walking away from the interaction[24] or ii) by giving training briefly but obfuscating key parts of the technique.

In this way, we design our training events to shut down the possibility of sorting out while still allowing for moral hazard in training. Specifically, at the events, we pair skilled and unskilled

[24]For instance, telling the unskilled worker that they would return and train her later.

workers and provide the workers with a parcel of land (away from others) on which the training can be done. However, to allow the skilled workers the space for maneuver to avoid training their partner, we introduce the following conditions: i) All the training is unsupervised, ii) we provide games and other leisure activities that individuals can engage in, and iii) the experimental payments are conditional only on attendance, not on training (more on this below).

Treatments. In both experiments, we create opportunities for the incumbents who work in the local labor market and are skilled in row planting to share knowledge of a technology with unskilled individuals during a training event. We then cross-randomize two treatments, which provide different incentives to share (or withhold) information at the event:

(i) Provenance of event participants:

<u>Competitor training.</u> In this treatment, the incumbent and unskilled workers are individuals from the same village (labor market).
<u>Non-Competitor training.</u> In this treatment, the incumbent and unskilled workers come from different villages.

Interpretation. With the technology held fixed, sharing with an individual in the same village introduces a potential competitor in the labor market, whereas sharing with an individual operating in a different, distant labor market should not impact the incumbent's labor market returns. We predict that if incumbents are concerned that diffusion will lower their returns, there will be less sharing in the *Competitor villages* arm, where the unskilled operate in the same local labor market as the incumbent.

Of course, there are reasons why an individual might have a lower willingness to train an individual from her own village, regardless of the perceived effects on the labor market. Therefore, to account for the differences that training an unskilled worker from the same or a different village entails, we also cross-randomize the following treatments:

(ii) Nature of training:

<u>R–Row-planting technology.</u> In this experimental arm, at the training event, the incumbent is encouraged to train the unskilled in the planting practices.
<u>P–Placebo technology.</u> In this experimental arm, at the training event, the incumbent is encouraged to train the unskilled worker in placebo technologies (composting and storage techniques), which i) require explicit training and ii) yield private returns that do not depend negatively on others' adoption decisions.

Interpretation. We argue that for one technology, the planting practices, the rivalrousness of market returns is more salient. Working in row-planting generates a wage premium that workers perceive would diminish if there were more skilled workers in the village. By contrast, there is no active hiring market for composting or post-harvest storage tasks.

3.2 Two experiments

We design two experiments to answer the following questions: i) Do incumbents hoard their knowledge of new technologies with others when they perceive that broader diffusion of this knowledge would decrease their own returns? ii) Does an increase in knowledge diffusion when unskilled workers are exposed to incumbents who do not hoard knowledge have meaningful impacts on the village economy? In each experiment, we conduct training events as described above. The laborers at these events are assigned to one of the treatment conditions described above.

We conduct our experiments in 223 villages (sous-collines, in Burundi) in three Burundian provinces (Muramvya, Gitega, and Mwaro). We sample two types of villages and run one experiment in each (see Figure A.2a in the appendix).

Knowledge-sharing experiment: Documenting knowledge hoarding in new villages. The goal of this experiment is to document the extent of knowledge hoarding when we vary the incumbents' perceived returns from sharing. To do so, we sample laborers from 102 villages where the NGO started its operations in 2023 or later. This experimental feature ensures that both the planting practices and our placebo technologies have not yet diffused widely beyond the seeds.

We invite laborers from these villages to training events randomized into one of the four arms described in the previous section (section 3.1): *Competitor training–Planting practices*, *Non-Competitor training–Planting practices*, *Competitor training–Placebo*, *Non-Competitor training–Placebo*.

For incumbents, the assignment to training events is at the village level—i.e., at any given event, the incumbents all come from the same village and attend only one event. In contrast, the unskilled workers always come from villages where the incumbents are randomly assigned to the *T1-Same village* training events. This implies that the unskilled workers from *Competitor training* villages (where the skilled are assigned to train unskilled workers from their own village) attend two training events: one where they are paired with skilled workers from their own village and another where paired with skilled workers from a different village.[25] This element of the design ensures that the selection of unskilled workers is exactly the same. To minimize attrition, we hold the two training events on the same day and randomize whether planting practices is the first or second training.

These villages are not suitable for us to measure downstream outcomes of reductions in knowledge hoarding for two reasons. First, given the limited presence of the NGO in the villages, it is unclear

[25] The unskilled workers from *Competitor training* villages do not attend any event.

whether the level of knowledge of the focal technologies in the village is at steady state. Second, the NGO's ongoing expansion means that changes over time across the sample are likely to also reflect this expansion. Because of this unsuitability, we measure the equilibrium effects of reductions in knowledge hoarding in a second set of villages, in which the NGO has been present for longer and so diffusion of the planting practices should be closer to its steady state level.

Market effect experiment: Equilibrium effects of knowledge hoarding. We utilize a second set of villages where the NGO had been active for at least 3 years and had completed its expansion at the time of the experiment. These villages are plausibly close to their steady state in terms of diffusion of the technologies, and it is unlikely that further NGO expansion would drive substantially more diffusion.

This experiment deviates from the previous one in four key aspects. First, because of the NGO's relatively long-term presence in the sample villages, the placebo technologies that we study have already diffused widely in them (see Figure 1 and Table A3). Therefore, we assign villages to only one of two treatment arms: *Competitor training–Planting practices* and *Non-Competitor training–Planting practices*.

Second, we randomize some of the villages into a pure *Control* treatment. Therefore, the villages are assigned to one of three conditions in total: In *Control* villages, the unskilled do not receive any training; in *Competitor training* villages, the unskilled are invited to the events with incumbents (skilled workers) from the same village; and in *Competitor training* villages, the unskilled workers attend the events with skilled workers from a different village (a village in either the *Control* or the *Non-Competitor training* arm), where we randomize the provenance of the skilled workers.

Third, we expand the number of laborers who we invite to our training events. Specifically, to study equilibrium effects, we invite to the event a number of unskilled workers equal to one-third of the labor force. Finally, we use follow-up surveys to measure both the immediate and downstream effects on local economic activity of exposing the unskilled to incumbents who plausibly do (*Competitor–Planting practices*) and do not (*Non-Competitor–Planting practices*) have knowledge-hoarding motives.

3.3 Training events

Protocol. All the training events follow the same structure.

Sequence of events. First, the enumerators take attendance and introduce the participants to the event activities. Next, the participants take a short screening survey, after which they are assigned a small plot of land (separated from others) and begin the activities, unmonitored. The training portion lasts 3 hours for the knowledge-sharing experiment or 5 hours for the market effect experiment, at the end of which the skilled workers can leave. Throughout the training, the enumerators interact with the participants only when they take the baseline survey. The unskilled workers, instead, are surprised at the end of the event with an incentivized quiz testing their learning of the technology trained at the event. They also take a short survey about their interaction with their paired skilled worker during training.

Forming the training pairs. Surveyors have a list of event participants randomly ordered. We pair individuals with the same order number from each list, with two exceptions. If an individual in the pair is not eligible or is absent, we pair the other with one individual from a "buffer list." The latter are invited to the event and receive compensation but do not participate in the training unless they are needed as a substitute for an ineligible or absent participant. If fewer skilled workers than expected are eligible, we assign two unskilled workers to one skilled worker.

Event logistics. To ensure that there is no differential selection based on distance from the events, all training events take place in a village outside those in the study, and transportation costs are covered.
Participants learn about their treatment status—i.e., the nature of the training and whether they are paired with someone from their own or a different village—only when they arrive at the event location. The material necessary for the training (e.g., hoes, ropes, storage bags, material to produce composting bins) is provided.

Screening. At the beginning of the event, we conduct a short survey to confirm that i) the individuals are active in the labor market as casual laborers; ii) skilled workers are knowledgeable of the row-planting technique, while unskilled workers are not, iii) participants do not belong to the same household.[26] No screening occurs for the spillover sample.

Creating "wiggle room." Crucially, several aspects of the design mimic natural interactions that would occur outside the experimental setting and ensure skilled workers have some room for maneuver to avoid completing the training. First, in addition to training material, participants are provided with some leisure activities, such as games and material to weave baskets. This guarantees that the skilled workers do not provide training out of boredom and gives them an excuse to avoid training. Second, to minimize the risk that the skilled workers feel compelled to train, the participants engage with the enumerators during the event only to complete the surveys. Finally, the participants' compensation is conditional only on their taking the surveys, not completing the training.

Alternative to events. In the market effect experiment, the unskilled workers from the *Control* group do not attend any training events. Likewise, skilled workers from either the *Control* or *Non-Competitor* villages not randomly assigned to training do not attend any training events. However, to avoid differential selection based on event attendance, they are also invited in a different village,

[26]Likewise, for *Control* unskilled workers and skilled workers not randomized to training in the main sample, the screening occurs before the beginning of the surveys.

with the only difference that they only take surveys during that time.

3.4 Sampling

Village selection. We started by compiling a list of villages suitable for the experiments. We used NGO administrative data to compile a list of villages where the NGO operates in the provinces of Muramvya, Gitega, and Mwaro. We then screened out villages that were unreachable by vehicle during the planting season, villages where the NGO had fewer than 20 laborers active in the labor market, and villages where beans were not the major crop planted during the season B. We also screened out villages where the share of labor force that was skilled (i.e., that had mastered the row-planting practices) was less than 10% or more than 65%.

Finally, for the knowledge-sharing experiment, we retained only villages where the NGO had begun operations in 2023 or later, to ensure that the placebo technologies (composting and post-harvest storage techniques) were relatively new. In contrast, we restricted the sample in the market effect experiment to villages where the NGO had started its operations before 2023. We group villages according to geographical proximity and randomize the treatment status stratified by these geographical clusters.

Village-level randomization. We randomize our treatments at the village level so that all the individuals from the skilled (unskilled) sample in the same village have the same treatment assignment. We stratify the treatment assignment based on geographical clusters. This ensures the feasibility of the *Competitor village* training events, where the skilled and unskilled workers come from different villages.

In the knowledge-sharing experiment, the geographical clusters comprise an even number of villages, which ensures that the unskilled workers in each *Competitor training* village can be randomly matched with the skilled workers in a *Competitor training* village. As discussed in Section 3.1, the unskilled workers from *Competitor training* villages receive two trainings, one on the planting practices and one on the placebo practices. The randomization determines which group of skilled workers—from the same or a different village—provides which training. Finally, we randomize which training is performed first.

Creating the worker sampling frame. Our main sample comprises casual agricultural workers who are active in the spot labor market. We rely on local administrators (chiefs) and local NGO officers to compile a list of the village labor force. We classify a worker as skilled if she has mastered the row-planting technique and is regularly hired to implement it for employers and as unskilled if she regularly works for employers during the agricultural season but has not mastered the practice. We validate our lists with the help of a sample of village employers. We randomly sample the participants for our experiments from these lists, according to the protocol described below.

Participant sampling. In each village, we randomly order the lists of skilled and unskilled workers. In the knowledge-sharing experiment, we select the first 10–15 individuals (depending on village size) from each list, who are then recruited by field officers.

For the market effect experiment, we proceed in two steps. First, in each village, we randomly assign each individual on the lists to either the "main sample" of potential event invitees or the "spillover sample." Then, we randomly select potential event participants. This procedure guarantees that we have comparable samples in each treatment arm. In each village, we also sample 15 skilled and 15 unskilled workers from the spillover sample, with whom we conduct only surveys (see Figure A.2b).

4 Data & Empirical Strategy

4.1 Timeline

Our experiment follows farmers over the course Burundian agricultural "Season B"—one of the two main agricultural seasons—, during which farmers prepare, plant, weed and harvest their fields (see Appendix Figure A.1 for a timeline).

This season runs from February to July—with most of the planting activities concentrated early in the season. The training events for the "Market Effect experiment" take place during December 2023 and January 2024. Instead, the training events for the "Knowledge-sharing experiment" take place in August 2024. We measure hiring of daily laborers, adoption and agricultural employment in a first visit between April and June 2024. Finally, in September and October 2024 we conduct a second survey with unskilled workers in the main sample to measure harvest outcomes.

4.2 Data Collection

We collect data using surveys and through practical tests administered through enumerators. Survey data is collected during a baseline survey at the training events and through surveys over the course of the agricultural year. We also administer practical tests to measure competence in the agricultural technologies that are demonstrated at the training events once the training events finish.

Baseline variables During the training event, enumerators administer a baseline survey for a subset of participants. The baseline survey elicits individual and household demographic, agricultural labor market experience recalls, including an employer-level roster for agricultural worker in the previous agricultural season, as well as farm information. For skilled workers, we also elicit hypothetical questions about training other individuals in rowplanting. For the sample who are not assigned to the training and spillover sample workers are also invited to take these surveys in a separate location.

Learning outcomes At the end of the training event, the unskilled workers undertake an incentivized test on the topic of the training. We also survey the unskilled workers about their training, including the time they spent learning the technique with the skilled worker as well as qualitative descriptions of how the incumbent and novice interacted at the event.

Labor market outcomes. Several months after the training events, during the planting season, we conduct surveys with both employers and workers to measure labor market and farming activities. To construct measures of hiring, we ask each farmer whether they hired workers, and then ask for each worker hired i) the days that the worker worked, ii) the tasks completed in these days and the days spent completing the row planting or fertilizer microdosage during those days and, iii) payments made. In addition, questions about days worked and tasks completed were also asked for each family member who worked during the planting season. To measure employment, we ask each sampled worker the number of employers for which they worked during the planting season. We then collect data for this roster of employers including, i) the number of days they worked for this employer, ii) the payment received and iii) the tasks completed, iv) of the total number of days worked, the total days that the worker was hired to do row-planting or microdosage, among other information. Finally, workers were also asked whether they did any other work during Season B, and total earnings from such work.

Planting outcomes. During the same survey, we measure adoption of rowplanting as well as other outcomes related to farmer's planting decisions. Prior to the beginning of the survey, enumerators demonstrate to the respondent what correct rowplanting entails, and are told that one of their plots may be chosen at random for verification.[27] We then conduct a plot roster where, for each plot, we ask farmers i) the crops they planted; and, for plots planted with beans, i) whether it is planted using row planting or broadcasting, ii) whether microdosage was adopted; and iii) the area of the plot and on which proportion of the field rowplanting and microdosage were adopted.

Harvest data. Several months after harvesting is completed, we conduct a final survey to measure harvest outcomes. For budgetary reasons, we decided to survey only unskilled workers from the main sample. This is the sample in which we expect any change in harvest outcomes. The survey consists in a crop roster. For each crop the farmer may have planted, we ask each farmer the quantities harvested and its price. We construct crop revenues by multiplying the quantities of crops harvested by the price of the crop at the nearest market.[28] To measure profits, we also elicit

[27] We decided to implement the verification with small probability because, in a companion paper, (Cefalà et al., 2024) finds that the self-reported and verified data are very similar.

[28] Because crops come in different varieties with different prices, we multiply the quantity harvested by the farmer's

the amount of money the worker spent on labor and non-labor inputs, and subtract these from the revenues.

4.3 Summary Statistics

Knowledge-sharing experiment Appendix Tables A6 and A7 provide summary statistics for skilled and unskilled workers attending the Knowledge Sharing experiment. Balance in the sample appears reasonable with 1 of 19 tests with $p < 0.05$ in Appendix Table A6 and 1 of 18 in Appendix Table A7. The average age of skilled laborers is 40, with 35% being male.

Both skilled and unskilled laborers are active in the labor market, working 15.7 and 14.2 days in total during the agricultural season. Skilled laborers earn FBU 40,000 (around USD 13) from this work, which is the bulk of household earnings from agricultural laborer.

Skilled workers are positively economically selected from the labor force. Compared to unskilled laborers, skilled laborers have on average 20% more land, have three times the amount of savings (22,000 FBU versus 8,000 FBU). Skilled laborers also have higher yields than unskilled workers, potentially due to an increased likelihood of adopting row planting on their fields. Finally, consistent with the motivating evidence presented earlier, the average wage among skilled laborers in the sample is 5% higher than that of unskilled laborers.

Market Effect experiment Table A5 presents tests for balance for the Market Effect experiment for the skilled and unskilled laborer samples, and provides similar descriptives as the previous table. The sample appears to be reasonably balanced, with 2 of 32 tests having p< 0.05.

Skilled and unskilled workers work meaningful numbers of days in the agricultural labor market (around 14 on average) with skilled workers doing a large share of row-planting work (around 50%). Skilled workers in these villages also earn meaningfully higher wages (FBU 2900 per day as opposed to 2400 per day). Similarly to the Knowledge Sharing experiment, skilled workers are positively selected on landholdings (14 versus 7 ares) and savings (24,000 versus 9,000 FBU).

4.4 Empirical Strategy

Our primary specification to estimate treatment effects in the Knowledge-sharing experiment is:

$$y_i = \alpha + \beta_1 \text{Rowp}_{v(i)} + \beta_2 \text{Non-competitor}_{v(i)} + \beta_3 \text{Non-competitor}_{v(i)} \times \text{Rowp}_{v(i)} + \gamma_r + \epsilon_i \qquad (1)$$

estimate of the price of that variety at the nearest market. We also show robustness to using the median price of respondents in the same area.

in this specification $\text{Row}_{v(i)}$ is an indicator variable for whether skilled laborer i in village v is assigned to a training event where row-planting is the technology that skilled laborers are encouraged to train, and $\text{Non-competitor}_{v(i)}$ is an indicator variable for whether individual i in village v is assigned to training event where unskilled laborers came from a different village, and γ_r are cluster fixed effects. Therefore, β_3 is our coefficient of interest which measures whether skilled laborers train differentially unskilled laborers from a different village when the technology that the skilled laborer is asked to train is row-planting.

We are also interested in the heterogeneous treatment effect by incumbents of unskilled characteristics. In this case, we augment Equation 1 and run:

$$\begin{aligned} y_i \;=\; & \alpha + \beta_1 \text{Rowp}_{v(i)} + \beta_2 \text{Non-competitor}_{v(i)} + \beta_3 \text{Non-competitor}_{v(i)} \times \text{Rowp}_{v(i)} \\ & + \beta_4 \text{Non-competitor}_{v(i)} \times (\text{Het}_{v(i)} = 1) \\ & + \beta_5 \text{Non-competitor}_{v(i)} \times \text{Rowp}_{v(i)} \times I(\text{Het}_{v(i)} = 1) + I(\text{Het}_{v(i)} = 1) + \gamma_r + \epsilon_i \end{aligned} \tag{2}$$

where $I(\text{Het}_{v(i)} = 1)$ is a dummy equal to 1 if individual i has that dimension of heterogeneity. We are interested in β_5, the coefficient on the triple-interaction $\text{Non-competitor}_{v(i)} \times \text{Rowp}_{v(i)} \times I(\text{Het}_{v(i)} = 1)$, which tells us whether the individuals with the dimension of heterogeneity $\text{Het}_{v(i)}$ have a differential treatment effect from being trained in Rowplanting from an incumbent from a different village.

In the Market Effect experiment, we run the following regression by subsample:

$$y_i^g = \alpha + \beta_1 \text{Same}_{v(i)}^g + \beta_2 \text{Non-competitor}_{v(i)}^g + X_i + \gamma_r + \epsilon_i \tag{3}$$

where g indicates the subsample the individual i belongs to (Skilled or Unskilled, Main or Spillover), $Same_{v(i)}$ is an indicator variable equal to one if unskilled laborers in the village were assigned skilled workers from the same village at a training event, and $Diff_{v(i)}$ is an indicator variable equal to one if unskilled laborers in the village were assigned skilled workers from a different village at the training event. X_i is a vector of individual baseline characteristics, and γ_r are geographical strata fixed effect.

In this regression, we test whether $\beta_i = 0, i \in \{1, 2\}$, *i.e.*, if they are statistically different from the Control; as well as, $\beta_1 = \beta_2$, *i.e.* whether outcomes for laborers (skiled and unskilled) who live in villages that where unskilled laborers were exposed to skilled laborers at the training event from the same village are the same as outcomes for unskilled laborers exposed to skilled laborer who were from a different village.

Furthermore, to obtain outcomes representative at the level of the labor force, we run Equation 3 pooling all the subsample, using inverse probability weights to account for their sampling probability.

Finally, we are also interested in the equilibrium wage for the rowplanting task. To do this, we run, we define the dummy $rowplant_{ij} = 1$ if i does rowplanting task for employer j. Then, we define x_{ij} the vector of T task dummies other than rowplanting, where x_{ij} is a dummy equal to 1 if i performed task x for employer j. We run:

$$\begin{aligned} w_{ij} = {} & \gamma_1 \text{Same} \times \text{Rowp}_{ij} + \gamma_2 \text{Non-competitor} \times \text{Rowp}_{ij} + \text{Rowp}_{ij} + \\ & \sum_{t=1}^{T} \text{Same} \times x_{ij} + \sum_{t=1}^{T} \text{Non-competitor} \times x_{ij} + \sum_{t=1}^{T} x_{ij} + \epsilon_{ij} \end{aligned} \tag{4}$$

where w_{ij} is the average daily wage received by i when they work for employer j, and each individual is weighted by population shares. We are interested in $\gamma_1 = \gamma_2$, *i.e.* if the wage for the rowplanting task is different in the Different village from the Same village arm.

5 Results

5.1 When do incumbents hoard knowledge?

In the knowledge sharing experiment, we test whether incumbents withhold information from the unskilled when the latter are perceived as potential competitors, which depends on both the technology being shared and the identity of the individual.

Figure 2a shows the treatment effect of an unskilled worker's being paired with a skilled worker from the same or a different village on her likelihood of being trained at the event in both technologies. We classify an individual as having been trained if she receives a score of at least 60% on the incentivized quiz provided at the end of the event, a measure that we preregistered. The first two bars show the likelihood of the unskilled worker's being trained in row planting: Unskilled workers trained by oncompetitor incumbents are 38.2 p.p. likelier to become trained—relative to almost no unskilled workers being trained in the *Competitor training* events (in total, 3% of the unskilled workers in the *Competitor training* treatment are trained; see also Table A9 for the regressions).

It is possible that this unwillingness to share row-planting technology with the unskilled in one's own village reflects an idiosyncratic cost associated with training that individual, even though typically we might expect such effects to drive *more* sharing, as opposed to less, when a skilled worker trains someone from the same village. We therefore test whether we observe the same pattern of knowledge transfer with the placebo technologies. At the events where sharing of the placebo technologies is encouraged, however, we find dramatically different results: Both the *Competitor* and *Noncompetitor* unskilled workers have high learning rates—as measured by their likelihood of passing the quiz (96%, $p = .57$).

What drives this difference in learning outcomes? One possibility is that, in the *Noncompetitor* training, the incumbents spent more time training the unskilled workers regardless of the technology but that this leads to a difference only in the row-planting training because of ceiling effects in learning the placebo technologies. However, Figure 2b, which shows the cumulative distribution function (CDF) for the quiz score in the placebo technology, suggests that there is still a substantial fraction of individuals who do not fully learn. Furthermore, regardless of the threshold that we choose, there is no difference in quiz scores between unskilled workers trained by competitor or noncompetitor incumbents (i.e., residing in the same or a different village).[29]

Second, while the incumbents spent more time training the noncompetitor unskilled workers, this effect appears to be too small in magnitude to drive the large differences in training outcomes that we observe. Figure 3a, which plots the average amount of time that the unskilled workers reported having been trained, shows that in the *Noncompetitor training* treatment, skilled workers spend on average 28 more minutes training unskilled workers in row planting ($p < 0.001$) than they do in the *Competitor* treatment. However, this increase is over a baseline of 2.5 hours, making it unlikely that this 19% increase in training time leads to such dramatically different training outcomes.[30]

Instead, Figure 3b suggests that a major difference between workers in the *Competitor training* and *Noncompetitor training* treatments is the *quality* of the row-planting training. While the incumbents are as likely to verbally explain the practices (first set of bars, p-value of differences = .503) regardless of the provenance of the unskilled worker, the incumbents in the *Noncompetitor* training (i.e., residing in a different village from the unskilled) were 67% likelier to provide feedback and correct the unskilled workers ($p < .001$). These results are consistent with the idea that learning how to row plant involves procedural learning, which occurs through repetition and practice (see Section 2.2). Furthermore, we see that *Competitor training* skilled workers convey theoretical knowledge about row planting: Over 90% of the unskilled can correctly state the optimal spacing between seedbeds in both arms. However, we find evidence that skilled workers obfuscate key aspects of the row-planting technique that are required to plant well. For instance, when workers are first taught how to row plant, they are taught to use string to trace rows across multiple lines to ensure that they are set up in equally spaced intervals. Over time, however, skilled workers can do this process by eye and do not have to use the string line by line. Strikingly, we find that incumbents training a competitor are *more* likely to omit this detail of how to trace lines correctly using string (Figure 3b, last column sets on the right). This confirms once more that

[29]Instead, Figure 2c shows that the distribution of the row-planting quiz scores for *Noncompetitor* unskilled workers first-order stochastically dominates the score distribution of the *Competitor* unskilled workers (those residing in the same village as the incumbents).

[30]For the placebo technologies, the skilled workers in the *Noncompetitor training* treatment spend slightly *less* time training in the placebo technology (12 minutes, $p = .073$; see Column (1) in Table A9).

skilled workers can successfully obfuscate some aspects of their training in row planting.

Heterogeneity analysis. Next, we ask whether individuals who have higher stakes in the labor market are likelier to hoard. Table 1 shows the results from the specification in Equation 2 on the likelihood that the unskilled individuals pass the quiz, where we restrict the sample to the training in row planting (i.e., $\text{Rowp}_{v(i)} = 1$, although we refer the reader to Panel B in Appendix Table A11 for the results for the full sample).

We leverage the fact that the skilled and unskilled workers are randomly matched at the events to look at whether, within the same village treatment, skilled workers who stand to lose more from training are less likely to train. We find evidence consistent with this hypothesis. Unskilled workers paired with a competitor (i.e., an incumbent from the same village) who has less cultivable land (Column (2)), who derives more earnings from the labor market (Column (3)), and who does more row-planting work in the labor market (Column (4)) consistently receive less training. Unskilled workers paired with a skilled worker who has below-median holdings of farm land (i.e., is likelier to work as a laborer) are 14.4 p.p. less likely to be trained. Similarly, unskilled workers paired with skilled workers with above-median earnings are 16.7 p.p. less likely to be trained than those paired with skilled workers with below-median earnings (p=0.040), with similar magnitudes emerging if we split the sample by whether the skilled worker has an above-median number of days spent in row-planting work ($p = .004$). These results hold, albeit with more noise, when we compare this differential treatment effect in row planting with the placebo training (Columns (2)–(4) in Appendix Table A11). Furthermore, incumbents seem to better train individuals who are less likely to work in the labor market, as proxied by the amount of land that they own (Column (7)) or by demographics (Column (8)): When the *Competitor* unskilled worker has above-median holdings of farm land, she is 13 p.p. likelier to be trained (p=.018), and elderly women are 23.5 p.p. likelier to learn how to row plant (Column (8), p=0.001).

Taken together, our results document that incumbents hoard knowledge when sharing it may prove consequential for their market returns. Specifically, workers i) withhold information from the unskilled, ii) they do this only for information that could erode market returns, and iii) the willingness to share information is lower among skilled workers who plausibly stand to lose more from such sharing. In the next section, we turn to the question of whether this hesitancy to share meaningfully changes local economic outcomes.

5.2 Unskilled workers' gains from less knowledge hoarding

In section 5.1, we find that when unskilled workers are paired with skilled workers from the same local labor market (*Competitor training* arm), they are *less* likely to be trained in row planting than when they are paired with skilled workers from a different local labor market (*Noncompetitor* *training* arm). We turn to the market effect experiment to test whether removing the knowledge-hoarding motive is consequential for local economic activity and to measure the incidence of these changes on the unskilled and incumbents.

First stage: Training results. We start by showing the results for our first stage: Specifically, we test whether unskilled workers learn differentially when they reside in the the incumbent's same or a different village. The results from our first stage qualitatively match our results in the knowledge sharing experiment. Appendix Figure A.3a shows the likelihood that unskilled workers receive training by treatment status. While 5% of the workers who attend the event with skilled workers from the same village are trained, this share increases to 38.3% for workers who attend with skilled workers from a different village. As we can see from Column (4) of Appendix Table A9, this result is remarkably similar to what we find in the knowledge sharing experiment. However, the treatment effect on the time spent learning row planting is larger in magnitude, reflecting the longer window we provided for the training in the outcome experiment events (see section 3.3). We also find evidence that the worse training outcomes result from moral hazard in training. Unskilled workers in the *Noncompetitor training* arm are 74% and 57% likelier to report that the skilled workers observed them multiple times while practicing and corrected their mistakes.

Labor market gains. Panel A in Table 2 shows the treatment effects on the labor market outcomes of the unskilled workers invited to the training event. The training event significantly increases the labor market earnings (during the agricultural season) of the unskilled workers assigned to the *Noncompetitor training* treatment. On average, during the agricultural season, they earn 7.9% more than the control group (Column (1), $p = .017$), an amount corresponding to 1.2 times the average daily wage for an unskilled worker. In contrast, the *Competitor training* unskilled workers experience a much more modest and not statistically significant increase in earnings ($p = .663$). The effect for the unskilled workers in *Noncompetitor training* villages is also significantly larger than the *Competitor training* treatment effect on earnings (+6%, $p = .035$).

The treatment effect on labor market earnings for *Noncompetitor training* unskilled laborers does not reflect a change in total days worked: Column (4) shows no significant change in the total days worked in agriculture between the unskilled in both treatments and unskilled in the control group ($p = 0.454$ and $p = 0.429$, respectively). Instead, the treatment effect on agricultural earnings for unskilled workers in the *Noncompetitor training* treatment reflects substitution toward row planting and complementary tasks and away from other tasks during the agricultural season. Unskilled workers in the *Noncompetitor training* arm work 1.3 more days in row planting (Column (3)) than unskilled workers in the control group—an increase twice as large as that in *Competitor training* villages (p-value of the difference $< .001$).

Consistent with the training in row planting yielding labor market returns, the wages of unskilled workers in *Noncompetitor training* villages rise in comparison to those of the unskilled in control villages. Column (2) shows that workers in *Noncompetitor training* villages earned an average daily wage 5.2% larger than that of control workers (p = .001), in comparison to the 3.2% increase in *Competitor training* villages (p = .036), although these two effects are not statistically distinguishable from one another at conventional levels of significance (p = .248).

Importantly, these returns might compound if they persist over many seasons. Consistent with this possibility, *Noncompetitor training* workers became more optimistic about their future employment prospects: They expect to work 8% more days than control workers expect to work (Column (5), p = .001). This is not the case for *Competitor training* workers, whose expected number of days of work does not differ from the expectation in the control group (p = .749), suggesting that *Competitor training* workers may realize that their knowledge is still inadequate.

Technology adoption on trainees' own farms In the previous section, we tested whether the unskilled gained labor market returns from learning row planting. In addition, learning row planting might generate own-farm returns if it changes how the unskilled plant their own farms and increases farm profitability. Table 4 shows that the unskilled also change their own-farm labor and planting decisions after attending the training event. The treatment causes 72.9% of trained unskilled workers in *Noncompetitor training* villages to adopt row planting on at least one of their plots (Column (5)), an effect 40.7% larger than that in *Competitor training* villages.[31]

5.3 Costs to incumbents from less knowledge hoarding

Our motivating evidence suggests that incumbents perceive a cost from sharing information about new technologies with the unskilled. Table 2 tests whether incumbents residing in villages where the unskilled are exposed to training do indeed incur a cost from the greater information diffusion.

Negative labor market effects. Panel B of Table 2 shows the labor market outcomes for incumbents living in control, *T1-Competitor training* and *Noncompetitor training* villages (i.e., villages where the unskilled did not attend a training event, villages where they attended an event with skilled workers residing in their same village, and villages where they attended an event with skilled workers from a different village), pooling the main and spillover samples.

We find evidence that the increased skilled labor supply in *Noncompetitor training* villages causes a meaningful earnings decrease for skilled workers residing in those villages. Column (1) shows that, on average, they earn 6% less than the control group (p = .014)—which is a significant reduction with respect to incumbents residing in *Competitor training* villages, as well (p = .041). *Noncompetitor training* skilled workers work on average 5.4% fewer days in the agricultural labor market

[31]The fraction of treated unskilled workers who adopt row planting in the control group is negligible.

than do the control skilled workers ($p = .036$). This negative treatment reflects for *Noncompetitor training* incumbents reflect a decrease in both days of work in row planting and their average daily wage. Column (3) shows that they experience a 13.2% decrease in the days of work in row planting performed for their employers in comparison to the control group ($p < .001$). The effect in *Competitor training* villages, instead, is much smaller in magnitude: Skilled workers work on average 6.9% fewer days in row planting than the control group ($p = .073$)—an effect statistically different from that on *Noncompetitor training* skilled workers ($p = .045$).

Finally, it is important to note that the *Noncompetitor training* skilled workers' earnings loss likely reflects a short-term effect. In fact, it is plausible that labor demand in this setting adjusts with a delay to the increase in the supply of skilled labor. This is because employers make their production decisions—such as the purchase of inputs and land rental—several months before the start of the planting season, whereas they might learn about the extent of workers' skills only shortly before the time of planting.

5.4 Aggregate labor market effects

Table 3 aggregates our findings for all laborer samples to document how our treatments change the local labor market equilibrium. Columns (1)–(3) focus on changes to the labor market for row planting. The total days worked in row planting per laborer increase by 0.24 in *Noncompetitor training* villages, an increase of 13% over the number of days worked in pure control villages ($p <0.001$), whereas the days worked in *T1-Competitor* villages increase by only 0.10 ($p = 0.112$, test of difference between treatments $p = 0.036$). This change in employment leads to increases in the number of fields that laborers report having row planted for employers, which increases by 0.29 and 0.13 in *T2-Noncompetitor* and *T1-Competitor* villages, respectively, corresponding to increases of 25% ($p < 0.001$) and 11% ($p < 0.001$), respectively (test of difference between treatments $p < 0.01$).[32]

Changes to the stock of skilled labor lead to lower wages for the row-planting task in the labor market. Wages decrease by 3% in *T2* villages (Column (3), $p = 0.068$) and negligibly in *T1* villages ($p = 0.953$).

Aggregating our effects to all agricultural tasks, we find noisy and insignificant effects on the number of days worked and earnings for laborers overall (Columns (4) and (5)).

[32]To construct this measure, we ask the respondents, for each employer who hired them, the number of plots on which they worked and the number of these plots where they performed the row-planting tasks. To avoid double counting, we also ask about the total number of workers who worked on each plot and assign an equal fraction to each worker.

5.5 Alternate explanations & robustness

In this section, we consider whether the results described in section 5.1 can be explained by alternative hypotheses. However, we first note that any alternative explanations must explain why skilled workers appear to be more willing to train an individual from a different village to row plant than they are one from their own village but are equally likely to train individuals from either type of village in our placebo techniques.

Pre-existing knowledge of placebo technologies. One concern is that we cannot detect a treatment effect in the placebo techniques because i) the unskilled workers already mastered the techniques and no further learning could happen or ii) the skilled workers were not knowledgeable. The first concern is mitigated by design because we run these tests in villages where the NGO has only recently expanded, so it is unlikely that the relevant knowledge has already diffused widely—as we document in Table A3. The same table also shows high rates of adoption of the placebo techniques among skilled workers at the event, which mitigates the latter concern.

Furthermore, we asked skilled and unskilled workers some knowledge questions related to placebo techniques at the beginning of the training event.

Reputational consequences. Chandrasekhar et al. (2022) suggest that the potential for negative reputational consequences may prevent some individuals from sharing information about a technology. In our context, some skilled workers may fear that their reputation as someone who has mastered the technology could be damaged if they fail to effectively teach row planting at the training event. This concern may be more pronounced for row planting than for the placebo techniques because it may hinder their future employment opportunities. Which sign we might predict for the treatment effect on these individuals is ambiguous: They may *increase* their effort to make a good impression on the trainee, or they may abstain from training at all.

To understand whether this issue is a cause of concern, we ask skilled workers at baseline whether they think there would be reputational consequences if someone did a poor job training another individual in the village. We then use the answers to this question to run an heterogeneity analysis by whether the respondents express such a concern. Column (3) in Table 1 shows that unskilled workers paired with incumbents who express more concerns about their reputation are *more* likely to be trained if they are in the *Competitor training* arm than in *T2*, suggesting that, if anything, the effect of skilled workers' reputational concerns would run counter to that of their concerns over the rivalrous character of the returns to row-planting skills. Moreover, we observe no heterogeneity in the effects by self-reported training ability ($p = .302$, Column (9)).

Greater returns from training of outsiders. Skilled workers might perceive the returns from sharing knowledge with an unskilled worker from another village to be greater because sharing with an outsider may open up new trading opportunities. To evaluate this conjecture, in the planting survey (approximately four months after the training events), we ask skilled workers about their trading outside their village. First, we document that there is little evidence of any trading across villages. Second, we do not find that incumbents paired with unskilled workers from a different village are any likelier to have initiated such efforts.

6 Discussion: Costs of Sharing Knowledge and External Validity

Section 5 shows that an increase in the supply of skilled laborers, which we generate by inducing experimental interactions between unskilled workers and skilled workers who lack knowledge-hoarding motives, increases overall agricultural productivity. However, while the gains from this diffusion accrue to new adopters, they come at some cost for incumbents.

The losses we estimate, however, result from a shift of the economy from one equilbrium to another, and therefore they do not necessarily approximate the losses that would be incurred if a single unskilled worker were trained. In particular, in a standard model of labor markets where workers are atomistic, the pecuniary externality from training a single additional unskilled worker is presumably negligible. Moreover, prior studies indicate that there might be reasons why individuals *do* want to actively share information with others—for instance, they may be altruistic or expect reciprocation, or they may feel pressure from kin. The potential for such transactions, in turn, raises the question of why the low-sharing equilibrium does not unravel.

In this section, we provide evidence that coordinated actions by skilled workers in the focal communities limit the likelihood of transmission of information with rivalrous returns to unskilled workers. Specifically, to raise the cost of sharing information with unskilled workers, skilled workers threaten to sanction other skilled workers who train unskilled workers who would subsequently compete with other skilled workers for jobs. In addition, to limit unskilled workers' demand for training, skilled workers spread misinformation about the costs and benefits associated with obtaining training.

Social sanction. We find evidence that nonpecuniary costs—in this case, social sanctions that skilled workers impose on other skilled workers who train individuals who compete with them for jobs—increase the cost of sharing and contribute to the limited-sharing equilibrium we observe.

These norms appear to have emerged because, while a skilled worker's training of an unskilled worker might not decrease the earnings of the worker who provides the training, it *is* likely to decrease the earnings of a skilled worker who is *connected* to that unskilled worker. To show this, we look at heterogeneity in the effects on skilled workers' earnings by whether the skilled worker worked for the same employers as the unskilled worker at baseline. To do this, we regress our outcome variables for skilled workers on treatment dummies, interacted with dummies for whether the skilled worker shared a common employer with an unskilled worker at the event. To allay concerns about the endogeneity of connectedness, we use the fact that workers were randomly sampled for the event to construct an instrument for expected connectedness using the method of (Borusyak and Hull, 2023).

We find that the earnings losses for skilled workers connected to the unskilled workers who received training are more sizable than the losses for those not connected to the newly trained unskilled workers. Even though the results are not statistically significant at conventional levels, the coefficients are large. This suggests that while training an unskilled laborer might not generate a meaningful pecuniary cost for the trainer, it would produce a meaningful pecuniary cost for a linked skilled worker.

This pecuniary cost that skilled workers anticipate will be borne by their own network from their training of the unskilled is exacerbated by the fact that, on average, skilled workers expect that once information escapes, it is likely to spread rapidly throughout the unskilled worker network, with consequences for their own and others' earnings. We elicit the incumbents' beliefs in the baseline survey in the market effect experiment about what would happen to their earnings if an additional laborer working for their employers learned the row-planting techniques. We find that only 5% of the skilled workers believe that their earnings would decrease as a consequence of an additional worker's being trained. However, 78% of the skilled workers agree that training another individual would result in much greater diffusion of knowledge to other unskilled individuals. They also believe greater diffusion would have meaningful consequences for their earnings: Over 60% believe they would see their earnings reduced if the number of skilled workers in the village increased by a third, with the average earnings loss estimated at 20% of the current earnings. These costs appear to be internalized because skilled workers appear to have the strongest social ties with other skilled workers.

This fear of meaningful costs to the in-group from information leakage, coupled with strong social ties among the group, appears to lead the group to a set of norms and sanctions designed to limit transmission to potential competitors. Consistent with recent work on collusive norms in low-income settings (Breza et al., 2019; Banerjee et al., 2022),[33] we hypothesize that the implicit coordination among skilled workers to stop the diffusion of knowledge of the planting practices beyond those initially seeded may contribute to the low-sharing equilibrium.

We provide evidence consistent with this hypothesis through several exercises. First, we exploit

[33] Banerjee et al. (2022) find evidence consistent with the existence of anticompetitive norms among Indian vegetable sellers. In the context of rural labor markets, Breza et al. (2019) show the existence of a social norm against undercutting wages that affects individual labor supply choices.

random matching of workers at the training event to test whether skilled workers train unskilled workers less if the latter are connected to those skilled workers' employers. To allay concerns about the endogeneity of connections, we use the random matching at the event to construct the expected number of connections at many simulated events and use this to instrument for whether the skilled worker is actually connected (Borusyak and Hull, 2023). Appendix Table A14 shows the results of this regression. We find that, across all dimensions of training (time spent by skilled worker, unskilled worker's learning, skilled worker's likelihood of providing feedback), the two workers' sharing of a common employer *decreases* the likelihood that the unskilled worker is trained (Column (2)) and the incumbent's effort (Columns (4) and (5)). However, the treatment effect is still large and negative, suggesting that fear of immediate pecuniary externalities is not the main concern.

To corroborate this finding, we collect supplementary data with 347 skilled workers from the market effect experiment villages. When asked whether others in the village would find it acceptable if a skilled worker trained an unskilled individual, less than 30% of the respondents agree (left-most column in Figure 4a). Furthermore, almost 80% state that they would expect some form of social sanction from other incumbents if they found out (Figure 4b). Such sanctions range from the work to the social domain: The most common form of sanction mentioned is exclusion from future work opportunities (37%) and badmouthing (to either employers or friends). Overall, these responses suggest that the expectation of social sanction appears to be a strong deterrent to knowledge-sharing.

Curbing demand through misinformation. The prior section suggests that coordinated action among skilled workers limits the supply of training by means of threats of additional, nonpecuniary costs if skilled workers provide training. However, skilled workers residing in the same village as untrained workers do not have a monopoly on the information with rivalrous returns: Individuals outside this network also know how to row plant. Given the high returns to this knowledge in the labor market, unskilled workers could seek the information outside this local network.

We document an additional strategy that incumbents adopt to curb the demand for learning that contributes to this low-learning equilibrium. Specifically, incumbent skilled workers may strategically hinder knowledge diffusion, for instance, by inflating the costs associated with learning to row plant when interacting with unskilled workers. To show this, we organize enumerator-led focus groups in which skilled workers are invited to discuss several aspects of modern agricultural practices, including how much time it takes to learn to row plant. In half of the focus groups, chosen at random, we also invite unskilled workers to participate.

Figure 6 shows that, when unskilled workers are not present, skilled workers state that learning the planting practices takes approximately 3 days, a figure somewhat close to what we observe in practice. Strikingly, when unskilled workers are present, skilled workers state that it takes more than *twice* as long to learn the practices as what they state when the unskilled workers are absent. This dramatic difference suggests that skilled workers intentionally attempt to curb unskilled workers' demand for learning the skills.

Unraveling in our experiment. While it is too early for us to assess whether the expectation that newly trained individuals will widely diffuse their new knowledge is correct, we find suggestive evidence that this fear may not be warranted. In fact, it appears that newly trained unskilled workers might find it self-serving to embrace the status quo norm: *Non-Competitor training* unskilled workers are 60% less likely than control ones to agree that it is acceptable for a skilled worker to train an unskilled worker (Figure 4a). This suggests that they may themselves opt to hoard their new knowledge.

7 Conclusion

This study employs two field experiments in 223 Burundian villages to investigate whether incumbents limit the diffusion of new agricultural skills if they anticipate that their returns will be reduced by further diffusion. We randomize i) whether unskilled workers were trained by skilled workers from the same village (direct labor market competitors) or by skilled workers from a different village (non-competitors) and ii) whether the training was in the planting practices or in composting (a nonrivalrous technology). We find that the skilled workers are 40 p.p. less likely to share row-planting knowledge with laborers from their own village than they are with laborers from another village. However, they shared knowledge of skills with nonrivalrous returns equally with both groups. This knowledge hoarding has meaningful economic consequences: Villages exposed to knowledge hoarding produce less agricultural output in aggregate. However, reducing knowledge hoarding entails a cost for incumbents.

This paper has important implications for how we consider the diffusion of technologies and skills in LICs. Classic growth models consider ideas, technologies and skills to be nonrivalrous (Romer, 1990a). The literature on agricultural technology diffusion has also argued that there are limited strategic incentives to exclude others from acquiring knowledge, or otherwise a contract would emerge between incumbents and knowledge seekers (Foster and Rosenzweig, 1995; Conley and Udry, 2010). However, this paper highlights that in segmented markets with inelastic supply or demand, incumbents can benefit from rents when dissemination is limited. This in turn, can generate strategic incentives to limit further diffusion. This insight also contributes to a recent and important literature suggesting that costs faced by incumbents when sharing knowledge can meaningfully slow its diffusion (BenYishay and Mobarak, 2018; Chandrasekhar et al., 2022).

Many open questions remain. Further work should examine the effects of diffusion at scale of new technologies and the local price effects (if any) of such diffusion. Moreover, alternate behavioral sources of rivalrousness (such as peer comparison) may also produce rivalrous returns for incumbents. Finally, we require a deeper understanding of what policies, such as improved market access, might mitigate knowledge hoarding. We leave these questions for future research.

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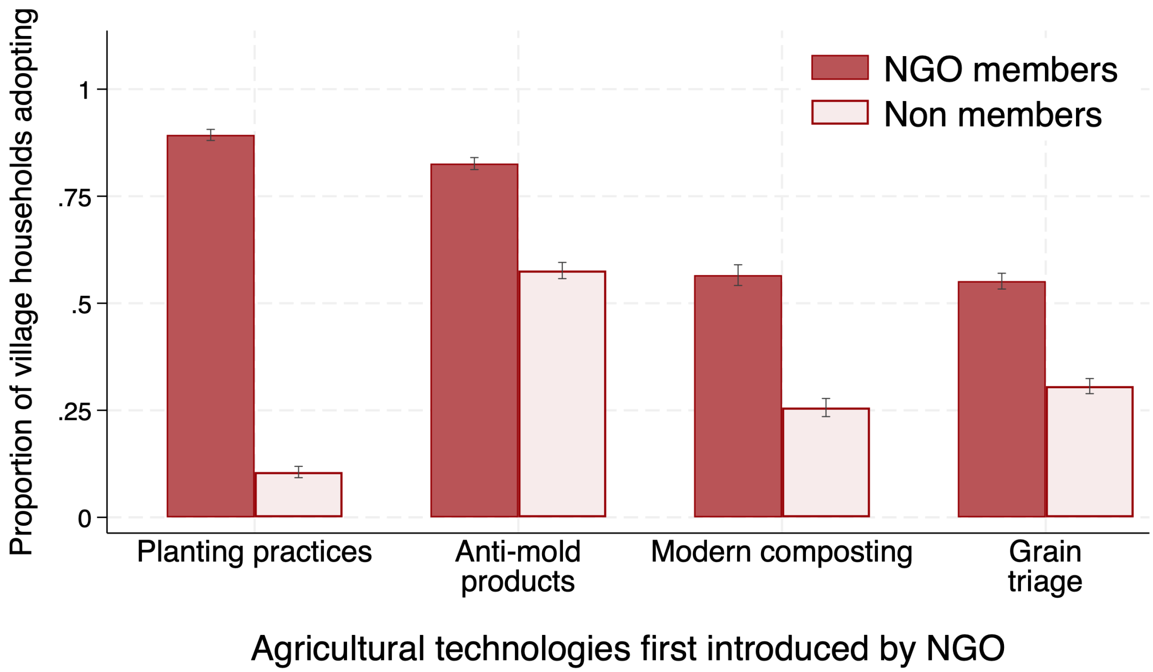
Figures

Figure 1: Technology diffusion from village census



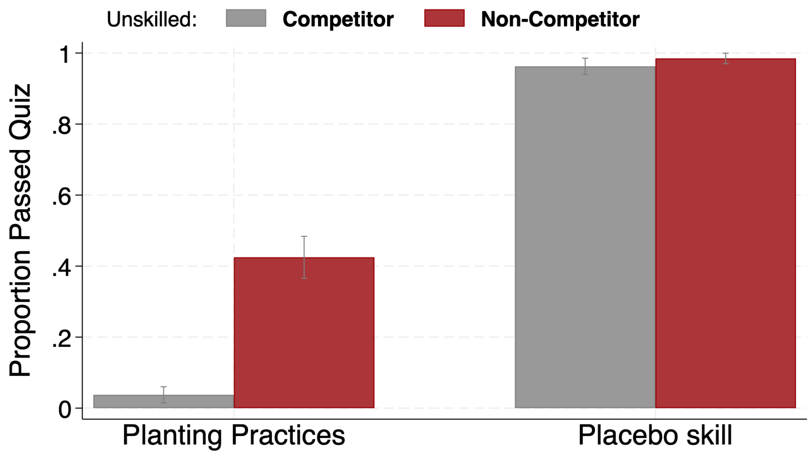
Notes: This figure shows the adoption rate of different agricultural technologies among village households split by whether one of the family members belongs to the NGO or not. Given the limited availability of other sources of learning, any diffusion among nonmembers must have likely occurred through social learning.

We focus on three technologies that were originally introduced in these villages to its members by a large agricultural NGO, namely the production and use of compost, post-harvest grain storage techniques, and the planting practices. While knowledge of the latter—planting practices—grants a wage premium in the labor market, there is no hiring to perform composting or post-harvest storage techiques, making their returns nonrivalrous. Modern techniques for compost production require i) the use of different vegetable residuals in given proportion and (optional) animal manure; ii) a long enough maturation time (2 months). The diffusion of this technology has been studied in other contexts in Sub-Saharan Africa as an example of a nonrivalrous good (e.g. BenYishay and Mobarak, 2018; Beaman and Dillon, 2018). Post-harvest storage techniques are a series of techniques aimed at preserving the harvest and minimize the risk of mold or other parasites. In particular, in this figure we show the adoption of two steps that happen before the grains are put in containers for storage: "grain triage" (i.e., the separation of bad grains from healthy ones); and application of anti-molds products. Finally, the planting practices consists of planting seeds in evenly-spaced seedbeds on the field. The first set of bars refers to the share of households that adopted the planting practices on their farm. The second refers to adoption of anti-mold products, the third to the share of households that produced compost in the modern way, and the third sets refer to the share of households of each type that "triaged" the grains.

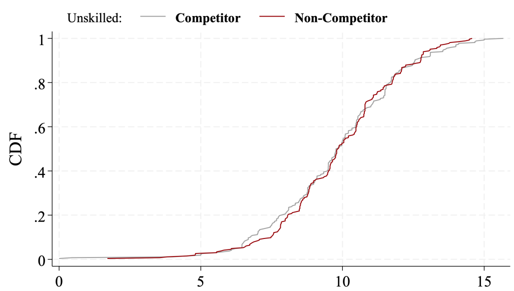
These data come from a household census survey we conducted in 15 study villages, where we interviewed the household head, or another adult family member if the head was absent.

Figure 2: Treatment effect on the unskilled workers' learning outcomes by treatment arm

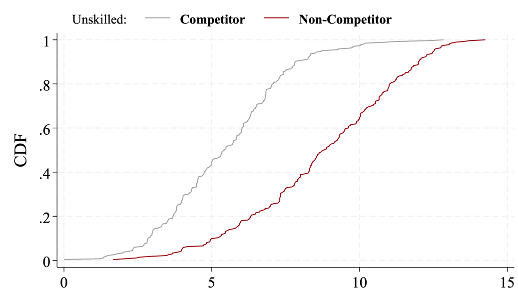
(a) Proportion of unskilled workers who passed the incentivized quiz



(b) CDF of score in the placebo skill quiz



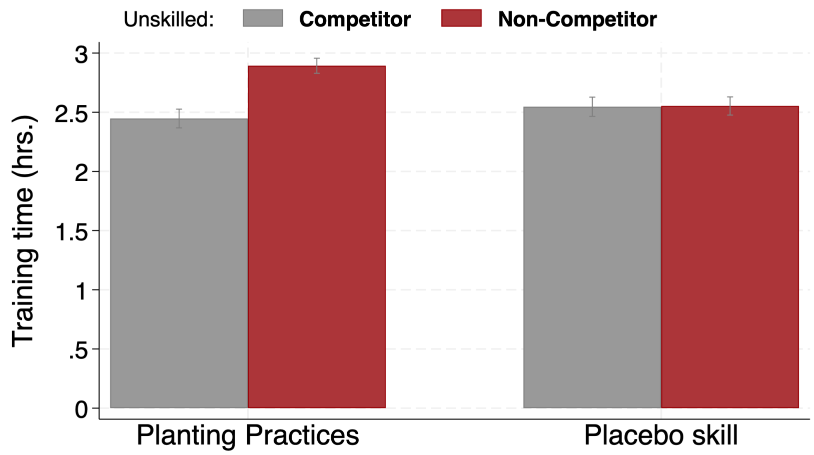
(c) CDF of score in the planting practices quiz



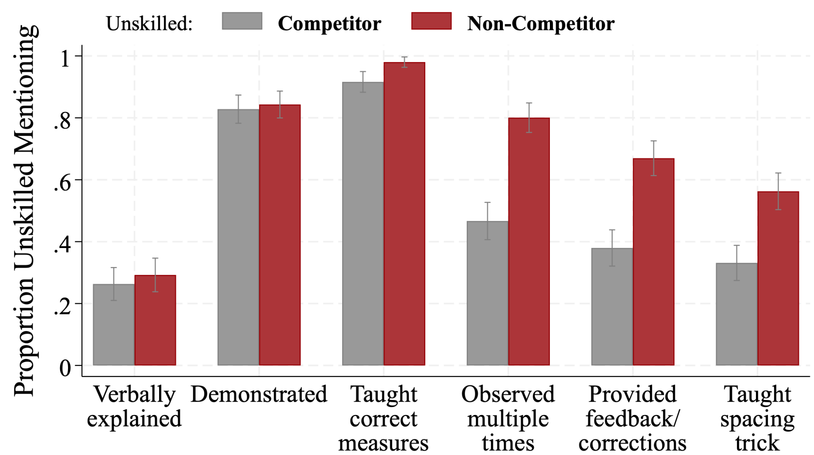
Notes: This figure shows the unskilled workers' learning outcomes in each treatment arm, measured through an incentivized quiz in the trained technology. The quiz for knowledge of the placebo technologies consists of a series of questions about practical aspects of these technologies, while the quiz for the planting practices is a practical quiz. Panel 2a shows the proportion of unskilled workers in each arm that passed the test according to a preregistered threshold. Panel 2b shows the cumulative distribution function of the quiz scores in the placebo technology, while panel 2c shows the distribution of scores in the planting practices quiz. The gray bars (lines) refer to the outcomes of unskilled workers who attended the training event with incumbent skilled workers from their same village (Competitor), whereas the red bars refer to unskilled workers who attended the event with incumbent from a different village (Non-Competitor). Both CDFs show the outcomes residualized by geographical strata fixed effects, and dummies for the time of the training (morning or afternoon), whether an unforeseen circumstance (e.g., rains, visits by local authorities) interfered with the training. See Appendix Tables A9 for regression results.

Figure 3: Knowledge-sharing experiment: Training effort and quality

(a) Time the incumbent spent training the unskilled worker by treatment arm



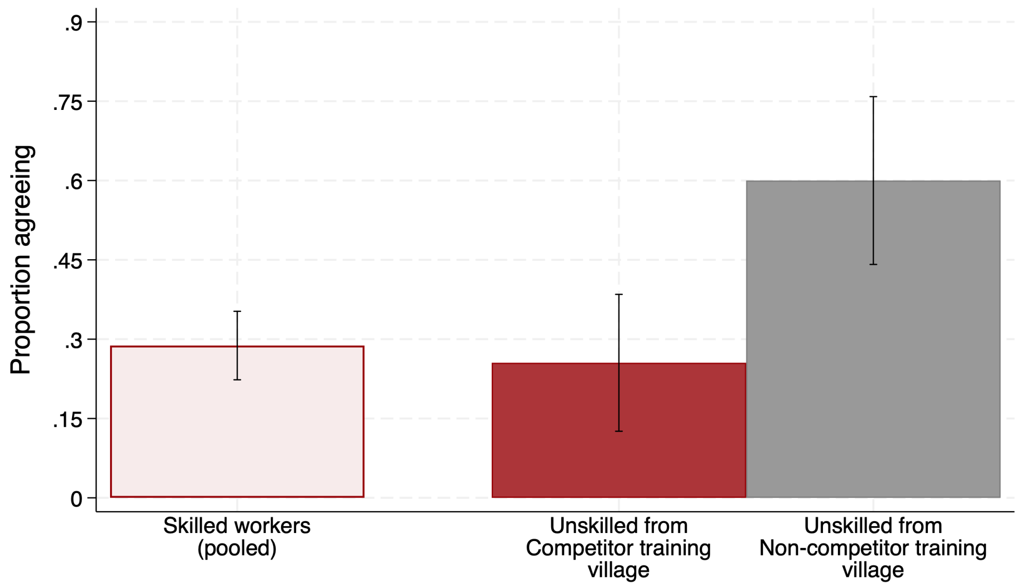
(b) Activities performed by the incumbent in the planting practices training treatment arm



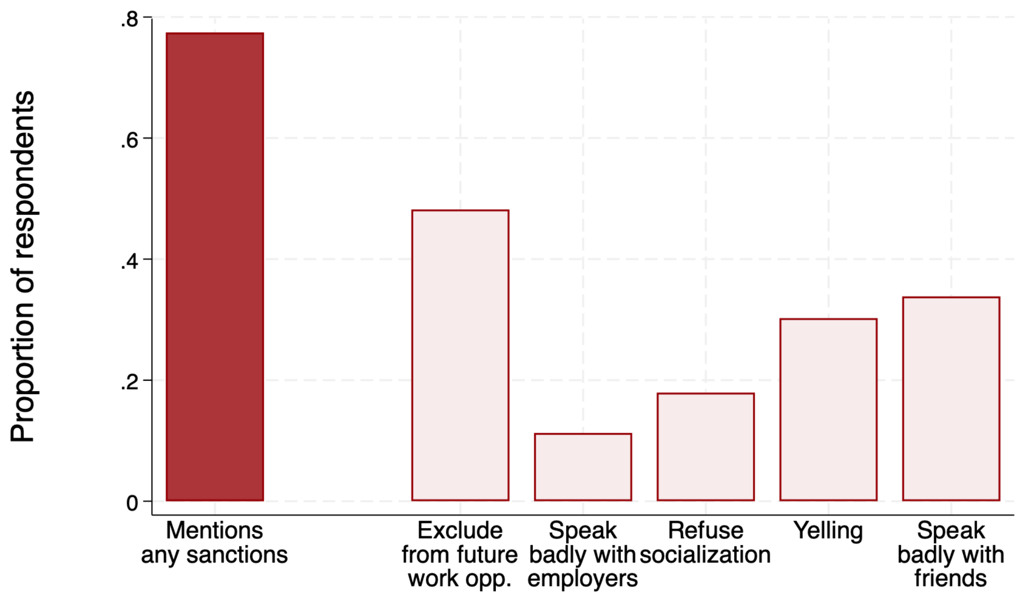
Notes: This figure presents measures of the quantity and quality of training given by skilled workers in the Competitor and Non-Competitor training treatment arms. Panel 3a of this figure shows the amount of time the incumbent skilled workers spend training unskilled workers at the training event by treatment arm, while panel 3b shows the actions performed by the incumbent while training the unskilled worker in planting practices. Both measures are self-reported by the unskilled worker during a survey at the end of the training. The gray bars report the outcomes for unskilled workers who were trained by an incumbent skilled worker from the same village (Competitor), while the red bars refer to unskilled workers trained by an incumbent from a different village (Non-Competitor). Both figures show the raw means for each group. See Appendix Tables A9 and A10 for regression results.

Figure 4: Norms around training someone unskilled from the same village in the planting practices

(a) Acceptability of training a laborer from the same village



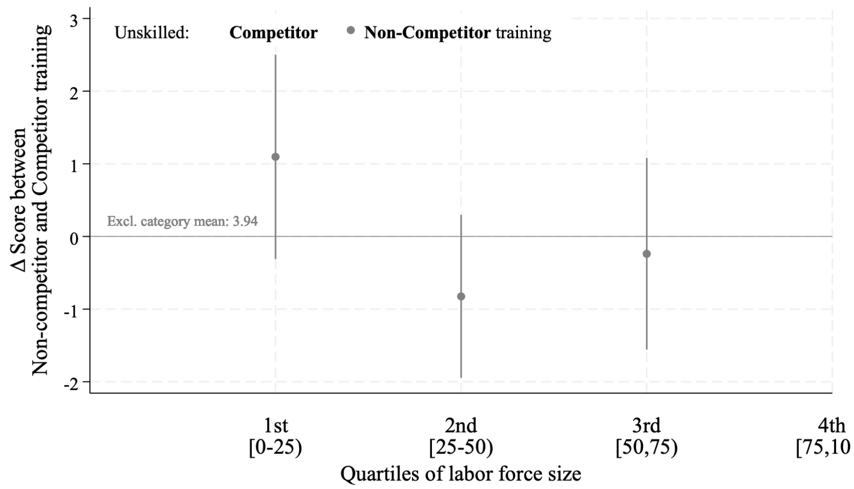
(b) Skilled workers – Consequences of training another laborer



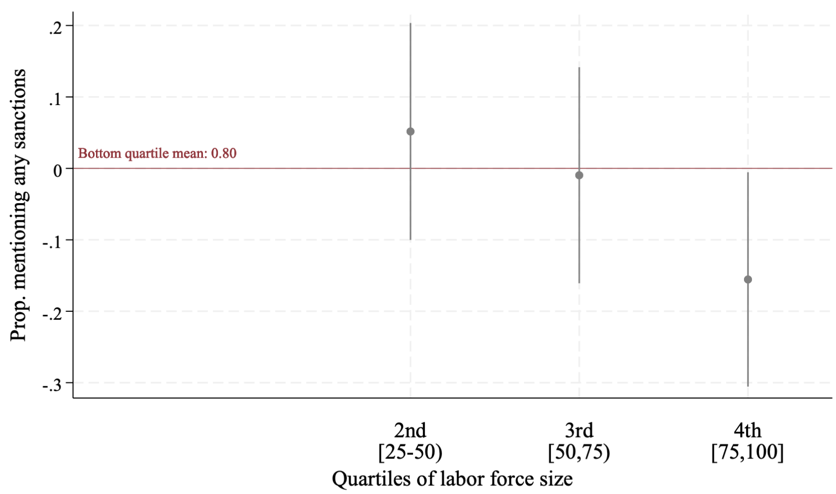
Notes: This figure presents evidence of the stated acceptability and expected sanctions for training an unskilled worker in the same village. Panel 4a shows the fraction of respondents agreeing with the statement that it would not be acceptable for a skilled worker to train an unskilled worker in the same village in the planting practices. The first bar on the left comprises the response of a sample of incumbent skilled workers. The columns on the right comprise random samples of unskilled workers from Competitor training and Non-Competitor training villages, respectively. This vignette-style question was asked to a subsample of 661 study participants in the Market effects experiment villages. The answers for incumbent skilled workers are pooled across treatment arms. 95% confidence bars are reported. Standard errors are clustered at the village level. Panel 4b reports the social sanctions that an incumbent would expect to incur if they trained an village unskilled worker in the planting practices. The left-most column reports the fraction of respondents that mention *any* sanction. The shaded bars on the right report the proportion stating each consequence. The answers were unprompted, and the enumerators marked the most appropriate category based on a list formed based on focus groups. The sample comprises 347 skilled workers from a subsample of villages in the Market effect experiment.

Figure 5: Market Experiment – Treatment effect heterogeneity by labor market characteristics

(a) Outcome: Unskilled workers' quiz score in the planting practices



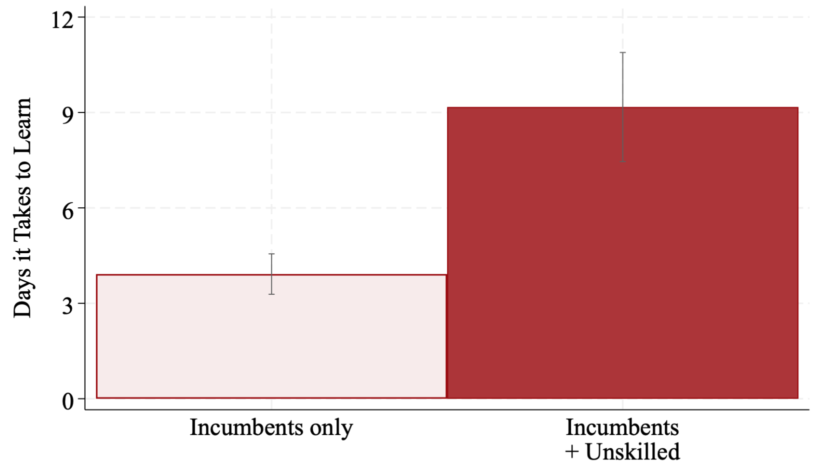
(b) Outcome: Incumbent mentions any sanctions for training



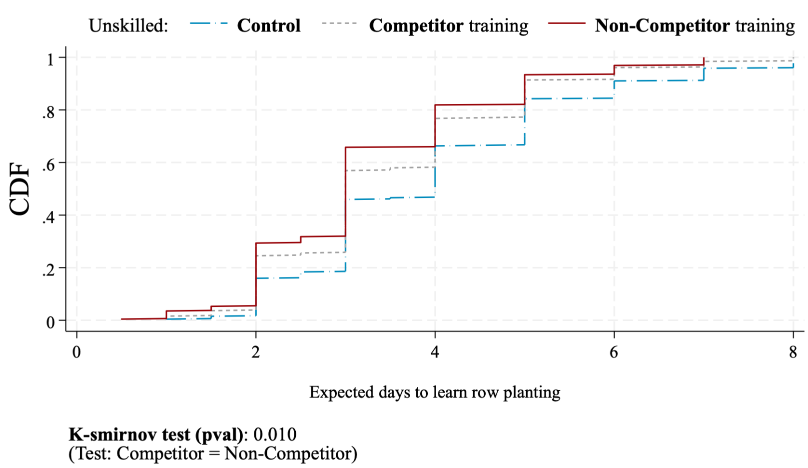
Notes: This figure shows the heterogeneity in the Market effect experiment outcomes by the size of the village labor market. Panel 5a shows the treatment effect heterogeneity on the unskilled workers' quiz score in the planting practices by quartiles of the labor force size. We plot the coefficients of two sets of dummies: labor force quartile dummies, and the interaction terms between a quartile dummy and the Non-Competitor training treatment. We interpret the score in the Non-Competitor training arm as the counterfactual score that the unskilled would have obtained absent knowledge hoarding motives. Hence, the larger the gap, the more prevalent knowledge hoarding is. We report the average score in the Competitor arm in villages in the first quartile (0-25^th percentile) of the labor force size (i.e., the excluded category). The sample consists of all the unskilled workers that attended the training event in the Market effect experiment (N=2090). The regression is weighted by the number of observations (event attendees) in each village. Panel 5b shows the proportion of incumbents expecting that others would impose any social sanctions if an incumbent trained an unskilled worker in the same village (see also Figure 4). The excluded category consists of the villages in the bottom quartile (0-25^th percentile) of the village labor force size distribution. The data come from a supplementary sample of N=347 skilled workers randomly sampled from villages in the Market Effect experiment. The regression is weighted by the number of observations in each village. In all regressions, the standard errors are clustered at the village level and geographical strata fixed effects are included.

Figure 6: Misinformation – Beliefs about the time it takes to learn

(a) Skilled workers' stated beliefs about the time it takes to learn the planting practices



(b) Treatment effect on the expected number of days (cost) it takes to learn



Notes: Panel 6a in this figure shows the incumbents' average stated beliefs about the number of days it would take an unskilled worker to master the planting practices. These statements were collected during 95 focus groups held with 6 to 9 village laborers, where we randomized whether the group was exclusively formed by incumbent skilled workers, or by a mix of incumbents and unskilled workers. An enumerator asked a series of questions about modern planting practices, facilitated the discussion, and noted each participant's answers. Focus groups were also recorded. The bar on the left refers to the average answer stated by skilled workers when they were alone in the focus groups, while the bar on the right shows the average time stated by skilled workers when unskilled workers also participated in the focus group. Standard errors for the confidence bars are clustered at the focus-group level. Panel 6b shows the treatment effect on the distribution of beliefs about the time it takes to learn among unskilled workers in the Market Effect experiment. These data were collected 8-9 months after the training. The red solid line shows the average number of days stated by the unskilled workers in the Non-Competitor training arm, the dashed gray line represents the Competitor training arm, and the blue line represents the Control group. The graph shows the raw averages. The p-value for the k-smirnov test for equality of the Competitor training and Non-Competitor training is reported on the bottom left. To compute the p-value, we residualize the outcome to control for the geographical strata fixed effects. The p-value for the test for the raw means is $p = .036$. In both figures, the outcomes are winsorized at the 99^{th} percentile.

Tables

Table 1: Knowledge-sharing Experiment – Treatment effect heterogeneity

	Outcome: Unskilled is trained							
	None	Skilled characteristics					Unskilled charateristics	
Heterogeneity dimension	(Bench-mark)	< median village farm land	≥ median labor earnings	≥ median days work practices	≥ median importance reputation	< median training ability	> median village farm land	Elder woman
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Competitor training	-0.363	-0.317	-0.285	-0.270	-0.427	-0.385	-0.436	-0.386
	(0.041)	(0.045)	(0.052)	(0.048)	(0.066)	(0.055)	(0.054)	(0.044)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Competitor X heterog.		-0.112	-0.144	-0.167	0.096	0.064	0.132	0.235
		(0.050)	(0.068)	(0.054)	(0.064)	(0.095)	(0.054)	(0.068)
		[0.030]	[0.040]	[0.004]	[0.140]	[0.502]	[0.018]	[0.001]
Heterogeneity		0.052	0.113	0.126	-0.086	-0.043	-0.050	-0.175
		(0.051)	(0.062)	(0.052)	(0.062)	(0.090)	(0.053)	(0.079)
		[0.311]	[0.073]	[0.019]	[0.170]	[0.639]	[0.346]	[0.031]
Excluded cat. mean		0.393	0.381	0.370	0.478	0.461	0.475	0.444
N	533	533	533	533	533	533	533	533

Notes: This table reports the heterogeneous treatment effects on the likelihood that the unskilled worker is trained (i.e., achieved more than the pre-specified threshold on the incentivized quiz) by incumbent (column (2) through (5)) and unskilled (columns (7) and (8)) characteristics. Column (1) reports the benchmark regression, without heterogeneity. Estimates are obtained using the specification 1. We restrict the sample to the training in the planting practices alone, and refer to the Appendix Table A11 for the analysis including the training in the placebo technology. In all the regressions, Heterogeneity is a dummy equal to 1 if the unskilled is paired with a skilled worker with the characteristic specified in the column header. The excluded category is the unskilled in the Non-Competitor training events, trained in the planting practices, and having Heterogeneity = 0. All heterogeneity dummies in columns (2)-(7) refer to the median value of that characteristic in the village where the individual resides (i.e., the incumbent's village for columns (2)-(6), or the unskilled village in column (7)). "Importance reputation" (column (5)) refers to a question regarding the perceived reputational consequences if the skilled trained poorly someone from their village. "Training ability" (column (6)) refers to answering a question about the self-reported ability to train another individual. Elder woman refers to a woman who is above 50 (robust to other cutoffs).
Standard errors are clustered at the village level. All regressions control for geographical strata fixed-effects, the order of the training (whether it was the first or second training for the unskilled workers) and whether there was any disruption during the training (e.g. rain, delays or interruptions due to unforseen circumstances). Demographic controls include: skilled and unskilled gender, age, own farm size, baseline adoption and knowledge of the planting practices and the placebo technologies (composting, post-harvest storage techniques). Observations are weighted by the number of individuals in the regression sample by village.

Table 2: Market Effect Experiment – Treatment effect on skilled and unskilled workers' labor market outcomes

	Labor Earnings	**Daily Wage**	**Work days (Practices)**	**Work days (All tasks)**	**Exp. work days next season**
	(1)	(2)	(3)	(4)	(5)
Panel A: Unskilled workers					
Non-competitor training	3,197.8	140.2	1.297	0.316	1.198
	(1,319.7)	(41.4)	(0.080)	(0.420)	(0.353)
	[0.017]	[0.001]	[0.000]	[0.454]	[0.001]
Competitor training	554.4	85.3	0.653	-0.318	0.101
	(1,270.9)	(40.3)	(0.063)	(0.401)	(0.316)
	[0.663]	[0.036]	[0.000]	[0.429]	[0.749]
Test (p-value): Competitor training = Non-competitor training	0.035	0.248	<.001	0.090	<.001
Control mean	40,374.3	2,672.3	0.013	15.1	15.1
N	1706	1657	1706	1706	1706
Panel B: Skilled workers					
Non-competitor training	-2,703.143	-48.398	-0.498	-0.798	-2.864
	(1,087.5)	(50.1)	(0.136)	(0.313)	(0.276)
	[0.014]	[0.336]	[0.000]	[0.012]	[0.000]
Competitor training	-198.250	38.7	-0.254	-0.339	-1.642
	(1,262.5)	(57.0)	(0.140)	(0.312)	(0.275)
	[0.875]	[0.498]	[0.073]	[0.279]	[0.000]
Test (p-value): Competitor training = Non-competitor training	0.041	0.083	0.045	0.140	<.001
Control mean	45,490.0	3,087.1	3.771	14.9	15.1
N	3242	3179	3242	3242	3242

Notes: Panel A of this table reports treatment effects on labor market outcomes for unskilled workers who attended the event. Panel B reports results for skilled workers, pooling the event and spillover samples, weighted by their respective share of the population (see Appendix Figure A.2b for a visual representation of sampling within a village). Column (1) refers to the total earnings from agricultural jobs that the respondents earned during the agricultural season. Column (2) shows the average daily wage, computed as the total labor market earnings divided by the number of days worked. Column (3) reports the number of days of employment in the planting practices over the course of the agricultural season. Column (4) reports the total number of days of agricultural work, comprehensive of all tasks. Column (5) reports the expected days of agricultural work over the next agricultural season.

All regressions include 17 geographical strata fixed effects, and baseline work and demographic characteristics (age, gender, household size, marital status, days of waged agricultural work and expected average daily wage).

P-values of the test of equality of the Noncompetitive village and Competitive village coefficients are shown.

Standard errors are clustered at the village level and reported in parentheses. P-values are reported in squared brackets.

Table 3: Market Effect Experiment – Aggregate treatment effect at the labor-force level

	Work in row planting					Work in all tasks	
	For employers			Including own farm		For employers	
	Work days	Plots practices	Daily Wage	Work days	Plots practices	Work days	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-competitor training	0.243	0.290	-86.04	0.572	0.474	-0.156	-6.040
	(0.063)	(0.037)	(45.87)	(0.096)	(0.073)	(0.268)	(851.6)
	[0.000]	[0.000]	[0.063]	[0.000]	[0.000]	[0.560]	[0.994]
Competitor training	0.104	0.137	-2.91	0.349	0.291	-0.206	437.0
	(0.065)	(0.037)	(48.74)	(0.095)	(0.072)	(0.266)	(998.1)
	[0.112]	[0.000]	[0.953]	[0.000]	[0.000]	[0.440]	[0.662]
Test (p-value): Competitor training = Non-competitor training	0.036	<.001	0.068	0.034	0.025	0.834	0.633
Control mean	1.809	1.178	2,914.96	3.040	2.040	15.1	42,412.3
N	6673	6668	21316	6673	6673	6673	6673

Notes: This table shows the treatment effects on labor market outcomes, aggregated at the level of the labor force. The sample comprises skilled and unskilled workers from the main and spillover samples weighted by their respective share of the population (see Appendix Figure A.2b for a visual representation of the sampling strategy within a village). Columns (1) through (5) refer to agricultural work in the planting task. Columns (6) and (7) refer to work in any agricultural task. Column (1) and (3) refer to the number of days worked in planting practices for employers alone, and also including own farm work, respectively. Column (2) and (4) report the number of plots planted using the planting practices technology for employers alone, and for employers and on the respondent's farm. Column (3) refers to the daily wage for the planting practice task, computed at the worker-employer level. Column (6) is the overall days of work in agriculture, and column (7) is overall labor market earnings.

All regressions include 17 geographical strata fixed effects, and baseline work and demographic characteristics (age, gender, household size, marital status, days of waged agricultural work and expected average daily wage). P-values of the test of equality of the Noncompetitive village and Competitive village coefficients are shown. Standard errors are clustered at the village level and reported in parentheses. P-values are reported in squared brackets.

Table 4: Market Effect Experiment – Treatment effect on own-farm adoption of the planting practices for unskilled workers

	Any plot practices	**Share plots majority practices**	**Num. plots practices**	**Bean Harvest (kg)**	**Crops Value (Francs)**
	(1)	(2)	(3)	(4)	(5)
Non-competitor training	0.74	0.33	0.97	3.85	11,451.2
	(0.021)	(0.012)	(0.039)	(1.405)	(4,351.3)
	[0.000]	[0.000]	[0.000]	[0.007]	[0.010]
Competitor training	0.55	0.21	0.67	1.40	3,311.9
	(0.029)	(0.014)	(0.040)	(1.451)	(4,105.2)
	[0.000]	[0.000]	[0.000]	[0.335]	[0.421]
Test (p-value): Competitor train. = Non-competitor train.	<.001	<.001	<.001	0.096	0.056
Control mean	0.012	0.004	0.017	36.0	71,050.0
N	1706	1704	1706	1565	1565

Notes: This table shows outcomes related to own-farm adoption of the planting technology among unskilled workers in the main sample.

The dependent variable in column (1) is a dummy for whether the respondent adopted the planting practices on at least one plot. Column (2) reports the share of beans plots that were row planted for the major part. Column (3) shows the total number of plots rowplanted. Column (4) reports the beans harvest in kilograms. Finally, column (5) shows the total value of the harvest (beans and other crops) in Burundian Franc, computed as the product between the harvest amount for each crop, and the price of that crop at the nearest market as reported by the respondent. All regressions include 17 geographical strata fixed effects, and baseline work and demographic characteristics (age, gender, household size, marital status, days of waged agricultural work and expected average daily wage).

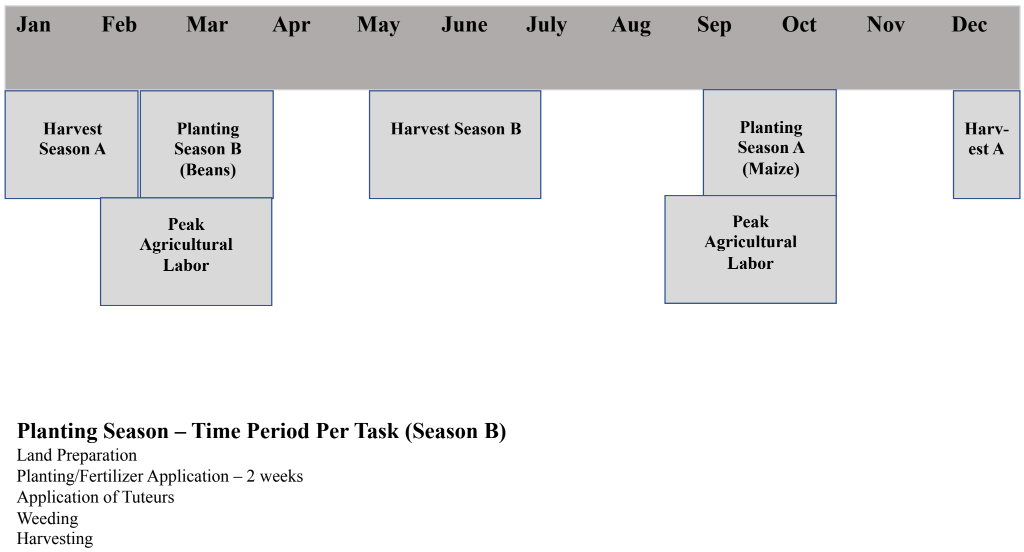
P-values of the test of equality of the Noncompetitive village and Competitive village coefficients are shown.

Standard errors are clustered at the village level and reported in parentheses. P-values are reported in squared brackets.

A Appendix Figures and Tables

A.1 Agriculture in Burundi

Figure A.1: Burundian agricultural calendar

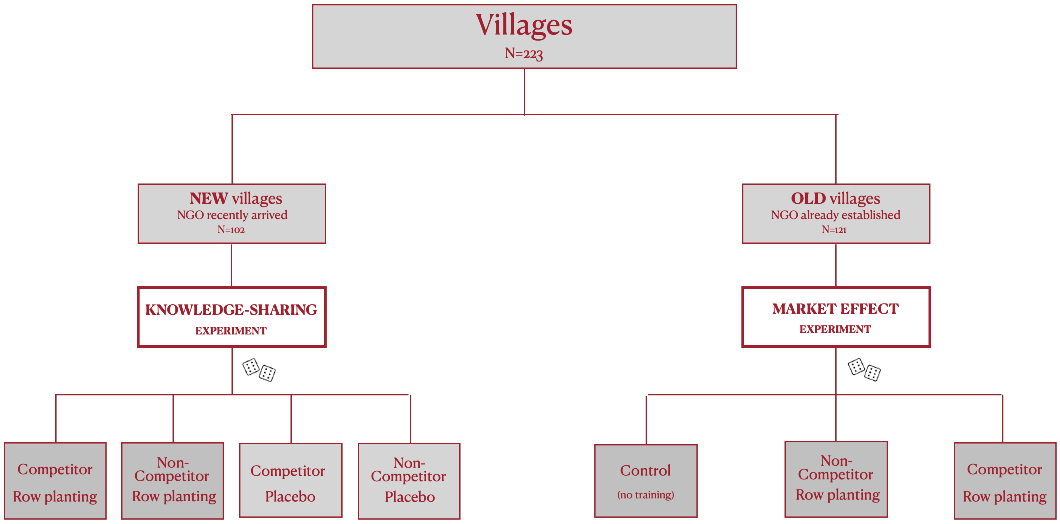


Notes: This figure shows details of the agricultural calendar and labor requirements in Burundi. The figure is based partially on a similar figure in Vinck et al. (2008).

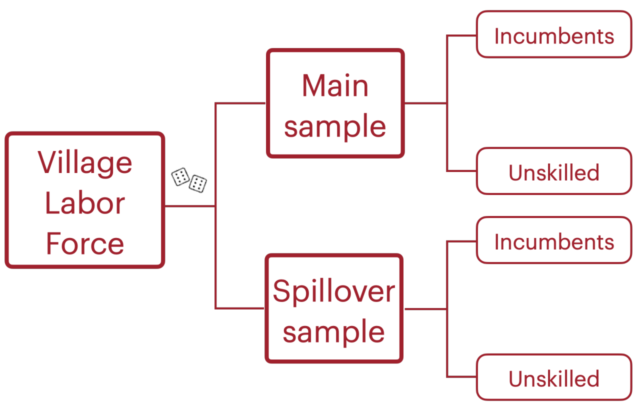
A.2 Sampling

Figure A.2: Village and participant sampling

(a) Sampling of villages for the two experiments



(b) Market effect experiment – Village sampling frame



Notes: Panel A.2a shows an overview of the sampling frame and treatment assignment for each experiment. Panel A.2b documents how laborers are sampled as part of the experiment. In each village (labor market), we compile a list of individuals participating in the labor market, and categorize them as either skilled or unskilled workers. We randomly assign workers from each category to either a main or spillover sample. Training event participants are, which enables us to measure effects at the labor force level.

Table A1: Characteristics of village households by participation in the labor market

	Households labor market participation			
	Hire only	Both hire & supply	Supply only	Neither
	(1)	(2)	(3)	(4)
Proportion of households	0.25	0.09	0.49	0.17
	(0.43)	*(0.29)*	*(0.50)*	*(0.37)*
=1 is bottom quartile land distribution	0.02	0.05	0.30	0.29
	(0.15)	*(0.22)*	*(0.46)*	*(0.45)*
=1 is top land distribution	0.67	0.53	0.09	0.08
	(0.47)	*(0.50)*	*(0.29)*	*(0.27)*
=1 is NGO member	0.61	0.67	0.46	0.36
	(0.49)	*(0.47)*	*(0.50)*	*(0.48)*
Savings (Fbu)	62,084	57,724	20,401	21,234
	(148,239)	*(87,765)*	*(32,189)*	*(46,568)*
Num. of adults in household	2.50	3.03	2.34	2.08
	(1.16)	*(1.35)*	*(0.93)*	*(1.03)*
Num. fam. members who supply labor	0.00	1.59	1.60	0.00
	(0.00)	*(0.86)*	*(0.73)*	*(0.00)*
=1 if adopted row planting	0.66	0.70	0.43	0.36
	(0.47)	*(0.46)*	*(0.50)*	*(0.48)*
=1 if adopted modern composting	0.88	0.80	0.71	0.71
	(0.33)	*(0.40)*	*(0.45)*	*(0.45)*
=1 if any member hired for rowplanting tasks	.	0.51	0.34	.
	.	*(0.50)*	*(0.48)*	.
Post-harvest tech. adoption indexs	2.40	2.34	1.90	1.66
	(1.02)	*(0.94)*	*(1.22)*	*(1.24)*

Notes: This table shows summary statistics of descriptive characteristics for households who i) hire, ii) supply, iii) hire and supply or iv) neither hire nor supply labor. Data are collected for 35 villages that we conduct censuses in. Mean values for each measure are displayed. Standard deviations are listed in parentheses.

Table A2: Characteristics of village households who only hire/supply labor by NGO membership

	Households			
	Only hire		Only supply	
	NGO members	Non members	NGO members	Non members
	(1)	(2)	(3)	(4)
Proportion of households	0.61	0.39	0.46	0.54
	((0.43))	0.49	*((0.50))*	0.50
=1 is bottom quartile land distribution	0.01	0.04	0.22	0.38
	(0.11)	*(0.20)*	*(0.41)*	*(0.48)*
=1 is top land distribution	0.69	0.64	0.14	0.05
	(0.46)	*(0.48)*	*(0.35)*	*(0.23)*
Savings (Fbu)	64,841	57,816	24,533	16,925
	(130,910)	*(171,735)*	*(34,249)*	*(29,923)*
Num. of adults in household	2.56	2.40	2.41	2.28
	(1.16)	*(1.15)*	*(0.99)*	*(0.86)*
Num. fam. members who supply labor	0.00	0.00	1.59	1.62
	(0.00)	*(0.00)*	*(0.75)*	*(0.72)*
=1 if adopted row planting	0.95	0.19	0.88	0.07
	(0.21)	*(0.40)*	*(0.33)*	*(0.25)*
=1 if adopted modern composting	0.92	0.78	0.84	0.57
	(0.27)	*(0.41)*	*(0.36)*	*(0.50)*
=1 if any member hired for rowplanting tasks	0.00	0.00	0.70	0.04
	(0.00)	*(0.00)*	*(0.46)*	*(0.20)*
Post-harvest tech. adoption indexs	2.61	2.08	2.41	1.47
	(0.94)	*(1.05)*	*(1.09)*	*(1.15)*

Notes: This table shows summary statistics for households that hire or supply labor, and are or are not members of an Agricultural NGO. Data is collected in 35 villages that we conduct censuses in. Standard deviations are listed in parentheses.

	Non members		NGO members	
	NGO Established	**NGO Recent**	**NGO Established**	**NGO Recent**
	(1)	(2)	(3)	(4)
Modern compost production	0.26	0.12	0.57	0.56
	(0.44)	*(0.33)*	*(0.50)*	*(0.50)*
Grain triage	0.31	0.17	0.55	0.73
	(0.46)	*(0.37)*	*(0.50)*	*(0.45)*
Apply anti-mold products	0.58	0.44	0.83	0.83
	(0.49)	*(0.50)*	*(0.38)*	*(0.37)*
Post-harvest tech. index (out of 4)	2.19	0.91	3.34	2.43
	(1.48)	*(0.92)*	*(1.18)*	*(0.75)*
Own farm rowplanting	0.11	0.00	0.89	0.96
	(0.31)	*(0.00)*	*(0.31)*	*(0.19)*
Individuals	2,624	560	2,808	931
Villages	35	53	35	106

Table A3: Adoption of different agricultural technology among households who are or are not NGO members, by the time of NGO arrival

Notes: This table shows summary statistics for households in 35 villages that we conduct censuses, which shows average values of observable characteristics of farmers who i) are and ii) are not members of an agricultural NGO and, who reside in either i) villages where the NGO was recently established and ii) villages where the NGO has been present for some time. Mean values for each characteristic are shown, and standard deviations in parentheses.

A.3 Balance Tables

Table A4: Balance table of village characteristics and training events attendance in the Market Effect Experiment

	Treatment arms			F-test p value
	Control	**Same Vill.**	**Diff. Village**	**(1) = (2) = (3)**
	(1)	(2)	(3)	(4)
Panel A: Village characteristics				
Labor force size	89.927	-0.591	-0.229	0.993
	(*25.095*)	5.141	5.005	
		[0.909]	[0.964]	
Share of skilled laborers	0.421	0.002	0.003	0.989
	(*0.085*)	0.019	0.022	
		[0.935]	[0.881]	
1(church=1)	0.513	0.120	0.014	0.498
	(*0.506*)	0.113	0.115	
		[0.288]	[0.904]	
1(school=1)	0.385	0.075	-0.007	0.734
	(*0.493*)	0.113	0.111	
		[0.505]	[0.954]	
1(shop=1)	0.487	0.029	-0.026	0.875
	(*0.506*)	0.103	0.106	
		[0.776]	[0.807]	
Panel B: Training events				
Unskilled at event		24.300	1.782	0.495
		(13.931)		
as share of labor force		0.268	0.025	0.158
		(0.078)		
N villages	41	40	40	

Notes: Panel A of this table shows a test of balance for village characteristics in the Market effect experiment. Column 1 provides covariate means and standard deviations for the reference group: villages in the Control arm. Column 2 and 3 report regression coefficients relative to the Control group. Panel B shows the average number of unskilled attending the training event in T1-Same and T2-Different village arms, and as a proportion of the labor force in each village. All specifications include 17 geographical strata fixed effects. Robust standard errors are reported in parentheses. P-values are reported in square brackets. P-value from Wald tests of joint significance of all treatment arms (relative to the Control group) are reported in Column 4.

Table A5: Market Effect Experiment – Balance table for skilled and unskilled workers in the main sample

	Skilled workers					**Unskilled workers**				
	Control	Competitor training	Non-competitor training	F-test Joint sig.	Obs	Control	Competitor	Diff. training	F-test training	Obs Joint sig.
Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Demographics										
Age	39.89	0.582	0.330	0.654	1757	37.93	-1.151	-0.115	0.220	1790
	(0.45)	(0.677)	(0.607)			(0.48)	(0.734)	(0.831)		
		[0.392]	[0.587]				[0.119]	[0.890]		
Male	0.280	-0.034	-0.065	0.228	1757	0.174	-0.025	-0.018	0.550	1790
	(0.018)	(0.034)	(0.038)			(0.015)	(0.023)	(0.028)		
		[0.326]	[0.086]				[0.278]	[0.527]		
Married	0.831	-0.014	0.014	0.364	1757	0.755	-0.031	0.021	0.190	1790
	(0.015)	(0.018)	(0.017)			(0.017)	(0.029)	(0.031)		
		[0.438]	[0.411]				[0.298]	[0.495]		
No schooling	0.319	0.004	0.005	0.982	1757	0.387	-0.011	0.012	0.762	1790
	(0.019)	(0.031)	(0.028)			(0.020)	(0.030)	(0.032)		
		[0.906]	[0.854]				[0.705]	[0.717]		
Panel B: Household characteristics										
Household size	4.579	0.103	-0.070	0.273	1758	4.030	0.016	0.021	0.983	1791
	(0.078)	(0.104)	(0.110)			(0.079)	(0.132)	(0.118)		
		[0.328]	[0.521]				[0.902]	[0.859]		
Savings (Fbu)	24,534	-1,891	-4,514	0.090	1758	9,002	270.492	662.797	0.799	1791
	(1,080)	(1,916)	(2,035)			(522)	(1,240)	(1,060)		
		[0.326]	[0.028]				[0.828]	[0.533]		
Land area (ares)	14.56	-1.146	-1.453	0.289	1757	7.379	0.723	0.258	0.284	1790
	(0.39)	(0.845)	(0.954)			(0.212)	(0.462)	(0.530)		
		[0.178]	[0.130]				[0.121]	[0.628]		
Plots of land	4.836	-0.221	-0.537	0.018	1757	3.612	0.097	-0.091	0.361	1790
	(0.089)	(0.181)	(0.189)			(0.069)	(0.137)	(0.152)		
		[0.224]	[0.005]				[0.480]	[0.551]		
Panel C: Past labor supply – ag.										
Skilled work days	7.454	-0.187	-0.197	0.844	1752					
	(0.182)	(0.390)	(0.390)							
		[0.632]	[0.615]							
Ag. work days	14.03	-0.159	-0.585	0.422	1752	14.90	-0.529	-0.877	0.157	1785
	(0.24)	(0.489)	(0.449)			(0.22)	(0.410)	(0.471)		
		[0.746]	[0.195]				[0.199]	[0.065]		
Ag. labor earnings (Fbu)	41,153	41.378	-2,434	0.215	1752	36,126	-114.307	-375.685	0.963	1782
	(797)	(1,769)	(1,606)			(554)	(1,277)	(1,397)		
		[0.981]	[0.132]				[0.929]	[0.788]		
Other hh ag. earning (Fbu)	5,654	363.396	419.576	0.882	1758	11,464	1,318	1,069	0.392	1791
	(505)	(913.925)	(904.230)			(623)	(1,034)	(1,029)		
		[0.692]	[0.643]				[0.205]	[0.301]		
Unemployment days	0.718	0.034	-0.160	0.144	1719	1.334	-0.054	0.029	0.844	1753
	(0.061)	(0.098)	(0.103)			(0.078)	(0.128)	(0.138)		
		[0.726]	[0.122]				[0.675]	[0.835]		
Panel D: Own farm										
Beans harvest kg/ares	9.149	0.422	-0.768	0.089	1754	7.468	-0.590	-0.010	0.399	1784
	(0.282)	(0.533)	(0.598)			(0.246)	(0.500)	(0.534)		
		[0.430]	[0.202]				[0.240]	[0.985]		
Work days on own farm	14.70	-0.363	-1.093	0.047	1757	10.14	0.188	0.246	0.873	1790
	(0.20)	(0.423)	(0.445)			(0.20)	(0.466)	(0.523)		
		[0.392]	[0.016]				[0.687]	[0.639]		
Fam labor on own farm days	12.27	-0.035	-1.204	0.078	1757	6.401	1.422	0.760	0.070	1782
	(0.31)	(0.550)	(0.561)			(0.236)	(0.609)	(0.694)		
		[0.950]	[0.034]				[0.021]	[0.276]		
Sold any harvest	0.547	-0.009	-0.057	0.401	1757	0.226	-0.001	-0.020	0.800	1790
	(0.020)	(0.038)	(0.043)			(0.017)	(0.035)	(0.034)		
		[0.812]	[0.190]				[0.982]	[0.570]		

	Same village		Diff. village		F-test	
	Placebo	Rowp.	Placebo	Rowp.	Joint sig.	Obs
	(1)	(2)	(3)	(4)	(5)	(6))
Demographics						
Age	40.21	-1.735	-1.395	-1.945	0.369	897
	(0.76)	(1.124)	(1.081)	(1.185)		
		[0.126]	[0.200]	[0.104]		
Male	0.350	-0.021	0.048	0.071	0.359	897
	(0.034)	(0.059)	(0.059)	(0.064)		
		[0.719]	[0.422]	[0.267]		
Married	0.833	-0.005	0.018	0.004	0.944	897
	(0.026)	(0.036)	(0.042)	(0.037)		
		[0.890]	[0.666]	[0.914]		
Spouse is migrant	0.330	-0.030	-0.067	-0.036	0.628	897
	(0.033)	(0.046)	(0.051)	(0.049)		
		[0.517]	[0.193]	[0.469]		
No schooling	0.310	-0.036	-0.011	-0.074	0.242	897
	(0.033)	(0.036)	(0.044)	(0.038)		
		[0.322]	[0.802]	[0.056]		
Believe able to train	0.680	-0.048	-0.036	0.029	0.353	897
	(0.033)	(0.045)	(0.054)	(0.046)		
		[0.288]	[0.511]	[0.527]		
Household characteristics						
Household size	4.419	0.145	0.034	-0.059	0.705	897
	(0.128)	(0.196)	(0.188)	(0.190)		
		[0.459]	[0.858]	[0.755]		
Savings (Fbu)	23,546	-1,820	-3,027	-2,385	0.628	897
	(1,485)	(2,415)	(2,387)	(2,406)		
		[0.453]	[0.208]	[0.324]		
Land area (ares)	10.80	0.847	0.752	0.446	0.551	897
	(0.46)	(0.599)	(0.690)	(0.635)		
		[0.160]	[0.278]	[0.485]		
Plots of land	4.419	0.045	-0.118	0.188	0.513	897
	(0.157)	(0.219)	(0.190)	(0.198)		
		[0.837]	[0.538]	[0.345]		
Past labor supply – ag.						
Work days in ag	15.77	-0.381	-0.805	0.689	0.209	897
	(0.46)	(0.695)	(0.723)	(0.708)		
		[0.585]	[0.268]	[0.333]		
Skilled work days	3.842	0.117	0.311	0.971	0.020	897
	(0.227)	(0.350)	(0.365)	(0.328)		
		[0.738]	[0.396]	[0.004]		
Ag. labor earnings	44,108	-886.525	-1,493	3,896	0.082	897
	(1,429)	(2,169)	(2,112)	(2,184)		
		[0.684]	[0.481]	[0.077]		
Oth. hh ag. earning	13,249	922.202	1,507	2,473	0.315	897
	(1,143)	(1,216)	(1,290)	(1,430)		
		[0.450]	[0.245]	[0.087]		
Any unemployment	0.384	-0.066	-0.015	-0.050	0.530	897
	(0.034)	(0.054)	(0.052)	(0.043)		
		[0.225]	[0.777]	[0.255]		
Own farm						
Beans harvest kg/ares	8.636	-0.676	-0.285	0.298	0.207	896
	(0.337)	(0.446)	(0.424)	(0.443)		
		[0.133]	[0.503]	[0.503]		
Work days on own farm	12.82	0.415	0.194	-0.053	0.581	897
	(0.27)	(0.353)	(0.386)	(0.352)		
		[0.242]	[0.617]	[0.881]		
Fam ag days on own farm	11.82	0.327	1.035	1.032	0.271	897
	(0.42)	(0.725)	(0.655)	(0.617)		
		[0.653]	[0.117]	[0.098]		
Sold any harvest	0.581	-0.009	0.029	0.035	0.728	897
	(0.035)	(0.045)	(0.052)	(0.046)		
		[0.840]	[0.585]	[0.447]		
Obs	203	231	223	240		

Table A6: Knowledge Sharing Experiment – Balance table for skilled workers attending the training event

Notes: This table shows summary statistics and a test of balance for skilled workers invited to training events in the Knowledge-Sharing experiment. Column (1) provides covariate means and standard deviations for the reference group: villages in the Same Village arm assigned to the training in the Placebo technology. Columns (2) through (4) report regression coefficients relative to the reference group. Column (2) refers to the Same village - Row planting technology arm. Column (3) and (4) refer to the Different village - Placebo technology and Row planting technology training, respectively. Standard errors clustered at the village level are shown in parentheses. P-values are reported in brackets. P-values from Wald tests of joint significance of all treatment arms (relative to the reference group) are reported in Column (5). All regressions control for geographical strata fixed effects, and observations are weighted by the number of individuals in the regression sample by village.

Table A7: Knowledge Sharing Experiment – Balance table for unskilled workers attending the training event

	Rowplanting Comptitor	Rowplanting Non-competitor	F-test Joint sig.	Obs
	(1)	(2)	(3)	(4)
Panel A: Demographics				
Age	37.35	-0.259	0.814	539
	(0.78)	(1.096)		
Male	0.187	0.074	0.099	539
	(0.024)	(0.044)		
Married	0.667	0.041	0.399	539
	(0.029)	(0.048)		
Spouse is migrant	0.438	-0.048	0.365	540
	(0.030)	(0.053)		
No schooling	0.356	0.097	0.024	539
	(0.029)	(0.042)		
Panel B: Household characteristics				
Household size	4.311	-0.161	0.234	539
	(0.121)	(0.133)		
Savings (Fbu)	8,678	-1,128	0.225	539
	(679)	(919)		
Land area (ares)	8.704	-0.645	0.113	539
	(0.363)	(0.400)		
Plots of land	3.734	-0.013	0.943	539
	(0.114)	(0.187)		
Panel C: Past labor supply – ag.				
Work days in ag	14.20	0.129	0.809	539
	(0.30)	(0.530)		
Ag. labor earnings	37,800	319.053	0.852	539
	(909)	(1,697)		
Oth. hh ag. earning	15,303	-1,322	0.335	539
	(1,053)	(1,360)		
Any unemployment	0.607	0.066	0.199	539
	(0.030)	(0.051)		
Panel D: Own farm				
Beans harvest kg/ares	5.964	0.305	0.264	538
	(0.232)	(0.270)		
Work days on own farm	11.57	-0.390	0.243	539
	(0.24)	(0.330)		
Fam ag days on own farm	10.32	-0.978	0.073	539
	(0.38)	(0.535)		
Plots of land	3.734	-0.013	0.943	539
	(0.114)	(0.187)		
Sold any harvest	0.330	-0.066	0.110	539
	(0.029)	(0.041)		
Obs	203	231		

Notes: This table shows summary statistics and a test of balance for unskilled workers who are invited to a training event in the Knowledge-Sharing experiment. Each unskilled worker participated in two training events, in both the Row planting technology and the Placebo technology. One event was with skilled laborers in the same village, the other was with skilled laborers in a different village. We randomized whether the training in Row planting (Placebo) was with skilled workers from the Same (Different) village. Column (1) provides covariate means and standard deviations for the reference group: unskilled workers that were assigned to the Row planting event with skilled workers from the same village and the Placebo training with skilled workers from a different village. Column (2) shows reports regression coefficients relative to the reference group for unskilled assigned to training events in Row planting with skilled from a different village. Standard errors clustered at the village level are shown in parentheses. P-values for equality of coefficients are reported in Column (3). All regressions control for geographical strata fixed effects, and observations are weighted by the number of individuals in the regression sample by village.

	Skilled workers						Unskilled workers					
	Vill. Census	Control	Same Village	Diff. Village	F-test Joint sig.	Obs	Vill. Census	Control	Same Village	Diff. Village	F-test Joint sig.	Obs
Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Demographics												
Age	37.41	39.23	0.227	0.517	0.548	3446	34.06	37.49	-1.506	-0.899	0.058	3581
		(0.33)	(0.540)	(0.471)				(0.34)	(0.625)	(0.623)		
			[0.676]	[0.274]					[0.018]	[0.152]		
Male	0.276	0.263	-0.022	-0.033	0.591	3446	0.339	0.167	-0.016	-0.018	0.688	3581
		(0.013)	(0.031)	(0.032)				(0.011)	(0.021)	(0.022)		
			[0.468]	[0.310]					[0.448]	[0.415]		
Married		0.827	0.008	0.025	0.220	3446		0.753	-0.004	0.038	0.035	3581
		(0.011)	(0.016)	(0.015)				(0.012)	(0.021)	(0.018)		
			[0.592]	[0.090]					[0.842]	[0.036]		
No schooling	0.297	0.328	-0.003	0.004	0.966	3446	0.325	0.382	-0.029	-0.007	0.493	3581
		(0.014)	(0.028)	(0.026)				(0.014)	(0.026)	(0.024)		
			[0.924]	[0.889]					[0.265]	[0.779]		
Panel B: Household characteristics												
Household size		4.527	0.052	0.022	0.828	3448		3.989	0.012	0.006	0.993	3585
		(0.056)	(0.084)	(0.080)				(0.055)	(0.098)	(0.083)		
			[0.540]	[0.784]					[0.905]	[0.947]		
Savings (Fbu)	32,073	21,386	-1,817	-3,342	0.085	3448	22,020	8,792	-181.011	523.183	0.606	3585
		(707)	(1,487)	(1,493)				(340)	(838.095)	(754.060)		
			[0.224]	[0.027]					[0.829]	[0.489]		
Land area (ares)	14.18	13.62	-0.787	-0.656	0.489	3446	11.48	7.708	0.290	0.054	0.709	3581
		(0.26)	(0.662)	(0.752)				(0.142)	(0.377)	(0.386)		
			[0.237]	[0.385]					[0.443]	[0.888]		
Plots of land		4.720	-0.264	-0.386	0.037	3446		3.620	0.032	-0.086	0.587	3581
		(0.061)	(0.144)	(0.154)				(0.050)	(0.130)	(0.136)		
			[0.070]	[0.014]					[0.807]	[0.527]		
Panel C: Past labor supply – ag.												
Skilled work days		7.162	0.117	-0.066	0.802	3436						
		(0.119)	(0.274)	(0.290)								
			[0.670]	[0.820]								
Ag. work days		13.59	0.143	-0.195	0.647	3436		14.28	-0.327	-0.416	0.414	3577
		(0.16)	(0.328)	(0.329)				(0.15)	(0.339)	(0.326)		
			[0.664]	[0.554]					[0.337]	[0.205]		
Ag. labor earnings (Fbu)		38,614	569.762	-1,008	0.451	3436		34,126	100.203	-32.054	0.988	3573
		(532)	(1,393)	(1,274)				(389)	(940.438)	(930.436)		
			[0.683]	[0.430]					[0.915]	[0.973]		
Other hh ag. earning (Fbu)		6,070	21.875	286.340	0.893	3448		13,582	305.477	801.290	0.607	3585
		(361)	(711.811)	(700.348)				(464)	(767.359)	(803.270)		
			[0.976]	[0.683]					[0.691]	[0.320]		
Unemployment days		0.699	-0.052	-0.156	0.146	3377		1.308	-0.017	-0.070	0.769	3500
		(0.043)	(0.081)	(0.082)				(0.053)	(0.102)	(0.099)		
			[0.528]	[0.060]					[0.870]	[0.478]		
Panel D: Own farm												
Beans harvest kg/ares		9.218	-0.148	-1.001	0.053	3442		6.722	-0.085	-0.049	0.974	3574
		(0.198)	(0.438)	(0.472)				(0.145)	(0.373)	(0.378)		
			[0.737]	[0.036]					[0.821]	[0.898]		
Work days on own farm		14.30	-0.332	-0.864	0.028	3446		9.999	-0.264	-0.128	0.838	3581
		(0.14)	(0.326)	(0.321)				(0.136)	(0.444)	(0.428)		
			[0.310]	[0.008]					[0.553]	[0.766]		
Fam labor on own farm days		12.10	-0.214	-0.703	0.324	3440		6.906	0.288	0.101	0.877	3572
		(0.21)	(0.441)	(0.466)				(0.167)	(0.568)	(0.514)		
			[0.627]	[0.134]					[0.613]	[0.845]		
Sold any harvest		0.551	-0.024	-0.057	0.311	3446		0.210	-0.012	-0.026	0.638	3581
		(0.015)	(0.036)	(0.037)				(0.012)	(0.029)	(0.027)		
			[0.508]	[0.127]					[0.665]	[0.346]		

Table A8: Market Effect Experiment – Balance table for entire sample (pooling main and spillover)

Notes: This table shows summary statistics and tests of balance for skilled and unskilled workers in the Market effect experiment. Skilled and unskilled worker averages are pooled across the main and spillover samples. Columns (1) and (7) show descriptive statistics for skilled and unskilled workers obtained from a household census we conducted in a sample of 35. Columns (1)-(6) refer to skilled workers, whereas (7)-(12) refer to unskilled workers. Column (1) and (5) provides covariate means and standard deviations for the reference group: skilled and unskilled workers from villages in the Control arm. Columns (3)-(4) and (9)-(11) report regression coefficients relative to the reference group. Columns (3) and (9) refer to the Same village arm. Columns (4) and (10) refer to the Different village arm. Standard errors clustered at the village level are shown in parentheses. P-values are reported in brackets. P-values from Wald tests of joint significance of all treatment arms (relative to the reference group) are reported in Columns (5) and (11). Columns (6) and (12) display the number of observations.

A.4 Knowledge-sharing experiment

Outcomes	Training time (hrs.)				Passed the test			
Sample	New villages			Old villages	New villages			Old villages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-Competitor X Rowplanting			0.604				0.399	
			(0.142)				(0.061)	
			[0.000]				[0.000]	
Non-Competitor	-0.173	0.451	-0.117	1.408	-0.00360	0.382	0.000567	0.383
	(0.105)	(0.091)	(0.105)	(0.063)	(0.012)	(0.046)	(0.026)	(0.023)
	[0.104]	[0.000]	[0.269]	[0.000]	[0.771]	[0.000]	[0.982]	[0.000]
Rowplanting training			-0.219				-0.938	
			(0.102)				(0.025)	
			[0.035]				[0.000]	
Training type	Placebo	Rowplanting	Combined	Rowplanting	Placebo	Rowplanting	Combined	Rowplanting
Outcome mean for excl. cat.	2.546	2.445	2.546	1.972	0.963	0.0375	0.963	0.0530
Obs.	533	540	1073	1132	533	540	1073	1152

Table A9: Knowledge-Sharing experiment— Treatment effect on training outcomes by training type

Notes: This table shows the training outcome for the knowledge-sharing experiment in columns(1)-(3) and (5)-(7), and of the Market Effect Experiment (columns, 4 and 8). "New villages" refer to the fact that the Knowledge-sharing experiment was implemented in villages where the NGO arrived recently, whereas "Old villages" refer to the fact that the Market Effect Experiment was implemented in villages where the NGO was established. The outcome variable in columns (1) to (4) is the time in hours the skilled worker spent training the unskilled worker, as reported at the end of training by the unskilled workers. The outcome variable in columns (5) to (8) is an indicator for whether the unskilled workers passed the quiz specific to their training. Our pre-registered definition of passing the quiz consists of scoring at least 60% in the quiz. The quiz for the placebo task tests practical knowledge on post-harvest storage and composting techniques.The quiz for rowplanting is a time trial where unskilled workers have to rowplant a plot of a given size within a certain amount of time. Both quizzes take place at the end of their training block and come as a surprise for the unskilled worker. Moreover, they are incentivized: the unskilled workers are told that the individual(s) with the highest score will receive a prize.

The sample in columns (1) and (5) is restricted to to the placebo technogy training sample, whereas columns (2), (4), (5), and (8) refer to row planting events alone. The excluded category comprises villages where event participants came from the Same village. Columns (3) and (7) report the results of a Difference-in-Difference regression, where the excluded category are villages where the incumbents trained unskilled from their village in the Placebo technologies.

All regressions control for geographical strata fixed-effects, the order of the training (whether it was the first or second training for the unskilled workers) and whether there was any disruption during the training (*e.g.* rain, delays or interruptions due to unforseen circumstances). Observations are weighted by the number of individuals in the regression sample by village. We show standard errors in parentheses. Standard errors in columns 1, 2, 4, and 5 are clustered at the village level. Standard errors in the difference-in-difference regressions (columns 3 and 7) are clustered at the unskilled laborer and village level. The p-values are shown in square brackets.

	Training activities					
	Verbally explained	Demonstrated	Taught correct measures	Observed multiple times	Provided feedback/ corrections	Taught spacing tricks
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Competitor	0.0278	0.0163	0.0578	0.321	0.267	0.221
	(0.041)	(0.031)	(0.021)	(0.044)	(0.039)	(0.036)
	[0.503]	[0.603]	[0.008]	[0.000]	[0.000]	[0.000]
Excluded cat. mean	0.258	0.828	0.921	0.476	0.397	0.341
Obs.	540	540	540	540	540	540

Table A10: Knowledge-Sharing experiment – Treatment effect on training activities in Same vs. Different villages in rowplanting training

Notes: This table shows measures of training quality for the knowledge-sharing experiment. The outcome variable in column (1) is an indicator variable for whether the skilled worker verbally explained to the unskilled worker how to row plant. The outcome variable in column (2) is an indicator variable for whether the skilled worker demonstrated to the unskilled worker how to row-plant. The outcome variable in columns (3) is an indicator variable for whether the skilled worker showed the unskilled workers the correct distances to plant between lines and pockets. The outcome variable in column (4) is an indicator variable for whether the skilled worker observed the unskilled worker practicing multiple times. The outcome variable in column (5) is an indicator variable for whether the skilled worker provided feedback or corrections to the unskilled worker. The outcome variable in column (6) is an indicator variable for whether the skilled worker provided instructions on how to space accurately across lines using string. The sample for these regressions comprise all unskilled workers in the Same village and Different village treatments. The excluded category in the regressions is unskilled workers in the Same Village treatment. All regressions control for geographical strata fixed-effects. All regressions, except for columns (4) and (8), also control for the order of the training (whether it was the first or second training for the unskilled workers) and whether there was any disruption during the training (*e.g.* rain, delays or interruptions due to unforseen circumstances.) Observations are weighted by the number of individuals in the regression sample by village. We show standard errors in parentheses. Standard errors in columns (1), (2), (4), and (5) are clustered at the village level. Standard errors in the difference in difference regressions (columns (3) and (6)) are clustered at the unskilled laborer and village level. The p-values are shown in square brackets.

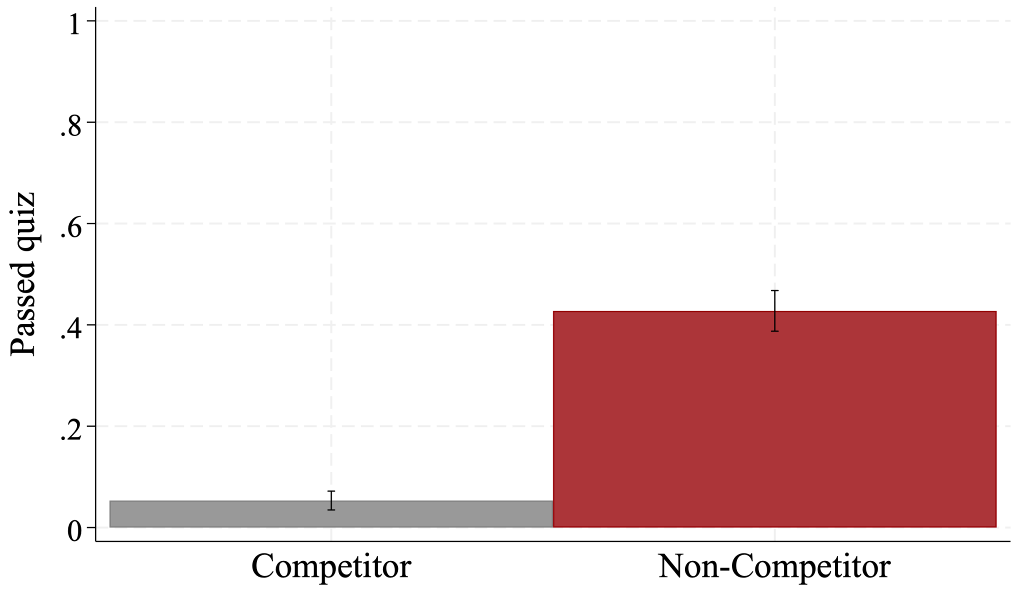
Table A11: Knowledge-sharing Experiment – Treatment effect heterogeneity

	Outcome: Unskilled is trained							
	None	**Skilled characteristics**					**Unskilled charateristics**	
Heterogeneity dimension	**(Bench-mark)**	**< median village farm land**	**≥ median labor earnings**	**≥ median days work rowp.**	**≥ median importance reputation**	**< median training ability**	**> median village farm land**	**Elder woman**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Row planting								
Competitor training	-0.363	-0.317	-0.285	-0.270	-0.427	-0.385	-0.436	-0.386
	(0.041)	(0.045)	(0.052)	(0.048)	(0.066)	(0.055)	(0.054)	(0.044)
		[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Competitor X heterog.		-0.112	-0.144	-0.167	0.096	0.064	0.132	0.235
		(0.050)	(0.068)	(0.054)	(0.064)	(0.095)	(0.054)	(0.068)
		[0.030]	[0.040]	[0.004]	[0.140]	[0.502]	[0.018]	[0.001]
Heterogeneity		0.052	0.113	0.126	-0.086	-0.043	-0.050	-0.175
		(0.051)	(0.062)	(0.052)	(0.062)	(0.090)	(0.053)	(0.079)
		[0.311]	[0.073]	[0.019]	[0.170]	[0.639]	[0.346]	[0.031]
Excluded cat. mean		0.393	0.381	0.370	0.478	0.461		0.444
N	533	533	533	533	533	533	533	533
Panel B: Row planting and placebo								
Row planting	-0.551	-0.583	-0.614	-0.622	-0.485	-0.530	-0.481	-0.531
	(0.055)	(0.055)	(0.058)	(0.063)	(0.082)	(0.066)	(0.067)	(0.056)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Competitor training	-0.015	-0.013	-0.017	0.003	0.041	-0.019	-0.016	-0.013
	(0.026)	(0.031)	(0.028)	(0.037)	(0.030)	(0.028)	(0.036)	(0.027)
	[0.553]	[0.680]	[0.543]	[0.945]	[0.182]	[0.502]	[0.658]	[0.637]
Competitor X row planting	-0.378	-0.340	-0.305	-0.313	-0.499	-0.408	-0.446	-0.399
	(0.061)	(0.064)	(0.067)	(0.071)	(0.089)	(0.072)	(0.077)	(0.064)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Competitor X rowp X heterog.		-0.092	-0.133	-0.119	0.179	0.085	0.127	0.224
		(0.054)	(0.074)	(0.065)	(0.074)	(0.103)	(0.062)	(0.103)
		[0.090]	[0.072]	[0.070]	[0.018]	[0.414]	[0.042]	[0.031]
Heterogeneity		-0.002	-0.006	-0.006	0.021	-0.015	0.060	0.035
		(0.024)	(0.015)	(0.020)	(0.021)	(0.025)	(0.023)	(0.053)
		[0.927]	[0.678]	[0.748]	[0.323]	[0.546]	[0.010]	[0.505]
Competitor X heterog.		-0.003	0.001	-0.029	-0.082	0.013	0.001	-0.007
		(0.030)	(0.028)	(0.042)	(0.034)	(0.033)	(0.033)	(0.067)
		[0.913]	[0.966]	[0.493]	[0.018]	[0.687]	[0.983]	[0.921]
Row planting X heterog.		0.076	0.118	0.125	-0.099	-0.060	-0.127	-0.238
		(0.042)	(0.066)	(0.050)	(0.066)	(0.097)	(0.049)	(0.085)
		[0.070]	[0.075]	[0.014]	[0.138]	[0.541]	[0.011]	[0.006]
Excluded cat. mean	0.707	0.682	0.674	0.650	0.712	0.730	0.717	0.707
N	1062	1062	1062	1062	1062	1062	1062	1062

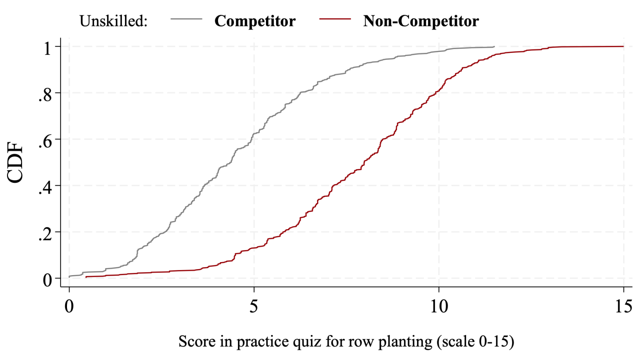
Notes: This table reports the heterogeneous treatment effects on the likelihood that the unskilled worker is trained (*i.e.*, achieved more than the pre-specified threshold on the incentivized quiz) by incumbent (column 2-5) and unskilled (columns 7-8) characteristics. Column 1 reports the benchmark regression, without heterogeneity. Estimates are obtained specification 1. Panel A restricts the sample to the training in row planting alone, while Panel B includes also the placebo technology training. In all the regressions, Heterogeneity is a dummy equal to 1 if the unskilled is paired with a skilled worker with the characteristic specified in the column header. The excluded category is the unskilled in the Non-Competitor training events, trained in row planting, and having Heterogeneity = 0. All heterogeneity dummies in columns 2-7 refer to the median value of that characteristic in the village where the individual resides (*i.e.*, the incumbent's village for columns 2-6, or the unskilled village in column 7). Importance reputation refers to a question regarding the perceived reputational consequences if the skilled trained poorly someone from their village. Training ability refers to answering a question about the self-reported ability to train another individual. Elder woman refers to a woman who is above 50 (robust to other cutoffs).
Standard errors are clustered at the village level. All regressions control for geographical strata fixed-effects, the order of the training (whether it was the first or second training for the unskilled workers) and whether there was any disruption during the training (*e.g.* rain, delays or interruptions due to unforseen circumstances). Demographic controls include: skilled and unskilled gender, age, own farm size, baseline adoption and knowledge of row planting and the placebo technologies (composting, post harvest storage techniques). Observations are weighted by the number of individuals in the regression sample by village.

A.5 Market Effect Experiment

(a) Treatment effect on likelihood of being trained



(b) Distribution of quiz score



(c) Distribution of time spent training

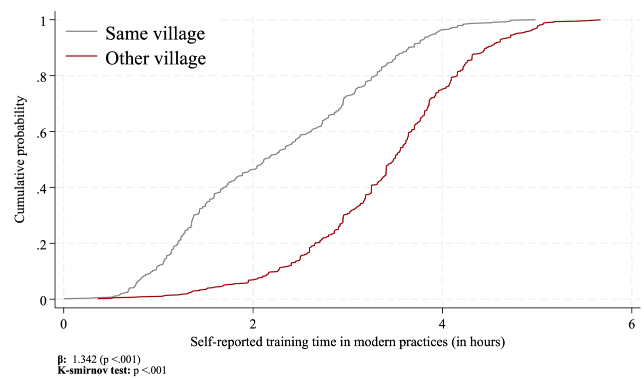


Figure A.3: Market Effect Experiment – Treatment effect on training outcomes

Notes: These figures show differences in training outcomes for unskilled laborers in the Market effect experiment by treatment. Panel A.3a shows the proportion of unskilled workers in the Same Village (panel a) and Different village (panel b) treatments who passed the row-planting quiz. Panel A.3c shows the CDF of unskilled workers scores in the row-planting quiz depending on whether they were assigned to the Same Village or Other Village treatment. Panel A.3b shows the CDF of unskilled workers self-reported time spent training depending on whether they were assigned to the Same Village or Other Village treatment.

Table A12: Market effect experiment – Treatment effect on labor market outcomes for unskilled workers in the spillover sample.

	Labor Earnings	**Daily Wage**	**Work days (Rowp.)**	**Work days (All tasks)**	**Exp. work days next season**
	(1)	(2)	(3)	(4)	(5)
Non-competitor training	488.1	4.618	0.011	-0.017	-0.081
	(1,024.8)	(32.3)	(0.008)	(0.350)	(0.314)
	[0.635]	[0.887]	[0.167]	[0.962]	[0.798]
Competitor training	289.9	31.4	0.018	-0.213	-0.237
	(1,146.0)	(35.3)	(0.008)	(0.395)	(0.302)
	[0.801]	[0.376]	[0.021]	[0.591]	[0.434]
Test (p-value): Competitor train. = Non-competitor train.	0.843	0.394	0.470		
	0.843	0.394	0.470	0.553	0.618
Control mean	40,351.6	2,647.7	0.005	15.4	15.0
N	1717	1687	1717	1717	1717

Notes: This table shows effects on labor market outcomes for the spillover unskilled workers in the Market effect experiment. The outcome variable in Column (1) is total labor market earnings during the agricultural season. The outcome variable in Column (2) is the average wage during the agricultural season. The outcome variable in Column (3) is the number of days that the laborer was employed doing rowplanting. The outcome variable in Column (4) is the number of days that the laborer was employed in any agricultural task. The outcome variable in Column (5) is the number of days that the laborer expects to work in agriculture during the subsequent agricultural season. All regressions include 17 geographical strata fixed effects, and baseline work and demographic characteristics (age, gender, household size, marital status, days of waged agricultural work and expected average daily wage).P-values of the test of equality of the T2-Different village and T1-Same village coefficients are shown. Standard errors are clustered at the village level and reported in parentheses. P-values are reported in square brackets.

	Any plot rowplanted	Share plots majority rowplanted	Num. plots rowplanted
	(1)	(2)	(3)
Non-competitor training	-0.00	0.03	0.00
	(0.004)	(0.015)	(0.062)
	[0.657]	[0.063]	[0.942]
Competitor training	-0.00	0.01	0.01
	(0.004)	(0.015)	(0.063)
	[0.862]	[0.709]	[0.848]
Test (p-value: T2–Different = T1–Same	0.800	0.112	0.903
Control mean	0.989	0.554	2.120
N	3242	3239	3242

Table A13: Market Effect Experiment – Treatment effect on own-farm outcomes for skilled workers

Notes: This table shows outcomes related to own-farm adoption of the row planting technology among skilled workers in the main sample.

The dependent variable in column (1) is a dummy for whether the respondent adopted row planting on at least one plot. Column (2) reports the share of beans plots that were row planted for the major part. Column (3) shows the total number of plots rowplanted. All regressions include 17 geographical strata fixed effects, and baseline work and demographic characteristics (age, gender, household size, marital status, days of waged agricultural work and expected average daily wage).

P-values of the test of equality of the T2-Different village and T1-Same village coefficients are shown.

Standard errors are clustered at the village level and reported in parentheses. P-values are reported in squared brackets.

A.6 Supplementary evidence

	Is trained		Training time		Received feedback
	(1)	(2)	(3)	(4)	(5)
Common employer (recentered)		0.020		0.108	-0.248
		(0.018)		(0.111)	(0.100)
		[0.269]		[0.335]	[0.020]
Rowplanting X Common employer		-0.123		-0.401	
		(0.068)		(0.151)	
		[0.075]		[0.011]	
Rowplanting	-0.936	-0.933	-0.195	-0.186	
	(0.018)	(0.018)	(0.090)	(0.090)	
	[0.000]	[0.000]	[0.036]	[0.045]	
Placebo train. mean	0.963	0.963	2.546	2.546	
Observations	535	535	535	535	267

Table A14: Heterogeneity in training outcomes by whether incumbent shares employers with unskilled