

# The Economic Consequences of Knowledge Hoarding\*

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## Abstract

Strong social ties among individuals can promote knowledge diffusion. In low-income countries (LICs), however, individuals in highly localized markets may also compete for economic opportunities, which can create incentives for incumbents to “hoard” their knowledge. In cross-sectional data from rural Burundi, we first document that the level of social diffusion for a given agricultural technology is starkly higher when its returns are nonrivalrous (i.e., unaffected by how many others adopt it). We then test whether knowledge hoarding inhibits diffusion of a specific high-return technology—row planting, which generates rivalrous rents through a wage premium in the local labor market. In a field experiment covering 223 villages (labor markets), we induce workers skilled in row planting to train unskilled workers. We randomize at the local labor market level whether the unskilled worker is a competitor (i.e., someone from the same labor market). When incumbents are matched with an individual from the same labor market, knowledge transmission occurs only 3% of the time but reaches 43% if the unskilled worker is not a competitor. In contrast, transmission of technologies with nonrivalrous rents (e.g., composting) is high regardless of the unskilled worker’s identity. Consistent with strategic behavior, the incumbent appears to obfuscate important aspects of row planting when training a competitor. We then examine the aggregate and distributional consequences of knowledge hoarding in the village economy. In villages where the unskilled are trained in row planting by noncompetitors, diffusion creates winners and losers: The earnings of the unskilled workers rise by 7% and farm output by 20%. However, skilled workers’ earnings decrease by 6%, and the equilibrium wage for the row-planting task falls by 3%. Reducing knowledge hoarding boosts overall village output: The number of days in row-planting employment is 16% higher, and the share of row-planted fields increases by 27%, suggesting estimated yield gains of 9%. Finally, our results suggest that fear of social sanction is a mechanism that sustains knowledge hoarding among the incumbents, highlighting how social ties can foster social learning but also inhibit it when knowledge diffusion threatens incumbents’ rents.

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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. 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.

The fundamental role of social connections in the transmission of knowledge and skills has long been recognized by social scientists (e.g., Bandiera et al., 2023; BenYishay and Mobarak, 2018). This is particularly true 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 (Rogers, 1983; Foster and Rosenzweig, 2001; Jackson et al., 2012).

In this paper, we posit that 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. 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.<sup>1</sup> The potential for such pecuniary externalities can create incentives to strategically withhold, or “hoard,” knowledge from others.<sup>2</sup> Moreover, the strength of social ties might enable individuals to enforce collective norms among incumbents against information sharing (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 increases in the output of one’s covillagers does not affect one’s own crop prices or revenues. However, as is the case in most developing country settings, labor markets are highly segmented—with the village constituting the local labor market (Jayachandran, 2006; Fink et al., 2020; Breza et al., 2021). This segmentation suggests that, in this setting, incumbents may be likelier to hoard

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<sup>1</sup>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).

<sup>2</sup>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).

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 three years, over 50% of households not directly trained adopted these technologies. In contrast, row planting, 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 row planting 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 (row planting) or placebo technologies for which there is no hiring in the labor market (compost production and post-harvest storage techniques). In this  $2 \times 2$  design, we expect knowledge-hoarding motives to be particularly pronounced when skilled workers train others from their own village in row planting but not under any of the other three conditions. We conduct this experiment with 1073 pairs of individuals across 102 villages where these technologies 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 row planting (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 row planting does not appear to be fully explained by the time that the incumbents spend training across the two arms: Unskilled workers paired with skilled workers from a different village receive only 26.3 more minutes of training. Rather, there is a stark difference in the *quality* of the training: Incumbents paired with noncompetitors are 24.3% likelier to have received feedback or corrections during training. These findings demonstrate both individuals’ ability to transmit knowledge to others and their reluctance to do so among those with whom they compete for jobs.

Remarkably, heterogeneity analysis suggests that individuals with higher stakes in the labor market are likelier to hoard. Unskilled workers paired with an incumbent from their own village who has above-median earnings or days of row-planting employment are 20% ( $p = .040$ ) and 26% ( $p = .004$ ) less likely to pass the proficiency quiz in row planting. Conversely, the likelihood of passing the quiz for an unskilled worker paired with an incumbent who has above-median farming land—implying that she depends less on wage work for her livelihood—increases by 28% higher ( $p = .030$ ). When we look at sharing of the placebo technologies, instead, these economic factors do not appear to be relevant. Rather, we find evidence that social ties further strengthen the rate of knowledge transmission: Among same-village trainer–trainee pairs, unskilled workers paired with an incumbent who is socially close to them are 6% likelier to learn the placebo technology ( $p = .027$ , from a baseline of 93%).

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 row planting: 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 row planting is more than *twice times* 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  $2 \times 2$  design to measure the economic costs of knowledge hoarding in row planting 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.<sup>3</sup> We collect data on labor market outcomes, adoption of row planting, 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' concerns about economic losses are, at least in part, warranted. Finally, at the village level, we observe a 19% increase in the number of days worked in row planting ( $p = .034$ ) and a 23% increase in the plots planted with this technology ( $p = .025$ ), 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 row planting with one unskilled individual would lead to much broader

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<sup>3</sup>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.

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.<sup>4</sup> 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).<sup>5</sup> 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.<sup>6</sup> 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 knowledge diffusion.<sup>7</sup> Our contribution to this empirical

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<sup>4</sup>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) and Conley and Udry (2010) are two foundational contributions.

<sup>5</sup>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.

<sup>6</sup>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 strategic considerations with other-regarding preferences (e.g., altruism, reciprocity).

<sup>7</sup>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.

literature is twofold. First, our unique experimental setting allows us to measure 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 and underscores the role of fragmented markets in shaping both the decision to share and the consequences of hoarding, which are also very relevant for informing policies.

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 reputations (*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](#)) provide 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](#); [Stenius et al., 2017](#); [Gagné et al., 2019](#)).<sup>8</sup> We contribute to this literature 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 region are relatively low.<sup>9</sup> We collaborate with a large international NGO that introduced several agricultural technologies that foster higher 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.

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<sup>8</sup>An important distinction between knowledge hoarding and lack of information transmission is that the former has some elements of strategic behavior. See also [Bilginoglu \(2019\)](#) for a review of the literature on knowledge hoarding.

<sup>9</sup>In 2018, average maize and bean yields 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](#)).



## 2.1 Row planting: A profitable but time-sensitive technology

**Main characteristics.** We study the diffusion of row planting, an agricultural technique that requires farmers to i) till the land and construct well-ordered seedbeds, ii) sow in parallel lines spaced by the same distance throughout the field, and iii) apply complementary inputs, such as fertilizer and compost, in the seedbeds in a specified order.<sup>10</sup> Additionally, microdosage of fertilizer is a complementary technique that requires farmers to apply fertilizer in a particular quantity and order. The alternative to row planting is broadcasting, whereby farmers throw seeds and other inputs semirandomly on their fields.

Row planting is clearly a more labor-intensive task than broadcasting. This has important implications in settings—such as Burundi—where agriculture production 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.<sup>11</sup>

**Returns to row planting: Own-farm adoption and labor market.** Agronomic studies estimate that row planting increases crop yields by 30–100% (Vandercaesteelen et al., 2020; Jeyabalasingh and Bayissa, 2018; Mihretie et al., 2021). The reasons for the yield gains from row planting 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 (Vandercaesteelen et al., 2020). Cefalà et al. (2024) find that adoption of row planting is profitable in this setting.<sup>12</sup>

In addition, knowledge of row planting 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 row planting requires approximately twice the number of man-days to plant the same area as traditional methods.

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<sup>10</sup>The introduction of high-yielding, more more labor-intensive technologies in agriculture is very 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 transplanted 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.

<sup>11</sup>The widespread notion that delays to planting decrease yields is consistent with a long agronomic literature (Howard et al., 2003; Kruger, 2016). In Burundi, this message is reinforced by the government, which provides geography-specific windows of time for planting.

<sup>12</sup>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 row planting in an additional 1.3 fields, increasing farm profitability by 14%.



**Mastering row planting: Key skills and sources of learning.** Learning row planting requires both technical and practical knowledge. The technical information includes how to exactly space the rows and seedbeds. This notion could potentially be inferred through observation, but farmers in this context appear to have limited ability to abstract the geometrical pattern from observation alone.<sup>13</sup> Furthermore, some specific aspects—such as the depth of the seedbeds and how to apply inputs (e.g., fertilizer and compost) in them—are unobservable once the fields are planted.

Farmers also require explicit training to learn some expedients for row planting in fields on uneven or sloping terrains—typical characteristics of farms in Burundi. While in principle farmers can acquire this knowledge through learning-by-doing, most experienced farmers mention that they first learned these expedients through direct NGO training. Finally, the limited time window available for planting requires that farmers be able to perform row planting accurately at a fast pace, which appears to be a prohibitive constraint for farmers without adequate training and practice.

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.<sup>14</sup> 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 row planting 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 focus on 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 com-

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<sup>13</sup>This limitation may be partly explained by the fact that 40% of nonadopters never attended school and only 10% studied beyond primary school (see Table A5).

<sup>14</sup>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.

post 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 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 knowledge: 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. Instead, modern compost production provides farmers with an affordable source of organic fertilizer, which improves the soil’s mineral content and, thus, its fertility.<sup>15</sup> 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 row planting 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 decreasing returns in the output market. However, we expect decreasing returns to be unlikely because only 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

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<sup>15</sup>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](#)).

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 row planting to NGO members in the villages, and just like row planting, they require some training to learn. However, while all these techniques require both theoretical and procedural knowledge, row planting relies more heavily on the latter (Ryle, 1949; Glaser, 1984; McCormick, 1997). Thus, learning how to row plant may require more active experimentation and practice, providing greater scope for feedback from the trainer to enhance performance. The subtleties involved in learning how to row plant described in section 2.1 may make it easier to withhold or obfuscate key aspects of the process from a competitor.

## 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.<sup>16</sup> 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 24 villages

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<sup>16</sup>In a full household census we conducted in 24 control villages, we find that the average number of households is 175.

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 row planting on their own farm (88% compared to 7%). Furthermore, skilled workers are almost exclusively NGO members.

## 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 24 *Control* villages from the market effect experiment and other nonstudy villages.<sup>17</sup> As Figure 1 shows, adoption rates among the NGO members are high for all the technologies: 91% of member households report having adopted row planting in at least one field, 64% produce compost, and 60% 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: 61% report adopting antimold products and 36% modern composting, and 36% report triaging their harvest. This suggests that some members are willing to incur some costs (e.g., time, hassle) to teach others.<sup>18</sup> However, fewer than 10% of the nonmember households report adopting row planting 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 row planting because of the wage premium that they would obtain from it in the labor market.<sup>19</sup>

<sup>17</sup>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.

<sup>18</sup>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).

<sup>19</sup>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 row planting remains open.

**Social network and diffusion.** We document the presence of social capital among skilled and unskilled workers through a network survey in the 24 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 unwillingness to share is specific to the employment domain.

## 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<sup>20</sup> 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 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:

Competitor training. In this treatment, the incumbent and unskilled workers are individuals from the same village (labor market).

Noncompetitor training. 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:

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<sup>20</sup>For instance, telling the unskilled worker that they would return and train her later.

(ii) Nature of training:

R–Row-planting technology. In this experimental arm, at the training event, the incumbent is encouraged to train the unskilled in row planting.

P–Placebo technology. 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, row planting, 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 row planting 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–Row planting*, *Noncompetitor training–Row planting*, *Competitor training–Placebo*, *Noncompetitor 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.<sup>21</sup> 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 row planting 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 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 row planting 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–Row planting* and *Noncompetitor training–Rowplanting*.

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 *Noncompetitor 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–Row planting*) and do not (*Noncompetitor–Row planting*) have knowledge-hoarding motives.

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<sup>21</sup>The unskilled workers from *Competitor training* villages do not attend any event.

### 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.<sup>22</sup> 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

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<sup>22</sup>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.

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, 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 row planting 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.<sup>23</sup> We then conduct a plot roster where, for each

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<sup>23</sup>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.

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.<sup>24</sup> To measure profits, we also elicit 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

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<sup>24</sup>Because crops come in different varieties with different prices, we multiply the quantity harvested by the farmer's 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.

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:

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

in this specification  $\text{Rowp}_{v(i)}$  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  $\gamma_r$  are cluster fixed effects. Therefore,  $\beta_3$  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:

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

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  $\beta_5$ , 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:

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

where  $g$  indicates the subsample the individual  $i$  belongs to (Skilled or Unskilled, Main or Spillover),  $\text{Same}_{v(i)}$  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  $\text{Diff}_{v(i)}$  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  $\gamma_r$  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 (skilled 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:

$$w_{ij} = \gamma_1 \text{Same} \times \text{Rowp}_{ij} + \gamma_2 \text{Non-competitor} \times \text{Rowp}_{ij} + \text{Rowp}_{ij} + \sum_{t=1}^T \text{Same} \times x_{ij} + \sum_{t=1}^T \text{Non-competitor} \times x_{ij} + \sum_{t=1}^T x_{ij} + \epsilon_{ij} \quad (4)$$

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 with the unskilled when the unskilled individuals are perceived as potential competitors, which depends both on the technology being shared, as well as the identity of the individual.

Figure 2a shows the treatment effect of being paired with a skilled worker from a same or different village on the likelihood of being trained at the event in both technologies. We classify an individual as having been trained if they receive a score of at least 60% in the incentivized quiz provided at the end of the event, a measure we pre-registered. The first two bars show the likelihood of being trained in row-planting: unskilled workers trained by Non-competitor incumbents are 38.2 percentage points more likely to become trained—, from almost no unskilled workers who are trained in the Competitor training events (in total 3% of 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 training 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 the sharing of the placebo technologies is encouraged, however, we find dramatically different results: both the Competitive and Non-Competitive 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 Non-competitor training, the incumbents spent more time training the unskilled workers regardless of the technology, but that this only led to a difference in the row planting training because of ceiling effects in learning the placebo technologies. However, Figure 2b, which shows the cumulative distribution function for the quiz score in the placebo technology, suggests that there is still a substantial fraction of individuals that did not fully learn. Furthermore, regardless of the threshold we choose, there is no difference in the quiz score between unskilled trained by competitor or non-competitor incumbents (*i.e.*, residing in the same or a different village).<sup>25</sup>

Second, it appears that, while there the incumbents spent more time training the Non-competitive unskilled workers, it appears that this effect is too small in magnitude to drive the large differences we observe in training outcomes. Figure 4a, which plots the average amount of time that the unskilled workers reported having being trained, shows that in the Non-competitor training treatment, skilled workers spend on average 28 more minutes training unskilled workers in row planting ( $p < 0.001$ ) than they do in the Same Village treatment. However, this increase is off of a base of 2.5 hours, making it unlikely that this 19% increase in time spent training leads to such dramatically different training outcomes.<sup>26</sup>

Instead, Figure 4b suggests that a major difference between workers in the Competitor training and Non-competitor training village 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 Non-competitor training (*i.e.*, residing in a different village than the unskilled) were 67% more likely to provide feedback and correct the unskilled workers ( $p < .001$ ). These results are consistent with the idea that learning how to rowplant involves procedural learning, which is obtained through repetitions

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<sup>25</sup>Instead, Figure 2c shows that the distribution of the row planting quiz scores for Non-competitive unskilled workers first-order stochastically dominates those of the competitive unskilled workers (residing in the same village as the incumbents).

<sup>26</sup>For the placebo technologies, the skilled workers in the Non-competitor training treatment spend a little *less* time training in the placebo technology (12 minutes,  $p = .073$ , see column 1 in Table A9).

and practice (see Section 2.2). Furthermore, we also see that Competitor training skilled workers convey theoretical knowledge about row planting: over 90% of 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 at 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 4b, last column sets on the right). This confirms once more that 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 more likely to hoard. Table 1 shows the results using the specification in Equation 2 on the likelihood that the unskilled individuals passes the quiz, where we restrict the sample to the training in row planting (*i.e.*,  $\text{Rowp}_{v(i)} = 1$ ), but we refer to Panel B in Appendix Table A11 for the full sample.

We leverage the fact that skilled and unskilled workers were randomly matched at events to look at whether, within the same village treatment, skilled workers who stood to lose more from training were less likely to train. We find evidence consistent with this. Unskilled workers paired with a competitor (*i.e.*, incumbent from the same village) who has less cultivable land, (column 2), who derive more earnings from the labor market (column 3), and who do more work in the labor market row-planting (column 4) consistently receive less training. Unskilled workers paired with a skilled worker who has below median farm land (*i.e.*, is more likely to work as a laborer), are 14. percentage points less likely to be trained, as compared to 31 percentage points for skilled workers with above median landholdings ( $p=0.023$ ). Similarly unskilled workers paired with skilled workers with above median earnings are 16.7 percentage points less likely to be trained than those paired with skilled workers with below median earnings ( $p=0.040$ ), with similar magnitudes if we split the sample by whether the skilled worker has above median days spent doing work row-planting ( $p= .004$ ). 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 train more those individuals who are less likely to work in the labor market, as proxied by the amount of land they own (column 7) or by demographics (column 8): when the Competitor unskilled has above median farm land, they are 13 percentage points more likely to be trained ( $p=.018$ ), and elderly women are 23.5 percentage point more likely to learn how to row plant (column 8,  $p=0.001$ ).

Taken together, our findings document that incumbents hoard knowledge when sharing maybe consequential to their market returns. Specifically, workers i) withhold information from the un-

skilled, ii) they do this only for information that could erode market returns and iii) the willingness to share information is lower for the skilled who plausibly stand to lose more from sharing. In the next section we turn to understanding 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 as compared to when they are paired with skilled workers from a different local labor market (Non-competitor training arm). We turn to the Market effect experiment to test whether removing the knowledge hoarding motive is consequential for local economic activity, as well as measuring the incidence of these changes on the unskilled and incumbents.

**First-stage: Training results.** We start by showing results for our first-stage: specifically we test whether unskilled workers learn differentially when they reside in the same, or in a different, village as the incumbent. Results for 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 workers in villages who attend the event with skilled workers from the same village are trained, this 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 found 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 worse training outcomes result from moral hazard in training. Unskilled workers in the Non-competitor training arm are 74% and 57% more likely 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 treatment effects on labor market outcomes for the unskilled invited to the training event. The training event significantly increases the labor market earnings (during the agricultural season) of the unskilled assigned to the Non-competitor 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 in Non-competitor 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 Non-competitor 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  $0.429$ , respectively). Instead, the treatment effect on agricultural earnings for unskilled workers in the Non-competitor training treatment reflects a substitution toward row planting and complementary tasks and away from other tasks during the agricultural season. Unskilled workers in the Non-competitor 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 in Competitor training villages ( $p$ -value of the difference  $< .001$ ).

Consistent with training in row planting having labor market returns, wages of unskilled workers in Non-competitor training villages rise as compared to the unskilled in Control villages. Column 2 shows that Non-competitor training villages workers earned an average daily wage 5.2% larger than Control workers ( $p = .001$ ), compared 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, Non-competitor training workers became more optimistic about their future employment prospects: they expect to work 8% more days than Control workers (column 5,  $p = .001$ ). This is not the case for Competitor training workers, who do not expect an increase with respect to the Control group ( $p = .749$ ), suggesting that Competitor training workers may have realized that their knowledge was still inadequate.

**Technology adoption on their 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 caused 72.9% of trained unskilled workers in Non-competitor training villages to adopt row planting on at least one of their plots (column 5), an effect 40.7% larger than in Competitor training villages.<sup>27</sup>

### 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 were exposed to training do incur a cost from greater information diffusion.

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<sup>27</sup>The fraction of treated unskilled workers who adopt row planting in the Control group is negligible.

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

We find evidence that the increased skilled labor supply in Non-competitor training villages caused a meaningful decrease in the earnings for skilled residing in those villages. Column 1 shows that, on average, they earned 6% less than the Control group ( $p = .014$ ), which is also a significant reduction with respect to incumbents residing in Competitor training villages ( $p = .041$ ). Non-competitor training skilled workers on average 5.4% fewer days in the agricultural labor market compared to the Control skilled workers ( $p = .036$ ). This negative treatment reflects for Non-competitor training incumbents reflect both a decrease in days of work in rowplanting and in their average daily wage. Column 3 shows that they experienced a 13.2% decrease in the days of work in row planting performed for their employers compared to the Control group ( $p < .001$ ). The effect in Competitor training villages, instead, was much smaller in magnitude: skilled workers worked on average 6.9% fewer days to do row planting than the Control group ( $p = .073$ )—an effect statistically different from Non-competitor training skilled workers ( $p = .045$ ).

Finally, it is important to note that the Non-competitor training skilled workers' earning loss likely reflected a short-term effect. In fact, it is plausible that the 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 could have learned about the unskilled workers 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 through 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 Non-competitor training villages, an increase of 13% on the number of days worked in pure control villages ( $p < 0.001$ ), whereas days worked in T1-Same Village villages increases 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 row planting for employers on, which increase by 0.29 and 0.13 in T2 and T1 villages respectively, corresponding to a 25% increase ( $p < 0.001$ ) and 11% ( $p < 0.001$ ), respectively (test of difference between treatments  $p < 0.01$ ).<sup>28</sup>

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<sup>28</sup>To construct this measure, we ask respondents, for each employer they were hired from, the number of plots they worked on, and the number of these plots where they performed the row planting tasks. To avoid double counting, we also asked the total number of workers that worked on each plot, and assigned an equal fraction to each worker.

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 (Col 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 days worked and earnings for laborers overall (Col. 4 and 5).

## 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 in row planting an individual from a different village than one from their own village, but are as likely to train them in our placebo techniques.

**Pre-existing knowledge of placebo techniques** One concern is that we cannot detect a treatment effect in 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. As for the first concern, it is mitigated by design because we ran these tests in villages where the NGO only recently expanded, so that it is unlikely that knowledge 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 some knowledge questions related to placebo techniques to skilled and unskilled workers at the beginning of the training event.

**Reputational consequences** Chandrasekhar et al. (2022) suggests that the potential for negative reputational consequences of sharing information about a technology may prevent some individuals from sharing with others. In our context, some skilled workers may fear that their reputation as someone who masters the technology could be damaged if they failed 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. The prediction of the sign of the treatment effect for these individuals is ambiguous: they may *increase* their effort to give a good impression to the trainee, or they may abstain from training.

To understand whether this is a cause of concern, we asked skilled workers at baseline whether they thought 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 based on whether the respondents expressed this concern. Column 3 in Table 1 shows that unskilled paired with incumbents who express more concerns about their reputation are *more* likely to be



trained if they were in the Competitor training arm than in T2, suggesting that, if anything, reputational concerns bias the results against our hypothesis. Moreover, we also do not observe heterogeneous effects by the self-reported training ability ( $p = .302$ , column 9).

**Greater returns from training outsiders** Skilled workers might perceive there are greater returns from sharing knowledge with an unskilled worker from another village because it may open up more trading opportunities. To allay this concern, in the planting survey (approximately four months after the training events) we asked skilled workers about their tradings outside of their village. First, we document that there is little evidence of any tradings happening across villages. Second, we do not find that incumbents paired with unskilled workers from a different village were any more likely to have initiated this effort.

## 6 Discussion: Costs of Sharing Knowledge

Section 5 shows that an increase in the supply of skilled laborers increases overall agricultural productivity but did so with an uneven incidence: while unskilled workers gain after learning row-planting, incumbent laborers incur earnings losses from training.

The losses we estimate, however, result from shifting the economy from one equilibrium to another, and therefore 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, consistent with prior studies, there might be reasons that individuals *do* want to actively share information with others, for instance if they are altruistic or expect reciprocation, or if they feel pressure from their kin. This, in turn, raises the question as to how the equilibrium does not unravel.

We provide two explanations that suggest that incumbents perceive that the cost of sharing is large, rationalizing their decision not to share even with a limited number of individuals.

**Beliefs about further diffusion.** First, 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 rowplanting techniques. We find that only 5% of skilled workers believe that their earnings would decrease as a consequence of the additional worker being trained. However, 78% of skilled workers agreed with the statement that that training another individual would result in a much greater diffusion of knowledge to other unskilled individuals. They also believe a larger 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.

**Social sanctioning.** Consistent with recent work on collusive norms in low-income settings (Breza et al., 2019; Banerjee et al., 2022),<sup>29</sup> we hypothesize that the implicit coordination among skilled workers to stop the diffusion of knowledge of row planting outside of those initially seeded may contribute to the low-sharing equilibrium. To corroborate this hypothesis, we run a supplementary data collection exercise with 347 skilled workers from the Market 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 respondents agree with the statement (left-most column in Figure 5a). Moreover, almost 80% stated that they would expect some form of social sanctioning from other incumbents if they found out (Figure 5b). The sanctions range from the work to the social domain: the most common form of sanctioning they mention is exclusion from future work opportunities (37%), and badmouthing (either with employers or with other friends). Interestingly, social norms appear to be stronger in smaller villages, where perhaps it is easier to coordinate. Figure 3b shows that the proportion of incumbents who mention expecting any sanctions if another incumbent trained an unskilled decreases the larger the market size. This could reflect both either a more elastic labor demand, or more ability to coordinate when the size of the group is small. This result is also consistent with our previous evidence that hoarding is more prevalent in smaller markets (Figure 3a). Overall, this suggests that the expectations of social sanctioning appears to be a strong deterrent to knowledge sharing.

Furthermore, while it is too early to assess whether the expectations of wide diffusion from the newly trained individuals 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 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 5a). This suggests that they may themselves opt to hoard their knowledge.

**Misinformation.** We document another strategy that incumbents may adopt to curb the demand for learning. 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. Indeed, at baseline we document that the unskilled workers estimate that, on average, it takes 40% more days to learn how to row plant than what incumbents state (4.08 vs. 2.73), with 10% of individuals who think it takes a week or more. To understand whether these biased beliefs are the result of their interactions with incumbents, we organize 95 focus groups led by enumerators where skilled workers are invited to discuss several aspects of modern agricultural practices, including the time it takes to learn to row plant. From the perspective of the participants, these focus groups were like any focus group activity—a way for the research team

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<sup>29</sup>Banerjee et al. (2022) finds evidence consistent with the existence of anti-competitive norms among Indian vegetable sellers. In the context of rural labor markets, Breza et al. (2019) shows the existence of a social norm against under-cutting wages, which affects individual labor supply choices.

to acquire information about agricultural practices.<sup>30</sup> The key difference is that, in a random half of the focus groups, we also invite unskilled workers residing in the same village as the incumbents to be in attendance.

Figure 6a shows that, when unskilled workers are not present, skilled workers state that learning row planting takes around 3 days, which is somewhat close with what we observe in practice. Strikingly, when unskilled workers are present, skilled workers state that it takes more than *twice* the amount of time that it takes to learn row planting. This dramatic difference suggests that skilled workers are intentionally attempting to curb unskilled workers' demand for learning the skills.

Finally, Figure 6b shows that, after being exposed to the training with Non-Competitor incumbents, unskilled workers revise downwards their expectations about the time it takes to learn: the distribution of beliefs in the Non-competitor arm is statistically different from that of the Competitive training arm (the p-value of the Kolmogorov-Smirnoff test for the equality of the distributions is 0.009).

## 7 Conclusion

This study employs two field experiments in 223 Burundian villages to investigate whether incumbents limit the diffusion of new agricultural technologies if they perceive their returns to 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 from a different village, and ii) whether the training was in row planting or in composting (a non-rival technology). We find that skilled workers are 40 percentage points less likely to share row planting knowledge with laborers from their own village compared to those from another village. However, they shared knowledge of skills with non-rival returns equally with both groups. Knowledge hoarding leads to lower economic returns: villages exposed to knowledge hoarding produce less in aggregate. However, reducing knowledge hoarding entails a cost for incumbents.

This paper has important implications for how we consider the diffusion of technologies in low-income countries. Classic models of growth consider ideas to be non-rival (Romer, 1990a). The literature on agricultural technology diffusion has also argued that there are limited strategic incentives to hire information (Foster and Rosenzweig, 1995; Conley and Udry, 2010). However, recent literature has pointed out the possibility of negative spillovers from increased diffusion in markets with inelastic supply or demand (Duflo et al., 2023).

Our paper builds on this insight and makes two contributions. First, it highlights that in segmented

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<sup>30</sup>It is not unusual for NGO members to participate in focus groups with the NGO staff for similar discussions on new products or trainings.

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 in turn builds on a recent and important literature suggesting that costs faced by incumbents from transmitting information about new technologies can meaningfully slow the diffusion of such technologies (BenYishay and Mobarak, 2018; Chandrasekhar et al., 2022).

Many open questions remain. A deeper understanding is required of the effects of diffusion at scale of new technologies and the local price effects (of any) of this diffusion. Moreover, alternate behavioral sources of rivalry (such as peer comparison) may also produce rival returns for incumbents. Finally, we require a deeper understanding of which policies, such as improved market access, might mitigate knowledge hoarding. We leave these questions for future research.

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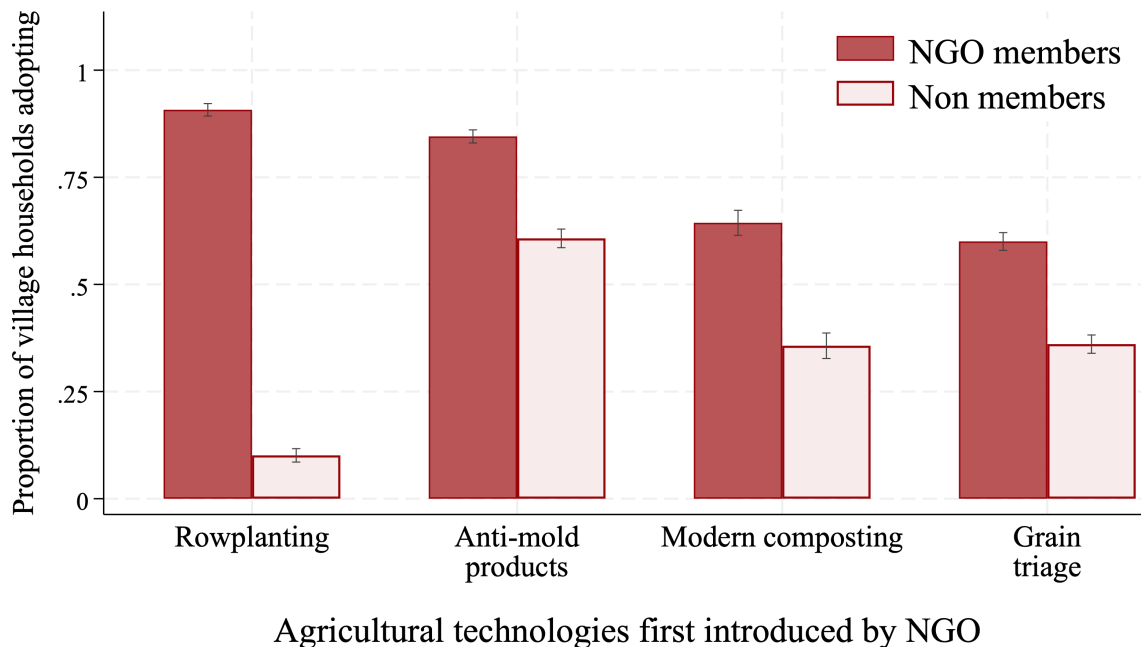


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# Figures

Figure 1: Technology diffusion from village census

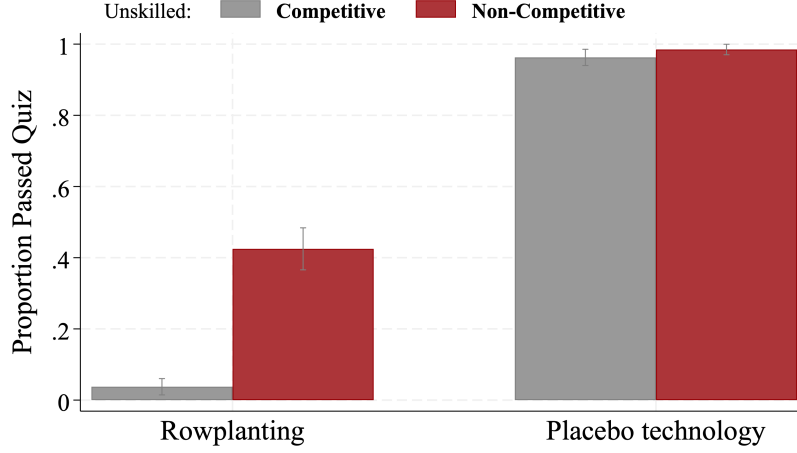


*Notes:* This figure shows the adoption rate of different agricultural technologies among village households split by whether one of the family members belongs to the NGO or not. Given the limited availability of other sources of learning, any diffusion among non-members must have likely occurred through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

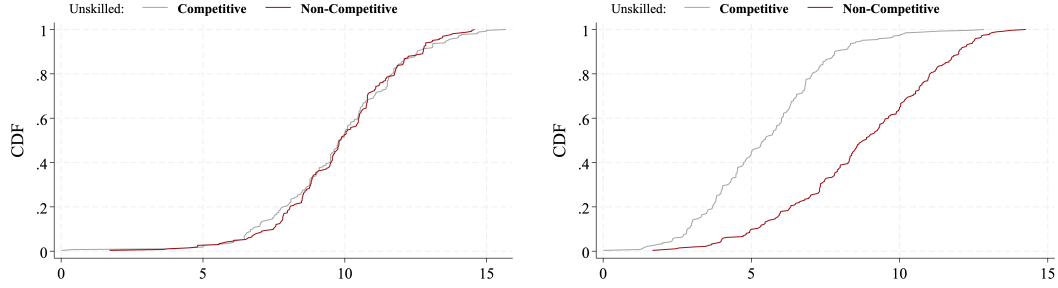
We focus on three technologies that were originally introduced in these villages to its members by a large agricultural NGO, namely the production and use of compost, post-harvest grain storage techniques, and rowplanting. While the knowledge latter—rowplanting—grants a wage premium in the labor market, there is no hiring to perform composting or post-harvest storage techniques, making their returns non-rival. 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 non-rival 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, row planting consists of planting seeds in evenly-spaced seedbeds on the field. The first set of bars refers to the share of households that adopted row planting 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) Time the skilled spent training by treatment arm

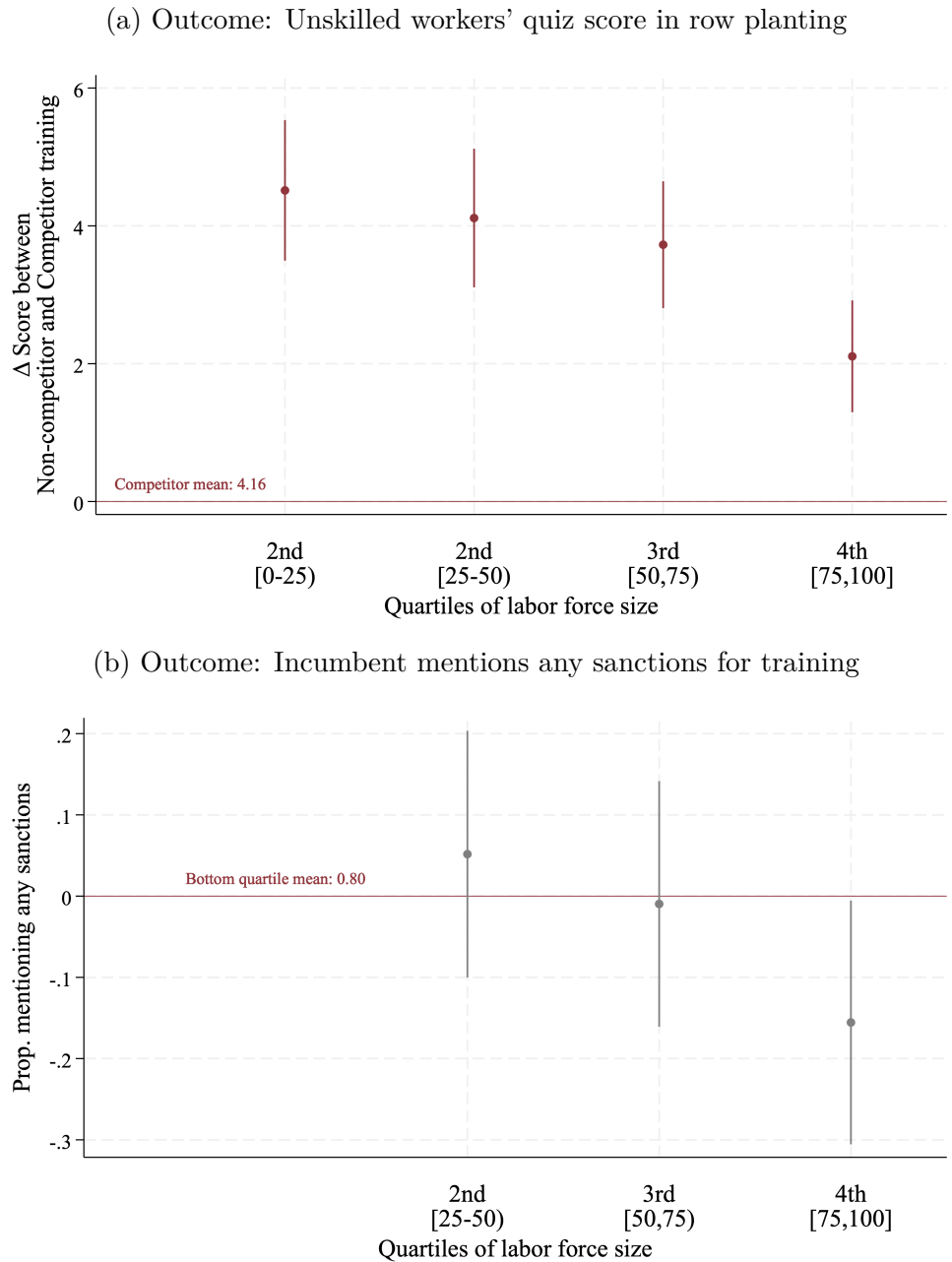


(b) CDF of score in the placebo technology quiz (c) CDF of score in the rowplanting practice quiz



*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 rowplanting 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 rowplanting quiz. 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.

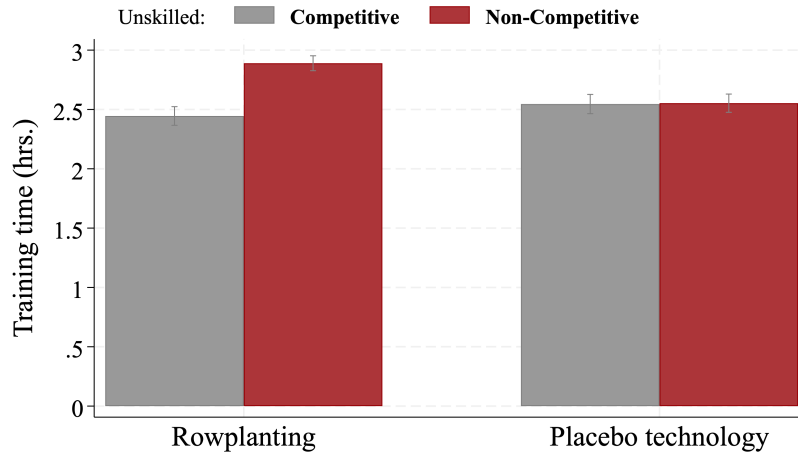
Figure 3: Market Experiment – Treatment effect heterogeneity by labor market characteristics



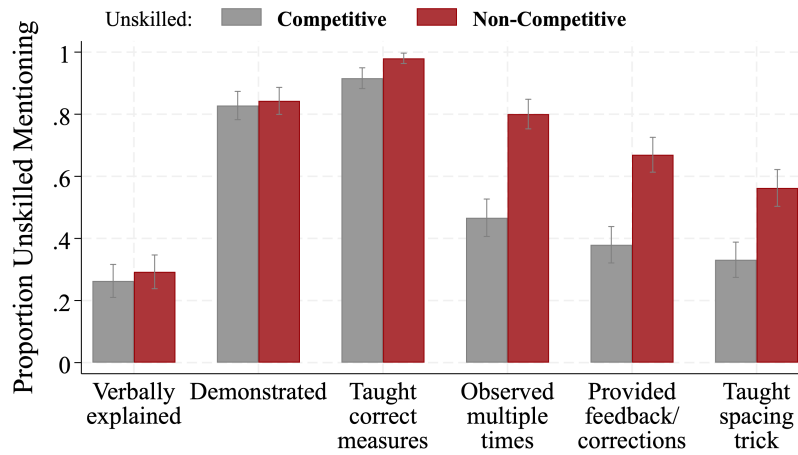
*Notes:* This figure shows the heterogeneity in the Market experiment outcomes by the labor market size. Panel 3a shows the treatment effect heterogeneity on the unskilled workers' quiz score in row planting by quartiles of the labor force size. We plot the coefficients of the interaction terms between 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. The sample comprises of all the unskilled workers that attended the training event in the Market experiment (N=2090). The regression is weighted by the number of observations (event attendees) in each village. Panel 3b 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 5). The excluded category are the villages in the bottom quartile (0-25<sup>th</sup> 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 4: Knowledge sharing experiment: Training effort and quality

(a) Time the skilled spent training by treatment arm

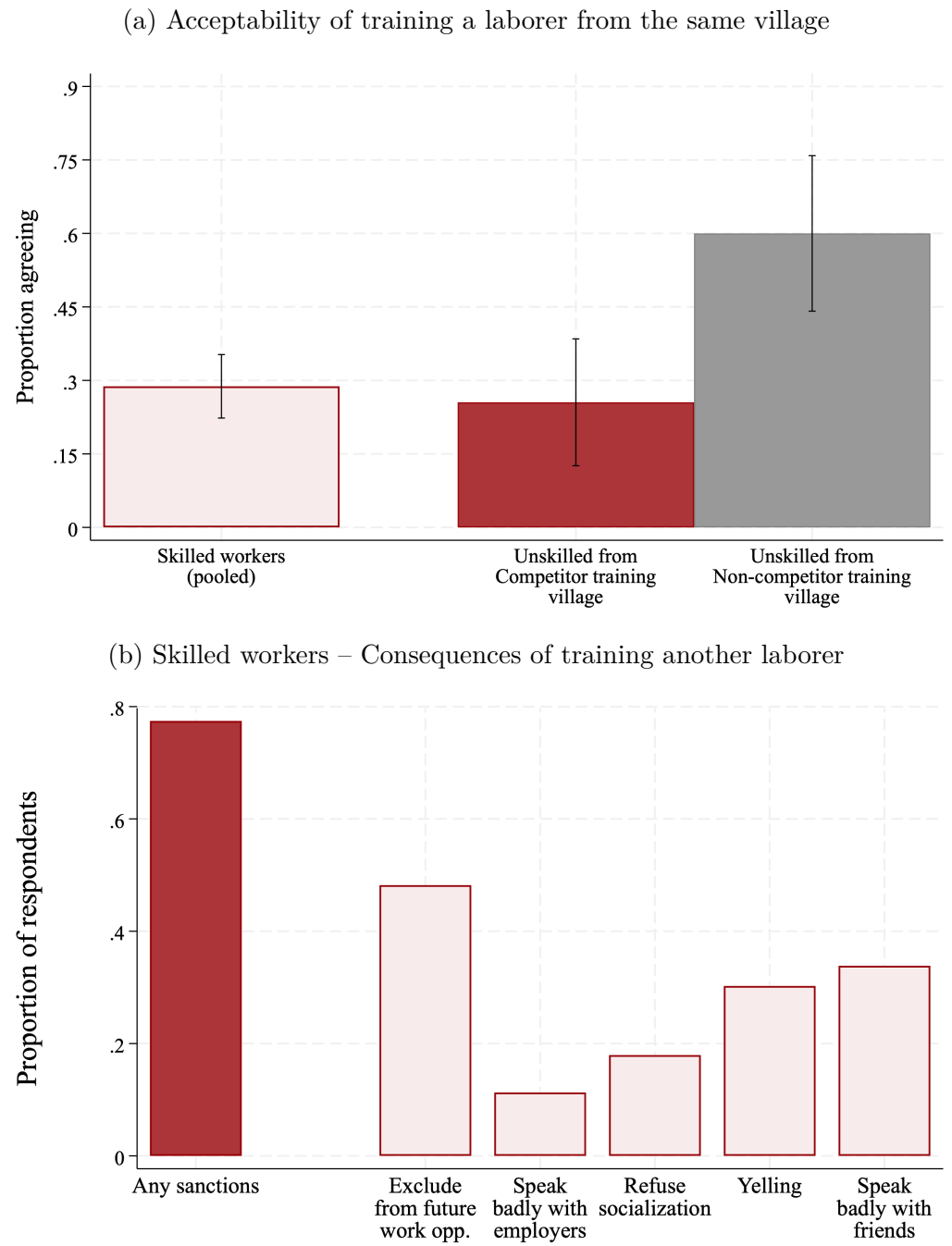


(b) Activities performed by the skilled worker during the rowplanting training



*Notes:* This figure presents measures of the quantity and quality of training given by skilled workers in the Same village and Different village treatments. Panel 4a of this figure shows the amount of time skilled workers spend training unskilled workers at the training event by treatment arm, while panel 4b shows the actions performed by the skilled worker while training the unskilled in rowplanting. Both measures are 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 a skilled worker from a different village, while the red bars refer to unskilled workers trained by a skilled worker in the same village. Both figures show the raw means for each group. See Appendix Tables A9 and A10 for regression results.

Figure 5: Norms around training someone unskilled from the same village in row planting

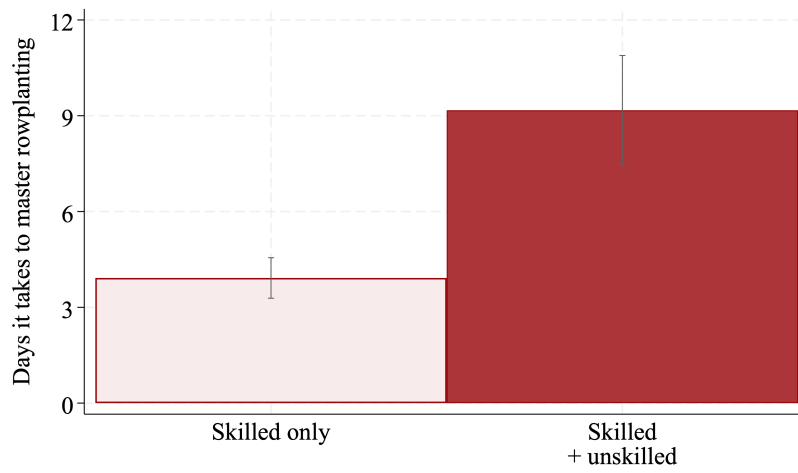




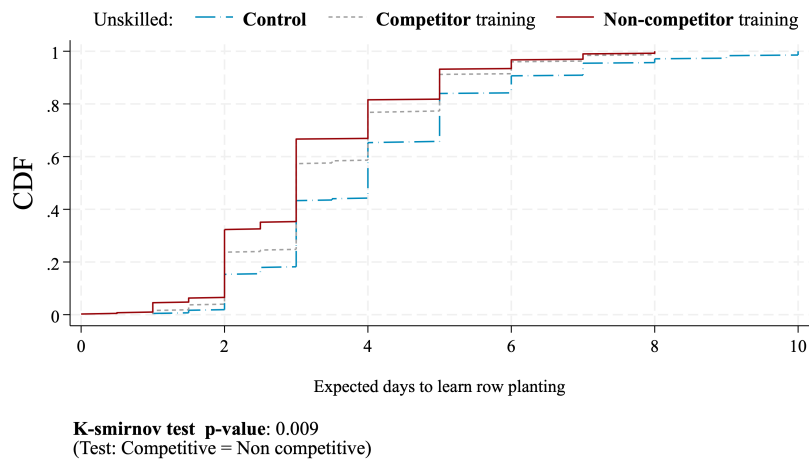
*Notes:* This figure presents evidence of the stated acceptability and expected sanctions for training an unskilled worker in the same village. Panel 5a shows the fraction of respondents agreeing with the statement that it would not be acceptable for a skilled worker to train an unskilled worker in the same village in row planting. The first bar on the left comprises the response of a sample of 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 skilled workers are pooled across treatment arms. 95% confidence bars are reported. Standard errors are clustered at the village level. Panel 5b reports the expected social sanctions if one trained an village unskilled worker in row planting. 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 6: Misinformation – Beliefs about the time it takes to learn

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



(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 rowplanting. These statements were collected during 95 focus groups held with 6 to 9 village laborers, where we randomized whether the group was formed by skilled workers exclusively, or by a mix of skilled and unskilled workers. An enumerator asked a series of questions about the modern planting practices, facilitated the discussion, and noted each participant's answers. The 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<sup>th</sup> percentile.

# Tables

Table 1: Knowledge-sharing Experiment – Treatment effect heterogeneity

Heterogeneity dimension	Outcome: Unskilled is trained							
	None	Skilled characteristics					Unskilled characteristics	
	(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)
Competitor training	-0.363 (0.041) [0.000]	-0.310 (0.044) [0.000]	-0.285 (0.052) [0.000]	-0.270 (0.048) [0.000]	-0.427 (0.066) [0.000]	-0.385 (0.055) [0.000]	-0.436 (0.054) [0.000]	-0.386 (0.044) [0.000]
Competitor X heterog.		-0.118 (0.050) [0.023]	-0.144 (0.068) [0.040]	-0.167 (0.054) [0.004]	0.096 (0.064) [0.140]	0.064 (0.095) [0.502]	0.132 (0.054) [0.018]	0.235 (0.068) [0.001]
Heterogeneity		0.050 (0.051) [0.337]	0.113 (0.062) [0.073]	0.126 (0.052) [0.019]	-0.086 (0.062) [0.170]	-0.043 (0.090) [0.639]	-0.050 (0.053) [0.346]	-0.175 (0.079) [0.031]
Excluded cat. mean	0.425	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 2-5) and unskilled (columns 7-8 ) characteristics. Column 1 reports the benchmark regression, without heterogeneity. Estimates are obtained using the specification 1. We restrict the sample to the training in row planting 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 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 unforeseen 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.

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

	Labor Earnings	Daily Wage	Work days (Rowp.)	Work days (All tasks)	Exp. work days next season
	(1)	(2)	(3)	(4)	(5)
<b>Panel A: Unskilled workers</b>					
Non-competitor training	3,197.8 (1,319.7) [0.017]	140.2 (41.4) [0.001]	1.297 (0.080) [0.000]	0.316 (0.420) [0.454]	1.198 (0.353) [0.001]
Competitor training	554.4 (1,270.9) [0.663]	85.3 (40.3) [0.036]	0.653 (0.063) [0.000]	-0.318 (0.401) [0.429]	0.101 (0.316) [0.749]
<i>Test (p-value):</i> 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
<b>Panel B: Skilled workers</b>					
Non-competitor training	-2,703.143 (1,087.5) [0.014]	-48.398 (50.1) [0.336]	-0.498 (0.136) [0.000]	-0.798 (0.313) [0.012]	-2.864 (0.276) [0.000]
Competitor training	-198.250 (1,262.5) [0.875]	38.7 (57.0) [0.498]	-0.254 (0.140) [0.073]	-0.339 (0.312) [0.279]	-1.642 (0.275) [0.000]
<i>Test (p-value):</i> 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 row planting 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 Non-competitive 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 rowp.	Daily Wage	Work days	Plots rowp.	Work days	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-competitor training	0.243 (0.063) [0.000]	0.290 (0.037) [0.000]	-86.04 (45.87) [0.063]	0.572 (0.096) [0.000]	0.474 (0.073) [0.000]	-0.156 (0.268) [0.560]	-6.040 (851.6) [0.994]
Competitor training	0.104 (0.065) [0.112]	0.137 (0.037) [0.000]	-2.91 (48.74) [0.953]	0.349 (0.095) [0.000]	0.291 (0.072) [0.000]	-0.206 (0.266) [0.440]	437.0 (998.1) [0.662]
<i>Test (p-value):</i> 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 row 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 row planting for employers alone, and also including own farm work, respectively. Column (2) and (4) report the number of plots planted using the row planting technology for employers alone, and for employers and on the respondent's farm. Column (3) refers to the daily wage for the row planting 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 Non-competitive 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 rowplanting for unskilled workers

	Any plot rowplanted	Share plots majority rowplanted	Num. plots rowplanted	Harvest (kg)
	(1)	(2)	(3)	(4)
Non-competitor training	0.74 (0.021) [0.000]	0.33 (0.012) [0.000]	0.97 (0.039) [0.000]	
Competitor training	0.55 (0.029) [0.000]	0.21 (0.014) [0.000]	0.67 (0.040) [0.000]	
<i>Test (p-value:</i> T2-Different = T1-Same	<.001	<.001	<.001	
Control mean	0.012	0.004	0.017	
N	1706	1704	1706	

*Notes:* This table shows outcomes related to own-farm adoption of the row planting technology among unskilled 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. Finally, column (4) reports the beans harvest in kilograms. 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 Non-competitive village and Competitive village coefficients are shown.

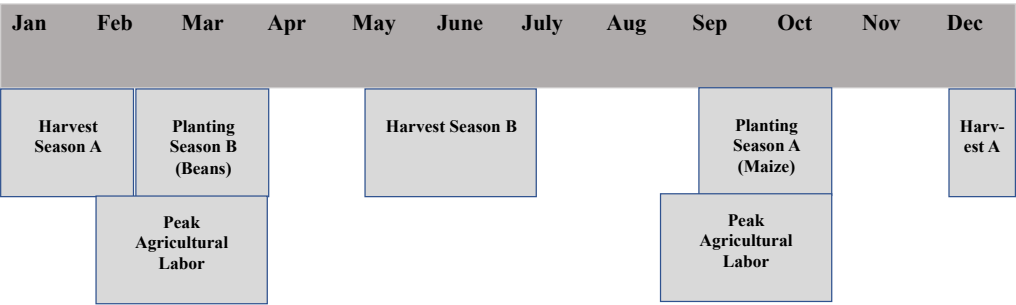
Standard errors are clustered at the village level and reported in parentheses. P-values are reported in squared brackets.



# A Appendix Figures and Tables

## A.1 Agriculture in Burundi

Figure A.1: Burundian agricultural calendar



**Planting Season – Time Period Per Task (Season B)**

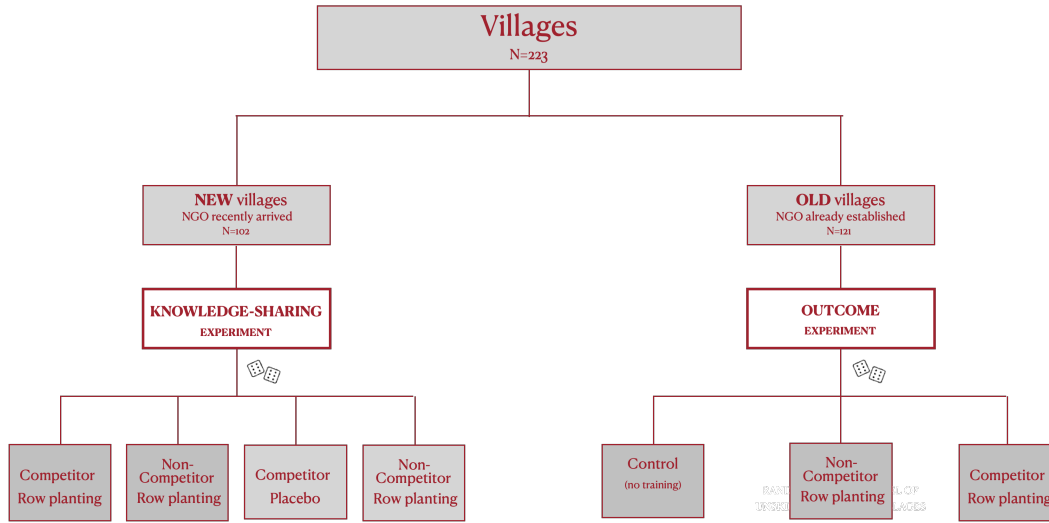
- Land Preparation
- Planting/Fertilizer Application – 2 weeks
- Application of Tuteurs
- Weeding
- Harvesting

*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. \(2008\)](#).

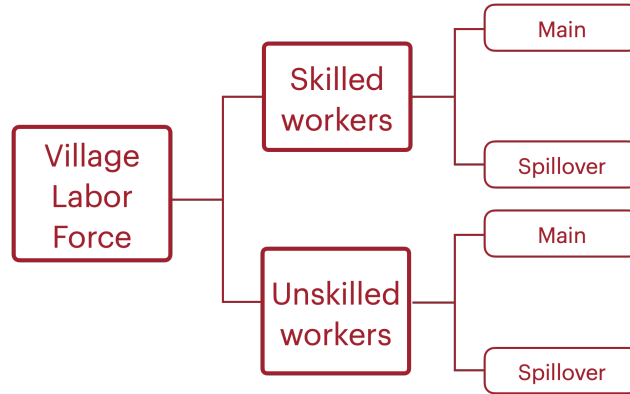
## A.2 Sampling

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 (0.43)	0.09 (0.29)	0.49 (0.50)	0.17 (0.37)
=1 is bottom quartile land distribution	0.02 (0.15)	0.05 (0.22)	0.30 (0.46)	0.29 (0.45)
=1 is top land distribution	0.67 (0.47)	0.53 (0.50)	0.09 (0.29)	0.08 (0.27)
=1 is NGO member	0.61 (0.49)	0.67 (0.47)	0.46 (0.50)	0.36 (0.48)
Savings (Fbu)	62,084 (148,239)	57,724 (87,765)	20,401 (32,189)	21,234 (46,568)
Num. of adults in household	2.50 (1.16)	3.03 (1.35)	2.34 (0.93)	2.08 (1.03)
Num. fam. members who supply labor	0.00 (0.00)	1.59 (0.86)	1.60 (0.73)	0.00 (0.00)
=1 if adopted row planting	0.66 (0.47)	0.70 (0.46)	0.43 (0.50)	0.36 (0.48)
=1 if adopted modern composting	0.88 (0.33)	0.80 (0.40)	0.71 (0.45)	0.71 (0.45)
=1 if any member hired for rowplanting tasks	. (0.50)	0.51 (0.50)	0.34 (0.48)	. (0.48)
Post-harvest tech. adoption indexes	2.40 (1.02)	2.34 (0.94)	1.90 (1.22)	1.66 (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 24 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
	<i>((0.43))</i>	0.49	<i>((0.50))</i>	0.50
=1 is bottom quartile land distribution	0.01	0.04	0.22	0.38
	<i>(0.11)</i>	<i>(0.20)</i>	<i>(0.41)</i>	<i>(0.48)</i>
=1 is top land distribution	0.69	0.64	0.14	0.05
	<i>(0.46)</i>	<i>(0.48)</i>	<i>(0.35)</i>	<i>(0.23)</i>
Savings (Fbu)	64,841	57,816	24,533	16,925
	<i>(130,910)</i>	<i>(171,735)</i>	<i>(34,249)</i>	<i>(29,923)</i>
Num. of adults in household	2.56	2.40	2.41	2.28
	<i>(1.16)</i>	<i>(1.15)</i>	<i>(0.99)</i>	<i>(0.86)</i>
Num. fam. members who supply labor	0.00	0.00	1.59	1.62
	<i>(0.00)</i>	<i>(0.00)</i>	<i>(0.75)</i>	<i>(0.72)</i>
=1 if adopted row planting	0.95	0.19	0.88	0.07
	<i>(0.21)</i>	<i>(0.40)</i>	<i>(0.33)</i>	<i>(0.25)</i>
=1 if adopted modern composting	0.92	0.78	0.84	0.57
	<i>(0.27)</i>	<i>(0.41)</i>	<i>(0.36)</i>	<i>(0.50)</i>
=1 if any member hired for rowplanting tasks	0.00	0.00	0.70	0.04
	<i>(0.00)</i>	<i>(0.00)</i>	<i>(0.46)</i>	<i>(0.20)</i>
Post-harvest tech. adoption indexs	2.61	2.08	2.41	1.47
	<i>(0.94)</i>	<i>(1.05)</i>	<i>(1.09)</i>	<i>(1.15)</i>

*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 24 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	0.34 (0.21)	0.12 (0.11)	0.66 (0.22)	0.55 (0.18)
Grain triage	0.39 (0.17)	0.17 (0.14)	0.61 (0.18)	0.72 (0.20)
Apply anti-mold products	0.62 (0.12)	0.44 (0.21)	0.86 (0.10)	0.83 (0.13)
Post-harvest tech. index (out of 4)	2.35 (0.41)	0.92 (0.37)	3.47 (0.41)	2.42 (0.31)
Own farm rowplanting	0.11 (0.07)	0.00 (0.00)	0.90 (0.05)	0.96 (0.11)
Villages	19	53	20	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 24 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)
<b>Panel A: Village characteristics</b>				
Labor force size	89.927 ( 25.095)	-0.591 5.141 [ 0.909]	-0.229 5.005 [ 0.964]	0.993
Share of skilled laborers	0.421 ( 0.085)	0.002 0.019 [ 0.935]	0.003 0.022 [ 0.881]	0.989
1(church=1)	0.513 ( 0.506)	0.120 0.113 [ 0.288]	0.014 0.115 [ 0.904]	0.498
1(school=1)	0.385 ( 0.493)	0.075 0.113 [ 0.505]	-0.007 0.111 [ 0.954]	0.734
1(shop=1)	0.487 ( 0.506)	0.029 0.103 [ 0.776]	-0.026 0.106 [ 0.807]	0.875
<b>Panel B: Training events</b>				
Unskilled at event		24.300 (13.931)	1.782	0.495
as share of labor force		0.268 ( 0.078)	0.025	0.158
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 training	Diff. training	F-test training	Obs Joint sig.
Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Panel A: Demographics</i>										
Age	39.89 ( 0.45)	0.582 ( 0.677)	0.330 ( 0.607)	0.654	1757	37.93 ( 0.48)	-1.151 ( 0.734)	-0.115 ( 0.831)	0.220	1790
Male	0.280 ( 0.018)	-0.034 ( 0.034)	-0.065 ( 0.038)	0.228	1757	0.174 ( 0.015)	-0.025 ( 0.023)	-0.018 ( 0.028)	0.550	1790
Married	0.831 ( 0.015)	-0.014 ( 0.018)	0.014 ( 0.017)	0.364	1757	0.755 ( 0.017)	-0.031 ( 0.029)	0.021 ( 0.031)	0.190	1790
No schooling	0.319 ( 0.019)	0.004 ( 0.031)	0.005 ( 0.028)	0.982	1757	0.387 ( 0.020)	-0.011 ( 0.030)	0.012 ( 0.032)	0.762	1790
		[ 0.906]	[ 0.854]				[ 0.705]	[ 0.717]		
<i>Panel B: Household characteristics</i>										
Household size	4.579 ( 0.078)	0.103 ( 0.104)	-0.070 ( 0.110)	0.273	1758	4.030 ( 0.079)	0.016 ( 0.132)	0.021 ( 0.118)	0.983	1791
Savings (Fbu)	24,534 ( 1,080)	-1,891 ( 1,916)	-4,514 ( 2,035)	0.090	1758	9,002 ( 522)	270.492 ( 1,240)	662.797 ( 1,060)	0.799	1791
Land area (ares)	14.56 ( 0.39)	-1.146 ( 0.845)	-1.453 ( 0.954)	0.289	1757	7.379 ( 0.212)	0.723 ( 0.462)	0.258 ( 0.530)	0.284	1790
Plots of land	4.836 ( 0.089)	-0.221 ( 0.181)	-0.537 ( 0.189)	0.018	1757	3.612 ( 0.069)	0.097 ( 0.137)	-0.091 ( 0.152)	0.361	1790
		[ 0.224]	[ 0.005]				[ 0.480]	[ 0.551]		
<i>Panel C: Past labor supply – ag.</i>										
Skilled work days	7.454 ( 0.182)	-0.187 ( 0.390)	-0.197 ( 0.390)	0.844	1752					
Ag. work days	14.03 ( 0.24)	-0.159 ( 0.489)	-0.585 ( 0.449)	0.422	1752	14.90 ( 0.22)	-0.529 ( 0.410)	-0.877 ( 0.471)	0.157	1785
Ag. labor earnings (Fbu)	41,153 ( 797)	41.378 ( 1,769)	-2,434 ( 1,606)	0.215	1752	36,126 ( 554)	-114.307 ( 1,277)	-375.685 ( 1,397)	0.963	1782
Other hh ag. earning (Fbu)	5,654 ( 505)	363.396 ( 913.925)	419.576 ( 904.230)	0.882	1758	11,464 ( 623)	1,318 ( 1,034)	1,069 ( 1,029)	0.392	1791
Unemployment days	0.718 ( 0.061)	0.034 ( 0.098)	-0.160 ( 0.103)	0.144	1719	1.334 ( 0.078)	-0.054 ( 0.128)	0.029 ( 0.138)	0.844	1753
		[ 0.726]	[ 0.122]				[ 0.675]	[ 0.835]		
<i>Panel D: Own farm</i>										
Beans harvest kg/ares	9.149 ( 0.282)	0.422 ( 0.533)	-0.768 ( 0.598)	0.089	1754	7.468 ( 0.246)	-0.590 ( 0.500)	-0.010 ( 0.534)	0.399	1784
Work days on own farm	14.70 ( 0.20)	-0.363 ( 0.423)	-1.093 ( 0.445)	0.047	1757	10.14 ( 0.20)	0.188 ( 0.466)	0.246 ( 0.523)	0.873	1790
Fam labor on own farm days	12.27 ( 0.31)	-0.035 ( 0.550)	-1.204 ( 0.561)	0.078	1757	6.401 ( 0.236)	1.422 ( 0.609)	0.760 ( 0.694)	0.070	1782
Sold any harvest	0.547 ( 0.020)	-0.009 ( 0.038)	-0.057 ( 0.043)	0.401	1757	0.226 ( 0.017)	-0.001 ( 0.035)	-0.020 ( 0.034)	0.800	1790
		[ 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)
<i>Demographics</i>						
Age	40.21 ( 0.76) [ 0.126]	-1.735 ( 1.124) [ 0.200]	-1.395 ( 1.081) [ 0.200]	-1.945 ( 1.185) [ 0.104]	0.369	897
Male	0.350 ( 0.034) [ 0.059]	-0.021 ( 0.059) [ 0.719]	0.048 ( 0.059) [ 0.422]	0.071 ( 0.064) [ 0.267]	0.359	897
Married	0.833 ( 0.026) [ 0.890]	-0.005 ( 0.036) [ 0.666]	0.018 ( 0.042) [ 0.914]	0.004 ( 0.037) [ 0.469]	0.944	897
Spouse is migrant	0.330 ( 0.033) [ 0.517]	-0.030 ( 0.046) [ 0.193]	-0.067 ( 0.051) [ 0.469]	-0.036 ( 0.049) [ 0.056]	0.628	897
No schooling	0.310 ( 0.033) [ 0.322]	-0.036 ( 0.036) [ 0.802]	-0.011 ( 0.044) [ 0.802]	-0.074 ( 0.038) [ 0.056]	0.242	897
Believe able to train	0.680 ( 0.033) [ 0.288]	-0.048 ( 0.045) [ 0.288]	-0.036 ( 0.054) [ 0.511]	0.029 ( 0.046) [ 0.527]	0.353	897
<i>Household characteristics</i>						
Household size	4.419 ( 0.128) [ 0.459]	0.145 ( 0.196) [ 0.459]	0.034 ( 0.188) [ 0.858]	-0.059 ( 0.190) [ 0.755]	0.705	897
Savings (Fbu)	23,546 ( 1,485) [ 0.453]	-1,820 ( 2,415) [ 0.208]	-3,027 ( 2,387) [ 0.208]	-2,385 ( 2,406) [ 0.324]	0.628	897
Land area (ares)	10.80 ( 0.46) [ 0.160]	0.847 ( 0.599) [ 0.160]	0.752 ( 0.690) [ 0.278]	0.446 ( 0.635) [ 0.485]	0.551	897
Plots of land	4.419 ( 0.157) [ 0.837]	0.045 ( 0.219) [ 0.837]	-0.118 ( 0.190) [ 0.538]	0.188 ( 0.198) [ 0.345]	0.513	897
<i>Past labor supply – ag.</i>						
Work days in ag	15.77 ( 0.46) [ 0.585]	-0.381 ( 0.695) [ 0.585]	-0.805 ( 0.723) [ 0.268]	0.689 ( 0.708) [ 0.333]	0.209	897
Skilled work days	3.842 ( 0.227) [ 0.738]	0.117 ( 0.350) [ 0.738]	0.311 ( 0.365) [ 0.396]	0.971 ( 0.328) [ 0.004]	0.020	897
Ag. labor earnings	44,108 ( 1,429) [ 0.684]	-886.525 ( 2,169) [ 0.684]	-1,493 ( 2,112) [ 0.481]	3,896 ( 2,184) [ 0.077]	0.082	897
Oth. hh ag. earning	13,249 ( 1,143) [ 0.450]	922.202 ( 1,216) [ 0.450]	1,507 ( 1,290) [ 0.245]	2,473 ( 1,430) [ 0.087]	0.315	897
Any unemployment	0.384 ( 0.034) [ 0.225]	-0.066 ( 0.054) [ 0.225]	-0.015 ( 0.052) [ 0.777]	-0.050 ( 0.043) [ 0.255]	0.530	897
<i>Own farm</i>						
Beans harvest kg/ares	8.636 ( 0.337) [ 0.133]	-0.676 ( 0.446) [ 0.133]	-0.285 ( 0.424) [ 0.503]	0.298 ( 0.443) [ 0.503]	0.207	896
Work days on own farm	12.82 ( 0.27) [ 0.242]	0.415 ( 0.353) [ 0.242]	0.194 ( 0.386) [ 0.617]	-0.053 ( 0.352) [ 0.881]	0.581	897
Fam ag days on own farm	11.82 ( 0.42) [ 0.653]	0.327 ( 0.725) [ 0.653]	1.035 ( 0.655) [ 0.117]	1.032 ( 0.617) [ 0.098]	0.271	897
Sold any harvest	0.581 ( 0.035) [ 0.840]	-0.009 ( 0.045) [ 0.840]	0.029 ( 0.052) [ 0.585]	0.035 ( 0.046) [ 0.447]	0.728	897
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		F-test	Obs
	Comptitor	Non-competitor	Joint sig.	
	(1)	(2)	(3)	(4)
<i>Panel A: Demographics</i>				
Age	37.35 ( 0.78)	-0.259 ( 1.096)	0.814	539
Male	0.187 ( 0.024)	0.074 ( 0.044)	0.099	539
Married	0.667 ( 0.029)	0.041 ( 0.048)	0.399	539
Spouse is migrant	0.438 ( 0.030)	-0.048 ( 0.053)	0.365	540
No schooling	0.356 ( 0.029)	0.097 ( 0.042)	0.024	539
<i>Panel B: Household characteristics</i>				
Household size	4.311 ( 0.121)	-0.161 ( 0.133)	0.234	539
Savings (Fbu)	8,678 ( 679)	-1,128 ( 919)	0.225	539
Land area (ares)	8.704 ( 0.363)	-0.645 ( 0.400)	0.113	539
Plots of land	3.734 ( 0.114)	-0.013 ( 0.187)	0.943	539
<i>Panel C: Past labor supply – ag.</i>				
Work days in ag	14.20 ( 0.30)	0.129 ( 0.530)	0.809	539
Ag. labor earnings	37,800 ( 909)	319.053 ( 1,697)	0.852	539
Oth. hh ag. earning	15,303 ( 1,053)	-1,322 ( 1,360)	0.335	539
Any unemployment	0.607 ( 0.030)	0.066 ( 0.051)	0.199	539
<i>Panel D: Own farm</i>				
Beans harvest kg/ares	5.964 ( 0.232)	0.305 ( 0.270)	0.264	538
Work days on own farm	11.57 ( 0.24)	-0.390 ( 0.330)	0.243	539
Fam ag days on own farm	10.32 ( 0.38)	-0.978 ( 0.535)	0.073	539
Plots of land	3.734 ( 0.114)	-0.013 ( 0.187)	0.943	539
Sold any harvest	0.330 ( 0.029)	-0.066 ( 0.041)	0.110	539
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)
<i>Panel A: Demographics</i>												
Age	37.41	39.23 ( 0.33)	0.227 ( 0.540)	0.517 ( 0.471)	0.548	3446	34.06	37.49 ( 0.34)	-1.506 ( 0.625)	-0.899 ( 0.623)	0.058	3581
Male	0.276	0.263 ( 0.013)	-0.022 ( 0.031)	-0.033 ( 0.032)	0.591	3446	0.339	0.167 ( 0.011)	-0.016 ( 0.021)	-0.018 ( 0.022)	0.688	3581
Married		0.827 ( 0.011)	0.008 ( 0.016)	0.025 ( 0.015)	0.220	3446		0.753 ( 0.012)	-0.004 ( 0.021)	0.038 ( 0.018)	0.035	3581
No schooling	0.297	0.328 ( 0.014)	-0.003 ( 0.028)	0.004 ( 0.026)	0.966	3446	0.325	0.382 ( 0.014)	-0.029 ( 0.026)	-0.007 ( 0.024)	0.493	3581
			[ 0.924]	[ 0.889]					[ 0.265]	[ 0.779]		
<i>Panel B: Household characteristics</i>												
Household size		4.527 ( 0.056)	0.052 ( 0.084)	0.022 ( 0.080)	0.828	3448		3.989 ( 0.055)	0.012 ( 0.098)	0.006 ( 0.083)	0.993	3585
Savings (Fbu)	32,073	21,386 ( 707)	-1,817 ( 1,487)	-3,342 ( 1,493)	0.085	3448	22,020	8,792 ( 340)	-181.011 ( 838.095)	523.183 ( 754.060)	0.606	3585
Land area (ares)	14.18	13.62 ( 0.26)	-0.787 ( 0.662)	-0.656 ( 0.752)	0.489	3446	11.48	7.708 ( 0.142)	0.290 ( 0.377)	0.054 ( 0.386)	0.709	3581
Plots of land		4.720 ( 0.061)	-0.264 ( 0.144)	-0.386 ( 0.154)	0.037	3446		3.620 ( 0.050)	0.032 ( 0.130)	-0.086 ( 0.136)	0.587	3581
			[ 0.070]	[ 0.014]					[ 0.807]	[ 0.527]		
<i>Panel C: Past labor supply – ag.</i>												
Skilled work days		7.162 ( 0.119)	0.117 ( 0.274)	-0.066 ( 0.290)	0.802	3436						
Ag. work days		13.59 ( 0.16)	0.143 ( 0.328)	-0.195 ( 0.329)	0.647	3436		14.28 ( 0.15)	-0.327 ( 0.339)	-0.416 ( 0.326)	0.414	3577
Ag. labor earnings (Fbu)		38,614 ( 532)	569.762 ( 1,393)	-1,008 ( 1,274)	0.451	3436		34,126 ( 389)	100.203 ( 940.438)	-32.054 ( 930.436)	0.988	3573
Other hh ag. earning (Fbu)		6,070 ( 361)	21.875 ( 711.811)	286.340 ( 700.348)	0.893	3448		13,582 ( 464)	305.477 ( 767.359)	801.290 ( 803.270)	0.607	3585
Unemployment days		0.699 ( 0.043)	-0.052 ( 0.081)	-0.156 ( 0.082)	0.146	3377		1.308 ( 0.053)	-0.017 ( 0.102)	-0.070 ( 0.099)	0.769	3500
			[ 0.528]	[ 0.060]					[ 0.870]	[ 0.478]		
<i>Panel D: Own farm</i>												
Beans harvest kg/ares		9.218 ( 0.198)	-0.148 ( 0.438)	-1.001 ( 0.472)	0.053	3442		6.722 ( 0.145)	-0.085 ( 0.373)	-0.049 ( 0.378)	0.974	3574
Work days on own farm		14.30 ( 0.14)	-0.332 ( 0.326)	-0.864 ( 0.321)	0.028	3446		9.999 ( 0.136)	-0.264 ( 0.444)	-0.128 ( 0.428)	0.838	3581
Fam labor on own farm days		12.10 ( 0.21)	-0.214 ( 0.441)	-0.703 ( 0.466)	0.324	3440		6.906 ( 0.167)	0.288 ( 0.568)	0.101 ( 0.514)	0.877	3572
Sold any harvest		0.551 ( 0.015)	-0.024 ( 0.036)	-0.057 ( 0.037)	0.311	3446		0.210 ( 0.012)	-0.012 ( 0.029)	-0.026 ( 0.027)	0.638	3581
			[ 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 24. 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 Sample	Training time (hrs.)				Passed the test			
	New villages			Old villages	New villages			Old villages
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-Competitive X Rowplanting			0.604 (0.142) [0.000]				0.399 (0.061) [0.000]	
Non-Competitive	-0.173 (0.105) [0.104]	0.451 (0.091) [0.000]	-0.117 (0.105) [0.269]	1.408 (0.063) [0.000]	-0.00360 (0.012) [0.771]	0.382 (0.046) [0.000]	0.000567 (0.026) [0.982]	0.383 (0.023) [0.000]
Non-Competitive			-0.219 (0.102) [0.035]				-0.938 (0.025) [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 technology 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 unforeseen 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-Competitive	0.0278 (0.041) [0.503]	0.0163 (0.031) [0.603]	0.0578 (0.021) [0.008]	0.321 (0.044) [0.000]	0.267 (0.039) [0.000]	0.221 (0.036) [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 unforeseen 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

Heterogeneity dimension	Outcome: Unskilled is trained							
	None	Skilled characteristics					Unskilled characteristics	
	(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)
<b>Panel A: Row planting</b>								
Competitor training	-0.363 (0.041) [0.000]	-0.310 (0.044) [0.000]	-0.285 (0.052) [0.000]	-0.270 (0.048) [0.000]	-0.427 (0.066) [0.000]	-0.385 (0.055) [0.000]	-0.436 (0.054) [0.000]	-0.386 (0.044) [0.000]
Competitor X heterog.		-0.118 (0.050) [0.023]	-0.144 (0.068) [0.040]	-0.167 (0.054) [0.004]	0.096 (0.064) [0.140]	0.064 (0.095) [0.502]	0.132 (0.054) [0.018]	0.235 (0.068) [0.001]
Heterogeneity		0.050 (0.051) [0.337]	0.113 (0.062) [0.073]	0.126 (0.052) [0.019]	-0.086 (0.062) [0.170]	-0.043 (0.090) [0.639]	-0.050 (0.053) [0.346]	-0.175 (0.079) [0.031]
Excluded cat. mean	0.425	0.393	0.381	0.370	0.478	0.461		0.444
N	533	533	533	533	533	533	533	533
<b>Panel B: Row planting and placebo</b>								
Row planting	-0.551 (0.055) [0.000]	-0.583 (0.055) [0.000]	-0.614 (0.058) [0.000]	-0.622 (0.063) [0.000]	-0.485 (0.082) [0.000]	-0.530 (0.066) [0.000]	-0.481 (0.067) [0.000]	-0.531 (0.056) [0.000]
Competitor training	-0.015 (0.026) [0.553]	-0.013 (0.031) [0.680]	-0.017 (0.028) [0.543]	0.003 (0.037) [0.945]	0.041 (0.030) [0.182]	-0.019 (0.028) [0.502]	-0.016 (0.036) [0.658]	-0.013 (0.027) [0.637]
Competitor X row planting	-0.378 (0.061) [0.000]	-0.340 (0.064) [0.000]	-0.305 (0.067) [0.000]	-0.313 (0.071) [0.000]	-0.499 (0.089) [0.000]	-0.408 (0.072) [0.000]	-0.446 (0.077) [0.000]	-0.399 (0.064) [0.000]
Competitor X rowp X heterog.		-0.092 (0.054) [0.090]	-0.133 (0.074) [0.072]	-0.119 (0.065) [0.070]	0.179 (0.074) [0.018]	0.085 (0.103) [0.414]	0.127 (0.062) [0.042]	0.224 (0.103) [0.031]
Heterogeneity		-0.002 (0.024) [0.927]	-0.006 (0.015) [0.678]	-0.006 (0.020) [0.748]	0.021 (0.021) [0.323]	-0.015 (0.025) [0.546]	0.060 (0.023) [0.010]	0.035 (0.053) [0.505]
Competitor X heterog.		-0.003 (0.030) [0.913]	0.001 (0.028) [0.966]	-0.029 (0.042) [0.493]	-0.082 (0.034) [0.018]	0.013 (0.033) [0.687]	0.001 (0.033) [0.983]	-0.007 (0.067) [0.921]
Competitor X heterog.		0.076 (0.042) [0.070]	0.118 (0.066) [0.075]	0.125 (0.050) [0.014]	-0.099 (0.066) [0.138]	-0.060 (0.097) [0.541]	-0.127 (0.049) [0.011]	-0.238 (0.085) [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 unforeseen 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

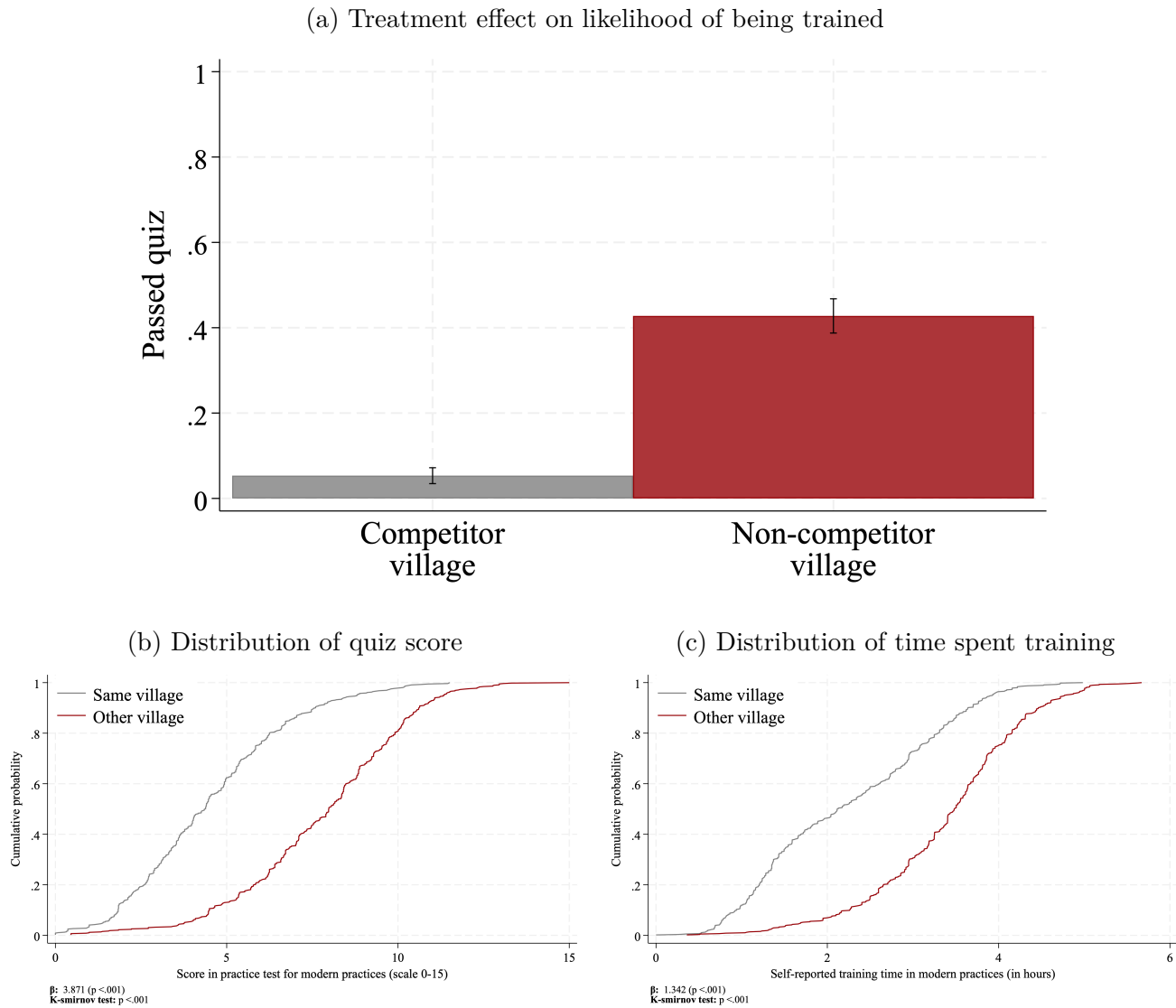


Figure A.3: Market Effect Experiment – Treatment effect on training outcomes

*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.

	<b>Labor Earnings</b>	<b>Daily Wage</b>	<b>Work days (Rowp.)</b>	<b>Work days (All tasks)</b>	<b>Exp. work days next season</b>
	(1)	(2)	(3)	(4)	(5)
Non-competitor training	488.1 (1,024.8) [0.635]	4.618 (32.3) [0.887]	0.011 (0.008) [0.167]	-0.017 (0.350) [0.962]	-0.081 (0.314) [0.798]
Competitor training	289.9 (1,146.0) [0.801]	31.4 (35.3) [0.376]	0.018 (0.008) [0.021]	-0.213 (0.395) [0.591]	-0.237 (0.302) [0.434]
<i>Test (p-value):</i>					
Competitor training = Non-competitor training	0.843	0.394	0.470	0.553	0.618
	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 (0.004) [0.657]	0.03 (0.015) [0.063]	0.00 (0.062) [0.942]
Competitor training	-0.00 (0.004) [0.862]	0.01 (0.015) [0.709]	0.01 (0.063) [0.848]
<i>Test (p-value:</i> 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.