Enhancing the diffusion of information about agricultural technology

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Abstract

We use a field experiment in 100 Indian villages to study the impact of farmer field days — short meetings where a new seed variety is explained, adopters share their experiences, and participants observe the crop performance. The experiment shows that field days increase adoption of that variety during the following season by around 40 percent. Further analysis demonstrates that the field days are both cost effective and more impactful for poorer farmers. The findings suggest that simple interventions to enhance learning between farmers can be quite effective at relieving information frictions and increasing diffusion of agricultural technology.

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1 Introduction

The adoption of improved technologies is an important engine of growth for smallholder farmers in developing countries. Yet, levels of adoption often remain disappointingly low and lack of credible information is an oft-cited barrier (Jack, 2011). Several studies show that peers serve as credible sources of information (Foster and Rosenzweig, 1995; Bandiera and Rasul, 2006; Conley and Udry, 2010; Krishnan and Patnam, 2014). Yet information is not always transmitted between farmers and one possible explanation is that private benefits from doing so are minimal (Banerjee et al., 2013; BenYishay and Mobarak, 2015). This suggests a need to study different approaches to increasing the rate at which information is shared between farmers.

In this paper we use a field experiment to measure the efficacy of the farmer field day — one such approach of increasing knowledge and encouraging technology adoption. The field day treatment consists of a short meeting facilitated by a local NGO. The meeting involves a discussion of the technology's benefits, discussion of experiences by adopters, and a visit to the field to observe the crop. The treatment is intended to disseminate information and demonstrate technology in an environment that mirrors the production conditions of local farmers. Most importantly, the intervention creates a venue for information transmission.

In the first step of the experiment we introduced 25 kilograms of a new high-yielding and flood-tolerant rice variety called Swarna-Sub1 in 100 villages of the Indian state of Odisha. Importantly, the technology has been shown to dominate existing technology, indicating that it should diffuse rapidly in the absence of barriers to adoption.[1] The farmers receiving seeds, henceforth the demonstrators, were chosen using one of three methods. In one third of the villages we used the status-quo approach of delivering the seeds to locally-elected village officials — ward members in the Gram Panchayat — who then chose how to further distribute the seeds amongst villagers.[2] In another third of the villages we used a participatory meeting where villagers were invited to jointly determine how the seeds should be allocated. Finally, we used village meetings with local women's groups (Self Help Groups or SHG's) in the remaining villages. This randomization allows us to test whether the field day intervention is more (or less) effective under different methods of identifying demonstrators.

In the second step of the experiment our partner NGO carried out the field days in 50 villages. These field days occurred approximately four months after the seeds were introduced

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[1] The variety was released in 2009. It offers flood tolerance without reducing yield during non-flood years (Dar et al., 2013). The technology also leads to significant welfare gains by inducing farmers to modernize production (Emerick et al., 2016)

[2] The method of delivering a small amount of seeds for testing and knowledge creation is a popular approach in South Asia. India's National Food Security Mission (NFSM) program uses these seed minikits and relies on members of the Gram Panchayat to help identify beneficiaries.

and while the crops of the demonstrators were nearly mature. The field days were simple two hour events where the variety was described, demonstrators spoke about their experience, questions were answered, and then attendees were taken to observe the crop of one or more of the demonstrators.

We measured demand directly by offering the new variety for sale after harvest and immediately prior to planting for the next growing season. The sales teams went door-to-door and asked a random sample of 15 households per village whether they were interested in buying seeds. Importantly, we fixed the price to be near the prevailing market price of the variety.[3] Therefore, offering seeds directly to farmers in this way allows us to measure demand with revealed preferences, and in the absence of supply constraints and high subsidies.

We have three main results. First, the field days treatment led to a sharp increase in uptake. Adoption increases by around 12 percentage points from 30 to 42 percent in field day villages. This effect is larger for adoption of a single seed package: purchases of one five kilogram packet rose by 59 percent, while purchases of two or more packages rose by only 25 percent.[4] In addition, the effect of field days on demand is the largest for poor and historically disadvantaged farmers. More specifically, the treatment effect is significantly larger for farmers that are in lower caste groups and farmers that are below the poverty line (BPL), as defined by the government's anti-poverty program.

The field days are cost effective. Our approximate estimate of the one-year revenue gains from field days is 416 dollars per village. This compares to the costs — including time cost of attendance — of around 230 dollars. This favorable benefit-cost ratio is encouraging, but is also driven partly by the profitability of the technology being promoted. Field days to provide information about less profitable technologies may generate similar gains in adoption, but less favorable benefit-cost ratios.

Second, the effect of field days is no larger when demonstrators are selected by meetings. If anything, the effect is largest when demonstrators are selected in consultation with locally elected officials. However, the differences in treatment effects across the three methods of identifying demonstrators are not statistically significant. Nonetheless, we can rule out large and positive interaction effects between that field days and village participation in selecting demonstrators.

This lack of heterogeneity exists despite the fact that meetings select a very different pool of demonstrators. Elected officials tend to favor their close friends when selecting

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[3] The new variety was not yet available at the government offices where most farmers purchase subsidized seeds. One company in the area was selling seeds of this variety at a price higher than the subsidized price. We refer to this as the prevailing market price.

[4] Almost all farmers that purchased seeds bought only one or two packages. This amount of seeds is enough to cultivate around 10-30 percent of their land.

demonstrators. This favoritism disappears when selection takes place during meetings. More concretely, the demonstrators are 31 percent and 62 percent less likely to be close family or friends of the ward member in village meeting and SHG meeting villages, respectively. In combination, we interpret these findings to suggest that field days reduce the barriers to information transmission, regardless of the identities of the demonstrators.

Despite the reduced favoritism, our third result is that selecting demonstrators via meetings also had no direct effect on adoption. Differences in adoption between ward member, SHG meeting, and village meeting villages are small and statistically insignificant. Our estimates are generally precise enough to rule out large effects such as the 40 percent effect of field days. The consequences of favoritism by local officials in distribution of free seeds for demonstration are minimal, at least in terms of the efficacy of demonstration. Farmers appear to learn equally well from demonstrators identified by local politicians compared to demonstrators identified with participation of the village more broadly.

We then exploit the experimental variation to distinguish between two competing explanations for the efficacy of field days. On the one hand, field days could simply extend information to farmers that lack any connection to demonstrators.[5] In other words, the field days substitute for peer-to-peer learning, particularly for farmers lacking connections to demonstrators. On the other hand, field days could instead complement peer-to-peer learning by providing additional information or re-enforcing existing knowledge.

The data are if anything more compatible with the complementarity explanation. Using either common surnames or geographic distance as measures of connectedness, the effect of field days is no larger for farmers that are less connected to the demonstrators, suggesting limited evidence for substitution. We further exploit the fact that field days were attended by both SHG and non-SHG households in villages where demonstrators were selected via SHG meetings (and hence almost always belonged to the SHG). The field day effect in these villages is concentrated amongst households with SHG members or with members that are friends or family of the SHG president, suggesting that field days worked better for households where women members were frequently interacting with demonstrators outside of the field days.[6] These additional findings emphasize that farmer-to-farmer learning doesn't happen automatically and can be complemented with interventions to encourage information sharing.

Our paper adds to the literature by offering experimental evidence on an alternative method of learning about agricultural technology. Numerous studies have established the

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[5] As shown by Golub and Jackson (2012), diffusion of information between individuals is limited by certain network structures where individuals only interact with people that have similar characteristics.

[6] SHG membership involves frequent interactive meetings between members.

importance of peers as sources of information about agriculture.[7] Building on these, recent work has turned to finding ways to increase the amount of learning that takes place in networks. For instance, BenYishay and Mobarak (2015) use an experiment in Malawi to show that compensating early adopters based on future village-level adoption leads to a 3 to 6 percentage point increase in adoption of pit planting — an improved planting technique for maize. Beaman et al. (2015) focus instead on selection and show that theory-based methods of selecting demonstrators outperform selection by Malawian extension agents. Specifically, their model-based treatments cause increases in adoption of the pit planting technique by around 3 to 4 percentage points. Kondylis, Mueller, and Zhu (2017) show that extensive training of demonstrators in Mozambique influences their adoption behavior, but has little effect on adoption by others. We consider field days as a different lever for enhancing learning amongst farmers. Our point estimate, i.e. the 12.2 percentage point increase in adoption, is relatively large in absolute terms compared to these studies. However, the baseline adoption rate of pit planting in Malawi is low, meaning that percentage gains in adoption are larger in those cases.

We also add to the broader literature on agricultural extension. There is a widespread view that agricultural extension in developing countries underperforms (Anderson and Feder, 2007). Building on this, recent work has focused on testing new methods of providing information to farmers. As one example, there has been a recent emphasis on mobile phones as an extension tool (Aker, 2011; Fafchamps and Minten, 2012; Cole and Fernando, 2014). We instead focus on the more traditional method of selecting demonstrators, i.e. entry points and contact farmers, and relying on these farmers to demonstrate technology for others. We consider how this traditional model can be augmented with a field days intervention to nudge learning.[8]

The rest of this paper is organized as follows. In Section 2 we give further background on the particular seed variety. We focus on why the variety is suitable for a study on learning and diffusion. We also walk through the conceptual motivation for the experiment. Section 3 discusses the experimental design and some basic characteristics of the population we study. Section 4 discusses our main results. Finally, Section 5 offers concluding remarks.

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[7] Conley and Udry (2010) is the leading example where peers are shown to be an important source of information about pineapple cultivation in Ghana.

[8] The “farmer field school” is a related extension technique, but is distinct from the method we study. Farmer field schools are usually carried out over multiple days, provide general information about agriculture rather than details about a specific technology, and don’t engage demonstrators to leverage peer-to-peer learning. Studies evaluating farmer field schools have relied on observational data and non-experimental methods such as matching (Godtland et al., 2004; Davis et al., 2012).

2 Background and conceptual framework

In this section we start by giving more details on the specific technology used in the experiment. We focus on its key benefits and how they make it a worthwhile technology for studying diffusion. We then discuss the conceptual reasoning behind the farmer field days intervention.

2.1 Details about technology

Swarna-Sub1 — the rice variety introduced as part of the experiment — offers flood tolerance as its key benefit. Swarna-Sub1 remains otherwise similar to Swarna, which is a popular type of rice cultivated throughout eastern India and Bangladesh. The technology was developed by moving a group of flood tolerance genes from a traditional rice landrace to the popular variety Swarna.[9] Plant breeders were able to rely on modern breeding techniques to create the improved variety without introducing other undesirable characteristics (Xu et al., 2006). This is important because it guarantees that the technology offers an added benefit without any obvious disadvantages during normal years.

Previous randomized experiments have conferred two channels through which this new variety improves welfare. First, the variety does improve output under flooding without compromising output when there is no flooding (Dar et al., 2013). Second, Swarna-Sub1 induces farmers to invest more in inputs, particularly at or near the time of planting. These effects likely arise due to the risk-reducing property of the technology (Emerick et al., 2016). As a result, the new variety improves outcomes even in years when flooding does not occur.

Swarna-Sub1 is appropriate for our study because it dominates existing technology and should diffuse rapidly in the absence of any frictions. This dominance makes it a worthwhile technology for studying different mechanisms to encourage diffusion. In addition, we can be confident that adoption is the appropriate outcome variable because there are no groups in the population for which adoption is strictly unprofitable.

2.2 Conceptual motivation

The intervention creates a venue where farmers can learn about a new variety, hear the experiences of demonstrators, and observe the variety in the field. A more passive approach would be to rely on demonstrators to directly provide this information to other farmers. However, any model that requires farmer-to-farmer learning suffers from the reality that farmers gain

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[9] The biological mechanism is that Swarna-Sub1 suppresses the plant's natural response of elongation during flooding. This allows the plant to retain the necessary carbohydrates for regeneration after the flooding is over (Voesenek and Bailey-Serres, 2009).

little from sharing information with others — i.e. demonstration and information-sharing create spillover benefits. This necessitates some method of encouraging demonstrators to spread information — direct incentives in the case of BenYishay and Mobarak (2015). In our case the field day allows demonstrators to stand out as leaders in the village by sharing their experience with a new technology. Consistent with this, demonstrators seldom declined to share their experience despite not being directly incentivized for participation.

There are two mechanisms through which field days could induce adoption. First, field days could provide information to farmers that otherwise would not have learned because they would not talk to demonstrators. Put differently, some people will lack information because they are not connected to demonstrators. Field days substitute for direct connections and give these people an opportunity to learn. Second, field days could reinforce knowledge that was already transmitted. Or, field days could make existing knowledge more precise by aggregating the experiences of several adopters, rather than just a single adopter sharing a connection with a given non-adopting farmer. One example would be a farmer that learns about the new variety from a demonstrator and thus possesses some signal of its effectiveness on her land. In this case the field day would aggregate the experiences of other demonstrators outside of this farmers social network. If the farmer needs to observe the experience of more than one demonstrator to adopt herself — as in Beaman et al. (2015) — then field days increase the likelihood of crossing the threshold of communicating with more than one demonstrator.

The first explanation delivers a simple prediction on who should benefit from field days: farmers that have no connection to demonstrators should benefit the most because they would otherwise possess less information about the technology. In contrast, there is no reason to expect this heterogeneity if farmers that were already connected to demonstrators also learn from the field day. We test this directly in the analysis that follows.

The alternative methods of selecting demonstrators allow us to further test whether field days are more effective when demonstrators are chosen by villagers rather than hand-picked by locally elected officials. There are two competing effects. One the one hand, farmers may learn better from similar individuals, particularly when returns are heterogeneous (Munshi, 2004; Tjernström, 2015; BenYishay and Mobarak, 2015). On the other hand, locally elected officials may hold better information about productivity and use that information in selecting demonstrators.[10] If more productive farmers are chosen as demonstrators then field days could send a stronger signal about potential output of a new technology under optimal management.

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[10] As one example, Basurto, Dupas, and Robinson (2017) show that chiefs in Malawi account for productivity differences when allocating subsidized inputs to villagers.

3 Details of the experimental design

In this section we give specific details on the experimental design. The section starts with more information on how the treatments were carried out and the timing of data collection. We also describe some basic summary statistics as well as randomization balance.

The experiment took place in 100 villages in Balasore — a district in the northeastern corner of the state of Odisha. The villages are located in three blocks — an administrative unit two levels above villages — where our partner NGO frequently works. We randomly selected these villages from the subset of villages that were affected by flooding for at least 2 days in 2011 or 2013. We used satellite images of flooding during these years to classify affected villages. The sample focuses on flood-prone areas to ensure that adoption is a profitable outcome and therefore diffusion is important to study.

We next describe the timing of events, which we also display graphically in Figure 1. We first administered a baseline survey to 10 farmers in each village.[11] A local village leader identified these farmers for our survey teams. The main purpose of the baseline was to measure whether farmers in the sample villages had any past experience with Swarna-Sub1. Past experiences were indeed limited: only two farmers that were surveyed at baseline had cultivated Swarna-Sub1 during the previous season in 2013. In contrast, 74 percent of farmers were cultivating Swarna, the variety that is otherwise similar except for flood tolerance. This makes the technology ideal for the experiment because it is profitable relative to the most popular variety, but unknown to farmers, making learning an important consideration.

Shortly after completion of the baseline in May 2014, enumerators returned to each village to distribute seeds to demonstrators. Each village was provided with 25 kilograms — an amount that is sufficient to cultivate one or two acres. But more importantly, the seeds were already packaged into five kilogram packages to encourage that at least five farmers be selected as demonstrators.

Villages were randomly assigned to one of three methods for identifying demonstrators. The seeds were delivered to the locally elected village ward member in 33 villages. The ward member is elected to represent the village in the local Gram Panchayat, or the next administrative unit above villages. A representative from our partner NGO delivered the seeds directly to the ward member and informed them that the NGO was giving the seeds to the village. The enumerator then asked the ward member to identify the most suitable cultivators of the new seeds. In all cases the seeds were left to the ward member and she independently decided about their further distribution, including whether to keep some for herself. This approach simulates the common approach of both government and NGO's of

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[11] Enumerators were unable to carry out the baseline survey in one of the 100 villages.

using local political figures to distribute seed minikits, as in Bardhan and Mookherjee (2006, 2011).

We used two types of village meetings to identify demonstrators in the remaining villages. In 34 meeting villages NGO staff first visited the village and informed as many villagers as possible that they were carrying out a short meeting to describe a new flood-tolerant rice variety. Enumerators were specifically instructed to put the seed minikits at the front of the meeting and describe the benefits of the new variety relative to Swarna. Importantly, enumerators instructed villagers to jointly decide on the most suitable demonstrators. In all cases villagers were able to come to an agreement and all 25 kilograms of seed were distributed to farmers that were willing to plant. We used a process that was nearly identical to this in the remaining 33 villages — the only difference being that only Self Help Group (SHG) members were invited to the meeting. This approach makes the meeting inclusive of entirely women.

Enumerators returned to all villages in September 2014 to survey all of the demonstrators. This short survey had two purposes. First, the survey allows us to compare characteristics of demonstrators across treatment arms. Second, we collected information on how much area was planted with Swarna-Sub1, the current status of the crop, and the GPS boundaries for the plots of farmers that actually transplanted the seedlings.[12] Overall, we have plot locations for 452 (67 percent) of the farmers that received seeds.

Farmer field days were then carried out in 50 randomly selected villages during the month of November 2014. The field days were purposefully timed to take place slightly before harvest when the adopters had built some experience with the technology but while the crop was still in the ground for demonstration. The field days were short. Staff from our partner NGO again described Swarna-Sub1 and its properties, spoke about seed quality and how to distinguish quality seeds, and then gave the demonstrators an opportunity to discuss their experience with Swarna-Sub1. The meetings were fairly well attended: an average of 41 farmers — or 59 percent of rice-farming households — attended the field days. Table A1 shows that household characteristics in general do not strongly predict attendance. The field days appear to have been attended by a broad group of villagers and not just the wealthiest or most elite farmers.

We then carried out a survey with approximately 15 farmers in each village in order to measure knowledge about Swarna-Sub1. We refer to this group as the non-adopting farmers, i.e. those that were not in the group of demonstrators. The surveys took place

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[12] Some farmers were affected by flooding during the nursery stage before seedlings had been transplanted in the main field. No rice variety is tolerant to submergence at this stage. As a result, 29 percent of farmers lost their seedlings and were unable to plant the crop in the main field.

during February to March 2015. We used the list of households from the 2002 Below the Poverty Line (BPL) census to randomly select households.[13] We removed the demonstrators before randomly selecting the households. Each respondent was asked several questions to measure their knowledge of Swarna-Sub1. These included whether they knew about it at all, knowledge of its main benefit, which areas are suitable for cultivation, and duration (time from planting to harvest).

Our NGO sent a new team of staff members to each village in May 2015. Each farmer that was surveyed in February-March was visited and given the opportunity to purchase Swarna-Sub1 seeds. There was only one other NGO selling Swarna-Sub1 to farmers for a price of around 20 rupees per kilogram. Our price was set to 20 rupees in order to mimic this market price. Thus, farmers benefitted mostly from free delivery when given this purchasing opportunity. Most of the farmers in our sample did not know how to obtain Swarna-Sub1.[14] The door-to-door sales therefore reveal demand at the market price in the absence of these barriers. We observed a strong demand for the technology in the door-to-door sales: 36 percent of farmers bought at least one package of seeds.

The inability to record adoption from other sources — largely other farmers in the village — is the disadvantage of measuring demand with the door-to-door sales. Swarna-Sub1 is an inbred rice variety that can be multiplied, reused, and traded with other farmers. Many estimates indicate that this informal seed system of either reusing one's own seed or obtaining from neighbors accounts for a meaningful portion of seed supply in South Asia.

We remedied this issue by carrying out a door-to-door adoption census starting in July 2015. Survey teams went door-to-door in each village and asked each household a small set of questions, including whether they were currently cultivating Swarna-Sub1. A total of 6,511 households were surveyed. This additional dataset allows for measurement of adoption from all potential sources, not just our door-to-door sales. The data do show the importance of supply barriers. Only 14 percent of all households adopted Swarna-Sub1.[15] This compares to a 36 percent adoption rate in the door-to-door sales sample.

Table 1 shows summary statistics. We focus on the sample of approximately 15 farmers per village that we use for much of the analysis. Farm sizes of just over two acres are roughly representative of farm sizes in coastal flood-prone parts of the state. Swarna is also a popular variety: over half the farmers in the sample cultivated Swarna during the season

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[13] We selected all households in the villages where there were fewer than 15 non-adopting households.

[14] One of the main questions that came up during the field days was how to obtain the seed. Private seed dealers do not operate in this area and the seeds were generally not available at the local block office where most farmers buy their seeds. There was only one other NGO with access to seeds and most farmers were unaware of this NGO.

[15] This figure includes the demonstrators from the previous year.

before the survey. The table also considers covariate balance by regressing each characteristic on the field day indicator and block fixed effects. This is the same form of regression used in the main analysis that follows. The two experimental groups are roughly comparable on observable characteristics.

Table A2 shows that household characteristics vary little across the three different methods of choosing demonstrators. Characteristics are roughly balanced even considering that the estimation sample is a random subset of farmers that were not chosen as demonstrators. Part of the reason we introduced only 25 kilograms was to limit any selection into the group of non-demonstrators. More concretely, around 6 to 8 farmers — or 10 percent of the rice-farming population — were selected as demonstrators in most villages. Thus, the sample of non-demonstrating farmers represents most of the village. We also show adoption effects for the entire village — including demonstrators, which shuts down the possibility that this type of selection influences any of the estimates.

4 Results

This section is divided into separate components for each of the main results. We first outline the simple empirical framework afforded by the experimental design. The next subsection documents the effect of field days on knowledge. We then turn to the main effects on uptake. In addition to these average effects, two sources of heterogeneity are considered: the different methods of choosing demonstrators and the wealth or caste status of the household. The treatment effects are then applied to the impact estimates from Emerick et al. (2016) to consider cost effectiveness. The analysis concludes by showing suggestive evidence on whether the field days were direct substitutes for farmer-to-farmer learning.

4.1 Regression framework

Our experimental design affords us a simple estimating equation. The reduced-form effect of field days is estimated with

$$y_{ivb} = \beta_0 + \beta_1 FieldDay_{vb} + \beta_2 X_{ivb} + \alpha_b + \varepsilon_{ivb}, \tag{1}$$

where y_{ivb} is some outcome (usually seed adoption) for household i located in village v and block b. Our main estimate is β_1, the average treatment effect of the field days. In some specifications we verify that our results are unaffected when including household covariates X_{ivb}. The villages in the experiment were spread across three blocks (an administrative unit which was a stratification variable) and therefore we include blocked fixed effects, i.e. the α_b

terms. Finally, ε_{ivb} is a random error term which we allow to be correlated within villages but assume to be uncorrelated across villages.

The variation induced by the experiment also identifies the effects of field days for each of the three methods of choosing demonstrators:

$$\begin{aligned} y_{ivb} = \beta_0 + \beta_1 FieldDay_{vb} + \beta_2 FieldDay_{vb} * Meet_{vb} + \beta_3 FieldDay_{vb} * SHG_{vb} \\ + \beta_4 Meet_{vb} + \beta_5 SHG_{vb} + \beta_6 X_{ivb} + \alpha_b + \varepsilon_{ivb}. \end{aligned} \tag{2}$$

The main coefficients of interest are β_2 and β_3 which measure whether field days are more (or less) effective in villages where demonstrators were identified with meetings.

4.2 Effects of field days on knowledge

A useful question before turning to demand effects is was there any measurable learning from the farmer field days? Prior to the door-to-door sales, enumerators surveyed households to assess their knowledge of Swarna-Sub1. Farmers were asked several questions, starting with whether they had ever heard of Swarna-Sub1 and how many farmers they had spoken to about the variety. We then asked several multiple choice questions such as the two differences between Swarna-Sub1 and Swarna, the length of flooding that Swarna-Sub1 can tolerate, and the duration of Swarna-Sub1 (days from planting to harvesting).[16]

Table 2 shows some modest effects of the field days on these observable measures of learning. Starting with column 1, farmers are six percentage points more likely to have ever heard of Swarna-Sub1 in field day villages. However, this is compared to a fairly high base: almost 80 percent of farmers knew of Swarna-Sub1 in control villages. Column 2 shows that farmers in field day villages report talking to an additional 0.12 famers about Swarna-Sub1 — an approximate 20 percent effect. Knowledge on attributes of the technology in columns 3 through 6 are somewhat mixed. The strongest effect is in column 4 where field days led to an approximate 55 percent increase in knowledge of how long Swarna-Sub1 can survive when flooded. Column 6 shows that farmers in field day villages were slightly more likely to know the duration of Swarna-Sub1, although this effect is also modest because of high knowledge in the control group.

The inability to measure all aspects of learning is an important caveat of this exercise. There are certainly some attributes of the technology that we do not measure. It is further difficult to know what type of information would be transmitted since most of the field days involved an unstructured discussion. These results on knoweldge therefore represent a more intermediate check before considering the more important outcome of adoption.

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[16] In addition to flood tolerance, Swarna-Sub1 has a white husk, making it distinguishable from Swarna.

4.3 Effects of field days on adoption

The experiment was designed with the policy objective of increasing the diffusion of a proven agricultural technology. Adoption serves as the main outcome of interest. We first measure adoption directly with the door-to-door sales which were carried out just before planting.

Table 3 shows the main result that field days caused increased adoption. Focusing on column 1, the field days increased adoption by 12.2 percentage points. The rate of adoption in control villages was 29.7 percent. The point estimate therefore indicates that this relatively simple method of bringing farmers together to discuss a new technology leads to a 41 percent gain in adoption. Interestingly, column 2 shows a larger effect on adoption of a single package of seeds. Adoption of just one five kilogram package increases by 8.6 percentage points — or 59 percent. On the other hand, adoption of two packages increases by only 3.6 percentage points and this effect is statistically insignificant. Our data do not allow us to pinpoint an exact reason for this difference. Nonetheless, one possibility is that the field days provided additional information to farmers that were near the threshold of simply trying the new seed. But field days were less impactful for farmers that had already decided to plant the variety on a larger share of their land.

Including several household control variables does not change the main result (columns 4 through 6). The point estimates remain nearly identical to those that only use the experimental variation. This is not surprising given that the randomization was successful at achieving balance between the experimental groups.

Figure 2 helps further understand this effect by showing the distribution of the village-level adoption rates for the two types of villages. Two things stand out from the figure. First, the field days decreased the frequency of little or no adoption at the village level. 35 percent of control villages had adoption rates lower than 10 percent. In contrast, only 12 percent of field day villages had such low adoption rates. Second, the distribution for field day villages puts much more mass on adoption rates greater than 50 percent. 38 percent of field day villages had adoption rates of 50 percent or higher, while only 19 percent of the control villages had at least half of the farmers adopt.

Figure 3 verifies that the effect of field days is strongly correlated with attendance. The adoption rates of farmers in treatment villages that did not attend the field days are almost identical to adoption rates in control villages. In contrast, adoption rates are about 50 percent higher for attendees in treatment villages. Attendance is certainly non-random and perhaps correlated with unobservables that we do not measure in Table A1. Nonetheless, the result presents useful — but certainly suggestive — evidence that whatever learning happened at the field days likely did not spill over to non-attendees.

Turning to heterogeneity, the field days were most effective for poorer farmers. Two indicators of poverty status are readily available in the data. Around a third of the sample belongs to the scheduled castes or tribes, the most disadvantaged castes in the country. Members of scheduled castes and tribes obtain less education and earn lower incomes relative to higher caste individuals (Munshi and Rosenzweig, 2003). Column 1 in Table 4 shows that the marginal impact of field days on adoption for higher caste farmers is 8.3 percentage points. This impact increases to 20.1 percentage points for farmers belonging to the scheduled castes and tribes — however the large differential effect is not statistically significant (p=0.14). Column 2 shows that this differential effect is largely driven by inducing lower caste farmers to purchase a single package of seeds. The effect of field days on adoption of a single package is only 4.6 percentage points for higher caste farmers. In contrast, the effect is over three times larger for scheduled castes and tribes and the differential effect is statistically significant at the 10 percent level. Column 3 shows that there is virtually no differential effect for scheduled castes and tribes on the probability of purchasing two packages.

In addition to caste status, we also use possession of a "below the poverty line" card, which is meant to deliver various social assistance benefits to poor households. About 62 percent of our sample holds one of these cards, which are allocated based on results from a proxy means test. We observe a similar pattern where field days are more likely to induce BPL households to purchase a single package of seeds. In particular, column 5 shows that the effect of field days on adoption of five kilograms is larger by 10.1 percentage points for BPL households.

In combination, the results suggest that field days increase equity by delivering the largest impacts for the poorest farmers. These gains exist despite a lack of evidence that poor farmers learn better from field days. Tables A3 and A4 show that the effect of field days on observed learning is not significantly larger for either ST/SC or BPL households. Therefore, the differential effects on adoption must arise for a reason different than differential learning — at least for the attributes measured by our survey.

4.4 Does the effect of field days vary by method of selecting demonstrators?

We first establish that demonstrators identified by either village or SHG meetings differ from demonstrators selected by ward members. In short, the meetings reduced the likelihood that demonstrators would be close family and friends of ward members. We estimate regressions of the form

$$y_{ivb} = \beta_0 + \beta_1 Meet_{vb} + \beta_2 SHG_{vb} + \varepsilon_{ivb}, \tag{3}$$

where y is a characteristic of the demonstrator or their household and $Meet_{vb}$ and SHG_{vb} are indicators for meeting and SHG meeting villages, respectively.[17] We show these coefficient estimates for various characteristics that were collected when our survey teams returned to villages after seed distribution to gather additional information from demonstrators.

Table 5 shows the results. Elected officials favor their close family and friends when selecting demonstrators. The first row in column 1 shows that 30.5 percent of demonstrators are either the ward member or her family / close friend in villages where demonstrators were selected by ward members. This falls by 9.6 and 18.8 percentage points in meeting and SHG meeting villages, respectively. Turning to row 2, part of this effect is the ward member keeping seeds for him or herself. 11 percent of demonstrator households had an elected panchayat representative in ward member villages. The share of households having an elected panchayat representative was significantly lower in village meeting and SHG meeting households.[18]

In addition to proximity to the ward member, other characteristics of demonstrators vary across the treatments. Not surprisingly, demonstrators in SHG meeting villages come from entirely SHG households. This is a consequence of using SHG meetings which consist entirely of women. Row 4 shows that demonstrators in SHG villages are significantly more likely to be family or close friends with the SHG president — relative to village meeting villages. Focusing on the other rows, adopters in SHG villages are more likely to be part of agricultural cooperatives, are younger, and are less likely to come from disadvantaged castes.

Other than connections to elected officials, there are few differences between demonstrators in ward member and village meeting villages. Livestock ownership is the only significant difference between the two groups. A plausible — but somewhat surprising — interpretation of this pattern is that people connected to the ward member are roughly representative of the average villager.

How were the demonstrators chosen at the meetings? Table A5 shows results where we aggregate various outcomes to the village level in order to study whether the seed was demonstrated differently in the different types of villages. There are three noteworthy results. First, almost all 25 kilograms were distributed and planted in each village.[19] Second,

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[17] These regressions are meant to measure selection. The block fixed effects are dropped from the regression to avoid absorbing any selection effects.

[18] We removed the ward member households from the data and re-estimated the regression in the first row. The coefficient for village meeting villages decreases and becomes statistically insignificant, but the coefficient for SHG meeting villages remains negative and statistically significant. In addition, both variables jointly explain the likelihood that the adopter is connected to the ward member (p-value of F statistic = 0.05). This helps ensure that this effect is not entirely driven by the ward member keeping seeds for their own household.

[19] At this stage we define planting as accepting the seeds and planting them in a nursery bed.

village meetings — but not SHG meetings — were over twice as likely to result in more than five demonstrators relative to selection via ward members. In other words, the five kilogram bags were more likely to be opened and redistributed in smaller quantities in village meeting villages. Third, some demonstrators were unsuccessful with the variety because the seedlings were planted in an area affected by flooding and the technology is not tolerant to flooding when seedlings have just emerged.[20] This was slightly more likely with demonstrators selected from SHG meetings relative to village meetings.

Despite these differences in characteristics and numbers of demonstrators, the field days were no more effective when demonstrators were identified with meetings. Table 6 shows the complete specification in (2) where the field day indicator is interacted with the indicators for village and SHG meetings. The second rows shows that if anything, the field days were less effective when early adopters were selected by SHG meetings. The coefficient on the interaction term between field days and SHG meetings is negative, somewhat large, but statistically insignificant. Similarly, the coefficient on the interaction between field days and village meetings is also negative and imprecisely estimated. Nonetheless, our findings are not compatible with the idea that farmers gain more from field days when demonstrators are chosen to represent the village rather than by an elected offical. Farmers appear to gain just as much — if not more — from participating in field days when demonstrators are identified by ward members.

The level effects of using meetings to identify demonstrators are also small and statistically insignificant. Using SHG meetings in the absence of field days led to 7.3 percentage point increase in adoption, but this effect is imprecisely estimated. The effect of using village meetings is closer to zero and also imprecisely estimated. The meetings were ineffective in field day villages. The impact of selecting demonstrators with SHG meetings is a 5.2 percentage point decrease (-.052 = .073 - .125) in adoption. The effect of village meetings in field day villages is also negative and of a similar order of magnitude.

Table 7 clarifies this result by showing the average effect of both types of meetings. The average effect of using meetings across both field day and control villages is effectively zero. The point estimate in column 1 of -.005 is estimated with more precision since more observations are pooled together. In particular, the 95 percent confidence interval allows us to rule out effects larger than 8.8 percentage points or 24.7 percent relative to adoption in villages where ward members selected demonstrators. Columns 2 through 6 show that this conclusion changes little when splitting adoption into purchases of one or two bags or when

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[20] The common planting technique in our sample is transplanting, i.e. planting seedings in a small area and then uprooting these seedlings after three weeks and transplant them to the larger field where the crop will be grown.

controlling for household characteristics.

In combination, we don't find any evidence that engaging villagers to participate in selection of demonstrators drives future technology diffusion. In practice the result means that although relying on elected officials to identify early program beneficiaries does induce favoritism, this favoritism has no consequences on the rate at which the new technology diffuses to other villagers. This result contrasts somewhat with the literature on targeting of anti-poverty programs. Engaging villagers in meetings has been shown in that context to improve the efficiency at which anti-poverty benefits are targeted to the poor, largely because villagers possess better information on poverty status (Alatas et al., 2012). While targeting anti-poverty programs is a different objective than encouraging learning about technology, our results are less favorable for the use of participatory meetings when it comes to identifying demonstrators of new technology.

Finally, we show that results change little when measuring adoption from all sources — not just our door-to-door sales. In addition to the sales, farmers could have obtained seeds directly from demonstrators. Adoption from our door-to-door sales would present an inaccurate picture if either of the two meeting types were more successful at identifying early adopters that are better seed distributors or if field days induced more farmer-to-farmer sharing of seeds.

The results are similar when estimated with the entire village. Column 1 in Table 8 shows the specification for all villagers — including demonstrators. The coefficients for both village and SHG meetings remain small and statistically insignificant. However, we continue to estimate large positive effects of field days on seed demand at the village level. Field days caused in increase in adoption of 6.2 percentage points, or around 60 percent.[21] Column 2 shows that we still fail to detect significant interaction effects between meetings and field days in this larger sample.

Measuring adoption for the entire village eliminates any concern that selection into the sample of demonstrators is responsible for the results. The relatively small number of demonstrators — relative to village size — seems to limit this particular type of sample selection.

4.5 Are field days cost effective?

A simple calculation suggests that field days are cost effective. The average village in our sample has 69 rice-farming households. Thus, a field day would be expected to generate around 8.3 additional adopters. In Emerick et al. (2016) we estimate revenue gains of 10

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[21] The much lower adoption rate at the village level is indicative of supply constraints. 35 percent of our sample that received door-to-door sales adopted, a number much larger than the adoption rate amongst other villagers.

percent or around 2,969 rupees. This effect arises largely due to the crowd-in effect of inducing farmers to use more inputs. Thus, field days generate one-year revenue gains of around 24,643 rupees, or around 410 dollars. Our partner NGO required approximately 200 dollars per village to execute the farmer field days. A rough estimate of the time cost to farmers of attending the field days is 29.7 dollars.[22] Therefore, farmer field days are immediately cost effective after a single growing season.

One the one hand this calculation is encouraging because the one-year benefits are a lower bound on the flow of benefits received from continued adoption. On the other hand, the calculation should be interpreted with caution for two reasons. First, it is unclear whether the average cost of the treatment would rise with wider implementation. There could be additional costs of coordinating field days in a broader area. Second, our estimated treatment effect is conditional on the absence of supply frictions. We can only think of field days as being cost effective when seeds are readily available to farmers at market prices.

4.6 Are field days complements or substitutes for farmer-to-farmer learning?

Field days could work by simply transmitting knowledge to farmers that do not communicate with demonstrators. Put differently, learning from peers is imperfect due to the underlying structure of the social network. This explanation implies that the treatment should be most effective for farmers who are unlikely to communicate with demonstrators. We test this idea using various measures of social distance between sample farmers and demonstrators.

The data are less compatible with the substitutability explanation. Starting with column 1 in Table 9, we interact the treatment indicator with the number of demonstrators sharing the same surname.[23] While substitutability would predict a negative coefficient, the point estimate on the interaction term is effectively zero. Columns 2 and 3 combine GIS data on the plot locations of demonstrators and the houses of farmers in the estimation sample. We calculate for each farmer the number of Swarna-Sub1 plots of demonstrators within a given radius from their household. We allow this radius to vary from 250 to 500 meters.[24] The field days are no more effective for farmers that lived the furthest from the Swarna-Sub1

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[22] This calculation is based on daily wages of 174 rupees (2.9 dollars), i.e. the wage in the central government's labor guarantee program. We multiply this by 0.25 since the field days took approximately two hours. Finally, an average of 41 farmers attended the field days.

[23] Surnames are a common marker of social connection in rural India, largely because of their tight association with caste. The average farmer in the sample shares the same surname with one demonstrator. Figure A1 shows the distribution of this measure. 60 percent of farmers have a surname that is common with zero demonstrators. At the maximum, one farmer in the sample has the same surname as 11 demonstrators.

[24] Figure A2 shows the distribution of the two measures across the entire sample.

fields. Additional results in the online appendix show that the same conclusion is reached when we relax the assumption that the treatment effect varies linearly with the number of connected demonstrators.[25]

If anything, the field days were complementary with having an opportunity to learn from peers. More specifically, we focus our estimation on the SHG villages and test whether the field days were less effective for households having an SHG member. Essentially all of the demonstrators were SHG member households in SHG meeting villages. Therefore, we would expect field days (with the whole village) to be the least effective for SHG households if field days only transmit information to unconnected farmers. The interaction effect between field days and the SHG household indicator in column 4 of Table 9 is large and positive, but not statistically significant (p=0.22). Column 5 estimates a larger — and marginally significant — interaction effect when we use an indicator for households that report being a friend or family member of the SHG president. In short, the field days were only effective for households that are close friends or family of the SHG president.

Table A7 shows that these two interaction effects are robust to controlling for household characteristics. The coefficients actually increase in size and significance when adding controls. More importantly, we also estimate the same two models for the ward member and village meeting villages. The field days had no differential effect for SHG households in these villages. This falsification exercise rules out the possibility that unobserved correlates of SHG affiliation make those households more likely to benefit from field days.

In sum, we interpret these findings to suggest that the field days do more than substitute for direct contact between farmers. The findings suggest that if anything the field days enhanced learning for farmers most likely to be linked to demonstrators and therefore know about the technology.[26] Or, farmers benefit more from field days when they share common characteristics with demonstrators.

5 Concluding Remarks

Slow learning is one of the several explanations why agricultural technology diffuses slowly. It simply takes time to observe a new seed variety or agricultural practice under the multiple states of nature where that variety or practice would be beneficial. The ability to learn from nearby farmers has been shown to improve this process and thus likely makes technology

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[25] Figure A3 shows separate treatment effects for farmers that are "close" to 0, 1, or 2 or more demonstrators. There is no clear pattern where the effect of field days is smallest for farmers that are close to more demonstrators.

[26] Table A6 shows evidence that independently of field days, SHG households possess more knowledge about Swarna-Sub1 in villages where SHG meetings were used to identify demonstrators.

diffuse faster. The important next question is are there ways to improve how farmers learn from each other?

We have shown that the farmer field day — where the experiences of demonstrators are explicitly shared with other farmers — is one way to improve learning and increase technology adoption in a cost effective manner. The magnitude of this effect is non-trivial: field days increased adoption rates of an improved technology by 40 percent. This result suggests that learning is a key barrier that slows the diffusion of agricultural technology. Consequently, it is not enough to rely on select farmers to demonstrate and spread information. Rather, there is room to increase adoption by intervening to encourage farmers to better learn from each other's experience, particularly if peers will continue to be the dominant source of learning about new agricultural technology.

We showed suggestive evidence that the mechanism behind this effect is not that the field days simply engaged farmers that otherwise were unlikely to learn from demonstrators. We found that the field days were equally effective or if anything, they enhanced the learning ability of those that were the most likely to interact with demonstrators. More concretely, the field days appear to be a complement to learning from peers rather than a substitute.

In addition, we considered whether ex-ante the selection of demonstrators can be improved by seeking the input of various farmers through village meetings. We found that these meetings do change the composition of the group of demonstrators. More specifically, using meetings shifts the pool of demonstrators away from friends and family of the locally elected political figures. However, this has no meaningful effect on technology adoption one season later. This result in no way means that careful selection of demonstrators is unimportant. Indeed, Beaman et al. (2015) and BenYishay and Mobarak (2015) suggest that improved selection of demonstrators can increase adoption. Instead, our results suggest that using meetings to engage villagers in this selection process does little to drive adoption. Thus, future work is needed to identify the most policy relevant and scalable methods to improve the selection of demonstrators.

The experiment delivers a straightforward policy lesson. It is important to consider methods to enhance or optimize the ways in which farmers learn about new technologies. Our results suggest than an easy method does not involve much of a deviation from traditional agricultural extension. Rather than exploiting social learning alone, improved extension models could combine social learning from selected demonstrator farmers with simple interventions to encourage learning and sharing of results from demonstration.

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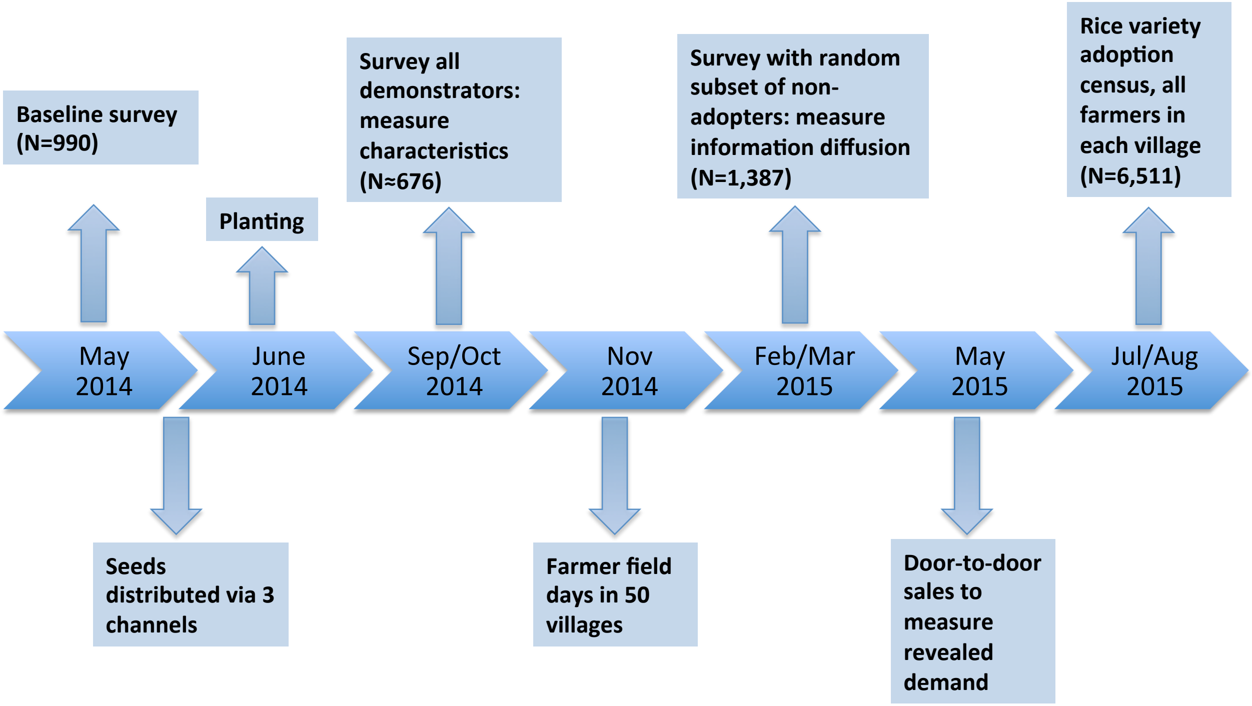
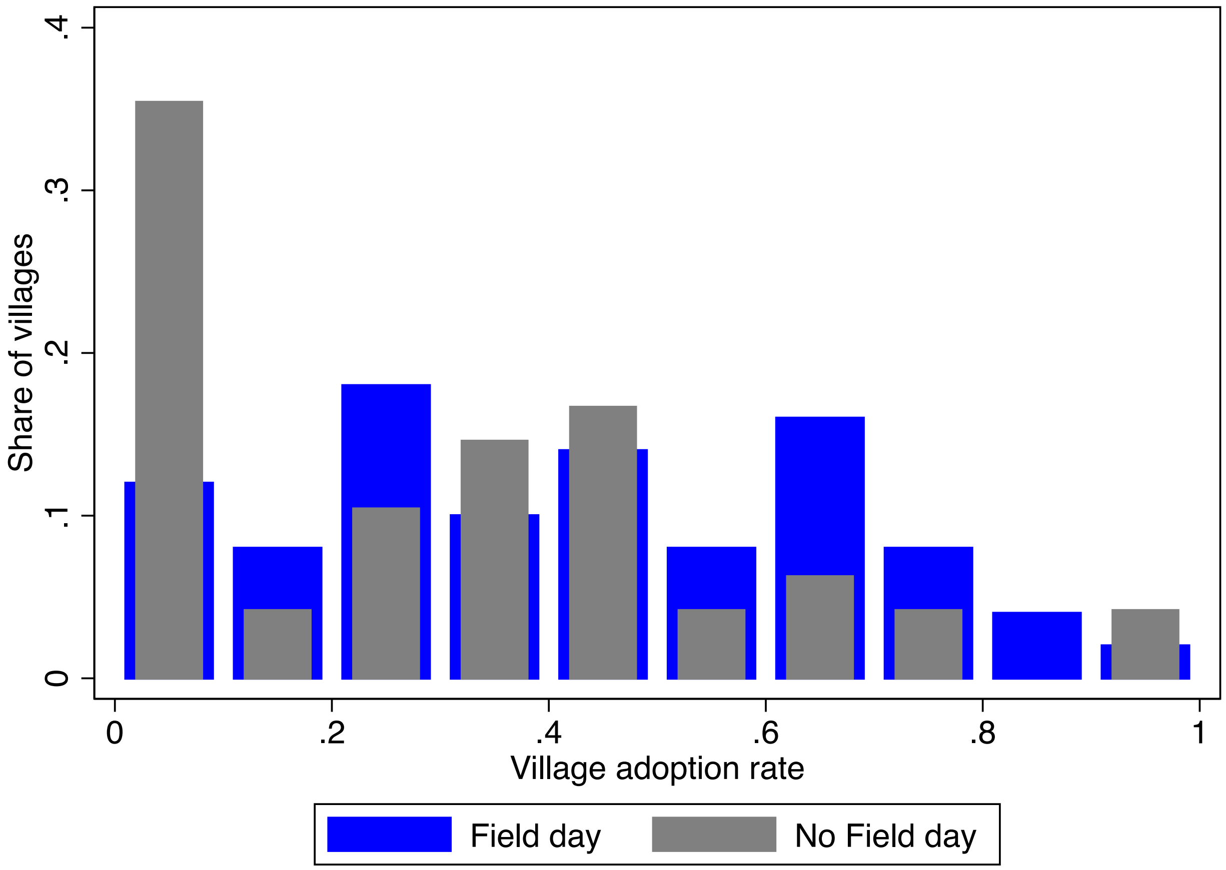
Figures

Figure 1: Timeline of the experimental design



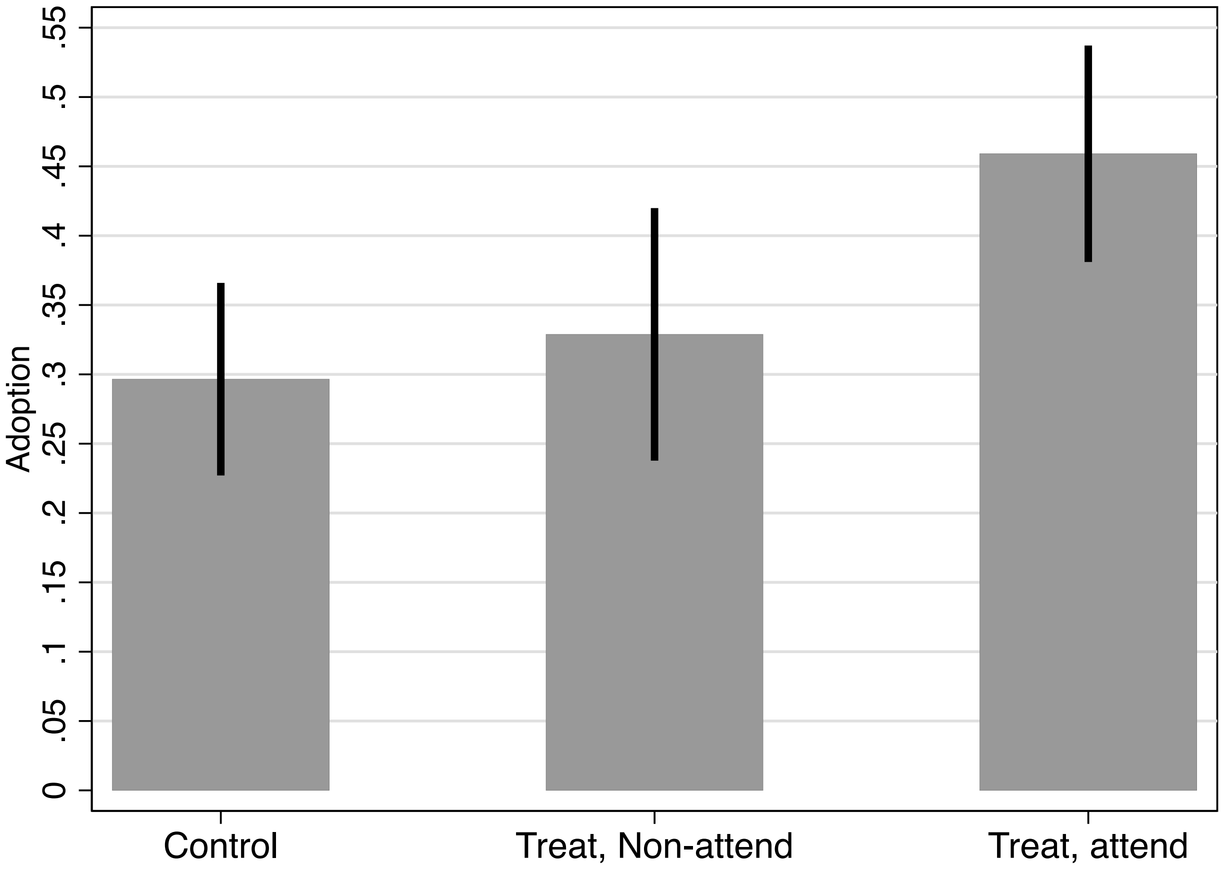
Notes: The figure shows the timing of the activities that were carried out as part of the experiment. Planting for each season occurs in June and harvesting generally occurs in late November to December.

Figure 2: Distribution of village-level adoption rates by treatment



Notes: The figure shows the distribution of the village-level adoption rate for field day and non-field day villages separately. The distributions are based on the adoption data for the approximately 15 farmers per village that received door-to-door sales.

Figure 3: Adoption rates separately for field day attendees and non-attendees



Notes: The figure shows the raw adoption rates for farmers in control villages, farmers in treatment (field day) villages that did not attend the field days, and farmers in treatment villages that attended the field days. The black heavy lines are 95 percent confidence intervals, where standard errors are clustered at the village level.

Tables

Table 1: Summary statistics and balance tests

	Means		
	No Field Day	Field Day	p-value
Access to electricity	0.781	0.750	0.467
Mud walls	0.579	0.562	0.515
Thatched roof	0.333	0.340	0.925
Number rooms in house	2.044	2.193	0.128
Years education	5.998	5.970	0.903
Area cultivated in wet season (acres)	2.310	2.094	0.625
Private tubewells in house	0.353	0.331	0.927
Number of cows owned	1.805	1.862	0.627
Swarna user	0.526	0.610	0.088
Has SHG member	0.635	0.649	0.567
Owns mobile phone	0.792	0.837	0.119
BPL card holder	0.598	0.633	0.331
NREGS job card holder	0.671	0.642	0.445
Scheduled Caste or Tribe	0.312	0.337	0.760
Owns television	0.529	0.543	0.679
Owns motorbike	0.175	0.196	0.372
Owns refrigerator	0.042	0.061	0.155

The table shows summary statistics and balance tests using the main estimation sample of 1,387 farmers. Column 1 displays the mean value of each characteristic in the 50 villages without farmer field days. Column 2 shows mean values in the field day villages. Column 3 shows the p-values for tests of equality, where standard errors are clustered at the village level and each regression includes block fixed effects (randomization strata).

Table 2: Effects of field days on knowledge

	(1) Ever heard of Swarna-Sub1	(2) Number farmers talked to	(3) Difference with Swarna	(4) Maximum survival when flooded	(5) Most suitable land type	(6) Length of growing cycle
Field day	0.060*	0.116*	-0.037	0.133***	0.057	0.070**
	(0.031)	(0.065)	(0.042)	(0.045)	(0.038)	(0.034)
Mean in control villages	0.794	0.572	0.431	0.243	0.725	0.819
Number of Observations	1385	1369	1387	1387	1387	1387
R squared	0.071	0.025	0.109	0.133	0.081	0.127

Data are for 1,387 households that were surveyed in between harvesting during the first season and planting for the second season. The dependent variables are as follows: Column 1 is an indicator for whether the respondent has ever heard of Swarna-Sub1 prior to the interview. Column 2 is the number of other farmers in the village talked to about Swarna-Sub1. Column 3 is an indicator for selecting both flood tolerance and husk color as the main differences between Swarna and Swarna-Sub1. Column 4 is an indicator for knowledge that Swarna-Sub1 can survive up to 2 weeks when the field is flooded during the vegetative stage of the growing season. Column 5 is an indicator for knowledge that Swarna-Sub1 is most appropriate for medium land where flash flooding occurs. Column 6 is an indicator for knowledge that the duration (time from planting to harvest) of Swarna-Sub1 is approximately 140 days. All regressions include block fixed effects. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * level

Table 3: Effects of field days on demand revealed with door-to-door sales

	(1) Buy	(2) Buy 5 KG	(3) Buy 10 KG	(4) Buy	(5) Buy 5 KG	(6) Buy 10 KG
Field day	0.122**	0.086**	0.036	0.121**	0.083**	0.038
	(0.048)	(0.043)	(0.032)	(0.047)	(0.042)	(0.032)
HH Controls	No	No	No	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean in control villages	0.297	0.147	0.150	0.297	0.147	0.150
Number of Observations	1384	1384	1384	1384	1384	1384
R squared	0.042	0.028	0.012	0.062	0.043	0.028

The dependent variable in columns 1 and 4 is an indicator for whether the farmer purchased Swarna-Sub1 when given a sales offer. The dependent variable in columns 2 and 5 is an indicator for purchase of 1 seed package (5 kg). The dependent variable in columns 3 and 6 is an indicator for purchase of 2 seed packages (10 kg). Household controls are indicator for ST or SC, indicator for BPL card, indicator for NREGS job card, cultivated area, indicator for thatched roof, indicator for mud walls, education of the farmer, age of the farmer, indicator for SHG membership, indicator for private tubewell ownership, indicator for cultivating Swarna, and elevation of the household. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 4: Differential effects of field days as functions of caste and poverty status

	(1) Buy	(2) Buy 5 KG	(3) Buy 10 KG	(4) Buy	(5) Buy 5 KG	(6) Buy 10 KG
Field day	0.083	0.046	0.036	0.073	0.022	0.051
	(0.050)	(0.048)	(0.039)	(0.062)	(0.057)	(0.039)
Field day * ST or SC	0.118	0.114*	0.004			
	(0.079)	(0.065)	(0.055)			
Field day * BPL card				0.079	0.101*	-0.022
				(0.059)	(0.055)	(0.044)
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean in non-field day villages	0.297	0.147	0.150	0.297	0.147	0.150
Number of Observations	1384	1384	1384	1384	1384	1384
R squared	0.066	0.047	0.028	0.064	0.047	0.028

The dependent variable in columns 1 and 4 is an indicator for whether the farmer purchased Swarna-Sub1 when given a sales offer. The dependent variable in columns 2 and 5 is an indicator for purchase of 1 seed package (5 kg). The dependent variable in columns 3 and 6 is an indicator for purchase of 2 seed packages (10 kg). Household controls are indicator for ST or SC, indicator for BPL card, indicator for NREGS job card, cultivated area, indicator for thatched roof, indicator for mud walls, education of the farmer, age of the farmer, indicator for SHG membership, indicator for private tubewell ownership, indicator for cultivating Swarna, and elevation of the household. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 5: Characteristics of demonstrators

		Coefficients and SE:		
	(1) Ward Mean	(2) Village meeting	(3) SHG meeting	(4) p-value (2)-(3)
Ward member, family,	0.305***	-0.096*	-0.188***	0.074
or close friend	(0.040)	(0.054)	(0.054)	
HH has elected	0.114***	-0.064**	-0.082***	0.319
panchayat representative	(0.023)	(0.026)	(0.026)	
HH has member of SHG	0.476***	0.056	0.508***	0.000
	(0.071)	(0.092)	(0.071)	
Family or close	0.138***	-0.030	0.064	0.039
friend with an SHG president	(0.033)	(0.041)	(0.050)	
Self reported	0.257***	-0.023	-0.028	0.922
village leader	(0.033)	(0.044)	(0.054)	
Cooperative member	0.210***	-0.001	0.179**	0.035
	(0.051)	(0.068)	(0.088)	
Scheduled Caste or	0.462***	-0.034	-0.196*	0.066
Tribe	(0.088)	(0.111)	(0.104)	
Education	5.652***	-0.293	0.092	0.497
	(0.599)	(0.710)	(0.729)	
Age	49.010***	1.526	-7.646***	0.000
	(0.992)	(1.344)	(1.337)	
Area cultivated	2.232***	-0.096	-0.305	0.312
	(0.212)	(0.279)	(0.234)	
Mud walls	0.533***	0.053	0.041	0.881
	(0.059)	(0.073)	(0.088)	
BPL card holder	0.610***	0.026	0.023	0.973
	(0.062)	(0.078)	(0.081)	
Cows owned	2.790***	-0.509*	-0.806***	0.247
	(0.234)	(0.296)	(0.296)	
Sharecrops land	0.452***	-0.024	-0.037	0.856
	(0.067)	(0.079)	(0.088)	
Cognitive ability	3.015***	-0.033	-0.119	0.552
	(0.086)	(0.130)	(0.136)	

The data are from the first survey with all 676 demonstrators. Each row shows regression coefficients of the listed characteristic on indicators for village meeting and SHG meeting villages. The omitted category is the ward member villages and thus the coefficient reported in column 1 is the constant for each regression. Column 4 reports the p-value for the test of equality of the village meeting and SHG meeting villages. Standard errors that are clustered in the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 6: Interaction effects between farmer field days and meetings

	(1) Buy	(2) Buy 5 KG	(3) Buy 10 KG	(4) Buy	(5) Buy 5 KG	(6) Buy 10 KG
Field day	0.184**	0.139**	0.045	0.187***	0.142**	0.045
	(0.070)	(0.058)	(0.044)	(0.067)	(0.055)	(0.043)
Field day * SHG meeting	-0.125	-0.148	0.023	-0.134	-0.154	0.020
	(0.108)	(0.100)	(0.071)	(0.102)	(0.096)	(0.070)
Field day * Village meeting	-0.066	-0.020	-0.047	-0.072	-0.032	-0.041
	(0.113)	(0.098)	(0.075)	(0.110)	(0.094)	(0.075)
SHG meeting	0.073	0.082	-0.009	0.078	0.092	-0.014
	(0.082)	(0.073)	(0.042)	(0.079)	(0.071)	(0.042)
Village meeting	0.015	0.017	-0.002	0.021	0.028	-0.007
	(0.078)	(0.055)	(0.058)	(0.074)	(0.053)	(0.057)
HH Controls	No	No	No	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean in control villages	0.297	0.147	0.150	0.297	0.147	0.150
Mean in ward villages	0.357	0.185	0.172	0.357	0.185	0.172
Number of Observations	1384	1384	1384	1384	1384	1384
R squared	0.046	0.035	0.015	0.066	0.050	0.030

The dependent variable in columns 1 and 4 is an indicator for whether the farmer purchased Swarna-Sub1 when given a sales offer. The dependent variable in columns 2 and 5 is an indicator for purchase of 1 seed package (5 kg). The dependent variable in columns 3 and 6 is an indicator for purchase of 2 seed packages (10 kg). Household controls are indicator for ST or SC, indicator for BPL card, indicator for NREGS job card, cultivated area, indicator for thatched roof, indicator for mud walls, education of the farmer, age of the farmer, indicator for SHG membership, indicator for private tubewell ownership, indicator for cultivating Swarna, and elevation of the household. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 7: Pooled effects of identifying demonstrators with meetings on seed adoption

	(1) Buy	(2) Buy 5 KG	(3) Buy 10 KG	(4) Buy	(5) Buy 5 KG	(6) Buy 10 KG
Village or SHG meeting	-0.005	0.007	-0.012	-0.004	0.013	-0.016
	(0.047)	(0.041)	(0.031)	(0.046)	(0.040)	(0.031)
Field day	0.123**	0.086**	0.037	0.121***	0.083**	0.039
	(0.047)	(0.042)	(0.032)	(0.046)	(0.042)	(0.032)
HH Controls	No	No	No	Yes	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean in non-field day villages	0.297	0.147	0.150	0.297	0.147	0.150
Mean in Ward villages	0.357	0.185	0.172	0.357	0.185	0.172
Number of Observations	1384	1384	1384	1384	1384	1384
R squared	0.043	0.028	0.013	0.062	0.043	0.029

The dependent variable in columns 1 and 4 is an indicator for whether the farmer purchased Swarna-Sub1 when given a sales offer. The dependent variable in columns 2 and 5 is an indicator for purchase of 1 seed package (5 kg). The dependent variable in columns 3 and 6 is an indicator for purchase of 2 seed packages (10 kg). Household controls are indicator for ST or SC, indicator for BPL card, indicator for NREGS job card, cultivated area, indicator for thatched roof, indicator for mud walls, education of the farmer, age of the farmer, indicator for SHG membership, indicator for private tubewell ownership, indicator for cultivating Swarna, and elevation of the household.. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 8: Effects of field days on adoption for the entire village, including demonstrators

	(1)	(2)
Village meeting	-0.008	-0.003
	(0.028)	(0.031)
SHG meeting	0.016	0.040
	(0.025)	(0.029)
Field day	0.062^{***}	0.079^{**}
	(0.022)	(0.033)
Field day * SHG meeting		-0.044
		(0.046)
Field day * Village meeting		-0.007
		(0.056)
Strata FE	Yes	Yes
Mean in control villages	0.103	0.103
Mean in Ward villages	0.147	0.147
Number of Observations	6511	6511
R squared	0.054	0.055

The dependent variable in both columns is an indicator for whether the farmer adopted Swarna-Sub1 for the 2015 season. The data are from a census of varietal adoption that was carried out with all households in each village shortly after planting decisions were made for the 2015 season. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table 9: Differential effects of farmer field days as a function of connectedness to demonstrators

	All villages			SHG villages	
	(1) Demo. w/ same surname	(2) Demo. fields within 250 M	(3) Demo. fields within 500 M	(4) SHG member household	(5) Friend / family of SHG president
Field day	0.130**	0.111**	0.090	-0.025	-0.023
	(0.052)	(0.052)	(0.061)	(0.116)	(0.099)
Interaction with Field day	-0.001	0.008	0.009	0.147	0.204*
	(0.025)	(0.024)	(0.013)	(0.118)	(0.101)
Level term	0.022	0.001	-0.000	0.032	-0.074
	(0.018)	(0.017)	(0.010)	(0.083)	(0.082)
Strata FE	Yes	Yes	Yes	Yes	Yes
Mean in control villages	0.303	0.296	0.296	0.350	0.350
Number of Observations	1354	1332	1332	445	445
R squared	0.048	0.044	0.046	0.057	0.052

The dependent variable in all columns is an indicator for whether the farmer purchased Swarna-Sub1 when given a door-to-door sales offer. Columns 1-3 contain all observations and columns 4-5 only includes SHG villages. The second row reports the coefficient on the interaction between the field day indicator and the variable corresponding to the column title. The third row reports the coefficient on the variable corresponding to the column title. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Appendix - For Online Publication

Table A1: Correlates of farmer field day attendance

	(1)	(2)
SHG meeting	0.015	0.014
	(0.097)	(0.094)
Village meeting	0.025	0.024
	(0.089)	(0.088)
ST or SC		0.056
		(0.046)
Age		0.001
		(0.001)
Educ. in years		0.011*
		(0.006)
HH has Below Poverty Line Card		-0.025 (0.044)
Rice area		-0.009
		(0.014)
Strata FE	Yes	Yes
p-value Village=SHG	0.88	0.88
Mean in Ward villages	0.671	0.671
Number of Observations	724	722
R squared	0.130	0.142

The dependent variable in both regressions is an indicator for attending the farmer field day. The data are for the 50 villages where field days took place. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A2: Summary statistics and balance tests

	Means			
	Ward Member	SHG Meeting	Village Meeting	Joint p-value
Access to electricity	0.780	0.725	0.788	0.379
Mud walls	0.558	0.596	0.558	0.689
Thatched roof	0.326	0.386	0.300	0.197
Number rooms in house	2.098	2.164	2.107	0.846
Years education	5.905	6.220	5.836	0.698
Area cultivated in wet season (acres)	1.874	2.652	2.093	0.363
Private tubewells in house	0.367	0.339	0.317	0.663
Number of cows owned	1.811	1.832	1.862	0.976
Swarna user	0.604	0.516	0.589	0.341
Has SHG member	0.670	0.652	0.604	0.411
Owns mobile phone	0.849	0.810	0.786	0.174
BPL card holder	0.556	0.648	0.648	0.060
NREGS job card holder	0.616	0.683	0.672	0.401
Scheduled Caste or Tribe	0.357	0.219	0.396	0.022
Owns television	0.552	0.491	0.565	0.353
Owns motorbike	0.185	0.172	0.201	0.685
Owns refrigerator	0.046	0.047	0.063	0.489

The table shows summary statistics and balance tests using the sample of 1,387 farmers that were not demonstrators during year one. Column 1 displays the mean value in villages where ward members (local politicians) were used to determine demonstrators. Columns 2 and 3 show means in villages where demonstrators were selected by SHG meetings and village meetings, respectively. Column 4 shows the p-values for equality of means in all three arms. Standard errors are clustered at the village level and each regression includes block fixed effects (randomization strata).

Table A3: Heterogeneous effect of field days on knowledge by caste

	(1) Ever heard of Swarna-Sub1	(2) Number farmers talked to	(3) Difference with Swarna	(4) Maximum survival when flooded	(5) Most suitable land type	(6) Length of growing cycle
Field day	0.064*	0.070	-0.035	0.179***	0.048	0.087**
	(0.036)	(0.079)	(0.048)	(0.048)	(0.042)	(0.037)
Field day * ST or SC	-0.015	0.145	-0.008	-0.139*	0.028	-0.049
	(0.048)	(0.104)	(0.075)	(0.073)	(0.065)	(0.064)
ST or SC	0.038	-0.122*	0.016	0.031	-0.044	-0.036
	(0.039)	(0.066)	(0.055)	(0.060)	(0.052)	(0.052)
Mean in control villages	0.794	0.572	0.431	0.243	0.725	0.819
Number of Observations	1385	1369	1387	1387	1387	1387
R squared	0.073	0.027	0.109	0.140	0.082	0.135

Data are for 1,387 households that were surveyed in between harvesting during the first season and planting for the second season. The dependent variables are as follows: Column 1 is an indicator for whether the respondent has ever heard of Swarna-Sub1 prior to the interview. Column 2 is the number of other farmers in the village talked to about Swarna-Sub1. Column 3 is an indicator for selecting both flood tolerance and husk color as the main differences between Swarna and Swarna-Sub1. Column 4 is an indicator for knowledge that Swarna-Sub1 can survive up to 2 weeks when the field is flooded during the vegetative stage of the growing season. Column 5 is an indicator for knowledge that Swarna-Sub1 is most appropriate for medium land where flash flooding occurs. Column 6 is an indicator for knowledge that the duration (time from planting to harvest) of Swarna-Sub1 is approximately 140 days. All regressions include block fixed effects. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * level

Table A4: Heterogeneous effect of field days on knowledge by BPL status

	(1) Ever heard of Swarna-Sub1	(2) Number farmers talked to	(3) Difference with Swarna	(4) Maximum survival when flooded	(5) Most suitable land type	(6) Length of growing cycle
Field day	0.015	0.049	-0.027	0.218***	0.056	0.093**
	(0.029)	(0.085)	(0.056)	(0.053)	(0.050)	(0.045)
Field day * BPL Card	0.076	0.115	-0.014	-0.140**	-0.001	-0.042
	(0.046)	(0.110)	(0.062)	(0.060)	(0.059)	(0.039)
BPL Card	-0.088**	-0.150**	-0.070	0.068	-0.020	0.009
	(0.034)	(0.059)	(0.043)	(0.043)	(0.044)	(0.029)
Mean in control villages	0.794	0.572	0.433	0.244	0.728	0.821
Number of Observations	1385	1369	1385	1385	1385	1385
R squared	0.078	0.029	0.115	0.139	0.082	0.130

Data are for 1,387 households that were surveyed in between harvesting during the first season and planting for the second season. The dependent variables are as follows: Column 1 is an indicator for whether the respondent has ever heard of Swarna-Sub1 prior to the interview. Column 2 is the number of other farmers in the village talked to about Swarna-Sub1. Column 3 is an indicator for selecting both flood tolerance and husk color as the main differences between Swarna and Swarna-Sub1. Column 4 is an indicator for knowledge that Swarna-Sub1 can survive up to 2 weeks when the field is flooded during the vegetative stage of the growing season. Column 5 is an indicator for knowledge that Swarna-Sub1 is most appropriate for medium land where flash flooding occurs. Column 6 is an indicator for knowledge that the duration (time from planting to harvest) of Swarna-Sub1 is approximately 140 days. All regressions include block fixed effects. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * level

Table A5: Village-level production characteristics of demonstrators

	(1) KG planted	(2) Number adopters	(3) More than 5 adopters	(4) Share successful adopters	(5) Acres transplanted
Village meeting	0.112	1.862	0.236**	-0.011	0.052
	(0.597)	(1.200)	(0.115)	(0.085)	(0.126)
SHG meeting	-0.339	-0.498	0.104	-0.153	-0.197
	(0.991)	(0.936)	(0.113)	(0.094)	(0.133)
p-value Village=SHG	0.63	0.03	0.28	0.13	0.07
Mean in Ward member villages	24.31	6.56	0.22	0.77	0.86
Number of Observations	96	96	96	96	96
R squared	0.003	0.054	0.043	0.036	0.040

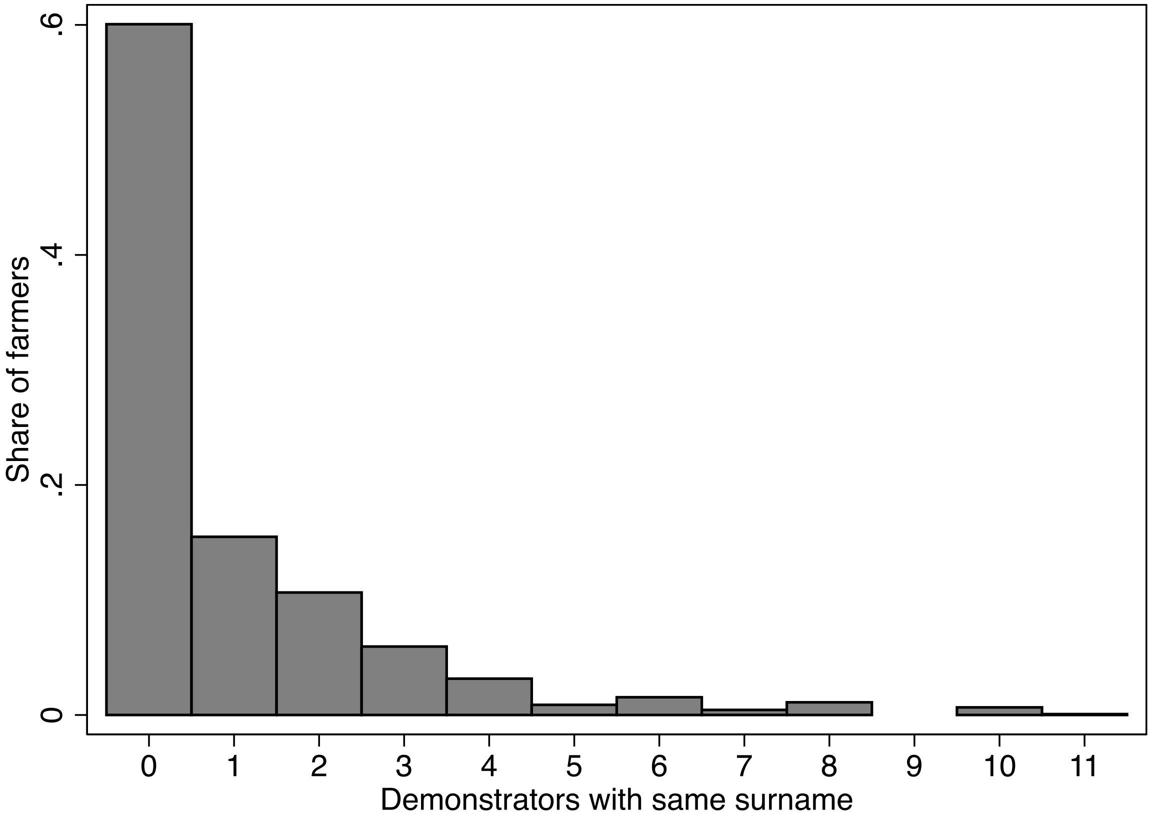
The data are from the first survey with all 676 demonstrators of Swarna-Sub1 and are collapsed to the village level. The survey was carried out in 96 of the 100 villages. The dependent variables are the total amount of kilograms planted in the nursery bed in the village (column 1), the total number of farmers that received any seed during year 1 (column 2), an indicator variable if there were more than 5 recipients in the village (column 3), the number of recipients that did not lose the crop during nursery-stage flooding (column 4), and the total acres transplanted in the main field (column 5). Heteroskedasticity robust standard errors are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.

Table A6: Heterogeneous effect of selecting demonstrators with SHG meetings

	(1) Ever heard of Swarna-Sub1	(2) Number farmers talked to	(3) Difference with Swarna	(4) Maximum survival when flooded	(5) Most suitable land type	(6) Length of growing cycle
SHG meeting	-0.015	-0.063	-0.103	-0.012	-0.040	0.019
	(0.048)	(0.098)	(0.066)	(0.073)	(0.062)	(0.064)
SHG meeting*SHG household	0.121**	0.158	0.170**	0.106	0.035	0.016
	(0.052)	(0.109)	(0.072)	(0.066)	(0.064)	(0.057)
SHG household	-0.126***	-0.105	-0.153***	-0.076	0.018	0.031
	(0.030)	(0.067)	(0.044)	(0.052)	(0.040)	(0.029)
Village meeting	0.044	-0.046	0.021	0.080	0.024	0.026
	(0.040)	(0.082)	(0.057)	(0.062)	(0.044)	(0.043)
Mean in control villages	0.794	0.572	0.431	0.243	0.725	0.819
Number of Observations	1385	1369	1387	1387	1387	1387
R squared	0.088	0.025	0.123	0.123	0.080	0.121

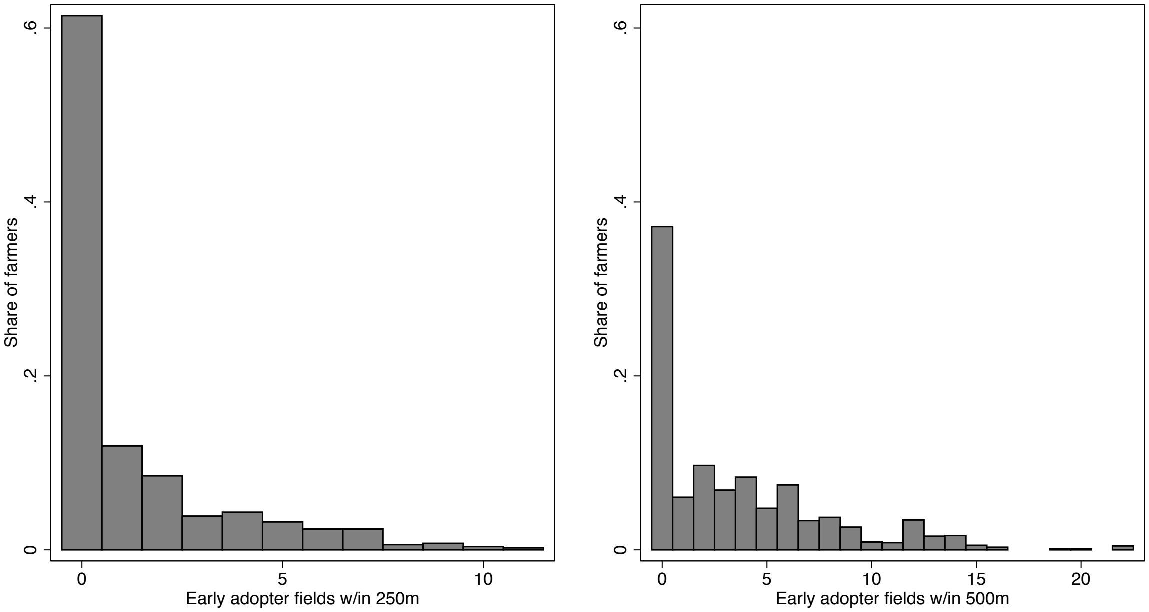
Data are for 1,387 households that were surveyed in between harvesting during the first season and planting for the second season. The dependent variables are as follows: Column 1 is an indicator for whether the respondent has ever heard of Swarna-Sub1 prior to the interview. Column 2 is the number of other farmers in the village talked to about Swarna-Sub1. Column 3 is an indicator for selecting both flood tolerance and husk color as the main differences between Swarna and Swarna-Sub1. Column 4 is an indicator for knowledge that Swarna-Sub1 can survive up to 2 weeks when the field is flooded during the vegetative stage of the growing season. Column 5 is an indicator for knowledge that Swarna-Sub1 is most appropriate for medium land where flash flooding occurs. Column 6 is an indicator for knowledge that the duration (time from planting to harvest) of Swarna-Sub1 is approximately 140 days. All regressions include block fixed effects. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * level

Figure A1: Distribution of surname connections to demonstrators



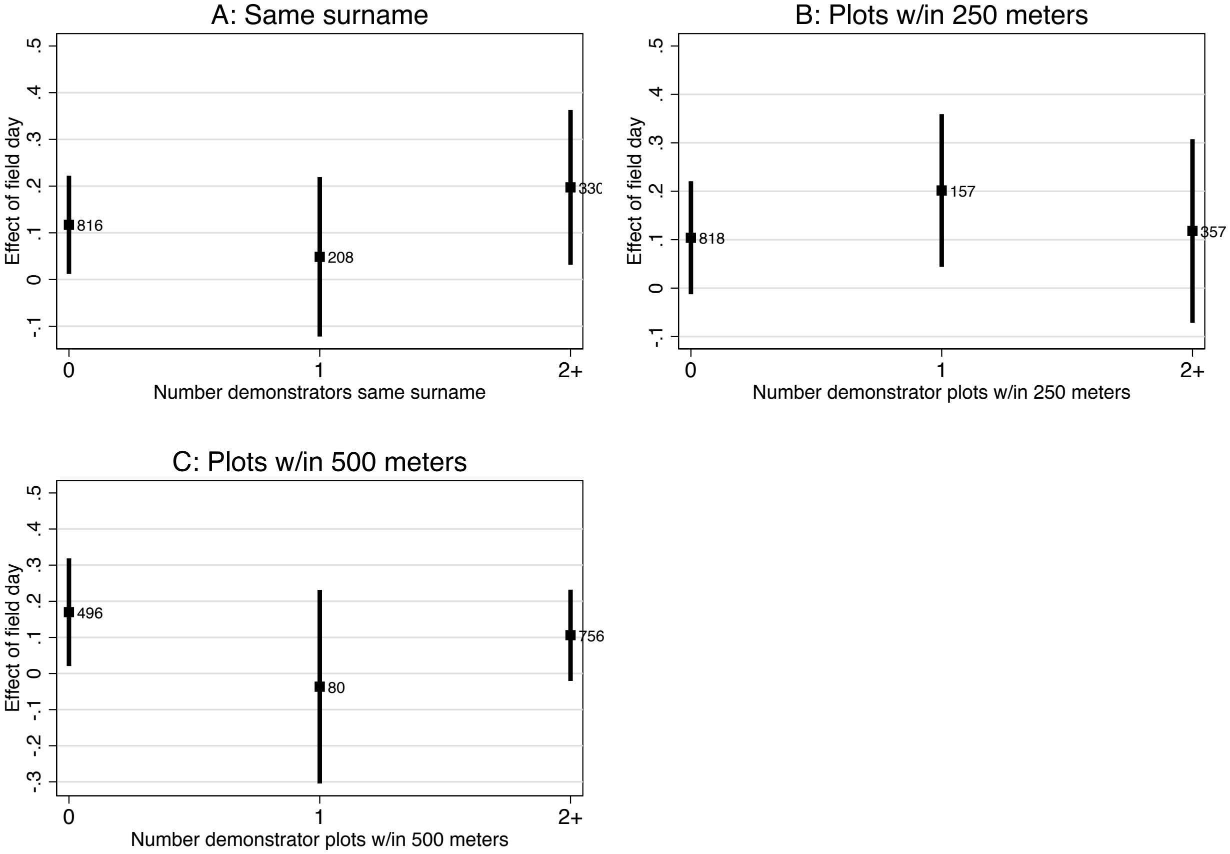
Notes: The figure uses the data for 1,387 non-adopting farmers to plot the histogram of the number of demonstrators with the same surname. The average farmer in the sample shares a surname with one demonstrator.

Figure A2: Distribution of number of demonstrator plots within 250 and 500 meters of the household



Notes: The figure uses the data for the sample of 1,340 non-adopting farmers to plot the histogram of the number of demonstrator fields within 250 meters (the left panel) and 500 meters (the right panel) of the household. There are 47 households for which we did not have GIS coordinates.

Figure A3: Heterogeneous effects of field days as function of distance to demonstrators



Notes: Each figure shows the effect of farmer field days for three separate groups of farmers. The squares represent treatment effects and the bands are 95 percent confidence intervals. The number printed next to each estimate gives the number of observations in that regression. Panel A shows separate effects of field days for farmers that had 0, 1, and 2 or more demonstrators sharing their same surname. Panels B and C show effects by proximity between demonstrators' plots and respondents' households. Each regression includes block fixed effects and standard errors are clustered at the village level — just as in our main specifications.

Table A7: Effects of farmer field days in SHG villages with household controls

	(1) SHG villages	(2) SHG villages	(3) Other villages	(4) Other villages
Field day	-0.092	-0.080	0.144	0.140*
	(0.119)	(0.095)	(0.088)	(0.074)
Field day * HH has SHG member	0.208*		0.008	
	(0.120)		(0.091)	
HH has SHG member	-0.009		0.064	
	(0.089)		(0.053)	
Field day * Friend/family of SHG president		0.277**		0.028
		(0.106)		(0.083)
Friend/family of SHG president		-0.120		0.076
		(0.098)		(0.059)
Strata FE	Yes	Yes	Yes	Yes
HH Controls	Yes	Yes	Yes	Yes
Mean in control villages	0.350	0.350	0.273	0.273
Number of Observations	445	445	939	937
R squared	0.113	0.112	0.065	0.069

The dependent variable in all columns is an indicator for whether the farmer purchased Swarna-Sub1 when given a door-to-door sales offer. The data are for SHG villages only in columns 1 and 2 and for ward member and village meeting villages in columns 3 and 4. Household controls are indicator for ST or SC, indicator for BPL card, indicator for NREGS job card, cultivated area, indicator for thatched roof, indicator for mud walls, education of the farmer, age of the farmer, indicator for private tubewell ownership, indicator for cultivating Swarna, and elevation of the household. Standard errors that are clustered at the village level are in parentheses. Asterisks indicate that coefficient is statistically significant at the 1% ***, 5% **, and 10% * levels.