Can Referral Improve Targeting? Evidence from a Vocational Training Experiment*

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Abstract

We seek to improve the targeting of vocational training by inviting past trainees to select future trainees from a candidate pool. Some referees are rewarded or incentivized. Training increases the adoption of recommended practices and improves performance on average, but not all trainees adopt. Referred trainees are 3.7% more likely to adopt, but rewarding or incentivizing referees does not improve referral quality. When referees receive financial compensation, average adoption increases and referee and referred are more likely to coordinate their adoption behavior.

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1. Introduction

The returns to many policy interventions vary a lot across individuals. To be imparted in a cost-effective manner, these interventions need to target individuals who most benefit from them. This is particularly true for vocational training, especially when the trainee needs to have a specific combination of ability, interest, and need in order to put the imparted skills to use. Mistargeting results in wasted training resources, and wasted time for the trainees.

In this paper we present results from a field experiment designed to test whether the targeting of vocational training can be improved by relying on referral by past trainees. The experimental design is inspired from the work of Beaman and Magruder (2012) – hereafter BM2012. In that experiment, lab subjects who performed an incentivized productivity task on day one were invited to refer a friend for the same task on day two. Some referees were paid a fixed fee; others were paid more if the person they referred turned out to be more productive. BM2012 find that referred day-two subjects are, on average, less productive than the day-one subjects who referred them. This difference is partially eliminated when referees are incentivized to refer someone productive.

Our experiment deviates from this design in two important dimensions. First, BM2012 focus on work referral while we focus on vocational training. Secondly, they rely on a laboratory experiment while we rely on a randomized controlled trial with a standard vocational training intervention. The reason for these changes is to make our findings more policy-relevant. Employers have incentives to use whatever recruitment method yields the best candidates – including referral if deemed useful (e.g., Granovetter 1974). Hence the need for policy intervention is unclear. In contrast, vocational training is often offered for free or at a subsidized price by a governmental or non-profit organization. This makes efficient targeting more difficult and less likely to arise serendipitously. Hence the need to explore better ways of selecting trainees in order to avoid the waste of public funds.

Our design differs from BM2012 in other, more subtle ways. We expand the range of incentives offered to referees to include no payment for referral. The objective of this inclusion is to investigate whether a payment is necessary or even useful – e.g., perhaps paying referees blunts intrinsic incentives to refer someone suitable (e.g., Gneezy and Rustichini 2000a, 2000b, 2011). Secondly, in BM2012, referral is largely unconstrained. In contrast, we allow trainees to only refer someone within a small pre-selected set of individuals. This introduces random variation in the extent to which referees face constraints in who they can refer. This helps us cast light on the motives pursued by referees when they recommend someone.

In practical terms, the intervention that we study is a short training course on a System of Rice Intensification (SRI) offered to rice farmers in rural Bangladesh. SRI is a low-input-intensity approach to rice cultivation that increases yields but requires more time and attention from the farmer (Uphoff 2003). While it offers promising prospects in Bangladesh, given the prevalence of rice cultivation and the abundance of labor, it is known not to be well suited for all farmers because it requires superior farming management skills (Moser and Barrett 2006). This makes it suitable to investigate whether referral can help target SRI training towards farmers capable of adopting it. The short training course was developed by BRAC, a large non-profit organization with operations in Bangladesh and other parts of the developing world. The SRI training that we offered follows the standard BRAC curriculum for SRI, ensuring the external validity of our results to this particular form of agricultural extension. Our measure of targeting quality is the extent to which trainees subsequently adopts some aspects of SRI: since SRI does not suit all farmers, cost-effectiveness considerations dictate that the training ought to be targeted towards farmers who are most likely to adopt it.

Our paper makes contributions to several literatures. First, we contribute to the literature on referral. Since Montgomery's (1991) seminal paper, referral has been studied principally in the context of labor markets. Referred workers have often been shown to earn higher wages, have higher productivity, and enjoy lower turnover and higher tenure than other workers (Datcher 1983; Korenman and Turner 1994; Holzer 1997; Kugler 2003; Antoninis 2006).[1] Such findings have often been interpreted by these authors as evidence of better match quality for referred workers (see also Castilla 2005). But there have been some dissenting voices, e.g., Fafchamps and Moradi (2016) find that Ghanaian army recruits hired through referral have lower unobserved quality. Others have argued that referral enhances efficiency by increasing effort and productivity through employee monitoring (e.g., Kugler 2003; Bandiera, Barankay and Rasul 2005; Heath 2017).

The experimental work of Beaman and Magruder (2012) have cast suspicion on the wisdom of relying blindly on worker referral to identify high productivity workers: even when referral is incentivized, the productivity of referred workers is no better than that of workers recruited directly. Our findings go in the same general direction, except that they apply to trainees of a vocational training course. In our case, trainee quality is assessed by their likelihood of putting in practice what they learned during their training. We find that referred trainees are on average little or no better than trainees selected at random from the same pool of potential candidates. Contrary to BM2012, we do not find that incentivizing referral produces a sizeable improvement in the average match quality of recruits for the training course. But when referees are *more* constrained in their choice of referral, they tend to pick a *less bad* match than they otherwise would. We also uncover evidence that, when constrained, referees are more likely to recommend someone with whom they have social ties.

Our findings also contribute to the literature on the diffusion of information in local com-

[1] See however Bentolila, Michelacci and Suarez (2010) who find that US and European workers referred through family and friends have a lower start-up wage.

munities, e.g., agricultural extension (Foster and Rosenzweig 1995, Bandiera and Rasul 2006, Conley and Udry 2010, Duflo et al. 2011, Genius et al. 2013), microfinance (Banerjee et al. 2013), or health information (Centola 2011, Oster and Thornton 2012). A common approach to extension is to rely on a small number of local agents or 'model farmers' who receive training and are then expected, without incentives, to spread the information to others in their community. Beaman et al. (2015) use a randomized controlled trial to test the effectiveness of this diffusion policy in Malawi. They find little evidence that agricultural knowledge spreads beyond the individuals directly targeted for training: most farmers need to learn about the technology from multiple people before they adopt themselves. In the same vein, Berg et al. (2017) show that health information diffused in local communities by unincentivized trained agents is often confined to members of the same caste. Only by incentivizing agents does information reach beyond caste boundaries. These examples illustrate the role of incentivization in circulating information locally. Our results unfortunately suggest that incentivization is insufficient to induce the elicitation of local information in order to better target information diffusion.

Our results also suggest that, when incentivized, the referral process generates peer effects. Different types of peer effects have been discussed in the context of diffusion processes. Some simply relate to the diffusion of information and its subsequent effect on behavior (e.g., Ryan and Gross 1947, Topa 2001, Oster and Thornton 2012, Fafchamps and Quinn 2016, BenYishay et al. 2016). Others have emphasized herding behavior and imitation (e.g., Banerjee 1992, Bobonis and Finan 2009, Centola 2010, Cai et al. 2013). Some of the spillover effects that we uncover could be driven by either of these processes. One possible channel that has received less attention is coordinated behavior between peers. An example of such pattern is documented in Bandiera et al. (2010) who show that, when matched into the same team, peers tend to adopt a similar behavior. The monitoring of referred co-workers can be put into the same broad category (e.g., Kugler 2003; Bandiera, Barankay and Rasul 2005; Heath 2017). We find that, when incentivized, a referee is more likely to coordinate his adoption behavior with that of the person he referred. A possible behavioral interpretation is that the referred trainee only adopts if the referee adopts as well – as if the referee is expected to 'put his money where his mouth is', that is, to practice himself what he recommended to a friend whose adoption will benefit him. Put differently, it is as if incentivizing the referee casts doubt on the value of the recommendation in the eyes of the referred trainee, and the referee has to demonstrate his own interest in the technology by adoption as well. If the referee fails to do so, the referred trainee refrains from doing as well.

The paper is organized as follows. In Section 2 we describe the experiment and sampling. Our conceptual framework and testing strategy are presented in Section 3. Empirical results appear in Section 4. The last section concludes.

2. Experimental design

The experiment is organized around a training program introducing farmers to a set of rice management practice commonly referred to as SRI (System of Rice Intensification). This set of practices has a demonstrated potential for increasing rice yields without requiring additional purchased inputs. For this reason SRI is often billed as pro-poor innovation. But it requires careful management of the plants, soil, water, and nutrients. Consequently it is intensive in labor and requires detailed knowledge and strong management skills.[2] For these reasons, it is not suited to all farmers. Targeting SRI training towards suitable farmers should therefore improve its cost-effectiveness. Unfortunately external agencies – such as BRAC, the provider of SRI training in our case – seldom have enough information to target farmers effectively, and

[2] More details about SRI are given in Appendix A.

adoption rates after training are low (Stoop et al. 2002; Karmakar et al. 2004).

The objective of our experimental design is to improve targeting by accessing the knowledge that rice farmers have about each other's labor capacity, management skills, ability to learn – and hence potential interest in SRI. To this effect, we divide the training into two batches, named B1 and B2. Farmers in the first batch are selected randomly. We ask them at the end of their training – when they have a better understanding of SRI requirements – to each nominate one other farmer for the second batch of training. The main premise behind the experiment is that the benefits from SRI training vary across farmers. Since only farmers who benefit from SRI should adopt it, we assume throughout that unobserved variation in the usefulness of training is correlated with subsequent adoption of the technique.

We expect trainees to nominate farmers for whom SRI is better suited if two conditions are satisfied: first, trainees are better able to predict who would most benefit from the training than random assignment by the training agency; secondly, they are willing to share this information with the training agency; and thirdly, they care enough about other farmers to want to nominate those who would benefit most from receiving the training.

The first condition is a priori reasonable: in small rural communities, farmers often know much about each other's strengths and weaknesses. It nonetheless requires that trainees not just know the characteristics of other farmers, but also be able to identify those characteristics required to benefit from SRI training. If the other conditions are satisfied, the first condition can be tested by comparing adoption rates between farmers who are referred for training and farmers who are randomly assigned to training. If referees are able to predict who benefits from training, adoption rates should be higher among trainees who were referred (i.e., B2 farmers) than among trainees who were chosen randomly (i.e., B1 farmers).

For this test to work, however, the other two conditions must hold. The second condition may fail if referring a well suited farmer takes care and effort. Without receiving a compensation for this effort from the training agency, the referee may refrain from putting sufficient effort in working out who would most benefit from training. To investigate this possibility, we vary the unconditional compensation offered to referees: in the first referral treatment (T1), referees receive no compensation, while in the second (T2) they receive a fixed fee for serving as referee. If referees are capable of identifying suitable candidates but need to be compensated to put in the effort, adoption rates among referred trainees should be higher under T2 than under T1.

It is also conceivable that conditions 1 and 2 are satisfied, but the third condition fails: trainees do not care enough about other farmers to want the training to be allocated to those who would benefit most. If farmers are indifferent to other farmers, there is no reason for them to make the effort to refer those who benefit from training: they need to be compensated to make the effort. To investigate this possibility, we introduce a third treatment (T3) in which referees receive a payment that is conditional on subsequent adoption by the person they referred. If trainees only recommend suitable farmers when incentivized, then adoption rates among randomly selected trainees should be equal to that of referred trainees in treatments T1 and T2, but lower than that of trainees referred in T3.

If referees resent farmers more successful than themselves, we expect them not to refer farmers that are more successful than themselves. To the extent that SRI requires good management skills and enough cognitive ability to understand and put in practice the complex SRI recommendations, it is reasonable to expect that those who would benefit from SRI already are better farmers before training. If this is true and referees behave in a rival or invidious manner, we expect referred trainees to be, on average, less likely to adopt SRI than randomly selected trainees. Incentivizing referees, either unconditionally (T2) or conditionally (T3), may nonetheless reverse this tendency. In this case, we expect adoption rates among referred trainees to be higher in T2 and/or T3 than in T1. If incentives fully compensate for rivalry, then adoption rates should be higher in T2 and/or T3 than among randomly assigned trainees.

As described so far, our experimental design resembles the work referral experiment of Beaman and Magruder (2012), except that it applies to farmers invited to an actual training program, and that an unincentivized referral treatment (T1) has been added. There is, however, one important innovation in our design to which we now turn. In the original experiment of Beaman and Magruder, referees can refer anyone they like – with a few exceptions (e.g., household members). In our experiment, referees must choose someone within a specific pool of farmers identified by the training agency as potential targets for SRI. This seriously limits the range of individuals they can refer. In practice this is achieved by first identifying in each study village a pool of 30 or so potential trainees. We then set the size of each training batch b_v in a village v to be a random value between 5 and 15. A number b_v of farmers is then randomly selected from the village pool to be trained first. We refer to these as B1 farmers. We then train a second batch of farmers, referred to as B2 farmers. These are selected as follows. At the end of their training, each B1 trainee in treatments T1, T2 or T3 is asked to refer one farmer out of those remaining in the pool. Since each B1 trainee refers one and only one B2 training, the size of the B2 training pool is also b_v. The selection is done sequentially, as follows. Trainees are first put in a random order. The trainee at the top of the line is asked to refer one trainee out of the remaining $30 - b_v$. That trainee is then taken out of the remaining pool. The next trainee is then invited to refer someone out of the remaining $30 - b_v - 1$, and so on until all trainees have referred one farmer from the pool. As a result, trainees who select first have more room for choice than those who select last. Variation in b_v further ensures variation across villages in how constrained the choice of B2 farmers is for referees.

This design has two benefits. First, it enables us to investigate whether farmers referred first are different from those referred last. This is particularly useful to clarify the respective roles of altruism and rivalry in explaining referral patterns. If farmers seek to refer those most likely to benefit from SRI training, then we should observe that those referred last are less likely to adopt SRI than those referred first. This is because it is easier to find high adopters when the pool is large than when it is small. The opposite is also true: if farmers deliberately seek out low adopters, e.g., out of spite for high adopters, or in the (misguided) intention of helping less able farmers, then those referred first should be less likely to adopt than those referred last. Second, it also generates exogenous variation in social and economic proximity between trainees, depending on the order in which they select a referral. This may provide better identification in the identification of peer effects, a point discussed more in detail at the end of the empirical section.

3. Testing strategy

Our testing strategy is directly based on our experimental design, and can be summarized as follows. The first three tests verify that the conditions are satisfied for targeting to be a relevant policy question. Tests 4 and 5 estimate the average treatment effects of selection due to the referral treatments. Test 6 investigates whether referral quality is higher when the choice of referees is less constrained. These are the main tests coming from our experimental design. All regressions have standard errors clustered at the village level. To check the robustness of our results to possible lack of balance on some household characteristics, we also estimate each test with additional controls.

1. Does training induce SRI adoption? To answer this question we test whether SRI adoption is higher among treated villages. If training has no effect on adoption, there is no point in

testing the effect of referral. The regression estimated over the entire sample is:

$$y_i = \alpha_0 + \sum_{k=1}^{3} \alpha_k V_{ki} + u_i \tag{3.1}$$

where y_i is an SRI adoption index for farmer i, with $y_i = 1$ if i adopts, and $V_{ki} = 1$ if farmer i resides in a village that received treatment k and 0 otherwise. If farmers in untreated villages do not practice SRI, then $\alpha_0 = 0$. If training induces SRI adoption, then $\alpha_k > 0$ for all k. To demonstrate that the treatment has real effects on material welfare, we also test whether the treatments affect crop production, revenue, costs, and profits using the same regression model.

2. Does training induce SRI adoption only by some farmers? The purpose of this test is to verify our assumption that returns to the SRI training vary across individuals. The estimated model is:

$$y_i = \alpha_0 + \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{ for } i \in C \cup B1 \tag{3.2}$$

where C denotes the set of control farmers, and $T_{ki} = 1$ if trainee i received treatment k and 0 otherwise. If SRI is not suitable for all farmers (or SRI training is not fully effective), then trainees will not all adopt SRI, and $\alpha_k < 1$ for all k. We only use B1 trainees because they are randomly selected.

3. Does the knowledge imparted by the SRI training diffuse immediately to all farmers in a village? If this is the case, we expect adoption rates to be similar between trained and untrained farmers within a village. If SRI knowledge diffuses easily, the policy relevance

of better targeting of the training vanishes. The estimated model is:

$$y_i = \alpha_0 + \sum_{k=1}^{3} \alpha_k T_{ki} + S_i \sum_{k=1}^{3} \beta_k T_{ki} + u_i \text{ for } i \in C \cup U \cup B1 \tag{3.3}$$

where U denotes the set of untrained farmers in treated villages and $S_i = 1$ if farmer i was trained and 0 otherwise. If untrained and trained farmers have the same propensity to adopt SRI, then $\beta_k = 0$ for all k. The bigger β_k, the bigger the role of training; the bigger γ_k, the stronger diffusion is.

4. Do B1 trainees refer individuals who are better targets for training? To answer this question, we test whether SRI adoption is higher among B2 trainees than among B1 trainees under any of the treatments. The estimated regression is:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + R_i \sum_{k=1}^{3} \beta_k T_{ki} + u_i \text{ for } i \in B1 \cup B2 \tag{3.4}$$

where $R_i = 1$ if farmer i was referred (i.e., belongs to B2) and 0 otherwise. If referral yields better targeting for treatment k, then $\beta_k > 0$.

5. Do B1 trainees refer better training targets when they are compensated or when they are incentivized? To answer the first question, we test whether SRI adoption if higher among B2 trainees under T2 and T3 than under T1. To answer the second, we test whether SRI adoption if higher among B2 trainees under T3 than under T1 and T2. The estimated regression is:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{ for } i \in B2 \tag{3.5}$$

The first test implies $\alpha_1 < \alpha_2, \alpha_3$. The second test implies $\alpha_3 > \alpha_2, \alpha_1$.

6. Do B1 trainees refer better training targets when their choice is less constrained? To

answer this question, we estimate a model of the form:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + C_i \sum_{k=1}^{3} \beta_k T_{ki} + u_i \text{ for } i \in B2 \tag{3.6}$$

where C_i measures the size of the pool faced by the farmer who recommended i for training.[3] If $\beta_k = 0$ it means that targeting does not depend on the size of the pool from which B1 farmers can select someone to recommend. If referees make an effort to identify farmers who would most benefit from the training, we expect $\beta_k > 0$: the less constrained their referee is, the more likely they are to have been positively selected.

We also investigate the presence of other patterns of interest in the data, in order to provide additional support to our findings. In particular, we test the following:

1. Does the referral behavior of B1 trainees suggest a preference towards socially proximate individuals? If referees tend to favor friends and relatives, it may be preferable to exclude such individuals from the list of people they can recommend. The estimated model is:

$$x_{ij} = \beta_0 + \beta_1 L_{ij} + \beta_2 L_{ij} C_i + \varepsilon_{ij} \tag{3.7}$$

where $x_{ij} = 1$ if trainee i refers farmer j and 0 otherwise, $L_{ij} = 1$ if i and j are socially close, and C_i measures the size of the selection pool when i made a referral. If referral is influenced by social proximity, we expect $\beta_1 > 0$ – farmers are more likely to refer someone socially proximate – and $\beta_2 > 0$ – preferential referral is more likely when the pool is less

[3] More precisely, let N_v be the number of sampled farmers in village v and let r_j be the referral rank of the B1 farmer who referred i – i.e., $r_j = 3$ if i was referred by B1 farmer j who was in third position when called to refer a B2 trainee. Then:

$$C_i = \frac{N_v - b_v - r_j}{30}$$

It follows that $C_i = 0$ when i was the only farmer that his referee could have recommended, i.e., the only remaining farmer in the pool. Division by 30 facilitates interpretation of coefficient β_k: when $C_i = 1$ it means that i's referee could have pick i among any of the 30 farmers in the (average) village sample.

constrained (and i is more likely to find a socially proximate person in it). If we do find evidence of such behavior in T1, we can investigate whether unconditional and conditional compensation offered in treatments T2 and T3 mitigate these effects by adding interaction terms.

2. Can referees predict SRI adoption better than what an external observer such as BRAC could do based on observables? The purpose of this test is to provide confirmation that referees have access to relevant information that the training agency could not extract directly from farmers' observables. Only if this is the case does it make sense to incentivize referral.[4] To investigate this possibility we first estimate a predictive regression based on a vector of farmer observables Z_i:

$$y_i = \theta_0 + \theta_1 Z_i + u_i \text{ for } i \in B1 \tag{3.8}$$

We only use B1 farmers to avoid selection effects. Given Z_i, the training agency could predict SRI adoption based on observables. We then use the estimated model to obtain a prediction of SRI adoption for B1 and B2 farmers. Let that prediction be denoted $\widehat{y}_i$. We then test whether the predicted adoption is better for referred farmers:

$$y_i = \lambda_0 + \lambda_1 \widehat{y}_i + \lambda_2 R_i + u_i \text{ for } i \in B1 \cup B2 \tag{3.9}$$

where R_i as before is 1 if i was referred and 0 otherwise. If $\lambda_2 > 0$ this indicates that referees have access to additional predictive information. If referral quality varies by treatment, we can expand the above regressions to include treatment dummies. In this case, the

[4] Or even to use referral at all, if it is more cumbersome to implement in the field.

estimated models become:

$$y_i = \theta_1 Z_i + \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{ for } i \in B1 \quad (3.10)$$

$$y_i = \lambda_0 + \lambda_1 \widehat{y}_i + R_i \sum_{k=2}^{3} \lambda_k T_{ki} + u_i \text{ for } i \in B1 \cup B2 \quad (3.11)$$

4. Implementation and data collection

The experiment was conducted in collaboration with BRAC, a large international NGO based in Bangladesh. The day-long SRI training follows the curriculum defined by BRAC and was administered by specially trained BRAC staff.[5] It included a multimedia presentation and a video demonstrating the principles of SRI in Bangladesh. At the end of the training, each farmer completed a test of their SRI knowledge.

Five districts were chosen for the experiment: Kishoreganj, Pabna, Lalmonirat, Gopalgonj and Shirajgonj. Within these districts, a total number of 182 villages were identified as suitable for SRI training by BRAC.[6] The 182 villages were then randomized into: 62 villages assigned to a control treatment without training; and 40 villages were assigned to each of the three treatments (T1, T2 and T3). In control villages, no one receives SRI training.

Within each of the 182 selected villages, BRAC conducted a listing exercise of all potential SRI adopters, defined as all farmers who cultivate rice and have a cultivate acreage of at least half an acre (50 decimals) and at most 10 acres.[7] From these lists we randomly drew approximately

[5] The trainers were recruited among BRAC agricultural field officers. They received a five-day training administered by experienced SRI researchers who have previously worked at the Bangladesh Rice Research institute (BRRI).

[6] Suitability in a village is determined according to the following criteria: SRI cultivation is feasible in the Boro season; and SRI is not already practiced in the village. In addition, attention is restricted to villages in which BRAC already operates, partly for logistical reasons, and partly to ensure that farmers are familiar with BRAC in order to minimize trust issues.

[7] In Bangladesh, more than 10 acres of land is regarded as too large a farm for our intervention. Farmers with less than 0.5 acre of land are excluded because they tend to be occasional or seasonal farmers.

30 farmers in each village. Table 1 summarizes the breakdown of the sample into the different treatments.

The first batch of B1 farmers is randomly selected from the list and invited for SRI training.[8] As explained earlier, the number of invited B1 farmers is randomly varied across villages to be between 5 and 15. At the end of training, each of the B1 farmers in treated villages (T1, T2 and T3) is asked to refer one farmer from those remaining in the pool, in the sequential way explained in the previous section. Each B1 farmer refers one and only one B2 farmer. Unselected farmers are left untreated. The total number of trainees by village varies between 10 and 30. We present in Figure 1 a histogram of the proportion of farmers available to be referred by each B1 farmer, expressed as a percentage of the village sample. Given that the average sample contains 30 farmers, we see that there is widespread variation in the size of the pool from which each B1 trainee can select a referral: clearly some B1 trainees are more constrained in their choices than others.

For both B1 and B2 farmers, an invitation to the training was delivered in writing by a BRAC staff at the farmer's residence. The training took place one week after the invitation was distributed. B2 farmers in treated villages were also told they were nominated for training by a fellow farmer. B2 farmers received training one week after B1 farmers. All trainees received BDT 300 for their participation in the training, which is slightly more than the agricultural daily wage. In addition, they were given lunch, refreshments and snacks for the day. They were also given a certificate from BRAC.

Referees in treatment T1 received no compensation in addition to their participation fee. In contrast, referees in treatment T2 received an additional fixed payment of BDT 300 while referees in treatment T3 received a payment of BDT 600, but only if the referred farmer sub-

[8] Selection was implemented using balanced stratified sampling with four cells: farmers aged below and above 45; and farm size below and above the median of 120 decimals (i.e., 1.2 acres).

sequently adopted SRI practices.[9] The rules of compensation were explained to referees before they selected someone from the pool. For both T2 and T3 farmers, compensation was paid a few weeks after training, at a time when the adoption of SRI practices could be verified in the field by BRAC staff.

Each participating farmer completed a baseline household survey covering demographics, income, and assets. Detailed agricultural production information was gathered on input use, crop output, production techniques, knowledge about cultivation methods, and attitudes towards the adoption of new agricultural techniques – such as SRI. We also performed three tests of cognitive ability – Raven's matrices, numeracy, and memory span – and we measured numerical reasoning using simple deduction and counting tests.

In addition, respondents were asked detailed information about their social ties to other farmers in the village sample: family ties (close relative, neighbor, friend, or other); and social ties (how often they discuss agriculture and finance-related matters, frequency of social visits, whether they regard the listed person to the best Boro farmer in their village). We also collected information on the physical distance between the home or land of each pair of farmers in the village sample. In addition, each respondent was asked to recommend up to five farmers who could potentially engage in SRI farming. This rich level of information was collected to measure social proximity R_{ij} for the estimation of regression model (3.7).

We also conducted an endline survey after the harvesting season to capture SRI adoption, as well as a short survey at transplanting to find out whether the respondent had applied any of the SRI recommendations on his field. Our measure of SRI adoption is constructed from these two data sources. Using visual assessments of BRAC trainers through field visits, a farmer is considered to have adopted SRI for the purpose of this paper if he followed at least three of the

[9] The level of compensation for T2 and T3 is intended to be the same in expected value, assuming a 50% SRI adoption rate.

six key principles of SRI on any of his plots.[10] We find similar results when we use alternative measures of adoption, or if we consider each of the six SRI principles separately.[11]

Balance on key demographic and socio-economic characteristics is illustrated in Table 2. In the first panel of the Table, we compare control and treatment villages. We find that none of the p-values is statistically significant, indicating that the randomized partition of villages into treatment and control was successful. Pairwise comparisons between the three treatment arms T1, T2 and T3 similarly confirms adequate balance: differences in household characteristics between treatments are small in magnitude and generally not significant, except for a slightly higher average education level in T2. We repeat this comparison for B1 trainees in the three treatment arms (Panel B of Table 2), and find no significant differences, as it should be since B1 trainees are selected at random. If we repeat the same exercise for B2 trainees, we find that referred farmers in treatment T1 are slightly older, and they have a slightly larger household size in treatment T3. This is the first indication that the treatments may have induced different types of selection. The differences are not large in magnitude, however.

As can be seen from Table 1, attrition between baseline and endline is around 10% in the sample at large, with some variation across treatments and controls. Attrition analysis is presented in Appendix Table 1. We estimate a probit model of overall attrition and attrition by treatment status controlling farmers' characteristics. We find little evidence that treatment differentially predicts attrition in our data.

[10] The six key principles consist of the following interdependent components: early transplanting of seedlings (20-days-old seedlings); shallow planting (1–2 cm) of one or two seedlings; transplanting in wider i spacing (25 x 20 cm); reduced use of synthetic chemical fertilizers; intermittent irrigation; and complementary weed and pest control. Regarding the spacing, age, and number of seedlings, practitioners recommend adoptin values adapted to the local context. This is the set of practices recommended by BRRI and BRAC for SRI in Bangladesh.

[11] The alternative measures of adoption of SRI are: (1) direct response from farmers if they have adopted SRI (self-assessed SRI adoption); (2) enumerator assessed SRI adoption (whether enumerator thinks that a farmer followed SRI principles on any plot of land); and (3) proportion of land on which SRI principles were applied.

5. Empirical analysis

5.1. Average treatment effect

We start by testing whether treatments T1, T2 and T3 have an effect on the adoption of SRI practices. Coefficient estimates for model (3.1) are reported in Table 3, without and with additional household controls. All participants are included in the regression, and treatment dummies refer to the status of each village. Results should thus be interpreted as intent-to-treat estimates since only a subset of farmers received the training. Results show that treatment triggered some adoption in all cases, relative to baseline adoption which was 0%. They are virtually identical when we include additional controls, providing reassurance that findings are not affected by imbalance that may have arisen on these variables. The ITT effect is large in magnitude: 28% for T1, and 34-35% for T2-T3.

In Table 4 we present similar estimation results for crop production, revenue, costs, and profits. Each dependent variable is measured at endline and is expressed per unit of land area. Following current practice, the baseline value of the dependent variable is included as additional regressor to capture possible persistence over time. The baseline level of the dependent variable is shown at the bottom of each regression. We find a large significant ITT effect of treatments on production, revenue, and profits per unit of land area. Except for treatment T2, the results also indicate a significant positive effect on input and labor costs – and hence on total costs. In all cases, the magnitude of ITT coefficients is large relative to baseline: production and revenue per area both increase by 17 to 19% while profit increases by 19 to 27%. Total production costs per area increase by 3 to 18%. Results are virtually identical if we include household controls.

From this evidence we conclude that training has a positive effect on adoption of SRI practices and on material crop outcomes. However, adoption falls far short of 100% even among those who receive training. To document this in a way that does not suffer from possible selection bias, we compare B1 trainees to control farmers using regression model (3.2). Results are presented in Table 5. We see that average adoption rates among B1 trainees varies between 37 and 49%, depending on treatment. This suggests that farmers differ in their interest in SRI – and hence their propensity to adopt it.

Before we test whether referral helps targeting, we need to verify that the knowledge imparted by the SRI training does not diffuse immediately to all farmers in the village. Testing this formally is the object of regression model (3.3), which compares untreated and B1 farmers to control farmers. Results are presented in Table 6. We note some spillover of training onto untrained farmers in treated villages: being in a treated village significantly increases SRI adoption for all three treatments. The magnitude of the spillover effect, however, remains well below that on B1 farmers, as evidence by the large magnitude and significance of the coefficient on being a B1 farmer in a treated village.

Taken together, Tables 5 and 6 indicate that efficiency could be improved by targeting training towards those most susceptible to benefit from it. This provides the necessary justification for seeking to improve targeting by incentivizing trainees to refer individuals who are more likely to adopt. To this we now turn.

5.2. Referral, selection, and targeting

We start by using regression model (3.4) to investigate whether referral brings to training farmers who are more likely to subsequently adopt SRI. Results are shown in Table 7. Contrary to expectations, we find that referred B2 farmers are not significantly more likely to adopt SRI than randomly selected B1 farmers: the interaction coefficient between referral and treatment is positive for all three treatments, but never statistically significant. To confirm this finding, we estimate regression model (3.5) and formally test whether SRI adoption by B2 farmers is statistically different across different incentivization treatments. Results, shown at the bottom of Table 7, are all non-significant. From this we conclude that incentivizing referees does not improve targeting on average.

The average may nonetheless hide differential targeting depending on how constrained referees are when selecting a trainee among those not already selected. We investigate this possibility by applying regression model (3.6) to B2 farmers. Results are presented in Table 8. To recall, C_i is the proportion of sample farmers from which the referee of B2 trainee i could have selected. It captures how unconstrained the referee is: the higher C_i, the less constrained was the referee of trainee i. Since less constrained referees are in a better position to identify a farmer who is more likely to benefit from treatment, we expect the coefficient of C_i to be positive in general, but particular in treatments T2 and T3 when referees are rewarded or incentivized. Results show that less constrained referees select better targeted trainees in T1, but not in the other two treatments: for treatment T2 the coefficient on C_i is even negative. In both cases, however, the coefficient is not significant. At first glance, this result does not agree with our initial expectations. However, if we plot the predicted adoption of B2 farmers relative to C_i for each of the three treatments (see Figure 2), we see that all three treatments yield the same level of predicted adoption when the referee is unconstrained, suggesting that incentivizing unconstrained referees does not, at least, reduce the quality of referral. Furthermore, while the quality of B2 trainees falls in T1 as C_i falls, this decrease in quality essentially disappears in T2 and T3. One possible explanation is that, when rewarded (T2) or incentivized (T3), constrained referees make more of an effort to identify a better target for training. In contrast, T1 referees identify good trainees when unconstrained, but the quality of the farmers they refer drops significantly as the choice set of possible referees shrinks. This is a priori consistent with T2 and T3 farmers making more of an effort to identify good targets for training.

Another way to look at the evidence is to examine whether, in the absence of reward or incentive, B1 farmers are more likely to recommend socially proximate individuals, especially when the set of farmers they can choose from is unrestricted. We investigate this issue by estimating dyadic regression model (3.7). The dependent variable x_{ij} is defined for each pair of farmers in the village sample. It takes value 1 if i refers j and 0 otherwise. Standard errors are clustered at the village level, which also takes care of network interdependence across observations (e.g., Fafchamps and Gubert 2007). The unconditional average of x_{ij} low since each B1 farmers only recommend one farmers out of the set of possible referees.

Different estimates are presented in Table 9, based on different possible definition of social proximity. The evidence clearly shows that, on average, B1 farmers tend to refer farmers to whom they are socially close, irrespective of how closeness is defined: the coefficient of the 'socially close' dummy is positive and significant in all cases except one – when social proximity only includes friends and neighbors. The effect is large in magnitude: in column 1, for instance, the probability that $x_{ij} = 1$ rises from 5.2% (the intercept term) to 6.3% for a socially proximate farmer – an increase of 21%. For other columns, the relative increase is even larger: $41 - 42\%$ for columns 2, 5, and 6, and 64% in column 4.

This pattern, however, is significantly weaker when B1 farmers are *less* constrained: the coefficient of the interaction coefficient between social proximity and C_i is negative and significant in all cases except one – when socially close individuals only include relatives. Given that C_i varies between 0.8 (least constrained) to 0 (most constrained), estimated coefficients imply that homophily is reversed when referees are least unconstrained. To illustrate, let us compare a highly constrained farmer to a less constrained farmer while keeping C_i within the range of plausible values shown in Figure 1: when $C_i = 0.1$, a B1 farmer is $0.011 - 0.022 \times 0.1 = 0.9\%$

more likely to refer an socially close individual – an increase of 17% over socially distant farmers. In contrast, when $C_i = 0.7$, the net effect becomes a negative -0.4%. Put differently, B1 farmers are less likely to refer a socially close farmer when they have more freedom to choose.

This results could suggest that respondents strive to refer non-socially proximate farmers when choices are less restricted. Does it follow that they make more of an effort when their are rewarded or incentivized? Table 9 dispels this notion: interacting selection with treatment yields coefficients that are small in magnitude and never significant. What may happen instead is that referees first aim to recommend someone who is widely known to be a good farmer. When this choice has already been taken, however, they pick someone whose name they recognize, and this tends to be a friend, neighbor or relative. This suggests that participants use friends and relatives as fallback when more appropriate trainees are no longer in the pool of selectable individuals.

Our last attempt at uncovering evidence of targeting relies on estimating predictive model (3.8) on B1 farmers, and testing with models (3.9) and (3.10) whether referral has an added predictive power over and above what can be predicted from characteristics observable to BRAC agents. Results for equation (3.8) are shown in Table 10. The low R^2 means that observable characteristics have little predictive power on SRI adoption – and hence that accessing information in the hands of other farmers may improve targeting. Results for (3.9) and (3.10) are presented in Table 11. The evidence presented in column 1 suggests that referred farmers are on average 3.7% more likely to adopt SRI, conditional on their predicted adoption rate if they were randomly selected. There is, however, no evidence that rewarding or incentivizing referees improves referral quality: when we interact being a B2 farmer with each of the three treatments, we find no evidence that T2 or T3 have a larger coefficient than T1 – if anything, point estimates are small. None of the coefficients is individually significant although, from column 1, we know that they are jointly significant.

Taken together, the evidence therefore suggests that referred B2 farmers are slightly more likely to adopt SRI after training than randomly selected B1 farmers. But rewarding or incentivizing farmers does not improve targeting. We do find some evidence that rewards and incentives induce referees to recommend more promising trainees when their choice set is most constrained. But there is nothing in the results to suggest that rewarded or incentivized referees are less likely to refer socially proximate individuals when constrained: they may select a trainee more carefully under constraint, but they are nevertheless equally likely to select someone from their social circle.

5.3. Peer effects

So far we have implicitly assumed that the treatments have no effect on the adoption behavior of B1 farmers. This assumption arises from the observation that since B1 referees are selected randomly from their village pool, they are not affected by selection effects. Treatments are randomly allocated across villages in a balanced way, so there is no reason to expect SRI to be more suitable to T2 and T3 farmers. Furthermore, B1 farmers receive no incentive to adopt other than the training, and the training is identical across treatment villages. Based on this, it is a priori reasonable not to expect any systematic variation in adoption by B1 trainees across treatments. Still, there may be.

To investigate this possibility, we estimate test whether adoption by B1 farmers varies with treatment:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{ for } i \in B1 \tag{5.1}$$

Results, presented in Table 12, show that adoption by B1 farmers in T2 and T3 is approximately 12% higher than in T1 villages. Why this is the case is unclear. One possibility is that providing financial compensation to referees heightens interest in the training – e.g., because of a salience effect, or because of reciprocity or experimenter demand considerations. Another possibility is that the referral process creates a symbolic link between referee and referred, and this link causes them to coordinate their adoption decisions. If so, we would expect the link to be stronger when referees receive financial compensation. Indeed referred farmers may point out to their referee that they should 'put their money where their mouth is'. To understand why, put yourself in the shoes of the referred farmer. Another farmer was paid to recommend someone for training and chose to recommend me. If that farmer thought that the training was a waste of time, it would have been unkind of him to recommend me. I therefore expect the referee to demonstrate interest in the technology by adopting it himself. Not doing so would demonstrate a lack of care about the value of my time, and a mercenary attitude to friendship. This is particularly true in T3 when the financial compensation received by the referee depends on my adoption. If this were the reasoning following by referred farmers, we would expect correlation in adoption decisions between referred and referee: the fact that my referee adopts convinces me that he thinks the training was beneficial, and hence that I too should adopt.

To investigate this possibility, we test whether the adoption behavior of referee and referred is more similar than for other farmer pairs in the same village. We do this in two ways. The simplest model is of the following form:

$$|y_i - y_j| = \alpha_0 + \alpha_1 m_{ij} + m_{ij} \sum_{k=2}^{3} \alpha_k T_{ki} + u_i \text{ for all } ij \text{ pairs in } B1 \cup B2 \tag{5.2}$$

where $m_{ij} = 1$ if i referred j or vice-versa. A negative α_2 or α_3 means that adoption decisions are more similar – i.e., less different – for referee-referred pairs. Estimation results are presented in Table 13. We find α_3 is significantly lower than 0, with our without village fixed effects. This implies that T3 induces adoption decisions of referred and referee to be more similar than those of other farmer pairs – indicating coordination in adoption. No such effect is found for treatment T2.

One possibility we wish to eliminate is that correlation is due to homophily: B1 farmers recommend someone sharing similar characteristics and thus a similar propensity to adopt, and this drives the correlation in their decisions. To circumvent this possibility, we look at the correlation in behavior that cannot be predicted from similarity in characteristics. Formally, let $\widehat{y}_i$ as before be the predicted adoption from regression (3.10) estimated from B1 farmers, and define $\widehat{u}_i = y_i - \widehat{y}_i$ for $i \in B1 \cup B2$. In other words, $\widehat{u}_i$ captures the variation in $\widehat{y}_i$ that cannot be predicted from i's characteristics. If referee and referral coordinate their adoption over and above the coordination that naturally arises from shared characteristics, then $\widehat{u}_i$ should be more correlated with $\widehat{u}_j$ when i referred j than for any other farmer pair. This yields the following test:

$$\widehat{u}_i = \widehat{u}_j \sum_{k=1}^{3} \tau_k T_{ki} + \widehat{u}_j m_{ij} \sum_{k=1}^{3} \varphi_k T_{ki} + \varepsilon_{ij} \text{ for all } ij \text{ pairs in } B1 \cup B2 \tag{5.3}$$

We expect $\tau_k = 0$ since the predicting equation (3.10) includes treatment dummies. If there is coordination in adoption for treatment k, then $\varphi_k > 0$. Results, presented in Table 14, confirm the findings from Table 13: $\varphi_3 > 0$, indicating that, in treatment T3, there is significantly more correlation in adoption choices of referee and referred than would arise from random pairing. We cannot reject that $\varphi_3 = \varphi_2$ – a coordination effect may also be present in treatment T2, but the point estimate is not statistically significant for T2.

If T2 and T3 increase adoption not because of better selection but because of peer effects, this raises the concern that increased adoption is achieved by attracting farmers less able to benefit from the SRI technology. If this were the case, SRI should be less productive and less profitable for the 12% additional farmers who adopt SRI in those treatments. If training increases the performance of additional T2 and T3 farmers less than that of T1 farmers, ITT estimates would increase less than proportionally with increased adoption. If ITT estimates fall with increased adoption, this implies that additional adopters induced by T2 and T3 on average lose from adoption. From Table 3 we know that T2 and T3 increase adoption rates relative to T1. Table 4 reported ITT effects on various economic outcomes.

We find that T3 uniformly reduces ITT estimates for all performance categories, suggesting that additional adopters *lose* from adoption: they have lower yields, lower revenues, and lower profits. They also incur lower costs per area than T1 farmers, suggesting improper SRI adoption – to recall, SRI requires more inputs, especially in management and labor. In contrast, additional adopters under T2 appear to gain disproportionately more than T1 adopters, while using much fewer inputs. Why this is the case is unclear. These findings nonetheless suggest caution when interpreting increased adoption as a beneficial outcome of treatment: we cannot rule out that the peer effects triggered by the incentivized referee treatment induced adoption by inframarginal farmers whose performance decreases as a result of adoption.

6. Conclusion

Many policy interventions provide vocational training that is expected to benefit only a subset of the target population. Implementation agencies are often unable to identify all potential beneficiaries, and self-selection into treatment is ineffective if members of the target population are unable to assess beforehand whether they would benefit from the training – i.e., they do not know what they do not know. As a result, vocational training is poorly targeted and financial incentives are often required to encourage potential beneficiaries to attend.

In such a context, asking past trainees to recommend potential candidates for training could potentially improve matters: after receiving the training, past trainees are better able to assess its usefulness, not only for themselves but also for others like them. Hence they may be able to identify individuals who would benefit more from the training – possibly with a suitable reward or incentive.

We investigated this possibility using a randomized controlled trial in Bangladesh. Vocational training on SRI is offered to rice farmers. The first batch of trainees is presented with a list of farmers from the same village, and asked to recommend someone for subsequent training. Treated referees received either an unconditional reward for recommending someone from a list of potential candidates; others received a reward conditional on adoption by the referred person. Controls did not receive any financial incentives.

Results indicate that training significantly raises the likelihood of SRI adoption, with some spillover to untrained farmers in treated villages. Results also indicate that treated villages have higher yields, revenues, and profits per area, as well as higher input costs. But only 40-50% of trainees adopt SRI and many adopters do not follow all recommended practices, suggesting that training is probably not targeted towards farmers most likely to benefit from it.

Does referral improve targeting? We find that referred farmers are on average 3.7% more likely to adopt SRI, conditional on their predicted adoption rate if they were randomly selected. But there is no evidence that rewarding or incentivizing referees improves referral quality. We nonetheless find faint evidence that rewarded and incentivized trainees make more of an effort to identify good targets for training when their choice of potential beneficiary is more constrained. The results also suggest that participants use friends and relatives as fallback when more appropriate trainees are no longer in the pool of selectable individuals. In the aggregate, the targeting gain from using referral is not statistically significant.

We also find that, when referees are rewarded or incentivized, average adoption increases by 12 percentage points, for both referees and referred farmers. Why this is the case is not entirely clear, although it may consistent with a demonstration effect: by offering financial incentives, the training agency may have convinced more farmers of the relevance of the training. We also find that, when referees receive a payment conditional on adoption by the farmer they referred, they are more likely to coordinate their adoption behavior with that farmer. However, the data also suggests that, while an increase in adoption rate is achieved when referees are rewarded or incentivized, this increase does not translate into increased performance for all. Simple calculations indeed suggest that the additional adopters generated by referee incentivization experience a fall in performance. Incentivizing referees appears to have triggered a feedback mechanism that encouraged inframarginal farmers to adopt a technology for which they were ill-suited – i.e., it reduced targeting efficiency. While it is unclear to what extent our findings would generalize to other settings, they are nonetheless sufficiently troubling to suggest caution when introducing trainee referral for targeting purposes, especially with financial compensation.

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7. Appendix A: SRI

The System of Rice Intensification (SRI) was developed in Madagascar in the 1980s for smallholder farmers like those in Bangladesh (Moser and Barrett, 2006). SRI involves changing a range of rice management practices in which the management of soil, water, plant and nutrients is altered in order to achieve greater root growth and to nurture microbial diversity resulting in healthier soil and plant conditions (Karmakar et al. 2004). The SRI practices enhance the rice plants' growing conditions by reducing the recovery time seedlings need after transplanting; reducing crowding and competition; promoting greater root development; and optimizing soil and water conditions. Specifically, it involves transplanting single young seedlings with wider spacing, carefully and quickly into fields that are not kept continuously flooded, and whose soil has more organic matter and is actively aerated. It requires neither new seed varieties nor additional external inputs. SRI is, however, a knowledge-intensive technique and requires significant labor for field preparation, water management, weeding, and harvesting, and is to be adapted in local context. It has demonstrated dramatic potential for increasing rice yields without requiring additional purchased inputs (seed, fertilizer, etc.), nor increased irrigation.

A number of non-experimental studies indicate that SRI is associated with significantly higher yields (30–80%) and increased profits. Takahashi and Barrett (2014) show that the SRI generates average yield gains of around 64% relative to conventional methods in a study of Indonesian farmers. Sinha and Talati (2007) find average yield increases of 32% among farmers who partially adopt SRI in West Bengal. Styger et al. (2011) show 66% increases in yields among plots farmed using the SRI relative to experimentally controlled plots using farming methods similar to those of local rice farmers in Mali. Barrett et al. (2004) find SRI yields to be 84% higher than those produced by alternative strategies practiced by farmers in Madagascar. A pilot project conducted in Bihar, India—the state with the lowest agricultural productivity and highest share of marginal farmers in India, which is very similar to Bangladesh in many respects—has recorded 86% increases in rice productivity resulting from SRI adoption. Another pilot project conducted by the BRAC in Bangladesh (see Islam et al., 2012) shows higher yields of around 50% among those who adopt SRI.

In spite of this, SRI diffusion has been sluggish, and uptake rates have been low in many of the areas where it has been introduced (Moser and Barrett, 2006). Given its purported productivity and earnings potential, the low uptake of SRI technology, even in countries with surplus and unemployed family labor, is puzzling. The primary impediments to adoption appear to involve the effort needed to learn the principles and practices required for this knowledge-intensive method and the possible social constraints to adopting visibly different rice production and water management methods within ostensibly homogenous production communities (Moser and Barrett 2006), or what we now term 'homophily' (Banerjee et al., 2013).

There is evidence that farmers are constrained by the information and skills necessary for local adaptation. Yield risk appears to be greater under SRI than under traditional cultivation methods (Barrett et al., 2004); thus, farmers must be willing and able to absorb increased output risk. Finally, in the absence of inter-household uptake coordination, adopting visibly different rice production and water management methods within ostensibly homogenous production communities may produce social stigma effects (Moser and Barrett, 2006). Because SRI fields differ visibly from traditional rice fields, social norms and conformity pressures may discourage adaptation and the ultimate decision to adopt. In the rural Bangladeshi context of resource constraints on extension and adaptive research facilities and limited access to formal finance sources, social (i.e., village, kinship, or friendship) networks may offer a viable alternative.

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Table 1: Sample Breakdown

	Villages	Farmers at baseline	B1	B2	Untreated	Farmers at endline
Control	62	1856	0	0	0	1663
Treatment T1	40	1192	407	342	443	1036
Treatment T2	40	1216	394	351	471	1124
Treatment T3	40	1222	384	348	490	1111
Total	182	5486	1185	1041	1404	4934

Table 2: Balancedness

Panel A: All farmers	Control	All Treatment farmers				p-value			
		Overall	T1	T2	T3	(1)	(2)	(3)	(4)
Average age of the household (above 15 years)	36.4	36.8	37.0	36.8	36.6	0.14	0.47	0.19	0.53
Average education of the household	4.3	4.3	4.2	4.5	4.2	0.67	**0.01**	0.90	**0.01**
Cultivable farm area in last Boro season (decimals)	165.9	163.5	160.7	163.6	166.6	0.57	0.68	0.39	0.60
Household size	5.2	5.1	5.1	5.1	5.1	0.25	0.69	0.68	1.00
Maximum education of any household member	8.7	8.5	8.5	8.7	8.4	0.14	0.10	0.90	**0.07**
Working age members in the household	3.2	3.1	3.2	3.1	3.1	0.58	0.61	0.28	0.55
No. of observations	1856	3630	1192	1216	1222				

Panel B: B1 farmers	B1 trainees			p-value		
	T1	T2	T3	(2)	(3)	(4)
Average Age of the household (above 15 years)	36.9	36.9	36.5	0.97	0.43	0.43
Average Education of the household	4.3	4.4	4.3	0.54	0.98	0.56
Cultivable farm area in last Boro season (decimals)	149.5	154.5	163.2	0.57	0.14	0.32
Household size	5.1	5.2	5.1	0.54	0.88	0.64
Maximum education by any household member	8.6	8.7	8.6	0.68	0.79	0.89
Working age members in the household	3.2	3.2	3.1	0.81	0.32	0.2
No. of observations	407	394	384			

Notes: Reported p-values are for a two-tailed test of the null hypothesis that group means are equal. Column 1 compares controls to all treatment farmers; column 2 compares T1 and T2 farmers; column 3 compares T1 and T3 farmers; column 4 compares T2 and T3 farmers.

Table 3: ITT effect on SRI-adoption in the village

Dependent variable	SRI-adoption (1=yes, 0=No)	
	(1)	(2)
Treatment T1	0.280***	0.285***
	(0.034)	(0.034)
Treatment T2	0.350***	0.353***
	(0.035)	(0.034)
Treatment T3	0.336***	0.336***
	(0.038)	(0.038)
Controls	No	Yes
Observations	4,934	4,934
R-squared	0.142	0.147

Notes: Estimator is linear probability model. Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. All sampled farmers are included. Baseline adoption in control villages is 0. Controls include: age dummy if household head is above 45 years of age; education dummy if household head has primary schooling; farm size dummy if cultivable area is above median land size of 120 decimals.

Table 4: ITT effect on agricultural performance by area

Variables of Interest	Yield in Kg	Revenue per area	Input cost per area	Labor cost per area	Total cost per area	Estimated profit per area
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment T1	3.824***	136.684***	18.395***	31.517***	50.777***	83.769***
	(0.694)	(23.042)	(4.912)	(11.922)	(16.161)	(23.64)
Treatment T2	4.248***	135.369***	3.606	7.846	9.294	119.598***
	(0.647)	(19.591)	(5.134)	(10.875)	(15.594)	(21.17)
Treatment T3	3.718***	123.157***	15.307***	23.430*	36.639**	82.835***
	(0.681)	(23.91)	(5.274)	(12.052)	(16.902)	(23.416)
Baseline value	0.230***	0.189***	0.019***	0.212***	0.037**	0.015
	(0.025)	(0.022)	(0.007)	(0.025)	(0.016)	(0.016)
R-squared	0.12	0.117	0.037	0.093	0.035	0.043
Baseline level in control villages	**22.51**	**733.14**	**147.87**	**140.09**	**287.97**	**445.17**

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. All dependent variables are measured at endline and expressed in quantity or value per decimal. Input costs include seed, urea, pesticide, etc. Labor costs include both hired and contractual labour. Total cost combines input and labor costs. All values are in BDT. All outcome variables are expressed in per decimal terms. All sampled farmers are included. Number of observation is 4763. Similar results are obtained if we add the same controls as in Table 3.

Table 5: Training effects on SRI-adoption by B1 trainees

Dependent variable	SRI-adoption (1=yes, 0=No)
Treatment T1	0.371***
	(0.048)
Treatment T2	0.489***
	(0.044)
Treatment T3	0.494***
	(0.057)
Observations	2,741
R-squared	0.342

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Only B1 trainees are included. Similar results are obtained if we add the same controls as in Table 3.

Table 6: Training effects on SRI-adoption by B1 trainees and non-trainees

Dependent variable	SRI'(adoption (1=yes, 0=No)
Village Treatment T1	0.077***
	(0.014)
Village Treatment T2	0.080***
	(0.018)
Village Treatment T3	0.067***
	(0.013)
Village Treatment T1 x trainee	0.293***
	(0.042)
Village Treatment T2 x trainee	0.409***
	(0.036)
Village Treatment T3 x trainee	0.427***
	(0.054)
Observations	3,975
R-squared	0.295

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Includes observations on B1 trainees and non'(trainees. B2 trainees are excluded. Similar results are obtained if we add the same controls as in Table 3.

Table 7: Training effects on SRI-adoption by B1 and B2 trainees

Dependent variable	SRI-adoption (1=yes, 0=No)
Village Treatment T1	0.371***
	(0.048)
Village Treatment T2	0.489***
	(0.044)
Village Treatment T3	0.494***
	(0.057)
Village Treatment T1 x B2 trainee	0.058
	(0.039)
Village Treatment T2 x B2 trainee	0.041
	(0.035)
Village Treatment T3 x B2 trainee	0.029
	(0.032)
Observations	2,047
R-squared	0.479

Notes: Standard errors in parentheses are clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Only B1 and B2 trainees are included. Similar results are obtained if we add the same controls as in Table 3.

P-values for pairwise comparisons of B2 trainees are as follows:
T1=T2 p=0.74;
T1=T3 p=0.57;
T2=T3 p=0.80.

Table 8: Training effects on SRI-adoption by B2 trainees

Dependent variable	SRI-adoption (1=yes, 0=No)
Treatment T1	0.261**
	(0.107)
Treatment T2	0.648***
	(0.113)
Treatment T3	0.451***
	(0.168)
Treatment T1 x Ci	0.381*
	(0.225)
Treatment T2 x Ci	-0.237
	(0.215)
Treatment T3 x Ci	0.122
	(0.294)
Observations	885
R-squared	0.501

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Only B2 trainees are included. Similar results are obtained if we add the same controls as in Table 3.

Table 9: Dyadic regression on referral and social proximity

Dependent variable	Whether B2 trainee as referred by B1 trainee					
	(1)	(2)	(3)	(4)	(5)	(6)
Socially close	0.011**	0.018**	-0.008	0.030*	0.019***	0.018**
	(0.005)	(0.008)	(0.006)	(0.016)	(0.005)	(0.008)
Socially close x Ci	-0.022***	-0.022***	-0.025***	0.022	-0.008	-0.023***
	(0.004)	(0.004)	(0.005)	(0.035)	(0.01)	(0.003)
Socially close x Ci x T2	-0.001	-0.002	-0.001	-0.042	-0.006	-0.001
	(0.003)	(0.003)	(0.004)	(0.028)	(0.008)	(0.002)
Socially close x Ci x T3	-0.002	0.00	0.00	-0.032	-0.012*	-0.001
	(0.003)	(0.002)	(0.004)	(0.026)	(0.007)	(0.002)
Constant	0.052***	0.044***	0.068***	0.047***	0.045***	0.044***
	(0.005)	(0.008)	(0.004)	(0.001)	(0.002)	(0.008)
Observations	21,063	21,063	21,063	21,063	21,063	21,063
R-squared	0.0003	0.0003	0.001	0.002	0.001	0.0003

Notes: Observations include all possible pairs of B1 and B2 trainees in a village. The dependent variable is 1 if the B2 trainee was referred by the B1 trainee. Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Similar results are obtained if we add the same controls as in Table 3. The meaning of socially close varies across columns: (1) neighbor, friend or has neighboring land; (2) neighbor, friend or close relative; (3) neighbor or friend; (4) close relative; (5) makes social visits each month; (6) discusses agricultural or financial matters with referee.

Table 10: Predicting adoption of B1 trainees

Dependent variable	SRI-adoption (1=yes, 0=No)	
	(1)	(2)
Age dummy	-0.023	-0.018
	(0.03)	(0.03)
Education dummy	0.064*	0.064*
	(0.035)	(0.034)
Farm size dummy	0.104***	0.100***
	(0.033)	(0.033)
Treatment T2		0.114*
		(0.064)
Treatment T3		0.118
		(0.073)
Constant	0.380***	0.302***
	(0.041)	(0.057)
Observations	1,078	1,078
R-squared	0.017	0.029

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Age dummy=1 if household head is above 45 years of age. Education dummy=1 if household head has primary schooling. Farm size dummy=1 if cultivable area is above median land size of 120 decimals. Only B1 trainees are included.

Table 11: Training effects on SRI-adoption by B1 and B2 trainees

Dependent variable	SRI-adoption (1=yes, 0=No)	
	(1)	(2)
Predicted SRI adoption from Table 10	0.897***	0.920***
	(0.216)	(0.207)
B2 trainee	0.037*	
	(0.02)	
B2 trainee x Treatment T1		0.045
		(0.044)
B2 trainee x Treatment T2		0.042
		(0.043)
B2 trainee x Treatment T3		0.022
		(0.046)
Constant	0.047	0.036
	(0.097)	(0.094)
Observations	2,047	2,047
R-squared	0.016	0.025

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Observations include B1 and B2 trainees.

Table 12: ITT effect on SRI-adoption of B1 trainees.

Dependent variable	SRI-adoption (1=yes, 0=No)
Treatment T2	0.118*
	(0.065)
Treatment T3	0.123
	(0.075)
Constant	0.371***
	(0.048)
Observations	1,078
R-squared	0.013

Notes: Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Observations include only B1 trainees.

Table 13: Dyadic regression on coordination between referee and referral

Dependent variable	Dummy=1 if adoption by referee and referral differ	
Referred dummy	0.028	0.023
	(0.031)	(0.022)
Referred dummy x T2	0.005	-0.039
	(0.053)	(0.038)
Referred dummy x T3	**-0.112****	**-0.059****
	(0.044)	**(0.029)**
Constant	0.355***	0.497***
	(0.018)	(0.002)
Village fixed effects	No	Yes
Observations	8,546	8,546
R-squared	0.001	0.13

Notes: Observations include all possible ij pairs of B1 and B2 trainees in a village. The dependent variable is 1 if the adoption decision of the B2 trainee j differs from the adoption decision of the B1 trainee i (i.e., one adopts and the other does not). The referred dummy =1 if the B2 trainee j was referred by the B1 trainee i. Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Dyadic regression of correlation in residuals between referee and referral

Dependent variable	Ui
Treatment T1 x Uj	0.008
	(0.01)
Treatment T2 x Uj	-0.006
	(0.006)
Treatment T3 x Uj	-0.010**
	(0.005)
Treatment T1 x Uj x Referred dummy	0.009
	(0.04)
Treatment T2 x Uj x Referred dummy	0.053
	(0.06)
Treatment T3 x Uj x Referred dummy	0.081*
	(0.043)
Village Fixed Effects	Yes
Observations	8,546
R-squared	0.347

Notes: Observations include all possible ij pairs of B1 and B2 trainees in a village. Ui and Uj are estimated residual from a predictive regression similar to that presented in Table 10 that includes B1 and B2 trainees. Regressors include: Age dummy=1 if household head is above 45 years of age; Education dummy=1 if household head has primary schooling; Farm size dummy=1 if cultivable area is above median land size of 120 decimals; and Treatment dummies T1, T2 and T3. As in Table 13, the referred dummy =1 if the B2 trainee j was referred by the B1 trainee i. Standard errors in parentheses are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A1. Attrition by Treatment Status: Balancedness

Panel A: All farmers	Control	All Treatment farmers				p-value			
		Overall	T1	T2	T3	(1)	(2)	(3)	(4)
Average age of the household (above 15 years)	37.01	37.1	37.59	36.21	37.14	0.9	0.18	0.65	0.42
Average education of the household	4.36	4.55	4.7	4.31	4.56	0.41	0.28	0.66	0.48
Cultivable farm area in last Boro season (decimals)	141.52	141.03	150.4	126.42	139.95	0.97	0.22	0.59	0.45
Household size	4.68	4.93	4.72	4.98	5.2	0.16	0.31	**0.08**	0.48
Maximum education of any household member	8.56	8.78	8.99	8.24	8.92	0.53	0.15	0.87	0.2
Working age members in the household	2.91	3.04	2.91	3.17	3.13	0.27	0.12	0.23	0.83
No. of observations	193	359	156	92	111				

Notes: Reported p-values are for a two-tailed test of the null hypothesis that group means are equal. Column 1 compares controls to all treatment farmers; column 2 compares T1 and T2 farmers; column 3 compares T1 and T3 farmers; column 4 compares T2 and T3 farmers.

Figure 1: proportion of farmers available to be referred by each B1 farmer
(% of the village sample)

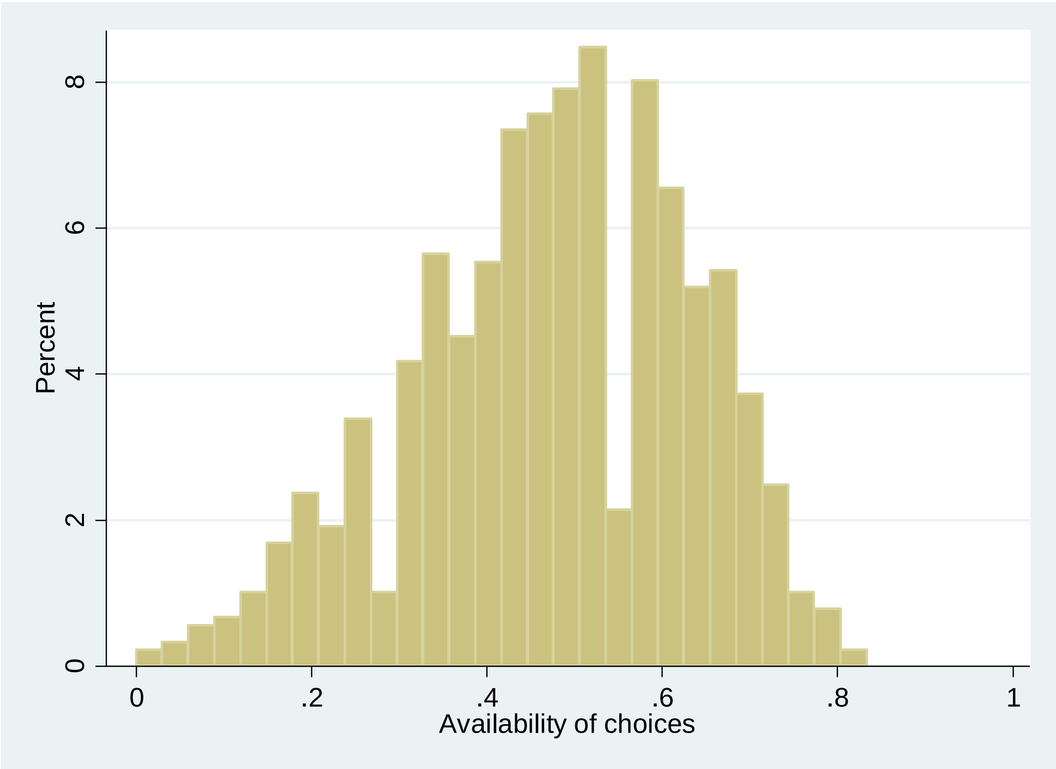

Figure 2. Targeting and incentives