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Can referral improve targeting? Evidence from an agricultural training experiment☆

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ABSTRACT

We seek to better target agricultural training by inviting current trainees to refer future trainees. Some referees are rewarded or incentivized. Training increases the adoption of recommended practices and improves performance, but not all trainees adopt. Referred trainees are 4.2% more likely to adopt than randomly selected trainees, and 3.4-3.8% more likely than what can be predicted from observed characteristics of trainees. This implies that referral provides a slight improvement in targeting. Rewarding or incentivizing referees does not improve referral quality, however. When referees receive financial compensation, referees and referred farmers are more likely to coordinate their adoption behavior. Incentivized referees are more likely to adopt, to incur losses from adoption, and to abandon the new practices in the following year.

1. Introduction

Agricultural productivity in LDCs remains low due to limited adoption of innovation. A number of studies have documented the role of social networks in technology diffusion (e.g., Bandiera and Rasul, 2006; Conley and Udry, 2010). Yet – with a few exceptions such as Beaman et al. (2018a,b) on targeting – little rigorous work exists in LDCs on how to mobilize local social networks for agricultural innovation.

One particular issue that has received insufficient attention is how to target extension training towards individuals most likely to benefit from it. This is especially true in LDCs where farm sizes are often small and little information is publicly available on the ability of individual farmers and their interest in new agricultural practices. As a result, efforts to introduce innovations to small farmers are often mistargeted, leading to limited adoption, wasted resources, and a loss of credibility for extension services. This raises the question of whether local knowledge can be leveraged to better target agricultural extension services.

In this study, we conduct a large-scale randomized controlled trial (RCT) to test whether local farmers are able to identify potential beneficiaries of a new rice cultivation technique called ‘System of Rice Intensification’. 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 not well suited for all farmers because it requires superior farming management skills (Moser and Barrett, 2006). BRAC – a large NGO with operations in Bangladesh and other LDCs – offers SRI training in villages. As we will see, a significant minority of trainees subsequently adopts some of the principles of SRI, but the majority does not. This makes it suitable to investigate whether referral can help target

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SRI training towards farmers capable of adopting it.

The experiment is undertaken in partnership with BRAC, ensuring the external validity of our results to this particular form of agricultural extension. By asking SRI trainees to recommend other farmers for the BRAC training, we examine whether within-village referral can result in better targeted extension, higher adoption, and higher agricultural performance. Our measure of targeting quality is the extent to which referred trainees subsequently adopts some aspects of SRI. Treated villages are randomly assigned to one of three treatments. In the first, referrals are not compensated; in the second, referees receive a fixed payment; and in the third, the referee's compensation depends on whether the referred trainee adopts SRI. No training is offered in control villages. We also vary the size of the pool from which referees can recommend farmers for training. These different sources of experimental variation serve to cast light on the motives pursued by referees when they recommend someone.

The main findings are as follows. First, training is effective in inducing the adoption of at least some SRI principles, and adopters on average have better farm outcomes. Second, referred farmers are more likely to adopt SRI. Furthermore, referral produces a slight increase in expected adoption relative to what can be predicted using characteristics of trainees that are observable by the training agency. Third, when trainees select from a more restricted pool of potential subjects, they are more likely to refer socially close people. Incentivized referees, however, tend to pick a *less bad* match than they would if unincentivized. Fourth, rewarding referees does not improve the relative adoption rate of referred farmers compared to the trainees who refer them. But it leads to higher adoption rates for both referees and referred farmers, with evidence of coordinated adoption between referee-referred pairs. However, additional adopters in villages with rewarded referees derive little or no benefit in terms of agricultural outcomes – consistent with over-adoption. Overall, we find that SRI training does induce adoption and raises agricultural performance among adopters; referral yields a slight improvement in targeting compared to the selection method used by BRAC; and incentivizing or rewarding referees leads to over-adoption.

The paper makes contributions to several literatures. First, we contribute to the literature on referral. Since Montgomery (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 as evidence of better match quality for referred workers (see also Castilla, 2005). Others have argued that referral enhances efficiency by increasing effort and productivity through employee monitoring (e.g., Kugler, 2003; Bandiera et al., 2005; Heath, 2018).

There have been some dissenting voices, however. Using observational data, Fafchamps and Moradi (2015) find that Ghanaian army recruits hired through referral have lower unobserved quality. In a lab experiment conducted by Beaman and Magruder (2012), subjects who performed an incentivized productivity task on day one were invited to refer a friend for the same task on day two. The authors find that referred day-two subjects are, on average, less productive than the day-one subjects who referred them. This difference is only partially eliminated when referees are incentivized to refer someone productive, thereby casting suspicion on the wisdom of relying blindly on worker referral to identify high productivity workers. Our findings go in the same general direction – we also find a small improvement in targeting as a result of referral. But, we also find that incentivizing referral induces over-adoption, which is reversed in the subsequent year.

Our findings also contribute to the literature on the diffusion of information in local communities, 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, 2010; 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. (2018a,b) 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. (2019) 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 that incentivization can potentially play in circulating information locally.

In contrast, our results suggest that, when incentivized or rewarded, 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, 1943, Topa, 2001; Oster and Thornton, 2012; Fafchamps and Quinn, 2018; BenYishay and Mobarak, 2019). 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 et al., 2005; Heath, 2018). We find that, when incentivized or rewarded, 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 what he recommended to a friend whose adoption will benefit him. Put differently, it is as if paying 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; it is intensive in labor; and it requires detailed knowledge and strong organizational skills.[2] Since SRI is not suited to all farmers, targeting its training towards suitable farmers should improve its cost-effectiveness. Unfortunately external agencies – such as BRAC, the

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

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

provider of SRI training in our case – seldom have enough information to target farmers effectively, and 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 (B1) are selected randomly. At the end of their training – when they have a better understanding of SRI requirements – we ask each B1 farmer to nominate one other farmer for the second batch of training (B2).[3] The selection process is presented in detail in the following section. 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 three 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. Provided that the other conditions are satisfied, the first condition can be tested by comparing adoption rates between farmers referred for training and farmers 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 paid to make the effort, adoption rates among referred trainees should be higher under T2 than under T1. This behavior can be understood as a form of reciprocity or conditional cooperation: the experimenter give something, and the subject responds by giving something in return – in this case, an effort to identify suitable trainees.

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.

The reason for having the three treatments T1, T2 and T3 is to identify three types of motivations for helping BRAC better target its training. People who are intrinsically motivated or unconditional cooperators will make good referrals even in T1 since, for them, cooperation is its own reward. People who are conditional cooperators will make good referrrals in T2 to reciprocate for the fixed payment they receive for their participation. Finally, people who are self-interested and rational (i.e., as in game theory) will not exert effort in either T1 or T2 because their compensation does not depend on effort. For these individuals, effort needs to be incentivized explicitly, as in T3.[4] Hence intrinsically motivated people provide effort even in T1; conditional cooperators provide effort in T2 to reciprocate towards the experimenter for receiving a fixed payment; and rational self-interested individuals free ride in T1 and T2 and only provide effort in T3.

It is also possible that referees resent farmers more successful than themselves. In this case, we expect B1 trainees not to refer farmers that are more successful than them. 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. Rewarding 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 the payment for referral fully compensates for rivalry, then adoption rates should be higher in T2 and/or T3 than among randomly assigned trainees. It is also conceivable that compensation is not necessary or even useful – e.g., paying referees may blunt intrinsic incentives to refer someone suitable (e.g., Gneezy and Rustichini, 2000a; 2000b; Gneezy et al., 2011).

As described so far, our experimental design bears some resemblance with the work referral experiment of Beaman and Magruder (2012). 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 that they can refer.[5] In practice this is achieved by first identifying in each study village a pool of 30–35 or so potential trainees.[6] 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

[3] Both B1 and B2 farmers are invited in person through a home visit by a field staff appointed by BRAC.

[4] Economic theory typically assume that people fall in the third category, i.e., they only respond to incentives, that is, to future rewards and punishments conditioned on their actions. Lab subjects often contradicts this assumption, however, sometimes cooperating without being incentivized, or failing to respond to incentives. The conceptual categories of 'intrinsically motivated' and 'conditional cooperator' have been introduced to account for some of these observed behaviors.

[5] In a recent paper, Beaman et al. (2018a,b) restricted the pool of people who could be referred. They find that when choice is restricted, the quality of referrals improves relative to when choice is unrestricted.

[6] The selection of farmers and villages are explained in section 4.

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. 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.[7] 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.[8] 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.[9] From these lists we randomly drew approximately 30–35 farmers in each village.[10] Table 1 summarizes the breakdown of the sample into the different treatments. Farmers are then invited for SRI training according to the protocol detailed below. The Table shows that the level of participation by farmers is the same across all treatments. Participation rates by both B1 and B2 farmers do not differ significantly across T1, T2 and T3. All the training takes place at approximately the same time, before the rice season has begun. This means that B1 farmers have not had an opportunity to experiment with SRI in their field before nominating another farmer. Referral is based purely on what B1 farmers have learned about SRI during training. This approach may lower referral quality because referees do not have full knowledge of what SRI adoption would entail. It nonetheless offers several advantages. First, it matches the field conditions under which farmer training takes place: BRAC delivers extension services as part of a training campaign targeting a few villages at a time. It is logistically cheaper for them to deliver all the training in a village at approximately the same time. Secondly, since most B1 trainees do not adopt, asking them to refer after a year would mean that some referees have adopted and learned more about SRI, while others have not and presumably forgotten their training. This would create a strong selection bias in referral quality. Our design obviates these problems.

The first batch of B1 farmers is randomly selected from the list and invited for SRI training.[11] 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.[12] Unselected farmers are left untreated. The total number of trainees by village varies between 10 and 30. We present in Fig. 1 a smoothed distribution of the proportion of farmers available to be referred by each B1 farmer, expressed as a percentage of the village sample. Given that the sample contains 30–35 farmers in each village, 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 face a more constrained choice set than others.[13]

B1 and B2 farmers are both invited in writing for training by a BRAC staff member who visits them in person at their home. They are told that the training will introduce them to a new and improved rice cultivation method. B1 farmers are told they are selected by lottery. B2 farmers are told that they were selected by another farmer who had received the training, and who recommended them. Otherwise the BRAC invitation protocol to B1 and B2 farmers is identical across treatment arms. B1 farmers are not informed *ex ante* that they will be asked to nominate another farmer, or that they will (or will not) be compensated for doing so.

The training takes place one week after the invitation is distributed. B2 farmers receive training one week after B1 farmers. All trainees receive BDT 300 for their participation in the training, which is slightly more than the agricultural daily wage. In addition, they are given lunch, refreshments and snacks for the day. They are also given a training certificate from BRAC.

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

[7] 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).

[8] These districts are spread all over the country. 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.

[9] 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.

[10] The actual number of famers per village varies between 29 and 36, with an average of 31. Most villages have 30 farmers. We conduct a census of all farmers in each village and identify those who cultivate rice on owned or leased land during the Boro season. Experimental subjects are selected randomly from the list of those who meet this criterion. In large villages with many eligible farmers, we identify geographically distinct neighborhoods and regard these as a village for the purpose of the experiment.

[11] 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).

[12] All B1 farmers who attended the training did refer someone from the list of allowed candidates. Invited B1 farmers who did not come to training could not, by design, refer anyone. More than 90% of invited B1 and B2 farmers attended the training. The participation rate does not vary across treatment arms. The main reasons given for not attending training are illness and absence from home on the day of the training.

[13] On average, more than 60% of all the farmers in a selected village were randomly selected to participate in the study. For large villages in terms of population and area, we limit the study to some neighborhoods to ensure that participating farmers are sufficiently close to each other.

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

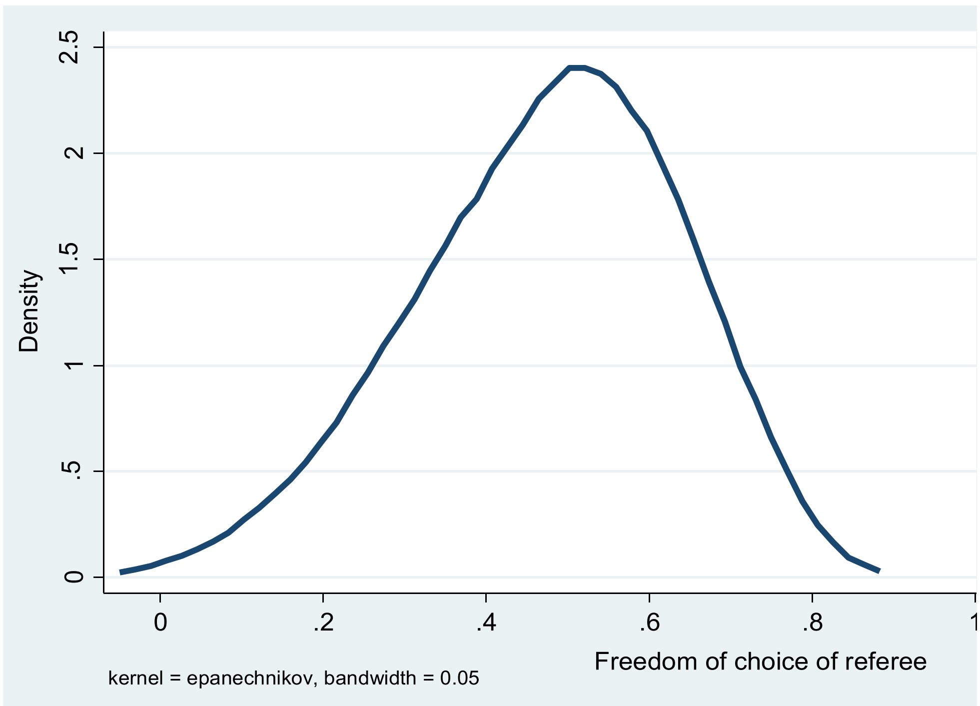

Fig. 1. Proportion of farmers available to be referred by each B1 farmer.

quently adopts SRI practices.[14] The rules of compensation are explained to referees before they select someone from the pool. For both T2 and T3 farmers, compensation is paid a few weeks after training, at a time when the adoption of SRI practices can be verified in the field by BRAC staff. It is important to note that the compensation offered to referees in T2 and T3 is negligible relative to the potential material and labor cost of wrongly adopting SRI. It is therefore unlikely that a T3 referee would be able to induce a B2 farmer into adopting only to share the incentive payment with him.

Each participating farmer completes a baseline household survey covering demographics, income, and assets. Detailed agricultural production information is 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 perform three tests of cognitive ability – Raven's matrices, numeracy, and memory span – and we measure numerical reasoning using simple deduction and counting tests.

In addition, respondents are 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 farmer in their village). We also collect information on the physical distance between the home or land of each pair of farmers in the village sample. In addition, each respondent is asked to recommend up to five farmers who could potentially engage in SRI farming. This information is collected in order to measure social proximity R_{ij} for the estimation of regression model (7) in next section.

We also conduct 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 has 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 follows at least three of the six key principles of SRI

[14] The compensation level for T2 and T3 was chosen so as to be similar in expected value, based on a 50% SRI adoption rate. B1 farmers were only informed of the nature of the referral compensation they would receive after the training had ended and when they were asked to refer a B2 farmer. No B1 farmer was informed by BRAC of the existence of referral, whether compensated or not, at the time they were invited for training.

on any of his plots.[15]

Balance on key demographic and socioeconomic 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.[16]

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.

4. 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:

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

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:

$$y_i = \alpha_0 + \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{for} i \in C \cup B1 \tag{2}$$

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 potential rice 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:

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

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:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + \beta R_i + u_i \text{for} i \in B1 \cup B2 \tag{4}$$

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 $\beta > 0$.

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:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{for} i \in B2 \tag{5}$$

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:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + C_i \sum_{k=1}^{3} \beta_k T_{ki} + u_i \text{for} i \in B2 \tag{6}$$

where C_i measures the size of the pool faced by the farmer who recommended i for training.[17] 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.

[15] 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 × 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 values adapted to the local context. This is the set of practices recommended by BRRI and BRAC for SRI in Bangladesh.

[16] We do not test balance for B2 farmers since, by design, lack of balance would indicates targeting, which is precisely what we are investigating. Nonetheless, if we do 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 a different type of selection. The differences are not large in magnitude, however. More about this below.

[17] 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.

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

Table 2
Balance.

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	0.12	0.47	0.19	0.53
Average education of the household	4.3	4.3	4.2	4.5	4.2	0.75	**0.01**	0.89	**0.01**
Cultivable farm area in last Boro season (decimals)	165.9	163.4	160.9	162.6	166.6	0.55	0.81	0.41	0.48
Household size	5.2	5.2	5.1	5.2	5.1	0.37	0.59	0.74	0.82
Maximum education of any household member	8.7	8.5	8.5	8.7	8.4	0.21	0.06	0.89	**0.04**
Working age members in the household	3.2	3.1	3.2	3.2	3.1	0.78	0.76	0.24	0.36
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	0.31
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	0.20
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.

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:

$$x_{ij} = \beta_0 + \beta_1 L_{ij} + \beta_2 L_{ij} C_i + \varepsilon_{ij} \tag{7}$$

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 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 use referral.[18] To investigate this possibility we first estimate a predictive regression based on a vector of farmer observables Z_i:

$$y_i = g(Z_i) + u_i \text{for} i \in B1 \tag{8}$$

We only use $B1$ farmers to avoid selection effects. Predictions from model (8) represent what BRAC could have forecasted from farmer observables Z_i based on predictions obtained from the adoption behavior of their normally recruited trainees. We include in Z_i a large number of farmer observables at our disposal, and we estimate model (8) using five functional forms for $g(.)$: OLS; logit; two versions of LASSO; and random forest. The latter two are included to allow for machine learning. OLS is the obvious benchmark. Logit imposes some structure on the data generating process and thus could potentially increase prediction efficiency. LASSO estimators seek to reduce OLS over-fitting by penalizing – and dropping – regressors that contribute little to fit. This could improve out-of-sample fit. In contrast, random forest maximizes in-sample fit – and customarily achieves R^2 coefficients well in excess of 90% – in an attempt to improve out-of-sample fit. To improve machine learning, all Z_i regressors are normalized to have mean 0 and unitary variance.
We use each of these estimated models to obtain a prediction $\hat{y}_i$ of SRI adoption for $B1$ farmers (in-sample) and $B2$ farmers (out-of-sample). We then test whether knowing that a farmer was referred improves this prediction. To this effect we test whether $\lambda_1 = 0$ in a model of the form:

$$y_i - \hat{y}_i = \lambda_0 + \lambda_1 R_i + e_i \text{for} i \in B1 \cup B2 \tag{9}$$

where $y_i - \hat{y}_i$ is the prediction error which, by construction, has mean 0 for $B1$ farmers. A positive and significant λ_1 means that a referred farmer is λ_1 percentage points more likely to adopt than what BRAC could have forecasted from farmer observables Z_i. This would suggest that referral increases adoption. We also test this separately for each of the three treatments $T_{ki} \in (T1, T2, T3)$:

$$y_i - \hat{y}_i = \lambda_0 + \sum_{k=1}^{3} \lambda_k T_{ki} + e_i \text{for} i \in B1 \cup B2 \tag{10}$$

using a predictive model in which $T2$ and $T3$ dummies are added to Z_i. The interpretation is the same.

3. Next we ask whether referred farmers are predicted to have higher yields and profits than what BRAC could have predicted, based on observables Z_i, from outcomes for the farmers they train. To this effect, we estimate prediction models of the same form as (8) but replacing the dependent variable y_i with rice yield and farm profit, respectively. Logit is not included since yield and profit are not dichotomous variables.

We then test whether referred $B2$ farmers on average have higher yields and farm profits than what could be predicted for $B1$ farmers with similar observables. Keep in mind that this prediction has an intent-to-treat interpretation: if referred farmers adopt more but have similar yields and profits on average, this implies (with some assumptions) that the additional adopters induced by referral do not benefit from adoption. We revisit this last issue in detail at the end of the paper.

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

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	4934	4934
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.

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 (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. Similar results are obtained if we use alternative measures of adoption – see Online Appendix Table A1.[19] These findings are interesting in themselves, given the adoption difficulties encountered elsewhere with SRI cultivation (Moser and Barrett, 2006).

In Table 4 we present similar estimation results for crop production, revenue, costs, family labor inputs,[20] 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. These results indicate that exposing Bangladesh rice farmers to SRI training has on average a beneficial effect on their agricultural performance in rice production. Except for treatment T2, the results also indicate a significant positive effect on input and labor costs – and hence total costs – as well as on unpaid family labor. This as expected: SRI is known to be more labor and management intensive than more traditional methods of production. In all cases, the magnitude of ITT coefficients is large relative to baseline: production and revenue per area both increase by 14–19% while profit increases by 18–30%. Total production costs per area (net of family labor) increase by 2–12% while family labor inputs increase by 2–12%. Results are virtually identical if we include household controls.

To provide a better sense of the magnitude of the SRI benefits for adopters, we estimate in Appendix Tables A2a-d a local average treatment effects (LATE) version of Table 4 in which we instrument adoption with treatment. Estimates indicate that, on average, rice yield and the revenue from rice cultivation increase by 52% and 45% relative to control farmers, respectively. Cost increases are all positive, particularly for family labor (+34%) and hired labor (+28%), as expected. Total cost goes up by a quarter and profits by 65–70% relative to controls. These effects are large in magnitude. From this we conclude that SRI is beneficial for adopters, even if more costly. We also note that increases in yield and crop revenue are slightly lower for T2 and T3 relative to T1: although the latter treatments increase adoption, they also reduce slightly the average yield and revenue gain from adoption. This is our first indication that these treatments attract additional adopters who, on average, benefit less from SRI than T1 adopters. In other words, T2 and T3 seem to reduce targeting quality, an issue that we revisit in detail below.

From this evidence we conclude that training has a positive effect on the 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 (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: not all trainees adopt the new technology.

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), which compares untreated and B1 farmers to control farmers. To recall, the purpose of the regression is to test whether adoption rates among untreated and B1 farmers is identical, which would happen if information dispensed during training diffuses to all and all farmers are similarly attracted by SRI. 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 this diffusion effect nonetheless remains well below the effect of training on B1 farmers, as evidenced 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 (4) to investigate whether referral brings to training farmers who are more likely to subsequently adopt SRI. Results shown in Table 7 indicate that the interaction coefficient between referral and treatment is positive and significant: a 4.2 percentage points increase in the propensity of adopting SRI. From this we conclude that B2 farmers are moderately more likely to adopt SRI than randomly selected B1 farmers. This finding is consistent with referral providing somewhat better targeting.

Next we examine whether rewarding or incentivizing referees makes a difference in referral quality. To this effect we report in the second column of Table 7 the referral selection effect for each treatment separately. Point estimates do not support the idea that incentivizing ref-

[19] The alternative measures of adoption of SRI are: (1) direct response from farmers that 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); (3) the extent of SRI adoption (number of adopted practices); and (4) the proportion of land on which SRI principles were applied.

[20] Given that trainees are told SRI requires higher labor inputs, this variable is subject to response bias due to experimenter demand. This is why we report regression results for total costs and profit with and without imputed family labor.

Table 4
ITT effect on agricultural performance per area.

Dependent variable	Yield in Kg	Revenue per area	Hired labour per area	Total labour cost per area	Total cost per area	Total cost per area (including family labor)	Estimated profit per area	Estimated profit per area (including family labor)
Treatment T1	3.824∗∗∗	136.684∗∗∗	31.517∗∗∗	51.059∗∗∗	50.777∗∗∗	69.878∗∗∗	83.769∗∗∗	65.275∗∗
	(0.694)	(23.042)	(11.922)	(11.044)	(16.161)	(14.867)	(23.640)	(27.971)
Treatment T2	4.248∗∗∗	135.369∗∗∗	7.846	19.626∗	9.294	23.482	119.598∗∗∗	106.070∗∗∗
	(0.647)	(19.591)	(10.875)	(10.775)	(15.594)	(14.824)	(21.170)	(22.592)
Treatment T3	3.718∗∗∗	123.157∗∗∗	23.430∗	38.332∗∗∗	36.639∗∗	53.841∗∗∗	82.835∗∗∗	67.059∗∗∗
	(0.681)	(23.91)	(12.052)	(11.081)	(16.902)	(15.080)	(23.416)	(25.214)
Baseline value	0.230∗∗∗	0.189∗∗∗	0.212∗∗∗	0.039∗∗	0.037∗∗	0.025∗∗	0.015	0.034∗∗
	(0.025)	(0.022)	(0.025)	(0.016)	(0.016)	(0.010)	(0.016)	(0.015)
R-squared	0.120	0.117	0.093	0.032	0.035	0.042	0.043	0.030
Baseline level in control villages	**22.65**	**867.93**	**197.86**	**296.25**	**412.82**	**511.22**	**455.11**	**356.71**

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 labor. Family labor cost is calculated at the same wage rate as hired labor. 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	2741
R-squared	0.342

Notes: Standard errors in parentheses are clustered at the village level. ∗∗∗ <0.01, ∗∗p < 0.05, ∗p < 0.1. *Only B1 trainees and all farmers from control villages are included as mentioned in* equation (2). 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	0.075∗∗∗
	(0.014)
Village Treatment T2	0.081∗∗∗
	(0.018)
Village Treatment T3	0.067∗∗∗
	(0.013)
Village Treatment T1 x trainee	0.296∗∗∗
	(0.043)
Village Treatment T2 x trainee	0.408∗∗∗
	(0.036)
Village Treatment T3 x trainee	0.427∗∗∗
	(0.054)
Observations	3968
R-squared	0.296

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. *Samples include B1 and untreated farmers from treatment villages (B2 trainees are excluded) and all farmers from control villages as mentioned in* equation.(3). Similar results are obtained if we add the same controls as in Table 3.

erees improves referral: if anything, the coefficients of the interaction terms between B2 trainees and treatments T2 and T3 are smaller than that for T1. These differences between estimated coefficients are not, however, statistically significant. From this we conclude that rewarding or 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 (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 Fig. 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 (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} is low since each B1 farmers only recommend one farmers out of

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

Dependent variable	(1)	(2)
	SRI-adoption (1 = yes, 0 = No)	
Village Treatment T1	0.377***	0.371***
	(0.047)	(0.048)
Village Treatment T2	0.490***	0.489***
	(0.042)	(0.044)
Village Treatment T3	0.488***	0.494***
	(0.056)	(0.057)
Village Treatment T1, T2, T3 x B2 trainee	0.042**	
	(0.021)	
Village Treatment T1 x B2 trainee		0.054
		(0.039)
Village Treatment T2 x B2 trainee		0.042
		(0.036)
Village Treatment T3 x B2 trainee		0.029
		(0.032)
Observations	2044	2044
R-squared	0.479	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.

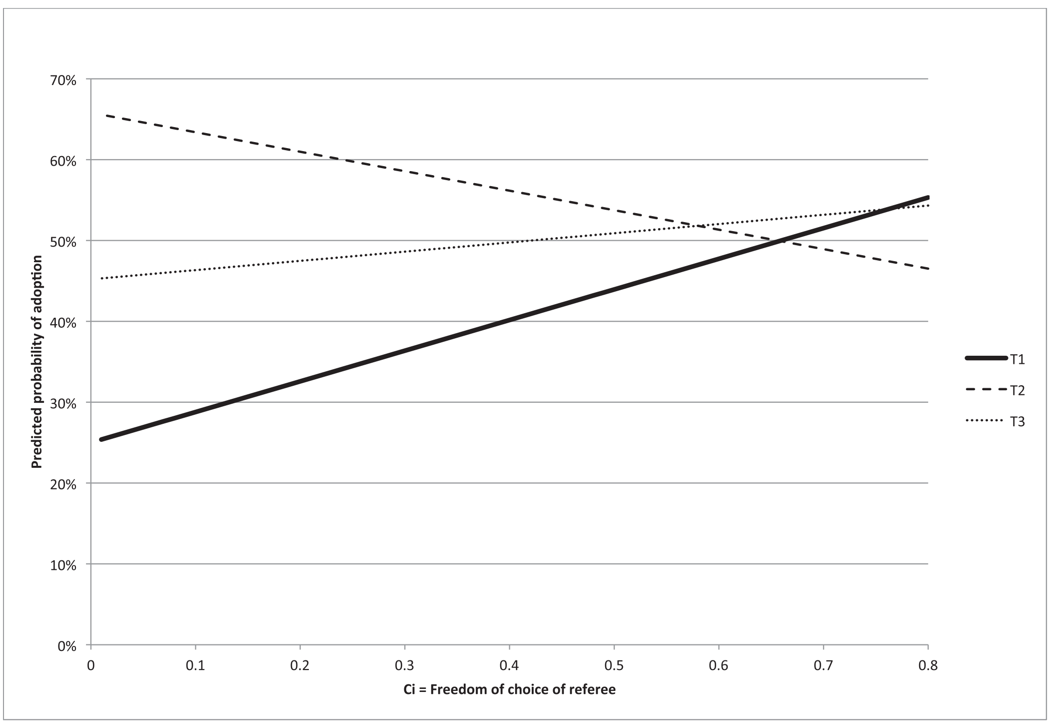

Fig. 2. Targeting and incentives. $C_i = 0$ when i was the only farmer that his referee could have recommended, i.e., the only remaining farmer in the pool. Division by 30 facilitates interpretation of coefficient β_k: when $C_i = 1$ it means that i's referee could have picked i among any of the 30 farmers in the (average) village sample. In practice C_i is always less than 1 since B1 farmers are automatically excluded from the picking list.

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 Fig. 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 differ-

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

Dependent variable	SRI-adoption (1 = yes, 0 = No)
Treatment T1	0.260**
	(0.114)
Treatment T2	0.658***
	(0.120)
Treatment T3	0.452***
	(0.177)
Treatment T1 x C_i	0.379*
	(0.225)
Treatment T2 x C_i	−0.241
	(0.216)
Treatment T3 x C_i	0.114
	(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 as mentioned in* equation (6). Similar results are obtained if we add the same controls as in Table 3.

ently, B1 farmers are less likely to refer a socially close farmer when they have more freedom to choose.

This result 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 they 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 (8) on B1 farmers, and testing with models (9) and (10) whether referral has an added predictive power over and above what can be predicted from characteristics observable to BRAC agents. As explained in Section 4, we experiment with various predictive models. Most models are fitted using a total of 19 farmer characteristics that are potentially observable by BRAC. We also check the robustness of our findings to the possibility that BRAC may know less about farmers than the 19 characteristics used. To do this we only use three easily observable predictors – i.e., dummies for whether the farmer is older, more educated, and cultivates more land than the median farmer. Predictive models are fitted using only B1 farmers who, having been selected at random from a BRAC-selected pool, constitute a representative sample of the farmers BRAC normally trains. Table A3 in the online appendix shows all the 19 estimated coefficients for the OLS case.

The in-sample goodness-of-fit of each model is presented in Table A4, which shows R^2 statistics for each of the estimated models, and for each of the outcomes of interest, namely, SRI adoption, rice yield, and agricultural profit. We note that, for each of these outcomes, the full OLS model, Logit (when suitable), and the two LASSO estimators are all similar in terms of in-sample fit – the LASSO estimators achieve lower fit by design since they drop some regressors. Unsurprisingly, OLS with only three dummies as regressors achieves a lower fit, especially for yield and profit. For these methods, the low R^2 means that observable characteristics have little predictive power – and hence that accessing information in the hands of other farmers may improve targeting by BRAC. In contrast, random forest achieves an improbably high fit for all three outcomes, a common finding with this method. The hope is that it also fits better out of sample.

The results for model (9), presented in Table 10, are remarkably consistent across all predictors. Regarding SRI adoption, we see that being a $B2$ trainee increases the predicted probability of adoption by 3.42–3.76 percentage points. This point estimate is significant at the 10% level irrespective of the predictor used, with the 3-regressors OLS doing (with an in-sample R^2 of 0.017) as well as random forest (with an in-sample R^2 of 0.978). What this means is that referral captures information that BRAC could not have learned from observing the farmer characteristics and adoption pattern of their regular trainees. In other words, referral reveals information about the likelihood of SRI adoption that is unobservable to BRAC. Although the point estimate is not large, it nonetheless suggests that referral does improve targeting in terms of the propensity to adopt. The rest of Table 10 is less encouraging. While farmer characteristics are equally good at predicting yield and profit in-sample as they are at predicting adoption, being a $B2$ farmer does not predict higher yield or profit. This is an important point that we revisit in detail in Section 5.4.

Results from model (10) are shown in Table 11. Keep in mind that the predicting regressions for this Table include T2 and T3 dummies for $B1$ farmers. The coefficients reported in Table 11 should thus be interpreted as the predictive value of referral in each of the three treatments. Regarding SRI adoption, point estimates are similar, on average across treatments, to those reported in Table 10. They are also fairly stable across predictors – with the possible exception of random forest, which shows more variation across treatments. We find no evidence that T2 or T3 have a larger coefficient than T1 for $B2$ farmers – if anything, point estimates are smaller for T3 when material incentives are provided. As a whole, we conclude that there is not strong evidence that rewarding or incentivizing referees improves referral quality. Possibly because of a lack of power, none of the coefficients is individually significant, however.

To summarize the lessons from this Section, the evidence suggests that referred B2 farmers are slightly more likely to adopt SRI after training than randomly selected B1 farmers. But rewarding or incentivizing referees 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:

$$y_i = \sum_{k=1}^{3} \alpha_k T_{ki} + u_i \text{for} i \in B1 \tag{11}$$

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.

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 C_i	−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 C_i 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 C_i 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.

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. In contrast, there is no reason to expect such coordination to arise from an experimenter demand effect or salience effect.

To investigate this hypothesis, 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:

$$|y_i - y_j| = \alpha_0 + \alpha_1 m_{ij} + m_{ij} \sum_{k=2}^{3} \alpha_k T_{ki} + u_i \text{forall} ij \text{pairsin} B1 \cup B2 \tag{12}$$

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. As a by-product, model (12) yields a test of Montgomery (1991) key assumption, namely that, because of homophily, referee and referred tend to be more similar than randomly matched pairs. If this is true, we should observe $\alpha_1 < 0$: referee and referred ought to be less different on average than random farmer pairs.

Estimation results are presented in Table 13. We find that α_1 is positive and not significantly different from 0. This provides no support from the assortative matching hypothesis central to Montgomery's model. We however find that 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. Since a similar effect is absent from T1 and T2, this suggests coordination in adoption only for T3 subjects.

To refine the above test, we would like net out correlation due to positive assorting on observables: B1 farmers may recommend someone sharing similar characteristics and thus a similar propensity to adopt, and this may drive the correlation in their decisions. To purge our estimates from this possibility, we look at the correlation in behavior that cannot be predicted from similarity in characteristics. Formally, let $\hat{y}_i$ as before be the predicted adoption from regression (10) estimated from B1 farmers, and define $\hat{u}_i = y_i - \hat{y}_i$ for $i \in B1 \cup B2$. In other words, $\hat{u}_i$ captures the variation in $\hat{y}_i$ 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 $\hat{u}_i$ should be more correlated with $\hat{u}_j$ when i referred j than for any other farmer pair. This yields the following test:

$$\hat{u}_i = \hat{u}_j \sum_{k=1}^{3} \tau_k T_{ki} + \hat{u}_j m_{ij} \sum_{k=1}^{3} \varphi_k T_{ki} + \epsilon_{ij} \text{forall} ij \text{pairsin} B1 \cup B2 \tag{13}$$

We expect $\tau_k = 0$ since the predicting equation (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: φ_1 is again positive and non-significant. More importantly, $\varphi_3 > 0$, 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.

5.4. Over-adoption

If T2 and T3 increase adoption not because of better selection but because of peer effects, this raises the concern that increased adoption attracts farmers less able to benefit from the SRI technology. Indeed, in Tables 10 and 11 we found that while referral predicts higher adoption among *B*2 farmers, it does not predict higher yields or profits. This is of course suspicious: if referral selects more suitable trainees, we would expect them to adopt more often *precisely* because, on average, they get higher yields and profits. This leads us to wonder whether rewarding or incentivizing referral leads to over-adoption. How can we figure it out?

By definition, the average treatment effect for a village is the village total effect divided by the number of villagers. Thus if a treatment increases adoption but reduces the village total relative to another treatment, it must mean that the additional adopters reduce the village total – i.e., they experience a reduction in performance as a result of adoption. To formalize this intuition, let z_t be the proportion of adopters

Table 10
Do batch 2 trainees have outcomes than differ from what can be predicted for batch 1 trainees?

The predictive model was:	(1) OLS (small)	(2) OLS (full)	(3) Logit	(4) Lasso (1)	(5) Lasso (2)	(6) Random forest
A. SRI Adoption						
Dummy = 1 if batch 2 trainee	0.0357*	0.0347*	0.0342*	0.0373*	0.0376*	0.0375*
	(0.0205)	(0.0205)	(0.0205)	(0.0206)	(0.0206)	(0.0214)
Constant	8.29e-10	−5.53e-10	−1.49e-08	1.11e-10	7.60e-11	−0.00181
	(0.0285)	(0.0266)	(0.0266)	(0.0271)	(0.0272)	(0.00978)
Observations	2044	2044	2044	2044	2044	2044
R-squared	0.001	0.001	0.001	0.001	0.001	0.003
B. Rice yields						
Dummy = 1 if batch 2 trainee	−0.023	−0.208		−0.198	−0.188	−0.171
	(0.0404)	(0.283)		(0.280)	(0.279)	(0.355)
Constant	2.34e-09	5.31e-09		3.54e-09	−6.19e-09	0.00707
	(0.0638)	(0.411)		(0.415)	(0.418)	(0.158)
Observations	2044	2044		2044	2044	2044
R-squared	0.000	0.000		0.000	0.000	0.000
C. Agricultural profit per decimal						
Dummy = 1 if batch 2 trainee	0.0072	−0.576		0.0630	0.324	0.928
	(0.0424)	(9.748)		(9.674)	(9.678)	(10.70)
Constant	8.57e-10	7.36e-07		5.38e-07	7.36e-07	−0.107
	(0.0595)	(12.68)		(12.89)	(12.93)	(4.867)
Observations	2044	2044		2044	2044	2044
R-squared	0.000	0.000		0.000	0.000	0.000

Notes: Each regression reports the coefficients of an OLS regression in which the dependent variable is the difference between the outcome of interest and its predicted value derived from one of the five predictive models considered here. Observations include B1 and B2 trainees as mentioned in equations (9) and (10). Each regression captures the extent to which being a batch 2 trainee explains a significant fraction of the prediction error. In the first panel the dependent variable is the difference between actual SRI adoption and predicted SRI adoption from each of the five predictive models. In the second panel the dependent variable is the difference between actual rice yield and predicted rice yield from each of the predictive models. In the third panel the dependent variable is the difference between actual agricultural profit and predicted agricultural profit from each of the predictive models. Yield and profit have been normalized to have mean 0 and unit variance. This means that coefficients for yield and profit can be interpreted as the predicted difference in output measured in standard deviation units. The actual mean rice yield for batch 1 and 2 trainees is 27.1 Kg per decimal with a standard deviation of 7.6. The actual mean profit among batch 1 and 2 farmers is 560 Takas per decimal with a standard deviation of 248. Standard errors are given in parentheses; they are clustered at the village level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Predictions are estimated separately using the five possible predicting models considered here. In-sample goodness of fit statistics for each predictive model are reported in Table Azz. The logit predictor is the predicted probability of adoption. It cannot be obtained for yield and profit since they are not dichotomous variables. Lasso (1) is the cross-validation Lasso optimal predictor. Lasso (2) is the standard lasso with AIC information criterion. Both are estimated using the add-on LASSOPACK set of Stata commands. The Random Forest predictor is obtained using the add-on randomforest Stata command. Regressors used in all the predictive regressions are the following: age of household head; dummy = 1 if age of household head is above median age; years of education of the household head; dummy = 1 if education of head is above median education; log of number of cultivated decimals; dummy = 1 if cultivated land is above median; dummy = 1 if head is not a Muslim; dummy = 1 if head is married; dummy = 1 if main occupation of head is agriculture; log of last month expenditures at baseline; log of last year expenditures at baseline; log of average monthly income at baseline; self-reported relative income status at baseline (1–7); self-reported food self-security index at baseline (1–5); total land at baseline; log of asset value at baseline; average education of household members at baseline; household size at baseline; number of working age adults at baseline. Prior to estimation of the predictive model, each regressor is normalized to have mean zero and unit variance. See Table A3 for coefficient estimates of all the regressors in the OLS predicting regressions.

Table 11
Do B2 trainees have treatment outcomes than differ from what is predicted for B1 trainees?

The predictive model was:	(1) OLS (small)	(2) OLS (full)	(3) Logit	(4) Lasso (1)	(6) Random forest
A. SRI Adoption					
Dummy = 1 if B2 trainee x T1	0.0468	0.0448	0.0462	0.0348	−0.00486
	(0.0476)	(0.0467)	(0.0466)	(0.0471)	(0.0462)
Dummy = 1 if B2 trainee x T2	0.0400	0.0471	0.0451	0.0530	0.0684*
	(0.0431)	(0.0397)	(0.0397)	(0.0399)	(0.0367)
Dummy = 1 if B2 trainee x T3	0.0172	0.0102	0.0101	0.0199	0.0442
	(0.0479)	(0.0451)	(0.0450)	(0.0455)	(0.0477)
Constant	4.92e-09	1.13e-09	−1.46e-08	6.22e-10	−0.00164
	(0.0281)	(0.0263)	(0.0262)	(0.0266)	(0.00969)
Observations	2044	2044	2044	2044	2044
R-squared	0.002	0.002	0.002	0.002	0.006
B. Rice yields					
Dummy = 1 if B2 trainee x T1	−0.0934	−0.0999		−0.0987	−0.0901
	(0.111)	(0.111)		(0.110)	(0.110)
Dummy = 1 if B2 trainee x T2	−0.0124	−0.0255		−0.00703	−0.00159
	(0.106)	(0.106)		(0.105)	(0.104)
Dummy = 1 if B2 trainee x T3	0.0334	0.0317		0.0162	0.0151
	(0.104)	(0.101)		(0.102)	(0.113)
Constant	7.81e-10	−4.49e-10		−7.59e-10	0.00261
	(0.0637)	(0.0596)		(0.0603)	(0.0227)
Observations	2044	2044		2044	2044
R-squared	0.001	0.001		0.001	0.002
C. Agricultural profit per decimal					
Dummy = 1 if B2 trainee x T1	−0.0330	−0.0440		−0.0404	−0.0494
	(0.104)	(0.104)		(0.103)	(0.101)
Dummy = 1 if B2 trainee x T2	−0.0341	−0.0496		−0.0182	0.00695
	(0.0897)	(0.0865)		(0.0875)	(0.0847)
Dummy = 1 if B2 trainee x T3	0.0898	0.0875		0.0619	0.0678
	(0.0948)	(0.0926)		(0.0940)	(0.104)
Constant	1.25e-09	5.78e-10		−3.60e-10	0.000144
	(0.0584)	(0.0543)		(0.0555)	(0.0204)
Observations	2044	2044		2044	2044
R-squared	0.001	0.002		0.001	0.002

Notes: Each regression reports the coefficients of an OLS regression in which the dependent variable is the difference between the outcome of interest and its predicted value derived from one of the four predictive models considered here. Observations include B1 and B2 trainees as mentioned in equations (9) and (10). Each predictive model contains two additional regressors than in Table 10: being in treatment 2 and being in treatment 3. Each regression therefore captures the extent to which being a batch 2 trainee explains a significant fraction of the prediction error relative to batch 1 trainees in the same treatment. In the first panel the dependent variable is the difference between actual SRI adoption and predicted SRI adoption from each of the five predictive models. In the second panel the dependent variable is the difference between actual rice yield and the predicted rice yield from each of the predictive models. In the third panel the dependent variable is the difference between actual agricultural profit and predicted agricultural profit from each of the predictive models. Yield and profit have been normalized to have mean 0 and unit variance. This means that coefficients for yield and profit can be interpreted as the predicted difference between batch 1 and 2 farmers in each treatment, measured in standard deviation units. The actual mean rice yield for batch 1 and 2 trainees is 27.1 Kg per decimal with a standard deviation of 7.6. The actual mean profit among batch 1 and 2 farmers is 560 Takas per decimal with a standard deviation of 248. Standard errors are given in parentheses; they are clustered at the village level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Predictions are estimated separately using the five possible predicting models considered here. In-sample goodness of fit statistics for each predictive model are reported in Table Azz. The logit predictor is the predicted probability of adoption. It cannot be obtained for yield and profit since they are not dichotomous variables. Lasso (1) is the cross-validation Lasso optimal predictor. It is estimated using the add-on LASSOPACK set of Stata commands. Lasso (2) is not reported for this Table because it fails to converge. The Random Forest predictor is obtained using the add-on randomforest Stata command. Regressors used in all the predictive regressions are the following: age of household head; dummy = 1 if age of household head is above median age; years of education of the household head; dummy = 1 if education of head is above median education; log of number of cultivated decimals; dummy = 1 if cultivated land is above median; dummy = 1 if head is not a Muslim; dummy = 1 if head is married; dummy = 1 if main occupation of head is agriculture; log of last month expenditures at baseline; log of last year expenditures at baseline; log of average monthly income at baseline; self-reported relative income status at baseline (1–7); self-reported food self-security index at baseline (1–5); total land at baseline; log of asset value at baseline; average education of household members at baseline; household size at baseline; number of working age adults at baseline. Prior to estimation of the predictive model, each regressor is normalized to have mean zero and unit variance. The predictive models also include dummies for treatment T2 and T3, to control for the effect of these treatments on batch 1 farmers.

under treatment t and let w_t be the average outcome for adopters. The ITT effect is $z_t w_t$ divided by the number of farmers n – which we assume identical across villages and subsequently ignore. We wish to compare two treatments i and j, the second of which contains an additional intervention on top of treatment i. We observe higher adoption in treatment j – i.e., $z_j > z_i$. Since treatment j combines treatment i with another intervention, we can think of the z_j adopters are composed to two groups: z_i adopters who enjoy treatment effect w_i, and additional adopters $\widetilde{z}_j = z_j - z_i$ who enjoy treatment effect $\widetilde{w}_j$. We have:

$$z_j w_j = z_i w_i + \widetilde{z}_j \widetilde{w}_j$$

$$\widetilde{w}_j = \frac{z_j w_j - z_i w_i}{z_j - z_i} \tag{14}$$

where $z_i w_i$ and $z_j w_j$ are the ITT coefficients for treatments i and j, respectively, and $z_j - z_i$ is the increase in adoption achieved by going from treatment i to treatment j. The sign of $\widetilde{w}_j$ tells us whether, on average, adoption increase the outcome variable for infra-marginal individuals induced to adopt by switching from treatment i to j.

Adoption probabilities z_t are given in Table 3, while ITT estimates

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	1078
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 as mentioned in* equation (11).

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	8546	8546
R-squared	0.001	0.13

Notes: *Observations include all possible ij pairs of B1 and B2 trainees in a village as mentioned in* equation (12). 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. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

$z_i w_i$ are given in Table 4 for different outcome variables. In Table 15 we use formula (14) to estimate the value of $\widetilde{w}_j$ from these estimated coefficients. Results indicate that additional adopters in the T3 treatment on average *lose* from adoption: if we exclude family labor, which is always notoriously difficult to measure (and, in this case, subject to response bias), 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. Additional adopters under T2 appear to have higher yields and earn higher profits than T1 adopters, but they also use much fewer inputs, which also indicates a superficial adoption of SRI practices. Why T2 additional adopters have higher yields – and hence profits – is unclear. But these findings nonetheless suggest caution when interpreting increased adoption as a beneficial outcome of treatment: the peer effects triggered by incentivizing referees seem to have induced adoption by infra-marginal farmers whose performance decreases as a result of adoption, and whose adoption is often incomplete. Put differently, rewarding or incentivizing referral reduced targeting quality.

The same qualitative reasoning applies to the results presented in Table 10. There we noted that $B2$ farmers adopt more than $B1$ trainees but they have the same rice yields and the same profits as $B1$ farmers. This suggests the extra $B2$ adopters do not experience an increase in yield and profits that would raise the average of $B2$ farmers relative to $B1$ farmers – and hence are not really better targeted than $B1$ trainees in terms of outcomes.

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

Dependent variable	U_i
Treatment T1 x U_j	0.008
	(0.010)
Treatment T2 x U_j	−0.006
	(0.006)
Treatment T3 x U_j	−0.010**
	(0.005)
Treatment T1 x U_j x Referred dummy	0.009
	(0.040)
Treatment T2 x U_j x Referred dummy	0.053
	(0.060)
Treatment T3 x U_j x Referred dummy	0.081*
	(0.043)
Village Fixed Effects	Yes
Observations	8546
R-squared	0.347

Notes: *Observations include all possible ij pairs of B1 and B2 trainees in a village as mentioned in* equation (13). U_i and U_j 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. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

5.5. Persistence over time

As a final check on the evidence, we revisited 60 of the treated villages a year after the original endline survey,[21] and asked identical questions about SRI adoption in the preceding agricultural season. No additional SRI training was provided in the intervening year. Average adoption levels in the second endline are reported in Table 16 for all villagers, B1 trainees only, and B1+B2 trainees only. Since adoption in control villages remains 0, these coefficients represent average treatment effects one year after treatment.

Estimates reported in the first two columns are directly comparable to those in Table 4. We see that, one year after treatment, average adoption levels remain broadly similar to those observed at the first endline. There is a slight drop in adoption levels in T2 and T3 relative to Table 4, however. This finding is consistent with the idea that rewarding or incentivizing referral induced adoption by infra-marginal farmers, who subsequently reverted to their original practices. This reversion effect is particularly noticeable for B1 farmers, as we would expect if peer effects induced inefficient adoption. This is best seen by comparing the results reported in Table 5 to those in columns 3 and 4 of Table 16: average adoption among B1 trainees falls slightly relative to endline in treatments T2 and T3, but rises slightly among T1 farmers. A similar conclusion can be drawn for B2 trainees: in treatments T2 and T3 their average adoption rate was around 52–53% at first endline; they were much lower at second endline, suggesting a similar rate of retrenchment as for B1 trainees. We do not observe a similar retrenchment for T1 farmers. Taken together, these findings support our interpretation that rewarding or incentivizing referral triggered unwanted peer effects that induced a temporary over-adoption of the SRI technology by infra-marginal farmers; these farmers subsequently reverted to their old practices.

[21] These 60 villages are selected randomly from the original sample of 120 treated villages, with 20 villages from each of the three treatment arms. Village and farmer characteristics are similar to those of the 60 unselected villages.

Table 15
Imputed change for additional adopters induced by treatments T2 and T3.

Dependent variable	Yield in Kg	Revenue per area	Hired labour per area	Total labour cost per area	Total cost per area	Total cost per area (including family labour)	Estimated profit per area	Estimated profit per area (including family labour)
Treatment T2	6	−19	−338	−449	−593	−663	512	583
Treatment T3	−2	−242	−144	−227	−252	−286	−17	32

Note: Calculated from Tables 3 and 4 using formula (14). Revenue, cost and profit values are in BDT per decimal.

Table 16
Persistence of adoption at second endline.

Dependent variable	SRI adoption (1 = yes, 0 = No) at second endline					
Treatment T1	0.302***	0.304***	0.415***	0.416***	0.419***	0.421***
	(0.052)	(0.052)	(0.067)	(0.067)	(0.065)	(0.064)
Treatment T2	0.209***	0.210***	0.307***	0.307***	0.314***	0.315***
	(0.042)	(0.041)	(0.073)	(0.073)	(0.064)	(0.063)
Treatment T3	0.301***	0.301***	0.464***	0.464***	0.426***	0.426***
	(0.045)	(0.043)	(0.062)	(0.062)	(0.054)	(0.054)
Sample	All	All	B1	B1	B1&B2	B1&B2
Controls	No	Yes	No	Yes	No	Yes
Observations	2752	2752	1858	1858	2240	2240
R-squared	0.171	0.174	0.351	0.352	0.298	0.300

Notes: *Observations include all control villages plus 60 treated villages revisited a year after the first endline.* Estimator is linear probability model. Standard errors in parenthesis are clustered at the village level. ***p < 0.01, **p < 0.05, *p < 0.1. Baseline adoption in control villages is 0. Controls include: age dummy if household head is above 45 years of age; education dummy household head has primary schooling; farm size dummy if cultivable area is above median land size of 120 decimals.

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. These results are interesting in their own right because efforts to introduce SRI have not been particularly successful elsewhere (Moser and Barrett, 2006). Yet only 40–50% of trainees adopt SRI and many adopters do not follow all recommended practices, suggesting that training may not be perfectly targeted towards farmers most likely to benefit from it. SRI is not for everyone.

Does referral improve targeting? We find that referred farmers are on average 4.2% more likely to adopt SRI than randomly selected trainees, and 3.4–3.8% more likely after conditioning for the adoption rate that can be predicted from characteristics observed by the training agency. But there is no evidence that rewarding or incentivizing referees improves referral quality. We nonetheless find evidence that rewarded and incentivized trainees make more of an effort to identify potential adopters 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.

When we compare the behavior of referees and referred, we find that, when referral is rewarded or incentivized, average adoption increases by 12 percentage points for both referees and referred farmers. This may consistent with a demonstration effect: by offering financial incentives, the training agency may have convinced more farmers of the relevance of the training. Another possibility is that a referee who has received money to recommend someone else for treatment needs to 'put his money where his mouth is', that is, must demonstrate interest in the technology by adopting it himself. If this is true, adoption is more likely by the referred when the referee adopts too, and vice versa. We test this prediction and indeed 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.

The data also indicate 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 only adopt superficially and that they experience a fall in performance. Furthermore, we find that the additional adoption induced by rewarding or incentivizing referees is reversed a year later. Incentivizing referees appears to have triggered a feedback mechanism that encouraged infra-marginal 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.

CRediT authorship contribution statement

Marcel Fafchamps: Conceptualization, Methodology, Formal analysis, Writing - original draft. **Asad Islam:** Conceptualization, Methodology, Formal analysis, Writing - original draft. **Mohammad Abdul Malek:** Investigation, Resources, Project administration, Funding acquisition. **Debayan Pakrashi:** Formal analysis, Visualization.

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 irrigation or purchased inputs (e.g., seeds, fertilizer).

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.

Appendix B

Table 1
Attrition by Treatment Status: Balance

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.

Appendix C. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2019.102436.

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