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Mobilizing P2P Diffusion for New Agricultural Practices: Experimental Evidence from Bangladesh

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1 Introduction

This paper is interested in peer-to-peer dissemination to promote innovation. We wish to disseminate information about new practices that can be beneficial for some individuals. Since targeting everyone directly is costly, can we train a small number of potential beneficiaries and ask them to train others? This question is of practical relevance for a wide range of applications: e.g., introduction of new technologies to producers (e.g., Bandiera and Rasul 2006); dissemination of better business practices to firms (e.g., Bloom et al. 2013; Fafchamps and Quinn 2018); training workers on firm-specific equipment and practices (e.g., Campos et al. 2017); and introduction of new products to consumers (e.g., Miller and Mobarak 2015).

Agricultural extension has long practiced the 'model farmer' approach whereby a small number of farmers deemed more responsive to innovation are trained first, and then asked to disseminate the innovation to others. Despite its intuitive appeal, it is yet unclear whether this approach works (e.g., Beaman et al. 2018), under what conditions it can work, and what are the channels of peer-to-peer dissemination, if any.

To revisit this issue, we focus on a specific case: the introduction to small farmers of a new way of getting higher yields on an existing crop. The practice we disseminate does not require additional purchased inputs, which obviates the issue of credit constraints. But it demands more precise crop management – and therefore more visits to the field. Adoption is known to be beneficial for some producers, but not all (e.g., Barrett et al. 2004, Takahashi and Barrett 2014, Fafchamps et al. 2020).

Focusing on this technology offers many advantages in terms of design and is easily amenable to experimentation. First, the practice is relevant for millions of small producers facing relatively similar conditions. This ensures that lessons learned in one place stand a good chance of being replicable elsewhere. Second, small farmers do not employ permanent workers[1] and they provide all the crop management themselves. This means that we need not worry about the acquired knowledge being embedded in workers who can leave their employer after the training and benefit others. Finally, many innovations benefit from network or market externalities, making coordinated adoption essential for their successful introduction. In contrast, this technology can benefit a single farmer irrespective of what others do. All these features are ideal for a randomized controlled trial, i.e., the benefits from adoption are essentially i.i.d. and impact can easily be assessed by randomizing the intervention across similar units.

We conduct a large randomized controlled trial in Bangladesh in collaboration with BRAC. The intervention trains farmers in a set of rice growing practices called the System of Rice Intensification (SRI).[2] Within each study village we let BRAC identify a set of suitable farmers

[1] Although they do occasionally employ agricultural day laborers.

[2] SRI is a rice management practice in paddy rice cultivation. The practice involves transplanting single young seedlings with wider spacing, carefully and quickly into fields that are not continuously flooded, and whose soil has more organic matter and is actively aerated. It has a demonstrated potential for dramatically increasing rice yields without requiring additional purchased inputs (seed, fertilizer, etc.) or increased irrigation. A number of studies have shown significantly higher yields and increased profits associated with SRI (see Barrett et al. 2020 for references).

for rice cultivation and within this set we select a small number of farmers to be trained. These trainees are then asked to teach two other farmers from the list identified by BRAC. For the purpose of this paper, we call the trainees 'teacher-trainees' or 'teacher' and the two selected farmers 'students'. Unselected farmers are not targeted for training and are referred to as 'non-students'. This treatment design is randomized across villages, with control villages not receiving any SRI training from BRAC.

We experimentally investigate two subsidiary questions. First, we test whether we can improve information dissemination by incentivizing randomly selected teacher-trainees. As noted by Foster and Rosenzweig (1995), farmers may not fully internalize the benefits they can impart onto others when they acquire new information through learning or experimentation. Inviting trained farmers to teach others may suffer from the same problem. Financial incentives have been shown to be strong motivators of behavior in a variety of contexts (e.g., Ariely et al. 2009; BenYishay and Mobarak 2019; Duflo et al. 2011; Heath 2018).[3] By incentivizing teacher-trainees we may induce them to pay more attention to the training and to more effectively disseminate SRI knowledge to other farmers.

Second, we test whether information diffuses better from teacher-trainee to student when they are socially proximate. The literature has indeed shown social proximity to influence peer-to-peer behavior in various ways (e.g., Bobonis and Finan 2009, Bandiera et al. 2010, Banerjee et al. 2013), including agriculture (e.g., Conley and Udry 2010, Cai et al. 2013, Genius et al. 2013). By combining the two treatments, we can assess the respective roles of incentivization vs. social proximity. Disseminating health information across castes has for instance been shown by Berg et al. (2019) to be problematic unless the disseminator is incentivized. Our experiment uses an original random matching design to test whether a similar effect is observed in agricultural extension.

We find that, compared to control villages, farmers in treated villages are much more likely to adopt at least some of the SRI recommended practices. Adoption rates are highest among those farmers trained directly by BRAC. But it is also high among students, and even non-students display an adoption rate significantly higher than controls. We also find higher crop yields and profits among teacher-trainees and students, with no significant increase in input and labor costs. From this reduced-form evidence alone we can conclude that BRAC teacher-trainees are capable of conveying the usefulness of the new practices to other farmers and that these practices are, on average, beneficial to farmers. Next we compare adoption rates by teacher-trainees to those of simple SRI trainees studied by Fafchamps et al. (2020). We find that inducing trainees to teach SRI to other farmers essentially doubles their adoption rate.

To examine whether the peer-to-peer (P2P) transmission of new practices can be improved

[3]Other examples of studies examining the effects of financial incentives include: Bandiera et al. (2007) on incentives for managers; Muralidharan and Sundararaman (2011), Duflo, Hanna, and Ryan (2012), and Lavy (2002) on incentives for teachers; Gneezy and List (2006) on incentives for workers; Leuven et al. (2010) on incentives for students; and Gneezy and Rustichini (2000) on incentives for children volunteers. In contrast, Guiteras and Jack (2018) find that higher incentives do not attract more productive workers in day labor markets in Malawi.

through incentives, a randomly selected half of the teacher-trainees are offered a monetary payment conditional on the performance of their students at a quiz on SRI knowledge. We find evidence that incentivization improves learning. But it has no significant effect on adoption: point estimates are in general positive but not statistically significant. From this we conclude that incentivizing teacher-trainees does not significantly improve diffusion in our case.

We also investigate whether teacher-trainees better transmit SRI knowledge and practices to student farmers who are socially proximate. To this effect, all farmers in the village listed by BRAC are asked to nominate five other farmers from whom they would like to learn. We then randomly assign students to teacher-trainees such that half of the students are taught by the farmer they nominated, and the other half are taught by someone else. Results do not provide evidence that students matched with a teacher-trainee that they nominated do better on the test. In fact, they are less likely to adopt SRI than students taught by someone they did not nominate. From this we conclude that randomly matching teacher-trainees with people who nominated them does not improve dissemination – and does not justify the added cost and logistical complexity of the nomination and matching process.

We perform a mediation analysis to identify likely channels of influence in the adoption decision: is adoption correlated with answers to a quiz about the new practices, which would suggest that formal knowledge of the technology is important; and is adoption correlated with how closely the trained farmer applies the new practices, as would be the case if teaching by example increases adoption. We find that SRI knowledge – as assessed in a formal test – predicts the subsequent adoption of certain SRI practices. This suggests that grasping the new practices at an academic level helps adoption. In addition, we find that adoption by teacher-trainees helps predict adoption by their student farmers, suggesting that students follow the example of their teacher. This result is reminiscent of the co-adoption finding of Fafchamps et al. (2020).

We end the paper with a back-of-the-envelope cost-benefit analysis, combining estimated treatment effects on farm profits with detailed cost information from the field experiment. We estimate a social return of 156% on our intervention. For comparison purpose, we report an estimated cost-benefit analysis for the SRI training referral experiment of Fafchamps et al. (2020). That intervention produces a higher social return but a lower level of adoption overall.

The main contribution of this paper is empirical. We complement the literature on technology diffusion along social networks discussed earlier. We also make a methodological contribution by showing how to approach the estimation of average treatment effects when assignment to a treatment within the experiment is performed by a stratified random matching algorithm partly based on self-reported matching preferences – in our case, nominating a farmer as teacher. Such situations arise more frequently now that researchers are incorporating matching algorithms in their experiments (e.g., Abebe et al. 2018).

2 Experimental design

The P2P experiment presented in this paper was implemented in collaboration with BRAC, a well-known NGO operating worldwide and the largest NGO in Bangladesh. Having collaborated with BRAC on their SRI extension training in another project (e.g., Fafchamps et al. 2020), we wondered whether it would be possible to strengthen the diffusion of the knowledge that farmers acquire through BRAC training. The standard 'model farmer' approach relies on teaching a new agricultural technique to a few farmers and relying on them to spread this knowledge to others in the village. In our earlier research cited above, we show that this approach does generate some diffusion of new agricultural techniques, but it is far from achieving the same results as direct training.

We therefore proposed to BRAC to encourage trained farmers to pass their knowledge to others by formally asking each trainee to teach SRI to farmers selected by us. BRAC showed interest in the idea and became a full partner in the experiment. To maximize the external validity of our experiment, we rely on BRAC's extension wing, the Agriculture and Food Security Program (AFSP), to provide the SRI training itself as they normally do. The data collection and evaluation part of the research is implemented by BRAC Research and Evaluation Division (RED), which was established in 1975 and has evolved as a multi-disciplinary independent research unit within BRAC (Chowdhury et al. 2014). The distinct organizational nature of RED and AFSP helps researchers to conduct independent and credible experimental evaluation of any BRAC intervention.[4] Furthermore, the division of competence within BRAC offers the merit of reducing the potential for experimenter demand effects and has been used for instance to evaluate BRAC's well-known ultra-poor (Bandiera et al. 2017) and their tenant farmers credit program (Hossain et al. 2019).

The P2P experiment was implemented in 100 villages selected from the two districts of Rangpur and Bagura in Bangladesh. Of these 100 villages, 60 were randomly selected for treatment. The remaining 40 villages are controls. Selected farmers in treated villages receive a one-day training session on a rice farming technology entitled SRI cultivation. SRI training focuses on a small set of simple yet non-traditional practices that are more demanding in management and labor, but do not require the purchase of additional farm inputs (see Latif et al. 2005; Sinha and Talati 2007 for evidence on Bangladesh and West Bengal in India).[5] In particular, SRI imposes a specific transplanting time window and emphasizes a wider spacing and different arrangement of the transplanted rice.

[4]BRAC had previously worked with these communities, albeit not on SRI. Because they are a trusted partner, the study benefits from a high rate of compliance, a balanced sample, and very low attrition. This prior relationship of trust with study subjects may lead to higher uptake relative to diffusion from a random source (Usmani et al. 2018). Since BRAC is the agency most likely to be interested in applying our findings, we regard this as increasing external validity for Bangladesh.

[5]There are six principles associated with SRI, as verified and adapted by BRAC in the context of agro-climatic conditions in Bangladesh. 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 spacing (25 x 20 cm); reduced use of synthetic chemical fertilizers; intermittent irrigation; and complementary weed and pest control.

Treated villages are further divided randomly into two treatment arms of 30 villages each. In Treatment B villages, teacher-trainees receive an incentive payment; in Treatment A, they do not. The financial incentive given to teacher-trainees is based on the performance of their students at a quiz.

In each of the 100 villages, about 30 farmers are identified as potential SRI adopters by BRAC. Criteria for selection are the same as those used by Fafchamps et al. (2020), i.e., owning more than 50 decimals of land (i.e., half an acre)[6] but less than 10 acres. All farmers answer a baseline questionnaire gathering basic information about household composition and farm assets. We choose to focus on these farmers for two reasons. First, it makes little sense to target costly extension at unlikely adopters. Given its extensive SRI experience, BRAC is best placed to identify those most likely to adopt and we rely on their expertise. Secondly, we want our results to be policy relevant – which, in this case, means easy to integrate into BRAC's normal workflow.

As part of the baseline survey, each farmer [7] is asked to nominate up to five farmers (from the set of 30) who can act as their opinion leader or role model for rice cultivation methods and practices. We then rank each of the 30 farmers in each village based on the number of nominations received from other farmers. This ranking is used to select 6 teacher-trainees for training as follows: four teacher-trainees are selected at random from those with above-median number of nominations; and two from those with below-median rank. The reason for selecting more trainees above the median is because we expect them to be better teachers. We account for this stratified selection in the analysis. The training lasts for an entire day and is delivered by BRAC in the village itself. At the end of their one-day training, trainees take a quiz of 15 questions testing their knowledge of SRI. As per standard BRAC practice, all trainees receive a payment of 300 Taka as financial compensation for missing work for a day – approximately $4.

Of the remaining 24 farmers, 12 are randomly selected to be trained by the 6 teacher-trainees. Each teacher is randomly assigned two students: one who nominated the teacher as opinion leader or role model at baseline; and one who did not. This is done using a matching algorithm that combines information on nominations with random assignment – more about this in the next section. A priori we expect students to learn better if matched with a teacher-trainee that they nominated. The remaining 12 farmers do not receive any SRI training from BRAC.[8] We refer to these farmers as 'non-students'.

Teacher-trainees are given the names of the two students assigned to them. They are not told that one of them nominated them as opinion leader or role model. Teacher-trainees are then asked to teach these two students about the principles of SRI during one week and they are instructed to convey to them the same information as they received from BRAC trainers. To help them in their task, teacher-trainees are provided with three copies of a short brochure about

[6]A decimal of land is approximately equal to one hundredth of an acre.

[7]Due to budget constraints, we were unable to collect this information in control villages.

[8]Although we cannot (and do not seek to) prevent teachers from sharing SRI information with non-students if they wish to.

SRI – of which one copy is for the teacher and one is for each of their students. All teacher-trainees are informed that, at a pre-specified time and day at the end of the teaching week, their students will be given a short quiz to test their knowledge of SRI. In the weeks after that, all teacher-trainees and students can receive extension services on SRI from BRAC. Student farmers do not receive a payment for getting training from the teacher farmers. Certificates are provided to both teacher-trainees and student farmers a week after completing the SRI training. Such certificates are believed to have social recognition (e.g., Islam et al. 2018) and to encourage learning. Teacher certificates are labeled differently from that of student farmers.

The 60 treated villages are randomly assigned to one of two teacher treatments. In Treatment A – the unincentivized treatment – teacher-trainees receive a flat payment of 250 Bangladeshi Taka per student at the end of their teaching week. This payment is made shortly after the students have taken the quiz, but it does not depend on the students' quiz performance. In Treatment B – the incentivized treatment – teacher-trainees receive a payment that depends on the performance of each of their students on the quiz. For each student, the teacher receives 300 Taka if the student answers all 15 questions correctly, minus 20 Taka for each wrong answer. If the student responds less than 5 questions correctly, the teacher receives nothing for that student. Given the average number of correct answers on the quiz, teacher-trainees can expect to receive approximately the same payment under the two treatment schemes. Teacher-trainees are informed of the type of payment they will receive at the time they are told the name of their two students.[9] They are also told that they will receive no payment if their assigned students report not getting any training from them.

In addition to the core aspects of the intervention described above, we invite teacher-trainees to guess how their two students score on the quiz. This is done immediately after the students take the quiz and before the students are told their score. If the teacher can guess the number of correct answers given by each student, they receive an extra 50 Taka per student. They only receive this amount if their guess is equal to the number of correct answers plus or minus one. To illustrate, if a student answers 12 questions correctly and the teacher guesses 11, 12, or 13, the teacher receives 50 Taka – and nothing otherwise. This payment depends on their guess, not on how the student performs on the quiz.

For some of the analysis at the end of the paper, we complement the data from the P2P experiment with data from another SRI experiment. This experiment was conducted by the same research team in collaboration with BRAC in the same region (albeit not the same district) of Bangladesh (see Fafchamps et al. 2020). In that experiment, 182 villages were divided at

[9] More precisely, the experimental protocol instructs the trainers to say the following to the teachers: "We will go to your peer farmers who have been matched with you to teach/train them about SRI. We will pay you after we ask a similar set of questions to these peer farmers, which we asked to you in the post-training SRI test, based on the teaching materials given to you to test them about their knowledge about SRI provided by you. Your task is now to teach these peer farmers about SRI. You can discuss about what you have learned in this training. In addition, you share one copy of the training materials to these farmers. We advise you not to mention to peer farmers the payment we will give to you." Students and non-students farmers were not told that teachers were paid for their teaching. There was no incentive for adoption or diffusion of SRI paid to any farmer including teachers, students, and non-students.

random between 62 controls that received no SRI training, and three sets of 40 treated villages in each of which a batch of randomly selected farmers received SRI training. The sampling protocol followed in that study is identical to that followed here, but trainees were not asked to teach SRI to other farmers. The focus of this experiment is instead on the referral of potential trainees by farmers having just received SRI training themselves. A detailed description of the design of that experiment is provided in an online Appendix. The dataset from this SRI training referral experiment is used only to measure the *additional* effect of asking trainees to teach other farmers, over and above the effect of training itself.

3 Estimating treatment effects

Before presenting our testing strategy in detail, we briefly discuss how the experimental design affects the estimation of treatment effects. As is clear from the description of the experimental design, assignment of villages to treatment or control is random. Furthermore participating farmers are selected in each study village in the same way. This means that samples of farmers in control and treated villages are directly comparable in terms of means. This implies the reduced-form causal effect of being assigned to a treatment village can be evaluated simply by comparing simple means of the relevant outcome variable y_i across control and treated villages, i.e.:

$$ATE^t = \overline{y}^t - \overline{y}^c \tag{1}$$

where $\overline{y}^t = \frac{1}{n_c}\sum_{j=1}^{n_t} y_j$ and $\overline{y}^c = \frac{1}{n_c}\sum_{j=1}^{n_c} y_j$, and where n_t is the number of subjects in treated villages and n_c is the number of subjects in control villages. The same is true for assignment to treatment A (no incentives) or B (incentivized teachers), which is also randomized across villages.

Within treated villages, participating farmers are assigned to one of three roles: teacher; student; or non-student. Furthermore, some students are assigned to a teacher they nominated while others are not. There are therefore four possible assigned treatments $r = (1,2,3,4\}$. The process by which subjects are assigned to these different treatments is entirely under the control of the researchers and, just like in any sample stratification, combines a random element with a deterministic element based on observables. This implies two important properties of the role-specific sub-samples. First, there is no self-selection into treatment: assignment is entirely under the control of researchers based purely on observables. This means that we can ignore selection on unobservables.

Second, the probability of assignment to a particular treatment varies depending on observables, which is another way of saying that the selection of farmers from treated villages into a particular treatment is achieved using stratified sampling. Hence, obtaining the consistent mean for a particular treatment requires weighting each observation i by the inverse of the probability that the individual i was assigned to that treatment (Horvitz and Thompson 1952; Imbens and

Wooldridge 2009).[10] Formally, let $p_i^r(x_i)$ denote the probability that individual i with observable characteristics x_i is assigned to treatment r and let y_i denote an outcome of interest for individual i. Provided that $p_i^r(x_i) > 0$ for each i and each r, a consistent estimator of the mean of y_i for treatment r is given by:

$$\overline{y}^r = \frac{1}{n_r} \sum_{i=1}^{n_r} \frac{k}{p_i^r(x_i)} y_i \tag{2}$$

where n_r is the number of subjects assigned to treatment r and the sum in equation (2) is taken over all the subjects assigned to that treatment, and $k = \frac{1}{n_r} \sum_{i=1}^{n_r} p_i^r(x_i)$ is a normalization. Since farmers assigned to treated villages are directly comparable to controls, the average effect of being assigned to treatment r is simply given by:

$$ATE^r = \overline{y}^r - \overline{y}^c \tag{3}$$

where $\overline{y}^c = \frac{1}{n_c} \sum_{j=1}^{n_c} y_j$. We use an unweighted mean for $\overline{y}^c$ since there is no stratification of subjects into being controls – which is equivalent to saying that control farmers all have a sampling weight of 1. This immediately implies that obtaining a consistent estimate of ATE^r does *not* require observing x_i among control subjects.[11]

By the same reasoning, we can compare treatment effect across treatments r_1 and r_2 as the difference between two weighted sums:

$$ATE^{r_1} - ATE^{r_2} = \overline{y}^{r_1} - \overline{y}^{r_2} \tag{4}$$

It is important to remember that, here as in other experiments with possible peer effect/externalities within villages, each ATE^r measures the effect of being assigned to treatment r in an experiment in which other subjects in the same village are assigned to the other treatments in the same proportions as in our experiment. It does not measure the average effect of being assigned treatment r in general, or being assigned treatment r in another experiment with different assignment proportions (i.e., different saturation rates). The same observations applies to differences in ATE^r's. This limitation is common to all RCT's.

What makes our experimental design different from other stratified sampling cases is that the probabilities $p_i^r(x_i)$ are not set explicitly. Rather they are implied by the internal structure of an assignment algorithm that combines random elements with observables x_i. Dependence on observables x_i arises in several ways. First, in each village we select 4 teachers from farmers with number of nominations above the median number of nominations and 2 teachers from

[10] To illustrate, suppose that teachers below the nomination median adopt SRI with probability x and teachers above the median with probability $2x$. Further assume that, as in our data, control farmers do not adopt. The true ATE is $0.5x + 0.5 \times 2x = 1.5x$. In our sample, however, 4 of the teachers are above the median and 2 below. If we take the sample average of teachers, we get a estimated treatment effect of $\frac{2x+4\times 2x}{6} = 1.66x$, which is an over-estimate. However, if we reweigh the observations by their sampling probability (i.e., their probability of assignment to treatment), we get $\frac{2}{6}\frac{x}{0.33/0.5} + \frac{4}{6}\frac{2x}{0.66/0.5} = 1.5x$ QED.

[11] This is different from Propensity Score Matching (PSM) whereby control and treated observations are compared pairwise based on a propensity score calculated from observables. We do not need to use PSM in this case since we can obtain consistent estimates of the relevant means without it.

below the village median. This is to ensure we have enough nominated teachers to assign to nominating students. Second, we match 6 of the student farmers with a teacher they regard as role model/opinion leader, and the other 6 with a teacher they did not nominate as role model/opinion leader.

This is achieved by a sequential algorithm that starts by randomly sorting the 24 non-teacher-trainees and 6 teacher-trainees. The algorithm then sequentially picks, for each teacher, a farmer who nominated him. This is done by going through the randomly sorted list until one such farmer is found. When all nominating students have been selected in the manner, the algorithm then looks for non-nominating students. This is achieved in a similar manner: the algorithm again starts with the first teacher on the (randomly sorted) list of teacher-trainees, and looks through the 18 farmers remaining on the randomly sorted list for a non-nominating student for that farmer. The process is then repeated for the next teacher, and so on until all 6 teacher-trainees have been assigned a non-nominating student. Note that the pattern of nominations varies from village to village, depending on the ease with which, at each of its steps, the algorithm finds a farmer that meets the required criterion. Farmers not assigned to be a teacher or a student fall in the non-student category. Randomness comes from the arbitrary order in which individual farmers are considered at the different steps of the algorithm. It is therefore possible to obtain counterfactual assignments by reshuffling the order in which farmers are considered in the algorithm. By doing this a sufficient number of times, we can recover, for each subject, the true stratification probabilities $p_i^r(x_i)$ that are implicit in the algorithm.[12]

Formally, let s denote a particular replication of the assignment algorithm. In this replication, each individual i is assigned one of the four possible treatments – teacher, nominating student, non-nominating student, or non-student – in the exact proportions imposed by the experimental design. Let $q_i^r(s|x_i) = \{0, 1\}$ be an indicator function indicating whether subject i was assigned to treatment $r = \{1, 2, 3, 4\}$ in replication s. By construction, each subject can only be assigned one treatment in each replication, which implies:

$$q_i^1(s|x_i) + q_i^2(s|x_i) + q_i^3(s|x_i) + q_i^4(s|x_i) = 1 \tag{5}$$

A close approximation of the assignment probabilities can be recovered as:

$$p_i^r(x_i) = \frac{1}{n_s} \sum_{s=1}^{n_s} q_i^r(s|x_i) \tag{6}$$

for n_s large enough. Since each set of $q_i^r(s|x_i)$ sums to one for each subject, the $p_i^r(x_i)$'s also sum to one for each i. For this paper, the $p_i^r(x_i)$ were obtained using 300 counterfactual assignments of subjects in each treated village to the four treatment categories. Experimentation shows that, in our sample, the $p_i^r(x_i)$'s tend to converge rapidly to a stable value, such that 300 replications suffice.

[12] This approach is similar to that of Abebe et al. (2018), except that our setting is much simpler since we do not have to 'predict' nominations out of sample.

As noted above, a consistent estimate of the stratified sample mean can be recovered by inverse probability weighting (IPW) provided that $p_i^r(x_i) > 0$ for each i and each r – i.e. provided that each subjects has some probability of being assigned to either of the four possible treatments (i.e., common support). While it is difficult to ascertain that this condition is satisfied ex ante, it is easy to verify ex post. To this effect, we present in Figure 1 the frequency distribution of sampling weights $1/p_i^r(x_i)$ to all treatment categories for all farmers in treated villages. To facilitate understanding, sampling weights have been scaled by actual sample proportions. This means that a farmer who has a relative sampling weight of 1 for being a teacher has a probability of being a teacher equal to the proportion of teachers in the sample. A number larger than 1 means the farmer has a higher than average chance of being assigned the role of teacher-trainee, and vice versa when the number is smaller than 1.

In Figure 1, a value of $p_i^r(x_i)$ close to 0 for any role r would translate into a very large value of the sampling weight. As we see in Figure 1, there are no such cases: the highest inverse probability weight is less than 2.5. Similarly we find no inverse probability weight inferior to 0.5. From this we conclude that the assumption required for equation (**??**) are satisfied in our case. Finally we note that if individual-specific treatment effects are uncorrelated with $p_i^r(x_i)$, the $\overline{y}^r$ averages are similar whether we apply inverse probability weights or not. In this case, any approximation error that could possibly remain in formula (6) has no effect on estimates of average treatment effects. Hence to provide an extra layer of reassurance in our approach, we compare the results obtained with IPW to unweighted estimates, whenever relevant.

4 Testing strategy

We start by discussing average treatment effects on the main outcome variable of interest, which is SRI adoption. To measure the extent of adoption by each farmer, we rely on BRAC's Research and Evaluation Division (RED) staff visits to the fields of each farmer to gauge how closely they follow key precepts of the SRI approach. Information is collected on aspects of SRI technology adoption that can visually be assessed by BRAC expert staff, such as recommendations regarding: the age of the seedlings; the number of seedlings per bundle; and the spacing of the bundles. BRAC staffers also assess the proportion of cultivated land on which SRI practices are used, and the total number of SRI principles applied. BRAC enumerators provide a summary measure of SRI adoption that combines all the above. These different ways of measuring SRI adoption are correlated with each other, but not perfectly, so that they all capture valuable data variation that can be used to assess the effect of treatment on adoption. We are interested in finding a dominant pattern in the data.

We first estimate treatment effects of the four main categories of treated farmers, depending on their assigned role: teacher-trainees; students matched with a teacher-trainee they regard as role model (i.e., 'nominating student'); students not matched with one of their role models ('non-nominating student'); and non-students. These four groups of treated subjects are compared to farmers in control villages. Formally, we estimate:

$$y_{iv} = \alpha + \sum_{r=1}^{4} \beta_r T_{ivr} + u_{iv} \tag{7}$$

where y_{iv} is an outcome of interest for farmer i in village v, $r = \{1, 2, 3, 4\}$ denotes the four possible roles/treatment types as before, $T_{ivr} = 1$ if farmer i in village v is assigned to treatment r, and β_r is the ATE for treatment r. Note that, by construction, each treatment is mutually exclusive so that $T_{ivr} = 1$ for at most one treatment per farmer. In control villages, $T_{ivr} = 0$ for all i and r. To correct for stratified sampling of farmers in treated villages into the four possible treatments, we estimate regression (7) with inverse probability weights (IPW) $1/p_i^r(x_i)$, as explained in the previous section. Having estimated (7), we can then test for the pairwise equality of the different treatments, e.g., whether $\beta_r = \beta_l$ for $r \neq l$. Implementation issues and robustness checks are discussed in the empirical section.

We know from previous work (e.g., Latif et al. 2005) that, in Bangladesh the use of SRI is limited. This is also the case for our control farmers: very few apply any of the SRI recommended practices. Fafchamps et al. (2020) find that 37% of the randomly selected, unincentivized farmers who receive SRI training from BRAC adopt some of its practices. We therefore expect a similar adoption frequency among teacher-trainees if teaching SRI to others has no additional effect on its adoption – but a higher adoption rate if it does. Among students, we expect an average adoption rate equal or below the adoption rate of BRAC trainees – reasoning that farmers assigned the role of teacher-trainee cannot be as good at conveying SRI knowledge as professional BRAC trainers. Based on previous evidence, we also expect some adoption among non-students because SRI knowledge seems to circulate somewhat within treated villages. Finally, we expect more adoption among nominating students, that is, students assigned to a teacher they regard as a role model.

Next we compare students assigned to non-incentivized or incentivized teacher in treatments A and B. Incentivizing teacher-trainees is anticipated to increase their effort in transferring SRI knowledge and this, in turn, ought to lead to higher adoption among their students. To test whether incentivizing teacher-trainees increases the transfer of knowledge, we compare the performance of students on the quiz between the villages with incentivized and non-incentivized teacher-trainees:

$$q_{iv} = \alpha + \beta_m T_{ivm} + u_{iv} \tag{8}$$

where q_{iv} denotes the quiz performance of student i in village v, and $T_{ivm} = 1$ if student i is in a village v that was assigned to the incentivized treatment, denoted m. Regression (8) can only be estimated on students and teacher-trainees since the quiz was not administered to non-students in treated villages and to control farmers. Coefficient β_m hence capture the *additional* effect of incentivizing teacher-trainees on students' quiz performance. As before, we use inverse probability weights (IPW) to correct for stratified sampling of treated farmers into the student category. We also estimate a similar regression to check whether incentivizing teacher-trainees affect their own quiz performance and SRI adoption – in case being incentivized induces teacher-trainees to pay closer attention to SRI instruction and hence learn better. We conduct a similar analysis to compare the quiz performance of nominating and non-nominating students, and to compare their adoption rates.

We continue with a mediation analysis to investigate the likely channels of causation in our data. We focus on two channels of particular interest to policy makers: (1) is adoption mediated by performance on the quiz; and (2) is adoption mediated by teacher example. To investigate the first question for treatments A and B, we estimate an SRI adoption regression of the following form:

$$y_{iv} = \alpha + \beta_m T_{ivm} + \gamma q_{iv} + u_{iv} \tag{9}$$

where, as before, q_{iv} is the quiz score of student i and $T_{ivm} = 1$ if the teacher of student i was incentivized. If the effect of T_{ivm} on adoption y_{iv} is through better SRI knowledge, then including q_{iv} in the regression should soak up much of the effect of T_{ivm} on adoption. We estimate a similar regression to compare nominating and non-nominating students, in which case $T_{ivm} = 1$ if i is a nominating student. We also examine whether students' quiz performance is higher when their teacher performed well on the quiz. In all these regression, IPW is similarly applied – as it is to all regressions below.

To investigate question (2), we follow a similar procedure, replacing q_{iv} with the adoption of the farmer who taught student i, which we denote by y_{jiv}:

$$y_{iv} = \alpha + \beta_m T_{ivm} + \theta y_{jiv} + u_{iv} \tag{10}$$

If the effect of T_{ivm} on adoption y_{iv} is through teacher example, then including y_{jiv} in the regression should reduce the coefficient of T_{ivm} in regression (10). By construction, regressions (9) and (10) only use observations on students in treated villages – only student farmers have a teacher, and quiz data does not exist for non-students and control farmers.

The last part of the analysis compares SRI adoption rates in the P2P experiment with those in the earlier SRI training experiment on trainee referral. The object is to identify the *additional* effect of asking trainees to teach other farmers. To this effect we compare various indicators of SRI adoption between the P2P teacher-trainees and randomly selected trainees in the referral experiment. These trainees received the same BRAC SRI training session as in the P2P experiment, but they were not assigned to teach SRI to two other farmers. To the extent that the two sets of farmers are comparable to each other, the difference in adoption between them can be interpreted as the additional effect of asking trainees to train others. Formally we estimate a regression on the pooled data of the form:

$$y_{iv} = \alpha + \beta_p T_{ivp} + u_{iv} \text{ for } i \in S_p \tag{11}$$

where T_{ivp} is a dummy equal to 1 if farmer i in village v belongs to the P2P experiment, and 0 otherwise. We estimate regression (11) on different comparison sets S_p. Our main comparison of interest is between teacher-trainees and randomly selected (i.e., batch 1) trainees in the referral experiment. In that regression, β_p identifies the additional treatment effect of being a teacher. We also compare student farmers in the P2P experiment to batch 2 trainees in the referral experiment. Since student farmers did not receive SRI training directly from BRAC, we expect them to be less effective adopters of the new technology than farmers trained by BRAC. We choose batch 2 trainees in the referral experiment as the most appropriate comparison because, like P2P student farmers, SRI is recommended to them by a previously trained farmer. The two sets, however, are not completely comparable in terms of sampling methodology, so we regard this comparison as indicative only. We also compare farmers in treated villages who did not receive any direct or indirect (i.e., from teacher-trainees) training on SRI practices. This serves to investigate whether the P2P experiment generates smaller diffusion effects, given that it directly trains a much smaller proportion of sample farmers in treated villages (i.e., 6 out of 30 compared to 18 out of 30 on average in the referral sample). Finally we compare SRI adoption rates among control farmers in both experiments at endline. The purpose of this regression is to reassure the reader that any difference in adoption rates between the two samples is not due to some extraneous factor differentially affecting SRI adoption in the P2P study region.

5 The data

5.1 Balance Checks: Baseline

We check that our different experimental samples are properly balanced. In the first part of Table A1 we start by comparing farmers in control and treated villages. We report the mean of the control farmers and the average difference with farmers in treated villages together with the standard error of that difference. To ensure comparability with ATE estimation results, all reported estimates are obtained by regressing the variable of interest on a dummy for treatment, using IPW and clustering standard errors at the village level.

We find no significant difference, suggesting balance between control and treated villages in terms of age, education, and all key baseline agricultural indicators. Virtually identical results are obtained without sampling weights[13], indicating that sampling weights – and thus assignment to various treatments within treated villages – are not correlated with observable baseline characteristics. The second panel of Table A1 compares farmers in treatment A and B villages – that is, without and with incentivized teacher-trainees. Here too we find no evidence that farmers in the two categories of treated villages differ at baseline.

In Table A2 we look for balance in baseline characteristics between farmers within treated villages, depending on which treated category they are assigned to. In all cases we correct for sampling weights and cluster standard errors by village. We start by comparing teacher-trainees with other farmers. We see that teacher-trainees are in general slightly older and better educated than non-teacher-trainees, and they cultivate more land at baseline. These differences, however, are small in magnitude and not statistically significant. We similarly find no statistical

[13] See Appendix Table A1b.

differences in baseline characteristics between students and non-students. When we compare nominating to non-nominating students, we again find that none of the differences in baseline characteristics are statistically significant. Similar results are obtained if we do not correct for sampling weights.[14] From this analysis, we conclude that we have satisfactory balance across our different treatment categories.

From Fafchamps et al. (2020), we already know that the SRI training referral sample is balanced between treatment and control.[15] In Table A3 we verify that farmers in the P2P experiment are comparable to those in the SRI training referral experiment. We find that, even though the two studies are not conducted in the same districts, the differences between farmers in the two samples are very small in magnitude and never statistically significant. This is true not only for the sample as a whole, but also for each of the comparison sets that we examine in regression (11). This provides reassurance that farmers in the two samples are comparable along many dimensions.

5.2 SRI Knowledge, Adoption and Agricultural Performance

We present in Table 1 summary statistics for all the variables used in the analysis. The first panel presents our main outcome variables of interest. The first two variables measure the performance of student and teacher farmers on an SRI knowledge quiz administered by BRAC. The quiz is based on the training materials and is divided into two parts. Part A has 8 questions on the basic principles of SRI necessary in order to adopt SRI. Part B contains an additional 12 true-or-false questions covering a range of topics relevant to SRI, but not directly necessary to adopt it. There is more usable variation in answers to Part A, which is why we focus on those questions in our analysis. We also construct a dummy variable equal to 1 if the subject responds correctly to the three main questions on SRI principles. We see that, as could be anticipated since only teacher-trainees receive SRI training directly from BRAC, teacher-trainees perform better than students on the quiz. The difference in performance is significant at the 1% level.[16]

Next we present summary statistics on nominations made by fellow farmers. Farmers could nominate up to five other farmers in our sample. On average they nominated 4.9 farmers. By construction, the average number of nomination received equals that of nominations made. But nominations received are distributed much more unequally: the standard deviation of nominations made is 0.31 while that of nomination received is 3.85. The minimum of nominations received is 0 and the maximum is 26, compared to 2 and 5 for nominations made. Nomination data was not collected in control villages.

We rely on a set of six related measures to capture SRI adoption. The first measure is a dummy variable equal to 1 if the farmer has adopted at least three of the six major SRI

[14]See Appendix Table A2b. We also find no evidence of lack of balance if we use instead the list of balance variables used in Fafchamps et al. (2019) – see Appendix Tables A2c and A2d.

[15]See online appendix Table A3.

[16]On average teachers answer 7.5 questions correctly, while students were able to answer 6.7 questions out of 8 questions. Below we discussed in more details about the quiz performance of students and teachers, and how that correlate with adoption of SRI

principles on at least one of their plots. It is based on an assessment conducted in person by a BRAC extension agent visiting up to 3 plots of land for each farmer. The second measure of adoption that we use is the proportion of land on which SRI practices are adopted. The next variable captures the number of SRI principles the farmer has adopted, on a scale of 0 to 5.[17] The last three measures are dummies that focus on an individual practice: does the farmer follow the SRI-recommended age of seedlings at the time of transplanting; the number of seedlings per bundle; and the spacing between bundles. SRI recommends transplanting earlier and putting less seedlings per bundle while spacing the bundles more widely. These three simple practices have been shown to increase rice yields in many countries including in Bangladesh (e.g., Stoop et al. 2002, Karmakar et al. 2004, Moser and Barrett 2006, Takahashi and Barrett 2014, Fafchamps et al. 2020).

We immediately note a much higher adoption of SRI in treated villages, a point that is the focus of our remaining analysis. We do, however, also observe some SRI adoption in control villages. This can arise either because some control farmers, by chance, follow practices that are observationally similar to SRI. Alternatively, some control farmers may hear about SRI from farmers in treated villages – e.g., farmers who know each other through intermarriage.

In the rest of the Table we report endline values for agricultural performance. Yields are calculated in Kg per decimal, where a decimal is a Bangladeshi unit of land area equal to 1/100th of an acre. Output value is given in Bangladeshi Taka per decimal. The same applies for input costs, labor costs, total costs, and profits – which are equal to output value minus total costs. We note that treated villages have higher yields and profits, an observation we revisit below.

6 Econometric results

6.1 Treatment and adoption

We start by reporting coefficient estimates for equation (7) in Table 2. The unit of observation is a plot – with up to three plots per farmer[18]. Standard errors are clustered at the village level, which also controls for the fact that plot-level observations for the same farmer are highly correlated. As discussed earlier, the reported results include sampling weights correcting for variation in treatment assignment probabilities across farmers in treated villages.[19]

Estimates are reported for six different measures of adoption. The first one, presented in

[17]We did not consider the sixth principle here -mechanical weeding- as BRAC staffs could not verify this among all farmers considering the weeding was done at different times than the field visits in many places. In other cases, we use farmers' self-reported measure (whether they used mechanical weeding or not) during post-harvest period.

[18]If there are more than three plots we randomly selected three plots to obtain plot-level information.

[19]If we estimate the regression with propensity scores as additional regression, results are, unsurprisingly, virtually identical. We also estimate the regression without sampling weights or propensity score, to investigate whether propensity scores may be correlated with treatment effects. We find very little difference between the two sets of estimates, suggesting that, in our data, variation in sampling weights/propensity scores is not heavily correlated with treatment effects. Finally, we experimented with nearest neighbor matching using baseline characteristics for control and treated farmers. Results are again very similar, which is not surprising given that correcting for sampling weights or propensity scores has little effect on results relative to OLS.

column 1, is a dummy equal to 1 if the BRAC staff member who physically inspected each farmer's fields reports that the farmer has adopted at least three of the six major principles of SRI on (at least) one of their plots, and 0 otherwise. Because the dependent variable has been multiplied by 100, coefficient estimates can be read as changes in percentage points. Adoption among control farmers is close to 0. The second measure of adoption is the proportion of land under SRI cultivation. It similarly varies between 0 and 100%. In column (3) the dependent variable is the number of SRI principles adopted by the farmer. This number varies between 0 (none) and 5 (all). Most farmers adopt partially only. The last three columns of Table 3 focus on specific SRI practices, namely: the age of seedlings in days (SRI recommends transplanting rice seedling earlier than what farmers customarily do); the number of seedlings per bundle (SRI recommends that a fewer number of rice plants be transplanted in the same bundle); and the distance between bundles (SRI recommends a greater distance between bundles).

The coefficient estimates reported in Table 2 indicate that receiving the SRI intervention does affect the practices of all four categories of farmers in treated villages relative to farmers in control villages. With a couple of exceptions (age and number of seedlings for non-students), all point estimates are strongly significant and consistent across adoption measures. We also note that teacher-trainees adopt more than students; students adopt more than non-students; and non-students adopt more than control farmers; and these differences are also extremely consistent across adoption measures. We nonetheless find that, contrary to expectations, nominating students are, if anything, less likely to adopt than non-nominating students. We revisit this latter point below.

To see whether these differences across categories of farmers in treated villages are statistically significant, we conduct pairwise t-tests on the coefficients estimated in Table 2. Results are summarized in Table 3. We see that differences across treatment categories are in general statistically significant. The main exception is the difference between nominating and non-nominating students, which is mostly negative but non-significant – the only exception is the SRI adoption dummy, which is significantly different at the 8% level.[20]

Next, we compare the effect of the different treatments on farmer welfare, measured by agricultural performance. Results are shown in Table 4. To make sure our comparison does not omit important differences in by-products such as straw and husk, the value of crop output includes the imputed value of these by-products as well. Since SRI is believed to increase the crop management labor provided by the farmer, the labor costs include the imputed value of all family labor.

We see that teacher-trainees have statistically higher crop output and agricultural profits than controls. We also find that student farmers have significantly higher yields than control

[20]As additional robustness check, we reestimate Table 2 with augmented inverse probability matching based on baseline production per decimal of land, age of household head, years of education of household head, and cultivable land in decimals. The results are presented in Appendix Table A4. Additional matching on observables between control and treated farmers should make no difference since we already correct for stratification probability weights used in the treated villages. This is indeed what we find: point estimates in Tables 2 and A4 are close if not identical, and they are significant in the same way.

farmers, and enjoy agricultural profits that are 13 to 14 percentage points higher. All input costs, on the other hand, tend to be lower for teacher-trainees and students than for control farmers, although the difference is statistically significant only in one case. This confirms that adoption of SRI is beneficial for the average teacher or student farmer. Although estimated treatment effects tend to be larger for teacher-trainees than students, the second panel of Table 4 shows that none of these differences is statistically significant. There is also no significant treatment difference between nominating and non-nominating students.

6.2 Comparing different categories of students

We now compare the quiz performance of students falling in different treatment categories. Results are shown in Table 5. Performance of the farmer in the quiz is measured on a scale from 0 to 8. We also report in column 2 a dummy, which is equal to 1 if the farmer answers correctly all the three main questions that are most relevant for SRI practices.

We see that students of incentivized teacher-trainees do significantly better on the quiz – which is about 40% of the difference between the score of teacher-trainees (who were trained directly by BRAC) and students in the unincentivized treatment. This difference is confirmed in column 2, which focuses on the proportion of farmers who answer correctly to the questions most closely associated with SRI practices: incentivizing teacher-trainees increases the proportion of such farmers by 11 percentage points – which is equivalent to 65% of the difference between teacher-trainees and students in treatment A. Put differently, incentivizing teacher-trainees closes a significant fraction of the knowledge gap between student farmers – who did not receive BRAC training – and teacher-trainees – who did.

Turning to SRI adoption, the impact of incentivization is much less remarkable. In fact, as shown in Table 6, we find no effect of incentivizing teacher-trainees on students' SRI adoption. The Table also reports adoption by teacher-trainees – who may have responded to the incentive themselves. If anything, we find a fall in adoption rate among teacher-trainees in treatment B – but this difference is not statistically significant.

Next we turn to the quiz performance of nominating and non-nominating students. Results are presented in Table 7. They show no significant effect of being matched with a role model on quiz performance: point estimates are positive but not large enough to be significant. Turning to SRI adoption, we report in Table 8 the results from a regression analysis including only students. As before, standard errors are clustered at the village level and sampling weights are used in the estimation. Results confirm what we already reported from Table 3: if anything, students are less likely to adopt if matched with a teacher that they nominated as their role model or opinion leader. This difference, however, is only significant in the regression for the overall SRI adoption variable. None of the other adoption regressions returns a significant coefficient.

From this evidence we conclude that incentivizing teacher-trainees has a positive effect on how much SRI knowledge is conveyed to them. But, contrary to expectations, it does not affect adoption. We also find that, contrary to what we expected, being matched with a role model does not increase knowledge transmission and, if anything, it reduces adoption. Perhaps, as in the old adage, people should never meet their heroes.

6.3 Mechanisms: Mediation analysis

The last empirical section of this paper seeks to identify two possible channels by which teacher-trainees affect the SRI adoption of their students: knowledge transmission and example. To investigate the first alternative, we start by documenting whether students whose teacher does well on the test perform better on the test, and whether this correlation is responsible for the effect of incentivized teacher-trainees reported in Table 5. Results are presented in Table 9. We see that this is indeed the case: the coefficient of teacher performance is positive and significant whether we use the test score or the good performance dummy as dependent variables. We also observe that the coefficient of the incentivized teacher treatment remains basically unaffected. This suggests that the effect of incentivizing the teacher does not operate by inducing the teacher to pay more attention during training – and consequently to perform better on the quiz.

Next, we reestimate regression model (9). Results are shown in Table 10. The top panel shows the estimated treatment effect of incentivizing the teacher on different measures of SRI adoption, controlling for the farmer's quiz score. Performance on the quiz does predict some of the variation in adoption – they are more likely to follow the SRI principles related to age of seedlings at transplanting and larger distance between seedlings. But including it as a control does not change our earlier result from Table 6: incentivizing the teacher continues to have no effect on any of our measures of SRI adoption.

We repeat the same analysis in the bottom panel of Table 10 with being a nominating student as treatment variable. To recall, we previously found in Table 8 that this treatment is associated with a 10% significant fall in our first SRI adoption measure, and with negative but non-significant effects on other adoption measures. This pattern is unaffected by the inclusion of the farmer's quiz score – in fact, estimated coefficients hardly change at all. We repeat the same analysis using a dummy equal to 1 if the farmer answers the three main SRI questions correctly at the quiz. Results, shown in appendix Table A5, are similar to those in Table 10, except that the dummy has a slightly stronger predictive effect on adoption measures. From this we conclude that quiz performance is associated with higher adoption of some SRI-recommended practices, but transmission of knowledge does not seem to be the channel through which adoption is affected by being assigned an incentivized teacher or a role model teacher.

In Table 11 we repeat the same analysis focusing on the adoption behavior of the teacher as additional control. Results indicate that adoption behavior by the teacher is significantly associated with all but one measure of SRI adoption by their student. The addition of this variable does not, however, change any of our earlier results on students being matched with an incentivized or role model teacher.

From this we conclude that SRI knowledge of the student and SRI adoption by the teacher are both predictors of some dimensions of SRI adoption. But controlling for these channels of influence does not affect our lack of positive effects on adoption for teaching incentives and for teacher-trainees as role models.

6.4 Comparison to the SRI training referral experiment

We end the empirical analysis with a comparison between farmers in the P2P study and the earlier SRI training experiment. Results from the estimation of regression (11) are presented in Table 12. The first panel is the one which is the most comparable in the sense that the only substantive difference in treatment is the fact that P2P farmers teach two other farmers. Point estimates indicate a large additional treatment effect from being assigned two farmers as students: P2P teacher-trainees are 34 percentage points more likely to adopt SRI than randomly selected trainees in the first batch of the SRI training referral experiment. This is equivalent to a doubling of the treatment effect of training on adoption. Similar if not larger effects are found for all other indicators of SRI adoption: teacher-trainees allocate more land to SRI, adopt more practices, and follow the three most critical practices more consistently. This suggests that being asked to teach other farmers has a large additional treatment effect on adoption.

The next two panels of Table 12 check that this increased adoption among teacher-students is not achieved at the cost of lower adoption among other farmers in treated villages. The second panel shows that, relative to batch 2 trainees in the referral experiment, P2P student farmers are less likely to satisfy the criteria to be considered an SRI adopter (first column of Table 12). But for all the other indicators of adoption, they register a larger coefficient. This is remarkable given that, in the referral experiment, batch 2 trainees are (weakly) selected to be more interested in SRI than the average farmer in the sample. Given that student farmers receive no direct instruction from BRAC, it is quite remarkable how they by and large beat batch 2 trainees. From panel 3 we also find no evidence that diffusion is less effective in P2P villages: if anything, we observe significantly more adoption among untreated farmers in P2P treated villages. This suggests that, if anything, P2P helps diffuse SRI more effectively outside specifically targeted farmers.

The last panel of Table 12 shows that, by endline, P2P control farmers are also more likely to adopt than controls in the original experiment. While the differences are statistically significant in all but one case, SRI adoption nonetheless remains low among controls in both samples: 0.5% adopters in the original sample compared to 2.3% adopters in P2P. This compares to much larger P2P treatment estimates in all three panels of Table 12, ruling out the possibility that larger treatment effects are simply due to a systematic difference in adoption rates not due to treatment. Why P2P controls witness slightly more adoption is unclear, we cannot rule out the possibility of contamination, that is, SRI diffusing to control villages through word-of-mouth. This would be consistent with the observation that P2P generated a lot more interest in SRI among all farmers in treated villages, including those not directly targeted by the intervention.

For robustness purposes, we reestimated the same regressions by restricting the data to one plot per farmer. The objective of this check is to ensure that our findings are not affected by differences in the number of plots per farmer across the two experimental populations. Results, reported in Appendix Table A6, are very similar in terms of magnitude and significance.

7 Cost-benefit analysis

Before concluding, we estimate the costs and benefits of the P2P intervention, and compare it to the SRI training referral intervention discussed above. Detailed calculations are provided in Appendix Table A7. Benefits are obtained by taking, for each treatment category, the increased profit per decimal reported in Table 4 and multiplying it by the number of decimals cultivated by all the treated farmers. Summing over all treated farmers gives the total additional profit generated in the year of the experiment. Costs are calculated from administrative data obtained from BRAC (for training) and from our own field costs including incentives and participation fees paid to farmers.

The calculation yields a social return of \$2.56 for each \$1 invested in the experiment. For comparison purposes, we perform the same calculation for the SRI training referral experiment. We get a higher figure of \$7.3 of social return for each \$1 spent on the experiment. This figure is higher in spite of lower cost per farmer because the estimated effect on profit per decimal is higher in the SRI training referral experiment than in the P2P experiment.

These are high returns, especially if we consider that they are likely to extend over more than one year, something we have ignored in our calculations. We do, however, acknowledge that these figures omit personnel and administrative costs that would have to be incurred to scale up the intervention. Scaling up may also lead to a dilution of administrative competence and focus that could reduce the effect of the treatments.

8 Conclusion

We run a randomized controlled experiment in which farmers who receive SRI agricultural training are invited to teach what they have learned to two other farmers selected by us. We experimentally vary these two student farmers such that one farmer nominated the teacher and the other did not.

We find that villages exposed to the BRAC extension effort do experience significant – if partial – adoption of cultivation practices recommended under SRI. Compared to earlier findings by Fafchamps et al. (2020), teacher-trainees are 34 percentage points (i.e., twice) more likely to adopt SRI than farmers who simply receive SRI training from BRAC. This suggests that being invited to teach SRI to others increases trainees' interest in the new practices. Adoption by student farmers – who do not receive any BRAC training – is only 8 percentage points lower than adoption by trainees in the Fafchamps et al. (2020) experiment. This is a remarkable success rate for what is, after all, a cheaper way of dispensing knowledge. We also find a non-negligible amount of diffusion to non-students, with 9 percent more adoption than untreated farmers in treated villages from the SRI training referral experiment. Taken together, this evidence suggests that SRI adoption is increased considerably by making trainees teach two other farmers explicitly assigned to them. Why this is the case is unclear, but turning agricultural extension into a social event may induce trainees to become invested in the new technology they have to teach. If true, this conclusion is not too dissimilar from the 'put your money where your mouth is' effect discussed in Fafchamps et al. (2020).

To investigate whether incentives can improve the transmission of agricultural knowledge and practices, half of the teacher-trainees are offered a fee conditional on the performance of their students at a quiz on SRI knowledge. We find evidence that incentivization is associated with more transmission of knowledge. But it has no effect on adoption. From this we conclude that incentivizing teacher-trainees on knowledge transmission does not significantly improve adoption in our case.

To investigate whether teacher-trainees better transmit SRI knowledge and practices to students who are socially proximate, participating farmers are asked to nominate another farmer whom they regard as role model. We then assign half of the students to a teacher they nominated, while the other half are taught by a teacher they did not nominate. We find no evidence that matching students with a teacher they look up to improves either transmission of knowledge or SRI adoption: if anything, nominating students adopt SRI less than students matched with a teacher they did not nominate. From this we conclude that matching teacher-trainees with people who nominated them does not improve dissemination and may even hurt adoption.

We perform a mediation analysis to identify likely channels of influence in the adoption decision. We first ask whether adoption is correlated with quiz performance, which would suggest that formal knowledge of the technology is important. We find that the SRI knowledge of the teacher is correlated with that of their student, consistent with the transmission of knowledge between them (e.g., Oster and Thornton 2012). Results also show that SRI knowledge as assessed in a formal test predicts the adoption of some SRI practices. This suggests that grasping the new practices at an academic level helps inducing adoption.

Finally, we examine whether adoption is correlated with how closely the teacher farmer applies the new practices, as would be the case if teaching by example increases adoption. We find that, for five of our six measures of SRI adoption, the adoption by teacher-trainees helps predict adoption by their students, suggesting that students follow the example of their teacher.

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Online Appendix: The SRI training referral experiment

[The following description is borrowed largely from Fafchamps et al. (2020).]

The SRI training referral experiment is organized around a training program introducing farmers to SRI (System of Rice Intensification). The objective of the experimental design is to improve the targeting of the training 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). Both B1 and B2 farmers are invited in person through a home visit by a field staff appointed by BRAC.

The experiment was conducted in collaboration with BRAC. The day-long SRI training follows the curriculum defined by BRAC and was administered by specially trained BRAC staff.[21] 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.[22] 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.[23] From these lists we randomly drew approximately 30-35 farmers in each village.[24] 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

[21] 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).

[22] 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.

[23] 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.

[24] 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.

have learned about SRI during training.

The first batch of B1 farmers is randomly selected from the list and invited for SRI training.[25] 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.[26] Unselected farmers are left untreated. The total number of trainees by village varies between 10 and 30.

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 subsequently adopts SRI practices.[27] 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

[25] 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).

[26] 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.

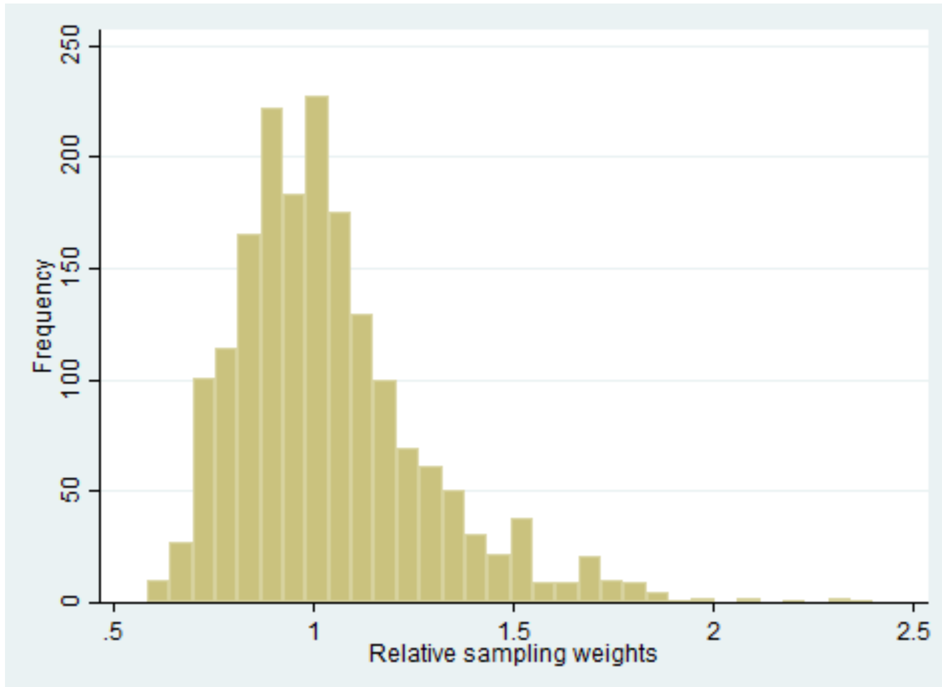
[27] The compensation level for T2 and T3 was chosen so as to be similar in expected value, based on on a 50% SRI adoption rate. B1 farmers were only informed of the nature of the referral compensation they would receive after the training had ended and when they were asked to refer a B2 farmer. No B1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

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.

We 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 on any of his plots.[28]

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

Figure 1. Simulated relative sampling weights of farmers in treated villages



Note: Sampling weights are obtained by rerunning the selection algorithm for each village 500 times and using the simulated frequency of assignment of each farmer as approximation for their sampling probability. To faciliate interpretation, sampling weights have been scaled by actual sample proportions. This means that a farmer who has a relative sampling weight of 1 for being a teacher has a probability of being a teacher equal to the sample proportion of teachers (which is 20% by design). Figure 1 shows the frequency distribution of sampling weights to all treatment categories for all farmers in treated villages. By construction, all control farmers (not shown here) have a sampling weight of 1.

Table 1. Descriptive statistics on key variables

	Sample	Observation	Unit	Control villages		Treated villages		Treated=Control p-value
				Mean	Std.dev.	Mean	Std.dev.	
Performance on the SRI knowledge quiz								
Score on a scale of 0 to 8	students+teachers	farmer	scale 1-8	n.a.		6.94	1.24	n.a.
Dummy=1 if answers the 3 main questions correctly	students+teachers	farmer	0-1	n.a.		88.0%	0.32	n.a.
Nominations by fellow farmers								
Number of nominations made	treated villages	farmer	number	n.a.		4.92	0.31	n.a.
Number of nomination received	treated villages	farmer	number	n.a.		4.92	3.85	n.a.
Measures of SRI adoption (All farmers within the village)								
SRI adopted by farmer on at least one plot, as evaluated by BRAC enumerator	all	plot	0-1	2.5%		33.2%		0.00
Proportion of land under SRI	all	plot	0-1	3.4%	16.41	27.5%	39.57	0.00
Number of SRI principles adopted on plot	all	plot	0-5	1.41	0.83	2.01	1.17	0.00
Dummy=1 if follows the SRI-recommended age of seedlings	all	plot	0-1	1.3%		5.0%		0.00
Dummy=1 if follows the SRI-recommended number of seedlings per bundle	all	plot	0-1	17.7%		32.5%		0.00
Dummy=1 if follows the SRI-recommended distance between bundles	all	plot	0-1	3.7%		20.6%		0.00
Agricultural performance at endline (All farmers within the village)								
Yield	all	plot	Kg/decimal	21.9	5.74	22.9	5.70	0.00
Value of crop output	all	plot	BDT/decimal	735.7	195.26	767.5	193.78	0.00
Input costs	all	plot	BDT/decimal	146.2	35.94	141.8	30.47	0.00
Labor costs	all	plot	BDT/decimal	156.3	127.22	157.5	140.16	0.73
Total costs	all	plot	BDT/decimal	302.5	132.43	299.4	144.75	0.39
Profit	all	plot	BDT/decimal	433.1	208.45	468.1	203.49	0.00

Source: The data on quiz performance comes from administrative data collected by BRAC training staff using the standard quiz administered at the end of SRI training sessions. Adoption data comes from field observations made by BRAC extension agents associated with the research project. SRI adoption variables are based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots. The six key principles of SRI 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 spacing (25 x 20 cm); reduced use of synthetic chemical fertilizers; intermittent irrigation; and complementary weed and pest control. The first SRI adoption variable equals 1 if at least 3 of the 6 main SRI recommendations are adopted by the farmer on any plot of land. The second SRI adoption variable equals the proportion of the farmer's plots on which at least 3 of the 6 main SRI recommendations are adopted. The third SRI adoption variable is the number of SRI principles adopted on the plot; this number ranges from 0 to 5 because one of the six SRI recommendations (using organic fertilizer) is not observed at the plot level. The last three adoption variables are dummies equal to 1 if the SRI-recommended value for a particular practice is applied on the plot. All adoption variables -- except the number of adopted SRI principles -- are expressed in percentages. BDT stands for Bangladeshi Taka, the national currency. 100BDT is worth approximately 1.2 USD. A decimal is a Bangladeshi unit of land area equal to 1/100 acre (40.46 square meters). To obtain USD values per acre, divide the reported values by 1.2. Reported p-values in the last column are for a pairwise test of equality of means between control and treated observations.

Table 2. Adoption by Treatment Status

Dependent variable is:	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI's age of seedlings	Follows SRI's number of seedlings/bundle	Follows SRI's distance between bundles
Treatments:						
Teacher-trainee	70.15***	44.98***	1.32***	9.39***	39.62***	43.54***
	(3.22)	(3.22)	(0.13)	(2.17)	(4.21)	(3.77)
Nominating student	27.21***	25.75***	0.63***	3.28**	14.89***	16.45***
	(3.53)	(3.43)	(0.12)	(1.47)	(3.89)	(3.08)
Non-nominating student	33.33***	26.88***	0.66***	4.07**	18.68***	17.04***
	(3.45)	(3.13)	(0.12)	(1.61)	(4.07)	(3.24)
Non-student	12.45***	12.31***	0.30***	0.42	5.56	6.22***
	(2.12)	(2.29)	(0.10)	(0.86)	(3.57)	(1.73)
Control mean	2.51%	3.38%	1.41	1.31%	17.68%	3.73%
Number of observations	7,230	7,659	6,789	6,789	6,789	6,789

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least 3 of the 6 main SRI recommendations are adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot; this number ranges from 0 to 5 because one of the six SRI recommendations (using organic fertilizer) is not observed at the plot level. The last three adoption variables are dummies equal to 100 if the SRI-recommendation for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. All regressions correct for sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table 3. Testing pairwise equality of coefficients in Table 2

	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI age of seedlings	Follows SRI number of seedlings/bundle	Follows SRI distance between bundles
Difference between:						
Teachers and nominating students	42.94	19.23	0.69	6.11	24.73	27.09
p-value	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Teachers and non-nominating students	36.82	18.1	0.66	5.32	20.94	26.5
p-value	*0.00*	*0.00*	*0.00*	*0.01*	*0.00*	*0.00*
Teachers and non-students	57.70	32.67	1.02	8.97	34.06	37.32
p-value	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Nominating students and non-students	14.76	13.44	0.33	2.86	9.33	10.23
p-value	*0.00*	*0.00*	*0.00*	*0.02*	*0.00*	*0.00*
Non-nominating students and non-students	20.88	14.57	0.36	3.65	13.12	10.82
p-value	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Nominating and non-nominating students	-6.12	-1.13	-0.03	-0.79	-3.79	-0.59
p-value	*0.08*	*0.65*	*0.71*	*0.62*	*0.16*	*0.85*
Number of observations	7,230	7,659	6,789	6,789	6,789	6,789

The values reported in the table are pairwise t-tests for equality of coefficients in Table 2. To recall, SRI adoption variables are based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least three of the 6 main SRI recommendations is adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot, which varies from 0 to 5. The last three adoption variables are dummies equal to 100 if the SRI-recommended value for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors were clustered by village, which also corrected for likely correlation across plots within farm. All regressions include sampling weights.

Table 4. Agricultural performance by treatment status

Dependent variable (in log):	Yield	Value of crop output	Input costs	Labor costs	Total costs	Profits
Treatments:						
Teacher-trainee	0.07***	0.07**	-0.03	0.02	0.00	0.14***
	(0.02)	(0.03)	(0.02)	(0.06)	(0.04)	(0.05)
Nominating student	0.04*	0.04	-0.05**	0.01	-0.00	0.09*
	(0.02)	(0.03)	(0.02)	(0.06)	(0.04)	(0.05)
Non-nominating student	0.05*	0.04	-0.03	-0.01	-0.02	0.14***
	(0.02)	(0.03)	(0.02)	(0.06)	(0.04)	(0.05)
Non-student	0.02	0.02	-0.03	0.03	0.01	0.02
	(0.02)	(0.03)	(0.02)	(0.06)	(0.04)	(0.05)
Control mean (in log)	2.62	6.03	3.74	4.60	4.44	5.77
Number of observations	5,831	5,831	5,831	5,540	5,540	4,653
p-value of pairwise coefficient comparisons between treatments:						
Teacher vs Nominating student	0.12	0.12	0.31	0.78	0.70	0.25
Teacher vs Non-nominating student	0.21	0.24	0.88	0.41	0.45	1.00
Nominating vs non-nominating student	0.79	0.72	0.48	0.45	0.55	0.25

The unit of observation is a plot. Yield is total sellable product per decimal of land (in kg) after adjusting for wastage due to floods, drought and diseases. The value of crop output includes the total sale revenue at the mean of farmer-reported prices at the district level, in Bangladeshi taka (BDT) per decimal of land, pluse the imputed revenue from grain, straw and husk evaluated at district level prices. Input cost (in BDT) includes all purchased factors: seed, fertilizer (both organic and chemical), irrigation (including fuel and electricity but not water), ploughing and tractor services, and pesticide and weedicide, all per decimal of land. Labor cost includes the wage cost per decimal of land for both hired and contract labor as well as the imputed cost of family labor evaluated at the mean of district-level reported wage rates. Total costs are the sum of input and labor costs. Profits is equal to the value of crop output minus total costs. All dependent variables are in log, which means that coefficient estimates can all be interpreted as percentage changes. All comparisons are relative to farmers in control villages. Standard errors are reported in parentheses. All standard errors are clustered by village, which also corrects for likely correlation across plots within farm. All regressions include sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level. All p-values reported in the second panel of the Table are the result of pairwise coefficient comparison tests between different types of treatment.

Table 5. Quiz performance for treatments A and B among students and teacher-trainees

	Score on a scale of 0 to 8	Dummy=1 if answers the 3 main questions correctly
Comparing students in treatments A and B		
Treatment B dummy (incentivized teacher-trainee)	0.37*	0.11**
	(0.210)	(0.050)
Mean for treatment A students	6.48	0.79
Number of observations (students)	710	710
Comparing teachers in treatments A and B		
Treatment B dummy (incentivized teacher-trainee)	0.13	0.00
	(0.120)	(0.030)
Mean for treatment A teacher-trainees	7.39	0.96
Number of observations (teacher-trainees)	356	356

Quiz performance based on BRAC administrative data. All regressions include sampling weights. Standard errors clustered at the village level are presented in in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Impact of teacher incentivization on SRI adoption among students and teacher-trainees

	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI age of seedlings	Follows SRI number of seedlings/bundle	Follows SRI distance between bundles
Comparing students in treatments A and B						
Treatment B dummy (incentivized teacher)	0.35	3.6	0.1	1.77	0.63	8.07
	(6.050)	(5.920)	(0.180)	(2.580)	(5.920)	(5.310)
Mean for treatment A students	32.83	28.2	2.01	4.03	34.95	17.17
Number of observations (students)	1,758	1,858	1,657	1,657	1,657	1,657
Comparing teachers in treatments A and B						
Treatment B dummy (incentivized teacher)	-4.45	1.58	0.07	-0.39	1.55	3.20
	(6.35)	(6.31)	(0.22)	(4.30)	(7.07)	(7.43)
Mean for treatment A teachers	74.7	47.6	2.67	10.14	55.89	45.7
Number of observations (teachers)	896	944	837	837	837	837

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least three of the 6 main SRI recommendations is adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot, which varies from 0 to 5. The last three adoption variables are dummies equal to 100 if the SRI-recommended value for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. All regressions include sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table 7. Performance at the quiz if student is matched with role model		
	Score on a scale of 0 to 8	Dummy=1 if answers the 3 main questions correctly
Comparing students by nomination status		
Nominating student dummy (matched with role model)	0.12	0.02
	(0.080)	(0.020)
Mean for non-nominating students	6.61	0.84
Number of observations	710	710

Quiz performance based on BRAC administrative data. All regressions include sampling weights. Standard errors clustered at the village level are presented in in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Impact of teacher-trainee nomination on SRI adoption						
	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI age of seedlings	Follows SRI number of seedlings/bundle	Follows SRI distance between bundles
Comparing students by nomination status						
Nominating student dummy (matched with role model)	-6.11*	-1.14	-0.03	-0.78	-3.81	-0.59
	(3.430)	(2.52)	(0.08)	(1.57)	(2.66)	(3.13)
Mean for non-nominating students	36.15	30.55	2.07	5.28	37.24	21.33
Number of observations (students)	1,758	1,858	1,657	1,657	1,657	1,657

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least three of the 6 main SRI recommendations is adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot, which varies from 0 to 5. The last three adoption variables are dummies equal to 100 if the SRI-recommended value for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. All regressions include sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table 9. Correlation in teacher-trainee and student knowledge

	Score on a scale of 0 to 8	Dummy=1 if answers the 3 main questions correctly
Comparing students in treatments A and B		
Treatment B dummy (incentivized teacher-trainee)	0.34*	0.11**
	(0.200)	(0.050)
Teacher's value of the corresponding quiz performance measure	0.29***	0.14*
	(0.100)	(0.080)
Number of observations	702	702
Comparing students by nomination status		
Nominating student dummy (matched with role model)	0.14*	0.02
	(0.070)	(0.020)
Teacher's value of the corresponding quiz performance measure	0.32***	0.14
	(0.110)	(0.090)
Number of observations	702	702

Quiz performance based on BRAC administrative data. All regressions correct for sampling weights. Standard errors clustered at the village level are presented in in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10. Mediation analysis of quiz scores						
	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI age of seedlings	Follows SRI number of seedlings/bundle	Follows SRI distance between bundles
Treatment B dummy (incentivized teacher-trainee)	-0.23	3.9	0.1	1.12	0.27	6.99
	(6.100)	(5.840)	(0.170)	(2.560)	(5.940)	(5.420)
Quiz score on a scale from 0 to 8	0.95	-1.68	-0.02	2.04***	0.18	3.14***
	(1.730)	(1.510)	(0.040)	(0.510)	(1.570)	(1.080)
Number of observations (students)	1,747	1,847	1,646	1,646	1,646	1,646
Nominating student dummy (matched with role model)	-5.96*	-0.88	-0.03	-0.89	-3.6	-0.68
	(3.440)	(2.540)	(0.080)	(1.580)	(2.670)	(3.160)
Quiz score on a scale from 0 to 8	0.99	-1.5	-0.02	2.10***	0.23	3.44***
	(1.730)	(1.540)	(0.040)	(0.530)	(1.580)	(1.070)
Number of observations (students)	1,747	1,847	1,646	1,646	1,646	1,646

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least three of the 6 main SRI recommendations is adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot, which varies from 0 to 5. The last three adoption variables are dummies equal to 100 if the SRI-recommended value for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. All regressions include sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table 11. Mediation analysis of teacher-trainee SRI adoption

	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI age of seedlings	Follows SRI number of seedlings/bundle	Follows SRI distance between bundles
Treatment B dummy (incentivized teacher-trainee)	0.38	2.68	0.07	2.27	0.02	7.13
	(6.190)	(5.750)	(0.150)	(2.700)	(5.650)	(5.170)
Teacher's value of corresponding SRI adoption measure	0.05	0.13**	0.23***	0.08*	0.11**	0.08**
	(0.050)	(0.050)	(0.050)	(0.040)	(0.050)	(0.040)
Number of observations (students)	1,669	1,755	1,571	1,571	1,571	1,571
Nominating student dummy (matched with role model)	-6.54*	-2.19	-0.04	-0.73	-3.97	-1.7
	(3.570)	(2.560)	(0.080)	(1.670)	(2.720)	(3.240)
Teacher's value of corresponding SRI adoption measure	0.05	0.13**	0.23***	0.07*	0.11**	0.08**
	(0.050)	(0.050)	(0.060)	(0.040)	(0.050)	(0.040)
Number of observations (students)	1,669	1,755	1,571	1,571	1,571	1,571

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least three of the 6 main SRI recommendations is adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot, which varies from 0 to 5. The last three adoption variables are dummies equal to 100 if the SRI-recommended value for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farm. All regressions include sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table 12: Comparison between subsets of P2P farmers and farmers in the SRI training referral experiment

Panel A: Teacher-trainees vs batch1 trainees	Adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI's age of seedlings	Follows SRI's number of seedlings/bundle	Follows SRI's distance between bundles
P2P dummy	33.78***	28.11***	1.380***	7.499***	28.22***	29.39***
	(4.471)	(3.510)	(0.132)	(2.201)	(4.236)	(3.745)
Constant	35.77***	19.99***	1.300***	3.951***	26.59***	16.18***
	(3.191)	(1.732)	(0.0743)	(0.820)	(2.464)	(1.577)
Observations	3,379	3,379	3,320	3,320	3,320	3,320
R-squared	0.091	0.126	0.186	0.019	0.067	0.092
Panel B: Student farmers vs batch2 trainees						
P2P dummy	-7.951*	7.747**	0.700***	2.750**	3.937	5.246*
	(4.432)	(3.466)	(0.110)	(1.320)	(3.847)	(3.021)
Constant	39.33***	21.73***	1.344***	2.187***	29.75***	15.27***
	(3.244)	(1.711)	(0.0680)	(0.533)	(2.435)	(1.555)
Observations	4,057	4,057	3,956	3,956	3,956	3,956
R-squared	0.007	0.012	0.081	0.006	0.002	0.005
Panel C: Non-student farmers in treated villages						
P2P dummy	8.873***	11.91***	0.777***	1.532**	5.702	5.694***
	(2.263)	(2.286)	(0.0849)	(0.741)	(3.541)	(1.687)
Constant	5.674***	3.772***	0.900***	0.414**	16.10***	3.393***
	(0.975)	(0.559)	(0.0527)	(0.192)	(2.238)	(0.783)
Observations	4,543	4,543	4,428	4,428	4,428	4,428
R-squared	0.022	0.054	0.152	0.007	0.005	0.015
Panel D: Farmers in control villages						
P2P dummy	1.803**	2.502***	0.683***	0.961**	2.834	2.544***
	(0.718)	(0.769)	(0.0994)	(0.458)	(3.281)	(0.825)
Constant	0.542**	0.652***	0.711***	0.339**	13.02***	0.718***
	(0.231)	(0.198)	(0.0702)	(0.149)	(2.229)	(0.231)
Observations	6,769	6,769	6,603	6,603	6,603	6,603
R-squared	0.007	0.012	0.159	0.003	0.002	0.011

Each panel presents the results from a pooled regression including specified subsets of the P2P sample and the SRI training referral sample. As in Table 2, the unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least 3 of the 6 main SRI recommendations are adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot; this number ranges from 0 to 5 because one of the six SRI recommendations (using organic fertilizer) is not observed at the plot level. The last three adoption variables are dummies equal to 100 if the SRI-recommendation for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A1: Balancedness test on key baseline characteristics in P2P sample

	Age of household head	Years of education of household head	Cultivable land in decimals	Baseline production per decimal	Revenue per decimal	Input cost per decimal	Labour cost per decimal	Total cost per decimal	Estimated profit per decimal	Number of observations
Balance between farmers in treated and control villages										
Mean for control farmers	45.06	5.09	145.70	19.54	641.27	153.56	102.74	256.30	384.98	1200
Difference with farmers in treated villages	-0.422	-0.045	-0.187	0.279	9.554	0.441	-2.334	-1.892	11.447	1800
Standard error of the difference	(0.587)	(0.281)	(6.466)	(0.985)	(31.027)	(3.617)	(4.091)	(5.502)	(29.822)	
Balance between farmers in villages in treatments A and B (non-incentivized vs. incentivized)										
Mean for farmers in treatment A villages	44.55	5.00	143.90	19.34	636.71	156.13	99.42	255.55	381.16	900
Difference with farmers in treatment B villages	0.121	0.102	5.518	0.953	27.956	-4.227	1.962	-2.265	30.222	900
Standard error of the difference	(0.843)	(0.377)	(7.324)	(1.146)	(35.625)	(5.173)	(4.753)	(6.943)	(33.702)	

There are 60 treatment villages and 40 control villages. Treatment villages are equally divided between treatments A and B. The reported difference coefficients and standard errors are based on regressions with sampling weights and clustering at the village level. Similar results are obtained without sampling weights. Standard errors are presented in in parentheses. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A1b: Balancedness test on key baseline characteristics in P2P sample – without inverse sampling weights

	Age of household head	Years of education of household head	Cultivable land in decimals	Production per decimal	Revenue per decimal	Input cost per decimal	Labour cost per decimal	Total cost per decimal	Estimated profit per decimal	Number of observations
Balance between farmers in treated and control villages										
Mean for control farmers	45.06	5.09	145.69	19.54	641.27	153.56	102.74	256.30	384.98	1200
Difference with farmers in treated villages	-0.42	-0.059	0.534	0.398	13.389	0.454	-2.199	-1.744	15.134	1800
Standard error of the difference	-0.587	-0.278	-6.412	-0.981	-30.948	-3.543	-4.088	-5.463	-29.783	
Balance between farmers in villages in treatments A and B (non-incentivized vs. incentivized)										
Mean for farmers in treatment A villages	44.55	5.00	143.91	19.49	641.58	156.29	99.52	255.80	385.78	900
Difference with farmers in treatment B villages	0.178	0.054	4.628	0.899	26.158	-4.546	2.048	-2.498	28.656	900
Standard error of the difference	-0.843	-0.368	-7.13	-1.131	-35.297	-4.945	-4.747	-6.799	-33.535	

There are 60 treatment villages and 40 control villages. Treatment villages are equally divided between treatments A and B. The reported difference coefficients and standard errors are based on regressions without sampling weights but with clustering at the village level. Standard errors are presented in in parentheses. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A2: Balancedness test on key baseline characteristics within treated villages

	Age of household head	Years of education of household head	Cultivable land in decimals	Baseline production per decimal	Revenue per decimal	Input cost per decimal	Labour cost per decimal	Total cost per decimal	Estimated profit per decimal	Number of observations
Balance between teachers and non-teachers in treated villages										
Mean for non-teacher farmers	44.54	4.964	145.2	19.88	652.80	153.80	99.96	253.76	399.03	1440
Difference with teachers	0.736	0.224	3.477	-0.269	-9.141	0.916	2.067	2.983	-12.124	360
	(0.684)	(0.257)	(5.642)	(0.396)	(13.239)	(2.036)	(2.848)	(3.567)	(12.572)	
Balance between students and non-students in treated villages										
Mean for non-student farmers	44.84	5.077	147.3	19.87	651.97	154.15	101.84	255.99	395.98	1080
Difference with student farmers	-0.546	-0.085	-3.042	-0.113	-2.902	-0.382	-3.647	-4.029	1.127	720
	(0.486)	(0.221)	(4.791)	(0.356)	(11.799)	(1.392)	(2.379)	(2.931)	(11.313)	
Balance between nominating and non-nominating farmers in treated villages										
Mean for non-nominating students	44.42	4.774	140.9	19.50	640.44	153.97	97.22	251.20	389.24	360
Difference with nominating students	0.009	0.287	5.624	0.499	17.100	-0.409	1.926	1.516	15.584	360
	(0.897)	(0.371)	(6.869)	(0.507)	(16.624)	(2.204)	(3.499)	(4.314)	(16.646)	

The results use all 60 treatment villages, equally divided between treatments A and B. The reported difference coefficients and standard errors are based on regressions with sampling weights and clustering at the village level. Similar results are obtained without sampling weights. Standard errors are presented in in parentheses. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A2b: Balancedness test on key baseline characteristics within treated villages -- without inverse sampling weights

	Age of household head	Years of education of household head	Cultivable land in decimals	Production per decimal	Revenue per decimal	Input cost per decimal	Labour cost per decimal	Total cost per decimal	Estimated profit per decimal	Number of observations
Balance between teachers and non-teachers in treated villages										
Mean for non-teacher farmers	44.54	4.96	145.23	19.92	653.94	153.89	99.88	253.77	400.17	1440
Difference with teachers	0.499	0.333	5.011	0.124	3.638	0.626	3.304	3.931	-0.293	360
	(0.630)	(0.242)	(5.547)	(0.330)	(10.938)	(1.593)	(3.084)	(3.518)	(10.577)	
Balance between students and non-students in treated villages										
Mean for non-student farmers	44.84	5.08	147.26	20.04	657.75	154.14	102.26	256.40	401.36	1080
Difference with student farmers	-0.501	-0.116	-2.571	-0.249	-7.725	-0.309	-4.296*	-4.606	-3.119	720
	(0.454)	(0.213)	(4.812)	(0.348)	(11.485)	(1.302)	(2.360)	(2.904)	(11.026)	
Balance between nominating and non-nominating farmers in treated villages										
Mean for non-nominating students	44.42	4.77	140.94	19.54	641.56	153.69	96.98	250.67	390.89	360
Difference with nominating students	-0.073	0.327	7.067	0.464	15.823	0.381	2.120	2.501	13.322	360
	(0.848)	(0.366)	(6.680)	(0.482)	(15.704)	(2.237)	(3.324)	(4.233)	(15.944)	

The results use all 60 treatment villages, equally divided between treatments A and B. The reported difference coefficients and standard errors are based on regressions without sampling weights but with clustering at the village level. Standard errors are presented in in parentheses. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A2c: Alternative balancedness test on key baseline characteristics withinP2P treated villages

	Average age of the household (above 15 years)	Average education of the household	Cultivable farm area in last Boro season (decimals)	Household size	Maximum education of any household member	Working age members in the household	Number of observations
Balance between teachers and non-teachers in treated villages							
Mean for non-teacher farmers	36.66	4.345	159.9	5.176	8.607	3.173	1440
Difference with teachers	0.168	0.0299	5.117	0.0985	-0.173	0.0376	360
	(0.367)	(0.102)	(10.13)	(0.194)	(0.155)	(0.0742)	
Balance between students and non-students in treated villages							
Mean for non-student farmers	36.89	4.356	165.6	5.206	8.626	3.202	1080
Difference with student farmers	-0.488	-0.0116	-11.63	-0.0213	-0.143	-0.0548	720
	(0.330)	(0.0846)	(7.854)	(0.152)	(0.133)	(0.0703)	
Balance between nominating and non-nominating farmers in treated villages							
Mean for non-nominating students	36.4	4.277	149.8	5.156	8.374	3.115	360
Difference with nominating students	0	0.133	8.307	0.0569	0.217	0.0647	360
	(0.478)	(0.138)	(10.39)	(0.268)	(0.178)	(0.0941)	

The object of this table is to allow comparison with balancedness tests reported in the SRI training referral paper. The results reported here use all 60 P2P treatment villages, equally divided between treatments A and B. The reported difference coefficients and standard errors are based on regressions with sampling weights and clustering at the village level. Similar results are obtained without sampling weights. Standard errors are presented in in parentheses. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A2d: Alternative balancedness test on key baseline characteristics in P2P villages

	Average age of the household (above 15 years)	Average education of the household	Cultivable farm area in last Boro season (decimals)	Household size	Maximum education of any household member	Working age members in the household	Number of observations
Balance between farmers in treated and control villages							
Mean for control farmers	36.97	4.268	176.3	5.035	8.585	3.17	1200
Difference with farmers in treated villages	-0.272	0.0835	-15.25	0.162	-0.0151	0.0104	1800
Standard error of the difference	(0.320)	(0.109)	(11.79)	(0.127)	(0.144)	(0.0676)	
Balance between farmers in villages in treatments A and B (non-incentivized vs. incentivized)							
Mean for farmers in treatment A villages	36.42	4.388	156.8	5.188	8.61	3.24	900
Difference with farmers in treatment B villages	0.556	-0.0740	8.256	0.0178	-0.0792	-0.117	900
Standard error of the difference	(0.377)	(0.149)	(13.01)	(0.185)	(0.190)	(0.0837)	

The object of this table is to allow comparison with balancedness tests reported in the SRI training referral paper. The results reported here use all 60 P2P treatment villages, equally divided between treatments A and B. The reported difference coefficients and standard errors are based on regressions with sampling weights and clustering at the village level. Similar results are obtained without sampling weights. Standard errors are presented in in parentheses. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A3. Balancedness test on key baseline characteristics between the P2P sample and the SRI training referral sample							
		Average age of the household	Average education of the household	Cultivable farm area in last Boro season	Household size	Maximum education	Working age members
Panel A: All treated and control villages from the pooled sample							
	P2P sample	0.229	-0.009	2.740	-0.031	0.005	0.039
		(0.206)	-0.079	(6.328)	-0.074	(0.106)	-0.04
	Constant	36.43***	4.339***	161.5***	5.186***	8.572***	3.100***
		(0.314)	(0.127)	(8.504)	-0.099	(0.174)	-0.058
	Observations	8,486	8,486	8,486	8,486	8,486	8,486
	R-squared	0.000	0.000	0.000	0.000	0.000	0.000
Panel B: 360 teacher-farmers vs 1185 farmers from batch 1 of the SRI training referral experiment							
	P2P sample	0.329	0.0718	9.825	0.156	-0.107	0.0812
		(0.422)	(0.136)	(10.93)	(0.196)	(0.192)	(0.0901)
	Constant	36.73***	4.367***	155.6***	5.105***	8.630***	3.149***
		(0.202)	(0.0888)	(5.004)	(0.0672)	(0.125)	(0.0414)
	Observations	1,545	1,545	1,545	1,545	1,545	1,545
	R-squared	0.000	0.000	0.001	0.001	0.000	0.001
Panel C: 720 students vs 1041 farmers from batch 2 of the SRI training referral experiment							
	P2P sample	-0.557	0.024	-13.07	0.048	0.027	0.023
		(0.386)	(0.130)	(8.874)	(0.136)	(0.180)	(0.0718)
	Constant	36.93***	4.308***	168.2***	5.114***	8.444***	3.121***
		(0.295)	(0.0922)	(5.939)	-0.07	(0.130)	(0.0459)
	Observations	1,761	1,761	1,761	1,761	1,761	1,761
	R-squared	0.001	0.000	0.002	0.000	0.000	0.000
Panel D: 720 non-student farmers in P2P treated villages vs 1404 untrained farmers in treated villages of the SRI training referral experiment							
	P2P sample	0.478	0.042	-2.198	-0.016	0.199	0.063
		(0.342)	(0.120)	(10.59)	(0.118)	(0.176)	-0.067
	Constant	36.69***	4.295***	166.4***	5.185***	8.519***	3.133***
		(0.235)	-0.088	(6.197)	-0.058	(0.132)	-0.042
	Observations	2,124	2,124	2,124	2,124	2,124	2,124
	R-squared	0.001	0.000	0.000	0.000	0.001	0.000
Panel E: 1200 vs 1856 control farmers							
	P2P sample	0.541	-0.076	10.32	-0.154	-0.077	0.024
		(0.355)	(0.126)	(10.72)	(0.111)	(0.168)	(0.0695)
	Constant	36.43***	4.344***	165.9***	5.189***	8.662***	3.146***
		(0.245)	(0.0974)	(4.272)	(0.0696)	(0.129)	(0.0453)
	Observations	3,056	3,056	3,056	3,056	3,056	3,056
	R-squared	0.001	0.000	0.001	0.001	0.000	0.000

Table A4. Adoption by treatment status, using augmented inverse probability matching						
Dependent variable is:	Dummy=1 if farmer adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI's age of seedlings	Follows SRI's number of seedlings/bundle	Follows SRI's distance between bundles
Teacher-trainees:						
Treatment effect	69.49***	45.17***	1.30***	10.16***	38.95***	42.09***
	(3.006)	(3.005)	(0.121)	(2.086)	(4.024)	(3.451)
Control mean	2.25***	3.05***	1.37***	1.38***	15.73***	2.99***
	(0.677)	(0.750)	(0.068)	(0.431)	(2.306)	.(0.787)
Number of observations	3,696	3,926	3,471	3,471	3,471	3,471
Student farmers:						
Treatment effect	26.76***	25.76***	0.64***	2.97**	15.04***	16.41***
	(3.393)	(3.382)	(0.105)	(1.382)	(3.771)	(2.960)
Control mean	2.26***	3.05***	1.37***	1.37***	15.74***	3.01***
	(0.681)	(0.750)	(0.068)	(0.427)	(2.306)	(0.794)
Number of observations	3,704	3,925	3,491	3,491	3,491	3,491
Non-student farmers:						
Treatment effect	12.86***	12.66***	0.31***	0.48	5.59	6.35***
	(2.022)	(2.167)	(0.085)	(0.864)	(3.511)	(1.648)
Control mean	2.25***	3.03***	1.37***	1.38***	15.72***	2.98***
	(0.680)	(0.747)	(0.067)	(0.435)	(2.294)	(0.786)
Number of observations	4,572	4,853	4,291	4,291	4,291	4,291

Each set of results is obtained using the Stata treatment effect estimator teffects using augmented inverse probability matching. As matching variables, we use the four variables that enter into the selection of teacher-trainees in treated villages: baseline production per decimal of land; household age; years of household education; and cultivable land in decimals. We have combined nominating and non-nominating students since they are similar in Table 2 and we do not have nominating information in control villages. The rest is identical to Table 2. Each set of results corresponds to a pairwise comparison between one category of treated farmers and the controls, weighted according to their similarity with treated farmers. This means that, in effect, control farmers with baseline features similar to teacher-trainees are used as controls for teacher-trainees; and similarly for the students and non-student treatment categories. Standard errors are clustered at the village level. Nearly identical point estimates are obtained using nearest neighbor matching.

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least 3 of the 6 main SRI recommendations are adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot; this number ranges from 0 to 5 because one of the six SRI recommendations (using organic fertilizer) is not observed at the plot level. The last three adoption variables are dummies equal to 100 if the SRI-recommendation for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A5. Mediation analysis of good performance on quiz						
	Dummy=1 if farmer adopts SRI on any plot	proportion of land under SRI	Number of adopted SRI principles	Follows SRI age of seedlings	Follows SRI number of seedlings/bundle	Follows SRI distance between bundles
Treatment B dummy (incentivized teacher)	-0.51	3.53	0.08	1.26	-0.09	6.92
	(6.070)	(6.000)	(0.180)	(2.540)	(5.930)	(5.380)
Dummy=1 if answers main questions correctly	6.45	-1.80	0.21*	5.54***	4.85	11.90***
	(6.730)	(5.540)	(0.110)	(1.350)	(5.670)	(2.870)
Number of observations (students)	1,747	1,847	1,646	1,646	1,646	1,646
Nominating student dummy (matched with role model)	-5.92*	-0.93	-0.03	-0.76	-3.59	-0.47
	(3.420)	(2.530)	(0.080)	(1.590)	(2.670)	(3.150)
Dummy=1 if answers main questions correctly	6.40	-1.17	0.22**	5.74***	4.85	12.97***
	(6.820)	(5.500)	(0.110)	(1.510)	(5.760)	(2.790)
Number of observations (students)	1,747	1,847	1,646	1,646	1,646	1,646

The unit of observation is a plot. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least three of the 6 main SRI recommendations is adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot, which varies from 0 to 5. The last three adoption variables are dummies equal to 100 if the SRI-recommended value for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farm. All regressions include sampling weights. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A6: Comparison between subsets of P2P farmers and farmers in the SRI training referral experiment -- using first plot only

	Dummy=1 if farmer adopts SRI on any plot	Proportion of land under SRI	Number of adopted SRI principles	Follows SRI's age of seedlings	Follows SRI's number of seedlings/bundle	Follows SRI's distance between bundles
Panel A: Teacher-trainees vs batch1 trainees						
P2P dummy	36.54***	28.69***	1.386***	6.572***	29.82***	29.92***
	(4.365)	(3.455)	(0.141)	(2.321)	(4.554)	(4.265)
Constant	35.06***	19.84***	1.299***	4.167***	26.22***	16.06***
	(3.153)	(1.727)	(0.0740)	(0.810)	(2.424)	(1.569)
Observations	1,322	1,322	1,282	1,282	1,282	1,282
R-squared	0.103	0.128	0.180	0.014	0.072	0.090
Panel B: Student farmers vs batch2 trainees						
P2P dummy	-5.594	8.295**	0.725***	2.631*	6.799*	6.281**
	(4.287)	(3.331)	(0.112)	(1.351)	(3.994)	(3.050)
Constant	38.32***	21.50***	1.334***	2.235***	28.60***	14.86***
	(3.187)	(1.690)	(0.0669)	(0.526)	(2.358)	(1.540)
Observations	1,558	1,558	1,491	1,491	1,491	1,491
R-squared	0.003	0.014	0.084	0.005	0.005	0.007
Panel C: Non-student farmers in treated villages						
P2P dummy	9.176***	12.10***	0.808***	1.953***	8.433**	5.912***
	(2.242)	(2.283)	(0.0819)	(0.743)	(3.746)	(1.784)
Constant	5.561***	3.702***	0.890***	0.269*	15.16***	3.318***
	(0.952)	(0.537)	(0.0513)	(0.155)	(2.177)	(0.745)
Observations	1,780	1,780	1,700	1,700	1,700	1,700
R-squared	0.024	0.056	0.157	0.009	0.011	0.016
Panel D: Farmers in control villages						
P2P dummy	2.066**	2.462***	0.717***	1.036**	4.832	3.295***
	(0.793)	(0.799)	(0.0986)	(0.400)	(3.375)	(1.007)
Constant	0.436**	0.668***	0.699***	0.311**	12.27***	0.436**
	(0.217)	(0.217)	(0.0701)	(0.135)	(2.169)	(0.179)
Observations	2,685	2,685	2,571	2,571	2,571	2,571
R-squared	0.008	0.011	0.169	0.004	0.005	0.015

Each panel presents the results from a pooled regression including specified subsets of the P2P sample and the SRI training referral sample. As in Table 12, the unit of observation is a plot, but we only use observations relative to the first/main plot so as to make sure we give equal weight to each farmer. SRI adoption is based on a field evaluation conducted by a BRAC extension agent. Information on adoption is collected for up to 3 plots per respondent. The first SRI adoption variable equals 100 if at least 3 of the 6 main SRI recommendations are adopted by the farmer on at least one of his three main plots, and 0 otherwise. The second SRI adoption variable equals proportion of land under SRI on a scale of 0 to 100. The third SRI adoption variable is the number of SRI principles adopted on the plot; this number ranges from 0 to 5 because one of the six SRI recommendations (using organic fertilizer) is not observed at the plot level. The last three adoption variables are dummies equal to 100 if the SRI-recommendation for a particular practice is applied on the plot, and 0 otherwise. Since all dummies have been multiplied by 100, coefficients in columns 1, 2, 4, 5 and 6 can be read as changes in percentage points. All standard errors are reported in parentheses and are clustered at the village level. This also corrects for likely correlation across plots within farms. Reported p-values: *** 1% level; ** 5% level; * 10% level.

Table A7. Cost-benefit calculation

	P2P Experiment						**SRI training referral experiment**				
BENEFITS	**Treatment group**	**Treatment effect/ profit per decimal over control**	**Number of farmers**	**Average Land sizen (in decimal land)**	**Total Profit (in BDT)**		**Treatment group**	**Treatment effect/ profit per decimal over control**	**Number of farmers**	**Average Land sizen (in decimal land)**	**Total Profit (in BDT)**
	Teacher-trainee	53.0295	360	150.2	2,868,100.30		T1	83.8	1036	160.7	13,946,298.72
	Student	43.16	720	144.7	4,496,105.99		T2	119.6	1124	163.6	21,992,445.67
	Non-student	17.4972	720	145.8	1,836,348.92		T3	82.8	1111	166.6	15,332,145.52
Total (BDT)					9,200,555.20						51,270,889.91
Total (USD) @80/1 exchange rate				1800	115,006.94				3271		640,886.12
Total benefit/person (USD)					**63.9**						**195.93**
COSTS		**Per unit cost**	**Number of farmers**	**Per unit cost**	**Number of farmers**	**Total incentive/fee**		**Per unit cost**	**Number of farmers**	**Farmers who received the training**	**Total incentive/fee**
Incentive/participation fee of farmers			**T1**		**T2**						
	Teacher-trainee	500	180	500	180	90,000.00		0	1036	749	0.00
	Student	0	360	0	360	0.00		300	1124	745	223,500.00
	Non-student	0	360	0	360	0.00		600	1111	732	219,600.00
	Participation fee	300	180	300	180	108,000.00		300	3271	2226	667,800.00
								in T3, there were about 50% farmers who adopted			
Total (BDT)						198,000.00					1,110,900.00
Total (USD) @80/1 exchange rate						2,475.00			3271		13,886.25
Total cost/farmer (USD)						**1.38**					**4.25**
Aministrative costs -- see calculations below						23.60					22.61
Total cost per farmer						**24.98**					**26.86**
BENEFFIT/COST RATIO						**2.56**					**7.30**

Notes: payments were on average the same for farmers in treatments T1 and T2 of the P2P experiment. In T1 we paid 250 taka/farmer and in T2 we paid 300 taka/farmer if ALL answers were correct, but penalized them by 20 taka for each wrong answer. There was no payment to students or non-student farmers.

Calculation of training Costs:

P2P experiment:

The cost for training includes only the training of teacher-trainees. There were 6 teacher farmers to train in each village. For 60 villages we needed 3 trainers to complete the work in one month (no repeat training), costing USD 2250 in total. Mobilizing farmers, testing etc. 2 enumerators assisting trainers at the same rate as above. For 60 treat villages, we needed to pay 6 enumerators @USD500/enumerators, total cost USD3000. Cost for venue/projector etc. in 60 locations @$100/location= USD600. 600 teacher farmers receiving lunch/snacks for training, costing $2.5/farmer=$1500.

Total cost =2250+3000+6000+1500	12,750.00
Cost per farmer training (student+teacher-trainees=540 farmers)	23.60

SRI training referral experiment

A trainer trained one village in a day, came back to train B2 farmers the following week. One average a trainer could train 5 villages in two weeks. There were 120 villages, which mean we needed 12 trainers to complete the training in a month, who were paid $750/month, costing USD9000 in total for salary of the trainer. In addition, there were cost for training, such as two enumerators assisting trainers in mobilizing farmers for the training in each village. They were paid $500/month, cost for them (24 enumerators) is=USD12000. Cost for venue/projector etc. in 120 locations for two times @$100/location= USD24000. 2226 farmers receiving lunch/snacks for training, costing $2.5/farmer=$5565.

Total cost = 9000+12000+24000+5565	50,565.00
Cost per farmer training	22.61