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Publication Date
2019-04-04

Series Name: WPS
Paper No.: 072
Issue Date: 4 April 2019

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Recommended Citation:

Fafchamps, Marcel; Malek, Mohammad Abdul; Pakrashi, Debayan; Islam, Asadul. (2019). Mobilizing P2P Diffusion for New Agricultural Practices: Experimental Evidence from Bangladesh. CEGA Working Paper Series No. WPS-072. Center for Effective Global Action. University of California, Berkeley.

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

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February 2019

Abstract

We run a randomized controlled experiment in which farmers trained on a new rice cultivation method (SRI) teach two other farmers selected by us. We find that farmers invited to teach others are much more likely to adopt new practices than farmers who only receive the BRAC training. Teacher farmers are effective at spreading knowledge and inducing adoption. Incentivizing teachers improves knowledge transmission but not adoption. Matching teachers with farmers who list them as role models does not improve knowledge transmission and may hurt adoption. Using mediation analysis, we find that the knowledge of the teacher is correlated with that of their student, consistent with knowledge transmission. We also find that SRI knowledge predicts adoption of some SRI practices, and that adoption by teachers

*We have benefitted from comments and suggestions received from Chris Barrett, Michael Carter, Sisira Jayasuriya; and from conference participants at the BRAC centre, the Department of Agricultural Extension (DAE) of the Ministry of Agriculture of Bangladesh, the International Growth Centre (IGC) conference in Dhaka, South Asian Development economics conference in Colombo, and seminar presentation at IIT Kanpur, and Monash Universirty. Sakiba Tasneem, Latiful Haque and Tanvir Shatil provided excellent support for the field work, survey design and data collection. This work would not be possible without encouragement and support from the late Mahabub Hossain, ex-executive director of BRAC. We thank the BRAC research and evaluation division for support and the BRAC agriculture and food security program for conducting the field work, training and surveys. We also received funding from IGC. The usual disclaimer applies.

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predicts adoption by their students, suggesting that students follow the example of their teacher. Explicitly mobilizing peer-to-peer (P2P) transmission of knowledge thus seems a cost-effective way of inducing the adoption of new agricultural practices.

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).

We focus on a specific case that is easily amenable to experimentation: 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. 2018).

Focusing on this technology offers many advantages in terms of design. 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 them-

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

selves. 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 experimentally investigate two subsidiary questions. First, we test whether we can improve information dissemination by incentivizing the teacher farmer. As noted by Foster and Rosenzweig (1995), farmers may not fully internalize the benefits they can impart onto others when 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 2018; Duflo et al. 2011; Heath 2018).[2] By incentivizing teachers 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 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

[2] 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.

proximity. Disseminating health information across castes has for instance been shown by Berg et al. (2017) to be problematic unless the disseminator is incentivized. Our experiment will tell us whether a similar effect is observed in agricultural extension.

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.

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). Within each study village we let BRAC identify a set of suitable farmers 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 'teachers' 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 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, i.e., the 'teachers'. But it is also high among 'students', and even 'non-students' display an adoption rate significantly higher than controls. From this we conclude that BRAC trainees are capable of teaching the new practices to other farmers.

To examine whether the peer-to-peer (P2P) transmission of new practices can be improved through incentives, half of the 'teacher' subjects 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 'teachers' does not significantly improve diffusion in our case.

We also investigate whether trainees better transmit SRI knowledge and practices to 'students' 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 assign students to teachers 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 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 matching 'teachers' 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.

Finally 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 'teachers' helps predict adoption by their 'students', suggesting that students follow the example of their teacher. This result is reminiscent of the co-adoption finding of Fafchamps et al. (2018).

The main contribution of this paper is empirical. We complement the literature on tech-

nology diffusion along social networks discussed earlier. We also make a small methodological contribution by showing how to approach the estimation of average treatment effects when assignment to a treatment within the experiment is 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 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).[3] In particular, SRI imposes a specific transplanting time window and emphasizes a wider spacing and different arrangement of the transplanted rice.

Treated villages are further divided into two treatment arms of 30 villages each. In Treatment B villages, 'teachers' receive an incentive payment; in Treatment A, they do not. The financial incentive given to teachers 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. (2018), i.e., owning

[3] 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.

more than 50 decimals of land (i.e., half an acre)[4] but less than 10 acres. All farmers answer a baseline questionnaire gathering basic information about household composition and farm assets.

As part of the baseline survey, each farmer [5] 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 nomination received from other farmers. This ranking is used to select 6 teachers for training as follows: four teachers 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, teachers 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 $5.

Of the remaining 24 farmers, 12 are selected to be trained by the 6 teachers. Each teacher is assigned two students: one who nominated the teacher as opinion leader or role model at baseline; and one who did not. A priori we expect students to learn better if matched with a teacher they nominated. The remaining 12 farmers do not receive any SRI training from BRAC.[6] We call these farmers 'non-students'.

Teachers 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. Teachers are then asked to teach these two students about the principles of SRI during one week and instructed to convey to them

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

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

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

the same information as they received from BRAC trainers. To help them in their task, teachers are provided with three copies of a short brochure about SRI – of which one copy is for the teacher and one is for each of their students. All teachers 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 teachers 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 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 – teachers 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 their quiz performance. In Treatment B – the incentivized treatment – teachers 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, teachers can expect to receive approximately the same payment under the two treatment schemes. Teachers are informed of the type of payment they will receive at the time they are told the name of their two students.[7] They are also told that

[7] 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."

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 teachers 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.

3. Testing strategy

We start by estimating 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 staff visits to the fields of each farmer to gauge how closely they follow key precepts of the SRI approach, 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 also 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: teachers; students matched with a teacher they regard as role model (i.e., 'nominating student'); stu-

dents 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:

$$y_{iv} = \alpha + \sum_{k=1}^{4} \beta_k T_{ivk} + u_{iv} \tag{3.1}$$

where y_{iv} is an outcome of interest for farmer i in village v, $k = \{1, 2, 3, 4\}$ denotes the four possible treatment types, $T_{ivk} = 1$ if farmer i in village v is assigned to treatment k, and β_k is the ATE for treatment k. Note that, by construction, each treatment is mutually exclusive so that $T_{ivk} = 1$ for at most one treatment per farmer. In control villages, $T_{ivk} = 0$ for all i and k. Having estimated (3.1), we can then test for the pairwise equality of the different treatments, e.g., whether $\beta_k = \beta_l$ for $k \neq l$.

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. (2018) 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 teachers 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 teachers 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.

The main difficulty we face in estimating model (3.1) is that different farmers have different

probabilities of being assigned to the four treatment groups. This is most obvious for teachers: in each village we select 4 teachers from farmers with number of nomination above and 2 teachers from below the village median. Hence, teachers are more representative of the above median farmers. If the probability of assignment to treatment is correlated with responsiveness to that treatment, a one-for-one comparison with control farmers will mismeasure treatment effects. To correct for this, we use sampling weights when estimating model (3.1) (e.g., Imbens and Wooldridge 2009).[8]

Sampling probabilities also vary for nominating and non-nominating students. First, the teacher assignment process implies that of the 24 farmers not assigned the role of teachers, 11 are above the median in terms of nominations while 13 are below the median. Secondly, remember that, by design, 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. In practice, this is achieved by a sequential algorithm that starts by randomly sorting the 24 non-teachers and 6 teachers. 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 teachers, 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 teachers have been assigned a non-nominating student.

[8] 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.

As is clear from the above description, assignment to various treatments involves randomness – i.e., the arbitrary order in which individual farmers are considered at the different steps of the algorithm. But not all farmers face the same probability of being assigned to each of the three possible roles – i.e., nominating student, non-nominating student, and non-student. This is because the pattern of nominations varies from village to village, making it easier or harder at each step of the algorithm to find a farmer that meets the required criterion. Furthermore, how easy it is to find, say, a non-nominating student to match with a particular teacher depends on which farmers are selected to be teachers, and which farmers are selected to be nominating students for these teachers, and so on. Given this, it is not, in general, practical to calculate algebraically what each farmer's probability is of being in one of the four possible treated categories. It is, however, easy to repeat the selection algorithm with a different random ordering of farmers to approximate, by simulation, the actual probability of assignment of each farmer to each of the four treatment categories. To achieve this, we simply rerun the selection algorithm for each village many times, and we use the simulated frequency of assignment of each farmer to each of the treatments as approximation for their sampling probabilities.

Figure 1 presents a histogram of relative sampling weights for all farmers in the treated sample.[9] 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 sample proportion. A number larger than 1 means the farmer has a higher than average chance of being assigned the role of teacher, and vice versa when the number is smaller than 1. As anticipated given the selection rule for teachers, the relative probability of teacher assignment is bimodal centered on 2/3 and 4/3, depending on whether the farmer is below or above the median number of nominations. There

[9] By construction, all control farmers have a sampling weight of 1.

is variation in the probability of assignment to different treatments across farmers in treated villages. But in all cases the distributions have full support: it is not the case that some farmers are always assigned to a specific treatment. This means that we can use the inverse of these simulated assignment probabilities as sampling weights when estimating (3.1) and for conducting all empirical analysis in the paper.[10]

Next we compare students assigned to non-incentivized or incentivized teacher in treatments A and B. Incentivizing teachers 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 teachers increases the transfer of knowledge, we compare the performance of students on the quiz between incentivized and non-incentivized teachers:

$$q_{iv} = \alpha + \beta_m T_{ivm} + u_{iv} \tag{3.2}$$

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 (3.2) can only be estimated on students and teachers 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 teachers on students' quiz performance. We also estimate a similar regression to check whether incentivizing teachers affect their own quiz performance and SRI adoption – in case being incentivized induces teachers to pay closer attention to SRI instruction and hence learn better. We conduct a similar analysis to compare the quiz performance of nominating and

[10] Since we know the actual treatment assignment probabilities of each farmer (or can compute them precisely by simulation), applying sampling weights perfectly corrects for the possibility of correlation between propensity scores and treatment assignment. To demonstrate this point in our data, we obtain regression estimates that correct for sampling weights and also include the propensity scores as additional regressors. The purpose is to reassure the reader that, as should be the case, including propensity scores as regressors adds nothing when sampling weights are included. Unsurprisingly, these estimates show very little variation compared to those presented here. They are omitted from the paper to save space.

non-nominating students, and to compare their adoption rates.

We end 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:

$$y_{iv} = \alpha + \beta_m T_{ivm} + \gamma q_{iv} + u_{iv} \tag{3.3}$$

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.

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}:

$$y_{iv} = \alpha + \beta_m T_{ivm} + \theta y_{jiv} + u_{iv} \tag{3.4}$$

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 (3.4). By construction, regressions (3.3) and (3.4) 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.

4. The data

4.1. Descriptive statistics

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 teachers receive SRI training directly from BRAC, teachers perform better than students on the quiz. The difference in performance is significant at the 1% level.[11]

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 principles

[11] 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

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 counts captures the number of SRI principles the farmer has adopted, on a scale of 0 to 5.[12] 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, Takashi and Barrett 2014, Fafchamps et al. 2018).

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.

[12] 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.

4.2. Balance

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 correct for sampling weights.[13] Standard errors are clustered 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. The second panel of Table A1 compares farmers in treatment A and B villages – that is, without and with incentivized teachers. 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 teachers with other farmers. We see that teachers are in general slightly older and better educated than non-teachers, and they cultivate more land at baseline. These differences, however, are small in magnitude and not statistically significant. We similarly find no statistical 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. From this analysis, we conclude that we have satisfactory balance across our different treatment categories.

[13] They are obtained by regressing the variable of interest on a dummy for treatment. The regression corrects for sampling weights and clusters standard errors at the village level. Very similar results are obtained without sampling weights.

5. Econometric results

5.1. Treatment and adoption

We start by reporting coefficient estimates for equation (3.1) in Table 2. The unit of observation is a plot – with up to three plots per farmer[14]. 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.[15]

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

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

[15] 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.

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 teachers 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.

From this evidence we conclude that villages exposed to the BRAC extension effort do experience significant – if partial – adoption of cultivation practices recommended under SRI. Adoption by teachers is 70%. This is nearly twice as much as the 37% adoption rate observed by Fafchamps et al. (2018) among farmers who receive SRI training from the BRAC trainers in similar areas of Bangladesh.[16] This suggests that being invited to teach SRI to others increases trainees' interest in the new practices. Adoption by student farmers – 27 to 33% – is 4 to 10 percentage points lower than adoption by unincentivized trainees in the Fafchamps et al.

[16] In that experiment, trainees are asked to refer other farmers for SRI training. Fafchamps et al. (2018) find that incentivizing referral generates excess SRI adoption among referring trainees. For this reason, we use the adoption rate of unincentivized, randomly selected trainees, who constitute the most relevant comparison group for our purpose.

(2018) experiment, suggesting a remarkable success rate for what is, after all, a cheaper way of dispensing knowledge. We also find a non-negligible amount of interest among non-students, with 12% adopting some SRI practices – which again compare favorably to adoption by untrained farmers in Fafchamps et al. (2018), which is about 7-8% (see their Table 6).

Next, we compare the effect of the different treatments on agricultural performance. Results are shown in Table 4. We see that teachers have statistically higher crop output and agricultural profits than controls. We also find that student farmers have significantly higher yields than control 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 teachers 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 teachers 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.

5.2. 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 quizz 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 teachers do significantly better on the quiz – which is about 40% of the difference between the score of teachers (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 teachers increases the proportion of such farmers by 11 percentage points – which is equivalent to 65% of the difference between teachers and students in treatment A. Put differently, incentivizing teachers closes a significant fraction of the knowledge gap between student farmers – who did not receive BRAC training – and teachers – 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 teachers on students' SRI adoption. The Table also reports adoption by teachers – who may have responded to the incentive themselves. If anything, we find a fall in adoption rate among teachers 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 teachers 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.

5.3. Mechanisms: Mediation analysis

The last empirical section of this paper seeks to identify two possible channels by which teachers 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 teachers 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 (3.3). 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 seddlings. 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 A3, 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 teachers as role models.

6. 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 other did not.

We find that, compared to earlier findings by Fafchamps et al. (2018), farmers invited to serve as 'teacher' are 33 percentage points more likely to adopt SRI practices than farmers who receive SRI training from BRAC trainers in nearby villages. We also find that teacher farmers are quite effective at spreading SRI knowledge and at inducing SRI adoption: student farmers

are only 7 percentage points less likely to adopt SRI practices than farmers who receive SRI training from BRAC in the Fafchamps et al. (2018) experiment. From this we conclude that BRAC trainees are capable of teaching the new practices to other farmers.

To investigate whether incentives can improve the transmission of agricultural knowledge and practices, half of the 'teachers' 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 'teachers' on knowledge transmission does not significantly improve adoption in our case.

To investigate whether 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 'teachers' 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 'teachers' helps predict adoption by their 'students', suggesting that students follow the example of their teacher.

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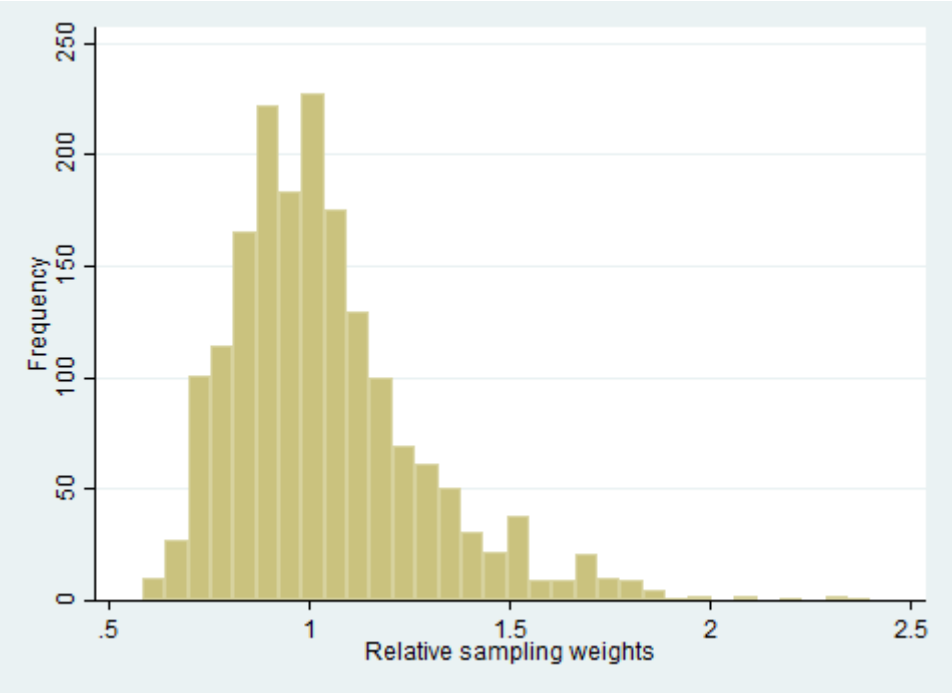
Figure 1. Simulated relative sampling weights of farmers in treated villages



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 0-8	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	2.3%	14.84	33.2%	47.08	0.00
Proportion of land under SRI	all	plot	0-1	3.1%	16.41	27.5%	39.57	0.00
Number of SRI principles adopted on plot	all	plot	0-5	1.37	0.83	2.01	1.17	0.00
Dummy=1 if follows the SRI-recommended age of seedlings	all	plot	0-1	1.4%	11.62	5.0%	21.79	0.00
Dummy=1 if follows the SRI-recommended number of seedlings per bundle	all	plot	0-1	15.7%	36.43	32.5%	46.84	0.00
Dummy=1 if follows the SRI-recommended distance between bundles	all	plot	0-1	3.0%	17.07	20.6%	40.42	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 major SRI recommendations are: 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 chemical fertilizers; intermittent irrigation; complementary weed and pest control (mechanical weeding). 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 mechanical weeding) is not observed at the plot level at the time of field visit. 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:	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
Treatments:						
Teacher	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 mechanical weeding) is not observed at the plot level at the time of field visit. 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

	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
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	Labour costs	Total costs	Profits
Treatments:						
Teacher	0.07***	0.07***	-0.03	-0.04	-0.02	0.16**
	(0.02)	(0.03)	(0.02)	(0.10)	(0.05)	(0.06)
Nominating student	0.04*	0.04	-0.05**	-0.05	-0.03	0.13*
	(0.02)	(0.03)	(0.02)	(0.09)	(0.05)	(0.06)
Non-nominating student	0.05*	0.05*	-0.03	-0.03	-0.04	0.14**
	(0.02)	(0.03)	(0.02)	(0.09)	(0.05)	(0.07)
Non-student	0.02	0.02	-0.03	-0.01	-0.01	0.07
	(0.02)	(0.03)	(0.02)	(0.09)	(0.05)	(0.07)
Control mean (in log)	2.62	5.72	3.74	3.68	4.55	5.59
Number of observations	5,831	5,831	5,831	5,540	5,540	5,299
p-value of pairwise coefficient comparisons between treatments:						
Teacher vs Nominating student	0.12	0.12	0.31	0.76	0.61	0.47
Teacher vs Non-nominating student	0.21	0.22	0.88	0.97	0.40	0.66
Nominating vs non-nominating student	0.79	0.77	0.48	0.62	0.75	0.77

The unit of observation is a plot. 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 teachers

	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)	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)	0.13	0.00
	(0.120)	(0.030)
Mean for treatment A teachers	7.39	0.96
Number of observations (teachers)	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 teachers

	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
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 nomination on SRI adoption

	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
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 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)	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

	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)	-0.23	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 SRI adoption

	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)	0.38	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 A1: Balancedness test on key baseline characteristics

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

	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	4.964	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 A3. 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/bundles	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 three 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 three 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	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.