The Effects of Exposure Intensity on Technology Adoption and Gains: Experimental Evidence from Bangladesh on the System of Rice Intensification*

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Abstract

We report the results of the first large-scale, multi-year experimental evaluation of the System of Rice Intensification (SRI), an innovation that first emerged in Madagascar in the 1980s and has now diffused to more than 50 countries. Using a randomized saturation design, we find that greater cross-sectional or intertemporal intensity of training exposure to the innovation has a sizable effect on Bangladeshi farmers' propensity to adopt (and not to disadopt) SRI. There is significant spillover learning from trained to untrained farmers. We find positive and significant impacts of SRI on rice yields, revenues, costs, and various household well-being indicators, and positive but imprecisely estimated impacts on rice profits. Average impacts of the technology are invariant to training exposure intensity, however, which seems to have mainly a scaling effect on diffusion of SRI, affecting performance with the technology only at the extensive margin, not the intensive margin. Our findings confirm many of the claims about SRI made based on observational data. But our results are inconsistent with economists' workhorse target-input model of learning in the literature on agricultural technology diffusion, although our findings are consistent with newer models of multi-object learning.

Keywords: agricultural development, diffusion, innovation, learning

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1. Introduction

Technological change in agriculture drives much of the structural transformation that defines the process of economic development in low-income agrarian nations (Gollin et al. 2002; Emerick et al. 2016; McArthur and McCord 2017; Mellor 2017). Such macro-scale effects require that new technologies boost productivity and well-being for individual farmers, and diffuse broadly through the farming population. Economists have therefore devoted considerable attention to agricultural technology adoption, and in particular to how farmers learn whether and how best to employ innovations.[1]

This paper makes two main contributions. First, we report the results of the first large-scale, multi-year, randomized controlled trial (RCT) evaluation of the System of Rice Intensification (SRI),[2] an innovation that first emerged in Madagascar in the 1980s and has now diffused to more than 50 countries (Styger and Traoré 2018). Widespread observational findings of dramatic rice yield gains have drawn sharp criticism from leading agricultural scientists incredulous at those claims, as discussed below. So, we conducted the first RCT of exposure to SRI training among 5,486 farmers in 182 villages in Bangladesh in 2014-15 and 2015-16, using a multi-year, randomized saturation design.[3] We find strong evidence that SRI uptake[4] increases with training exposure to the technology and that SRI uptake improves rice yields and various household well-being outcomes, consistent with the core claims made by the technology's proponents, although we find no consistent evidence that SRI increases rice profitability, as it also increases the costs of production. Given the extent to which SRI has diffused across the developing world without large-scale experimental evaluation, this is the paper's most direct, substantive contribution.

Second, our findings raise important questions about the way economists – and other social scientists – conceptualize learning about innovations. Our multi-year, randomized saturation design allows us to test two key hypotheses implied by the workhorse 'target-input' model of

[1] See the excellent reviews by Feder et al. (1985), Sunding and Zilberman (2002), and Foster and Rosenzweig (2010) for detailed discussion of these literatures.

[2] After fielding the experiment and analyzing our data, we learned of a parallel RCT on SRI in Haiti by Michael Carter, Travis Lybbert, Abbie Turiansky and collaborators at UC-Davis and OXFAM. As of yet, there is no paper reporting results from that field research.

[3] Multiple rounds of observations also allow us to filter out stochastic fluctuations in local growing conditions that may bias estimates of yield effects. Given that agricultural production depends on a large number of factors such as weather, irrigation, etc., multiple measurements over time also enables us to average out the noise in measuring yields and costs of agricultural production, generating estimates less vulnerable to bias due to unusual conditions for all subjects during the experimental period (McKenzie 2012; Rosenzweig and Udry 2019).

[4] We use the terms 'adoption' and 'uptake' interchangeably to refer to use of the introduced SRI technology, a key outcome variable in this study.

learning about a new technology, indeed by any model of learning about a single, performance-based object, such as expected profitability. First, the intensity of training exposure to the innovation in each of three dimensions – (1) randomly receiving SRI training versus untrained residents of a randomly selected training treatment village, (2) the randomized share of farmers within the village who randomly received training, or (3) the randomly assigned numbers of years of training received – affects only Bangladeshi farmers' propensity to adopt (and not to disadopt) SRI, but has no effect on farmers' performance with the new technology. These results are inconsistent with the target-input learning model that dominates the agricultural technology adoption literature in economics (e.g., Foster and Rosenzweig 1995; Conley and Udry 2010). These findings are, however, consistent with newer models of multi-object learning and technology diffusion, including rational inattention models that perhaps merit more attention in the agricultural technology adoption literature (Hanna et al. 2014; Nourani 2019). Second, we find widespread evidence of disadoption of SRI but find no evidence of heterogeneous returns to SRI. The fact that many farmers experiment with the method but then abandon it is also inconsistent with a target-input model of learning about how to most profitably employ a new technology, wherein learning by doing following endogenous adoption only further improves expected profitability, thus disadoption should not occur. Our findings do not let us formally test an alternative model of learning, although they are consistent with various models of multi-object learning. But they do clearly reject two core predictions of the target-input model on which much of the agricultural technology adoption and diffusion literature is based.

2. System of Rice Intensification

The System of Rice Intensification (SRI) offers an exceptionally good candidate innovation for studying questions about technology diffusion, learning processes, and impacts among smallholder farmers. SRI was originally developed for smallholder farmers in Madagascar in the 1980s, and has demonstrated considerable potential for raising rice yields without requiring additional purchased inputs (seed, fertilizer, etc.). SRI requires neither a new seed variety nor additional external inputs such as chemical fertilizers. Rather, the innovation involves a suite of principles designed to increase rice yields by changing the management of the plants, soil, and water.[5] In that sense, SRI is a "system" rather than a "technology" because it is less a fixed set of specific practices (e.g., transplant seedlings 12 days after nursery germination) than a package of principles (e.g., transplant seedlings early) for farmers to test, modify, and adopt as they deem

[5] For a brief introduction to SRI, see http://sri.ciifad.cornell.edu/aboutsri/origin/index.html. A large library of studies on SRI is available at http://sri.ciifad.cornell.edu/research/.

appropriate to their specific circumstances (Stoop et al. 2002).[6]

The core SRI principles involve early, careful transplanting of single seedlings with wider spacing into fields that have careful water management but are not continuously flooded, as well as soil that has more organic matter and is actively aerated, often using simple mechanical weeders. Practices that comply with these principles seem to improve the growth and functioning of rice plants' root systems and to enhance the number and diversity of the soil biota that contribute to plant health and productivity (Stoop et al. 2002; Uphoff 2003; Randriamiharisoa et al. 2006), although the exact bio-physio-chemical mechanisms behind the claimed performance improvements remain poorly understood by agricultural scientists. Some agronomists hold that SRI really represents the basics of good agronomy: full use of organic inputs, regular plant geometry, judicious use of water, good weed control, etc. thus the label really just reflects best management practices, not so much a new technology (McDonald et al. 2006). Indeed, the use of organic fertilizers, alternate wetting and drying (AWD) water management, and mechanical weeders are common recommendations, not unique to SRI. Each of those practices existed already (at varying prevalence levels) in our Bangladesh survey villages. The really distinguishing features of SRI are the three practices related to (i) much earlier transplanting, of (ii) a single seedling per spot, and (iii) much wider spacing among transplanted seedlings. Those were novel to our study sites and really distinguish SRI from other agronomic practices.

In this project, we adapt the approach taken by BRAC in Bangladesh through experimentation over the prior several years.[7] SRI is most appropriate during the *Boro* season (January-June), when irrigation management is easier. Heavy rainfall during the *Aus/Aman* season (April-August) makes careful AWD water management harder. As *Boro* coincides with winter, when plants grow more

[6] The distinction between broad principles and specific practices implicitly acknowledges heterogeneity in conditions that may necessitate customization of the specific practices to any particular farmer's context in order to adhere to the underlying principle(s) of the innovation. Agricultural and natural resources management researchers are therefore increasingly shifting from studying and promoting specific practices to broader principles instead (Stevenson et al. 2019).

[7] BRAC previously worked on SRI in two other sub-districts of Bangladesh, including a pilot project in association with Cornell University, on a small scale (among just 80 farmers). BRAC provided interest free credit (and in some cases grants) to farmers who agreed to adopt SRI. However, BRAC used a 'block' approach in which all farmers with neighbouring plots of lands within a village needed to agree to cultivate using the SRI approach. These blocks were typically large, 20-30 acres in size, and blocks often contained 50 or more farmers. It proved difficult for BRAC to convince such a large number of farmers to coordinate, especially around water management, so as to practice SRI. BRAC therefore wanted to change this approach, as it was neither financially sustainable nor effective. The approach we developed with BRAC, described in section 3, differs dramatically from its prior design. We did follow, however, the SRI principles that BRAC validated during these prior several years of experimentation elsewhere in Bangladesh. None of the control or treatment villages in our study were previously involved with BRAC efforts to promote SRI, nor with any other SRI promotion activities of which we are aware.

slowly, BRAC recommends transplanting seedlings when they are much younger than is the convention in Bangladesh, at about 20 days rather than 40-50 days, but a bit later than is typically recommended in SRI systems elsewhere (10-15 days). The basic SRI principles and specific practices advanced by BRAC in *Boro* season are: (1) transplanting younger (20-days-old) seedlings; (2) transplanting 1-2 seedlings per hill; (3) wide spacing of transplanted seedlings (25 × 20 cm); (4) providing organic matter amendments (e.g., compost, manure) to the soil; (5) following the AWD method of irrigation; and (6) mechanically weeding at regular intervals. The first three practices define SRI uptake as implemented in this study, with findings robust to different precise ways of measuring uptake, as we explain below.

Although SRI was first developed in Madagascar in 1983, not until the late 1990s and early 2000s did the proliferation of SRI across Africa and Asia prompt agricultural research scientists to carefully study the agronomy of SRI (Stoop et al. 2002). SRI has spread widely, to more than 50 different African and Asian settings, with strong backing from major international agencies such as Africare, OXFAM, WWF, and the World Bank. In a recent large-scale project across multiple countries in west Africa, more than 50,000 farmers were induced to try SRI (Styger and Traoré 2018). A very large number of observational studies, and some researcher-managed (not farmer-managed) experimental trials find SRI adoption associated with significantly higher yields, from 32-80 percent, and increased rice profits, even reduced water use and greenhouse gas emissions (Stoop et al. 2002; Barrett et al. 2004, Moser and Barrett 2006; Sinha and Talati 2007; Styger et al. 2011; Islam et al. 2012; Noltze et al. 2013; Takahashi and Barrett 2014; Gathorne-Hardy et al. 2016; Styger and Traoré 2018). Remarkably, there does not appear to be any prior large-scale experimental impact evaluation of SRI, however.

Despite widespread geographic diffusion of SRI and repeated observations of strong outcomes with the method, the conventional rice breeding community has remained vocally skeptical about the productivity gains attributable to SRI (Doberman 2004; Sheehy et al. 2004; Sinclair and Cassman 2004; Sheehy et al. 2005; McDonald et al. 2006). Indeed, criticisms of SRI by these distinguished agricultural researchers have been remarkably scathing, casting serious doubts about the veracity and replicability of SRI proponents' observational claims. Critics frequently decry the absence of experimental evidence from a large sample of farmers cultivating their usual fields, as distinct from purposively-collected samples, trials on experiment station plots, or observational data of non-random, natural diffusion processes. These "rice wars" led to calls for careful empirical work to assess the uptake and performance of SRI (Glover 2011).

Given the gains from SRI in observational data, one big puzzle concerns the surprisingly high rates of disadoption observed after farmers have tried the method. For example, only 25 percent of rice farmers adopted SRI in the rural Madagascar context from whence it originated, and 40 percent of those farmers subsequently ceased using the method (Moser and Barrett 2006). We likewise find high rates of disadoption of SRI after initial adoption. An oft-heard explanation for disadoption is that farmers face heterogeneous returns to SRI, leading to limited farmer experimentation by those who ex ante believe they might gain from SRI, and predictable subsequent disadoption by those dissatisfied with SRI's overall performance for them ex post of adoption. We find no evidence of such heterogeneity in returns to SRI adoption, however. High rates of disadoption are consistent with our estimates that the significant gains in rice yields reflect significant increases in input costs, resulting in no consistent, significant gains in rice profitability. Farmers might therefore be indifferent between SRI and equally profitable alternatives. Another candidate explanation is that because SRI is a knowledge-intensive innovation encompassing a suite of principles and practices that farmers must adapt to their specific context, it can be difficult to learn, consistent with our findings that farmers' performance with SRI does not seem to improve with increased exposure to the method. The learning challenge may be compounded if, at least initially, SRI proves more time-intensive than conventional rice cultivation methods (Moser and Barrett 2003, 2006; Berkhout and Glover 2011), as seems true in our data. Still another explanation is that the AWD method of irrigation requires careful coordination of water management among farmers cultivating adjacent plots. Our data do not let us test this latter, irrigation coordination hypothesis.

3. Learning Models in the Agricultural Technology Adoption Literature

Farmers must learn about new technologies in order to assess whether they should adopt the innovation and, conditional on adoption, to realize the innovation's full benefits. So how do farmers learn? This is a deep and complex question. While satisfactorily answering that question lies beyond the scope of this paper, our data do enable us to test two key predictions of the workhorse framework economists have used for decades to model the adoption and diffusion of agricultural technologies. In this section we briefly explain those predictions and flag alternative, recent learning frameworks that appear more consistent with our findings.

A rich literature on agricultural technology adoption in developing countries has followed the pathbreaking work by Foster and Rosenzweig (1995). In that seminal paper, they build on prior information theoretic work on learning (Wilson 1975; Jovanovic and Nyarko 1995) to model a producer choosing among multiple technologies that exhibit uncertain and endogenous profitability. The key innovation of Foster and Rosenzweig (1995) is to recognize that as farmers accumulate knowledge about a new technology – e.g., an improved seed, a new fertilizer, or a new suite of agronomic practices, like SRI – that information should help them steadily approach optimal expected practices, leading to higher expected profits. Each observation of an application of the new technology, whether by the farmer himself[8] – i.e., learning by doing – or by other farmers (or extension agents or agro-input dealers) – i.e., learning from others – creates another opportunity to learn and thereby to make better, more profitable production decisions. The greater a farmer's exposure to the technology – meaning, the more information he has received – the greater his learning about the true optimal practices and thus the greater his expected profitability with the innovation. A farmer optimally adopts the new technology only once he has learned enough that the expected profitability of adopting the new technology – including the future returns from learning by doing – exceeds that of sticking with the incumbent, traditional practices. As further information arrives through his own experience with the technology, he continues to learn and thereby improve his expected performance. The profitability of the new technology is thus endogenous to farmer learning and is itself the single object of that learning.

A considerable literature has built, explicitly or implicitly, on the Foster and Rosenzweig (1995) conceptualization of endogenous farmer performance with a new technology, with the optimal application of the technology – most commonly summarized in profits – as the unique object of learning. Conley and Udry (2010), in another seminal paper, offer a somewhat similar model of a stochastic profit function as the object of farmers' learning as they seek to optimize fertilizer application (in pineapple production in Ghana). New information arrives, from a farmer's own experiments with fertilizer and from observing the practices and outcomes of other farmers in his network. Farmers learn from those new observations and respond by adjusting fertilizer application rates. As they learn more, information-driven adjustments to practices lead to higher farmer profits.

Note that a central feature of these models – and many other recent papers that build on these – is that the sole object of farmer learning is a performance measure such as profit. This is a useful and powerful simplification that seems to fit well the data used by both Foster and Rosenzweig (1995), Conley and Udry (2010), and many others writing in this tradition. But this learning framework also carries some oft-overlooked implications, two of which are directly testable in our data.

[8] We use male pronouns to refer to farmers because all of our sample farmer respondents were male, as described below.

First, this model expressly assumes that the more farmers learn, the better they should perform with the new technology, on average. To quote Foster and Rosenzweig (1995, p. 1178), "the profitability of any new technology grows over time as knowledge accumulates." So one should be able to reject the null hypothesis that profit – or any measure that might equally be the performance-based object of learning, e.g., crop yield – is invariant with respect to a farmers' prior exposure to the technology in favor of the one-sided alternate hypothesis that performance improves with learning about the technology. Failure to reject the null that farmer performance does not improve with added learning calls into question the conceptualization of farmer learning as driven by a single, performance-based object of learning.

The second prediction is as stark as the first, but is only implied, not explicit. Adoption should occur if and only if the farmer has learned enough about how to apply the new technology optimally that he enjoys positive expected profits from adoption, up to the discounted future value of gains from subsequent learning-by-doing. And once a farmer has adopted, subsequent learning-by-doing should only further increase the expected profitability from the technology. Therefore, the farmer should never disadopt. Indeed, Conley and Udry (2010, p.62) expressly state that farmer "movements from positive to zero fertilizer use are mistakes." It could be that significant heterogeneity in the returns to a technology could generate disadoption (Marenya and Barrett 2009; Suri 2011). But in the absence of strong heterogeneity in returns – as is true in the case we study – the observation of non-trivial rates of disadoption of a technology, especially of a method that seems to generate significant gains on average, would expressly reject the conceptualization that farmers have a single, performance-based object of learning. Where significant disadoption of a new technology is observed, a single, performance-based object of learning cannot credibly explain adoption and diffusion patterns.

We can test those two hypotheses in our data. And we strongly reject both. But we do not offer an alternative model of learning in place of that workhorse model. Rather, we merely flag other models of learning that depart from the strong assumption of a single, performance-based object of learning and that do appear consistent with our findings.

One such thread of the literature posits multi-object learning that draws a key distinction between learning *whether* a new technology is likely to boost performance, versus learning *how* best to employ that same technology so as to boost performance (Fafchamps et al. 2016; Maertens et al. 2018; Banerjee et al. 2019; Nourani 2019). Learning whether it is likely worthwhile to try a new technology is commonly less costly than is learning how to use the technology to maximal effectiveness. Indeed, this is the fundamental problem of marketing. Sales agents aim to provide enough information to convince a prospective customer to try a product. The objective is not necessarily to optimize the customer's experience of the new technology. Similarly, agricultural extension agents directed (and rewarded) to promote uptake of a new technology that is, on average, superior, provide information that helps induce farmer uptake. But more exposure to that same information does not necessarily improve farmer performance with the new technology nor induce farmers to adjust their practices to optimize their experience with it. This distinction between single object learning about performance and multi-object learning matters to the design of effective agricultural information systems, such as public extension services.

A related thread of this literature further hypothesizes that in the face of costly learning, agents might fail to improve in their performance with a technology because they do not pay attention to the right pieces of available information. Such 'rational' or 'selective' inattention models (Gabaix et al. 2006; Schwartzstein 2014; Hanna et al. 2014; Ghosh 2016; Gabaix 2017; Wolitzky 2018) generate a prediction similar to multi-object learning models. Increased information access may favourably impact uptake but not performance. Having learned that a new technology is, on average, more productive than one's traditional practice, a farmer might rationally confirm that belief with further information, doubling down on the initial adoption choice, but not paying attention to other available data that might help him improve his performance with the technology. Hanna et al. (2014) study Indonesian seaweed farmers who optimize with respect to some of many production choice variables but underperform by failing to notice and adjust a key technology feature. As with SRI, seaweed farmers face a suite of multiple variables that matter to performance and might not be able, or willing, to pay attention to them all. In a world of selective inattention, greater farmer exposure to information may reinforce beliefs that lead farmers to try a new practice, like SRI, yet farmers might not pay attention to key information as to how they might improve their use of the new technology, perhaps especially if they enjoy gains from the new practice. This can result in satisficing-like behaviour. We cannot directly test whether Bangladeshi rice farmers pay attention to particular pieces of information or not, but we can demonstrate how modestly they change practices in response to additional information.

Ultimately, we find strong evidence against the dominant learning framework found in the economics literature on agricultural technology adoption. We find high rates of disadoption and document a novel empirical finding that performance with the new technology is impacted almost exclusively at the extensive margin of adoption, which is itself highly responsive to the intensity of exposure to information on the new technology. Although we cannot identify the right learning model in our data, recent innovations in models of multi-object learning and selective or rational inattention are broadly consistent with our findings.[9]

We find strong support for the hypothesis that SRI uptake increases with exposure. Directly trained farmers are more likely to adopt SRI than are untrained farmers who live in training villages, although these untrained-but-exposed farmers are significantly more likely to adopt SRI than are farmers in control villages. Among directly trained farmers, a second year of training further increases trainees' propensity to adopt SRI and decreases their likelihood of disadoption, relative to otherwise-identical farmers directly trained only in the first year. The effect on SRI adoption increases with the saturation rate within a training village, especially among untrained farmers indirectly exposed in villages with sustained, rather than one-off, training and higher saturation rates.

But farmers scarcely adjust their behaviors to greater exposure to information about SRI. They appear to be learning that SRI is profitable and that they should try it, and are more likely to learn that lesson the more exposure they have to SRI training. The farmers in our study are either not paying attention to information received that could improve their performance with SRI or the SRI training really helps only with one object of learning: the discrete adoption decision and not the finer-grained production decisions that drive productivity and profitability outcomes. Although greater exposure generates a strong scaling effect that translates into greater diffusion, it has only modest effects on compliance with the specific principles that extension agents taught farmers, and no significant effects on outcomes conditional on uptake, whether in terms of rice yields and profits, or household welfare indicators.

This suggests that intensity of exposure does not, in this case, reflect learning about *how* best to use the technology, only about *whether* to use the method. The distinction is a subtle but important one as promoting diffusion of a technology is different from advancing optimal performance as the method diffuses. Extension services in the developing world commonly organize (and are evaluated and compensated) simply around diffusion of new methods, rather than around farmers'

[9] Wolitzky (2018) shows that for outcome-improving innovations, such as the one we study, adoption increases the greater one's exposure to (i.e., data on) the new technology. He also demonstrates how rational agents can fail to learn from more observations of cost-reducing technologies. That latter result is superficially consistent with our empirical findings, except that the technology we study is demonstrably outcome-improving and cost-increasing.

performance in employing new methods. That approach works in a target-input world, in which new technologies are uniformly superior to traditional ones, as learning sufficient to induce uptake will necessarily lead to continued learning and further productivity and profitability improvements. But in a world of multiple objective learning, rational inattention, or both, the learning that induces uptake may not generate satisfactory productivity or profitability with the innovation. Farmers can persist in implementation errors or even disadopt when disappointed by their (perhaps suboptimal) performance with the new technology.

4. Experimental design

The RCT was conducted in 182 villages across five districts[10] in two successive years (2014/15 and 2015/2016) during the *Boro* rice seasons. We used a multi-stage randomization in year 1 (2014/15). First, 120 villages were selected randomly for training to introduce farmers to SRI; the remaining 62 villages served as controls and received no SRI training. BRAC already operates in these villages for other activities, so is a well-known and respected organization in all of the survey communities.[11] SRI was not previously practiced in any of these villages, nor in neighbouring villages.

While the SRI intervention was coordinated by BRAC's Agriculture and Food Security Program (AFSP), the research reported here was 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 organisational nature of RED and AFSP helps researchers to conduct independent and credible experimental evaluation of any BRAC intervention, for example, BRAC's well-known ultra-poor (Bandiera et al. 2017), and tenant farmers credit programs (Hossain et al. 2019), and in our case its SRI intervention program.[12]

[10] The five districts are Kishoreganj, Pabna, Lalmonirat, Gopalgonj and Shirajgonj.
[11] BRAC works across all of Bangladesh, offering a range of education, health, microfinance, skills training and legal support services.
[12] We acknowledge that BRAC's prior and ongoing presence in the study villages is both an advantage and a prospective source of bias. Because BRAC had previously worked extensively with these communities, albeit not on SRI, they were (and remain) a trusted partner. This enabled implementation of this study with very high rates of compliance, leading to the very clean results reported in the appendix balance and attrition checks. On the other hand, an implementing organization's prior relationships with study subjects can causally increase the estimated impact of an intervention (Usmani et al. 2018). Trust in BRAC might introduce an upward bias in uptake rates, relative to diffusion from a random source, if farmers place greater weight on information from BRAC than from other agencies. Likewise, upwardly biased ITT estimates could arise if farmers (mistakenly) perceive any *quid pro quo*, that if BRAC is promoting SRI a farmer had better try it or risk losing out on other services BRAC provides. Note, however, that if anything, that would downwardly bias LATE estimates of the causal impact of SRI on rice or household outcomes.

At the very beginning of the research program RED conducted a census before the 2014 *Boro* season to list all farmers in the sample villages who cultivated rice in the previous *Boro* season, and owned at least 0.5 but not more than 10 acres of land.[13] We then selected 30-40 farmers randomly from each village, including the controls, for the baseline survey.[14] In total, we surveyed 5,486 farmers, 1,856 from the control villages and 3,630 from the treatment villages (Appendix Table A1).

The second stage randomization involved selecting farmers randomly for SRI training within each of the 120 treatment villages. The SRI training took place just before the season started each year (during November-December of 2014 and 2015). In the first year, BRAC provided training and information on SRI following its standard farmer training curriculum model.

The number of farmers trained within each treatment village varied randomly following a randomized saturation design that generates experimental variation in the (cross-sectional) intensity of within-village-and-period exposure to treatment among treated farmers and villages. We randomly varied the number of farmers trained between 10 and 30 (Figure 1), so that the fraction of sampled farmers in training villages ranges from 25-80 percent, averaging just over 60 percent of the sample farmers in training villages. Randomized saturation can help identify spillover effects from the treated to the untreated (Baird et al. 2018), although as we show, it can also affect the treatment effect on the treated. In total, 2,226 farmers received the standardized SRI training in 2014. Another 1,404 reside in treatment villages but did not receive BRAC SRI training (Table A1).[15, 16]

Following the selection of farmers for training, local BRAC AFSP field workers and RED enumerators visited the farmers' homes and presented them with a letter from BRAC inviting them to a one day SRI training. The farmers were also briefly informed orally about the purpose of the training. All farmers received a fee (BDT 300≈4 USD) to participate in the training, worth slightly

[13] Farmers with less than 0.5 acres of land were excluded as they are usually seasonal farmers. Those with more than 10 acres are considered land rich in this context and not part of BRAC's target clientele.

[14] In case of a few very large villages, we divided the village into two or more paras/neighborhoods for both the baseline survey and the training. We surveyed only one neighborhood from each such village such that the farmers are geographically close to each other, mimicking more typical village settings.

[15] The random selection of villages and farmers was done by computer using STATA to make sure the randomization was conducted blindly without any influence of BRAC AFSP officials.

[16] The treatment villages were also divided equally into different categories to disseminate information and uptake of SRI. For more details see Fafchamps et al. (2017). The trainings were same across all the villages.

more than the average daily wage.[17] The training content was standardized across villages, involving both oral and multimedia presentations, including a video demonstrating the principles and practices of SRI used in other areas of Bangladesh, and interactive question-and-answer sessions to clarify the practices and principles. The trainers were existing AFSP agricultural officers trained by agricultural scientists who had previously worked on SRI elsewhere in Bangladesh.[18] The trainers were supported by RED enumerators and AFSP field workers in conducting the training session and the pre- and post-training interviews, while all other surveys (census, baseline, midline and endline surveys) were done by RED enumerators.

The third stage randomization occurred in the second year, generating experimental variation in the (intertemporal) intensity of exposure among treated farmers and villages. In 2015/16 (year 2) AFSP repeated the training only in half (60) of the treatment villages, selected randomly from the 120 year 1 treatment villages, inviting all (and only) the farmers who were offered training in year 1. Year 2 training consisted of two one day sessions. In the first session, case studies on successful adoption from first year of intervention were discussed. The session also included discussion with local farmers about the training in year 1 and rice cultivation practices as well as constraints that affected their decision to adopt (or not adopt) SRI in year 1. In the 2nd session, AFSP trainers provided the exact same training as in year 1, and also tried to make sure farmers clearly understood the key principles and practices of SRI. In the remaining 60 villages treated in year 1, there were no follow up training or information sessions. No training or information about SRI was provided in the control villages in either year.

The experimental design involves randomization of villages into treatment and control, and then of farmers into treated and untreated within treatment villages, coupled with randomized saturation within treated villages, and randomized repetition of training for a second year. This enables us to estimate the causal effects of SRI training (the experimental treatment) on SRI uptake and disadoption rates as a function of the cross-sectional and intertemporal intensity of treatment. This design also permits us to estimate the spillover effects of SRI training on control (untreated) farmers within treatment villages, again as a function of cross-sectional and intertemporal intensity of treatment. Because we find that SRI training sharply increases the likelihood of adoption and decreases the likelihood of disadoption (as we discuss below), precisely as one would expect, and that there are spillover effects on uptake by untreated farmers in treatment villages, we can then

[17] In addition, the farmers were given lunch, refreshments and snacks for the day. All farmers who attended the training were also given a certificate from the BRAC in recognition of their participation in the training.
[18] These scientists previously worked at the Bangladesh Rice Research Institute (BRRI).

use the randomized intensity of exposure as an instrument to estimate the local average treatment effects (LATE) of SRI uptake on rice productivity, costs of production, profitability and broader indicators of household well-being.

We label the farmers that received training in both years 1 and 2 as T2 (for two rounds of training), and their villages as V2. The farmers that received training in the remaining 60 treatment villages in year 1 only we label T1 (for one round of training) and their villages V1. The surveyed farmers who were not selected for SRI training but reside in the V2 villages we label U2 (for untreated in two training rounds villages), with U1 the analogous group of untreated farmers in the V1 villages where SRI training occurred only in year one. The control village farmers are denoted C. Comparisons among these five randomly assigned groups enables causal inference.

This two-dimensional (cross-sectional and intertemporal) randomization of intensity of treatment appears novel in the literature. Randomization in treatment intensity in the cross-section (i.e., within the village) generates exogenous variation in the number of prospective members of one's social network trained in the SRI, which may enhance learning from others, social acceptability, awareness of SRI, etc. In the time series dimension, the randomized intensity of training manufactures exogenous variation over time in exposure that could represent opportunities to learn through formal training and discussion with experts, sustained exposure to a message, a useful reminder, etc.

Appendix Table 1 reports sample sizes in the two years of intervention. Most of the invited farmers who were present in the villages on the training day attended the training. Only 4 farmers in year 1 refused to participate in the training while 3 farmers in year 2 did not attend the training. Thus, overall we do not have a compliance issue with take-up or participation in the training program. There is some attrition in the sample over time, which we discuss below.

Before beginning any SRI training, we conducted a baseline survey among all control and treatment households. Then, following each year's training, after each seedling transplant period but still during the growing season, we conducted a short survey to observe compliance with SRI practices and principles. SRI adoption was determined on the basis of plot visits by RED enumerators who were also supported by AFSP field workers, who verified visually whether the farmer adopted SRI techniques on any of his cultivable rice plots during the *Boro* season. A farmer is considered an SRI adopter if the BRAC field officer observed that the farmer practiced at least three of the six key SRI practices on at least one plot of land. Note that we use the mid-season verified observations of SRI practice, not farmer self-reports, when studying SRI adoption and disadoption. As a robustness check, we also use several other definitions of adoption, but find no qualitative differences among measures.[19]

Following each year's harvest, RED conducted a thorough survey to capture further details on rice inputs and output, as well as lots of background on the households, including various measures of well-being that we discuss below. The outcome measures we study – other than SRI adoption or disadoption – come from these post-season surveys.[20] Therefore, besides the baseline survey data and the mid-season checks on SRI uptake, we have household survey data for two more post-harvest rounds, one at the end of year 1 harvesting season (midline) and at the end of year 2 harvesting season (endline).

In the appendix, we provide evidence that the randomization was successful in both years (Tables A2, A3, and A4). Indeed, as shown in Figure 2, not only was there no mean difference between control and the four (i.e., T1, T2, U1, U2) treatment groups in rice yields at baseline, the distributions were effectively indistinguishable, with no stochastic dominance of any order among them. Appendix Tables A5 and A6 also indicate attrition is not a significant concern in this study.

5. Empirical Strategy

5.1 ITT and LATE estimates

We use random selection into one of the four non-control group categories as dummy variables (T1, T2, U1, U2) to estimate intent to treat (ITT) effects of SRI training. The randomized saturation design also enables us to exploit the continuous variation in treatment intensity to estimate treatment effects as a function of intensity of exposure, in cross-section, time series, or both.

Our main rice-specific outcomes of interests are SRI adoption, and yields, costs of production, revenue, and profits, defined as the difference between revenue and costs. SRI is arguably more labor intensive, and observational studies frequently suggest that SRI farmers might engage more family labor in rice cultivation (Moser and Barrett 2006). Since labor valuation can be problematic

[19] In particular, we also use farmers' self-assessed SRI adoption, as reported in the post-harvest survey, enumerators' evaluation of the extent of SRI adoption on a scale of 0 to 100, and the percentage of cultivated rice land under SRI. None of these alternate measures meaningfully change any of our results.

[20] RED conducted multiple visits after transplanting season to measure SRI adoption (or disadoption) considering different principles of SRI needed to be verified at different points in time. Hence, the sample size on adoption is different from post-harvest survey.

in settings where most labor is not hired, we consider costs with and without family labor so as to ensure that findings are not distorted by unobserved heterogeneity in shadow wage rates.

We first estimate the ITT effects of exposure to SRI training. Let T_{i1}=1 if a farmer i is trained and lives in village j that was treated only in year 1 (V1 village), T_{i2}=1 if the farmer is trained and lives in a village treated in both years (V2 village), U_{i1} = 1 if the farmer lives in a V1 village and was not trained, and U_{i2} = 1 if he lives in a V2 village and was not treated; all variables take value zero otherwise. These groups are mutually exclusive by design. To estimate the ITT effect of offering SRI training we run the following analysis of covariance (ANCOVA) estimation[21]:

$$Y_{ij,post} = \alpha_1 + \delta_1 Y_{ij,base} + \beta_{11} U_{i1} + \beta_{12} T_{i1} + \beta_{13} U_{i2} + \beta_{14} T_{i2} + \Pi_1 X_{ij} + \varepsilon_{ij} \qquad (1)$$

In equation (1) $Y_{ij,post}$ is the endline outcome of interest (e.g., SRI adoption, rice yields, cost, profits, etc.) for farmer i in village j at the end of year 2; $Y_{ij,base}$ is the corresponding pre-intervention (baseline) level outcome; X_{ij} includes control variables such as age, education of farmer, land size, household composition, and income (an imperfect proxy for liquidity constraints and risk preferences). For rice-focused outcome variables – SRI adoption, yields, costs, profits – we use plot-specific observations, but we omit the plot subscripts from equation (1) because for household well-being indicators we use household-specific observations. The parameters β_{12} and β_{14} estimate the two year (i.e., endline) ITT effects of receiving SRI training once (T1) and twice (T2), respectively, whereas the β_{11} and β_{13} parameters are the ITT estimates of the spillover effects of living in one-time and two-time training villages, respectively.[22] The omitted category is control villages in which SRI training was not available in either round, for which $U_{i1} = U_{i2} = T_{i1} = T_{i2}$ =0.

Equation (1) also allows us to test if there is any incremental effect of receiving an additional year of training by comparing the ITT estimates between T_1 and T_2 farmers, i.e., testing the null hypothesis that $\beta_{14} - \beta_{12} = 0$ versus the alternate hypothesis. We can likewise investigate whether repeated training induces faster diffusion or improved spillover outcomes than one-time

[21] McKenzie (2012) explains why ANCOVA estimation has more power than more conventional difference-in-differences estimation, and therefore is generally preferred, especially for outcomes with relatively low autocorrelation. The autocorrelation in outcome measures varies from -0.08 to 0.22 for our outcome measures, with the lone exception of midline-to-endline adoption (0.55), where the baseline-to-midline autocorrelation is necessarily zero since there was no SRI cultivation at baseline.

[22] Because more than 99% of those randomly selected for training attended the training, the effects of treatment on the treated (TOT) are effectively the same as the ITT effects, so we ignore the TOT effects here.

training by comparing U_1 with U_2 farmers, i.e., testing the null hypothesis that $\beta_{13} - \beta_{11} = 0$ versus the alternate hypothesis.

We exploit the randomized saturation design to also estimate equation (1) using the intensity of treatment (fraction of farmers treated in the village) instead of treatment status dummies (e.g., U1F instead of U1). Figure 1 shows the number of farmers treated across treatment villages. We define the treatment intensity (T_{ij}) for a farmer i living in village j as the share of the village sample treated:

$$T_{i,j}^{treated} \equiv \frac{N_{i,j}^{treated}}{N_{i,j}^{treated} + N_{i,j}^{untreated}}$$

where $N_{i,j}^{treated}$ refers to the number of treated farmers, and $N_{i,j}^{untreated}$refers to the number of untreated farmers in village j. Then the continuous treatment intensity variables are simply the product of the village-level treatment intensity and the individual group assignment, e.g., $T2F_{ij} \equiv T_{i,j}^{treated} * T_{i2}$. If we instead use estimates of village population – which is less precisely measured, thereby introducing measurement error into the exogenous intensity variable – we get qualitatively identical results.

Some farmers received SRI training but did not adopt the practice. Conversely, some farmers from treatment villages did not receive the training but nevertheless adopted SRI. In order to estimate the causal effects of SRI adoption on various outcomes of interest, given endogenous adoption of SRI, we estimate the Local Average Treatment Effect (LATE) using the treatment status (and treatment intensity) as an instrument for adoption. We estimate the LATE via the following regression:

$$Y_{ij,post} = \alpha_2 + \delta_2 Y_{ij,base} + \beta_2 \widehat{Adoption}_{ij} + \Pi_2 X_{ij} + \vartheta_{ij} \qquad (2)$$

where, $Adoption = 1$ if farmer i in village j adopted SRI in year 2 (endline). The first-stage involves using randomization into different treatment arms as the instruments for SRI adoption. Standard errors are always clustered at the village level. Equation (2) is our preferred specification for estimating the impact of SRI adoption.

Note that a farmer could cultivate rice in more than one plot of land. He might have selected a particular plot suitable for SRI.[23] The SRI plot could well differ from non-SRI plots along multiple unobservable dimensions. We address this issue by including pre-intervention level plot-specific yield, cost, and profit in the regression as, for example, higher quality of land of a given size should have higher yield in the absence of SRI.

Because we observe multiple plots per household,[24] in discussing farmer adaptation of SRI principles over time and in robustness checks we also use a plot difference-in-differences (DiD) estimator to control for unobservable, time invariant plot characteristics. More precisely, we estimate a plot-level panel regression using all three waves of data (i.e., baseline, midline, endline):

$$Y_{ipjt} = \alpha + \sum_{t=1}^{2}\sum_{j=1}^{2} \partial_{jt}\, U_{ijt}\pi_t + \sum_{t=1}^{2}\sum_{j=1}^{2} \gamma_{jt}\, T_{ijt}\pi_t + \sum_{t=1}^{2} \pi_t + \vartheta_{ipj} + e_{ipjt} \qquad (3)$$

where Y_{ipjt} represents an outcome variable of interest from farmer *i* cultivating plot p in village type *j* at time *t*, where time period refers to 2014 baseline (*t*=0), 2015 midline (t=1) and 2016 endline (*t*=2). U_{ijt}=1 for untrained farmers in village type *j*=1 (one year of training) or *j*=2 (two years of training) post-baseline (i.e., $t \in \{1,2\}$); T_{ijt}=1 for trained farmers in village type *j*=1 (one year of training) or *j*=2 (two years of training) post-baseline; π_t is a period fixed effect common to all farmers – capturing, for example, annual average prices and growing conditions – and ϑ_{ipj} is a plot fixed effect. So now the indicator variables reflect both the treatment arm to which a farmer – and thus his plot – was assigned (the first two characters, as we already use that notation) with the survey round appended as a third, numeric character. For example, U11 and U12 indicate an untrained farmer in a single treatment village at midline and endline, respectively.[25] In these regressions, ∂_{jt} is the ITT estimate of the impact of the SRI training on the outcome variable for the untrained farmers indirectly exposed to SRI in training villages of type *j* in period *t*, while γ_{jt} is the ITT estimate of the impact of SRI training on the outcome variable for farmers randomly selected into SRI training. These estimates compare changes in outcomes among trained and untrained farmers residing in training villages post-SRI training, relative to changes among

[23] Barrett et al. (2004) found that farmer and plot characteristics account for more than half of the observed yield difference between SRI and traditional rice plots in Madagascar.

[24] We obtained information for at most three plots of cultivable land. If a farmer cultivated more plots, we randomly picked three plots on which to collect the information. 65.7% of farmers have 3 plots in the sample, 21.1% have 2 plots, and 13.2% have just 1 plot. We collected adoption, yield, input cost, and revenue data for the same plot(s) of land from each farmer in all three rounds. Plots were visited with farmers to verify SRI uptake and the correspondence of the plot to the prior round of data.

[25] We also estimate equation (3) using continuous treatment intensities in place of the treatment dummies.

farmers in control villages, while controlling for average period-on-period changes. This method also has the advantage of controlling for unobserved, time invariant (household and) plot level heterogeneity (e.g., in drainage, slope, soil type, distance from the home, farmer skill) that may impact productivity, costs, etc. Standard errors are also clustered at the village level. The other benefit of this estimation strategy is that it disaggregates the effects between midline – the impact of initial exposure – and endline – so as to capture the impact of sustained exposure to SRI, through additional direct training to T2 farmers, or through additional indirect exposure via trainees and adopters in both V1 and V2 villages. Relative to the ∂_{11} and γ_{11}parameter estimates, ∂_{12} and γ_{12} reflect learning-by-doing and learning-from-others effects entirely within the village, without any further BRAC training in the *j*=1 villages. By contrast, the ∂_{22} and γ_{22} parameter estimates will include those same intra-village learning effects as well as any marginal benefits from the second year of BRAC training in the V2 villages.

5.2 Disadoption and delayed adoption

Our research design also allows us to study *disadoption* – SRI uptake in year 1 that is discontinued in year 2 – and *persistent adoption* — i.e., farmers who practiced SRI in both years 1 and 2. We can also identify farmers who adopted SRI only in year 2 – *delayed adopters* – and see how they differ from disadopters and persistent adopters. Foster and Rosenzweig (1995) point to the strategic gains from delayed adoption if one can observe neighbors' experimentation with a new technology. If learning from others is cheaper than learning by doing, then farmers with less capacity or willingness to experiment (e.g., lower education, less financial liquidity, smaller farm size, etc.) may be more likely to delay adoption. Because adoption is endogenous to various farmer-level unobservables, we cannot make causal inferences around disadoption, persistent adoption, and delayed adoption, but we can examine the correlates associated with each cohort.

More precisely, we study the association of yields, costs, farm size and farmers' characteristics with persistent adoption, disadoption, and delayed adoption using the multinomial logit regression model

$$f(Y_{ij}) = \alpha_4 + \delta_4 Yield_{ij,base} + \beta_4 Cost_{ij,base} + \theta_{11}U_{i1} + \theta_{12}T_{i1} + \theta_{13}U_{i2} + \theta_{14}T_{i2} + \Pi_4 X_{ij} + \mu_{ij} \qquad (4)$$

where Y_{ij} is a dummy variable indicating the status of adoption of farmer *i* in village *j* at the end of year 2: persistent adopter, delayed adopter or disadopter. The omitted base category is never-adopters. The polychotomous options are mapped by the multinomial predictor function f(·) onto the explanatory variables. The X_{ij} vector includes the household head's age, education, income, and farm size. $Yield_{ij,base}$ reflects baseline productivity, $Cost_{ij,base}$ is baseline cost, since a farmer's propensity to continue to practice SRI, or to delay adoption, should depend on his initial conditions.[26, 27] We also separately run a logit regression model comparing only disadopters with persistent adopters, i.e., looking just at the (non-random) sub-sample of midline adopters. This helps identify the correlates of those who abandon the practice while their neighbors continue.

6. Results

6.1 Summary Statistics: Adoption, Yields and Profits

Table 1 presents midline and endline (i.e., post-treatment) summary statistics for each of the five treatment arms. As seen in panel A, SRI training caused statistically significant SRI adoption. Uptake rates at midline were almost identical among T1 and T2 farmers, at 49.7 and 49.2 percent, respectively, as compared to a true 0 among the control village farmers. But SRI adoption then fell among T1 farmers by endline, to 38.9 percent due to disadoption (on which, more below), while adoption among T2 farmers increased further, to 53.0 percent, due to delayed adoption and learning. The endline difference between the two groups is statistically significant (p=0.00) although the midline adoption rates, when there was no difference between the two arms, are not significantly different. The same pattern holds between the U1 and U2 treatment groups, the training village farmers who were not themselves trained. SRI uptake increases more among U2 farmers with the added exposure in year 2. Overall, farmers adopted SRI on about 26% of the land in V2 villages in year 2 as compared to 22% of land in V1 villages (Panel B). Even adopters appear to experiment with SRI, not fully adopting it on all of their plots.[28] A qualitatively identical story emerges if we use the other adoption measures.

We also find that the extent to which farmers adhere to the principles of SRI varies by treatment exposure and intensity, but is generally fairly low. As Table 1 Panel B shows in reporting percentage of farmers who followed the SRI principles as taught, a larger share of farmers exposed to the SRI training (T1 and T2 farmers) followed each of the six rules of SRI cultivation than those who were not exposed (UI and U2 farmers). With the exception of mechanical weeding, at endline

[26] Note that we omit control villages from this regression as there was no SRI adoption in control villages.
[27] We repeated the same analysis using the continuous treatment intensity measures in equation (4) with no qualitative change in results, which are available by request.
[28] At endline, only 6.8% of T2 farmers and 3.5% of T1 farmers adopted SRI on all of their rice land.

(but not at midline) we also find that farmers who were trained twice were more likely to follow the principles as taught than were those who got the training only once. There was no significant increase in compliance with practices from midline to endline by farmers in V1 villages, except for U1 farmers' use of organic fertilizer and T1 farmers' space of seedlings and mechanical weeding. This suggests very little learning by doing or even learning from other farmers' initial experiences with SRI. The increase in compliance from midline to endline was consistently stronger in the V2 villages, among both U2 and T2 farmers. The key takeaway from the descriptive statistics on compliance with the SRI principles taught is that the vast majority of farmers who adopt SRI practice it on only part of their rice land, and they only partially comply with the principles as taught, with only modest updating from midline to endline among those exposed, directly or indirectly, to a second year training session.

Panel C of Table 1 reports the summary statistics on rice yields, revenues, cost and profits. Yields, costs, revenues, and profits are all higher in the treatment villages than in the control villages, but the profit impact estimates are imprecise and thus not statistically significantly different from zero. Yields and costs are not statistically significantly different between V1 and V2 villages, although gross revenues and profits are a bit – 2 and 8 percent, respectively – higher in V2 villages, each statistically significant (p=0.03). The simple descriptive statistics suggest significant productivity - and insignificant but positive expected profitability – impacts of exposure to SRI, although the differences among treatment groups are not often statistically significant. This suggests that the main impact of exposure to SRI occurs at the extensive margin, in inducing adoption more than by impacting performance through the enhanced learning opportunities that come from greater exposure.

6.2 Estimated Treatment Effects

Table 2 (Panel A) column 1[29] reports the ITT estimates of the effects of SRI training at endline. SRI training appears quite effective at inducing adoption, as T1 and T2 farmers are 39 and 53 percent, respectively, more likely to practice SRI two years after baseline than were farmers in control villages. Under the defensible assumption that participation in SRI training represents more intense exposure to the method than non-participation does, exposure intensity clearly matters to uptake. Training is the main mechanism for diffusing the method as the differences

[29] The post-harvest survey used for outcomes other than adoption included fewer rice plots than did the post-transplanting survey used to determine SRI adoption. Column 1' reports the analog to column 1, but restricted to just the observations used for the other outcome variables. No significant differences exist between the results. So we focus our discussion on the larger sample reported in column 1.

between the treated and untreated farmers within villages (T1 vs. U1 and T2 vs. U2) are quite large – roughly 30 percentage points in V1 villages, 40 in the V2 villages – and highly statistically significant. Repetition of SRI training had a statistically significant effect on uptake as well, as the 14 percentage point difference in adoption between T1 and T2 farmers is statistically significant at the one percent level. The likelihood of uptake is significantly increasing in the intertemporal intensity of exposure to SRI, both from zero exposure to one training session and from one annual training to two years of training.

There is also statistically significant spillover of SRI training to untrained farmers in the treatment villages, of 9 and 13 percent among U1 and U2 farmers, respectively. The 4-percentage point increase within V2 villages, as compared to V1 villages, in the diffusion of SRI beyond the trained farmer cohort is not statistically significant, however. SRI training is effective in inducing uptake, including by untrained farmers – presumably through social spillovers within the village – and increasing the within-village and intertemporal intensity of exposure to the innovation. Recall that the training sessions were only one day long and farmers had no prior exposure to SRI. Considering this, we consider the estimated uptake impacts quite a robust response to relatively modest exposure to the new method, and despite its relative complexity.

The ITT estimates also show statistically significant impacts on rice yields, revenues and input costs (per decimal of land) of 13-17 percent relative to farmers in the control villages. Remarkably, no statistically significant differences exist between treated and untreated farmers, nor based on the number of SRI trainings. These ITT estimates are broadly consistent with prior observational findings that SRI induces increased household labor effort that erases some of the apparent gains from increased physical rice yields (Moser and Barrett 2006; Noltze et al. 2013; Takahashi and Barrett 2014).[30]

The ITT estimates of the impact of SRI training on rice profits, computed either with or without taking family labor costs into consideration,[31] suggest an increase by a similar magnitude as yields

[30] It bears noting that unlike prior studies in sites (e.g., Indonesia, Madagascar, Timor Leste) with very hilly topography that necessitated considerable time spent leveling rice fields for good practice of AWD water management, our study villages in Bangladesh exhibit little slope. So, the added labor demands in land preparation were fairly minimal in these sites. Our estimated labor impacts might therefore be low relative to other locations with more sloped lands.

[31] At midline, wage data were collected for each individual worker in a sample household, by different types of work, including nursery bed preparation/seeding/seed treatment; land preparation; transplanting/sowing; irrigation/watering; weeding; applying fertilizer/pesticide; harvesting; and post-harvest threshing. Because the differences by task were minimal, at endline, wage data were collected only for two categories: the pre- and harvest/post-harvest periods.

and revenues, 4-23 percent, again without any statistically significant difference among the treatment arms based on intensity of exposure. But the profit effects are imprecisely estimated and never statistically significant at the five percent level. Although intensity of exposure to SRI training has a large and statistically significant effect on the likelihood of SRI adoption, intensity of exposure has no differential ITT impact on outcomes across groups nor does it unambiguously enhance rice profitability. We return to this point below when we discuss disadoption patterns.

Because less than half of trained farmers adopted SRI, these ITT estimates suffer from non-random compliance bias as estimates of the gains conditional on SRI adoption. Panel B of Table 2 therefore presents the LATE estimates of the effects of SRI adoption. The first-stage uses treatment dummies as instruments for SRI adoption (i.e., equation (1), with estimates reported in column 1 of panel A). We observe large and highly statistically significant gains in rice yields, revenues, and costs, all of 21-26 percent, but statistically insignificant (but positive) estimated effects on rice profits. Those point estimates fall in the lower range reported in the observational SRI literature summarized earlier. These are considerable yield gains from uptake of an innovation that requires no purchased inputs, but they seem to come mainly from increased family labor application, as we discuss below.[32] And the lack of consistent expected profit gains and increased labor input demands help explain the puzzle of high rates of SRI disadoption.

The same pattern holds when we replace the binary treatment variable with the continuous measure of village-specific treatment intensity, exploiting our randomized saturation experimental design.[33] Table 3 panel A presents the ITT estimates for this continuous treatment measure. A 1.00 percentage point increase in the treatment intensity increases adoption by 0.65 and 0.84 percent among T1 and T2 farmers, respectively. The difference arising from the extra year of training is statistically significant, signalling that treatment intensity matters along both the intertemporal and cross-sectional margins.

There remain statistically significant spillover effects to untrained farmers within the treatment villages and those effects are increasing in the treatment intensity in the village. A 1 percentage point increase in the treatment intensity increases adoption by untrained farmers by 0.16 and 0.24

Imputed wages for family labor during were calculated based on the average hired wage in the district for each type of work. Average hired and imputed wage for midline was 301.48BDT/day and for endline was 313.27BDT/day.

[32] We get statistically significant positive results on all outcomes when we perform the same analysis using midline data but using endline treatment status (Table A7).

[33] Note that treatment intensity is necessarily zero for all farmers in control villages.

percent in the one- and two-year training villages (i.e., U1 and U2), respectively, although the difference between the two is statistically insignificant. Those spillover effects remain statistically significantly less than the adoption impacts of receiving training directly.

Using the results above we can calibrate what might happen if one offered training to more farmers in the treatment villages. On average, 18 farmers out of 30 were trained (a treatment intensity of 0.60) in a treatment village. Training an extra 6 farmers, to increase the treatment intensity to 0.80, would boost overall adoption by 16 and 22 percent in V1 and V2 villages, respectively, considering both the direct (on T1 and T2) and indirect spillover (on U1 and U2) effects of the training.

As shown in Table 3, treatment intensity positively affects rice yields, revenues and costs in both the ITT (panel A) and LATE (panel B) estimates. Our results indicate substantial gains in rice productivity as a result of SRI training and induced SRI uptake, with those impacts consistently increasing in treatment intensity, both within village and over time. The productivity gains from SRI adoption, using the two continuous treatment intensity variables as instruments in the LATE estimation are large – 20-23 percent – and are similar to the comparable LATE estimates using the two binary treatment indicator variables as instruments (Table 2, panel B). But, as with the prior results that ignored treatment intensity, the positive estimated profit effects are statistically insignificant, and the productivity and profitability impacts of SRI training are statistically insignificantly different between trained and untrained farmers in treatment villages (i.e., T1 vs. U1, and T2 vs. U2), as well as between twice-trained and once-trained villages.[34]

Figure 3 offers a different, nonparametric look at the impact of treatment intensity on SRI adoption. The impact appears invariant up to or slightly beyond the sample mean/median of 0.60, after which point the slope of the relationship increases sharply and roughly linearly. In order to take account of this nonlinearity in the parametric regressions, we add a dummy in equation (1) if treatment intensity is above 0.70. The results, presented in Table 4, are broadly similar to those in Table 3. The nonlinearity apparent in Figure 3 arises entirely within the U2 cohort, those farmers who were not trained directly but who live in villages where training occurred in both years. Their propensity to adopt SRI responds strongly to treatment intensity; the impact for treatment intensity increments above 0.7 is twice that below the threshold.[35] This suggests a strong synergistic effect

[34] We again get qualitatively identical results when we perform the same analysis using midline data but using the continuous treatment intensity variable (Table A8).
[35] When we repeat this analysis with a 0.60 cut-off in treatment intensity we find broadly similar, but weaker results, which is consistent with Figure 3 as the kink occurs between treatment intensity of 0.65–0.70 (Table A9).

of cross-sectional and intertemporal intensity of exposure on uptake of innovations.

When we allow for nonlinear effects of treatment intensity, relative to the estimates in Table 3, the ITT point estimates of the effects on outcomes generally increase for values below 0.7 – roughly the 60th percentile of the distribution – and are negative beyond that point, signalling that the maximal ITT impact is in the middle third of exposure intensity range. Most of the coefficient estimates on the high intensity (above 0.70) interaction are not statistically significant, but those that are significant are all negative. The corresponding estimates for the profit are positive but not significant statistically. This nonmonotonic pattern of impacts on outcomes is consistent with the non-random selection patterns we observed previously. If greater exposure induces more farmers to adopt SRI, and if the farmers who gain the most from the new technology are those who adopt first conditional on exposure, then increasing exposure intensity can steadily drive uptake higher, but perhaps beyond the point of maximum average gains. That is, these findings suggest that the greater exposure to the technology seems to induce uptake by more farmers who enjoy relatively modest marginal gains from the new practice, a point we explore further below.

Since both the ITT and LATE estimates consistently suggest that SRI exposure and adoption increase the costs of rice cultivation, we next explore precisely which costs increase. These more nuanced findings might help us better understand the adoption and disadoption patterns we explore below. Recall that SRI principles call for increased use of organic soil amendments and mechanical weeders, and more frequent wetting and drying of plots – which requires increased operation of pumps to flood or drain fields – and that earlier seedling transplanting, more regular and careful water management, and increased harvests may increase labor demands, especially on family managers of the SRI plot(s). We would expect increased costs to be concentrated in those areas especially. Conversely, we might expect reduced herbicide and pesticide expenses due to mechanical weeding and better water management practices.

The data exhibit precisely those patterns. As shown in Table 5, the ITT estimates (panel A) of the impact of SRI training exposure and the LATE estimates (panel B) of the causal effects of SRI adoption on input costs clearly signal increased irrigation, and family labor costs among farmers exposed directly (T1 or T2) or indirectly (U1 or U2) to SRI training. These effects are not statistically significantly different among the distinct treatment cohorts. Significant differences between trained and untrained farmers do emerge in organic fertilizer use, which increases sharply among T1 farmers and significantly further among T2 farmers, but not at all among untrained (U1 and U2) farmers. SRI exposure significantly reduces herbicide and pesticide use, with those effects significantly larger the greater the intensity of exposure to training (i.e., T1 vs. U1, T2 vs. U2, and T2 vs. T1). SRI exposure does not significantly increase hired labor demand, which makes sense since SRI involves different handling of seedlings, weeding, and water management than local workers would have experienced prior to SRI introduction. Until familiarity with SRI diffuses more, it may be difficult to hire workers with knowledge of SRI practices and principles. SRI exposure has no impact on seed costs nor on inorganic fertilizer use.

Cumulatively, the findings across Tables 2-5 consistently tell a story that adoption of SRI increases significantly with the intensity of exposure to the innovation, whether measured as trained vs. untrained, trained twice versus trained once, or by village treatment intensity. And SRI has a sizeable positive effect on productivity indicators, while also driving up costs, especially for family labor, irrigation, and organic fertilizers, and positive but insignificant effect on rice profitability. These experimental findings confirm the mass of observational findings of gains from SRI uptake around the world.

An equally crucial observation is that the expected gains farmers enjoy from treatment exposure do not improve with exposure intensity. And compliance with the precise principles communicated in the training remains quite incomplete, even among those most exposed to training. Further, few farmers commit a large share of their rice land to the SRI method. Thus we have a puzzle. Farmers are highly responsive at the extensive margin, as a single day's training – even indirect exposure to a single day's training – has a significant effect on a farmer's likelihood of adopting SRI. But the degree of compliance with and extent of adoption of the practice respond much less strongly to exposure, and the ITT estimates of impacts are invariant to exposure.

Our core results all hold when we re-estimate panel regression estimates using the plot level difference-in-differences specification reflected in equation (3), both using the binary and continuous treatment intensity indicators. As reported in appendix Tables A10 and A11, we see a strong, immediate effect of direct training on SRI uptake by T1 and T2 farmers and an immediate spillover to U1 and U2 farmers. Farmers in villages with two years of training further expand SRI uptake from midline to endline, more than offsetting disadoption within those villages (on which, more below); that is not true in the one year training villages. Farmers in each of the four treatment arms enjoy increased yields, with positive expected effects of profits that are mainly statistically insignificant. The magnitudes of the ITT estimates are again large – e.g., 17-39 percent increases in plot-level rice profits – and statistically similar across treatment arms. We see no statistically significant improvement from midline to endline in any of the outcome measures. Indeed, the ITT point estimates of profit effects of SRI training fall from midline to endline for each group.

Intensity of exposure thus seems highly influential in driving individual acceptance of the innovation. But increased exposure does not boost fidelity to the principles taught nor does it magnify productivity gains due to the innovation. Indeed, given the strong differential effects on adoption propensity, the ITT outcomes estimates suggest strong non-random sorting based on expected productivity in farmers' endogenous decisions to adopt or not adopt SRI as a function of the intensity of their exposure to the method.

6.3 Insights from non-random selection into SRI uptake

The lack of statistically significant ITT differences in outcomes despite statistically significant differences among treatment groups in SRI adoption strongly signals non-random selection among farmers who choose to try SRI. Simply dividing the ITT estimate of the impact on yield outcomes by the ITT estimate of uptake for each group – i.e., the group-specific indirect least squares LATE estimate of impacts – yields a clear, but counterintuitive, ordering from 156.4 percent expected gains for U1 farmers who adopt, down to 31.4 percent gains for T2 farmers. These indirect least squares LATE estimates correlate inversely with exposure to the method through training, so cannot represent traditional learning effects. Rather, they tell us something about the farmers most likely to adopt a new technology quickly, especially those who do so based on limited, indirect learning through social connections. In particular, these patterns suggest that farmer propensity to adopt SRI is positively associated with unobservables (e.g., ambition, skill, social connectivity) that complement the new technology, even controlling for baseline outcomes. Recall from appendix Tables A2-A4 that no significant differences in observables exist among different treatment groups. And we find no significant heterogeneity in returns to SRI conditional on observables such as land and labor endowments, as discussed below.[36] So these differences necessarily arise due to farmer (or plot) unobservables.

Given strong selection on unobservables, greater exposure may induce more farmers to adopt SRI, and to thereby benefit from the method, but with diminishing average productivity gains from

[36] The only dimension in which we observe heterogeneity of returns is with respect to pre-treatment rice yield, with costs of production appreciably lower for those with higher baseline yields, albeit with no significant difference among treatment groups (Table A11). This seems likely to reflect farmer skill and other unobservables that confer higher expected gains from using the new technology conditional on exposure level.

adoption. Consider the following thought experiment. Imagine farmer productivity-enhancing unobservables as represented by a scalar variable, call it ability, with a standard, unimodal distribution, with farmers at the upper end of the distribution enjoying greater gains from any new technology than those further down the distribution. Any intervention that increases the likelihood of uptake equally throughout the distribution will necessarily generate more new users of modest ability than of high ability. Thus, if increased exposure to an innovation induces greater uptake but not to enhanced learning of how to use the method productively/profitably – as our results indicate – then one would expect precisely this pattern in the indirect least squares LATE estimates. They reflect a scaling effect in uptake due to increased exposure without any productivity boost from added exposure.

The implication is that adoption, an obviously non-random process, follows a natural ordering once people are exposed to the innovation. Those who adopt with only limited exposure to the new technology are likely to have higher expected productivity with the method. In a learning model, this would correspond to updating of beliefs – inducing a switch to the new method – that is increasing in both the expected outcome (conditional on factors both observable and unobservable to outsiders) of adoption and on the intensity of exposure. It takes fewer new observations – i.e., less intense exposure to the new technology – to induce adoption by farmers who expect to benefit more.

This is consistent with what we find in the data. Greater exposure to SRI training induces more farmers to adopt, and the number of adopters experiencing productivity or profitability at or above any given level is likewise increasing in exposure. This can be seen in Figures 4 and 5, which plot the ordered distribution of rice profits and output per decimal, respectively, at endline for SRI adopters within each treatment arm. The dotted horizontal line shows the control group mean. The leftmost observation – i.e., the most profitable (Figure 4) or highest yield (Figure 5) farmer – of each group-specific schedule, and all the subsequent observations within that cohort, follow a clear ordering that corresponds to the extent of SRI training exposure. Any number of T2 farmers has higher rice profitability or output per unit area than the same number of T1 farmers, who have higher profitability and yields than the U2 farmers, who are more profitable and productive than the U1 farmers. The vast majority of SRI adopters outperform the control group mean. The endline differences between T2 and U2 and between T1 and U1 are large, again consistent with the inference that most of the gains come from direct learning from the BRAC extension agents. The difference between the U1 and U2 farmers is quite modest.

The patterns observed in Figures 4 and 5 thus reflect almost exclusively a scaling effect. Increased intensity of exposure to SRI expands the numbers of adopters enjoying a given yield or profit level, but it does not transform the outcome (yield or profit) distribution. This is most easily seen by observing the treatment arm-specific conditional outcome distributions. As previously shown, at baseline, there was no stochastic dominance among the rice yield distributions for the control group, the directly trained, and the untrained within training villages. After training, the yield distributions for all four treatment arms first order stochastically dominate the control group, at both midline and endline (Figures A1 and A2). But no (first, second or third order) stochastic dominance exists among any of the treatment arms (Figures A3 and A4). The distribution of outcomes is the same, as reflected by the ITT estimates. Just the scale of uptake of SRI is expanded by greater exposure to the technology, as reflected in Figures 4 and 5.

The non-random nature of selection into SRI adoption is also apparent in comparison of the yield distributions of adopters and non-adopters within each treatment arm. As depicted in Figure 6, the yield distribution of SRI adopters during endline does not statistically significantly stochastically dominate (at first, second or third order) that of non-adopters within any of the treatment arms.[37] The p-values for the tests of first order dominance are decreasing in intensity of exposure to SRI training, consistent with the prior findings that greater exposure leads to slightly greater compliance with recommended practices. But the fact that within each treatment arm these differences are not statistically significant indicates that farmers make reasonably rational uptake decisions based on unobservables that affect performance outcomes.

6.4 Household Welfare Effects

Previous studies based on observational data question whether the rice productivity or profitability gains associated with SRI adoption translate into improvements in household living standards (Noltze et al. 2013; Takahashi and Barrett 2014). Those papers find evidence that SRI induces a reallocation of farm household labor from nonfarm activities to rice cultivation, such that the loss of non-farm income largely offsets the gains from increased rice productivity.[38] Since we likewise find significant yield gains but no statistically significant rice profit gains from SRI, we seek to establish experimentally whether the apparent gains in rice productivity, and large positive but statistically insignificant impacts on profitability, translate into improved household welfare

[37] Figure A5 shows the same for midline yield.
[38] In another observational and simulation study, Gathorne-Hardy et al. (2016) find that the gains that accrue to SRI farming households come at the expense of landless workers.

indicators.

Table 6 reports the ITT and LATE estimates of SRI exposure and adoption, respectively, on self-reported measures of savings, social status (relative to others in the village), food security, life satisfaction, and satisfaction with living standard.[39] Because the latter four variables are scored on five to ten point Likert scales, we use an ordered probit estimator for those regressions. Panel A reports the ITT estimates by treatment group. The various Likert scale measures of life satisfaction, household status, and food security are all positive and mostly (but not all) statistically significant for treated households, as compared to control village households, albeit with varied patterns in the ITT estimates of the effects of SRI exposure. We find that SRI training exposure increases household savings, but not statistically significantly, among all groups exposed to SRI training, whether directly (T1 and T2) or indirectly (U1 and U2), with no significant differences among them.

The LATE estimates of the impacts of SRI adoption consistently show that SRI uptake causes expected household welfare gains (Panel B Table 6). The effects are strongly and statistically significantly positive for food security and life satisfaction indicators, and positive but statistically insignificant for the other three indicators. These results suggest that the higher rice yields resulting from SRI exposure and uptake indeed translate into improved living standards for households, although those effects are far weaker than the rice yield effects, and more in line with the more mildly positive but imprecisely estimated rice profit effects of SRI. The gains from SRI do not appear fully offset through reallocation of family labor into rice production. But once again, there is no discernible gradient of gains with respect to cross-sectional nor intertemporal intensity of exposure to SRI training.

6.5 Disadoption, Delayed Adoption, and Farmer Heterogeneity

The multi-year design of our experiment permits us to study different adoption behaviors by farmers. More specifically, we distinguish among farmers who never practiced SRI (non-adopters), those who adopted SRI in both post-training periods (persistent adopters), those who adopted in the first year but then disadopted (disadopters), and those who only adopted in the second year (delayed adopters). Table 7 shows that among the 2,667 treatment village farmers for whom we have both year 1 and year 2 observations, 36% (317) of farmers who adopted SRI in

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[39] See the notes at the bottom of Table 8 for details on the construction of these variables.

year 1 disadopted in year 2 (disadopters).[40] On its surface, this high a rate of disadoption – which is similar to observational findings elsewhere on SRI (Moser and Barrett 2006) – is puzzling given the preceding results showing rather substantial gains from SRI adoption.[41] What lies behind this seemingly high disadoption rate of a productivity-enhancing innovation? Might this reflect heterogeneous marginal returns to SRI adoption? Or simply the dispersed estimated profit effects of SRI, leading some adopters to find the method generates no significant profit boost?

First, we consider the impact of intensity of exposure on disadoption. As shown in Table 7, not only is adoption higher in V2 villages, as established above, but persistent adoption is higher and disadoption lower in the villages that randomly received two years of training rather than just one. Among those who practiced SRI both years, 60% were from V2 villages, and only 40% from V1 villages. The disadoption rate conditional on year 1 SRI uptake was 61 percent (21/34) among U2 farmers, 53 percent (16/30) among U1 farmers, and 47 percent among T1 farmers (189/397), two to three times the 22 percent (91/415) disadoption rate among T2 farmers. Clearly, added exposure to SRI training strongly encouraged farmers to continue with the practice after they initially tried it. Social spillover effects resulting in delayed adoption were relatively modest, with just 6 and 10 percent of U1 and U2 farmers, respectively, adopting SRI only in year 2. Having been directly trained substantially increased the likelihood of delayed adoption, as 25 percent of T1 farmers only adopted in year 2, but additional training did not affect the likelihood of delayed uptake, with a statistically similar 30 percent of T2 farmers who did not adopt in the initial year trying it in year two. Not only did the intertemporal intensity of exposure to SRI training affect SRI adoption at the end of both year 1 (Tables A7 and A8) and year 2 (Table 3), but it matters to the likelihood that initial non-adopters eventually take up the method and that initial adopters persist in the practice of SRI.

Appendix Table A12 presents the characteristics of these different groups of farmers.[42] The descriptive statistics are revealing, if only indicative. Those who adopted SRI in year 1 (persistent adopters and disadopters) had larger land holdings, did better on simple memory tests[43], and were

[40] 1,056 farmers from 120 villages adopted SRI in year 1. At the end of year 2, we collected or verified the SRI adoption status on only 876 of those farmers; 180 year 1 adopters attrited from the sample. As reported previously (Tables A5-A6), attrition appears random, so should not impact our analysis.
[41] Duflo et al (2011) also show that in Western Kenya, adoption of fertilizers among farmers receiving a one-time subsidy dropped back to the same rate as among the comparison group as soon as the subsidy stopped, suggesting that such a one-time subsidy does not lead to persistent technology adoption but only has a temporary effect.
[42] When we conduct the analysis for the trained and T1 and T2 villages separately, we find similar patterns.
[43] We test short term memory using memory span exercise. The farmers heard ten words in a row, which they were asked to repeat immediately, and the number of words that they could repeat correctly was recorded. We then, after

slightly more risk averse[44] before the intervention began as compared to those who did not adopt SRI in year 1 (delayed adopters and never adopters). The midline non-adopters – delayed adopters and never adopters – had lower baseline cost of production and higher baseline profits (including family labor) compared to persistent adopters. Although their yields and profits increased from baseline to year 1 (a good growing year), both yields and profits were now inferior among the midline non-adopters as compared to those who adopted SRI in year 1.

Delayed adopters had lower baseline and midline yields, costs, and profits per decimal than never adopters, although they were more profitable than never adopters at midline. Delayed adopters' yields, costs, and profits had surpassed those of never adopters at endline, implying that the decision to switch paid off.

The never adopters had significantly lower baseline cost of production and higher profits compared to the other groups. They were also at least as well off (in baseline income terms) as the other three groups of farmers, relatively older, less educated, but with stronger memory, and greater appetite for risk. They also had the smallest land holdings among the four groups.

The cost of production was relatively low and profits high for all groups in year 1 (midline) compared to the baseline for all groups, reflecting a good growing year. However, at the end of year 1, the persistent adopters had experienced the largest (42 percent) decline in cost of production and the largest gain in profits (53 percent), compared to base year, of any group. The disadopters had significantly higher costs of production in year 1 than did persistent adopters. The midline rice profits per decimal from cultivating *Boro* rice were, if anything, slightly higher for disadopters than for persistent adopters. But the change in profits from baseline to midline was significantly higher for persistent adopters than disadopters. It does not appear that cross-sectional heterogeneity in returns to SRI explains disadoption so much as heterogeneity in the observed changes in returns. Farmers respond to their lived experience.

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ten minutes, asked them to repeat the words for the second time, and again recorded the total number of correct words, using it as a measure of their short-term memory. The farmers had average memory spans of 5.4 words and 4.6 words in the immediate and ten-minute afterward tests, respectively.

[44] During the baseline survey, we played the standard risk-taking lottery game proposed by Binswanger (1980) in order to study the individual attitudes toward financial risk. In this lottery game, famers were asked to choose an option from among six options that are basically various combinations of amounts of BDT as payoffs. Option one ensures a payment of BDT 100, whereas options two to six each involve a coin toss that gives an outcome of heads or tails with a 50–50 chance. The degree of riskiness of the lottery options increases in ascending order, with option 6 being the riskiest. A farmer is considered as risk-takers if he chooses option 6, otherwise he is risk-averse. Overall, 69% of the farmers in our sample are risk averse.

We further explore the possibility of heterogeneous returns based on exposure intensity by interacting the treatment dummies in equation (1) with baseline (i.e., pre-treatment) (i) production, (ii) cultivable land, (iii) household size, (iv) number of working adults in the household, and (v) household income. SRI might have differential effects based on farmer skill manifest in baseline productivity (Barrett et al. 2004), on land or labor availability or income (per the pro-smallholder claims of some SRI advocates), and labor as often been cited as a bottleneck to (persistent) adoption (Moser and Barrett 2006). As reported in appendix Tables A13 and A14, we find no evidence of statistically significant heterogeneity in SRI adoption or profitability along any of those baseline dimensions. We do find that SRI exposure significantly reduces production costs and revenues in proportion to baseline rice output, consistent with the hypothesis that unobserved farmer skill affects performance, as discussed above. As shown in appendix Figures A6-A9, there is only a slight suggestion of heterogeneity of impact according to baseline profitability, but no broad, general pattern of heterogeneous treatment effects based on observables appears in the data. The lack of any appreciable heterogeneity in performance with SRI reinforces the inconsistency of widespread disadoption with the canonical target-input model of agricultural technology adoption.

Table 8 (columns 1-3) reports the multinomial logit regression estimates (per equation 4) associating the (delayed or persistent) adoption or disadoption decision – relative to the never adopter comparison group in V1 and V2 villages – to both the experimental treatments and to baseline farmer characteristics. The U1 treatment arm serves as the comparison group. Column 4 reports the logit regression of disadoption conditional on year 1 uptake of SRI. Direct SRI training exposure sharply increases the likelihood of ever trying SRI. For T2 farmers, the effect is significantly larger on the likelihood of persistent adoption than of delayed adoption or disadoption. T1 farmers are far more likely to be persistent adopters than delayed adopters, but more likely to adopt with a delay than are untrained (U1 or U2) farmers. The higher a farmer's baseline production, the less likely he was to adopt SRI with a delay and the greater the likelihood that he disadopted, conditional on midline SRI uptake. Conversely, the higher a farmer's baseline costs, the more likely he was to adopt at midline, with equal effects on the likelihood of disadoption or persistent adoption. Older farmers and those with higher baseline income were much less likely to become persistent adopters and more likely to disadopt conditional on midline use of SRI. By contrast, farmers with more education, land, or both were more likely to become persistent adopters. Overall our regression estimates are consistent with the simple descriptive patterns found in Table 7.[45] Farmers are far more likely to experiment with SRI and are more likely to continue trying it the more exposed they have been to the method through training. But although SRI appears to generate real productivity gains across the board, its profit impacts are far more dispersed and farmers make adoption and disadoption decisions consistent with their personal experience of gains (or not) from their own experimentation. They do not appear, however, to treat experimentation as much as a means of learning how best to use the technology, so that they improve performance via learning by doing.

6.6 Farmers' adjustments of practices: implications for learning

We explore this question of farmer learning by studying how compliance with the six SRI principles taught in BRAC training varies with exposure intensity, including one's own experience with the practice (i.e., learning by doing), from midline to endline. We have already established that the greater is farmers' exposure to SRI training, the more likely they are to adopt the SRI principles. But we have also seen that they do not fully comply with the principles as taught. For example, while all the treatment arm groups transplant younger seedlings than do control farmers (Table 1), few go so far as to transplant as young as 20 days, the BRAC recommendation. By exploring farmers' adjustment of rice cultivation practices in response to SRI training exposure and own experience, we can perhaps gain some insights on the learning processes occurring.

Toward that end, we estimate a plot-level panel regression using all three waves of data (i.e., baseline, midline, endline) following equation (3), with a binary variable for adoption of different SRI principles as the outcome variables of interest. The estimation results are presented in Tables 9 and A15 (for the continuous treatment intensity measure). The takeaway from these tables is that learning the practices seems to occur mainly from direct training by BRAC agents, to a lesser degree from learning from other farmers, and least from learning by doing. Direct trainees are significantly more likely to follow each of the six recommended practices than are control farmers at midline, post-training. With the exception of using mechanical weeders or the AWD water management method, direct trainees are also statistically significantly more likely than untrained farmers in training villages to follow each principle at midline. But there was no statistically significant difference in compliance between T1 and T2 farmers at midline in any principle, consistent with them having received exactly the same exposure to SRI at that point in time. But other than modestly wider spacing among seedlings and increased use of mechanical weeders, the

[45] We estimate the same models using only the sub-sample of farmers who received training and find similar results.

T1 farmers did not significantly increase compliance with SRI training between midline and endline, in the absence of further training. They exhibit little learning by doing nor from others following their and their neighbors' initial year of experience with SRI. The U1 and U2 farmers did see modest, but jointly statistically significant, increases in compliance with the key three SRI principles (age, number and spacing of transplanted seedlings) from midline to endline, consistent with the lagged adoption impacts observed earlier as these farmers learned SRI from their BRAC-trained neighbors. By contrast, the T2 farmers significantly increased compliance with each of the six principles from midline to endline, with the exception of AWD water management compliance, which increased, but insignificantly. Limited compliance with that specific principle may arise because coordinating irrigation among farmers proved an obstacle, as a number of participants told us. Increased compliance among the T2 farmers relative to T1 farmers from midline to endline suggests the importance of formal extension programming to advance learning about the method, even of the short duration in this experiment. Even so, these adjustments were far more modest than overall adoption, and outcomes did not improve significantly for T2 farmers from midline to endline, relative to the other groups (Table 9).

Overall, the story is one of limited independent learning outside of BRAC's formal SRI extension effort within villages. The lone exception is the spacing of seedlings, which, along with AWD water management, is the most easily visible change of practice from traditional to SRI rice cultivation and thus most likely to be absorbed through passive learning from others.

7. Conclusions

Although the system of rice intensification (SRI) has now spread to more than 50 countries, the existing evidence on the purported gains from SRI previously relied exclusively on non-experimental evidence. Partly for that reason, claims of gains from SRI have remained contentious within the international agricultural research community. This paper offers the first truly rigorous SRI impact evaluation, based on a large-scale, multi-year RCT implemented among farmers in rural Bangladesh. We find that providing relatively brief training on the key principles and practices of SRI induces significant farmer adoption of the method in villages previously unexposed to the technology. It also increases rice yields significantly; the estimated local average treatment effect is +21%. Expected rice profits likewise increase with experimental exposure to SRI training, albeit not statistically significantly, as do various indicators of household well-being, suggesting that SRI generates real gains, on average, to households that adopt the practice. Our findings thus support existing claims, based on observational data, that SRI might play an important role in boosting agricultural productivity and farm household food security and well-being in the developing world.

Yet the profit and household welfare impacts of SRI are appreciably weaker and more dispersed than the method's yield impacts. As a result, SRI disadoption rates are also high, likewise confirming key findings of prior observational studies. Roughly one-third of the farmers adopt SRI and about 60% of early adopters continue the practice a year later, while almost identical numbers of farmers adopted SRI with a one year lag as disadopted after an initial year's experience with the method. Disadoption patterns directly reflect pre-treatment conditions and post-adoption experiences, with farmers who already did well or who saw little or no improvement from SRI in their first season's trial far more likely to disadopt.

Perhaps most striking, there is no significant difference in performance with SRI based on the extent of exposure to training in the method. Compliance with the six key SRI principles instructed by extension agents is greatest among those directly trained, but still relatively low. There appears to be far stronger direct learning from extension agents than learning from others, and even less from learning by doing. The main effect of greater exposure to SRI training is to scale up the number of farmers practicing – and benefitting from – SRI rather than to improve farmers' performance with SRI or compliance with the principles taught by extension agents.

The dramatic contrast between the sharp response of SRI uptake to the intensity of exposure to the new technology and the negligible response of performance with SRI to exposure intensity is inconsistent with core predictions of a workhorse model of learning that economists have used in studying agricultural technology adoption. The standard target-input model predicts both that average performance will improve with increased opportunities to learn and that farmers should not disadopt in any significant number. We strongly reject both of those hypotheses in this experiment. Relatively recent models of rational inattention and of multi-object learning – and perhaps others – seem to fit our data better. These results suggest a need to give more careful thought to the models used to conceptualize the agricultural technology adoption and diffusion process.

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Table 1: Summary statistics of SRI adoption, yield and profits

Panel A: SRI Adoption	Control (C)		U1		T1		U2		T2		p-value (U2-U1)		p-value (T2-T1)	
	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline
SRI adoption	0.00	0.00	6.92	9.13	49.72	38.88	8.40	12.46	49.19	53.04	0.14	0.01	0.72	0.00
Plot level observations	3973	3626	1474	1303	2307	2135	1306	1188	2474	2319				
Panel B: Other measures of adoption														
Self-assessed SRI adoption	1.01	0.00	7.14	8.57	50.28	38.33	8.73	11.85	49.29	52.62	0.12	0.01	0.49	0.00
Enumerator-assessed SRI adoption	0.45	0.00	5.10	7.30	38.00	32.99	6.74	10.83	39.40	47.64	0.07	0.00	0.32	0.00
Extent of SRI adoption	0.00	0.00	4.68	7.45	36.74	32.22	5.96	9.02	37.13	45.74	0.09	0.13	0.74	0.00
% of land used for SRI	0.50	0.00	3.64	4.73	22.84	22.14	4.01	7.20	19.07	25.83	0.53	0.00	0.00	0.00
No of principles adopted	0.69	0.72	0.86	0.96	1.32	1.46	0.94	1.16	1.30	1.74	0.02	0.00	0.52	0.00
Age of seedlings[1]	0.43	0.56	0.27	2.38	3.88	4.77	0.46	1.05	2.80	6.28	0.41	0.02	0.04	0.04
Age of seedlings (days)	41.02	40.74	36.13	35.10	27.04	26.43	35.92	35.40	26.83	25.97	0.32	0.25	0.07	0.00
No of seedlings per bunch[1]	12.50	10.11	14.34	12.59	27.15	30.86	17.69	17.30	27.48	34.47	0.02	0.00	0.79	0.02
Distance among seedlings[1]	0.43	0.53	2.18	5.99	15.56	24.95	4.59	9.08	14.71	29.82	0.00	0.01	0.42	0.00
Alternate drying & wetting[1]	46.94	53.32	59.08	57.66	66.45	63.99	57.50	71.41	64.21	75.42	0.40	0.00	0.10	0.00
Use of organic fertilizer[1]	8.87	7.70	10.27	17.78	18.87	21.64	13.71	17.59	20.43	27.60	0.01	0.91	0.18	0.00
Mechanical weeding[1]	3.02	0.43	1.16	9.68	3.31	12.53	0.77	4.88	2.23	9.35	0.30	0.00	0.02	0.00
Panel C: Production, Cost and Profit (per decimal)														
Yield (kg)	22.36	21.25	25.18	24.45	26.28	24.76	25.68	24.70	26.06	25.00	0.05	0.38	0.29	0.30
Estimated revenue	728.73	699.73	812.80	791.99	847.29	801.94	838.24	811.71	839.89	819.10	0.00	0.04	0.28	0.03
Input cost	148.14	135.62	157.73	154.80	162.22	156.41	156.45	153.99	156.81	156.14	0.46	0.72	0.00	0.98
Labor cost	135.47	274.73	159.94	301.75	158.14	323.27	160.41	299.98	152.45	313.87	0.90	0.88	0.04	0.45
Labor cost 2	299.46	387.39	324.64	435.92	335.63	467.43	311.99	443.87	328.95	463.84	0.00	0.48	0.07	0.76
Total cost	283.61	410.35	317.67	456.55	315.71	479.68	316.86	453.98	310.89	470.01	0.86	0.83	0.14	0.47
Total cost 2	447.60	523.01	482.37	590.71	497.85	623.84	468.44	597.87	485.76	619.98	0.01	0.53	0.01	0.77
Estimated profit	445.13	286.30	495.13	333.56	526.93	322.26	521.39	354.95	530.63	349.09	0.00	0.14	0.60	0.03
Estimated profit 2	281.13	175.00	330.43	200.66	349.44	178.10	369.81	213.19	354.13	199.12	0.00	0.39	0.54	0.09
Plot level observations	3967	3240	1471	1136	2295	1846	1306	1048	2467	2030				

Notes: *SRI adoption* is measured using verification at the planting and pre-harvesting period visits by BRAC field investigators. The reported p-values are from the two-tailed test with the null hypothesis that the group means are equal. Yield is total sellable product per decimal of land (in kg) after adjusting for wastage due to floods, drought and diseases. Total revenue is total sale revenue at market prices, in Bangladeshi taka (BDT) per decimal of land. Input cost 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 for both hired and contract labor as well as imputed cost of family labor per decimal of land. Total cost includes both labor and non-labor input cost per decimal of land. Labor cost 2, Total cost 2, and Estimated profits 2 include the cost of family labor measured at the market wage at the district level. One decimal=1/100 of an acre. The number of observations slightly differ between Panel A and Panel B as we collected the data on adoption and data on production at different times. Revenue, cost and profit are measured in BDT. One-time training (T1) and two-time training (T2) include only households which have received training in the treatment villages only. U1 and U2 include those did not receive training. *Self-assessed SRI adoption* reflects the farmers' declaration post-adoption. It's a dummy variable to indicate if a farmer himself thinks he has adopted or not; *Enumerator-assessed SRI adoption* uses assessment by enumerators at the time of midline or endline post-adoption survey. It is also a dummy variable based on enumerators' assessment about whether the respondent farmers followed SRI principles on any plot of land. *Extent of SRI adoption* is farmers' own assessment about the SRI adoption on 0-100. *% of land used for SRI* is percentage of total land under SRI. [1] dummy variable indicating if a farmer followed that principle as recommended by BRAC for SRI. Age of seedlings (days) is the average number of days of seedlings used for transplantation.

Table 2: ITT and LATE Effects at Endline

	(1)	(1')	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ITT	SRI Adoption (%)		Yield	Revenue	TC1	TC2	Profit 1	Profit 2
One-time untreated (U1)	9.273***	9.849***	0.145***	0.139***	0.147**	0.137***	0.143	0.139
	(1.819)	(2.026)	(0.030)	(0.033)	(0.069)	(0.045)	(0.102)	(0.165)
One-time treated (T1)	38.652***	38.738***	0.150***	0.142***	0.170**	0.173***	0.112	0.047
	(3.432)	(3.722)	(0.032)	(0.035)	(0.074)	(0.048)	(0.104)	(0.163)
Two-time untreated (U2)	12.535***	13.235***	0.149***	0.157***	0.125*	0.147***	0.232*	0.217
	(2.589)	(2.778)	(0.033)	(0.037)	(0.074)	(0.046)	(0.119)	(0.200)
Two-time treated (T2)	53.143***	51.937***	0.167***	0.172***	0.129*	0.163***	0.232*	0.188
	(4.214)	(4.235)	(0.030)	(0.032)	(0.074)	(0.047)	(0.122)	(0.191)
Baseline outcome			0.207***	0.259***	-0.018	-0.068***	0.022	0.107**
			(0.030)	(0.033)	(0.032)	(0.022)	(0.043)	(0.050)
Observations	10,297	8,830	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.285	0.276	0.085	0.102	0.057	0.056	0.020	0.012
p-value (U1-T1)	0.00	0.00	0.81	0.88	0.53	0.17	0.61	0.32
p-value (U1-U2)	0.30	0.32	0.90	0.62	0.75	0.83	0.46	0.70
p-value (T1-T2)	0.01	0.02	0.56	0.36	0.58	0.83	0.33	0.47
p-value (U2-T2)	0.00	0.00	0.36	0.44	0.94	0.58	0.99	0.84
Panel B: LATE								
Adopted SRI (IV=Treatment status)			0.238***	0.241***	0.219**	0.264***	0.245	0.114
			(0.045)	(0.048)	(0.108)	(0.071)	(0.163)	(0.254)
Baseline outcome			0.231***	0.278***	-0.007	-0.059**	0.024	0.104**
			(0.032)	(0.033)	(0.034)	(0.025)	(0.042)	(0.049)
Observations			8,830	8,830	8,821	8,821	8,821	8,821
R^2			0.039	0.061	0.013	-0.011	0.019	0.011
Adjusted R^2			0.037	0.059	0.011	-0.013	0.016	0.009
Hansen J			0.156	0.001	0.835	0.777	0.539	0.426
Prob>J			0.693	0.983	0.361	0.378	0.463	0.514
F-stat			121.40	121.73	116.61	120.03	121.2	121.2
Control Mean	0.00	0.00	21.25	699.73	410.35	523.01	286.30	175.00

Note 1: The treatment effects indicate the treatment status within the treatment villages where the base category is control group. The SRI adoption status variable was collected in multiple post-transplantation visits while the other outcome variables were obtained from a post-harvest survey. Hence, the difference in sample size between column 1 and the others. Control mean indicates raw (not log-transformed) mean for the variable at the baseline.
Note 2: Labor cost 1, total cost 1 (TC1) and profit 1 do not include the cost of family labor. Family labor cost is included in the variables labor cost 2, total cost 2 (TC2) and profit 2 using market wage. Yield, cost and revenue are expressed in logarithms. To accommodate observations with negative profits we ran regressions using untransformed profit data, and then then divide the estimated coefficients by the control group mean to obtain a comparable percentage change estimate to those in the log-based regressions. The controls used in the regressions are as follows: dummy indicating whether household head's age> 45, whether above primary level education, land size>median (120 decimals), head married or not, household size, (log of) household income, household composition such as number of children, women, working age people, and maximum education by any member in the household. Standard errors are clustered at the village level. The F-stat is from the first stage of the IV regression. *** p<0.01, ** p<0.05, * p<0.1

Table 3: ITT and LATE Effects of Treatment Intensity at Endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ITT	SRI Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
One-time untreated (U1F)	15.618***	0.235***	0.213***	0.221	0.198**	0.223	0.257
	(3.831)	(0.059)	(0.063)	(0.136)	(0.094)	(0.199)	(0.323)
One-time treated (T1F)	64.962***	0.210***	0.190***	0.232	0.239**	0.129	0.034
	(6.821)	(0.057)	(0.063)	(0.141)	(0.095)	(0.190)	(0.298)
Two-time untreated (U2F)	24.272***	0.229***	0.235***	0.162	0.206**	0.387*	0.394
	(5.893)	(0.064)	(0.070)	(0.131)	(0.081)	(0.208)	(0.341)
Two-time treated (T2F)	84.396***	0.225***	0.226***	0.139	0.208**	0.313	0.250
	(6.824)	(0.050)	(0.053)	(0.130)	(0.083)	(0.218)	(0.340)
Baseline outcome		0.210***	0.259***	-0.019	-0.067***	0.023	0.108**
		(0.031)	(0.033)	(0.032)	(0.023)	(0.043)	(0.049)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.286	0.073	0.090	0.051	0.043	0.018	0.012
p-value (U1F-T1F)	0.00	0.48	0.54	0.86	0.40	0.42	0.24
p-value(U1F-U2F)	0.19	0.93	0.76	0.76	0.83	0.47	0.72
p-value (T1F-T2F)	0.04	0.79	0.56	0.58	0.76	0.43	0.55
p-value (U2F-T2F)	0.00	0.91	0.82	0.72	0.91	0.64	0.57
Panel B: LATE							
Adopted SRI (IV=Treatment intensity)		0.211***	0.208***	0.170	0.227***	0.193	0.060
		(0.047)	(0.050)	(0.127)	(0.084)	(0.190)	(0.298)
Baseline outcome		0.229***	0.275***	-0.009	-0.058**	0.023	0.102**
		(0.032)	(0.033)	(0.034)	(0.024)	(0.042)	(0.049)
Observations		8,830	8,830	8,821	8,821	8,821	8,821
R^2		0.047	0.069	0.024	0.002	0.018	0.010
Adj R^2		0.045	0.067	0.022	0.000	0.016	0.008
Hansen J		0.069	0.006	0.660	0.459	0.491	0.363
Prob>J		0.792	0.937	0.417	0.498	0.483	0.547
F-stat		102.03	103.29	99.56	101.13	103.1	102.3
Control mean	0.00	21.25	699.73	410.35	523.01	286.30	175.00

Note 1: The treatment intensities are fractions of farmers trained in each village (between 0 and 1) and are the same for treated and untreated farmers within the same village, where the base category is the control group. Accordingly, U1F is the fraction of the farmers treated in a village multiplied by treatment dummy to denote whether a farmer from a V1 village belongs to the U1 treatment group or not, and so on. In the notation, we add the F suffix (e.g., U1F), for frequency, so as to distinguish these intensity of treatment variables from the dummy treatment indicators used in Table 2.
Note 2: same as Table 2.

Table 4: Nonlinearity in the ITT Effects of Treatment Intensity at Endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
One-time untreated							
(U1F)	15.734***	0.266***	0.239***	0.323**	0.281***	0.172	0.144
	(4.396)	(0.065)	(0.071)	(0.145)	(0.101)	(0.224)	(0.366)
U1F x > 70%	-3.831	-0.086	-0.066	**-0.299***	**-0.250****	0.168	0.349
	(5.033)	(0.062)	(0.074)	(0.180)	(0.117)	(0.225)	(0.350)
One-time treated (T1F)	63.512***	0.282***	0.256***	0.417***	0.375***	0.053	-0.115
	(8.223)	(0.062)	(0.068)	(0.138)	(0.091)	(0.209)	(0.324)
T1F x > 70%	2.256	**-0.157****	**-0.142***	**-0.387****	**-0.282****	0.158	0.291
	(10.253)	(0.064)	(0.078)	(0.181)	(0.122)	(0.247)	(0.393)
Two-time untreated	17.125***	0.264***	0.272***	0.256*	0.288***	0.340	0.271
(U2F)	(5.456)	(0.071)	(0.078)	(0.145)	(0.086)	(0.241)	(0.390)
U2F x > 70%	**34.309*****	-0.110	-0.124	**-0.281***	**-0.266*****	0.151	0.417
	(11.856)	(0.104)	(0.117)	(0.151)	(0.099)	(0.238)	(0.422)
Two-time treated (T2F)	83.505***	0.256***	0.259***	0.271**	0.307***	0.258	0.083
	(9.069)	(0.066)	(0.068)	(0.130)	(0.080)	(0.248)	(0.382)
T2F x > 70%	0.711	-0.040	-0.043	-0.190	-0.142	0.078	0.257
	(10.492)	(0.059)	(0.063)	(0.164)	(0.107)	(0.305)	(0.470)
Baseline outcome		0.206***	0.255***	-0.018	-0.066***	0.025	0.113**
		(0.031)	(0.034)	(0.031)	(0.021)	(0.043)	(0.049)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.290	0.078	0.094	0.069	0.063	0.019	0.014
p-value (U1F-T1F)	0.00	0.68	0.68	0.13	0.05	0.35	0.20
p-value (U1F-U2F)	0.83	0.98	0.69	0.65	0.94	0.52	0.77
p-value (T1F-T2F)	0.09	0.70	0.97	0.28	0.44	0.43	0.62
p-value (U2F-T2F)	0.00	0.84	0.75	0.83	0.69	0.56	0.42
Control mean	0.00	21.25	699.73	410.35	523.01	286.30	175.00

Notes: Same as Table 3

Table 5: Input costs impacts of SRI by treatment intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: ITT	Seed cost	Inorganic	Organic	Irrigation	Pesticide/herbicide	Other costs	Input cost	Hired labor	Family labor
One-time untreated (U1F)	0.059	0.035	-0.162	0.332***	-0.278	0.288***	0.246***	0.312	0.213
	(0.089)	(0.062)	(0.702)	(0.125)	(0.190)	(0.074)	(0.058)	(0.195)	(0.168)
One-time treated (T1F)	0.025	-0.035	1.173*	0.390***	-1.183***	0.147**	0.240***	0.298	0.232*
	(0.087)	(0.058)	(0.668)	(0.095)	(0.289)	(0.064)	(0.045)	(0.206)	(0.133)
Two-time untreated (U2F)	0.009	0.052	-0.015	0.402***	-1.483***	0.104	0.227***	0.167	0.373**
	(0.084)	(0.055)	(0.575)	(0.114)	(0.351)	(0.093)	(0.055)	(0.189)	(0.162)
Two-time treated (T2F)	0.020	-0.012	2.206***	0.307***	-1.859***	0.187***	0.202***	0.152	0.384***
	(0.079)	(0.052)	(0.474)	(0.111)	(0.252)	(0.052)	(0.051)	(0.188)	(0.137)
Baseline outcome	0.095***	0.033**	0.952***	0.159***	0.181***	0.058***	0.037**	0.319***	0.255***
	(0.017)	(0.015)	(0.034)	(0.020)	(0.033)	(0.014)	(0.017)	(0.036)	(0.028)
Observations	7,628	7,628	7,628	7,628	7,628	7,628	8,821	8,521	8,824
R^2	0.032	0.012	0.427	0.059	0.083	0.017	0.075	0.103	0.140
p-value (U1F-T1F)	0.57	0.16	0.01	0.48	0.00	0.01	0.85	0.91	0.83
p-value (U1F-U2F)	0.55	0.81	0.83	0.58	0.00	0.05	0.75	0.47	0.34
p-value (T1F-T2F)	0.94	0.71	0.08	0.40	0.05	0.44	0.40	0.50	0.21
p-value (U2F-T2F)	0.86	0.14	0.00	0.22	0.22	0.29	0.50	0.91	0.90
Panel B: LATE									
Adopted SRI (IV=Treatment Intensity)	0.016	-0.050	2.886***	0.348***	-1.997***	0.170***	0.219***	0.195	0.351***
	(0.080)	(0.051)	(0.516)	(0.111)	(0.283)	(0.055)	(0.051)	(0.189)	(0.119)
Baseline outcome	0.095***	0.033**	0.899***	0.169***	0.224***	0.065***	0.040**	0.334***	0.254***
	(0.017)	(0.015)	(0.035)	(0.021)	(0.032)	(0.014)	(0.017)	(0.038)	(0.029)
Observations	7,628	7,628	7,628	7,628	7,628	7,628	8,821	8,521	8,824
R^2	0.030	0.021	0.436	0.032	0.029	0.008	-0.017	0.091	0.150
Adjusted R^2	0.0275	0.0189	0.434	0.0292	0.0265	0.00520	-0.0188	0.0887	0.149
Hansen J	0.00883	0.316	0.866	1.516	0.780	0.0854	1.788	0.688	0.858
Prob>J	0.925	0.574	0.352	0.218	0.377	0.770	0.181	0.407	0.354
F-stat	89.19	88.70	103.9	91.09	89.31	88.94	102	95.08	95.31
Control mean	9.08	36.75	5.20	59.74	5.20	19.55	135.52	266.66	110.78

Notes: Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Seed cost includes the cost of purchasing seeds. Inorganic cost includes cost of urea, potash, phosphate and fertilizer; Irrigation cost includes fuel and electricity costs. Other cost includes the cost of non-labor ploughing cost (if any) and tractor costs. The treatment intensities are the fraction of farmers trained in each village (between 0 and 1) multiplied by the treatment indicators where the base category is control group. The F-stat is from the first stage regression.

Table 6: Estimated Welfare Effects of SRI by Endline

	(1)	(2)	(3)	(4)	(5)
Panel A: ITT (Treatment Status)	Savings[a]	Household status	Food security	Life satisfaction	Satisfaction with living standard
One-time untreated (U1)	0.164	0.204**	0.297***	0.208*	0.142
	(0.266)	(0.080)	(0.109)	(0.111)	(0.097)
One-time treated (T1)	0.135	0.162***	0.371***	0.257**	0.108
	(0.146)	(0.058)	(0.102)	(0.100)	(0.092)
Two-time untreated (U2)	0.344	0.126*	0.228**	0.261***	0.174*
	(0.212)	(0.075)	(0.106)	(0.098)	(0.100)
Two-time treated (T2)	0.145	0.101	0.207*	0.234**	0.185**
	(0.157)	(0.073)	(0.107)	(0.105)	(0.089)
Baseline outcome	0.035***	0.437***	0.080***	0.042***	0.047***
	(0.013)	(0.029)	(0.031)	(0.014)	(0.014)
Observations	4,703	4,703	4,703	4,703	4,703
R^2 / Pseudo R^2	0.017	0.167	0.055	0.017	0.016
p-value (U1-T1)	0.91	0.58	0.34	0.41	0.58
p-value (U1-U2)	0.56	0.40	0.51	0.58	0.76
p-value (T1-T2)	0.95	0.39	0.10	0.80	0.40
p-value (U2-T2)	0.30	0.72	0.80	0.64	0.88
Panel B: LATE					
Adopted SRI (IV=Treatment status)	0.148	0.039	0.356**	0.291**	0.143
	(0.264)	(0.123)	(0.145)	(0.141)	(0.120)
Baseline outcome	0.048***	0.432***	0.063*	0.065***	0.061***
	(0.016)	(0.031)	(0.034)	(0.015)	(0.015)
Observations	4,119	4,119	4,119	4,119	4,119
R^2	0.023				
Adjusted R^2	0.0185				
Hansen J	0.0231				
Prob>J	0.879				
F-stat	142.5				
Control mean	106.14	4.02	4.58	7.41	7.02

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are clustered at the village level. ***Saving*** is average monthly savings over the last one year, in Bangladesh Taka (BDT). Savings is calculated as the difference between average monthly income and expenditure over the last one year. ***Household status*** is "Compared to other people in your village, would you describe your household as ….. *(Cross one box)"* [1] The poorest in the village, [2] Among the poorest in the village, [3] A little poorer than most households, [4] About average, [5] A little richer than most households, [6] Among the richest in the village, [7] The richest in the village. ***Food security*** is "How often in the last year did you have problems satisfying the food needs of the household?"[1] Always, [2] Often, [3] Sometimes, [4] Seldom, [5] Never. ***Life satisfaction*** is "All things considered, how satisfied are you with your life? Pick a number between 0 and 10 to indicate how satisfied you are. The more satisfied you are, the higher the number you should pick. The less satisfied you are, the lower the number." ***Satisfaction with living standard*** is "How satisfied are you with your standard of living? Pick a number between 0 and 10 to indicate how satisfied you are. The more satisfied you are, the higher the number you should pick. The less satisfied you are, the lower the number." As some of the savings were negative we ran normal regressions for the saving variables and present the estimated coefficients divided by the mean of the control group. Ordered Probit was used to estimate the effects of SRI training on household status, food security, life satisfaction and satisfaction with living standard in Panel A. Ordered Logit estimates yield similar marginal effects.

Table 7: SRI Adoption and Disadoption Transition Matrix

SRI Adoption End of Year 1	SRI Adoption End of Year 2		Total
	Did not Adopt	Adopted	
Did not Adopt	*(Never adopters)* 1475 (82.36%) (U1=448, T1=308, U2=386, T2=333)	*(Delayed adopters)* 316 (17.64%) (U1=29, T1=101, U2=42, T2=144)	1791 67.15%
Adopted	*(Disadopters)* 317 (36.19%) (U1=16, T1=189, U2=21, T2=91)	*(Persistent adopters)* 559 (63.81%) (U1=14, T1=208, U2=13, T2=324)	876 32.85%
N %	1792 67.19%	875 32.81%	2667 100%

Note: Estimates are based on sample who were surveyed in both year 1 and year 2.

Table 8: Factors associated with disadoption, delayed adoption or persistent adoption

Base category	vs. Never adopters			vs. Persistent adopters
Variables of Interest	Disadopted	Delayed adopters	Persistent adopters	Disadopted
	(1)	(2)	(3)	(4)
Log of baseline production	0.471	-0.524**	-0.305	0.150***
	(0.300)	(0.224)	(0.252)	(0.051)
Log of baseline cost	0.418**	-0.306	0.418**	0.008
(adjusted for family labour)	(0.201)	(0.220)	(0.203)	(0.042)
T1	2.781***	1.607***	3.083***	-0.102
	(0.295)	(0.248)	(0.303)	(0.114)
U2	0.427	0.594*	-0.056	0.073
	(0.361)	(0.355)	(0.470)	(0.137)
T2	2.010***	1.997***	3.442***	-0.336***
	(0.316)	(0.299)	(0.374)	(0.116)
Household head's age greater than 45	-0.020	0.035	-0.679***	0.141***
	(0.191)	(0.184)	(0.174)	(0.034)
Household head has primary education	0.100	0.120	0.330**	-0.057
	(0.185)	(0.183)	(0.165)	(0.037)
Baseline cultivable land above median	0.174	0.275	0.450***	-0.059
	(0.161)	(0.168)	(0.155)	(0.045)
Baseline monthly household income	-0.570	-2.011	-3.561**	0.573***
	(0.594)	(1.508)	(1.385)	(0.197)
Observations	2,540	2,540	2,540	842

Notes: Columns 1-3 reports the coefficients form a multinomial logit model where the base category is never adopters. Column 4 shows the coefficients of a logit model where the base category is persistent adopters. Both the models control for all the household characteristics used in estimating the ITT effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Plot level difference-in-differences estimates for SRI principles

	(1)	(2)	(3)	(4)	(5)	(6)
	Age of seedlings	No of seedlings	Distance between seedlings	Alternate drying & wetting	Use of organic fertilizer	Mechanical weeding
Treatment Status						
U11	-0.217	1.607	1.685**	12.255*	1.545	-1.810*
	(0.287)	(3.717)	(0.837)	(6.825)	(2.635)	(1.059)
U12	1.787*	2.342	5.497***	3.959	10.545**	9.467***
	(1.002)	(3.888)	(1.416)	(7.448)	(4.225)	(3.169)
U21	-0.026	5.130	4.106***	10.717	4.905*	-2.249**
	(0.324)	(4.149)	(1.347)	(7.102)	(2.791)	(0.987)
U22	0.605	7.458	9.195***	17.603**	10.882**	4.424**
	(0.592)	(4.576)	(1.968)	(7.194)	(4.188)	(1.856)
T11	3.352***	14.640***	15.086***	19.808***	10.398***	0.399
	(0.832)	(3.769)	(1.913)	(6.497)	(3.316)	(1.545)
T12	4.107***	20.244***	24.430***	10.689	14.416***	12.308***
	(1.185)	(4.627)	(3.058)	(7.247)	(3.972)	(3.541)
T21	2.306***	15.035***	14.317***	17.439***	11.642***	-0.617
	(0.552)	(3.772)	(1.580)	(6.447)	(3.381)	(1.184)
T22	5.885***	24.828***	30.091***	21.767***	20.634***	9.197***
	(1.333)	(4.431)	(3.132)	(6.873)	(3.903)	(2.796)
Observations	33,244	33,244	33,244	33,244	33,244	33,244
R^2	0.024	0.155	0.133	0.504	0.119	0.068
Control mean	0.33	7.24	0.29	31.31	5.23	1.19
p-value (U11-T11)	0.00	0.00	0.00	0.04	0.00	0.02
p-value (U12-T12)	0.04	0.00	0.00	0.09	0.21	0.34
p-value (T11-T12)	0.53	0.03	0.00	0.03	0.26	0.00
p-value (U11-U12)	0.04	0.75	0.00	0.14	0.01	0.00
p-value (U21-T21)	0.00	0.00	0.00	0.06	0.00	0.01
p-value (U22-T22)	0.00	0.00	0.00	0.21	0.00	0.01
p-value (T21-T22)	0.01	0.00	0.00	0.28	0.02	0.00
p-value (U21-U22)	0.24	0.27	0.00	0.19	0.12	0.00

Notes: Each of these outcome variables are dummy variables (multiplied by 100) indicating if a farmer followed that principle as recommended by BRAC for SRI. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

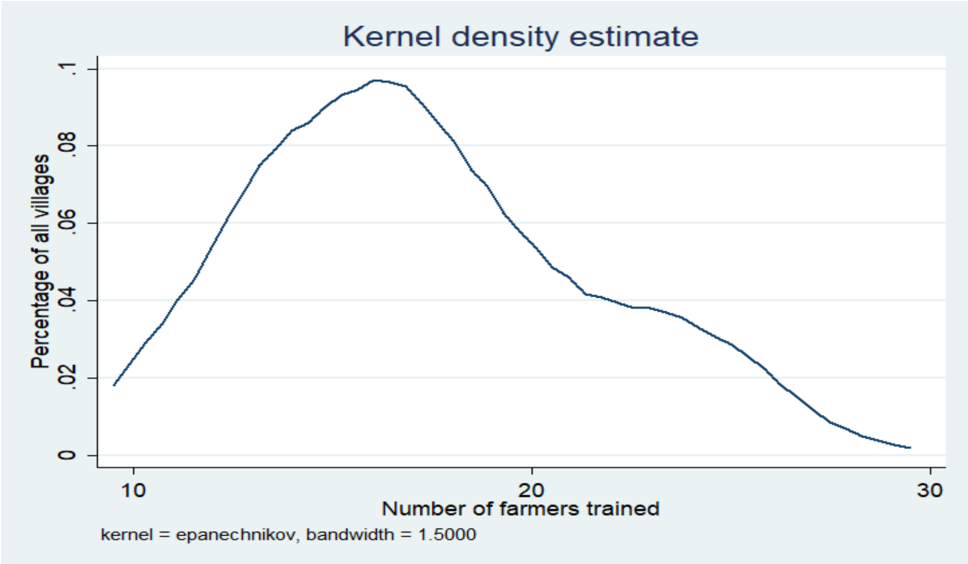
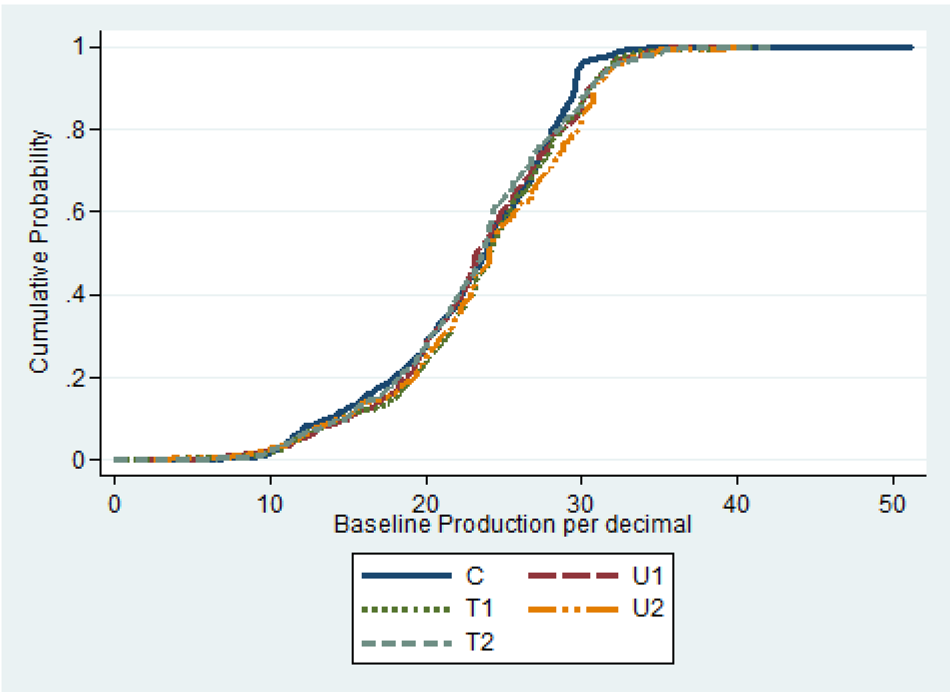


Figure 1: The distribution of the number of treated farmers across treated villages



Note: None of the five groups stochastically dominates each other based on Somers' D statistic.

Figure 2: Cumulative distribution function of baseline production per decimal of land

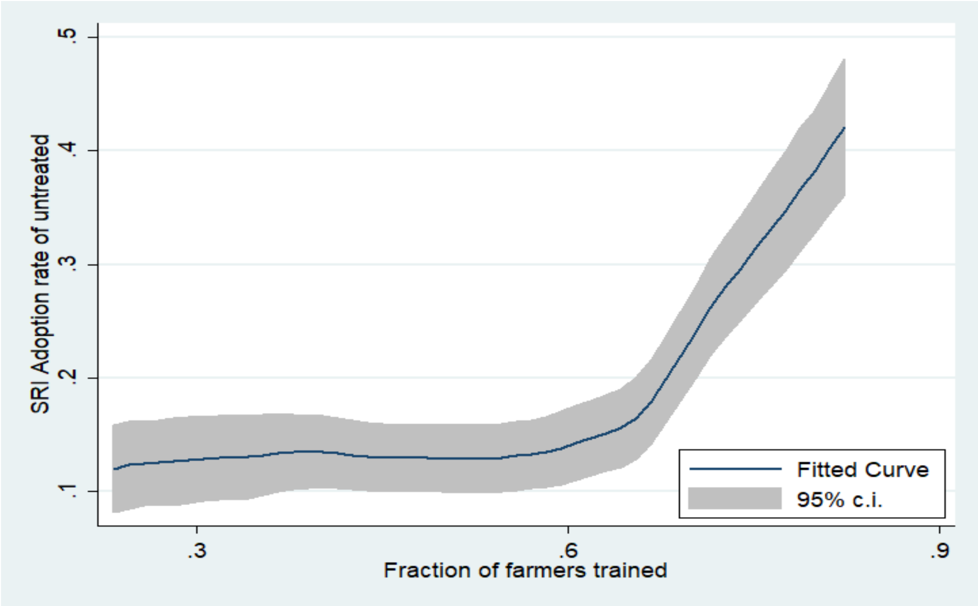
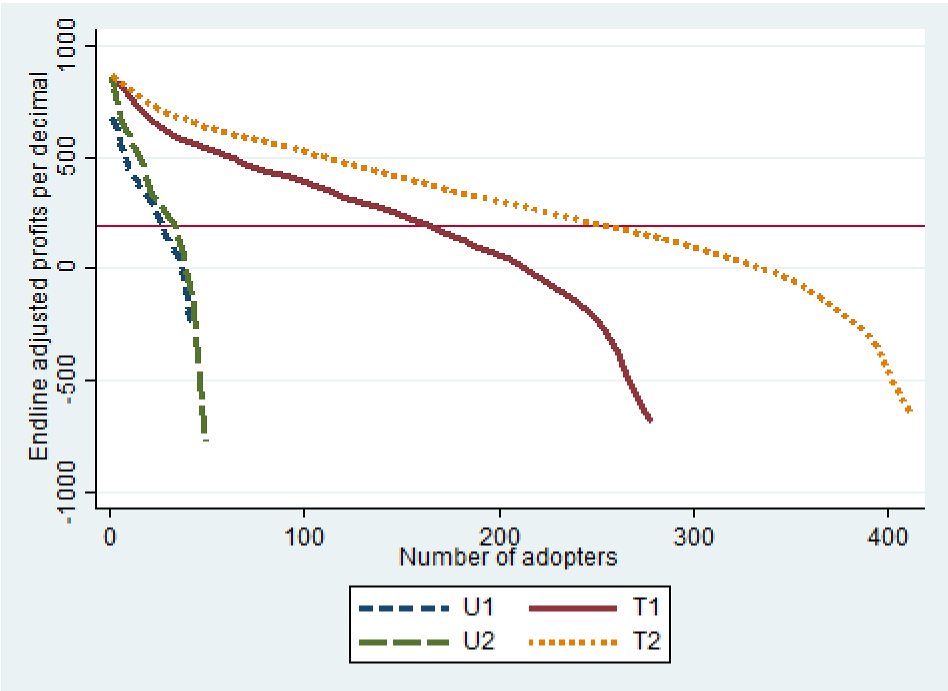
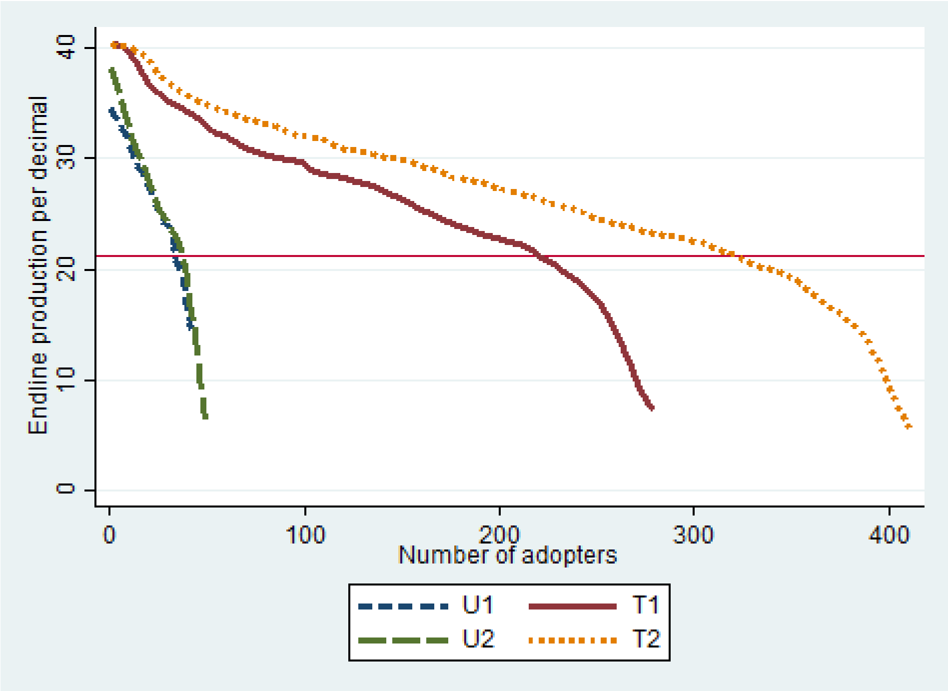


Figure 3: Village-level SRI adoption rate by proportion of treated farmers



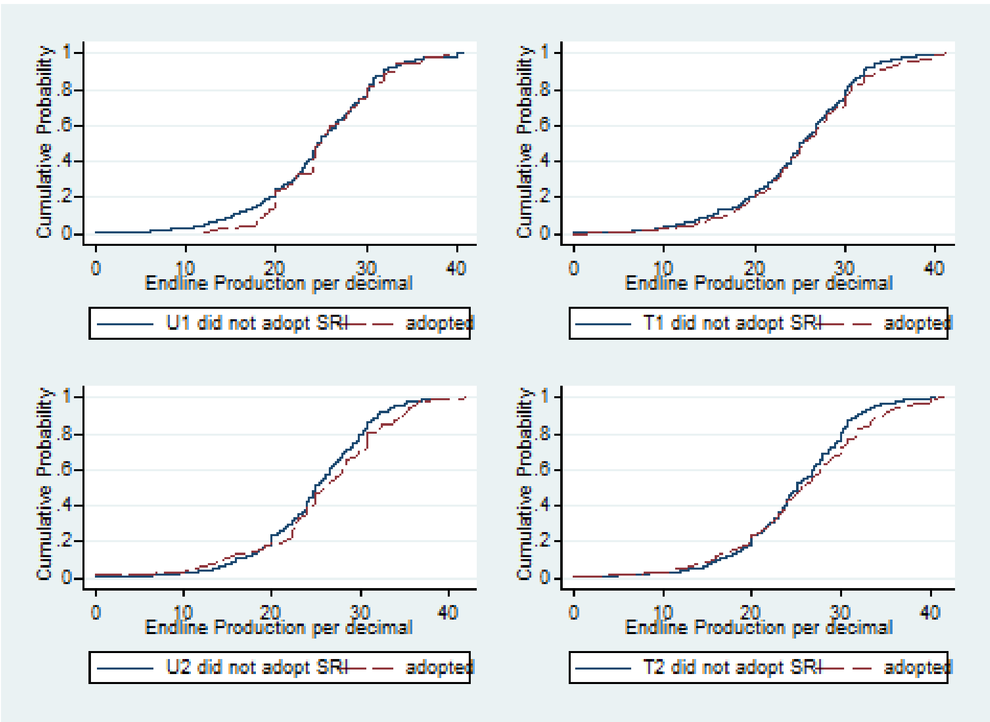
Note: The x axis shows the number of farmers who have adopted SRI, ranked from highest (1) to lowest (N) profitability within the treatment arm. The calculation of (endline) profits (profit 2 as in tables) takes into account of the imputed cost of family labour.

Figure 4: Ordered endline profits (adjusted for family labour) by treatment status



Note: The x axis shows the number of farmers who have adopted SRI, ranked from highest (1) to lowest (N) rice yield within the treatment arm.

Figure 5: Ordered endline yield by treatment status



Notes: p-value associated with Somers' D test for first order stochastic dominance for U1(adopted-did not adopt)=0.50, T1(adopted-did not adopt)=0.35, U2(adopted-did not adopt)=0.45 and T2(adopted-did not adopt)=0.20. Tests for second and third order dominance likewise find no statistically significant ordering.

Figure 6: Cumulative distribution function of endline production per decimal of land by treatment and adoption status

Online Appendix

Table A1: Sample distribution

Treatment status	No. of Villages	Total baseline farmers	Total midline (2014-15) farmers	Total endline (2015-16) farmers
Control (C)	62	1856	1663	1459
		(3983)	(3973)	(3626)
1 year training villages	60	1805	1646	1313
		(3820)	(3781)	(3438)
Trained farmers (T1)		1060	993	806
		(2330)	(2307)	(2135)
Untrained farmers (U1)		745	653	507
		(1490)	(1474)	(1303)
2 year training villages	60	1825	1625	1354
		(3786)	(3780)	(3507)
Trained farmers (T2)		1166	1051	892
		(2479)	(2474)	(2319)
Untrained farmers (U2)		659	574	462
		(1307)	(1306)	(1188)
Total	**182**	**5486**	**4934**	**4126**
		(11589)	**(11534)**	**(10571)**

Notes: Number of plot level observations used for adoption, profits, are shown in parenthesis. Most farmers have multiple plots of land for cultivation, hence the number of plot level observations are more than the number of farmers/households which are reported on top.

Randomization checks

Appendix Table A2 (Panel A) reports the basic demographic and socio-economic characteristics of the treatment and control households using data from the baseline survey round.[46] Household characteristics were similar between the treatment and control groups. There was no statistically significant difference at baseline between control and treatment households in farm size, family size, working age member, education, prior rice yields, costs, or profits, individually or jointly. The first-stage of randomization was therefore successful. In Panel B, we show the same characteristics but for farmers who were selected randomly for training and those left untreated within the treatment villages, the second stage of randomization. Again, there is also no statistically significant difference in these observable characteristics among the treated and untreated households within the treatment villages at baseline.

Appendix Table A3 compares the baseline characteristics of treated farmers in V1 and V2 villages. This comparison is relevant only for year 2 when we divided the treatment villages further, with the V2 villages selected for repeat training in year 2. We compare characteristics both at baseline and at midline, after all T1 and T2 households had received the same one year of training. The year 2 randomization was successful, as there are no significant differences of observable household characteristics between the T1 and T2 farmers at baseline (Panel A). Nor was there any significant difference in terms of rice yield, cost or profit between T1 and T2 farmers at either baseline (Table 2, Panel B) or midline, following the year one harvest, before T2 farmers received their second training (Panel C).

Appendix Table A4 provides further evidence that the randomization was successful, now concerning village level characteristics. Panel A compares village level characteristics between year 1 treatment (T) and control (C) villages, while panel B compares V1 and V2 villages. There are no significant differences between either treatment and control villages, or between V1 and V2 villages in terms of the size of the village, accessibility, electricity connection, or presence of NGOs.

[46] All monetary values are deflated to 2014 prices using rural CPI of Bangladesh.

Table A2: Baseline characteristics of farmers by treatment status

Panel A	Treatment (T)		Control (C)		p-value (T-C)
Household Characteristics	Mean	Std.dev	Mean	Std.dev	
Average Age of the household members (above 15 years)	36.75	0.13	36.43	0.18	0.14
Average Education of the adult member of household (years)	4.31	0.04	4.34	0.06	0.67
Farm size (cultivable) last Boro season (in decimals)	163.46	2.66	165.93	2.94	0.57
Household size	5.13	0.03	5.19	0.05	0.25
Maximum education by any household member	8.51	0.06	8.66	0.09	0.14
F-stat for joint equality		0.78			
p-value for joint equality		0.59			
Yield, Cost and Profit (per decimal)					
Yield (kg)	22.28	4.84	22.44	5.50	0.12
Total cost of production (BDT)	430.26	250.44	422.64	224.44	0.10
Estimated profit (BDT)	440.12	255.93	445.42	341.81	0.34
F-stat for joint equality		0.57			
p-value for joint equality		0.63			
No. of observations	3630		1856		

	Treatment Villages Only				
Panel B	Treated (T1 and T2)		Untreated (U1 and U2)		p-value (T1/T2-U1/U2)
Household Characteristics	Mean	Std.dev	Mean	Std.dev	
Average Age of the household members (above 15 years)	36.82	0.16	36.69	0.21	0.61
Average Education of the adult member of household (years)	4.34	0.05	4.29	0.07	0.59
Farm size (cultivable) last Boro season (in decimals)	161.47	2.99	166.40	4.97	0.37
Household size	5.11	0.04	5.19	0.05	0.25
Maximum education by any household member	8.54	0.07	8.52	0.10	0.85
F-stat for joint equality		0.38			
p-value for joint equality		0.89			
Yield, Cost and Profit (per decimal)					
Yield (kg)	22.35	4.88	22.17	4.78	0.13
Total cost of production (BDT)	427.63	242.69	434.77	263.54	0.23
Estimated profit (BDT)	441.22	255.92	438.23	256.42	0.62
F-stat for joint equality		0.94			
p-value for joint equality		0.42			
No. of observations	2226		1404		

Notes: Panel A compares all farmers in treatment and control villages. Panel B compares only treated farmers with those untreated from the same villages. The reported p-values are from the two-tailed test with the null hypothesis that the group means are equal. P-value compares the treated and untreated households from the treatment villages.

Table A3: Baseline characteristics of farmers by number of treatment rounds

Variables of Interest	Treatment Villages Only				
	One-time Training Village (T1)		Two-time Training Village (T2)		p-value (T2-T1)
Panel A; Household Characteristics (Baseline)	Mean	Std.dev	Mean	Std.dev	
Average Age of the household members (above 15 years)	36.44	0.24	36.97	0.23	0.11
Average Education of the adult member of household (years)	4.34	0.08	4.30	0.07	0.72
Farm size (cultivable) last *Boro* season (in decimals)	167.66	4.61	164.61	4.67	0.64
Household size	5.23	0.06	5.08	0.06	0.09
Maximum education by any household member	8.64	0.12	8.45	0.11	0.21
F-stat for joint equality			1.01		
p-value for joint equality			0.42		
No. of Observations	1060		1166		
Panel B: Yield, Cost and Profit (Baseline, per decimal)					
Yield (kg)	22.42	4.69	22.28	5.05	0.30
Total cost of production (BDT)	425.06	239.55	430.06	245.64	0.47
Estimated profit (BDT)	445.38	256.77	437.31	255.11	0.27
F-stat for joint equality			0.08		
p-value for joint equality			0.99		
No. of Observations	1060		1166		
Panel C: Yield, Cost and Profit (Midline)					
SRI Adoption	49.72	50.01	49.19	50.00	0.72
Yield (kg/decimal)	26.28	7.43	26.06	6.87	0.29
Total cost of production (BDT/decimal)	315.71	112.61	310.89	111.75	0.14
Estimated profit (BDT/decimal)	526.93	243.37	530.63	236.78	0.60
F-stat for joint equality			0.58		
p-value for joint equality			0.63		
No. of Observations	993		1051		

Notes: Panel A compares baseline household level characteristics for one time and two-time training villages. Panel B compares the same for baseline output, profits, etc. Panel C shows the difference between onetime and two-time training farmers' adoption, yield, cost, etc. during midline. to show that the farmers were otherwise similar. Standard errors are clustered at the village level. P-value shows level of significance for the difference between farmers in villages with one-time and two-time training.

Table A4: Village level baseline characteristics- treatment and control villages

Panel A: Treatment vs Control	Treat (T)		Control (C)		p-value
	Mean	Std.dev	Mean	Std.dev	(T – C)
Transport System in village (good=1)	0.325	0.04	0.403	0.06	0.30
Road in village (brick built=1)	0.300	0.04	0.355	0.06	0.45
Road between village and Upazila (brick built=1)	0.858	0.03	0.919	0.03	0.23
Distance between village and nearest Upazila (km)	11.52	0.98	12.35	1.48	0.63
Number of NGOs in the villages	4.975	0.18	4.820	0.16	0.58
Number of households in the villages	351.6	33.1	356.1	48.2	0.94
Electricity connection (yes=1)	0.900	0.03	0.919	0.03	0.67
F-stat for Joint equality		0.85			
p-value for Joint equality		0.55			
No. of Observations	120		62		

	Treatment Villages Only				
Panel B: V1 vs V2	Two-time training Village (V2)		One-time Training Village (V1)		p-value (V2-V1)
	Mean	Std.dev	Mean	Std.dev	
Transport System in village (good=1)	0.350	0.06	0.300	0.06	0.56
Road in village (brick built=1)	0.350	0.06	0.250	0.06	0.24
Road between village and Upazila (brick built=1)	0.867	0.04	0.850	0.05	0.80
Distance between village and nearest Upazila (km)	11.23	1.34	11.81	1.41	0.76
Number of NGOs in the villages	5.017	0.30	4.933	0.21	0.82
Number of households in the villages	305.53	32.92	396.93	56.84	0.17
Electricity connection (yes=1)	0.917	0.04	0.883	0.04	0.55
F-stat for Joint equality		1.24			
p-value for Joint equality		0.29			
No. of Observations	60		60		

Notes: The reported p-values are from the two-tailed test with the null hypothesis that the group means are equal. All villages have presence of at least one NGO.

Attrition

Appendix Table A5 reports balance tests to see if sample attrition occurred differentially among households with different treatment status. Panel A shows the attrition by treatment status in midline. Overall attrition was about 10% (Table 1) but it was slightly, but statistically insignificantly, higher (10.4%) in control villages than in treatment villages (9.8%). There is no individually nor jointly significant difference in attrition in terms of observed baseline household characteristics between treatment and control villages. In panel B, we examine the attrition at endline by the treatment status of villages: V1 versus V2 villages. Attrition was a bit higher from midline to endline (16.4%, Table 1) than from baseline to midline, and also a bit (but insignificantly) higher in V1 villages than V2 villages. When we compare the attrition status based on observables at the baseline we again find no individually nor jointly significant difference among attritors between treatment and control villages or between V1 and V2 villages. Just one of 14 individual test statistics has a p-value less than 0.05 and the joint test p-values are all well above 0.1. We also checked whether attrition is different between farmers who received training (T1 and T2) versus those did not (U1 and U2, panel C, Table A5). Again, there are no statistically significant differences across treatment groups.

We also examined the correlates of attrition using attrition status in each round as a dependent variable in a linear probability regression model. The results (Appendix Table A6) do not suggest any predictor for the attrition that differs by treatment status. We see that larger farms had a higher likelihood of attrition. But when we interacted land size with each treatment status, those interactions are not individually or jointly statistically significant (results not presented here). Thus, there was no obvious candidate for differential attrition due to different treatment statuses. Overall, Tables A5 and A6 strongly suggest that attrition is adequately random in our sample and should have no impact on causal inference based on the randomization.

Table A5: Attrition by treatment status: Balance test

	Control	Treat	p-value
Panel A: End of year 1 (Midline)	(1)	(2)	(2)-(1)
Average Age of the household members (above 15 years)	37.01	37.10	0.90
Average Education of the adult member of household (years)	4.36	4.55	0.41
Farm size (cultivable) last Boro season (in decimals)	141.52	141.03	0.97
Number of People in the households	4.68	4.93	0.16
Yield (kg/decimal)	21.76	22.42	0.73
Total cost of production	471.93	395.29	0.19
Estimated profit	458.68	514.37	0.28
F-stat for joint equality	0.70		
p-value for joint equality	0.59		
No. of observations	193	359	
Total sample at the baseline	1856	3630	

		Treatment villages			p-value	
Panel B: End of year 2 (Endline)	Control	All	T1	T2	(1)	(2)
Average Age of the household members (above 15 years)	36.52	36.67	36.20	37.23	0.82	0.17
Average Education of the adult member of household (years)	4.13	4.03	4.28	3.73	0.62	**0.01**
Farm size (cultivable) last *Boro* season (in decimals)	148.09	137.76	141.41	133.52	0.29	0.46
Number of People in the households	4.97	4.82	4.93	4.70	0.27	0.12
Yield (kg/decimal)	21.88	21.39	21.60	21.18	0.38	0.54
Total cost of production (BDT/decimal)	368.97	366.79	365.54	368.34	0.89	0.87
Estimated profit (BDT/decimal)	450.06	460.21	475.65	440.15	0.68	0.17
No. of observations	204	604	333	271		

Panel C: End of year 2 (Endline)	Control	All treatment Households			p-value	
		Overall	Treated	Untreated	(1)	(2)
Average Age of the household members (above 15 years)	36.52	36.67	37.03	36.23	0.29	0.70
Average Education of the adult member of household (years)	4.13	4.03	4.00	4.06	0.77	0.80
Farm size (cultivable) last *Boro* season (in decimals)	148.09	137.76	129.03	148.64	0.07	0.96
Number of People in the households	4.97	4.82	4.71	4.96	0.10	0.94
Yield (kg/decimal)	21.88	21.39	21.35	21.44	0.89	0.52
Total cost of production (BDT/decimal)	368.97	366.79	361.18	373.75	0.47	0.82
Estimated profit (BDT/decimal)	450.06	460.21	453.90	468.21	0.58	0.58
No. of observations	204	604	346	258		

Notes: The reported p-values are from the two-tailed test with the null hypothesis that the group means are equal. In Panel A, the p-value compares control with all treatment households for year 1 atritors. T1 farmers received training in year 1 only. T2 received training in both years- year 1 and year 2. As for Panel B, p-value (1) compares control and all treatment households while p-value (2) compares one-time and two-time training households for year 2 atritors. In Panel C, p-value (1) compares treated and untreated households while p-value (2) compares control and untreated households for year 2 atritors.

Table A6: Attrition Regressions

	(1)	(2)	(3)
Variables of Interest	Incidence of attrition		
Treat (Base: Control)	0.002		
	(0.019)		
Two-time (Base: One-time)		-0.019	
		(0.026)	
Treated (Base: Control)			-0.011
			(0.019)
Untreated			0.024
			(0.022)
Household head's age greater than 45	-0.008	-0.016	-0.007
	(0.012)	(0.015)	(0.012)
Household head has primary education	-0.012	-0.023	-0.011
	(0.014)	(0.017)	(0.014)
Baseline cultivable land above median	-0.043***	-0.039**	-0.043***
	(0.013)	(0.017)	(0.013)
Observations	4,681	3,046	4,681
R^2	0.012	0.016	0.013

Note: Attrition between baseline and endline. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: ITT and LATE - Effects of Treatment Status at Midline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ITT	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
One-time untreated (U1)	7.344***	0.105***	0.099***	0.106**	0.068**	0.117***	0.181**
	(1.199)	(0.025)	(0.025)	(0.049)	(0.031)	(0.043)	(0.076)
One-time treated (T1)	49.855***	0.140***	0.134***	0.120**	0.100***	0.185***	0.249***
	(3.905)	(0.026)	(0.025)	(0.048)	(0.030)	(0.045)	(0.077)
Two-time untreated (U2)	8.606***	0.129***	0.133***	0.107**	0.049	0.177***	0.317***
	(1.472)	(0.024)	(0.026)	(0.053)	(0.030)	(0.040)	(0.075)
Two-time treated (T2)	49.228***	0.124***	0.127***	0.084	0.077***	0.202***	0.275***
	(3.839)	(0.027)	(0.028)	(0.052)	(0.029)	(0.043)	(0.078)
Baseline outcome		0.147***	0.159***	0.112***	0.035**	0.018	0.031
		(0.022)	(0.023)	(0.028)	(0.015)	(0.018)	(0.022)
Observations	11,506	11,175	11,175	11,159	11,159	11,159	11,159
R^2	0.316	0.069	0.073	0.087	0.064	0.047	0.057
p-value (U1-T1)	0.00	0.05	0.05	0.58	0.05	0.03	0.21
p-value (U1-U2)	0.50	0.42	0.29	0.98	0.52	0.20	0.13
p-value (T1-T2)	0.91	0.63	0.83	0.49	0.43	0.73	0.79
p-value (U2-T2)	0.00	0.78	0.75	0.46	0.08	0.36	0.40
Panel B: LATE							
Adopted SRI (IV=Treatment status)		0.182***	0.179***	0.130**	0.140***	0.289***	0.348***
		(0.035)	(0.035)	(0.063)	(0.038)	(0.055)	(0.096)
Baseline outcome		0.150***	0.162***	0.113***	0.032**	0.024	0.036
		(0.023)	(0.023)	(0.028)	(0.016)	(0.018)	(0.022)
Observations		11,175	11,175	11,159	11,159	11,159	11,159
R^2		0.026	0.019	0.030	0.015	0.026	0.030
Adjusted R^2		0.0248	0.0174	0.0282	0.0131	0.0248	0.0282
Hansen J		0.145	0.0138	0.357	0.410	0.187	0.105
Prob>J		0.703	0.907	0.550	0.522	0.665	0.746
F-stat		155	154.7	155	156.9	156.2	156.9
Control mean	0.00	22.36	728.73	283.61	447.60	445.13	281.13

Notes: Labor cost 1, total cost 1 and profit 1 do not include the cost of family labor. Family labor cost is included in the variables labor cost 2, total cost 2 and profit 2 using market wage. Yield, cost and revenue are expressed in logarithms. As some of the profits were negative we ran normal regressions for the profit variables and present the estimated coefficients divided by the mean of the control group. The F-stat is from the first stage regression. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.

Table A8: ITT and IV – Effects Treatment Intensity at Midline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: ITT	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
One-time untreated (U1F)	10.388***	0.160***	0.142***	0.189**	0.132**	0.158*	0.230
	(3.122)	(0.051)	(0.052)	(0.094)	(0.058)	(0.085)	(0.148)
One-time treated (T1F)	85.763***	0.195***	0.179***	0.177**	0.169***	0.245***	0.292*
	(7.932)	(0.054)	(0.052)	(0.089)	(0.052)	(0.091)	(0.148)
Two-time untreated (U2F)	14.510***	0.193***	0.193***	0.157*	0.077	0.260***	0.465***
	(4.013)	(0.048)	(0.051)	(0.094)	(0.053)	(0.077)	(0.136)
Two-time treated (T2F)	77.612***	0.151***	0.149***	0.077	0.106**	0.257***	0.308**
	(6.364)	(0.047)	(0.048)	(0.091)	(0.048)	(0.075)	(0.132)
Baseline outcome		0.149***	0.158***	0.111***	0.035**	0.018	0.029
		(0.023)	(0.023)	(0.027)	(0.016)	(0.018)	(0.022)
Observations	11,506	11,175	11,175	11,159	11,159	11,159	11,159
R^2	0.316	0.057	0.059	0.083	0.063	0.036	0.047
p-value (U1F-T1F)	0.00	0.31	0.29	0.79	0.20	0.12	0.54
p-value (U1F-U2F)	0.33	0.59	0.43	0.76	0.34	0.27	0.19
p-value (T1F-T2F)	0.40	0.48	0.63	0.30	0.22	0.90	0.92
p-value (U2F-T2F)	0.00	0.23	0.21	0.14	0.32	0.95	0.10
Panel B: LATE							
Adopted SRI (IV=Treatment intensity)		0.151***	0.143***	0.101	0.135***	0.243***	0.249**
		(0.039)	(0.040)	(0.072)	(0.042)	(0.061)	(0.107)
Baseline outcome		0.151***	0.162***	0.115***	0.033**	0.022	0.031
		(0.023)	(0.023)	(0.028)	(0.016)	(0.018)	(0.022)
Observations		11,175	11,175	11,159	11,159	11,159	11,159
R^2		0.035	0.031	0.042	0.017	0.033	0.037
Adjusted R^2		0.0338	0.0297	0.0403	0.0152	0.031	0.036
Hansen J		0.161	0.0329	0.715	0.694	0.208	0.0911
Prob>J		0.688	0.856	0.398	0.405	0.649	0.763
F-stat		132.2	131.1	131.7	132.4	131.8	131.6
Control mean	0.00	22.36	728.73	283.61	447.60	445.13	281.13

Notes: Labor cost 1, total cost 1 and profit 1 do not include the cost of family labor. Family labor cost is included in the variables labor cost 2, total cost 2 and profit 2 using market wage. Yield, cost and revenue are expressed in logarithms. As some of the profits were negative we ran normal regressions for the profit variables and present the estimated coefficients divided by the mean of the control group. The treatment intensity is the fraction farmers trained within each village (between 0 and 1) multiplied by the treatment indicators where the base category is control group. The F-stat is from the first stage regression. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.

Table A9: Nonlinearity in the ITT Effect of Treatment Intensity at Endline (cut-off intensity = 60%)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
One-time untreated (U1F)	18.188***	0.297***	0.274***	0.270*	0.287***	0.369	0.349
	(5.210)	(0.072)	(0.078)	(0.161)	(0.107)	(0.235)	(0.381)
U1F cut-off (more than 60%)	-2.669	-0.074	-0.071	-0.085	-0.147	-0.143	-0.017
	(4.805)	(0.060)	(0.065)	(0.134)	(0.102)	(0.232)	(0.398)
One-time treated (T1F)	74.364***	0.288***	0.265***	0.284*	0.321***	0.339	0.234
	(10.051)	(0.073)	(0.080)	(0.166)	(0.107)	(0.233)	(0.375)
T1F cut-off (more than 60%)	-10.339	-0.070	-0.066	-0.059	-0.088	-0.180	-0.154
	(8.956)	(0.058)	(0.064)	(0.136)	(0.092)	(0.190)	(0.305)
Two-time untreated (U2F)	20.387***	0.350***	0.366***	0.161	0.229**	0.721***	0.860**
	(6.937)	(0.074)	(0.085)	(0.173)	(0.103)	(0.251)	(0.423)
U2F cut-off (more than 60%)	8.719	-0.181**	-0.199**	0.008	-0.023	-0.491**	-0.710*
	(7.398)	(0.076)	(0.080)	(0.125)	(0.077)	(0.231)	(0.391)
Two-time treated (T2F)	94.112***	0.361***	0.372***	0.106	0.208**	0.836***	0.946**
	(11.959)	(0.065)	(0.075)	(0.152)	(0.095)	(0.221)	(0.372)
T2F cut-off (more than 60%)	-9.200	-0.125**	-0.135**	0.039	0.009	-0.495**	-0.667**
	(10.170)	(0.051)	(0.056)	(0.115)	(0.075)	(0.204)	(0.332)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.290	0.082	0.099	0.051	0.046	0.030	0.020
Control mean	0.00	22.36	728.73	283.61	447.60	445.13	281.13

Notes: Labor cost 1, total cost 1 and profit 1 do not include the cost of family labor. Family labor cost is included in the variables labor cost 2, total cost 2 and profit 2 using market wage. Yield, cost and revenue are expressed in logarithms. As some of the profits were negative we ran normal regressions for the profit variables and present the estimated coefficients divided by the mean of the control group. The treatment intensities provide the interaction between treatment status and fraction trained within each village (between 0 and 1) where the base category is control group. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.

Table A10: Plot level difference-in-differences ITT estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
Treatment Status							
U11	6.860***	0.091**	0.118***	0.117*	0.020	0.179**	0.283**
	(1.232)	(0.036)	(0.040)	(0.062)	(0.047)	(0.088)	(0.124)
U12	9.282***	0.126***	0.152***	0.149*	0.072	0.163	0.223*
	(1.907)	(0.037)	(0.039)	(0.087)	(0.065)	(0.100)	(0.125)
U21	8.232***	0.101***	0.134***	0.095	-0.019	0.205**	0.350***
	(1.407)	(0.037)	(0.041)	(0.066)	(0.048)	(0.087)	(0.123)
U22	12.313***	0.112***	0.147***	0.110	0.076	0.155	0.181
	(2.487)	(0.040)	(0.043)	(0.094)	(0.070)	(0.103)	(0.131)
T11	50.024***	0.114***	0.135***	0.125**	0.029	0.204**	0.314**
	(4.007)	(0.035)	(0.039)	(0.059)	(0.048)	(0.088)	(0.127)
T12	37.892***	0.115***	0.136***	0.167*	0.084	0.100	0.152
	(3.644)	(0.039)	(0.041)	(0.091)	(0.069)	(0.097)	(0.124)
T21	49.272***	0.114***	0.137***	0.081	-0.006	0.261***	0.387***
	(4.004)	(0.038)	(0.042)	(0.071)	(0.054)	(0.092)	(0.131)
T22	53.306***	0.148***	0.173***	0.123	0.074	0.202*	0.252*
	(4.289)	(0.037)	(0.041)	(0.100)	(0.077)	(0.118)	(0.151)
Observations	34,510	32,660	32,660	32,108	32,108	32,108	32,108
R^2	0.343	0.027	0.129	0.157	0.038	0.114	0.087
Control mean	0.00	22.84	875.89	411.33	509.51	464.56	366.38
p-value (U11-T11)	0.00	0.33	0.48	0.83	0.76	0.59	0.63
p-value (U12-T12)	0.00	0.66	0.53	0.70	0.75	0.18	0.24
p-value (T11-T12)	0.00	0.99	0.99	0.42	0.21	0.07	0.04
p-value (U11-U12)	0.15	0.33	0.35	0.55	0.26	0.78	0.44
p-value (U21-T21)	0.00	0.57	0.88	0.76	0.70	0.19	0.56
p-value (U22-T22)	0.00	0.13	0.31	0.83	0.97	0.48	0.44
p-value (T21-T22)	0.20	0.31	0.30	0.41	0.08	0.34	0.10
p-value (U21-U22)	0.12	0.77	0.72	0.79	0.04	0.44	0.05

Note 1: The row corresponding U11 indicates outcomes of untreated farmers from one time treatment village at the midline, while U21 denotes the corresponding outcomes for the untreated farmers from two-time treatment villages at midline.

Note 2: Same as Table 2.

Table A11: Plot level difference-in-differences ITT estimates (with continuous treatment intensity)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
Treatment Status							
U11F	9.491***	0.103	0.142*	0.197	0.036	0.240	0.410*
	(3.248)	(0.070)	(0.076)	(0.121)	(0.092)	(0.164)	(0.233)
U12F	15.670***	0.170**	0.205***	0.205	0.058	0.247	0.390
	(3.960)	(0.074)	(0.078)	(0.176)	(0.138)	(0.188)	(0.243)
U21F	13.876***	0.139**	0.186**	0.128	-0.043	0.314**	0.546**
	(4.017)	(0.069)	(0.075)	(0.124)	(0.089)	(0.155)	(0.219)
U22F	23.549***	0.158**	0.208**	0.117	0.068	0.266	0.337
	(5.752)	(0.075)	(0.080)	(0.169)	(0.124)	(0.184)	(0.235)
T11F	86.350***	0.145**	0.170**	0.165	0.036	0.282*	0.443**
	(8.067)	(0.062)	(0.069)	(0.113)	(0.087)	(0.147)	(0.213)
T12F	63.792***	0.143*	0.166**	0.201	0.067	0.134	0.251
	(7.155)	(0.074)	(0.078)	(0.178)	(0.136)	(0.166)	(0.217)
T21F	77.729***	0.140**	0.163**	0.059	-0.035	0.378**	0.559***
	(6.672)	(0.060)	(0.067)	(0.133)	(0.097)	(0.146)	(0.208)
T22F	84.555***	0.200***	0.225***	0.110	0.047	0.316	0.428
	(7.002)	(0.061)	(0.068)	(0.188)	(0.148)	(0.205)	(0.269)
Observations	34,510	32,660	32,660	32,108	32,108	32,108	32,108
R^2	0.344	0.023	0.124	0.155	0.036	0.113	0.085
Control mean	0.00	22.84	875.89	411.33	509.51	464.56	366.38
p-value (U11F-T11F)	0.00	0.38	0.57	0.66	0.99	0.63	0.79
p-value (U12F-T12F)	0.00	0.59	0.45	0.97	0.91	0.23	0.25
p-value (T11F-T12F)	0.00	0.97	0.96	0.71	0.71	0.14	0.16
p-value (U11F-U12F)	0.07	0.33	0.35	0.94	0.81	0.95	0.90
p-value (U21F-T21F)	0.00	0.99	0.62	0.41	0.89	0.44	0.91
p-value (U22F-T22F)	0.00	0.36	0.73	0.96	0.82	0.68	0.58
p-value (T21F-T22F)	0.21	0.25	0.24	0.54	0.30	0.56	0.34
p-value (U21F-U22F)	0.12	0.78	0.74	0.90	0.17	0.68	0.18

Note: Labor cost 1, total cost 1 and profit 1 do not include the cost of family labor. Family labor cost is included in the variables labor cost 2, total cost 2 and profit 2 using market wage. Yield, cost and revenue are expressed in logarithms. As some of the profits were negative we ran normal regressions for the profit variables and present the estimated coefficients divided by the mean of the control group. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Basic characteristics of different groups of farmer households by adoption status

	Persistent adopters	Disadopters	Delayed adopters	Never adopters	p-value (1)	p-value (2)	p-value (3)	p-value (4)
Household head age dummy (older than 45 yrs=1)	0.48	0.64	0.60	0.61	0.00	0.26	0.80	0.00
Household head education dummy (have primary edu=1)	0.59	0.52	0.54	0.49	0.00	0.31	0.07	0.05
Cultivable land dummy (land>120 decimals=1)	0.63	0.58	0.57	0.53	0.00	0.12	0.19	0.19
Cultivable land (in decimals)	179.64	176.64	180.18	166.42	0.10	0.35	0.19	0.80
Monthly Household income (in lakh BDT)	0.11	0.13	0.12	0.14	0.00	0.26	0.14	0.00
Baseline production (in kg per decimal)	23.23	24.65	22.06	23.54	0.09	0.00	0.00	0.00
Production at end of year 1	26.08	27.86	24.94	25.66	0.05	0.00	0.01	0.00
Production at end of year 2	24.78	24.26	26.42	24.51	0.24	0.37	0.00	0.14
Baseline cost of production	471.63	454.12	399.70	424.66	0.00	0.00	0.01	0.20
Total cost at the end of year 1	275.12	312.36	310.23	338.11	0.00	0.00	0.00	0.00
Total cost at the end of year 2	405.37	423.74	532.16	486.90	0.00	0.00	0.00	0.20
Baseline cost of production (including family labor)	630.89	610.44	527.79	542.63	0.00	0.00	0.19	0.96
Total cost at the end of year 1 (including family labor)	475.11	506.17	473.13	493.60	0.00	0.02	0.00	0.00
Total cost at the end of year 2 (including family labor)	585.37	580.98	673.56	615.54	0.00	0.00	0.00	0.76
Baseline profit	364.89	434.78	417.99	444.75	0.00	0.35	0.01	0.00
Profit at end of year 1	557.62	564.66	520.60	500.17	0.00	0.00	0.02	0.51
Profit at end of year 2	387.45	338.41	351.30	316.30	0.00	0.13	0.02	0.00
Baseline profit (adjusted for family labor)	205.63	278.46	289.90	326.78	0.00	0.00	0.00	0.00
Profit at end of year 1 (adjusted for family labor)	357.63	370.85	357.70	344.68	0.10	0.01	0.18	0.00
Profit at end of year 2 (adjusted for family labor)	208.15	183.50	209.96	188.97	0.11	0.71	0.16	0.00
Extent of risk aversion[a] (risk loving=1)	0.55	0.63	0.68	0.81	0.09	0.29	0.43	0.02
Cognitive test[a] (on a scale of 0 to 10)	4.42	4.66	4.38	4.70	0.00	0.69	0.00	0.01
Memory test[a] (on a scale of 0 to 10)	5.50	5.77	5.02	5.38	0.04	0.00	0.00	0.00
Sample size- (number of farmers)	559	317	316	1475				

Notes: p-value of the difference between (1) persistent adopters and never adopters, (2) disadopters and never adopters, (3) delayed adopters and never adopters, and (4) disadopters and persistent adopters. [a]These statistics are available only for the farmers who received SRI training, as these tests were conducted during the training activity.

Table A13: Test for Heterogeneous ITT effects

	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
Baseline production (BY)	-0.711	0.061*	0.065	0.292***	0.155***	0.136	0.298*
	(0.485)	(0.033)	(0.039)	(0.058)	(0.038)	(0.098)	(0.160)
U_1*BY	2.953	0.005	-0.052	-0.401***	-0.247***	0.320**	0.411*
	(3.924)	(0.042)	(0.047)	(0.081)	(0.058)	(0.137)	(0.225)
T_1*BY	-5.723	-0.058	-0.121**	-0.521***	-0.308***	0.372***	0.405*
	(4.810)	(0.046)	(0.049)	(0.077)	(0.051)	(0.126)	(0.213)
U_2*BY	-4.493	-0.067	-0.117**	-0.368***	-0.259***	0.168	0.267
	(3.907)	(0.048)	(0.051)	(0.098)	(0.060)	(0.178)	(0.298)
T_2*BY	-7.582	-0.080*	-0.127***	-0.383***	-0.252***	0.182	0.240
	(5.662)	(0.041)	(0.045)	(0.079)	(0.050)	(0.137)	(0.222)
Observations	10,085	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.288	0.088	0.108	0.094	0.083	0.042	0.035
Baseline cultivable land	0.273	0.070***	0.077***	0.141***	0.069***	0.001	0.107
(BCL)	(0.511)	(0.025)	(0.025)	(0.037)	(0.023)	(0.062)	(0.101)
U_1*BCL	0.665	-0.013	0.000	-0.005	-0.022	-0.017	0.043
	(2.922)	(0.037)	(0.038)	(0.066)	(0.044)	(0.113)	(0.184)
T_1*BCL	2.793	-0.073*	-0.063	-0.099	-0.048	-0.092	-0.194
	(4.503)	(0.039)	(0.040)	(0.066)	(0.045)	(0.117)	(0.186)
U_2*BCL	10.383***	-0.053	-0.045	-0.024	-0.019	-0.065	-0.093
	(3.639)	(0.043)	(0.044)	(0.063)	(0.041)	(0.117)	(0.194)
T_2*BCL	7.901*	-0.057*	-0.050	-0.025	-0.038	-0.068	-0.046
	(4.232)	(0.033)	(0.033)	(0.063)	(0.038)	(0.111)	(0.173)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.287	0.086	0.104	0.058	0.056	0.020	0.013
Baseline Household size	1.342	-0.006	-0.012	-0.055*	-0.017	0.023	-0.007
(BHS)	(1.379)	(0.016)	(0.017)	(0.033)	(0.024)	(0.055)	(0.087)
U_1*BHS	-0.946	-0.051	-0.041	0.011	-0.018	-0.013	0.052
	(2.411)	(0.032)	(0.035)	(0.067)	(0.049)	(0.122)	(0.196)
T_1*BHS	8.101**	0.028	0.037	0.049	0.024	0.014	-0.001
	(3.686)	(0.035)	(0.037)	(0.060)	(0.043)	(0.108)	(0.181)
U_2*BHS	-5.708*	-0.021	-0.024	0.040	0.001	-0.050	-0.032
	(3.218)	(0.035)	(0.036)	(0.060)	(0.043)	(0.117)	(0.191)
T_2*BHS	-2.047	0.017	0.019	-0.020	-0.042	0.108	0.216
	(3.788)	(0.028)	(0.029)	(0.054)	(0.039)	(0.092)	(0.141)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.288	0.086	0.103	0.059	0.057	0.020	0.013
Baseline Working adults	4.363***	0.007	0.006	-0.087**	-0.010	0.113*	0.081
(BWA)	(1.447)	(0.021)	(0.022)	(0.036)	(0.025)	(0.058)	(0.094)
U_1*BWA	-1.506	-0.026	-0.022	0.066	0.004	-0.080	-0.020
	(2.308)	(0.031)	(0.033)	(0.061)	(0.047)	(0.117)	(0.191)
T_1*BWA	-0.311	-0.013	-0.007	0.019	-0.003	-0.045	-0.083
	(3.519)	(0.035)	(0.036)	(0.052)	(0.037)	(0.095)	(0.159)
U_2*BWA	1.550	-0.008	-0.007	0.102*	0.029	-0.156	-0.125
	(3.445)	(0.034)	(0.037)	(0.060)	(0.045)	(0.105)	(0.189)
T_2*BWA	-8.854**	-0.036	-0.033	0.082*	0.006	-0.177**	-0.154
	(3.767)	(0.029)	(0.030)	(0.047)	(0.032)	(0.086)	(0.134)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.287	0.085	0.103	0.059	0.056	0.021	0.012

	Adoption	Yield	Revenue	TC1	TC2	Profit 1	Profit 2
Baseline Household Income (BHI)	-0.492	0.049**	0.060**	0.189***	0.083***	-0.105	-0.062
	(0.685)	(0.021)	(0.023)	(0.044)	(0.030)	(0.064)	(0.104)
U_1*BHI	-6.137*	-0.010	-0.021	0.002	-0.057	-0.040	0.111
	(3.252)	(0.038)	(0.041)	(0.065)	(0.045)	(0.110)	(0.180)
T_1*BHI	-6.708*	0.019	0.003	-0.090	-0.069	0.157	0.222
	(3.940)	(0.035)	(0.037)	(0.064)	(0.042)	(0.114)	(0.178)
U_2*BHI	-0.926	0.003	-0.001	-0.025	-0.100**	0.093	0.337
	(4.214)	(0.045)	(0.049)	(0.069)	(0.044)	(0.128)	(0.214)
T_2*BHI	-12.908***	0.017	0.017	0.009	-0.010	0.011	0.046
	(4.183)	(0.032)	(0.034)	(0.070)	(0.044)	(0.116)	(0.182)
Observations	10,297	8,830	8,830	8,821	8,821	8,821	8,821
R^2	0.292	0.090	0.108	0.078	0.061	0.022	0.013

Notes: Total Cost 1 (TC1) and profit 1 do not include the cost of family labor. Yield, cost and revenue are expressed in logarithms. As some of the profits were negative we ran normal regressions for the profit variables and present the estimated coefficients divided by the mean of the control group. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.01. We do not report the coefficients on the treatment status dummies. They are similar to those presented in Table 5 in all four regressions.

Table A14: Quantile Regressions

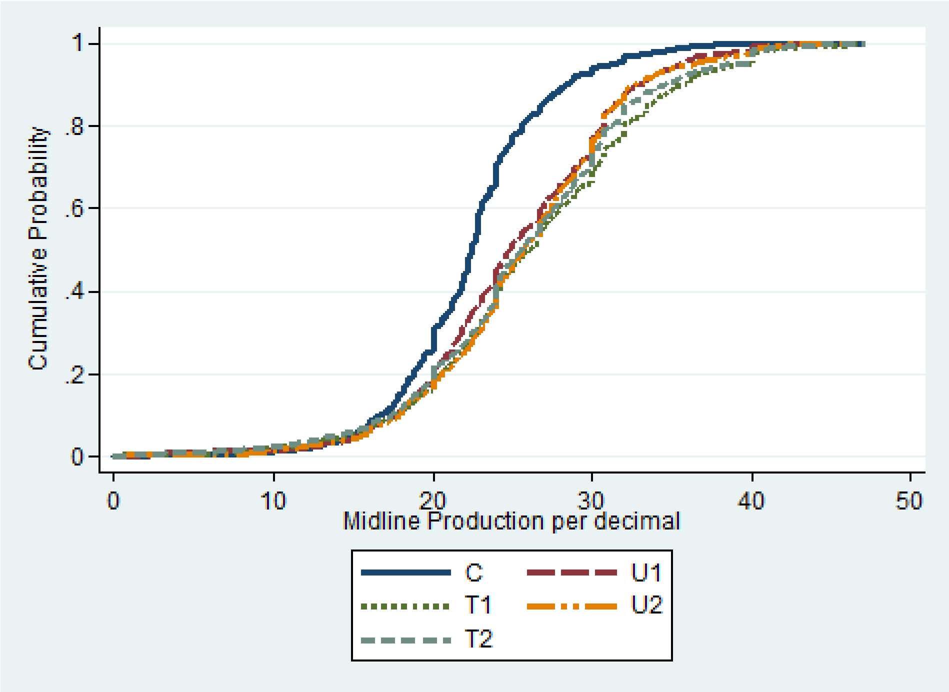
	Yield			Revenue		
Panel A: Treatment Status	0.25	0.5	0.75	0.25	0.5	0.75
One-time untreated (U1)	0.149***	0.121***	0.148***	0.140***	0.123***	0.131***
	(0.015)	(0.008)	(0.009)	(0.014)	(0.009)	(0.011)
One-time treated (T1)	0.144***	0.130***	0.168***	0.135***	0.131***	0.145***
	(0.014)	(0.007)	(0.010)	(0.016)	(0.010)	(0.010)
Two-time untreated (U2)	0.154***	0.129***	0.161***	0.138***	0.140***	0.146***
	(0.022)	(0.010)	(0.008)	(0.016)	(0.011)	(0.008)
Two-time treated (T2)	0.159***	0.146***	0.180***	0.153***	0.156***	0.174***
	(0.015)	(0.007)	(0.009)	(0.012)	(0.006)	(0.007)
Observations	8,830	8,830	8,830	8,830	8,830	8,830
Panel B:		TC1			TC 2	
One-time untreated (U1)	0.222***	0.147***	0.079***	0.146***	0.152***	0.167***
	(0.019)	(0.025)	(0.025)	(0.019)	(0.016)	(0.020)
One-time treated (T1)	0.200***	0.174***	0.190***	0.156***	0.188***	0.240***
	(0.021)	(0.022)	(0.024)	(0.017)	(0.014)	(0.020)
Two-time untreated (U2)	0.175***	0.165***	0.097***	0.170***	0.146***	0.159***
	(0.020)	(0.027)	(0.026)	(0.011)	(0.021)	(0.024)
Two-time treated (T2)	0.158***	0.127***	0.085***	0.155***	0.175***	0.203***
	(0.019)	(0.026)	(0.024)	(0.013)	(0.014)	(0.019)
Observations	8,821	8,821	8,821	8,821	8,821	8,821
Panel C:		Profit 1			Profit 2	
One-time untreated (U1)	0.084*	0.086	0.215***	-0.075	-0.011	0.276***
	(0.045)	(0.054)	(0.038)	(0.103)	(0.063)	(0.063)
One-time treated (T1)	-0.033	0.047	0.232***	-0.243***	-0.048	0.224***
	(0.034)	(0.031)	(0.026)	(0.068)	(0.060)	(0.059)
Two-time untreated (U2)	0.261***	0.196***	0.273***	0.070	0.131	0.382***
	(0.060)	(0.037)	(0.038)	(0.121)	(0.083)	(0.079)
Two-time treated (T2)	0.125**	0.219***	0.363***	-0.129*	0.165***	0.446***
	(0.049)	(0.027)	(0.023)	(0.067)	(0.046)	(0.030)
Observations	8,821	8,821	8,821	8,821	8,821	8,821

Note: Labor cost 1, total cost 1 (TC1) and profit 1 do not include the cost of family labor. Family labor cost is included in the variables labor cost 2, total cost 2 and profit 2 using market wage. Yield, cost and revenue are expressed in logarithms. As some of the profits were negative we ran normal regressions for the profit variables and present the estimated coefficients divided by the mean of the control group. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.

Table A15: Plot level difference-in-differences for different SRI principles (with continuous treatment intensity)

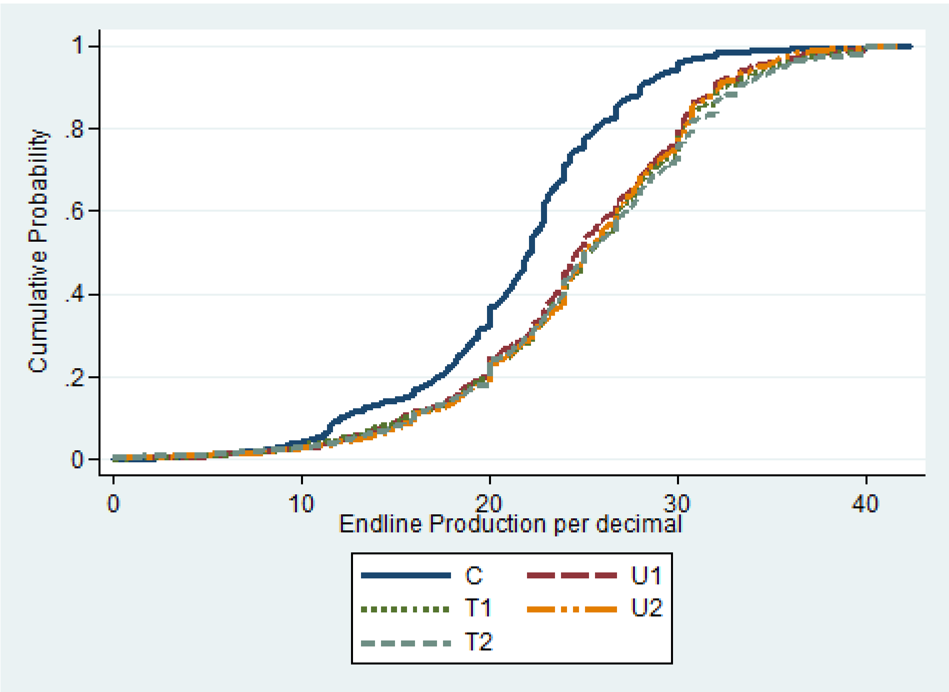
	(1)	(2)	(3)	(4)	(5)	(6)
	Age of seedlings	No of seedlings	Distance between seedlings	Alternate drying & wetting	Use of organic fertilizer	Machine based weeding
Treatment Status						
U11F	-0.576	4.989	3.103	16.241	2.852	-2.371
	(0.666)	(7.218)	(1.894)	(12.282)	(4.964)	(2.363)
U12F	4.970**	4.255	9.521***	5.538	15.491*	16.119***
	(2.066)	(6.992)	(2.970)	(13.618)	(8.126)	(5.723)
U21F	-0.005	11.347	8.118***	16.036	11.398*	-3.657**
	(0.840)	(7.363)	(2.822)	(11.891)	(5.785)	(1.676)
U22F	2.587	15.178*	17.028***	28.150**	15.417**	6.470
	(1.720)	(8.245)	(4.204)	(12.584)	(7.077)	(4.400)
T11F	5.777***	27.485***	27.267***	30.777***	19.816***	2.167
	(1.496)	(7.038)	(3.683)	(10.664)	(6.590)	(3.340)
T12F	8.732***	37.829***	42.819***	15.292	21.296***	19.002***
	(2.539)	(8.945)	(6.103)	(11.994)	(7.754)	(7.166)
T21F	3.337***	25.768***	22.917***	23.655**	20.258***	-0.284
	(0.848)	(6.087)	(2.768)	(9.740)	(5.917)	(1.851)
T22F	10.873***	41.117***	47.861***	32.595***	29.273***	13.032**
	(2.728)	(7.644)	(5.480)	(10.109)	(6.515)	(5.078)
Observations	33,244	33,244	33,244	33,244	33,244	33,244
R^2	0.027	0.158	0.135	0.502	0.117	0.061
Control mean	0.33	7.24	0.29	31.31	5.23	1.19
p-value (U11F-T11F)	0.00	0.00	0.00	0.02	0.00	0.01
p-value (U12F-T12F)	0.10	0.00	0.00	0.19	0.31	0.60
p-value (T11F-T12F)	0.25	0.03	0.00	0.05	0.82	0.00
p-value (U11F-U12F)	0.01	0.87	0.00	0.35	0.07	0.00
p-value (U21F-T21F)	0.00	0.00	0.00	0.26	0.05	0.00
p-value (U22F-T22F)	0.00	0.00	0.00	0.50	0.00	0.04
p-value (T21F-T22F)	0.01	0.00	0.00	0.15	0.17	0.00
p-value (U21F-U22F)	0.07	0.35	0.01	0.22	0.56	0.02

Notes: Each of these outcome variables are dummy variables (multiplied by 100) indicating if a farmer followed that principle as recommended by BRAC for SRI. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.



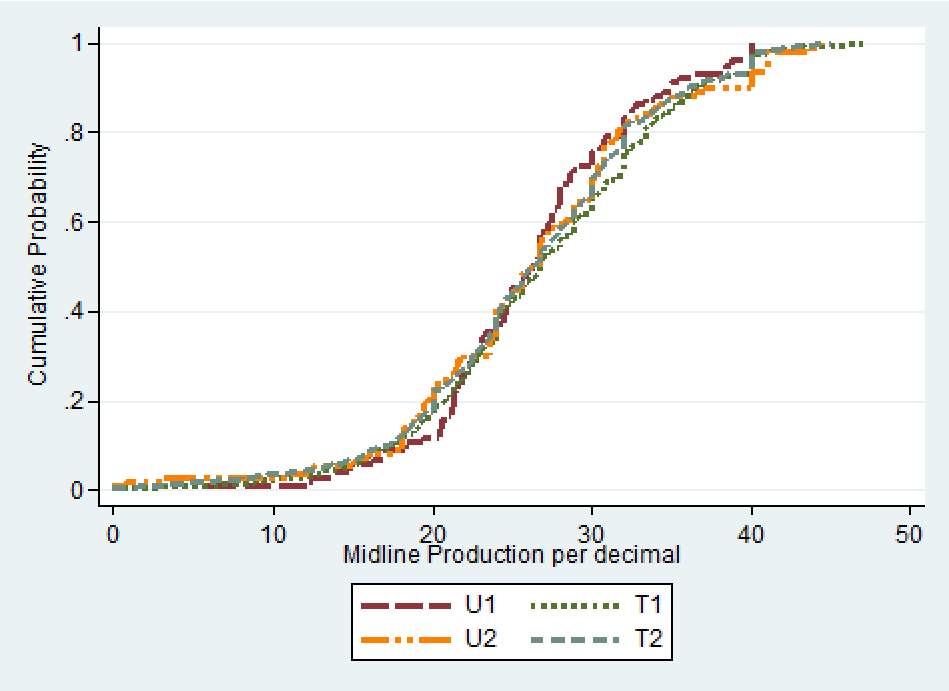
Note: Each of the four treatment groups (U1, U2, T1 and T2) first order stochastically dominates the control group (C) based on Somers' D statistic. But none of the treatment groups first, second, or third order stochastically dominates any other treatment group.

Figure A1: Cumulative distribution function of midline production per decimal of land



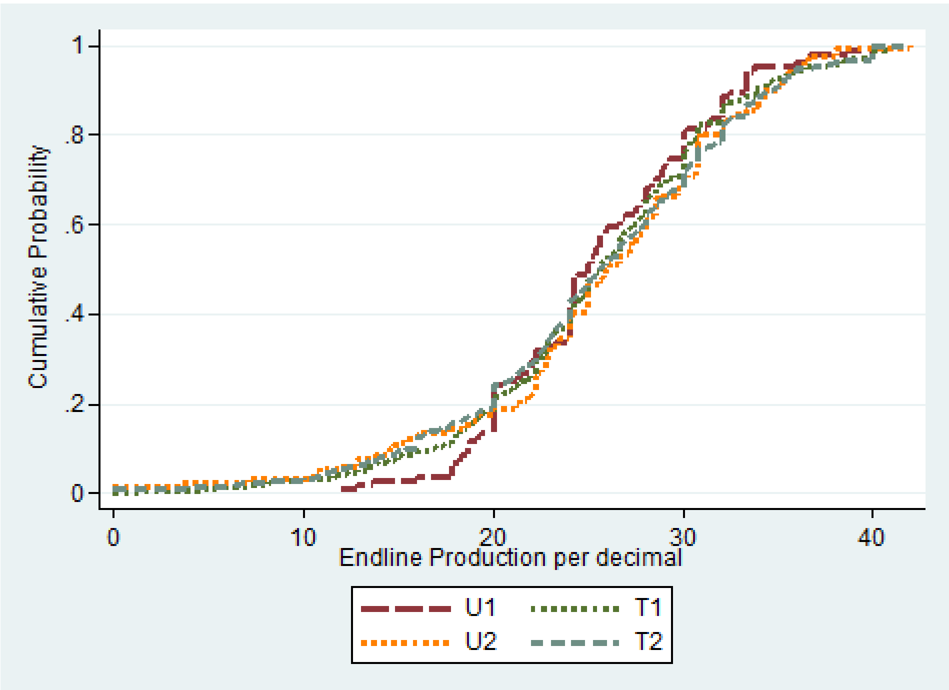
Note: Each of the four treatment groups (U1,U2,T1 and T2) first order stochastically dominates the control group (C) based on Somers' D statistic. But none of the treatment groups first, second, or third order stochastically dominates any other treatment group.

Figure A2: Cumulative distribution function of endline production per decimal of land



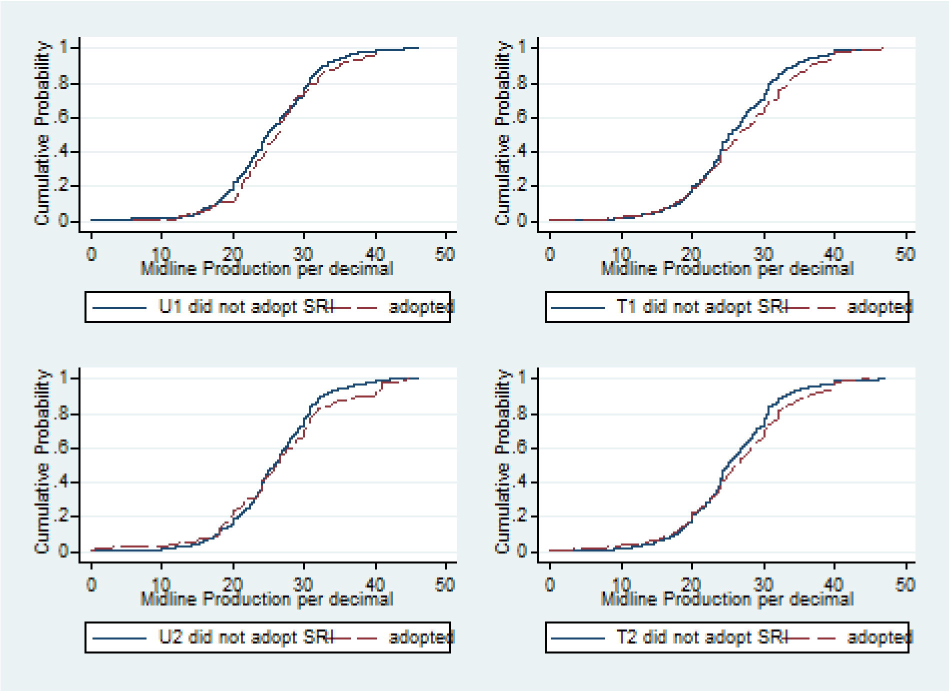
Note: None of the treatment groups first, second, or third order stochastically dominates any other treatment group.

Figure A3: Cumulative distribution function of midline production per decimal of land for SRI adopters



Note: None of the treatment groups first, second, or third order stochastically dominates any other treatment group.

Figure A4: Cumulative distribution function of endline production per decimal of land for non-adopters



Notes: p-value associated with Somers' D test for first order stochastic dominance for U1(adopted-did not adopt) =0.39, T1(adopted-did not adopt) =0.10, U2 (adopted-did not adopt) =0.67 and T2(adopted-did not adopt) =0.17. No second or third order stochastic dominance exists either.

Figure A5: Cumulative distribution function of midline production per decimal of land by treatment and adoption status

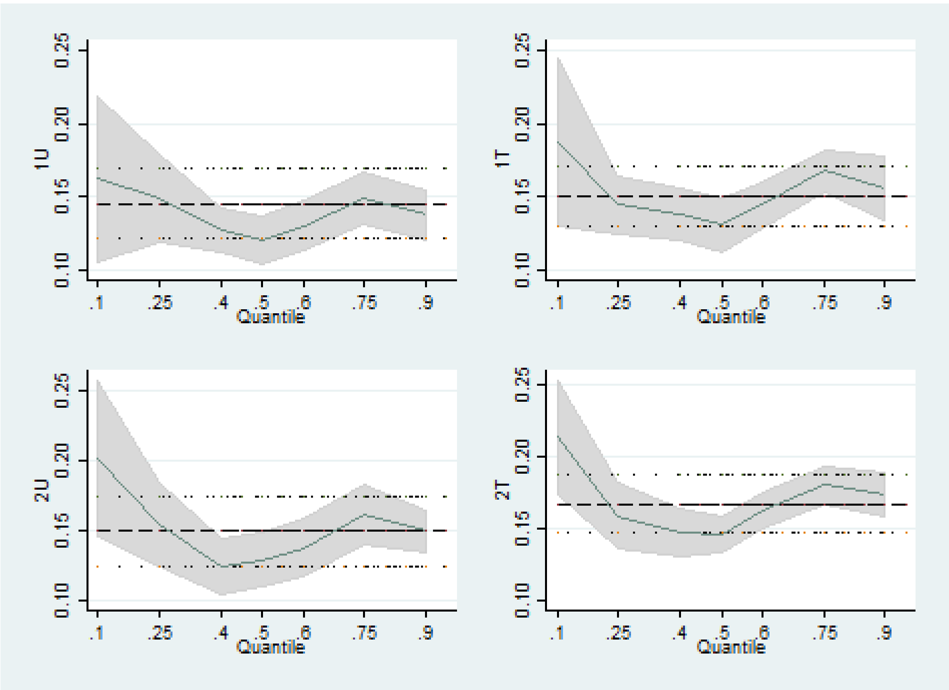
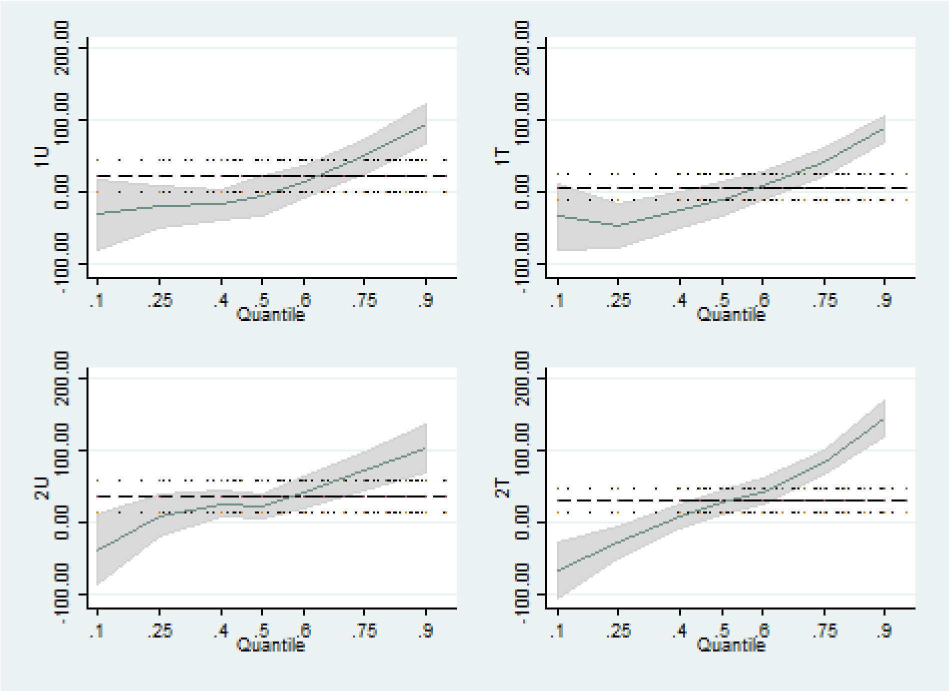


Figure A6: Quantile regression for baseline yield



Note: The endline profits using profit2 which consider the imputed cost of family labour.

Figure A7: Quantile regression based on baseline profits (adjusted for family labor)

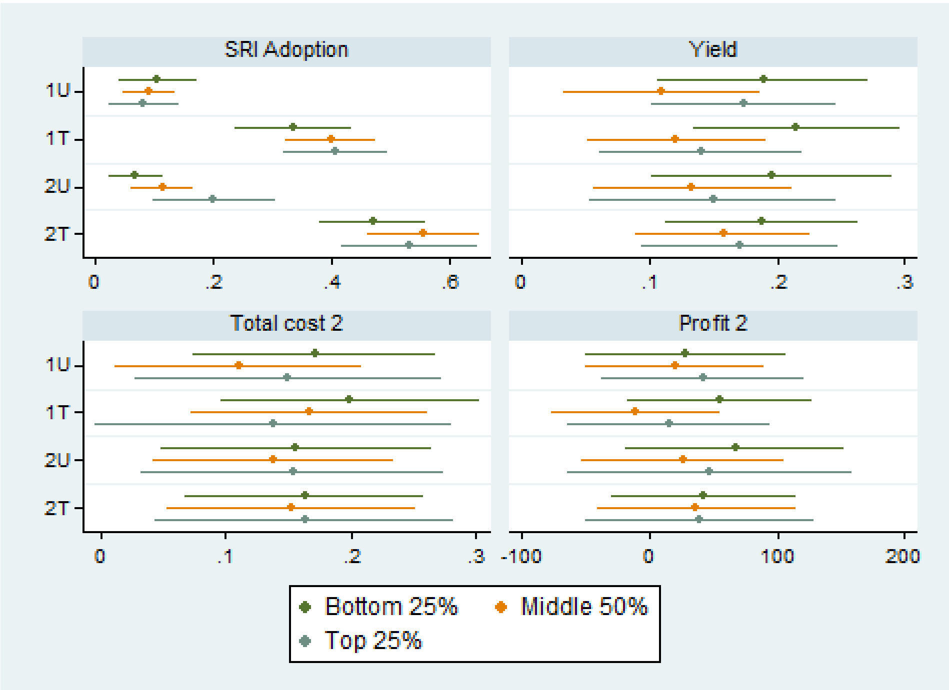


Figure A8: Quartile regressions based on baseline land size

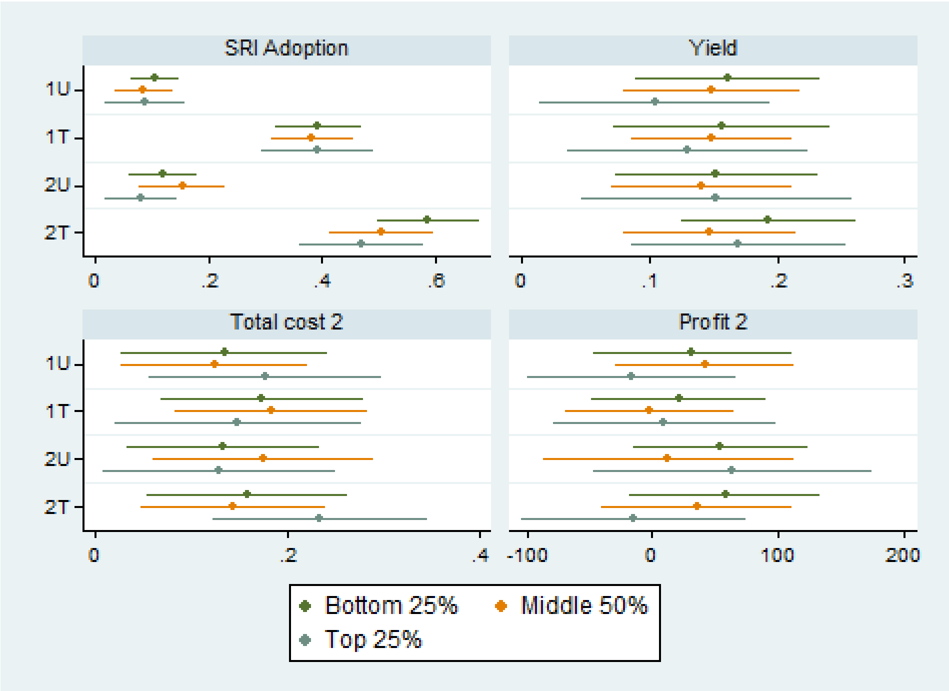


Figure A9: Quartile regressions based on adult working members