Better Technology, Better Plots or Better Farmers? Identifying Changes In Productivity And Risk Among Malagasy Rice Farmers

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Abstract

It is often difficult to determine the extent to which observed output gains are due to a new technology itself, rather than to the skill of the farmer or the quality of the plot on which the new technology is tried. We introduce a method for properly attributing observed productivity and risk changes among new production methods, farmers and plots by controlling for farmer and plot heterogeneity using differential production and yield functions. Results from Madagascar show that the new system of rice intensification (SRI) is indeed a superior technology. Although about half of the observed productivity gains appear due to farmer characteristics rather than SRI itself, the technology generates estimated average output gains of more than 84 percent. The increased estimated yield risk associated with SRI would nonetheless make it unattractive to many farmers within the standard range of relative risk aversion.

1. Introduction

Economic growth theory holds that technological change is the primary driver of long-run economic growth and improvements in human nutrition and well-being. However, the mere development of a technology is insufficient because its adoption by producers may be slow, partial, reversible or absent all together. Cross-sectional variation in the extent and rate of new technology adoption is often attributable to characteristics of the farmer or the farm, especially to differences in education, access to extension or financial services, risk preferences, plot biophysical conditions and farm size (Sunding and Zilberman 2001). But because farmers' adoption choices are typically limited by the availability of reliable information, finance, labor or other complementary inputs – perhaps especially among poor smallholders in the tropics – observed adoption patterns need not be optimal. Farmers' failure to take up high return technologies motivates project and policy interventions to stimulate technology diffusion.

But it can be tricky to establish whether a new technology indeed offers high returns and merits interventions to promote diffusion. Production economists working on agriculture have relied heavily on researcher-directed experimental trials – both on station and on farm – in order to establish how different technologies change expected yields, yield risk, and labor productivity under alternative treatment designs. This method has worked extremely well in developing improved seed, fertilizer and machinery, the staples of historically unprecedented agricultural output growth over most of the 20th century (Evenson and Gollin 2002). But ex post impact assessment to establish the true gains attributable to a particular technology under prevailing farm conditions and farmer management remains a methodological challenge. On farm results without researcher direction commonly differ from on-station results of researcher-managed trials.[1] Moreover, the observable and unobservable characteristics of farmers and their farms influence technology adoption decisions, input application choices and observed output. Standard cross-sectional analysis of a mixed sample of adopters and non-adopters will therefore tend to suffer a selection problem associated with farmers' choice to use (or not use) the new technology, yielding inconsistent estimates of the impact of the new technology.

This pervasive problem is perhaps most pronounced with respect to new technologies not at all embodied in physical inputs such as seed, fertilizer or machinery, but that solely reflect improved farmer cultivation and natural resource management practices. Because all changes then result from farmer choice and not from changes in the genetic traits of the plant, the biochemical attributes of nutrient amendments or the mechanical function of equipment, one might reasonably suspect ex post impact assessment to be vulnerable to selection bias problems. Farmer and farm heterogeneity leads to selection bias since more skilled farmers are commonly the first to adopt improved technologies and often apply them on their best plots. This paper offers a method to resolve this methodological challenge, to disentangle the output effects – in both mean and variance – that are rightfully attributable to a new technology from those associated with farmer- and plot-specific characteristics.

[1] For example, Goletti et al. (1998) found that yields on Malagasy farmers' fields from new rice seed varieties were only one-quarter those observed in experiment station trials.

We demonstrate our method using data from farmers in Madagascar who experiment with the System of Rice Intensification (SRI), a promising method of rice cultivation developed in the 1980s that relies exclusively on changing farmer agronomic practices. SRI involves no new seed or purchased inputs. Using contemporaneous observations taken on plots cultivated by the same farmers but under both the new and old technologies, we demonstrate how one can isolate the true productivity and risk effects of a new technology in cross-sectional data by controlling for farmer- and plot-specific effects using differential production and yield functions.

The remainder of the paper is organized as follows. After a brief explanation of SRI and its adoption patterns in Madagascar in section 2, we describe the production data and present descriptive statistics in section 3. Section 4 discusses methodological issues and introduces our estimation strategy. We present our estimation results in sections 5 and 6, the former focusing on changes in mean output and yield, the latter on production and yield risk effects. We end with some concluding remarks in section 7.

2. Motivation: The Promise and Puzzle of SRI

The System of Rice Intensification (SRI) has been researched, studied and debated since its development by a French missionary priest and agronomist, Fr. Henri de Laulanié, S.J., in rural Madagascar in the 1980s. Following his accidental discovery of substantial rice yield gains from early transplanting during the 1983 drought, Fr. de Laulanié gradually developed a set of principles based on the synergy among several techniques: seeding on a dry bed, transplanting plants younger than 15 days old with one plant per hill, spacing of at least 20 x 20 cm in a square grid, frequent weeding, and controlling the water level to allow for the aeration of the roots during the growth period of the plant (i.e., no standing water on the rice field). All of these components differ from traditional rice cultivation practices in Madagascar and elsewhere (Stoop et al. 2002, Uphoff et al. 2002). The knowledge-intensity of SRI is underscored by the name of the indigenous nongovernmental organization (NGO) formed by Fr. de Laulanié to promote the method among rural Malagasy: Association Tefy Saina, the latter two words meaning "to improve the mind" in Malagasy.[2]

Some agricultural scientists question the science underpinning SRI and the sustainability of its yields (e.g., Dobermann 2003). Among other things, the SRI philosophy that the aforementioned water management practices provide the best conditions for plant growth and yield is unorthodox and controversial from the perspective of the international rice research community. Both national agricultural research systems and the relevant international agricultural research centers have been relatively skeptical of SRI to date and slow to study it intensively. As a consequence, the conventional sequence of on-station development and trials, followed by researcher-managed, on-farm trials, and then carefully monitored farmer-managed, on-farm trials has not taken place with SRI. The technique was developed mainly through participatory, on-farm research by practitioners, with research scientists joining the process relatively late, although there have now been careful multifactorial, multilocal trials (Randriamiharisoa 2002). Basic questions about SRI's true productivity and yield risk effects thus remain largely unanswered.

Nonetheless, limited research center and on-farm trials from several countries in Africa and Asia have shown that yields can consistently be doubled (or more) with few or no externally purchased inputs such as seed or chemical fertilizer (Uphoff et al. 2002). The remarkable observed increases in yields associated with SRI adoption have led many to believe that this method could dramatically improve the lives of the many poor small farmers in Madagascar and other low-income rice-producing nations who lack the liquidity to purchase modern inputs and for whom rice is both a staple and an important income source. In 2001, the *Financial Times* of London described SRI as a new "agricultural revolution" (Madeley 2001) and SRI has recently begun to be taken up seriously in other rice-producing nations – including Bangladesh, Cambodia, China, Indonesia, the Philippines, Sierra Leone and Sri Lanka – with many positive preliminary productivity results (Uphoff et al. 2002).

[2] For more detailed information on Fr. de Laulanié and Tefy Saina, see http://www.tefysaina.org/.

While some Malagasy farmers have adopted and continued to practice SRI, much of the research community observing SRI have been puzzled by three facts that call into question the oft-asserted superiority of the method. First, adoption rates have been low. Second, rates of disadoption (abandonment) have been high in Madagascar (Moser and Barrett 2003a). Third, most farmers who practice SRI continue to practice traditional methods (henceforth referred to as SRT) on some of their land, even after several years of experience with the new method. For example, Moser and Barrett (2003a) found that even in areas served by extension agents devoted exclusively to SRI, only about 15 percent of rice farmers practiced the method five years after its introduction and 40 percent of farmers who tried the method had disadopted. The spread of SRI outside the areas where it was promoted is even lower. A 2001 nationwide census of more than 1300 communes, the smallest administrative unit in the country, found that 62 percent show no use of SRI, and less than 3 percent of the nation's communes report SRI use by at least one quarter of the jurisdiction's farmers (Moser and Barrett 2003b). Given SRI's origins in rural Madagascar and its oft-demonstrated productivity benefits, its slow and limited uptake by small farmers there has puzzled many observers and raises questions about whether this new rice production method really offers all the total factor productivity gains its advocates claim.

One mooted cause of low uptake is that SRI is more labor intensive, at least initially, than traditional methods and requires better water drainage and management. Previous studies of SRI adoption show that poorer farmers with little land are much less able and likely to adopt SRI than richer farmers with more land (Moser and Barrett 2003a,c). Poorer farmers lacking access to interseasonal credit appear unable to afford to reallocate labor away from wage employment that provides cash to meet immediate household consumption requirements during the hungry season (*soudure*), even if this reallocation would generate handsome yield increases several months in the future. By that hypothesis, a liquidity shortage (that prevents hiring labor or reallocating family labor away from off-farm employment for cash wages) creates a family labor shortage that precludes investment in labor-intensive SRI in spite of the promise of big productivity gains. In other words, low, incomplete and reversible adoption patterns can be reconciled with claims of large productivity gains.

However, previous adoption studies lacked detailed farm production data to verify either the productivity gains from SRI or that labor requirements indeed increased under SRI, especially early in the season. Moreover, they had less success explaining disadoption and the extent of adoption. By introducing a new method that allows us to establish the true productivity and risk effects of SRI using more detailed production data on adopters, we can shed more light on these three puzzles about SRI adoption in Madagascar.

3. Data

The data come from a study of 111 randomly sampled farmers contemporaneously practicing both SRI and SRT[3] using the same rice variety in four sites in Madagascar, Ambatondrazaka and Imerimandroso in the eastern province of Toamasina, and Antsirabe and Fianarantsoa in the central highlands provinces of Antananarivo and Fianarantsoa, respectively (Joelibarison 2001, McHugh et al. 2002). These are two of the nation's three main rice producing areas and the areas where SRI cultivation has become most widespread. The sample farmers were interviewed in Malagasy by agricultural extension agents and university agronomy students using a structured survey questionnaire about cultivation details, irrigation practices, household characteristics, etc. over three visits during the February-June 2001 growing season. At the season's end, grain yield was measured directly from 2x2 meter quadrats and calculated for paddy rice at 14% moisture content.

Malagasy farmers commonly cultivate many small plots. In our sample, the median numbers of SRI and SRT plots per farmer were four and six, respectively, with mean sizes of only seven and ten ares[4], respectively. In order to keep data collection simple, our survey gathered detailed production information on only one randomly selected plot

[3] The overwhelming majority of SRI farmers in the survey villages practice SRT as well. In a different data set on SRI adoption patterns, Moser and Barrett (2003c) found that only nine percent of SRI adopters ever put all their rice land into SRI in any single year.
[4] 1 hectare = 100 ares

of each type, SRI and SRT, for each sample farmer.[5] The plots were selected after preliminary interviews to classify each plot according to time of seedling transplanting (seedlings transplanted when less than 12 days or more than 20 days old for SRI and SRT, respectively), density of transplanting density (one seedling per hill versus three or more per hill for SRI and SRT, respectively), and plant spacing (square grid pattern for SRI versus random spacing for SRT). Because farmers' cultivate using both methods simultaneously, we can control for farmer- and plot-specific effects that typically bias cross-sectional productivity studies. Section 4 introduces our method for implementing such controls. The remainder of this section reports on key results from the sample descriptive statistics.

Farm and farmer characteristics

Table 1 describes key farm and farmer characteristics from the sample. These data are consistent with previous studies of SRI adopters using older and less detailed data (Moser and Barrett 2003a,c), which likewise found that adopters tend to be relatively well educated, to be more involved in farmer organizations and to own more rice land compared to non-adopters. About 34 percent of the SRI farmers are net rice sellers, roughly equal to the national average (Minten and Zeller 2000). The survey villages were purposively chosen based on their relative success with SRI. The resulting program placement effects may bias upwards the estimated productivity gains from SRI relative to farmers elsewhere in Madagascar, where water control, soil and market access conditions may not be as favorable. For example, national average rice land holdings are about 108 ares, as compared to nearly 133 ares in our sample. This further underscores the importance of controlling properly for farmer and farm characteristics. Unconditional cross-sectional comparisons of SRI yields in favored areas with national average yields will tend to overstate the gains to the technology.

Cultivation practices differ significantly between SRI and SRT fields – recall that the plot classification relied on the timing, spacing and density of transplanting – although other

[5] The sampled plots represent, on average, 52 and 27 percent of sample farmers' total SRI and SRT land, respectively.

inputs are not very different in aggregate. Because of the water management requirements of the system, none of the SRI plots rely solely on rain run-off for water, while 7 percent of SRT plots are exclusively rainfed. Chemical fertilizer is not widespread on either type of field, and mechanization (i.e., use of a tractor) is rare. Manure application rates are similar across SRT and SRI fields, as are the number of days of water shortage, the use of plots for growing off-season crops and the percent of fields on soils the farmers described as very rich (McHugh 2002).[6]

While in the abstract there may be valid concerns about plot-level selection effects biasing upwards estimated productivity gains due to a new technology, in this sample, SRI and SRT plots appear very similar. This is born out by soil tests on a subsample of the survey plots that showed very similar soil fertility between SRI and SRT plots. SRT plots had slightly higher phosphorus and carbon content than SRI plots and almost identical nitrogren, potassium and soil organic matter characteristics (Barison 2002). The main within-farm difference is therefore cultivation methods, not underlying soil fertility, although we will take care to control for what differences do seem to exist.

Unconditional land and labor productivity effects

Consistent with earlier studies of SRI (Rajonarison 1999, Rakotomalala 1997, Randriamiharisoa 2002), our data show that farmers' yields under SRI are nearly double on average their SRT yields and that labor productivity gains likewise appear high, on average (Table 2). However, SRT yields are also very high among these SRI adopters, with the mean SRT yield of 3.37 tons/hectare, more than 75 percent higher than the national average (which includes improved methods and varieties).

Both yields and labor productivity are considerably more variable under SRI than under SRT, both in terms of the standard deviation and the coefficient of variation. The increased yield risk appears attributable to SRI's transplanting and water management methods. First, farmers draining fields risk over-drying their fields if water supplies are

[6] African farmers' subjective reporting of soil conditions has been validated for scientific accuracy. See the February 2003 special issue of *Geoderma* (111, 3-4) on local soils knowledge for recent evidence.

unreliable due to rainfall, reservoir shortages, disputes among irrigation users over water sharing, or farmers' lack of time to check fields daily. Although irrigation is relatively widespread among rice farmers in the survey areas of Madagascar, the quality of this irrigation varies greatly. Draining fields increase weed growth, so farmers who lack the necessary time or equipment to weed effectively suffer large losses due to weed competition. Drained plots also improve access for rodents and ground insects that attack rice plants.[7] Transplanting very young, small plants also makes them more susceptible to being washed away. Farmers are advised to replace transplanted seedlings that are washed away or damaged by pests, but many either do not have time or just do not make the effort to replace these plants. Because of lower planting density, failure to replace a damaged or missing plant more significantly reduces yields at harvest. Finally, SRI might increase yield risk by increasing the time plants are in the field and thus vulnerable to a wide range of biotic and abiotic stresses. Longer field residence arises both due to earlier transplanting from small, well-protected nursery beds to larger fields and because harvest gets somewhat delayed as more numerous and fuller panicles grow a bit longer.

Reflecting the variability in output gains, labor productivity more than doubles for nearly one-third of SRI farmers, relative to SRT, yet labor productivity also falls for more than one-third of farmers in the sample. The fraction of farmers whose labor productivity fell with SRI is close to the proportion of farmers in another sample who disadopted (Moser and Barrett 2003a), hinting at a plausible explanation for observed abandonment of a technology that seems to increase rice yields on virtually all plots. Indeed, in our informal conversations with several farmers who disadopted SRI, each cited reduced current returns to labor as a factor in their disadoption decision.

In spite of increased variability in labor productivity and yields per unit cultivated area, SRI first order stochastically dominates SRT in terms of both yield per unit area cultivated and labor productivity. SRT appears to hit a yield ceiling at about 5 tons/hectare and exhibits negative skewness while SRI easily doubles that yield ceiling

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[7] In one of the co-authors' experimental trials in Beforona, in Toamasina province, about 42 % of seedling were lost on drained plots due to insects versus about 8% on flooded plots (McHugh 2002).

and is positively skewed. The unconditional yield distribution data thus suggest that SRI is an unambiguously superior technology.

However, stochastic dominance analysis assumes all farmers draw randomly from identical distributions, that observed differences in outcomes result only from the technology selected and from chance. We suspect, however, that productivity differences are not, in fact, identically distributed across farmers and plots and that, as a consequence, stochastic dominance comparison of SRI and SRT yields may be somewhat misleading as to the true productivity gains farmers might reasonably expect to enjoy from changing cultivation practices.

One way to test this hypothesis is to simulate SRI-SRT yield differences under the null hypothesis that differences in output realizations result purely from the choice of cultivation method and chance and then to compare the resulting simulated yield differences series with the observed yield differences series. We simulate yield differences under the null hypothesis that farmer and plot-specific effects do not matter by randomly drawing (bootstrapping) a large number (n=1248) of observations from the observed, unconditional SRI and SRT yield distributions, pairing the series into pseudo-farms so as to estimate the simulated within-farm yield difference by subtracting the random SRT yield draw from its paired random SRI yield realization. We can then compare the bootstrapped distribution of random productivity differences against the observed distribution of actual productivity differences between SRI and SRT plots for farmers in our sample.

As shown in Figure 1, observed, farm-specific SRI-SRT yield differences plainly result from more than merely random shocks. The bootstrapped pseudo-yield difference distributions first order stochastically dominate the observed yield difference distributions. Both are almost entirely positive – less than three (one) percent of actual (simulated) yield differences were negative – reflecting productivity gains associated with SRI. Nonetheless, actual on-farm yield gains are consistently and considerably less than would be the case were choice of cultivation method the only systematic source of productivity differences. For example, the median observed unconditional yield gain was about 2.5 tons/hectare, versus nearly 6.5 tons/hectare if the choice of method was the only deterministic difference in resulting output. Hence the importance of multivariate control for factors that might otherwise confound identification of the true effects of the SRI technology on stochastic output distributions. In the next section, we introduce a method for estimating productivity differences with such controls.

Labor demands and experience with the new technology

While there is some dispute over the labor demands associated with SRI, most observers seem to agree that SRI increases labor demands in field preparation (especially leveling for water control), transplanting, weeding and daily water management. This could be an important determinant of farmers' perceptions of the likely benefits from trying (adopting) SRI or from continuing to practice SRI, if they have already experimented with the method. Those with a high marginal opportunity cost of time – due to cash constraints on the poorer end of the income distribution or due to relatively high wages or salaried employment on the richer end of the income distribution – might find SRI unattractive if it demands more labor without sharply higher labor productivity. Within the community of SRI practitioners and researchers, one also hears many anecdotal claims that SRI's labor demands diminish rapidly with experience in using the technology.

These data indicate that most farmers with three or fewer years' experience with SRI indeed employ early season labor more intensively per unit area cultivated in SRI than in SRT. [8] Figure 2 depicts the median and span of the central half of the distribution (i.e., between the 25th and 75th percentiles) of farmers' observed early season labor use in SRI relative to SRT for different levels of experience with the new rice cultivation methods. The median farmer in his first three years with SRI uses 31.4 to 37.7 percent more labor per hectare in SRI. By the fourth year with SRI, the median farmer uses 4.2 percent less early season labor, improving to 10.9 percent less early season labor for those with five

[8] We define "early season" labor as including all labor for field preparation (e.g., leveling, plowing, irrigation, puddling), nursery preparation, transplanting, fertilizer application and weeding. It does not include labor devoted to guarding against birds or rats or to harvest.

or more years' experience with SRI. There does appear to be mild support for the hypothesis that labor demands decrease with experience in using SRI.

Nonetheless, at all experience levels, a large share of farmers experienced increased labor demands per area cultivated under SRI, with 30 percent or more at least doubling their labor application rate. This is consistent with previous findings that poorer, credit-constrained farmers choose not to adopt SRI due to increased early season labor demands that conflicts with their need to work off-farm for cash wages in order to buy food to meet immediate family subsistence needs and with findings that farmers with skilled or salaried off-farm employment are more likely to disadopt SRI after trying it (Moser and Barrett 2003a).

Some have suggested that experience should likewise improve productivity through learning by doing effects, that negative or low productivity gains may simply reflect a farmer's lack of experience with the technique. Because SRI requires significant changes in several different tasks throughout the growing season, Malagasy farmers often report that mastering the technique takes several years. Thus one might expect productivity differences to be low or even negative immediately after adoption, but to increase with farmer experience. Figure 3 and Table 3 show, however, that the proportion of farmers experiencing labor productivity losses with SRI cultivation does not fall significantly with experience, although median labor productivity does increase steadily. Of course, the rise in median productivity may well be attributable simply to attrition bias in these data, since those with extremely poor productivity under SRI would have been more likely to have disadopted SRI by the time of the survey, implying that those 2001 SRI farmers with significant past SRI experience are likely an upwardly biased sample of the farmers who first experimented with SRI several years earlier. In any case, learning by doing effects appear modest-to-negligible in these data.[9]

4. The Methodological Challenge

[9] Moser and Barrett (2003c), using a different data set, similarly found that learning by doing effects were not significant in explaining area planted in SRI once one controls for household fixed effects.

Establishing the superiority of one technology over another is difficult using observational data because of observed and unobserved farmer and plot attributes that are likely correlated with both farm productivity and the use of other inputs. Farmers who are especially productive with the new technology are likely relatively productive with the old technology as well because they have high levels of educational attainment, superior information networks or unobserved skills that positively affect productivity. Thus the failure to control for farm and farmer heterogeneity can lead to an overestimation of the returns to adoption of a new technology.

The gains to a new technology may not only be overstated by failing to control for farmer differences when making comparisons across farmers, but also by failing to control for plot selection. Because SRI fields need good water control and drainage, farmers will practice SRI on the plots with these characteristics—characteristics that would produce higher rice yields under virtually any method. In theory, if other inputs, such as compost (which is often recommended by SRI promoters) and mechanical weeders (which cannot be used on SRT fields if the farmer does not plant in rows), are used at a greater rate on SRI fields, then the yield gains may overstate the true total factor productivity gains due to SRI by capturing in part the effect of better complementary inputs.

There are several possible solutions to the potential problem of farmer and plot selection bias. One would be to use a standard Heckman model to control for the observable factors that lead to adoption so as to isolate the productivity gains. This can be an unsatisfactory solution, however, for several reasons. First, applied econometricians often have difficulty finding separate identifying instruments for the first-stage, selection equation and getting good fit in that equation. As a consequence, controlling for the discrete choice to use the new technology is commonly highly imprecise and the instrumented variable is then often correlated with other regressors in the second-stage regression. Second, the selection model method can only control for observed farm and farmer characteristics, although it seems highly likely that commonly unobserved characteristics (e.g., aptitude, motivation, information access, timing of abiotic stresses due to temperature and water, etc.) play a significant role in both agricultural technology adoption and in observed yields. Third, a Heckman-type sample selection model inherently discretizes a talent continuum into a binary adopt/disadopt variable and thereby cannot control for variation due to farmer- or farm-specific characteristics within the subpopulation of adopters. As a consequence such effects will typically be misattributed to the new technology, leaving a certain degree of "green thumb" bias in estimates of the productivity differences between the technologies. For example, in our sample, the raw correlation coefficient on SRI and SRT yields within the same farm equals 0.247, underscoring that there is significant correlation in productivity across technologies due to farm- and farmer-specific effects. A selection model could not control for these effects adequately.

There's a related problem if learning-by-doing effects are present. As discussed in the previous section, there does not seem to be much learning-by-doing taking place among the Malagasy SRI farmers we study. But so long as productivity in using a method might be increasing with experience, then comparing newly adopted technologies with long-established ones, lumping all adopters together irrespective of experience with the technology, may fail to account for a farmer's learning about the new technology. This will tend to bias downward estimates of the productivity differences between the two technologies.

Cross-sectional studies typically cannot control for these problems because they either do not observe both technologies in use by the same farmers at the same time – thereby permitting control for unobserved farmer attributes (the "green thumb" effect) –they do not observe how long the farmer has been using the new technology, or both.[10] The preceding two concerns are underscored by regressing crop yield under SRI on crop yield under SRT and years of experience with SRI. As reported in Table 4, once one controls for experience with SRI, a farmer's SRI yields increase essentially one-for-one with his SRT yields. This is true both in the sparsest specification (the left column of Table 4) as

[10] With the added expense and wait involved in collecting panel data – repeated observations over time on the same farmers and plots – one can make reflexive comparisons of productivity before and after adoption of the new technology, provided one has adequate controls for other intertemporal changes that may be associated with productivity differences. We focus here on methods appropriate in cross-sectional data.

well as with proper controls for labor and manure application rates, plot size, soil quality and water availability.[11] This simplistic exercise demonstrates that SRT and SRI yields are simultaneously determined by farm and farmer attributes not observed in this sample or in most data sets. This underscores the necessity of trying to separate unobservable farmer and farm-specific effects from those of the technology itself. The remainder of this section introduces a method for doing precisely that.

Differential Production and Yield Function Estimation

We draw on a growing panel data econometrics literature that controls for unobservable time invariant characteristics of observational units by first-differencing sample data.[12] In the case of plots simultaneously planted under two different methods, we have a matched pairs sample within which one might reasonably expect observable farm and farmer characteristics to be correlated with unobserved characteristics. In such a case, differencing across plots so as to remove the unobserved heterogeneity corrects for the bias and inconsistency otherwise introduced in regressing output or yields on production inputs and observable plot and farmer characteristics. This method has become commonplace in panel data analysis and in program evaluation. The approach we introduce differs from the standard difference-in-differences estimator in that we want to identify not only the new technology's unconditional effects on observed productivity, but also its effects on the marginal productivity of different inputs, especially land and labor, and how these effects might vary with experience in using the new technology. Moreover, we want to generate some estimates using just cross-sectional data.[13] We call this technique differential production or yield function estimation, depending on whether

[11] These results are qualitatively unchanged when one adds further regressors to control for farmer experience, education, gender, household composition, location and other factors that could partly explain both SRI and SRT yields. No matter the covariates included or the specification employed, SRT yields always appear positively and statistically significantly related to SRI yields, underscoring the basic point that unobserved heterogeneity among farmers significantly affects observed yields.

[12] See Wooldridge (2002), chapters 10 and 11, for an excellent discussion of the background econometric literature. See also Chapter 18 on the related literature on the estimation of average treatment effects (ATE). The ATE literature struggles with the inability to observe both the control and treatment (SRT and SRI cultivation, respectively, in our case) for the same individual. Since our sample was structured so as to observe both states simultaneously, our method is simpler than current ATE estimators based on propensity scores and various matching estimators.

[13] Conventional difference-in-differences (or "double difference") methods compare the control and treatment groups both before and after the treatment. This requires panel data.

one uses absolute output or output per unit area cultivated (i.e., yield) as the dependent variable.

As implied by the name, our approach relies on a contestable decision to estimate production or yield functions, rather than cost or profit functions. In estimating production functions, the types and quantities of inputs chosen are arguably endogenous, especially as the season progresses and farmers adjust input levels based on weather, pest conditions, etc. Yet only the dual (cost or profit) estimation approach explicitly endogenizes this choice. Consequently, some analysts consider the dual approach preferable to primal (production function) estimation. However, in a setting such as rural Madagascar, where few inputs are transacted and there is considerable spatiotemporal variability in input and output prices, the errors in variables problem associated with estimating the dual cost or profit function is likely no less severe than the endogeneity problem associated with primal estimation. Moreover, endogeneity problems must now be plot specific since we control for unobserved farmer and farm specific effects. These effects are likely relatively modest, although they surely exist and must be kept in mind as one interprets results.

The differential production or yield function estimation method works as follows. Imagine two different technologies, each with two sets of arguments that produce output y. First, the r-dimensional vector **x** comprises production inputs under the control of the farmer, such as land, labor, animal traction and organic or inorganic nutrient amendments made to the soil. Second, the t-dimensional vector **z** includes distinct farmer- or farm-specific characteristics that are exogenous (in the short-term, at least) to decisions regarding input application rates. The **z** vector includes environmental conditions such as rainfall (quantity and timing), temperature, sunlight and density of pathogens and pests in the area, as well as plot characteristics, such as location on the toposequence, water source and soil quality, and farmer characteristics, some of which are observable (e.g., experience with SRI, age, gender, education), and some of which are unobservable, such as farmer health and energy level, work ethic, farming aptitude, etc.

Because farmers worry about production risk as well as expected output, we follow the Just and Pope (1977) tradition, permitting inputs to have either positive or negative marginal effects on production risk. The two technologies can be represented by the general functional forms

$$y_f = f(x,z) + h_f(x,z)^{1/2}\zeta_f \tag{1}$$

$$y_g = g(x,z) + h_g(x,z)^{1/2}\zeta_g \tag{2}$$

where the f and g subscripts reflect the technology employed, and ζ is a shock with mean zero and variance σ_i^2 (i=f,g) that is independent across the cross-sectional observations. This implies that the conditional expectation functions are f(x,z) and g(x,z), respectively. Using a first-order flexible approximation (i.e., first-order with interaction effects) to the true conditional expectation function for each technology[14] gives us

$$E[y_f]=\alpha_{f0}+\Sigma_{i=1}^{r}\alpha_{fi}x_{fi}+\Sigma_{i=1}^{r}\Sigma_{j=1\neq i}^{r}\beta_{fij}x_{fi}x_{fj}+\Sigma_{i=1}^{t}\gamma_{fi}z_{fi}+\Sigma_{i=1}^{t}\Sigma_{j=1\neq i}^{t}\eta_{fij}z_{fi}z_{fj}+\Sigma_{i=1}^{r}\Sigma_{j=1}^{t}\tau_{fij}x_{fi}z_{fj} \tag{3}$$

$$E[y_g]=\alpha_{g0}+\Sigma_{i=1}^{r}\alpha_{gi}x_{gi}+\Sigma_{i=1}^{r}\Sigma_{j=1\neq i}^{r}\beta_{gij}x_{gi}x_{gj}+\Sigma_{i=1}^{t}\gamma_{gi}z_{gi}+\Sigma_{i=1}^{t}\Sigma_{j=1\neq i}^{t}\eta_{gij}z_{gi}z_{gj}+\Sigma_{i=1}^{r}\Sigma_{j=1}^{t}\tau_{gij}x_{gi}z_{gj} \tag{4}$$

We can turn equations (3) and (4) into standard regression models by adding a mean zero, normally distributed regression error term to each that is independently distributed across farms, ε_f and ε_g, respectively. We can also turn it into a yield equation by dividing elements of **x** and y by area cultivated.

The expected productivity gains attributable to the new technology are reflected in the differences between the estimable parameters of the two production functions. This can be captured directly by combining equations (3) and (4) into a switching regressions specification that yields both the parameter estimates associated with the old technology, g(x,z), and the marginal and base productivity change estimates associated with the new technology, f(x,z). To do this, we stack the observations from the two different technologies and add an indicator variable, NEW, taking value one on plots planted in the new technology and zero on those cultivated using the old technology. The switching regressions specification is then just

$$\begin{aligned}E[y]=&\alpha_{g0}+\Sigma_{i=1}^{r}\alpha_{gi}x_i+\Sigma_{i=1}^{r}\Sigma_{j=1\neq i}^{r}\beta_{gij}x_ix_j+\Sigma_{i=1}^{t}\gamma_{gi}z_i+\Sigma_{i=1}^{t}\Sigma_{j=1\neq i}^{t}\eta_{gij}z_iz_j+\Sigma_{i=1}^{r}\Sigma_{j=1}^{t}\tau_{gij}x_iz_j\\&+NEW[\alpha_0+\Sigma_{i=1}^{r}\alpha_ix_i+\Sigma_{i=1}^{r}\Sigma_{j=1\neq i}^{r}\beta_{ij}x_ix_j+\Sigma_{i=1}^{t}\gamma_iz_i+\Sigma_{i=1}^{t}\Sigma_{j=1\neq i}^{t}\eta_{ij}z_iz_j+\Sigma_{i=1}^{r}\Sigma_{j=1}^{t}\tau_{ij}x_iz_j]\end{aligned} \tag{5}$$

[14] The method we introduce works for any linear-in-parameters specification. Because of small sample size in our SRI application, we stick to a very simple linear approximation with limited interactions, as discussed in Section 5.

where α_0 captures captures the expected base productivity difference irrespective of input levels (α_{f0}-α_{g0}). Similarly, the slope coefficients within the bracketed term reflect expected marginal input productivity differences attributable to the new technology. If we could observe all the elements of x and z, we could directly estimate equation (5) using a random effects estimator in order to recover the productivity differentials attributable to the new technology.[15] Unfortunately, much of the key content of the z vector – attributes such as farmer aptitude, work ethic, the timing of rains, local pathogen and pest problems, etc. – rarely gets observed and recorded in farm production data. In so far as the observable x and z variables are correlated with the unobserved elements of z, unobserved heterogeneity will bias the estimated coefficients of the two production functions and will therefore also bias estimates of the base and marginal productivity differentials of interest if we estimate equation (5) directly.

If individual farmers are simultaneously using both technologies, however, we can use the resulting matched pairs sample – a sample of paired plots cultivated by the same farmer in the same year, one with the new technology treatment, the other with the traditional technology control – to resolve the unobserved heterogeneity problem. If instead of combining them into a single switching regressions specification, we instead subtract equation (4) from equation (3), we get the differential production function

$$dy = \alpha_0 + \Sigma_{i=1}^{r}\alpha_i dx_i + \Sigma_{i=1}^{r}\Sigma_{j=1\neq i}^{r}\beta_{ij} dx_i dx_j + \Sigma_{i=1}^{t}\gamma_i dz_i + \Sigma_{i=1}^{t}\Sigma_{j=1\neq i}^{t}\eta_{ij} dz_i dz_j + \Sigma_{i=1}^{r}\Sigma_{j=1}^{t}\tau_{ij} dx_i dz_j + d\varepsilon \qquad (6)$$

Where dy= $E[y_f]$-$E[y_g]$ is the difference in expected output, dx=x_f-x_g reflects the difference in input application rates on plots using the two different technologies, dz=z_f-z_g reflects exogenous differences in the plots (e.g., soil type, source of water, or location on the toposequence), $d\varepsilon = \varepsilon_f$-$\varepsilon_g$ is a mean zero, independent error term, and the α_i, β_{ij}, γ_i, η_{ij}, and τ_{ij} parameters directly estimate the marginal productivity differences between the two technologies, just as they did in the bracketed term of equation (5). All farmer-specific but plot-invariant characteristics, whether observed (e.g., farmer education, gender, age, prices, temperature, rainfall, sunlight) or unobserved (e.g., farming skill, timing of local biotic and abiotic stresses, capacity to motivate workers), have been

[15] We use feasible generalized least squares although one could equally do this via maximum likelihood.

differenced away to remove potential sources of bias.[16] Direct estimation of equation (6) therefore gives us consistent and unbiased estimates of the marginal productivity differences attributable to the new technology.

The new technology may change the returns to observed and unobserved farmer or farm characteristics that we have differenced away in equation (6). One might expect, for example, that because SRI requires careful, daily water control, returns to farmer self-discipline and dependability or to a farmer's constant presence on-farm (relative to those who may regularly migrate for brief periods and thus be unavailable on-farm each day) would increase as a farmer switched from SRT to SRI. An SRI adopter who was not reliable about daily water management would likely not enjoy gains as large as an otherwise identical farmer who performed the prescribed daily tasks. Our method explicitly allows for this. Because the underlying skill or characteristic does not vary across plots cultivated by the same farmer – only the prospective returns to the skill vary – differencing eliminates the source of bias with respect to the marginal productivity differential parameters of interest. However, the intercept estimate of α_0 from equation (6), call it $\hat{a}^{D}$, now captures the net mean change in returns on farm and farmer-specific characteristics as well as the true underlying unconditional change in productivity. Therefore, $\hat{a}^{D}$ provides a biased estimate of the true base productivity gains associated with the new technology, α_0, because it incorporates output gains associated with adopters' observable and unobservable attributes that may not be replicable in the broader population of non-adopters.

We can correct for this, however, by going back to the switching regressions specification of equation (5) and imposing the consistent and unbiased slope parameter estimates of α_i, β_{ij}, γ_i, η_{ij}, and τ_{ij} obtained from estimating the differential production or yield function in equation (6). We can then estimate the coefficients on the remaining farm-specific (but plot-invariant) variables in **z**, thereby generating a potentially different estimate for α_0, $\hat{a}^{RE}$, that fully controls for the observable farm and farmer characteristics

[16] In this pooled regression context, differencing across observations – as opposed to taking differences from the mean – also has the advantage of asymptotic efficiency given arbitrary correlation between observed and unobserved explanatory variables. See Wooldridge (2001) for technical details.

previously differenced away in equation (6). If our data set included non-adopters as well as adopters – unfortunately, ours does not – we could instrument for the SRI indicator variable and thereby also control for potential bias due to unobservables. The resulting $\hat{a}^{RE}$ intercept estimate for α_0 will be an unbiased and consistent estimate of the base productivity difference attributable just to the new technology irrespective of input application rates and farm and farmer-specific observable or unobservable characteristics.

With unbiased estimates of the base and marginal productivity gains from the new technology, one can then decompose observed output gains so as to establish the degree to which observed productivity gains are due to a better technology, better plots or better farmers. Gains attributable to changes in the marginal productivity of inputs equal the product of slope parameter estimates from equation (6) and the sample mean of the **x** variables. Gains due to differences in plot-specific characteristics equal the product of the slope parameter estimates from equation (6) and the sample mean values of **z**. Unconditional productivity gains from the new technology equal the corrected base productivity change estimate of α_0, $\hat{a}^{RE}$, plus the estimated direct effects of experience with the new technology times the sample mean experience level. Gains attributable to farmer-specific effects must then equal the residual output or yield changes not accounted for by the marginal input productivity, marginal plot productivity or unconditional productivity gains. This method thus permits proper attribution of observed gains between the technology and underlying farmer and plot characteristics associated with adoption of the new technology. Without such a method, one cannot be sure whether observed output gains reflect a better technology, adopters who are better farmers, or placement effects associated with better plots.

Differential production risk estimation

Production risk also matters to farmers. We can follow a similar strategy to estimate the differential production or yield risk associated with the new technology. The conditional variance of the Just-Pope functional forms specified in equations (1) and (2) are $V[y_f] = \sigma_f^2 h_f(x,z) = E[\varepsilon_f^2]$ and $V[y_g] = \sigma_g^2 h_g(x,z) = E[\varepsilon_g^2]$, respectively. If we could observe all the **x** and **z** variables – or if the unobserved variables were uncorrelated with the observables – then we could simply regress the squared residuals from equation (5) on a similar switching regressions specification in **x** and **z**. The coefficients on the terms interacted with the NEW indicator variable would then provide direct estimates of the base and marginal risk effects of the new technology. When the coefficient estimates on equation (5) are biased and inconsistent, however, due to farm or farmer-specific unobservables, this approach will yield similarly inconsistent and biased estimates of the production risk effects of the new technology.

Instead, we can compute the difference in output variance attributable to changing technologies as

$$s^2 = V[y_f] - V[y_g] = E[\varepsilon_f^2 - \varepsilon_g^2] = h(x,z) \qquad (7)$$

which can be estimated by differencing the squared residuals from the two technology-specific production functions and then regressing those differences on the **x** and **z** vectors.[17] The technology-specific production functions take as arguments the regressors from the differential production as well as the observable farmer-specific or plot-invariant effects (e.g., education, age, gender, a regional dummy to reflect rainfall, prices and local pathogens, etc.). We once again use a first-order flexible approximation to the true h function, estimating the differential production risk function as

$$s^2 = \theta_0 + \Sigma_{i=1}^{r}\theta_i x_i + \Sigma_{i=1}^{r}\Sigma_{j=1\neq i}^{r}\lambda_{ij}x_i x_j + \Sigma_{i=1}^{t}\varphi_i z_i + \Sigma_{i=1}^{t}\Sigma_{j=1\neq i}^{t}\pi_{ij}z_i z_j + \Sigma_{i=1}^{r}\Sigma_{j=1}^{t}\omega_{ij}x_i z_j + \psi \qquad (8)$$

where ψ is a mean zero, iid error term on the differential conditional variance regression. The parameters have similar interpretations with respect to production risk as did the estimable parameters of equation (5) have with respect to mean output. In particular, the slope coefficients estimate the marginal risk effects of the new technology as input application rates vary. The θ_0 estimate of the base risk differences captures the net mean change in risk due to farm and farmer-specific (but plot-invariant) characteristics as well as the true underlying unconditional change in production risk. One can correct for this by following the same strategy used to rid the α_0 estimates of bias due to selection on observables and unobservables, estimating the switching regression model in equation (5) with the interaction slope parameters imposed based on the results from equation (6),

[17] Alternatively, one could estimate only the $f_g(x,z)$ production function and take the difference between the squared residuals of the differential production function and two times the squared residuals from the estimated production function for technology g. A proof is available from the authors by request.

then regressing the squared residuals on **x** and **z** vectors, including observables differenced away in equation (8), now imposing the parameter estimates from the differential production risk function (8) and instrumenting, if possible, for the technology adoption indicator variable, NEW. Because our sample does not include non-adopters, we cannot instrument for adoption so as to control fully for the effect of unobservable farm and farmer characteristics on the base productivity gains from the new technology.

This technique of using differential production or yield function estimation based on equations (6) and (8) to generate efficient and consistent estimates of the marginal yield and yield risk effects, then reestimating the restricted random effects switching regression model to obtain consistent estimates of the base productivity and risk effects provides a method for establishing the differential effect of a new technology on expected outputs levels and on production risk using a matched pairs sample of data from farmers who practice both technologies simultaneously. We now apply this new method to our sample of SRI farmers from Madagascar.

5. Estimation Results Among Malagasy SRI Farmers

In implementing the method introduced in the previous section, we lose four observations due to incomplete data, leaving us with only 107 observations from paired, randomly selected SRI and SRT plots, each pair cultivated by the same farmer. With such a small sample, precise estimation of the parameters of interest proves difficult, so we have dropped variables that initial estimation runs showed were of both extremely low statistical significance (p-values greater than 0.5) and very low magnitude, including animal traction, details on plot location along the toposequence, age of plants at transplanting, number of weedings, and a variety of interaction terms between variables.[18]

Table 5 reports our regression results for rice yield (kg/hectare). Results for absolute output volumes (kg) are qualitatively very similar and therefore omitted in the interests of

[18] It is perhaps a bit surprising that age of seedling at transplanting did not seem to have an effect since controlled trials have shown this variable to have a significant, positive effect on yields (Randriamiharisoa 2002). This likely results from the limited variation in SRI transplant age in this sample of observational data. Experimental data, by contrast, generate sufficient variation to identify such effects.

brevity. The lefthand column shows the coefficient estimates of the unrestricted random effects switching regression, equation (5), that fails to control for farm and farmer-specific effects. In principle, all of the coefficient estimates in that specification are biased and inconsistent. The middle column displays the differential yield function estimates from equation (6). The marginal yield effect estimates are consistent under the this method, but the base productivity gain estimate is not. The righthand column reports the corrected base productivity change estimate from the restricted random effects model in which the slope estimates from the bracketed portion of equation (5) are restricted to equal the estimates generated by the preceding differential yield function (the middle column).

The results confirm both the economic and statistical importance of controlling for farm and farmer specific attributes in estimating the marginal input productivity and risk effects of SRI and the importance of correcting for the bias in direct estimates of α_0 and $\theta_0$using the differential yield function estimator. The unrestricted model implies base productivity gains more than double the restricted random effects model with proper controls for observables and unobservables, while the differential yield model base productivity gain estimate is 84 percent higher. These differences underscore the important role played by farm and farmer-specific effects among early adopters in generating the impressive yield gains observed in SRI and many other new technologies. The base productivity gains of SRI nonetheless remain considerable, nearly 1.4 tons/hectare.[19]

Once we difference away farm and farmer-specific effects, the only statistically significant marginal productivity effect of SRI arises in rich soils, where it generates an extra 1.1 tons/hectare expected yield. Some observed productivity effects are therefore due to plot selection. Absence of good water control reduces the expected productivity gains from SRI, as reflected in the negative (albeit statistically insignificant) coefficient

[19] We reiterate that the sample villages were purposively chosen to capture locations that had been relatively successful with SRI. This sample may thus generate some upward bias in the estimated base productivity gains associated with SRI because we cannot control adequately for village-level factors related to water and extension availability, soil quality, market access, etc.

estimate on the variable for days of water shortage experienced on the plot. Since careful water management – reducing water demands appreciably, in many cases – is a cornerstone element of the SRI method, it comes as little surprise that good water control increases the relative productivity gains associated with switching from traditional rice cultivation in fields with standing water. These results underscore that part of the output gains commonly observed in SRI farmers' fields could be due to plot-specific effects related to farm-specific differences in soil fertility or water control, although the former explanation would seem to have relatively little explanatory power for aggregate performance patterns, given the absence of significant average plot level fertility differences.

The other marginal yield effects of SRI are imprecisely estimated and thus statistically insignificantly different from zero at conventional significance levels, perhaps due to our small sample size and noise in these regressors. The point estimates are nonetheless intuitive and plausible. The marginal yield gains associated with SRI appear to be negatively associated with plot size, while expected marginal yield gains are increasing in labor and manure application rates[20] – helping explain higher mean application rates on SRI fields relative to SRT plots – and in experience with the method. However, because experience has a negative estimated effect on the marginal labor productivity effect of SRI, this should probably not be interpreted as a learning by doing effect. Marginal land productivity is effectively invariant to years in SRI, suggesting that some agronomists' concerns that SRI's higher yields might originate in soil nutrient mining are inconsistent with these data.[21] On balance, added experience does not seem generate significant yield increases with SRI, nor significant changes in yield risk (see section 6).[22]

[20] Chemical fertilizer application was omitted from these regressions because it was used by so few farmers (<10%) and because its use is likely endogenous and we have not suitable instruments for fertilizer use. Manure application, on the other hand, is more likely a function of the number of cattle owned and is typically applied earlier in the season than chemical fertilizers in this system.

[21] Uphoff et al. (2002) report on several experiments suggesting that SRI enhances mineralization of organic matter, increasing the pool of nutrients available for uptake by the plants and enabling sustainable yield increases.

[22] Indeed, we find no statistically significant relationship between years of SRI experience and proportion of rice area planted in SRI either in the simple bivariate regression of the latter on the former or in multivariate regressions adding a range of other control variables. Regression results available from the authors by request. Moser and Barrett (2003c) study SRI adoption in greater detail using a different data

Statistical tests confirm the appropriateness of our approach. A Hausman specification test rejects the random effects specification in the unrestricted random effects regressions (p-value = 0.009) but not in the restricted random effects model (p-value = 0.395). A Lagrange multiplier test finds the differences between the parameter estimates of the restricted and unrestricted random effects models jointly statistically significant, with a p-value of 0.0004.[23] The restricted random effects model fits the data well, with an adjusted r^2 of 0.64.

As explained in section 4, we can use the restricted random effects estimates to decompose the unconditional yield gains observed in sample (2.96 tons/hectare, per Table 2) into those properly attributable to SRI, those due to differences between SRI and SRT plots, and gains due to farm and farmer-specific effects (e.g., increased returns to the skills possessed by early adopters). The gains properly attributable to SRI include the unconditional productivity gains due to SRI itself – the restricted random effects model base productivity change estimate (1.36 tons/hectare) plus the differential yield function estimate of the direct effect of experience with SRI evaluated at sample mean experience (2.5 years) – as well as the marginal yield gains SRI induces in labor, land and nutrient amendment inputs, evaluated at the sample mean variable input levels (Table 6). By this method, roughly half the observed yield gains can be attributed to SRI itself. Of this amount, the estimated aggregate marginal productivity effect of SRI on variable inputs was slightly negative (-1.5%), which may help explain limited uptake of SRI. Since there were scant differences in plot soil quality or water availability, it should come as little surprise that only an estimated 0.8% of the observed yield gains appear attributable to plot selection effects. The observed and unobserved farmer-specific effects that we differenced away in estimating the differential yield function thus account for nearly half the total yield gains associate with cultivation using SRI methods. This is consistent with

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set and similarly find no positive relation between experience with SRI and the share of land planted in the technology.

[23] Diagnostic tests for residual serial correlation, a common source of inflated goodness-of-fit measures, cannot reject the null hypothesis of no serial correlation at any reasonable significance level.

the broader technology adoption literature, which consistently finds that initial adopters are better farmers overall (Feder et al. 1985, Rogers 1995, Sunding and Zilberman 2001).

The implication is that although raw descriptive statistics may lead to some exaggeration, the productivity gains from SRI are nonetheless quite substantial. Even with only about half of the observed productivity gains truly attributable to SRI practices, the estimated output gains due just to switching to SRI (i.e., setting experience equal to zero, evaluated at mean SRI plot size and labor and manure application rates) is 2.8 tons/hectare, or 84.2 percent over mean SRT yields in this sample. This is likely an upwardly biased estimate of the gains to be expected by farmers in lower potential regions – especially those with poorer water control – but it suggests that the productivity gains from SRI are indeed real and reproducible out of sample, irrespective of observable or unobservable farm and farmer attributes.

6. The Production Risk Implications of SRI

With such considerable yield gains on offer, why don't more Malagasy smallholders take up SRI and why don't more adopters apply this method on all of their rice land? In addition to seasonal financing and labor availability constraints explanation offered by Moser and Barrett (2003c), SRI's effects on yield risk may be a big part of the answer.

All three regression models suggest SRI increases yield risk, as reported in the bottom panel of Table 5. This is consistent with the discussion in section 3 about the risk effects of early seedling transplanting and SRI's more sensitive water management regime. SRI yields and labor productivity appear more variable in the simple descriptive statistics and indeed the technology does seem riskier once one uses more sophisticated econometric methods to identify its yield risk effects.

These results likewise confirm the appropriateness of the method we introduce. The consistent θ_0 estimate from the restricted random effects model is 58 percent higher than the inconsistent estimate generated by the differential yield function, suggesting that plot-invariant adopter characteristics dampen yield differences across plots. Once we control properly for unobservables, SRI appears to have no significant marginal risk effects. Not even experience with SRI seems to dampen the increase in yield risk attributable to SRI adoption.

The additional yield risk associated with SRI may explain why some more risk-averse farmers do not adopt the method at all and why most adopters do not put all their land in SRI. Newbery and Stiglitz (1981) offer a simple, back of the envelope method for estimating households' willingness to pay for risk reduction.[24] Assuming symmetric treatment of risk, this serves equally well as a rough estimate of willingness to accept increased risk. By this method, farmers should be willing to take on additional yield risk so long as their risk aversion – as measured by the conventional Arrow-Pratt coefficient of relative risk aversion, R – is less than twice the quotient of the proportional change in mean yield divided by the squared coefficient of variation of yield. Using the same decomposition method we employed in the previous section with respect to mean output changes to recover the expected change in yield variance attributable to SRI, we find that the minimum R is approximately 1.28, on the lower end of most published estimates of farmer risk aversion, which are typically in the 1.0-5.0 range.[25] This suggests that the increased yield risk associated with SRI adoption may not be acceptable for a large proportion of Malagasy rice farmers even given large expected returns. Only those farmers whose preferences exhibit greater-than-normal tolerance for risk or who have (financial or nonfinancial) means to insure against yield risk would likely be willing to adopt and stick with SRI. Among the set of adopters, moreover, risk management considerations may well make it rational to limit the extent of adoption of SRI because yield risk increases while expected yield gains decreases as one puts more land into SRI. The risk-return tradeoff thus works against expansion of area under SRI for those farmers whose risk preferences fall in the neighborhood of the threshold R. This would seem to provide a plausible explanation for widespread partial adoption of SRI.

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[24] Newbery and Stiglitz (1981) derive this result by setting certainty equivalent utility equal to expected utility, then taking a second-order Taylor expansion and rearranging terms.

[25] See Chetty (2003) for a recent review of past coefficient of relative risk aversion estimates and an argument that the parameter most likely falls on the lower end of that bound.

7. Conclusions

It can be exceedingly difficult to establish precisely the true productivity gains associated with a new production method because farmers choose whether or not to adopt. In so far as imperfectly observed farmer and farm heterogeneity can introduce selection bias in observational production data, it may be difficult to assess the true output gains attributable to the new technology, since more skilled farmers typically get more out of any technology – new or established – and are commonly the first to adopt improved technologies and often apply them on their best plots.

In this paper we introduced an econometric method to disentangle the yield effects in both mean and variance attributable to a new technology from those associated with farmer- and plot-specific characteristics. Applying the method to SRI, a promising new rice cultivation method, we find that SRI indeed generates substantial productivity gains, although only about half the observed yield increases associated with SRI use arise due to the new technology itself. Fully half the gains reflect farm- and farmer-specific effects. Straight comparisons of unconditional yield data therefore suffer what might be termed "green thumb bias". Little of the observed output gains from SRI are due to plot-specific attributes such as better soils or water control.

SRI also significantly increases yield risk. Rough estimates of the threshold relative risk aversion necessary for Malagasy farmers to find SRI adoption attractive fall on the lower end of the range of common estimates of risk preferences, suggesting that yield risk associated with early transplanting and changed water management may significantly limit uptake of SRI. Early evidence suggests that SRI uptake is proceeding more rapidly in the rice economies of east, south and southeast Asia (N. Uphoff, personal communication). This difference could be partly attributable to greater risk bearing capacity among somewhat wealthier Asian farmers than among their quite poor Malagasy counterparts. Future studies should explore the differences in SRI adoption patterns between the major rice economies.

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Table 1: Farm and Farmer Characteristics (n=111)

Farmer Characteristics		
Mean age of farmer	40.93	
(Standard deviation)	(12.05)	
Percent male	87%	
Percent of farmers belonging to farmer organization	51%	
Percent of farmers with no education	4%	
Percent of farmers with high school education or better	29%	
Mean months of *soudure* (no rice consumption)	2.63	
(Standard deviation)	(2.84)	
Farm Characteristics		
Mean total rice area (ares)	132.74	
(Standard deviation)	(132.87)	
Percent of Rice land in SRI	46%	
Percent with tractor	2%	
Rice Cultivation Methods	**SRI Fields**	**SRT Fields**
Mean years of experience with method	2.52	17.87
(Standard deviation)	(1.86)	(12.97)
Mean number of plots under method	6.4	8.6
(Standard deviation)	(7.5)	(8.5)
Mean days of water shortage in field	42.70	41.09
(Standard deviation)	(68.43)	(74.58)
Percent of fields on rich soils	29%	27%
Percent using manure on fields	21%	21%
Percent using chemical fertilizer on fields	10%	7%
Percent with rainfed fields	0%	7%

Table 2. Productivity of land and labor

	SRI		SRT		Mean Percent change	Median Percent Change
	Mean	Standard Deviation	Mean	Standard Deviation		
Yield (kg/hectare)	6327	1795	3368	506	88	85
Labor Productivity (kg/day)	9.5	14.6	5.5	4.4	73	52
Non-harvest labor (days/hectare)	1280	1052	1074	1075	19	27

Table 3. Labor and land productivity by years of experience

Years of Experience	Mean percent of land in SRI	Median percent yield increase	Median percent labor productivity increase	% of farmers with negative productivity gains with SRI	Number of farmers
1	36%	88%	62%	33%	42
2	38%	61%	12%	45%	22
3	30%	101%	59%	33%	21
4	28%	94%	99%	33%	9
5+	42%	101%	159%	27%	15
All	36%	85%	52%	35%	109

Table 4: SRI-SRT yield correspondence

(107 observations)

Dependent variable = SRI yield (kg/ha)	Estimates (robust standard errors)	
Intercept	2652.15*** (1119.34)	3450.90*** (1138.54)
SRT yield (kg/ha)	0.93*** (0.31)	0.88*** (0.31)
Years SRI experience	99.57 (81.54)	84.53 (80.97)
Area in SRI (ha)	—	0.78 (1.52)
Labor application rate (days/ha)	—	-35.93** (15.65)
Rich soils (dummy)	—	661.47** (345.34)
Manure application rate (kg/ha)	—	6.70 (6.38)
Days of water shortage	—	-3.32*** (1.35)
Adjusted r^2	0.071	0.17
F statistic (p-value)	4.85 (0.0097)	4.47 (0.0000)

*, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively.

Table 5: Estimated difference in mean rice yields under SRI

Mean yield (kg/hectare)	Model		
	Unrestricted Random Effects	Differential Yield	Restricted Random Effects
Base productivity change	2932.12*** (812.23)	2502.56*** (290.50)	1361.31*** (450.93)
Marginal yield changes:			
Land (ares)	-3.45 (3.70)	-3.59 (2.87)	*-3.59*
Non-harvest labor (days/are)	20.02 (29.84)	33.02 (20.69)	*33.02*
Experience (years)	178.92* (98.86)	61.65 (49.56)	*61.65*
Rich Soils (dummy)	923.91*** (276.83)	1110.54*** (361.30)	*1110.54*
Manure application (kg/are)	9.78** (4.83)	3.65 (4.11)	*3.65*
Days of water shortage	-4.19* (2.23)	-1.92 (1.78)	*-1.92*
Land x Experience	2.83** (1.19)	0.18 (0.83)	*0.18*
Labor x Experience	-13.12* (7.24)	-12.60 (9.55)	*-12.60*
Adjusted r^2	0.74***	0.14***	0.64***
Variance of yield x 10^{-4}			
Base risk change:	383.83* (198.63)	195.19*** (68.00)	307.92*** (92.10)
Marginal risk changes:			
Land (ares)	-1.31 (0.90)	0.29 (0.90)	*0.29*
Non-harvest labor (days/are)	3.01 (3.93)	-1.14 (5.98)	*-1.14*
Experience (years)	-39.23 (30.83)	-11.39 (16.84)	*-11.39*
Rich Soils (dummy)	-70.73 (89.09)	-17.99 (59.69)	*-17.99*
Manure application (kg/are)	1.92** (0.79)	2.73 (7.08)	*2.73*
Days of water shortage	-0.81** (0.37)	-3.40 (2.16)	*-3.40*
Land x Experience	0.30 (0.28)	0.21 (0.20)	*0.21*
Labor x Experience	-0.46 (1.08)	1.07 (1.67)	*1.07*
Adjusted r^2	0.18***	0.06	0.18***

Standard errors in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent levels, respectively. Random effects regressions include controls for gender, region, educational attainment and age. Italicized parameter values imposed from prior stage, not estimated.

Table 6: Decomposition of expected output gains by source

Percent of mean SRI output gains due to …	
SRI method, of which	49.8%
Unconditional productivity gains from	
Base productivity effect	46.0
Experience with SRI	5.3
Marginal yield gains from	
land, labor and manure inputs	-1.5
Plot-specific characteristics (soils and water availability)	0.8%
Farmer-specific effects	49.4%

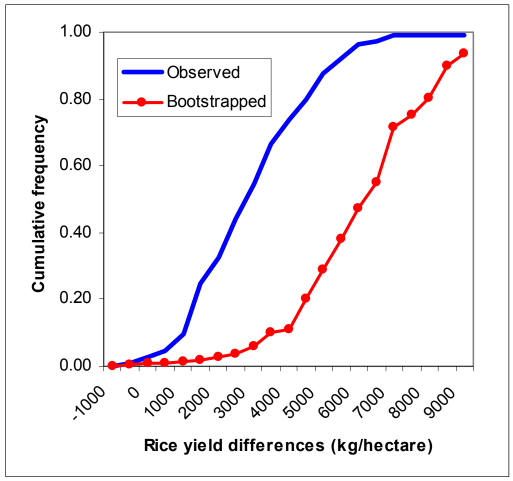

Figure 1: Actual versus bootstrapped SRI-SRT yield differences

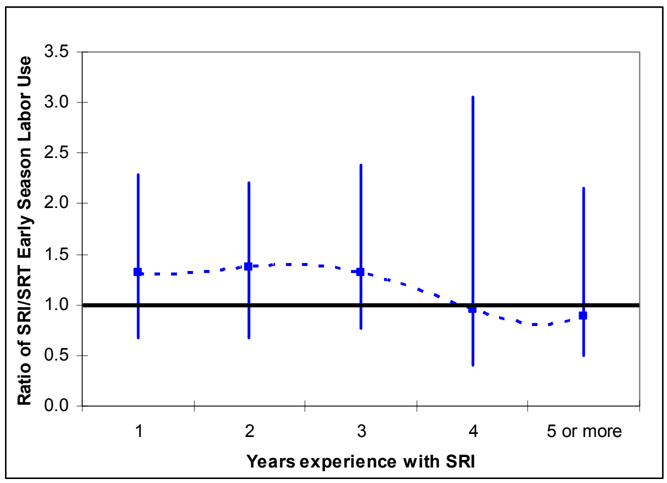

Figure 2. Median and span of labor use ratio

Figure 3. Median and span of the percent change in labor productivity