SENDING FARMERS BACK TO SCHOOL:

THE IMPACT OF FARMER FIELD SCHOOLS IN INDONESIA

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Abstract: This paper evaluates the impact of Farmer Field Schools, an intensive participatory training program emphasizing integrated pest management. The evaluation focuses on whether participation in the program has improved yields and reduced pesticide use among graduates and their neighbors who may have gained knowledge from graduates through informal communications. The study utilizes panel data covering the period 1991-1999 in Indonesia. The analysis, employing a modified "difference-in-differences" model, indicates that the program did not have significant impacts on the performance of graduates and their neighbors. Several plausible explanations for this outcome are discussed, and recommendations for improvements are suggested.

Keywords: Extension; Farmer Field Schools; Impact Evaluation; Indonesia; Participatory Training

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I. Background

Agricultural extension and farmer education programs are key policy instruments for governments seeking to improve the productivity of agriculture while protecting the environment. Correspondingly, there is great interest in the impact of such public investments and in their financial viability. The experience of extension systems over the past few decades has been mixed. Some studies estimate high rates of return to the investment in extension (Birkhaeuser, Evenson, and Feder), or to farmer education (Jamison and Lau; Lockheed, Jamison, and Lau). Yet, many observers document poor performance in the operation of extension and informal education systems, due to bureaucratic inefficiency, deficient program design, and some generic weaknesses inherent in publicly-operated, staff-intensive, information delivery systems (Feder, Willett, and Zijp). One deficiency highlighted by researchers and practitioners is the tendency of many public officers dealing with the transmission of knowledge to conduct their assignment in a "top-down" manner. Often, the information that is conveyed is presented as a technological package comprised of recommended practices. This is perceived as a less effective method for improving knowledge as compared to more participatory approaches (Axxin; Braun, Thiele, and Fernandez).

In recent years, a number of development agencies, including the World Bank, have promoted farmer field schools (FFS) as a more effective approach to extend science-based knowledge and practices to farmers. Though pioneered and first promoted by the Food and Agriculture Organization (FAO) as a practical way of diffusing knowledge-intensive integrated pest management (IPM) concepts and practices for East Asian rice-based systems (Kenmore

1991; van de Fliert), the FFS has since evolved to include a much broader coverage of other farm-relevant topics in its curriculum. The FFS training program utilizes participatory methods "to help farmers develop their analytical skills, critical thinking, and creativity, and help them learn to make better decisions" (Kenmore 1997). Such an approach, in which the trainer is more of a facilitator, rather than an instructor, reflects a paradigm shift in extension work (Roling and van de Fliert).

As an extension approach, the FFS concept does not require that all farmers attend FFS training. Rather, only a select number of farmers within a village or local farmers' group are trained in these informal schools, which entail weekly meetings in a season-long training course. However, in order to disseminate new knowledge more rapidly within the community, selected farmers receive additional training to become farmer-trainers, and are expected to organize field-school replications within the community, with some support from public sources. Furthermore, all FFS graduates are encouraged to share their knowledge and experiences with other farmers within their local village and community organizations. These farmer-to-farmer diffusion effects are expected to bring about cost-effective knowledge diffusion and financial sustainability, issues that have hampered many public extension systems in both developed and developing countries (Quizon, Feder, and Murgai; Hanson and Just).

However, there are risks inherent in the FFS approach. Given the complexity of the information conveyed in the training program, the ability to convey effectively to other farmers complex decision making skills through informal communications may be limited and may curtail the diffusion process. Moreover, the requirement from trainers to abandon earlier top-down approaches to which they may have been accustomed in favor of a facilitation mode is a challenge that poses risks to the effectiveness of the program.

Previous studies evaluating the impact of FFS at the farm-level report significant impacts of program participation on farm-level yields and profits, and a decline in pesticide use. For example, studies by Nanta in Thailand and Ekneligoda in Sri Lanka claim that pesticide applications decreased with more IPM knowledge and FFS training, while rice yields increased by as much as 25%. A similar study by Ramaswamy, Shafiquddin, and Latif for Bangladesh notes that FFS-schooled farmers had 8-13% higher rice yields than their non-FFS counterparts. Similarly high impacts on farm profits are also reported by studies conducted in Vietnam, Ghana, Cote d'Ivoire and Burkina Faso (cited in Kenmore 1997). Increases of profits of 40% in Sri Lanka, 30% in Thailand, and 10-25% in China are cited in FAO (2000, p. 18).

However, most previous studies have not accounted for econometric problems that arise in estimating program impact when the placement of the program across villages and the selection of farmers for participation in the program are not done at random. These, and other econometric issues discussed later in the paper, are likely to bias estimates of program impact. To our knowledge, the actual farm-level impact of this diffusion concept in an area where FFS operated on a large scale over a reasonable period of time has not been studied rigorously.

This paper evaluates the impact of the FFS effort on farm-level outcomes (yields and pesticide use), focusing on Indonesia as a case study.[1] We employ a modified version of a "difference-in-differences" approach to evaluate program impacts, utilizing a panel household survey that includes information on both field school graduates and other farmers. The empirical strategy allows us to separately identify the direct impact on farmers who participated in the program, as well as the secondary impacts through farmer-to-farmer diffusion from graduates to other community members. It also enables us to control for concurrent exogenous events and interventions that are likely to affect farm performance over time, and to deal with selection biases that arise due to the potential endogeneity of participation by farmers and biases inherent in non-random placement of training programs across communities.

The Indonesian experience holds lessons for development agencies and governments in developing countries, who are being encouraged to expand and promote the FFS approach on a wider scale. Presently, there are large national FFS programs in several countries, and pilot or smaller scale programs in many countries of Asia, Africa, and Latin America. Of the various countries, Indonesia has had amongst the longest experiences with the FFS approach and therefore, provides the opportunity to study long-term secondary diffusion impacts that may not yet be manifest in other countries where FFS is relatively recent. The Indonesian FFS program is typical of large scale applications of the FFS concept, and has served as a model for programs elsewhere (Kenmore 1991). Thus insights of the performance of the program in Indonesia are of relevance in other countries considering national programs. Farmer field schools are a significant public expenditure undertaking if implemented on a large scale, as the cost per trained farmer is significant (Fleischer, Waibel, and Walter-Echols; Quizon, Feder, and Murgai) and a rigorous investigation of their impact is important. Furthermore, the effectiveness of reliance on farmer-to-farmer diffusion of complex information is of general interest in contemplating extension approaches.

Our empirical results do not indicate that farmer field schools in Indonesia have induced significant improvements in yields or reduction in pesticide use by graduates relative to other farmers. Not surprisingly then, secondary diffusion effects on those exposed to graduates are also not significant. While we are unable to pinpoint a specific reason for these results, we advance some plausible arguments that might explain the absence of significant program impacts.

The rest of the paper is organized as follows. Section II develops a conceptual framework that underlies the empirical work. Section III describes the FFS program in Indonesia and the data set utilized in the study. In Section IV, we formulate the empirical specification and testable hypotheses, and in Section V present the key findings of the empirical analysis. The last section highlights the conclusions.

II. Conceptual Framework

FFS training aims to affect farmers' knowledge, which can be interpreted broadly to include the possession of analytical skills, critical thinking, and ability to make better decisions, as well as familiarity with agricultural practices and understanding of interactions within the agricultural ecosystem. Improved knowledge is, in turn, reflected in farmers' cultivation procedures, input decisions, and crop yields.

Because knowledge and farmer performance can improve over time even in the absence of FFS training (through communication with other farmers, contact with other sources of information etc.), we model performance as a growth process. As improved knowledge is reflected in cultivation decisions, farm performance indicators such as yields and input use (denoted by Y) may also change over time, at some rate, say α.[2,3] A field school can be expected to improve this performance growth rate for farmers by increasing farmers' knowledge.

Improvements in performance are expected for two groups of farmers. First, FFS training directly affects outcomes of field school "<u>graduates</u>" (i.e., those who have participated in the training course), as it improves their ability to acquire and process information (through experimentation, interpretation of experience, etc). Second, the training may also indirectly affect farmers who are "<u>exposed</u>" to FFS graduates. These are farmers who live in villages that have field school graduates, and therefore may have gained knowledge indirectly through farmer-to-farmer diffusion. "Control" farmers, who live in villages where no farmer has received training, are unaffected by the program, and therefore continue to grow at the pre-program growth rate of α.

In order to capture these features, consider a simple model in which an FFS program is introduced in some villages in cropping season T^*, while other villages are not exposed to the program. Define a period between two points in time T_0 and T_1, where $T_0 < T^* < T_1$, so that (T^*-T_0) represents the number of cropping seasons before exposure to the program, and (T_1-T^*) is the time period after exposure. In the villages without FFS, by definition, the number of seasons after program exposure is zero, and therefore $T_1=T^*$. Modeling a farmer's performance as an exponential growth process implies that outcome Y (e.g., yield or pesticide use) at time T_1 can be expressed as:

(1) $$Y_1 = Y_0 \cdot e^{\left\{\alpha\left(T^* - T_0\right) + \beta D_N\left(T_1 - T^*\right) + \mu D_G\left(T_1 - T^*\right) + \gamma\Delta X + \delta\Delta Z\right\}}$$

where α measures the pre-program growth rate in performance for all households, β is the growth rate of Y for exposed households after program exposure, and μ is the growth rate of Y for graduates after they have participated in the program. D_G and D_N are dummy variables for graduates and exposed farmers, with control farmers being the omitted category. Any fixed (time invariant) factors that affect outcomes at either the household or the village level are already embodied in Y_0, and thus do not need to be explicitly denoted. The variables X and Z denote vectors of farmer and village characteristics that also affect performance, γ and δ are the corresponding vectors of parameters, Δ denotes the differencing operator between periods T_0 and T_1 (e.g., $\Delta X = X_1 - X_0$), and e denotes the exponential operator.

Figure 1 illustrates the growth process embodied in equation (1) for the three groups of farmers. Control farmers maintain the original rate of performance growth α throughout the period ($T_1 - T_o$). This rate of growth is expected to improve for both graduates and exposed households, after program exposure at time T^*. In addition, the improvement among graduates is expected to be greater than among exposed farmers since graduates are intensively trained in FFS concepts. It is thus hypothesized that $\mu > \beta > \alpha$. The impact of FFS on performance of graduates and exposed farmers can be measured by $(\mu - \alpha)$ and $(\beta - \alpha)$, respectively. While in the case of yields the program is expected to induce a higher growth rate, the reverse holds in the case of pesticides where the desired outcome is a reduction in use.[4]

With the conceptual framework formulated, we turn to a description of the Farmer Field School program and its implementation in Indonesia. The econometric specification and testable hypotheses based on this conceptual framework are developed in Section IV.

III. Institutional Setting and Data

FFS in Indonesia

The typical FFS educates farmer participants on agro-ecosystems analysis, or what can be more generally described as integrated pest and crop management (IPCM), as it includes practical aspects of "… plant health, water management, weather, weed density, disease surveillance, plus observation and collection of insect pests and beneficials" (Indonesia National IPM Program Secretariat). Studies suggest that the information contained in the training program could, if property applied, lead to improved farm performance: As demonstrated in Settle et al., pest damage in rice could be contained using an integrated approach relying on biological and physical control while reducing costly pesticide applications (see also the review by Way and Heong). Pingali, Moya, and Velasco concluded that yield increases were possible if improved "second generation" cultivation practices were followed, and emphasized that training programs are particularly important for promoting such practices. Extensive supervised trials in Vietnam confirmed that the technology has the potential to reduce pesticide costs and improve yields, (Pincus).

The FFS approach relies on participatory training methods to convey knowledge to participants so as to make them into "confident pest experts, self-teaching experimenters, and effective trainers of other farmers" (Wiebers). An archetypal FFS entails some 8-12 weeks of hands-on, farmer experimentation and non-formal training during a single crop-growing season. A facilitator (typically government employee, but, in some cases, NGO or specially-trained farmer) leads this village-level program, focusing initially on problem-solving approaches in pest management, but also conveying knowledge pertaining to overall good crop management procedures and practices. Through group interactions, FFS participants sharpen their decision-making abilities, and are empowered by learning leadership, communication, and management skills (van de Fliert).

Farmer field schools were introduced in Indonesia in 1989 for disseminating IPM technology among rice growers. Villages and farmer participants were purposively selected for inclusion in the program. Village accessibility and the presence of active farmer groups served as criteria for FFS locations. The more affluent and better informed farmers in these villages were invited to participate in the field schools (van de Fliert, p.157). In 1994, the National IPM Training Project took over these pilot FFS activities and launched a nationwide FFS effort (with funding from the World Bank) for promoting integrated pest management and improved crop cultivation, mainly for rice, but also including some non-rice crops.

During the expansion phase, villages in which field schools were introduced were still non-randomly selected by Ministry of Agriculture officials, based on certain criteria (e.g., rice-growing, presence of a farmer group, cooperation of village leaders, accessibility of the village's field school to potential participants). Certain criteria were used to select the field school participants as well (e.g., rice farmer, ability to read and write, ability to participate in FFS activities and discussions). This selection was done with assistance from village-level officials and farmer group leaders. The program's strategy was not to train all farmers in the community, but rather to rely on the spread of knowledge through farmer-to-farmer diffusion. This included the formal use of farmer-facilitators, or select farmers identified during FFS who were invited to attend a "training-of-trainers" program towards their becoming FFS facilitators themselves. Furthermore, informal diffusion through farmers' communications among themselves was expected to improve the knowledge within the wider village community which did not participate in the field schools.

Since their inception in Indonesia, farmer field schools have remained separate activities, implemented in addition to the regular agricultural extension activities undertaken nationwide by the Ministry of Agriculture. There are some private sector-led extension efforts, including FFS activities run by NGOs and training activities sponsored by input supply companies, but these are very few and of limited coverage. By 1999, more than 500,000 farmers in tens of thousands of villages had been trained in field schools. Over 20,000 farmers had attended "training-of-trainers".

Data

The data come from a panel survey of Javanese households conducted by the Indonesian Center for Agro-Socioeconomic Research (CASER) in April/May 1991 and again in June 1999.

The baseline sample included randomly selected rice-growing villages that had already been covered by the program, as well as villages that were not yet covered by the program but were in areas where the program was planned to be implemented. All villages were visited in the repeat survey, but our analysis focuses only on those villages that had not yet been exposed to an FFS at the time of the baseline survey in 1991. Five of these villages had not yet been served with an FFS program even by the time of the 1999 survey, and the 52 households from these villages are thus a control group. Of the 268 households from villages where a field school had been implemented between 1991 and 1999, only 112 had actually participated in the training (whether administered by officials or by farmer-trainers) while the remaining 156 households had not attended a program, but had been potentially exposed to some of its effects through informal communications with graduates of the program.[5] Therefore, our data allow us to separately identify the effects of FFS on graduates and exposed farmers.

Another source of variation in program participation comes from the fact that field schools were first introduced in the sample villages at different times between the two surveys. Of the 21 program villages in the sample, 13 were first exposed to field schools within three years after the baseline survey (i.e., by April/May 1993). 5 villages were exposed the following year, and the remaining 3 were exposed after 1995/96. Therefore, our data also allow us to explicitly account for the length of pre- and post-program exposure for graduates and exposed farmers.

The 1991 survey collected information on households' farm operations and characteristics for the 1990/91 wet season and on their household attributes, activities and assets. The 1999 resurvey covered the wet season 1998/99 and included most of the questions of the baseline effort. To the extent possible, interviewers avoided changing the language and format of the original 1991 questions in order to preclude any bias that may arise when later comparing responses from both panels. The 1999 survey, however, collected additional data, such as when the household participated in FFS training, more information about the community, and some retrospective details, such as information on when the community was first exposed to FFS, details pertaining to the training attended by FFS participants and follow-up activities.

Table 1 reports selected descriptive statistics for the three household types. Household size and composition is similar across the three categories. However, consistent with non-random farmer selection, there are significant differences in asset ownership across the groups, with graduates owning more land and farm assets (e.g., sprayers), and being more educated than exposed households. In addition, graduate households are significantly more likely to participate in community activities and work as village officers.

IV. Empirical Specification and Testable Hypotheses

Any empirical analysis assessing the impact of FFS must take account of the special aspects of FFS program implementation. A commonly used approach to measure impact would be to use ordinary least squares (OLS), regressing farm-level outcomes on a variable indicating a farmer's participation in the program and on other variables that could affect farm outcomes. This type of regression is effectively a single difference that compares mean outcomes of program participants to mean outcomes of non-participants in order to measure program impact.

However, in the Indonesian context (as in other places where FFS programs have been established), two problems arise with such single difference comparisons based on cross-sectional data. First, the establishment of FFS programs in communities is determined by several factors, some of which are not observed by researchers and may be correlated with farm-level outcomes. For example, if villages where yields were the highest were selected into the program, cross-sectional comparisons of participants and non-participants may show that field schools are associated with higher yields. This is not really the effect of the program but instead comes from the rules that guide the placement of the program. Second, farmers who are selected to participate in FFS are likely to be different from other farmers, in ways that are unobserved to the researcher. For example, if more motivated farmers were more likely to be selected, comparing graduates to other farmers in an OLS regression would over-estimate the program's impact on farm-level outcomes.

Because of these features – non-random program placement and participant selection – it is well recognized that single difference comparisons of outcome measures (e.g., between participants and non-participants) can give severely biased estimates of impact. A popular approach for addressing this problem is the difference-in-differences or double difference (DD) estimator, obtained by comparing the change in performance before and after the program for a treatment group (i.e., graduates or exposed households) to the change in performance over the same time period for a control group that is unaffected by the program. Since the DD estimator relies on comparing *changes* in outcomes between participants and non-participants, it is not affected by selection biases that arise from time-invariant household or village unobservables (Glewwe and Jacoby).[6]

The empirical specification for measuring FFS program impacts implied by the conceptual framework laid out in Section II is a straightforward modification of the DD estimator. Using the same notation as earlier, the equation to be estimated is obtained by taking the natural log of equation (1) and rearranging:

$$\Delta(\ln Y) = \alpha(T^* - T_0) + \beta D_N (T_1 - T^*) + \mu D_G (T_1 - T^*) + \gamma \Delta X + \delta \Delta Z \tag{2}$$

where $\Delta(\ln Y) = (\ln Y_1 - \ln Y_0)$ and ($T^* = T_1$) for control farmers. Equation (2) can be estimated by regressing the change in log of performance indicator Y between 1990/91 and 1998/99 on a set of variables that measure the length of pre and post-program exposure and a set of other household, village, and district controls.[7]

To see how equation (2) is related to the standard DD estimator, consider a case where all villages are exposed to FFS programs in the same season (i.e., T^* is the same across all graduates and exposed farmers in the sample). In such a case, the length of exposure variables are the same across all exposed and graduate farmers, and therefore equation (2) collapses to a regression of the change in ln(Y) on the exposed and graduate dummies (scaled up by the constant (T_1-T^*)), and the set of other household, village, and district controls. In such a regression, μ and β measure the change in Y over time for graduates and exposed households relative to the change in Y for control households – these are the typical DD estimators of program impact.

Equation (2) modifies the DD estimator in order to account for the fact that FFS programs were introduced at different times across the sample villages. The underlying logic is that at the community level, diffusion of knowledge among individuals takes place over time. The longer the time interval between the time when some community members undertook training and the 1999 survey, the greater the spillover effects on the performance of non-graduates of FFS within the community. Similarly, for field school graduates, the longer the time elapsed since their training, the greater the opportunities to experiment with the new concepts and modes of decision-making, and thus the improvement in their performance is likely to be larger.[8] For these reasons, the length of time elapsed since the FFS was implemented is explicitly incorporated by multiplying both the graduate and exposed participation dummies with the length of post-program exposure.

Estimation of equation (2) in essence compares the performance growth rates of graduates and exposed farmers in the post-program period to the growth rate experienced by control farmers. It can identify parameters α (the pre-program growth rate in Y for all farmers), β (the growth rate of Y for exposed farmers after FFS was implemented in their community), and μ (the growth rate in Y for graduates after they undertook training). Given these parameter estimates, the impact of FFS on graduates and exposed farmers is simply (μ-α) and (β-α), respectively.

Note that because program impact is estimated in first differences, any time-invariant unobservable farmer or village characteristics that may have affected farmer selection or program placement are differenced out of the regression, and therefore do not bias estimates of the program impact parameters.

Another feature of equation (2) is that program impact on graduates and exposed households is estimated by comparing changes in their performance *relative* to changes in the performance of control households. Therefore, any variation in performance due to factors that affect all farmers, such as systemic climate changes, concurrent policy and price changes, nationwide non-FFS extension efforts or other secular trends, is also eliminated. In addition, in order to control for differences in extension efforts across regions at the sub-national level, we include district dummies in the estimation. This implies a 'within-district' estimation (i.e., graduates and exposed households are effectively compared to control households within the same district), and therefore the program impact estimates are not confounded with differences in access to extension efforts or other public services across districts.

In the empirical work that follows, we focus on two performance indicators: yields and pesticide use. As pointed out earlier, some observers have claimed that improvements in these indicators were in fact obtained by the FFS program in Indonesia, although those assertions were not backed by rigorous studies (Kenmore 1997, FAO 2000). Other inputs, such as labor and fertilizers, are not included as performance indicators, as it is not clear *a priori* in which direction their use should change as a result of the program. In the case of yields, both graduates and neighbors are expected to accelerate their rate of improvement in yields, although the improvement for neighbors is not expected to be as high as graduates, who receive intensive training. Pesticides are a special area of focus of the FFS, and IPM principles are highlighted in the curriculum. It is thus expected that pesticide use will be reduced after attending the program.

In order to test these hypotheses, in the case of the yield regressions, we perform the one-sided tests of ($\mu>\alpha$) and ($\beta>\alpha$) against the null hypothesis of no difference in pre- and post-program growth rates. In the case of the pesticide regressions, the signs of the tests are reversed, since the training aims to induce lower pesticide use.

V. Empirical Results

Table 2 reports the means of the yields and pesticide expenditures in the two survey years, for the three groups of farmers. Several observations are pertinent: First, yields have decreased between 1991 and 1999 for the whole sample (and for each group). This is compatible with an overall drop in Java yields of 5% over the same time period, as reflected in national statistics also presented in Table 2.[9] The second observation is that the sample, which was drawn from FFS program areas, has consistently higher yields compared to the Java average, apparently because the FFS program focuses on the relatively more productive, better irrigated rice producing areas. Third, none of the differences in changes over time between the sub-groups in either of the performance indicators are statistically significant.[10] And finally, note that expenditures on pesticides have significantly increased for the sample and within each group. This is consistent with aggregate production figures for the rice pesticide market, which show an increasing trend in production (in terms of both active ingredient weight and value) during the early to mid-nineties (Oudejans).

We turn now to multivariate analyses. Table 3 reports estimates of the pre-program and post-program growth rates of yields and pesticides using equation (2).[11] The estimates of pre-program growth rates reflect the trends reported in the descriptive statistics: on average, during the sample period, yields were declining and pesticide use increasing before the introduction of farmer field schools. Parameter estimates of post-program growth rates in yields and pesticide use suggest that these trends continued even after the introduction of FFS, both for graduates and exposed farmers. The question of interest is whether there was a significant acceleration (deceleration) in growth rates of yields (pesticides) for graduates and exposed farmers, relative to control farmers, after the introduction of field schools. This is tested in hypothesis tests A and B. Both tests find no evidence of impact, as there does not appear to be a significant difference in the trend growth rates in the post-program period for either graduates or exposed farmers. Test C which examines differences between graduates and exposed farmers in the post-program period similarly does not find any significant differences.

Table 4 checks the robustness of these results by estimating a less restrictive specification than equation (2) that allows for the possibility that graduates have a different pre-program growth rate in outcomes compared to exposed and control farmers. Differences in pre-program growth rates might be expected if those selected to participate in field school training were superior performers not only due to fixed traits (e.g., superior managerial ability), but also due to dynamic traits (e.g., faster learners). Therefore, in Table 4 separate pre-program growth rate parameters are estimated for graduates (α_2) versus other farmers (α_1).

The results in Table 4 indicate clearly that the graduates are not randomly selected to participate in FFS training. Test A, which compares the pre-program growth rates in outcomes for graduates with other farmers indicates that graduates had significantly higher yield growth and lower pesticide use growth than other farmers even before any FFS training had taken place. This implies that analyses not accounting for this selection bias (such as cross-section comparisons, or even the difference-in-differences formulation underlying Table 3) may confound the inherent advantage of these select individuals with the program effect.

The main tests of program impact, comparing changes in the growth rates for exposed farmers (Test B) and graduates (Test C) between the pre-program and post-program periods also do not provide evidence that yields of these farmers have increased or pesticide use declined after exposure to the program. These results do not lend support to the contention that the program has been effective.

Table 5 further checks robustness of the basic findings reported in Table 3 by specifying an alternative specification that also accounts for the intensity of FFS activities within a village. The intensity of activity (represented by the number of FFS conducted) within a village is claimed to be an important factor in the extent to which individual farmers apply FFS principles (FAO 1999, p.21). In order to allow for variation in the degree of impact with the intensity of FFS activity, in Table 5, the length of post-program exposure for exposed households is interacted with the number of FFS conducted in the village.[12] The results support the conclusions from Table 3. The tests of program impact (Tests A and B) do not provide evidence of improvements in yield growth and reductions in pesticide use for either graduates or exposed households. No differences in performance seem to have emerged between graduates and exposed farmers after the program (Test C).

The absence of significant impact that can be attributed to the FFS programs gives rise to an obvious question: How can an education effort focusing on matters of direct relevance to farmers' cultivation activities not influence positively their performance? This study cannot provide a direct explanation, but some plausible arguments can be advanced:

(i) Even if graduates gain knowledge in their training that could over time be reflected in improved performance, the change is rather small and cannot be detected in the econometric study which necessarily imposes a specific structural form that is only assumed to reflect reality. The rather impressive gains cited by promoters of the program likely exaggerate program impact because of improper attributions, confusion of selection biases with true program effects, and extrapolation of observations from small non-representative pilot situations and samples to the wider population.

(ii) It is difficult to achieve significant yield gains when there are systemic factors causing yield declines such as decline in soil fertility, increased plant diseases, and climate trends. Thus, the gains that could be achieved through training may have been small to begin with.

(iii) The knowledge gained in the course of FFS training is complex, as "farmers do not master a specific set of contents or 'messages', rather, they master a process of learning that can be applied continuously" (Dilts). The curriculum of an FFS includes agro-ecosystem concepts and decision making principles, that, if conveyed in casual and informal conversations, are not likely to be effectively transmitted. Indeed, a recent study of knowledge diffusion related to FFS activities in the Philippines (Rola, Jamias, and Quizon) found no significant differences in knowledge scores between farmers exposed to FFS graduates and other farmers. If neighbors of graduates do not adopt IPM measures, then the pests from their fields can re-infest the fields of graduates even if the latter apply IPM principles which reduce initially their infestation. This will induce pesticide use even by graduates, and yet the yield may decline due to increased pest resistance.

(iv) During the implementation of the World Bank-financed expansion of the FFS program in Indonesia, there were periods when training activities were afflicted by untimely transfers of funds to the field training organizers resulting in training not being fully synchronized with the rice-growing season calendar, irregular supply of training materials, and irregular availability of meals for participants. There was a relatively large rate of farmer absenteeism in school sessions during part of the period. These issues may indicate that the quality of knowledge achieved by graduates was adversely affected. These implementation problems are not uncommon when pilot projects get scaled-up to the national level.

(v) As the training program got scaled-up to tens of thousands of villages, a large number of trainers had to be inducted into the program. The average quality of trainers and their commitment to bottom-up approaches may have been negatively affected in the move to mass volume.

(vi) Pest management, on which the FFS training in Indonesia placed a major emphasis, is but a small component of the rice farmers' cost structure in normal years. Thus the cost of pesticides is less than 10% of the total production costs. Farmers are not placing as much importance on what is to them minor production impact as they should if the full environmental and health costs of pesticides were to be considered.

VI. Concluding Observations

The empirical results of the preceding section do not provide evidence of significant improvement in economic performance. And, although the analysis has focused on economic benefits, there is no evidence that the expected environmental and health benefits of the program are significant, because the empirical results do not indicate a program effect on pesticide use. The study shows that it is risky to extrapolate the results of small and early pilots, and the non-rigorous analyses that typically accompanied them. The impact of the FFS training can be much smaller than envisaged, when the program is applied on a large scale, rendering the economic, environmental and health benefits much less attractive than what decision makers were expecting.

The discussion of possible reasons for the insignificant program impacts suggests the need to exercise more caution in the design of FFS programs and in deciding on whether to scale them up to the national level. If the impacts on yields and pesticide use are not large, then the viability of the approach depends on maintaining low costs per farmer trained. This is particularly true in the case of rice in Asia, where yields are stagnant or falling due to systemic factors. The current design is expensive, once all overhead costs are accounted for.[13] Furthermore, the economic viability is undermined by the possibility that diffusion through informal communications among farmers is hindered due to the complex nature of the information.

There thus seem to be several implications for improving and reorienting the Farmer Field School initiative so as to improve the likelihood of economic viability:

(i) There is merit in reviewing the curriculum, and focusing the training on the highest priority topics, while simplifying the presentation of the information. There is a balance between presenting the operational implications of scientific research and the articulation of the science base on which these implications are based.[14] The FFS approach may have tilted more than is beneficial into the latter, thus diluting the operational effectiveness of the information.

(ii) The simplification of the program's content will make it not only more effective with respect to improving the performance of graduates, but also increase the likelihood and speed of diffusion of new knowledge among other farmers. Diffusion can also be enhanced (and made more cost-effective) by employing mass-media and other dissemination approaches for key aspects of the knowledge (e.g., safety rules regarding the use of chemicals).

(iii) The narrowing and prioritizing of the curriculum will also shorten the length of the training, and will thus cut the cost of the program – a key source of concerns regarding fiscal sustainability. With a significant reduction of the per-farmer training cost, a much larger number of farmers can be trained directly. When larger groups of farmers within a village are trained, there will be better prospects of collective action in pursuing coordinated pest management, so that cross-farm infestations do not occur. This will make the training more effective in yielding results.

(iv) Simplification of the curriculum, shortening of the training, and increasing the extent of simple decision rules in the training will make the program less dependent on trainer quality and thus more amenable for scaling up.

With changes of the types outlined above, there is less risk and higher prospects of success for large scale FFS programs.

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Econometrics Appendix

A) Basic difference-in-difference (DD) estimation

A commonly used approach to measure the impact of FFS on a farm-level outcome is to regress the outcome on a variable indicating a farmer's participation or exposure to the program and on other variables that could affect farm outcomes. For example, using the notation from section II, log of performance indicator Y for farmer i in village j and time period t is given by

$$\text{(1')} \qquad \ln\left(Y_{ijt}\right) = \alpha + \beta D_{Nijt} + \mu D_{Gijt} + \gamma X_{ijt} + \delta Z_{jt} + \lambda_i + \eta_j + \varepsilon_{ijt},$$

where X and Z are vectors of household and village characteristics that may change over time, and D_G and D_N are graduate and exposed household dummy variables. λ_i and η_j are unobserved determinants of Y that are fixed over time within a household and village respectively, and ε_{ijt} is a residual that represents all time-varying components of the error. In this framework, μ and β measure the direct impact of FFS on graduates and the secondary effects on exposed households, respectively, under the assumption that the graduate and exposed household dummies are orthogonal to the residual.

However, nonrandom program placement across villages and farmer selection into the program are likely to violate the orthogonality assumptions. Nonrandom program placement across villages leads to correlation between D_N and η_j, while nonrandom farmer selection into the program leads to correlation between D_G and λ_i. These correlations imply that μ^{OLS} and β^{OLS} are biased estimates of the true causal effect of FFS on graduates and exposed households.

One solution to the problems of participant selection and program placement bias is to estimate probit models that explain which villages and farmers are selected for participation in FFS, and then use the Heckman lambda approach (Greene, p. 981) to correct for selection bias in equation (1'). However, this model would be only weakly identified in the Indonesia case, as there are no evident instrumental variables that would explain farmer or village participation but would have no direct effect on performance. For example, the criteria typically used for selecting farmers for training in Indonesia – rice farmer with irrigation, owner-operator, educated, belongs to an active farmer group – are also likely to influence farm yields and pesticide use. In the absence of valid instruments, the probit selection correction would be identified only by relying on a strong distributional assumption of joint normality of the error terms in the selection and performance equations in order to identify program impacts; indeed, in the absence of valid instruments, it is impossible to identify program impacts with cross-sectional data without invoking strong distributional assumptions (Heckman and Robb).

Weaker identifying assumptions are required when panel data with observations before and after the program, for treatment and control groups, are available. With two periods of data, household and village level unobservables (λ_i and η_j) can be eliminated by estimating the regression in first difference form. Subtracting *Y* in the first period from the second provides the change in *Y*, conditional on change in program participation:

(2')
$$\Delta(\ln Y_{ijt}) = \alpha + \beta\Delta D_{Nijt} + \mu\Delta D_{Gijt} + \gamma\Delta X_{ijt} + \delta\Delta Z_{jt} + \Delta\varepsilon_{ijt}$$
$$= \alpha + \beta D_{Nijt} + \mu D_{Gijt} + \gamma\Delta X_{ijt} + \delta\Delta Z_{jt} + \Delta\varepsilon_{ijt}$$

where ΔD_G and ΔD_N are dummy variables for graduates and exposed households, respectively (1 if household graduated from or was exposed to FFS between the two survey years, 0 otherwise), while control households are the omitted category.[15] μ (β) measures the change in *Y* over time for graduates (exposed households) relative to the change in *Y* for control households. This difference-in-difference (DD) estimator provides an unbiased estimate of the effect of FFS on graduates and exposed households under the identifying assumption that *change* in Y for graduates, exposed, and control households would have been the same in the absence of the program, even though *levels* of Y in any given season may differ (Moffitt).[16] Notice that the cross-sectional estimator in equation (1') requires that the levels of Y be the same for graduates, exposed, and control households in the absence of the program. The identifying assumption in the first difference estimator is a much weaker condition and will more plausibly hold in the Indonesia context given the methods of program placement and farmer selection outlined in section III.

B) DD estimation, accounting for variation in length of program exposure

The specification in equation (2') can be readily adapted to incorporate the dynamic growth process outlined in the conceptual framework. As in the basic DD estimation, the identifying assumption is that in the absence of the program, farm performance would have changed at the same per period growth rate for controls, exposed and graduate households, and program impact on graduates (exposed) can be measured as the change in the per period growth rate for graduates (exposed) relative to the per period growth rate for controls. The main difference of the text equation (2) with the specification in equation (2') is that now the graduate and exposed household dummies are interacted with the length of program exposure in order to account for the fact that the change in outcomes for households that have been exposed for longer periods may be greater.

Incorporating these considerations (and suppressing subscripts to simplify the exposition) leads to:

(3') $$\Delta(\ln Y) = \alpha\left(T^* - T_0\right) + \beta D_N\left(T_1 - T^*\right) + \mu D_G\left(T_1 - T^*\right) + \gamma\Delta X + \delta\Delta Z + \Delta\varepsilon$$

where T_0 and T_1 refer to the two survey years, and T^* is the year in which graduates underwent training within the village, such that $T_0 < T^* < T_1$.[17] α measures the pre-program exposure growth rate in Y (assumed to be the same for graduates, exposed and control households in 3'). μ and β estimate the growth rates in Y for graduates and exposed household after they were exposed to FFS. Since the growth rate of Y for control households is measured by α, program impact on graduates and exposed households is simply (μ-α) and (β-α), respectively. It can be shown that if the program were introduced at an identical time in all villages (in which case T^* is identical across observations), then the specification in (3') reduces to the standard difference-in-difference formulation, with the parameters scaled up by the fixed factor (T_1-T^*).

Table 1. Descriptive statistics, by farmer category

	Farmer Category		
	Controls	***Exposed***	***Graduates***
I. Household Composition (1990/91)			
Household size	4.48	4.74	4.69
	(1.51)	(1.92)	(1.52)
% adult males	0.29	0.25	0.32
	(0.11)	(0.17)	(0.18)
% adult females	0.25	0.29	0.29
	(0.15)	(0.15)	(0.15)
II. Household Assets (1990/91)			
Land area owned (ha)	0.61	0.71	1.24
	(0.52)	(1.12)	(2.73)
% sawah area rainfed	0.58	0.27	0.13
	(0.50)	(0.43)	(0.33)
No. of 4 wheel and hand tractors	0.06	0.05	0.06
	(0.42)	(0.25)	(0.28)
No. of sprayers	0.46	0.46	0.63
	(0.61)	(0.59)	(0.83)
No. of years of education of household head	5.71	5.15	5.77
	(2.61)	(2.71)	(2.70)
III. Participation in Community Activities (1998/99)			
% who work as a village officer	0.13	0.10	0.29
	(0.34)	(0.34)	(0.45)
% in farmer groups	0.81	0.55	0.82
	(0.40)	(0.50)	(0.38)
% in cooperatives	0.19	0.16	0.24
	(0.40)	(0.37)	(0.43)
IV. Number of households	52	156	112

Notes. – Standard deviations reported in parentheses.

Table 2. Sample averages of outcome indicators, by farmer category

	1990/91	1998/99	Change between 1990/91 and 1998/99	
			absolute diff.	% change
Yield (kg/ha)				
Control farmers	6620.2	5594.5	-1025.8	-15%
	(1153.6)	(1451.5)	(1478.0)	
Exposed farmers	5757.3	5326.3	-431.1	-7%
	(1369.7)	(1567.1)	(1783.1)	
FFS graduates	6116.3	5472.0	-644.3	-11%
	(1194.5)	(1376.0)	(1579.0)	
All farmers	6023.2	5420.9	-602.7	-10%
	(1310.1)	(1482.9)	(1675.1)	
Java Average (from National statistics)	5104.0	4849.0	-255.0	-5%
Pesticide ('000s of 1998 Rp/ha)				
Control farmers	65.6	176.5	110.9	169%
	(66.7)	(265.7)	(275.1)	
Exposed farmers	92.8	202.0	109.1	118%
	(91.6)	(236.7)	(233.8)	
FFS graduates	111.3	201.9	90.6	81%
	(108.6)	(184.3)	(177.3)	
All farmers	94.9	197.8	102.9	109%
	(95.6)	(224.5)	(222.9)	

Notes. – Standard deviations reported in parentheses. CPI used to inflate 1990/91 pesticide expenditures to 1998/99 Rupiah.

Table 3. Impact of FFS on Rice Yields and Pesticide Use

Parameters			Yield	Pesticides
Pre-program growth rates				
All Farmers (α)			-0.028	0.251
			(2.85)*	(5.29)*
Post-program growth rates				
Exposed farmers (β)			-0.033	0.365
			(3.28)*	(4.84)*
Graduates, after own training (μ)			-0.036	0.362
			(3.22)*	(4.32)*
R^2			*0.34*	*0.37*
Hypotheses Tests (p-values):				
	Yield	**Pesticides**	**Prob(T > t)**	**Prob(T < t)**
A:	$\beta > \alpha$	$\beta < \alpha$	0.854	0.994
B:	$\mu > \alpha$	$\mu < \alpha$	0.902	0.984
C:	$\mu > \beta$	$\mu < \beta$	0.810	0.445

Notes: Dependent variables are the differences in logs of household-level yields and pesticide use on the main rice plot between the 1998/99 and 1990/91 rice growing seasons. Both regressions control for household and village characteristics and district dummies. These sets of variables are jointly significant at 5%. Absolute value of robust t-stats, corrected for clustering within villages, reported in parentheses. * significant at 5%. Sample size in both regressions is 320.

Table 4. Robustness check: allowing for differences in pre-program growth rates

Parameters		Yield	Pesticides
Pre-program growth rates			
Exposed and Control Farmers (α_1)		-0.006	0.141
		(0.58)	(2.59)*
Graduates (α_2)		0.005	0.084
		(0.52)	(1.22)
Post-program growth rates			
Exposed farmers (β)		-0.012	0.260
		(1.16)	(2.88)*
Graduates, after own training (μ)		-0.017	0.266
		(1.45)	(2.79)*
R^2		*0.35*	*0.37*
Hypotheses Tests (p-values):			
Yield	**Pesticides**	**Prob(T > t)**	**Prob(T < t)**
A: $\alpha_2 > \alpha_1$	A: $\alpha_2 < \alpha_1$	0.000*	0.006*
B: $\beta > \alpha_1$	B: $\beta < \alpha_1$	0.895	0.996
C: $\mu > \alpha_2$	C: $\mu < \alpha_2$	0.999	0.998

Notes: Dependent variables are the differences in logs of household-level yields and pesticide use on the main rice plot between the 1998/99 and 1990/91 rice growing seasons. Both regressions control for household and village characteristics and district dummies. These sets of variables are jointly significant at 5%. Absolute value of robust t-stats, corrected for clustering within villages, reported in parentheses. * significant at 5%. Sample size in both regressions is 320.

Table 5. Robustness check: allowing for differences in number of FFS

Parameters	Yield	Pesticides
Pre-program growth rates		
All Farmers (α)	0.006	-0.120
	(1.06)	(2.07)*
Post-program growth rates		
Exposed farmers *X* No. of FFS (β)	-0.001	0.007
	(0.78)	(0.71)
Graduates, after own training (μ)	-0.005	0.011
	(1.08)	(0.42)
R^2	*0.34*	*0.36*

Hypotheses Tests (p-values):

	Yield	Pesticides	Prob(T > t)	Prob(T < t)
A:	$\beta > \alpha$	$\beta < \alpha$	0.908	0.983
B:	$\mu > \alpha$	$\mu < \alpha$	0.945	0.973
C:	$\mu > \beta$	$\mu < \beta$	0.853	0.568

Notes: Dependent variables are the differences in logs of household-level yields and pesticide use on the main rice plot between the 1998/99 and 1990/91 rice growing seasons. Both regressions control for household and village characteristics and district dummies. These sets of variables are jointly significant at 5%. Absolute value of robust t-stats, corrected for clustering within villages, reported in parentheses. * significant at 5%. Sample size in both regressions is 320.

Endnotes

[1] It is important to note that in this study, the analysis is limited to measurable economic outcomes – i.e., yields and pesticide use which are two performance indicators frequently highlighted by promoters of the program. The analysis does not deal with other effects that field school training is claimed to bring about such as improved health, enhanced political activism, a sense of pride and empowerment, and build-up of social capital (Dilts; Roling and van de Fliert).

[2] Modeling performance as a dynamic process is compatible with the way sociologists and economists perceive the process of innovation uptake. For example, Rogers describes the process of innovation adoption as "the mental process an individual passes from first hearing about an innovation to final adoption." Similarly, the learning-by-doing literature suggests an evolution of performance over time (Feder, Just, and Zilberman, p. 259). The dynamic performance model is also compatible with Bayesian information-updating processes, describing the way perceptions regarding technology parameters change over time with improved information (Feder and Umali, p. 216).

[3] While the conceptual framework is applicable more broadly to a range of farm-level outcomes, in the empirical work we focus specifically on rice yields and pesticide use on the main rice plot, as the improvements in rice yields and reduction in pesticide use have been claimed to be important economic achievements of the Indonesian FFS program (Kenmore 1997; FAO 2000).

[4] Note that even if the "without program" trend in yields is negative (due to external factors affecting the whole farm sector such as systemic climate changes or declining soil fertility), the program's impact is expected to be positive in that it enables participants (and their neighbors) to slow the rate of decline compared to control farmers. Similarly, even if the overall "without program" trend is one of increased pesticide use (due to factors such as increased pest resistance), the program is expected to slow the growth of pesticide use by graduates and exposed farmers compared to control farmers.

[5] In the empirical analysis, we do not differentiate between graduates trained by officials versus farmer-trainers because previous studies have shown that the quality of training provided by the two sources was similar (van de Fliert, Pontius, and Roling).

[6] For a more detailed discussion of the econometric issues in our context, please see the Appendix. In brief, the econometric problem caused by non-random program placement and participant selection is that if selection is based

on unobservable household and regional traits that also affect the outcomes of interest, then the participation variables in a cross-sectional regression of outcomes on participation are correlated with the error term. Therefore the estimates of parameters associated with these variables are biased. The DD or first-difference estimator eliminates this source of endogeneity as long as the unobservables are time-invariant.

[7] Household controls include plot size and irrigation, education of household head, and household size and composition. Village-level variables control for availability of different types of infrastructure and services, including cooperatives, schools, observers, production input kiosks, irrigation, road quality, and distance from district center. District-level differences are controlled for by including district dummies.

[8] While it may be argued that the passage of time could cause "erosion" of knowledge, this seems to be less likely in the case of knowledge gained through FFS, as the program is said to have engendered communal activities and continuous focus on FFS themes through vibrant alumni associations (FAO 1999, pp. 19-21).

[9] A secular decline in Asian irrigated rice yields over time has been observed by researchers in recent years (e.g., Cassman, Olk, and Dobermann), and is attributed to declines in soil fertility, increased resistance to pesticides, and increased vulnerability to diseases such as sheath blight and blast. In addition, global warming has been associated with a decline of Java rice yields (Amien et. al.). Finally, the 1998/99 rice season in Indonesia was mildly affected by La Nina. While these factors negatively impact on all farmers, it is expected that farmers trained in FFS (and those who learn from them) will have experienced smaller declines due to their better knowledge of pest and crop management.

[10] The only exception is the difference in change in yields, between exposed and control group households. Yields for the control group declined by significantly more than the decline observed for the exposed households.

[11] Dependent variables in the regressions are the differences in log(yield) and log(pesticide use per hectare) on the main rice plot between the main 1998/99 and 1990/91 rice growing seasons.

[12] Post-program exposure for graduates is not interacted with number of FFS since it is most likely that the main effect on their performance is related to the intensive training they have received and the diffusion through informal conversation is of second order magnitude.

[13] A conservative estimate of the cost per farmer trained is approximately $49 per farmer (Quizon, Feder, and Murgai).

[14] Pingali, Hossain, and Gerpacio (pp. 267-271) discuss the trade-off between costly and intensive IPM training and cheaper dissemination of simple science-based messages.

[15] Note that the change in participation variables are the same as the dummy variables defined in equation (1') since none of the households in the sample had graduated or been exposed to FFS in the first survey year. That is, $\Delta D_G = D_G$ and $\Delta D_N = D_N$.

[16] This first difference estimator is often termed a *difference-in-differences* estimator because it computes the program impact by comparing the change in outcomes for participants to the change in outcomes for non-participants.

[17] Interacting the participation dummies with the length of exposure variables implies that the three variables attached to parameters α, β, and μ are specified as follows for the three groups: (a) for all control households, the variables take the values (T_1-T_0), 0 and 0; (b) for exposed households, the variables are (T^*-T_0), (T_1-T^*), and 0, where T^* varies across exposed farmers depending on the season when the first FFS was held in the village; (c) for graduates, the variables are (T^*-T_0), 0, and (T_1-T^*) where T^* varies across graduates depending on when the graduate attended a FFS, respectively.

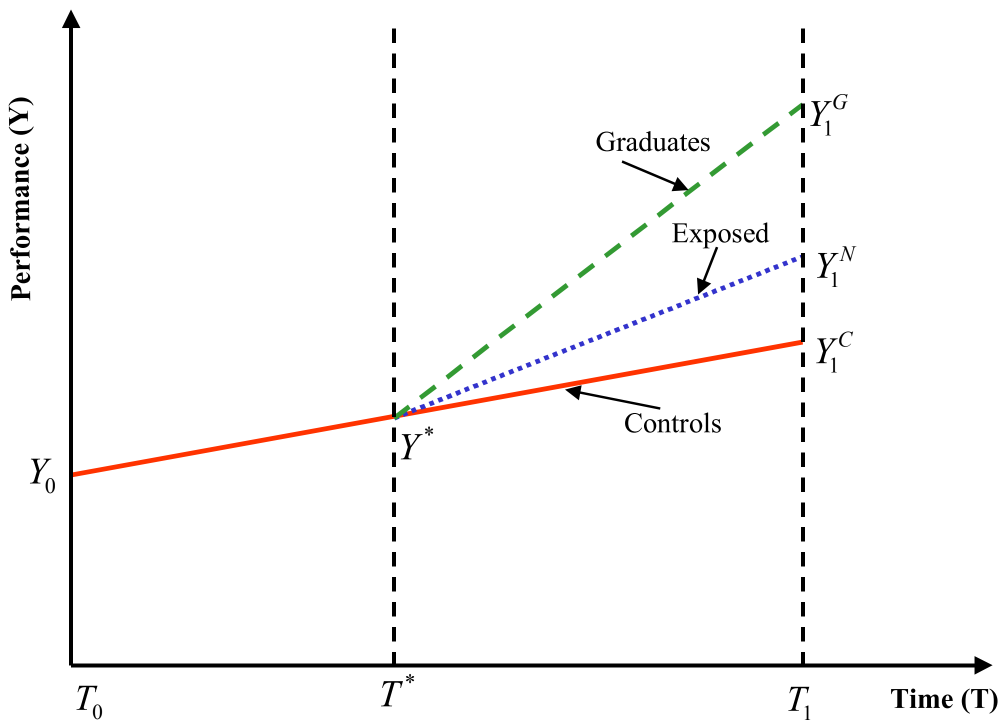

Figure 1. The Evolution of Performance over Time