Stochastic Transfers, Risky Investment and Incomes: Evidence from an Income Guarantee Program in Thailand *

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Abstract

From 2009 to 2011, the Thai government implemented an income guarantee program for rice, tapioca and maize farmers. Essentially, this program added a non-negative but stochastic component to the incomes of registered farmers. We evaluate the impact of the program on risk attitudes and investment behavior of small-scale rice farmers in relatively poor North-eastern Thailand. To control for self-selection into the scheme, we use propensity score matching. We find that that participation in the program significantly makes farmers less risk-averse, induces higher investments and boosts incomes. Medium-term effects are stronger than short-term effects.

JEL Codes: D13, H25, I38, Q12
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1 Introduction

In both developed and developing economies, agricultural subsidy and insurance programs are implemented for various reasons: as shields against the exposure to market fluctuations, as a hedge against agricultural risks, as political devices to garner popularity with the rural electorate, or as instruments of social assistance (Mahul and Stutley, 2010). While traditional policies of supply control and price support programs aim at keeping prices high and stable, income support programs do not target the stochastics of market prices itself but rather offer contingent payments to farmers that are meant to off-set losses should prices fall too low (relative to some legislated level). Such insurance-type interventions provide direct income support to the farmers and help to smoothen disposable incomes across contingencies.

By altering the stochastic distribution of incomes, government-run programs may impact on farmers' risk-management via income and insurance effects (Hennessy, 1998, Wright and Hewitt, 1994). They potentially affect investment decisions, risk attitudes or consumption choices and, thus, may have long-lasting consequences for farmers' wealth and well-being (Alderman and Yemtsov, 2014). Such effects are important in developing economies where low incomes, lack of assets, credit constraints and high risk aversion are major obstacles to investment by poor farm households (Rosenzweig and Binswanger, 1993, Karlan et al., 2014, Hill and Viceisza, 2012, Cole et al., 2013, Cai et al., 2009).

In this paper we investigate a subsidy program, called the "Farmer Income Guarantee Program" (FIGP), that was active in Thailand between 2009 and 2011 (only), mainly for rice and cassava farmers. The program made transfers to registered farmers that depended on crop prices but that were independent of farmers' actual yields, revenues or farming activities (see Secion 2 for a description). Thus, the FIGP differed from most crop-insurance programs in that payments were unrelated to farming outcomes. In essence, the program offered to farmers a first-order stochastic dominance shift in

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the distribution of their incomes.[1]

Using four waves of a comprehensive household panel in rural Thailand (see Section 4 for details), we study the effects of this program on risk preferences and investment behaviour of signed-up farmers. Our motivation for this study is fourfold: First, while there is substantial research on the effect of, say, crop insurance on the composition of farmers' agricultural portfolio (Wright and Hewitt, 1994), only little is known on the repercussions of income support and revenue stabilization programs on risk attitudes and investment behaviour in general.

Second, if they decrease farmers' risk aversion and encourage investment, income support and stabilization programs could help to kick-start economic development in poor rural areas. It, thus, appears worthwhile to study whether programs have longer-lasting impact rather than only one-time, instantaneous effects. While the Thai FIGP was a one-time program (it in fact only ran for one year), we still observe its effects on participants after the program expired, indicating the long-run potential of such programs.

Third, while in most agricultural insurance programs indemnifiable losses are endogenous to the actions taken by the insured farmer, the FIGP does not involve any such moral hazard issues:[2] its payouts are independent of the insureds' activities (except for registration). This peculiarity allows us to study a "pure" transfer program with income effects only (no distortions in relative prices).

Fourth, the theoretical literature on decision making under uncertainty is concerned with changes in behaviour when the underlying random variables undergo stochastic or deterministic transformations (see Eeckhoudt and Gollier (2000) for a survey). First-order stochastic dominance shift in random variables are the most elementary concept in that literature. Such changes rarely arise in reality – but the FIGP is a

[1] The FIGP is a peculiar mixture between a yield-based crop insurance (it uses some predetermined, average yield to calculate indemnities – but ignores actual yields) and a crop revenue insurance (its payouts arise irrespectively of the selling of the crop). See Mahul and Stutley (2010) for a taxonomy of agricultural insurance.

[2] A great number of studies shows that moral hazard indeed prevails with respect to agricultural insurance, at least in developed economies. See, e.g., the survey in Smith and Glauber (2012).

case in point: it adds stochastic, exogenous but non-negative payments to farmers' disposable income. Our study of the behavioral effects of the FIGP, thus, puts conceptual theoretical considerations of choice under risk to an empirical test (which they, by and large, pass).

In our study we want to identify the impact of the FIGP on its members, as compared to the non-members. To circumvent self-selection issues that might arise when simply comparing registered and non-registered farmers we conducted a propensity score matching. We then applied a difference-in-differences (DiD) approach to the matched sample that avoids potential biases arising from differences in time-invariant characteristics between registered and non-registered farmers. Hence, we can interpret the effects we find as causal.

In particular, we find that, compared to farmers who did not register for the program, Thai rice farmers in the FIGP ...

- became less risk-averse: they felt more prepared to take up risks;
- increased their risky investment: they increased the size of land used for rice cultivation, spent more on agriculture expenditures, and took up higher loans related to agricultural investments or expenses;
- experienced higher growth in total household income both in the short and in the medium-run, indicating that the program had impact beyond its cancellation.

In short, the FIGP generated positive and potentially lasting positive effects on those who signed up.

The rest of this paper is organized as follows: Section 2 describes the Thai FIGP. Section 3 provides a conceptual framework for our hypotheses. We present data sources in Section 4. The matching procedure, the difference-in-difference estimation as well as baseline characteristics of our sample are described in Section 5. Section 6 presents and discusses our results. Section 7 briefly concludes.

2 The Farmer Income Guarantee Program (FIGP)

General structure. The Farmer Income Guarantee Program (FIGP) was launched by the Thai Government and was active (only) from 2009 till 2011.[3] The program replaced a previously existing rice pledging scheme. In different ways, both programs aimed to protect farmers against crop price shocks. With the old pledging scheme the government bought the rice crops from farmers at previously fixed prices and re-sold the produce in the market.[4] In the new program, the government would not buy any crops from farmers. Rather, for a certain guarantee period it would ex ante fix an "insured price" per ton of rice (of various types). Roughly following market prices it would then announce "benchmark prices" every two weeks during the guarantee period. If these benchmark prices were lower than the insured price, farmers who had registered with the program were eligible to receive as a payment the price differential per ton of rice they had "insured". The insured amount of rice was calculated by applying a notional expected yield per acre (also fixed by government) to the area which farmers had registered to the program. Farmers could only claim compensation once per season, but were free to choose the point in time. Farmers decided themselves whether, when and to whom to sell their crops; this way the program attracted also small farmers who mainly produce for own consumption. Payouts would be based on the ex-ante determined notional yields, irrespective of actual yields.

[3] A quick overview of the programme is provided in World Bank (2010). For a complete survey, see Isvilanonda (2010). Officially the program was active from mid 2009 to mid of 2011. However, only the first year of the FIGP was carried out as announced: during the following harvesting period payments were no longer reliable as many delays and regional inconsistencies occurred. In the wake of a government change in Thailand, the program was replaced by a rice price-pledging scheme in 2011.

[4] Initially, the pledging scheme aimed to supply illiquid farmers with low-interest loans early in the harvesting season to enable them to delay sales of their produce until the rice price rose later. The government essentially lend money to farmers, taking their rice as a collateral. Farmers paid an annual net interest of 3 percent for their loan. If they did not redeem their pledge after five months, the rice would go to the government. As conditions of this loan usually were better than market conditions, farmers mostly would not redeem their rice (http://www.aessweb.com/pdf-files/141-148.pdf).

Hence, despite its name, the program is not exactly a full income guarantee, as it only pays a transfer in case of low market prices. It does not compensate for other crop-related income shocks due to, e.g., adverse weather conditions or crop pest damage.

Registration. Eligible for the FIGP were (rice) farmers who own farm land – or tenants of such land, but only if the owner had agreed and did not register himself. Registration was costless but required some paperwork and personal attendance of the owner at registration.[5] The agriculture administrations of districts maintain registries that collect data on land ownership of farm land (plot sizes) for different crops. For each crop a notional yield per rai (= 1,600 m^2) was officially fixed, roughly reflecting the expected average yield.

Before planting a crop, land owners could register their land with the program. Registration had to be done at the Bank for Agriculture and Agricultural Cooperatives (BAAC), which also administered the payouts. Authorities recorded the *anticipated* quantity of produce based on the size of the registered land, capping it by a fixed maximum per household, depending on the variety (e.g., maximally 14 tons of jasmine rice, 16 tons of glutinous varieties, and 25 tons of other paddy per household).

Payouts. Before the planting season, the government fixed an "insured" price per ton for each crop for the whole harvest period. In 2010, the guarantee price ranged from 9,500 Baht ($\sim$ 300 US-$, as of August 2010) per ton of glutinous rice to 15,300 Baht (482 US-$) per ton of jasmine rice. During the harvest period, the Thai Ministry of Commerce announced so-called "benchmark prices" bi-weekly. These benchmark prices would roughly follow market prices. When claiming payments, a registered household was paid, via the BAAC, the difference between the insured price and the current benchmark price at this point in time per ton of crop registered with the program. If the benchmark price exceeds the insured price, farmers would

[5] Our data and anecdotal evidence suggests that this requirement was not always enforced consistently. In some (but not all) places, family members of the holders of land titles were able to register.

not get anything. The choice of the point in time when farmers could cash in on the BAAC payment was – within broad limits – upon to the farmer (for rice in Isarn: between November and March). In particular, the farmer could delay claiming payments if he expected the benchmark price to fall in the future.[6]

As shown in Figure 1, for most of the guarantee period the insured rice price was above the benchmark prices, which roughly tracked market prices and varied quite substantially over time.

Recall that the FIGP based payments on *registered* tons of crops, not on actual harvests (or even seeded farmland). Farmers could keep their produce for own consumption as well as choose to sell it.

3 Theoretical Analysis

3.1 The decision problem

The effects of the FIGP can theoretically be captured in the following stylized decision problem: a farmer has to decide how much to invest in rice cultivation. "Investments", denoted by x, may represent expenditures for seeds or fertilizer, the size of his land used to grow rice, or the time or labor devoted to growing rice. Investment means foregoing immediate consumption. It generates yields, $Y(x)$ where the yield function Y is strictly increasing and concave in investments x and satisfies $Y(0) = 0$. Presupposing price-taking behavior, the harvest has monetary value $p \cdot Y(x)$, where p denotes the crop price – which is stochastic ex ante. If the farmer is an expected-utility maximizer, his investment problem is given by:

$$\max_x V_0(x) := \int u\left(p \cdot Y(x) - x\right) dF(p).$$

[6]Hence, the program also entailed some speculative features, which we will henceforth ignore, however.

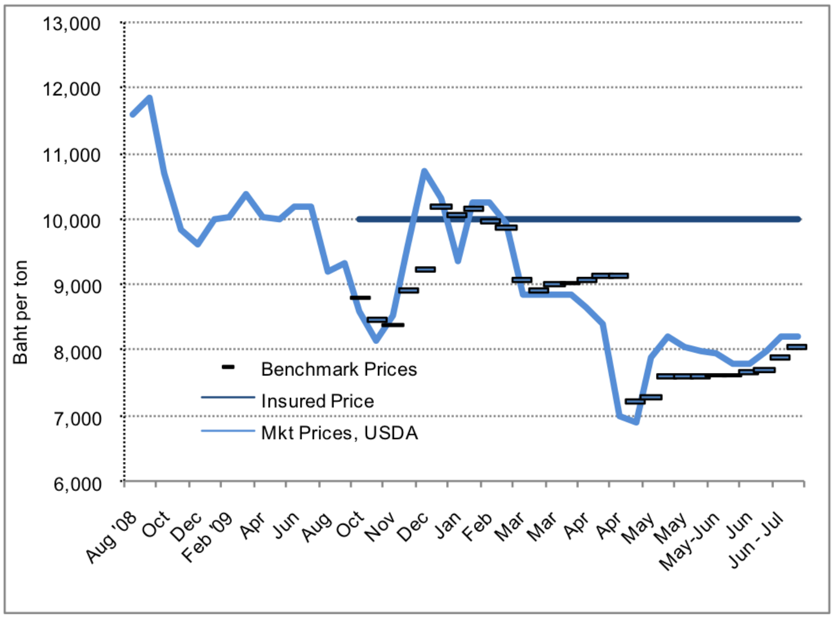


Figure 1: Insured, benchmark and actual rice prices, 2008-2010. Source: World Bank (2010, p.35).

Here, $u = u(c)$ is a strictly increasing (vNM) utility index over consumption or final wealth (denoted by c) and F is the distribution of the crop price. In line with the literature (Moschini and Hennessy, 2001, Mahul and Stutley, 2010), we assume that farmers are risk averse: $u''(c) < 0$ for all c. Denote the optimal investment in the above problem by x_0 and the attending utility level by $V_0^* = V_0(x_0)$.

We denote payouts from the FIGP for a (registered) farmer by I (= "indemnity"). Denoting the insured price by $\bar{p}$ and the notional yield, calculated on the basis of registered land, by $\bar{y}$, the regulations of FIGP outlined above stipulate the indemnity I as follows:

$$I = \max\{p - \bar{p}, 0\} \cdot \bar{y}. \tag{1}$$

We shall henceforth suppress in notation the dependence of I on notional yield and the fixed insured price; both are exogenous (once registered) and invariant. The design features of (1) that matter for our predictions are:

- Payouts only vary with the price but neither with the yield Y nor with its driver x. Formally, $I = I(p)$.
- Payouts are non-negative: $I(p) \geq 0$ for all p.
- Payouts weakly decrease in price: $I'(p) \leq 0$.

For a farmer registered in the FIGP the investment problem is rendered into:

$$\max_x V_1(x) := \int u\left(p \cdot Y(x) - x + I(p)\right) dF(p).$$

Denote the optimal solution to this problem by x_1 and the attending utility level by $V_1^* = V_1(x_1)$.

3.2 Theoretical predictions

Formally, for any given x, the FIGP induces a first-order stochastic dominance (FSD) shift in the probability distribution of the farmer's income. Hence, under the premise that farmers are expected-utility maximizers, a couple of predictions can be derived from the theory of decisions under risk (the proofs of (2) to (4) are relegated to the Appendix).

Registration. Since the FIGP and the program doled out non-negative payments under all circumstances and guaranteed strictly positive payments in some states of the world, *not* to register for the program is at odds with having not-satiated preferences: it violates the first-order stochastic dominance rule (see, e.g., Levy, 2006, pp. 55f.).

Well-being. In the presence of the FIGP, farmers are better off. This is an implication of the monotonicity of u and the FSD feature of the FIPG:

$$V_1^* > V_0^*. \tag{2}$$

Investment decisions. Behavioral responses with respect to FSD shifts in the distribution of payoffs from risky choices have been characterized by Ormiston (1992). While the necessary conditions for unambiguous comparative statics are restrictive, decreasing absolute risk aversion (DARA; $-u''(c)/u'(c)$ is decreasing in c) is a sufficient condition in the current framework such that

$$x_1 > x_0. \tag{3}$$

I.e., farmers covered by FIGP should invest more in rice: increase the land devoted to rice cultivation, devote higher expenditure to rice farming etc. If such investment cannot be financed from own wealth, we would expect the farmer to take additional loans.

Incomes. Since yield is assumed to increase monotonically in investment x, farmers who registered for the FIGP will experience higher expected gross incomes and consumption levels:

$$Ec_1 > Ec_0. \tag{4}$$

Wealth. If, in a dynamic framework, investments x at least partly go to finance assets that do not depreciate immediately then participation in the FIGP would result in a higher stock of physical assets. The effect on financial assets is, however, not clear: if savings are a normal good, then farmers will save more when they experience a FSD shift in their incomes. However, there might be offsetting effects from a lower necessity to build financial buffers through precautionary saving. The same applies to a buffer goods like stored crops or livestock.

Risk aversion. It is well-known that DARA is *not* a sufficient condition for the farmer to become less risk-averse when experiencing a *stochastic* increase in wealth (as with FIGP). Rather, risk vulnerability plays a crucial role. The dependence on the crop price, which also enters multiplicatively, further complicates things, as now *relative* risk aversion and its monotonicity properties matter. For CRRA utilities, Franke et al. (2011, Lemma 2) show that (in our terms) if the non-negative risk generated by the FIGP is "small" or "large" then derived relative risk aversion, defined for V_D^* (with $D = 0, I$) is increasing and concave in wealth, but may decrease in the intermediate range. Hence, it is unclear – and largely an empirical question – whether farmers who invest and earn more end up with higher or lower risk-aversion.

Portfolio effects. Increasing production activities and investment for rice potentially has repercussions (unmodelled so far) on other components of farmers' income portfolios: crowding out cultivation of other crops or livestock; a stronger focus on agriculture might reduce off-farm activities or wage employment, etc. To the extent that these alternative activities are risk-free, the FIGP effects on them are captured in the model by the opportunity costs. If these activities have risky returns themselves, the FIGP constitutes a (possibly correlated) background risk. The comparative statics of background risks vary considerably with the economic setting and depend, in general, on risk attitudes of higher order than just risk aversion and its monotonicity (see, e.g., Schlee and Gollier, 2006, Franke et al., 2011). We therefore refrain from any theoretical predictions (which, due to missing data on higher-order risk-attitudes, could not be verified in our sample anyway).

4 Data

General description. For the empirical analysis of the effects of the FIGP, we use four waves from an extensive panel of rural households conducted in the relatively poor Northeast of Thailand. The survey started in 2007, when a three-stage cluster sampling strategy was applied to select 2200 households that are representative for the rural population.[7] We are only interested in households that cultivate rice, which leaves us with a sample of 1580 households.[8] The panel provides, in each of its waves, detailed information on the characteristics, income sources, land holdings, wealth, assets, investments, shocks, expectations and risk attitudes of the sampled households.

Two of the four waves in our study had been conducted before the farmer income guarantee program became active. The third and fourth waves were conducted when the FIGP was active for one year and two years after it had been suspended, respectively. See Table 1 for a time line on data coverage and policy status.

Take-up and its determinants. About 60 percent of the farm households in our sample registered for FIGP in 2009, while the remaining farmers did not. This rate of

[7] The surveys were carried out in the project "Impact of shocks on the vulnerability to poverty – consequences for the development of emerging Southeast Asian economies" (DFG FOR 756, German Research Foundation). For a detailed description of the sampling strategy, see Hardeweg et al. (2013).

[8] The FIGP guarantees, in an identical fashion as for rice, prices for cassava and corn. We focus on rice since the sampled region is dominantly used for rice cultivation; our data set contains only very few cassava or corn farmers.

Table 1: Panel Waves and Policy Status

Panel Wave	Data Period Covered	Policy Status
Wave 1 (W1)	May 2006 - April 2007	Pledging scheme
Wave 2 (W2)	May 2007 - April 2008	Pledging scheme
-	May 2008 - April 2009	Pledging scheme
Wave 3 (W3)	May 2009 - April 2010	FIGP
-	May 2010 - April 2011	FIGP
-	May 2011 - April 2012	Pledging scheme
Wave 4 (W4)	May 2012 - April 2013	Pledging scheme

non-take-up is substantial and, due to the give-away structure of the FIGP, requires further discussion.

First, the theoretical prediction of full take-up requires that farmers are correctly informed about the program's existence and registration formalities. Moreover, transaction costs must be negligible. Since registration involved contact to the next branch of the BAAC (which as well offers government subsidized loans for farmers) and some paper work, distance to BAAC as well as reading and writing skills, therefore, might be important for registration decisions. Also eligibility for the program was officially bound to land tenure status, although implementation strictness of this rule seemed to depend on local authorities.

To better understand motivational determinants of take-up, we asked all non-registered eligible farmers in an open interview question of the 2013 household survey to recall their reasons for not registering. The answers are categorized in Table 2. Apart from being excluded for registration due to land tenure status (2%), non-registered farmers stated that they forgot to register (7%), that registration was too much effort (19%), that they lacked (correct) information (40%), or that they had political reasons (8%). About a quarter of the respondents did not remember or did not reveal their reasons for not registering. The answers suggest that it is likely that registered and non-registered farmers differ in unobserved characteristics like overall

Table 2: Self-reported Reasons for Not Registering for FIGP

Reason	Frequency	Percent
Forgot to register	42	6.6
Does not have a land title or owner registered himself	15	2.4
No or wrong information about the program	252	39.6
Not satisfied with government policies in previous years or does not trust government	52	8.2
Registration is too much effort or too complicated	120	18.8
Cannot remember or did not reveal reason for not registering	156	24.5
Total	637	100

motivation, proneness to procrastination or hesitance. In terms of accessing correct information, social networks or membership in organizations might play a role. Some of these characteristics might not only have influenced registration decisions but also have an impact on risk attitude and investment behavior.

As the descriptive statistics in panel *Raw Sample* of Table 3 show, the group averages of registered and non-registered farmers differ markedly in many characteristics that might be relevant for investment behavior and risk-taking.[9] Registered households are slightly more likely to be headed by a male. On average, they used more land (and, thus, can be expected to benefit more from the program), own more agriculture-related assets and cultivated more rice in both pre-registration periods available to us. This leads to higher incomes from crop cultivation in both periods but does not seem to consistently translate into a higher total income. Furthermore, registered farmer on average lost, and feared to lose, more income due to agricultural shocks. They hold more live stocks, savings and stored crops, which could be seen as measures of informal insurance. They, furthermore, hold more loans related to agricultural investment or production expenses and more loans in total.

[9] Imbens and Rubin (2015) discuss rules-of-thumb and identify that standardized differences should optimally be about 0.25 or smaller. As recommended by Rosenbaum and Rubin (1985) we assess the covariate differences across treated and control units by looking at standardized differences rather than t-statistics as the latter depend on sample size. I.e., if the sample is large even small differences might show statistical significance even though they are substantively small.

Although registered and unregistered farmers differ in many characteristics they have almost identical pre-treatment risk attitude: 4.20 and 4.23, respectively, on a scale from 0 ("try to avoid risks") to 10 ("feel fully prepared to take risks"). This balance suggests that the potentially higher readiness to take risks due to higher wealth in the registered group might have led them to undertake more investments projects and take higher loans – which in turn reduces the readiness to take (even) more risks.

In summary, the groups of registered and non-registered households are clearly not identical: who self-select into the program do not only differ in registration status but also in other observed (and potentially also in unobserved) characteristics that might mutually influence registration decision and investment behavior.

5 Method

5.1 General

Our objective is to estimate causal (treatment) effects of the subsidy program on risk attitudes, investment behavior and household income. As the ideal of comparing identical farmers with and without registration is not feasible and the groups of farmers in the FIGP and those who chose not to register differ markedly we resort to a propensity score matching to obtain unbiased estimates, as suggested by Rosenbaum and Rubin (1983) and surveyed in Caliendo and Kopeinig (2008). With a matching algorithm we produce a suitable counterfactual group that does not significantly differ from the group of registered farmers in relevant observable characteristics before the program launch. To preclude potential biases due to time-invariant unobserved characteristics we then pursue a difference-in-difference estimation strategy with the matched sample.

Following Heckman et al. (1997) and Smith and Todd (2005), we estimate the treatment effect on the treated by an average nearest k-neighbor matching estimator on differenced outcomes, defined by

$$\widehat{ATT} = \frac{1}{n_1} \cdot \sum_{i \in I_1} \left(\Delta Y_{1i} - \frac{1}{k} \sum_{j \in C_0(p_i)}^{k} \Delta Y_{0j} \right) . \tag{5}$$

Here, I_1 and I_0 denote the treatment ($D = 1$) and the non-treatment ($D = 0$) group, i.e., the sets of registered and non-registered farmers, n_1 and n_0 their sizes, and p_i the propensity score, i.e., the probability of any i to receive treatment. $C_0(p_1) \subset I_0$ is the set of comparison units (non-registered farmers), matched to treatment unit i, and k is the pre-specified number of units to be matched to each individual $i \in I_1$. The neighborhood $C_0(p_i)$ for each registered farmer i is defined as the set of nearest neighbors,

$$C_0(p_i) = \left\{ j \in I_0 : |p_i - p_j| = \min_{k \in I_0} \{|p_i - p_k|\} \right\} .$$

In (5), the difference in outcomes $\Delta Y_{1i} = Y_{1i,t} - Y_{0i,t-1}$ before and after the intervention of each registered farmer $i \in I_1$ is matched with a weighted average of differences in outcomes $\Delta Y_{0j} = Y_{0j,t} - Y_{0j,t-1}$ of neighboring non-registered farmers. The unknown propensity score p_1 has to be estimated based on farmers' selected characteristics: $p_1 = p(X_1) = Prob(D = 1|X_1)$.

The quality of the matching depends on the specification of the propensity score. In particular, two conditions have to be fulfilled to credibly estimate an average treatment effect. First, we assume conditional independence, given by

$$E(Y_{0,t} - Y_{0,t-1} \,| p(X), D = 1) = E(Y_{0,t} - Y_{0,t-1} \,| p(X), D = 0) \ . \tag{6}$$

In other words, conditional on characteristics X, the differenced outcomes of selected non-registered farmers should have the same distribution as registered farmers would have experienced had they not been registered.

Second, overlap is required, given by

$$0 < p(X) < 1, \tag{7}$$

for all X. I.e., farmers with identical characteristics have positive probability of both being registered or not-registered. This assumption ensures that a match can be found for all treated farmers.

The remainder of this section will explain the procedure of selecting a balanced sample by obtaining the propensity scores, trimming and matching the sample and assessing whether Assumptions (6) and (7) hold in our data.

5.2 Propensity score algorithm

For Assumption (6) on conditional independence, it is important to identify and include all characteristics X in the propensity score specification that could be correlated with the registration decision as well as potential non-treatment outcomes. Waves W1 and W2 of the data panel provide us with a large set of such potential characteristics. As these waves were sampled, respectively, about 28 and 16 months , respectively, before the registration to the FIGP started (see Table 1), we are confident that respondents in W1 and W2 were not affected by the program or its anticipation. Therefore, we use this rich data set to calculate the propensity score for registration to the program.

We run two different specifications of the propensity score model. In Specification 1, we linearly include variables X for all relevant characteristics. In addition, we select Y_{t-1} and (where available) Y_{t-2}, i.e., the first- and second-order lags of outcome variables in levels, to be linearly included in the model.[10]

Specification 2 allows for non-linear relationships and covariate interactions. We run the algorithm proposed by Imbens (2014) to select the variables to be included in the propensity score model.[11]

[10] See Table 3 for a full list of variables. Heckman et al. (1997) find that matching estimators perform best (in terms of smaller bias) the richer the set of conditioning variables.

[11] Given the pool of variables X, Y_{t-1} and Y_{t-2}, the algorithm applies likelihood ratio tests for all first and all possible second-order terms (quadratic and interaction) in multiple loops. It selects those variables for inclusion which improve the fit of the model by more than a certain threshold.

Table 3: Covariate balance before and after matching

Covariate	Raw Sample						Trimmed Sample		Matched Sample			
	Control		Treatment						Spec.1, NN(10)		Spec.2, NN(10)	
	Mean	SD	Mean	SD	$SDiff_p$	Diff (t-stats)	$SDiff_p$	Diff (t-stats)	$SDiff_p$	Diff (t-stats)	$SDiff_p$	Diff (t-stats)
W1, (Cube root) Income from rice cultivation	4.63	7.35	6.18	7.91	0.20	1.5***	0.10	0.7	0.00	0.0	0.01	0.1
W1, (Cube root) Income from life-stock products	0.78	5.76	0.52	6.39	−0.04	−0.3	0.04	0.3	0.02	0.1	−0.02	−0.1
W1, (Cube root) Income from off-farm wage employment	7.14	7.08	6.53	6.80	−0.09	−0.6*	−0.04	−0.3	−0.03	−0.2	*-0.14*	−0.9
W1, (Cube root) Income from off-farm self-employment	2.65	5.55	2.07	5.10	−0.11	−0.6**	−0.02	−0.1	−0.01	−0.1	*-0.07*	−0.4
W1, (Cube root) Income from public transfers	1.93	2.83	1.58	2.59	−0.13	−0.3**	−0.04	−0.1	0.03	0.1	0.00	0.0
W1, (Cube root) Total household income	16.12	7.23	15.96	6.75	−0.02	−0.2	0.03	0.2	−0.03	−0.2	−0.05	−0.3
W1, Expenditures related to rice cultivation	90.72	99.70	117.74	108.37	**0.26**	27.0***	0.04	3.9	0.01	0.6	0.02	2.1
W1, (Log) Land used for rice cultivation (Rai)	0.25	1.10	0.76	0.98	**0.49**	0.5***	0.18	0.2**	0.01	0.0	−0.04	0.0
W1, Land used for rice cultivation (Share)	0.73	0.33	0.81	0.27	**0.27**	0.1***	0.06	0.0	0.00	0.0	*-0.07*	0.0
W2, (Cube root) Income from rice cultivation	5.93	7.80	7.46	8.34	0.19	1.5***	0.09	0.7	0.02	0.1	−0.03	−0.2
W2, (Cube root) Income from life-stock products	1.10	1.97	1.50	2.22	0.19	0.4***	0.12	0.3	0.05	0.1	0.01	0.0
W2, (Cube root) Income from off-farm wage employment	4.93	6.19	5.46	6.49	0.08	0.5	0.04	0.2	0.04	0.3	0.02	0.1
W2, (Cube root) Income from off-farm self-employment	2.81	5.91	2.63	5.81	−0.03	−0.2	0.03	0.2	0.01	0.1	0.01	0.1
W2, (Cube root) Income from public transfers	2.57	2.89	2.51	3.08	−0.02	−0.1	0.09	0.3	0.00	0.0	0.04	0.1
W2, (Cube root) Total household income	15.14	6.86	15.66	7.25	0.07	0.5	0.04	0.2	0.05	0.3	0.02	0.1
W2, HH nucleus size	4.06	1.62	4.25	1.62	0.12	0.2**	0.02	0.0	0.02	0.0	−0.02	0.0
W2, # of agricult. related shocks in ref. period	1.00	1.10	0.99	1.06	−0.01	0.0	−0.05	0.0	−0.03	0.0	−0.04	0.0
W2, Average severity of agricult. related shocks in ref. period	1.36	1.25	1.43	1.28	0.06	0.1	−0.06	−0.1	−0.05	−0.1	−0.03	0.0
W2, (Log) Income loss based on agricult. related shocks in ref. period	0.86	4.15	1.52	4.44	0.15	0.7***	0.02	0.1	−0.02	−0.1	−0.01	0.0
W2, Risk attitude	4.23	2.85	4.20	2.78	−0.01	0.0	0.01	0.0	−0.01	0.0	0.00	0.0
W2, If you won 100,000 Baht, how much would you invest?	2635.34	1444.03	2614.91	1462.75	−0.01	−20.4	−0.04	−58.5	−0.03	−42.2	−0.03	−45.6
W2, Average anticipated severity of income loss due to agricult. risks	0.60	0.47	0.76	0.52	**0.32**	0.2***	0.12	0.1	0.02	0.0	0.05	0.0
W2, Average anticipated severity of asset loss due to agricult. risks	0.33	0.45	0.46	0.58	**0.26**	0.1***	0.05	0.0	0.03	0.0	0.00	0.0
W2, # of agricult. related risks anticipated for future 5 years	8.35	6.36	9.39	6.38	0.16	1.0***	0.10	0.6	0.04	0.2	0.04	0.2
W2, How many days you need to get 60,000 Baht if you needed them?	13.02	16.06	11.21	14.06	−0.12	−1.8**	−0.01	−0.1	0.02	0.2	−0.02	−0.3
W2, (Log) Used land (area)	0.74	0.80	1.15	0.70	**0.54**	0.4***	0.18	0.1**	0.01	0.0	0.01	0.0
W2, (Log) Used land (value)	10.12	1.00	10.47	0.82	**0.38**	0.3***	0.15	0.1*	0.00	0.0	0.03	0.0
W2, (Log) Stored crops (kg)	5.22	2.93	5.85	2.58	0.23	0.6***	0.07	0.2	0.00	0.0	*-0.11*	−0.3
W2, (Log) Life stock (value)	4.40	3.78	5.38	3.39	**0.27**	1.0***	0.06	0.2	0.05	0.2	0.04	0.1
W2, (Log) HH debt (value)	5.24	4.33	5.94	4.17	0.16	0.7***	−0.02	−0.1	*0.06*	0.2	−0.04	−0.2
W2, (Log) Travel time to BAAC	3.12	0.56	3.08	0.56	−0.09	−0.0*	0.00	0.0	−0.01	0.0	−0.04	0.0
W2, (Log) HH savings (value)	3.45	3.52	4.08	3.11	0.19	0.6***	0.10	0.3	−0.01	0.0	−0.02	−0.1
W2, (Log) Agricultural assets (value)	4.30	3.55	5.39	2.91	**0.34**	1.1***	0.14	0.5*	0.02	0.1	0.01	0.0
W2, (Log) Loans related to agricultural investment	1.88	4.76	2.87	4.91	0.20	1.0***	−0.01	0.0	0.02	0.1	−0.01	0.0
W2, (Log) Expenditures related to rice cultivation	4.99	2.69	5.94	2.14	**0.39**	0.9***	0.12	0.3	−0.03	−0.1	−0.03	−0.1
W2, Age o HH head	55.14	13.36	54.72	12.51	−0.03	−0.4	0.00	0.0	0.01	0.2	0.04	0.5
W2, Gender of HH head	0.71	0.46	0.79	0.41	0.19	0.1***	0.12	0.1	−0.01	0.0	*0.06*	0.0
W2, HH is member of political organization	0.15	0.35	0.20	0.40	0.14	0.1***	0.03	0.0	−0.05	0.0	*-0.08*	0.0
W2, HH head has completed primary education	0.89	0.31	0.89	0.32	−0.02	0.0	−0.04	0.0	−0.04	0.0	0.01	0.0
W2, Owned land (share of total used land)	0.77	0.38	0.76	0.36	−0.03	0.0	−0.01	0.0	0.01	0.0	0.03	0.0
W2, HH owns rice plot	0.65	0.48	0.67	0.47	0.04	0.0	−0.03	0.0	−0.02	0.0	*-0.06*	0.0
W2, (Log) Revenue from crop production	−1.86	1.93	−1.41	2.71	0.19	0.4***	0.05	0.1	0.03	0.1	0.03	0.1
W2, (Log) Land used for rice cultivation (Rai)	0.23	1.10	0.79	0.97	**0.54**	0.6***	0.21	0.2***	−0.01	0.0	−0.03	0.0
W2, Land used for rice cultivation (Share)	0.72	0.33	0.78	0.28	0.21	0.1***	0.12	0.0	−0.03	0.0	−0.04	0.0
W2, (Log) Seeds reserved for next planting period (kg)	3.56	2.59	4.37	2.53	**0.31**	0.8***	0.14	0.3*	−0.02	−0.1	−0.04	−0.1
W2, # children of age <6 years in HH	0.42	0.65	0.41	0.62	−0.02	0.0	−0.05	0.0	0.02	0.0	0.01	0.0

Notes: $SDiff_p$ is the standardized difference, i.e. the difference in means or proportions divided by pooled standard deviation. Formula taken from Rosenbaum and Rubin (1985).

Table 4: Propensity score model statistics

Raw Sample	# 1st order terms	# 2nd order terms	LL	df	Chi^2	$p > Chi^2$	pseudo R^2	AIC	BIC
Spec. 1	46	0	-776	46	209.02	0.00000	0.11873	1.258	-7498
Spec. 2	42	212	-449	254	878.57	0.00000	0.49465	1.066	-6763

The model fit statistics of the two specifications for the raw sample are presented in Table 4. In Specification 1, all 46 variables listed in Table 3 are included. In Specification 2 the algorithm selected 42 variables to be included linearly and 212 second-order terms. The value of the log-likelihood function, the pseudo-R^2 and Chi^2 are much higher in Specification 2 than in Specification 1. However, none of these indicators takes into account that the degrees of freedom are very different. The information criteria, which adjust for degrees of freedom, reveal that AIC prefers Specification 2 while BIC prefers Specification 1. As we are more interested in an overall correctly predicted score than in correct estimates for individual predictors, we choose the specification preferred by AIC at this stage.

5.3 Trimming

The majority of the household characteristics have modest standardized differences. Some of the covariates, however, show standardized mean differences greater than 0.25 in absolute value, including some of the lagged outcome variables and other characteristics, which might directly influence our post-treatment outcomes.[12] Regression analysis relies heavily on extrapolation and will be sensitive to the choice of specification and outliers in the presence of substantial pre-matching differences. To achieve more robust estimates we trim the sample before matching using a trimming

[12] As a rule-of-thumb, differences greater than 0.25 indicate substantial differences across groups.

parameter of 0.1 as recommended in Crump et al. (2009).[13] We drop observations from the sample with assigned propensity scores greater than 0.9 or smaller than 0.1. This means discarding 204 observations from the control group and 437 observations from the treatment group. As shown in Table 3, the covariate balance is much improved in the trimmed sample. Though some moderate biases still persist for a few variables across groups, the remaining bias is more balanced with all standardized differences below 0.25 in absolute value. Of course, trimming implies that the results of our analysis are only valid for the limited sample. Matching based on the trimmed sample, however, is more likely to lead to more credible and robust estimates.

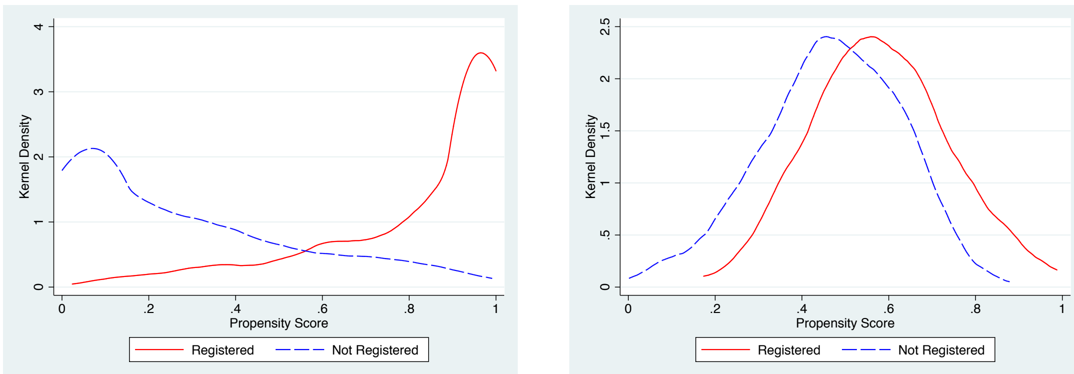
Based on the trimmed sample, we calculate the final propensity score specifications in Specifications 1 and 2. For the latter, the algorithm includes 19 first- and 26 second-order terms now. In both specifications the log-likelihood is improved while the degrees of freedom are the same or substantially smaller, respectively.

5.4 Overlap

Before carrying out the actual matching we verify the validity of Assumptions (6) and (7). We assess overlap graphically by studying at the kernel density functions of the propensity score distributions by groups. As shown in Figure 2, the two distributions greatly overlap, indicating that Assumption (7) holds. However, a small share of the registered farmers at the upper end of the distribution does not have a very close counterfactual. Limiting the sample to the region of common support, i.e., to the range of propensity scores where the distribution of both groups overlap, does not essentially change our results.

[13] The simulations reported in Crump et al. (2009) suggest that in many settings the choice of a trimming parameter $\alpha = 0.1$ is feasible. Trimming in the context of propensity score matching was first recommended in Heckman et al. (1997).

Figure 2: Kernel density of propensity score before and after trimming



(a) Raw Sample

(b) Trimmed Sample

5.5 Matching quality and conditional independence

In the actual matching, as described in Section 5, we match on $k = 1$, 5 and 10 neighbors. Table 5 summarizes in which of the matched samples the balance in the pre-treatment covariates has improved most. The indicators suggest that matching on ten neighbors performs best to reduce the bias in the pre-treatment covariates both in Specifications 1 and 2. For these two matched samples we additionally report the covariate balance in detail in Table 3. For Specification 1, matching has successfully removed the bias: according to their t-statistics, none of the covariates significantly differs across groups. All standardized differences are below 0.06 with a mean of 0.02, indicating that the sample is well-balanced. For Specification 2, the matching performance is almost as good as in Specification 1. The treatment group differs from the control group in terms of primary education of the household head; moreover, they have slightly lower total income and revenues from crop production. The overall mean of standardized differences across covariates is 0.034, indicating a reasonably -balanced sample. In addition, Specification 2 balances a number of interaction and quadratic terms (which are not shown here). Hence, we will report estimates based on both specifications.

Table 5: Propensity score model statistics

Trimmed Sample	pseudo R^2	Chi^2	$p > Chi^2$	Mean of SDiff
Unmatched	0.030	27.9	0.984	6.7
Spec. 1, PSM NN(1)	0.031	30.3	0.964	4.6
Spec. 1, PSM NN(5)	0.008	7.5	1.000	2.7
Spec. 1, PSM NN(10)	0.006	6.0	1.000	2.2
Spec. 2, PSM NN(1)	0.048	47.4	0.415	5.3
Spec. 2, PSM NN(5)	0.024	23.9	0.997	3.8
Spec. 2, PSM NN(10)	0.018	18.2	1.000	3.3

Although Assumption (6) of conditional independence cannot be truly tested, the balance of the pre-treatment outcome variables in the matched sample indicates whether it is reasonable to assume conditional independence in our sample. For all W2 pre-treatment outcome variables the standardized differences are very small, suggesting unconfoundedness in this setting.

6 Results and discussion

Table 6 reports the DiD estimates of average treatment effects for registered versus non-registered households over time. Both panels take the pre-treatment wave W2 as their base year. In Panel A, short-term DiD estimates are presented, using W3 as the follow-up period. Panel B, using W4, presents medium-term effects. We will focus on the results for the propensity score Specification 1 and later discuss where they substantially differ from the estimates based on Specification 2.

Table 6: Difference-in-difference estimates for FIGP impacts: Short- and medium-term effects on risk attitude, incomes and investment related measures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	△ Risk attitude	△ (Log) Agri. loans	△ (Log) Agri. assets (value)	△ (Log) Land used for rice cultivation (area)	△ Land used for rice cultivation (share)	△ (Log) Expenditure for rice cultivation	△ Income from rice cultivation	△ Income from lifestock products	△ Income from off-farm wage employment	△ Income from off-farm self-employment	△ Income, total	△ Income from public transfers (incl. FIGP)
Panel A: Short-term effects, 2010 (FIGP active for one year) - 2008 (Pre-treatment)												
Spec.1, PSM NN(10)	0.507**	0.758**	-0.238	0.078	0.026	0.496***	192.346	10.238***	-10.024	-126.973	353.236	44.540**
	(0.22)	(0.31)	(0.20)	(0.05)	(0.02)	(0.09)	(127.09)	(3.82)	(155.68)	(122.54)	(376.57)	(21.77)
Spec.2, PSM NN(10)	0.531*	0.726	-0.123	0.078	0.015	0.525***	305.162**	10.025**	-20.467	-421.779**	207.251	51.522*
	(0.28)	(0.50)	(0.19)	(0.05)	(0.02)	(0.11)	(119.12)	(4.07)	(157.44)	(191.74)	(433.69)	(27.04)
Panel B: Medium-term effects, 2013 (FIGP already cancelled for two years) - 2008 (Pre-treatment)												
Spec.1, PSM NN(10)	0.302	0.595*	0.477**	0.596***	0.146***	0.245***	522.762***	6.108	1296.610**	-111.653	2577.344***	-8.593
	(0.23)	(0.34)	(0.24)	(0.08)	(0.02)	(0.06)	(171.60)	(5.95)	(505.83)	(121.94)	(747.10)	(17.67)
Spec.2, PSM NN(10)	0.242	1.062**	0.333	0.607***	0.146***	0.282***	591.836***	2.253	1323.582***	-178.198	2193.490***	-1.451
	(0.31)	(0.46)	(0.26)	(0.09)	(0.03)	(0.09)	(172.70)	(5.35)	(471.03)	(141.88)	(742.02)	(20.36)
Panel C: Baseline means of dependent variables (2008)												
Matched sample from Spec. 1, PSM NN(10)												
Registered	4.23	2.41	4.99	0.62	0.80	5.62	1065.9	22.3	808.0	437.9	4594.5	101.9
Non-registered	4.26	2.32	4.93	0.62	0.80	5.68	993.9	21.2	713.7	380.1	4312.0	95.6
Matched sample from Spec. 2, PSM NN(10)												
Registered	4.24	2.43	4.98	0.62	0.80	5.62	1064.9	22.2	805.7	437.9	4640.5	102.7
Non-registered	4.24	2.45	4.95	0.64	0.81	5.68	1099.4	22.3	736.8	372.0	4453.1	88.2

Notes: Heteroscedasticity-consistent standard errors adjusted for clustering at the village level in parentheses. Income and expenditures are given in PPP$ (constant, 2005). *Risk Attitude* is determined by asking the respondent on a scale between 0 and 10 whether he usually tries to avoid risk or feels fully prepared for taking risks. *Agri. loans* sum up those loans a farmer took up for investments or expenditures related to agricultural production. *Agri. assets* sum up those assets a farmer uses in the agricultural production process. *Land used for rice cultivation (share)* is the area of land used for rice cultivation as a share of total used land. *Expenditure for rice cultivation* include expenditure on fertilizer, pesticides, seeds, labor, tractor rental, and other input factors related to rice cultivation. *Income from rice cultivation* is the sum of income obtained from the cultivation of subsidized crops, i.e. several varieties of rice crops. *Income, total* includes income from rice cultivation and public transfers (incl. FIGP payments) as well as all other household income. *Income from public transfers* includes payments from FIGP as well as income form other government social assistance programs like the universal pension scheme, scholarships, food aid etc.

6.1 Short-term impacts of FIGP

Risk aversion and investment. The program triggered a ceteris paribus reduction in risk-aversion in the short-term (Column 1). In addition, we see a substantial difference in the percentage changes of rice cultivation expenditures, consisting of expenditure on fertilizer, pesticides, seeds, labor, tractor rental, and other rice cultivation inputs (Column 6). In line with the theoretical predictions in Section 3, this might reflect increased risk-taking (materialized by short-term investments); complementarily, it might indicate a portfolio shift towards the subsidized crop. There is no sign, however, that the share of land used for rice cultivation has risen (Column 5).

Related to investments in more durable agricultural production factors, we do not find evidence that registered farmers experienced significantly higher asset growth yet: their change in agricultural assets or land holding during the first year of the program does not significantly differ from the non-registered group (Columns 3 and 4). Further, we only find weak evidence for a higher growth rate in loans related to agricultural production expenses and investment in the group of registered households (see Column 2, this result is not significant across all specifications).

Incomes. The significance of the short-term impacts on income varies across specifications and should, therefore, be interpreted as suggestive evidence only. In the set of estimates which is based on the matched sample of Specification 1 we do not see any significant income effect yet, except for an upward shift in income from life-stock products. Further, we find weak evidence for a higher increase in income from rice cultivation, and a larger decline of income from off-farm self-employment in Specification 2. The actual difference over time in income from public transfers (including FIGP payments) is higher for the registered group of farmers but seems to play a limited role in terms of volume compared to total baseline income (Column 9 and Panel C, Column 11).[14] Overall, the estimates for the difference in total household

[14] This is driven by three effects. First, the average benchmark price set by the government was around 9,000 Baht per ton of rice during the major harvesting period from October till December

incomes are positive, yet not significantly so.

6.2 Medium-term impacts of FIGP

Risk aversion and investment. Comparing the differences across groups between 2013 and base year 2008 (Panel B) reveals that, over the five-year period, the growth rates for registered farmers of agriculture-related loans and assets as well as of expenditures and land used for rice cultivation are substantially higher. This is in line with the theoretical predictions in Section 3.

Compared to the short-run estimates in Panel A, differences in risk attitudes have ebbed off (Panel B, Column 1). In conjunction with the moderate short-run investment effects (see above), this may indicate that changes in risk attitude precede changes in investment behaviour. The remaining (non-significant) difference in risk attitudes might be due to the higher overall wealth in the treatment group.

Incomes. In line with the theoretical predictions in (4), FIGP participants experienced stronger medium-term increase in household incomes than members of the control group (Column 11). While the difference in transfer incomes is now negligible and insignificant (Column 12),[15] the estimated DiD in rice-related income is substantial and significant two years after the program was ended, partly explaining the shift in total income (Column 7).

These results indicate that the positive income effects of the FIGP extend beyond the active phase of the program. The strong difference in investment and income

2009 (World Bank, 2010). As the insured price was 10,000 Baht, farmers could on average only reap a meagre compensation payment of 1,000 Baht (approx. 56 PPP-$ (constant, 2005)) per registered ton of produce (on average, farmers harvested approximately four tons of produce). Second, the benchmark price was higher than the insured price from mid-December 2009 till the end of March 2010. Farmers who did not file their claim before mid of December, hence, did not receive any compensation before the end of the surveyed period. Third, even farmers who had staked their claims early reported that payments were often delayed and might not have arrived by the time of the interview in April 2010.

[15]Naturally, the difference across groups in public transfers between 2008 and 2013 is negligible as the program was no longer active in 2013.

effects might be explained by the change in risk attitudes in the short-run and the subsequent take-up of loans, as evident from the medium-run estimates. In the medium-run, the program seems to have triggered a sustainable upward shift in household incomes.

We cannot rule out that agricultural assets and rice-growing land were increased at the cost of other production factors, unrelated to rice cultivation. The substantial differences between the size of the estimates of crop income and total income, however, suggests that the opposite might be true. It seems reasonable to assume that households have not only increased their inputs for cultivating subsidized crops but also increased inputs or investments in other income generating activities resulting in shifts in other income categories. Our estimates suggest, for instance, a strong increase in off-farm wage income (Column 9) that explains about half of the difference in total income. An interpretation consistent with this finding would be that farmers may have invested in machines or hired seasonal wage labor to free some of the household members to seek more lucrative off-farm employment opportunities.[16] Then, the FIGP might have provided the necessary push in income and risk attitude for farm households to expand their portfolio of income generating activities outside of agriculture.

7 Conclusion

Understanding how subsidy and income support programs influence farmers' investment behavior is important for well-informed agricultural policy making and, potentially, for the economic development of rural areas. The analysis is often complicated by the fact that payments from such programs are state-contingent and correlated with the risks that farmers face. The overall effect of such programs on the riskiness of farmers' incomes is, thus, not clear *a priori*. We investigate the Thai Farmer

[16] A recent study by Birthal et al. (2015) on Indian small-scale farmers lends to such an interpretation. While 40 percent of the farmers in that study dislike farming as a profession because of low profits, high risk, and lack of social status they neverthelles continue with farming due to a lack of opportunities outside agriculture.

Income Guarantee Program (FIGP), a subsidy program for rice farmers that was active from 2009 till 2011. As a rare feature, this program implied a FSD shift in farmers' incomes.

Empirically analyzing such a voluntary program is plagued by potential selection issues: registration for the program is not random and the sample is highly unbalanced in relevant covariates between registered and non-registered farmers. We applied a propensity score matching combined with difference-in-difference estimators to balance the sample and to estimate the genuine impact of the program on investment measures and related indicators.

We find that the predictions of expected-utility theory on the effects of FSD shifts in risky incomes bear out quite well. However, positive effect seem to need some time to materialize. For the short run, we find that farmers became less risk-averse while the program is active and that the program leads to increases in total sum of agriculture-related loans. Importantly, the income support progam affected farming and investment behavior resulting in a shift in household incomes that lasts beyond the active phase of the program. This shift appears not only to result from higher investments and an expansion in the the cultivation of the subsidized crop but also from additional engagements in off-farm employment activities. For future research, this suggests to analyse the effects of income support programs in a portfolio approach.

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Appendix: Proofs for Section 3

Proof of (2): Almost trivially,

$$V_0^* = V_0(x_0) < V_1(x_0) \leq V_1(x_1) = V_1^*.$$

The first inequality comes from the FSD improvement through I, conditional on x. The second (weak) inequality is by the maximum property of x_1.

Proof of (3): Denote by

$$c_0 = pY(x_0) - x_0 \quad \text{and} \quad c_1 = pY(x_1) - x_1 + I(p).$$

the final wealth levels with and without the FIGP. The strict concavity of u implies that the first-order conditions (FOC)

$$\int (pY_x(x_D) - 1) \cdot u'(c_D) dF(p) \quad = \quad 0$$

$(D = 0, 1)$ are necessary and sufficient for optimal investments x_0 and x_1. Due to the strict concavity of u, we get that $x_0 < x_1$ if, and only if, the LHS of the FOC for $D = 1$ is positive when evaluated at x_0.

Evaluating the LHS of the FOC for $D = 1$ at x_0 and expanding by $u'(c_0)$ under the integral, we obtain:

$$\int (pY_x(x_0) - 1) \cdot u'(pY(x_0) - x_0 + I(p)) dF = \int (pY_x(x_0) - 1) \cdot u'(c_0) \cdot \psi(p) dF$$

with

$$\psi(p) := \frac{u'(pY(x_0) - x_0 + I(p))}{u'(pY(x_0) - x_0)}.$$

By Chebyshev's Algebraic Inequality, if ψ is strictly increasing [strictly decreasing] in p, then

$$\int (pY_x(x_0) - 1) \cdot u'(c_0) \cdot \psi(p) dF > \; [<] \int (pY_x(x_0) - 1) \cdot u'(c_0) dF \cdot \int \psi(p) dF = 0,$$

where the final equality is due to the FOC-property of x_0 for $D = 0$. Denoting by $c_+ := pY(x_0) - x_0 + I(p)$, verify that

$$\psi'(p) = \frac{1}{u'(c_0)^2} \cdot (u'(c_0) u''(c_+)(Y(x_0) + I') - u'(c_+) u''(c_0) Y(x_0))$$

is positive [negative] for all p if and only if

$$-\frac{u''(c_+)}{u'(c_+)} \cdot \frac{Y(x_0)+I'}{Y(x_0)} < [>] - \frac{u''(c_0)}{u'(c_0)}.$$

Since $c_+ > c_0$, $u''(c) < 0$, and $I' \leq 0$ for all p, DARA ensures that the $<$-relation holds above. Hence, $x_0 < x_1$.

Proof of (4): We will show that expected *gross* incomes (i.e., $E(pY - x)$) already are higher under FIGP. Adding, for FIGP, non-negative payouts I reinforces this effect at the expected consumption level.

From the concavity of Y in x and (3) we get that, given p,

$$p \cdot (Y(x_1) - Y(x_0)) - (x_1 - x_0) \geq (pY_x(x_1) - 1) \cdot (x_1 - x_0).$$

By risk-aversion, the FOC for x_1,

$$E\left[(pY_x(x_1) - 1)\, u'(pY(x_1) - x_1 + I(p))\right] = 0,$$

implies that $E\,(pY_x(x_1) - 1)$ is strictly positive. Hence the claim.