

Impact Evaluation of Land and Property Rights Interventions (PhD) Randomized Controlled Trials (RCTs)

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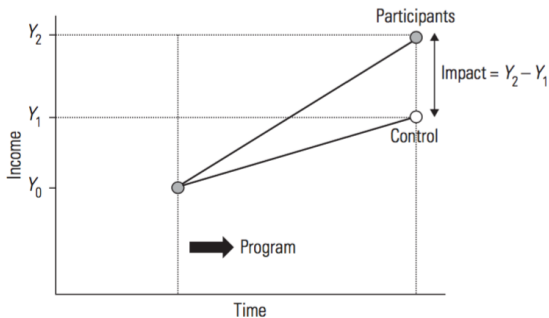
Reference

- Khandker, S. R., Koolwal, G. B., and Samad, H. A. (2010). Handbook on Impact Evaluation: Quantitative Methods and Practices. World Bank. Chapter 3.

Setting the Counterfactual

- Randomized Controlled Trials (RCTs), initially practiced in natural science, ensure that outcomes of the control group really do capture the counterfactual for a treatment group

Figure 3.1 The Ideal Experiment with an Equivalent Control Group



RCTs - Statistical Design

- In practice, making the control group similar to the treatment group is difficult
- Should be a two-stage process:
 - 1 Stage 1: select potential participants randomly from the relevant population
 - The Sample should be representative of the population with certain sampling error
 - The **external validity** of the RCT is must be ensured at this stage
 - 2 Stage 2: Individuals in the sample are randomly assigned to treatment and comparison groups
 - This stage ensures the **internal validity** of the RCT, i.e., subsequent observed changes in outcome measured are due to the program instead of other confounding factors

Calculating Treatment Effects

- Consider the classic problem of measuring treatment effects: let $T_i = 1$ if subject i is treated and 0 otherwise, $Y_i(1)$ and $Y_i(0)$ be the outcomes with and without treatment respectively
- $Y_i = [T_i * Y_i(1) + (1 - T_i) * Y_i(0)] \implies ATE = E[(Y_i(1) - Y_i(0))]$
- I.e., ATE is the difference in outcomes of the treated and nontreated groups
 - Key assumption: everyone in the population has an equally likely chance of being treated
- We however observe $E[(Y_i(1) | T_i = 1)]$ and $E[(Y_i(0) | T_i = 0)]$
- With nonrandom targeting, $E[(Y_i(1))] \neq E[(Y_i(1) | T_i = 1)]$ and $E[(Y_i(0))] \neq E[(Y_i(0) | T_i = 0)]$

Calculating Treatment Effects

- Typically therefore, alternative treatment effects are observed in the form of $TOT = E[Y_i(1) - Y_i(0) | T_i = 1]$
- TOT reflects the average gains for participants, conditional on these participants receiving the program
- If T is nonrandom, a simple difference between treatment and control areas,
$$D = E[Y_i(1) | T_i = 1] - E[Y_i(0) | T_i = 0] \neq TOT$$
- $TOT - D = E[Y_i(0) | T_i = 1] - E[Y_i(0) | T_i = 0] = B$, the bias we discussed in the previous lecture

Randomized Experiments

Theory

$$TOT = E[Y_i(1) - Y_i(0) \mid T_i = 1] \quad (1)$$

$$= E[Y_i(1) \mid T_i = 1] - E[Y_i(0) \mid T_i = 1] \quad (2)$$

$$= D = E[Y_i(1) \mid T_i = 1] - E[Y_i(0) \mid T_i = 0] \quad \text{if } E[Y_i(0) \mid T_i = 0]$$

$$= E[Y_i(0) \mid T_i = 1] \quad (3)$$

$$\implies TOT = D \quad \text{if } B = 0 \quad (4)$$

- $E[Y_i(0) \mid T_i = 1]$ cannot be directly observed to understand the extent of bias, but some intuition can be used to infer its direction (upward or downward bias)

Treatment Effect with Pure Randomization

- Treated and untreated households have the same expected outcome in the absence of the program
 - $E[Y_i(0) | T_i = 1] = E[Y_i(0) | T_i = 0] \implies$ selection bias $B = 0$

$$Y_i = \alpha X_i + \beta T_i + \varepsilon_i \quad (5)$$

- Where, T_i is the treatment indicator, and

$$Y_i \equiv [Y_i(1) * T_i] + [Y_i(0) * (1 - T_i)] \quad (6)$$

- T is random $\implies cov(T, \varepsilon_i) = 0 \implies \hat{\beta}_{OLS}$ is an unbiased and consistent estimator of the treatment effect β

Treatment Effect with Partial Randomization

- Pure randomization is not very common to conduct
- Partial randomization, where the treatment and control samples are chosen randomly, conditional on some observable characteristics X (e.g., land size and income) is common
- One needs the assumption of *conditional exogeneity of program placement* to estimate the treatment effect
- Denote $Y_i(1)$ as Y_i^T and $Y_i(0)$ as Y_i^C and write versions of eq. 5 for treatment and control groups:

$$Y_i^T = \alpha^T + X_i\beta^T + \mu_i^T \text{ if } T_i = 1, i = 1, \dots, n \quad (7)$$

$$Y_i^C = \alpha^C + X_i\beta^C + \mu_i^C \text{ if } T_i = 0, i = 1, \dots, n \quad (8)$$

Treatment Effect with Partial Randomization

- Pool the data for both groups and estimate a single equation
- Multiply eq.7 by T_i and eq.8 by $1 - T_i$, and use the identity in eq 6 to get

$$Y_i = \alpha^C + (\alpha^T - \alpha^C)T_i + X_i\beta^C + X_i(\beta^T - \beta^C)T_i + \varepsilon_i \quad (9)$$

- Where $\varepsilon_i = T_i(\mu_i^T - \mu_i^C) + \mu_i^C$
- The treatment effect from eq. 9 can be written as:
 $A^{TT} = E(Y_i | T_i = 1, X) = E[\alpha^T - \alpha^C + X_i(\beta^T - \beta^C)]$, where A^{TT} is the treatment effect on the treated
- A consistent estimate of the program effect with OLS can be obtained for eq. 9 if one assumes
 $E(\mu_i^T | X, T = t) = E(\mu_i^C | X, T = t) = 0, t = \{0, 1\}$ i.e., no selection bias because of randomization

Different Methods of Randomization

- 1 **Oversubscription:** if resources are limited, allocate program to selected eligible participants and keep the remaining as controls.
- 2 **Randomized Phase-in:** gradually phase the program across a set of eligible areas (those who have not yet benefited will be controls).
 - Alleviates equity issues and ensures that treatment and control groups are similar
- 3 **Within-group Randomization:** provide program to sub-groups in each targeted area
 - Spill-overs may be more likely
- 4 **Encouragement Design:** researchers randomly assign subjects an announcement or incentive to partake in the program

Concerns with Randomization

- **Ethical Issues:** often difficult to convince potential partners to randomly assign treatment, rather than to those who need it
- **External Validity:** the scale of the RCT is important to generalize and replicate the results
- **Lack of Compliance:** people assigned to a treatment may not take it (partial or imperfect compliance)
 - Nevertheless, the analysis should focus on groups created by the initial randomization
 - I.e., we should not exclude subjects or cut the sample according to behaviour that might have been affected by the random assignment
- **Spill-over Effects:** happens when the treatment helps the control as well as program participants
 - Also called contamination of program effects

Intent-to-treat (ITT) Estimates and Measuring Spill-overs

- Some key challenges in identifying program impacts:
 - 1 Spill-overs: select program and control areas so that they are located sufficiently far apart
 - Migration across the two areas should be unlikely
 - 2 Targeted subjects may also not participate in the program
 - 3 Attrition: dropping out of the program or control areas (check if it is systematic or not)
- Address 1 & 2 by:
 - 1 Using intent-to-treat (ITT) estimates
 - 2 Instrumenting actual program participation by randomized assignment strategy

Baseline Survey - Advantages

- 1 Enables investigation of interactions between initial conditions and the impact of the program
 - Useful to assess external validity
- 2 To conduct policy experiments: comparison of uptake before and after the randomized intervention
- 3 To verify that randomization was implemented properly
- 4 To test and refine data collection procedures

Baseline Survey - Issues to Consider

- Baseline surveys can be very costly
- May lead to bias in program impacts by altering the counterfactual
- A careful evaluation should be done before spending resources on a baseline

Reference

- Ali, Daniel Ayalew, Matthew Collin, Stefan Dercon, Klaus Deininger, Justin Sandefur and Andrew Zeitlin. 2016. Small price incentives increase women's access to land titles in Tanzania. *Journal of Development Economics* 123: 107-122.

Research Questions

- Does providing price incentives for land titling increase uptake of land titling certificates by households in general, and by female household members in particular?

Motivations

- Formalizing land tenure promotes economic development and women's empowerment
 - Determines long-run economic growth
 - Improves savings, investment, female labor supply and fertility choices
- Customary land tenure (the counterfactual to formalization) is heavily biased against women

Motivations Cont.

- Two obstacles prevent women from benefiting from land titling in African countries like TZ
 - 1 Limited implementation of land titling programs due to prohibitive cost of land surveying for all households (extensive margin)
 - 2 Exclusion of women in most implemented registrations (intensive margin)
- Empowering women through land titling improves the welfare of the entire household

Contributions

- Using a credible identification strategy (RCT) to identify the impact of price incentives on land titling
- Distinguishing the additional impact of joint-titling from the overall impact of titling - significant implication for empowerment of women

Female Land Ownership in TZ

- The 1999 Land Act of TZ explicitly recognizes the rights of women as land co-owners when they live with their husbands, but this has not been put in practice
 - In Dar es Salaam, (the study area), only 13% of hhs report women as default owners
 - Only 40% state at least one woman must agree before selling land
 - Around 70-75% of all land registered with a residential license is done so with a single male name
 - Only 25% those asked about a formal land titling mention a woman as one of the owners
- The Land Act conflicts the 1971 Law of Marriage: property assigned solely to one spouse cannot be claimed by the other later on
- Then, does co-titling improve the de facto state of ownership

The Cost of Formalization in TZ

- TZ implemented MKURABITA - a property and Business Formalization Programme during the 2000s
 - Provision of business registration and formal titles - tradable and usable as collateral for credit
 - Coverage of the poor has however been limited
 - Less than 15% of the total lands is covered by a formal title, even in urban areas
 - Key reason being high cost of the formal titling process - most importantly cadastral surveying
 - Surveying companies in Dar charge US\$ 600 - US\$ 3000 depending on factors like size, location etc..
 - TZ ministry of land estimates cost to be aprox. TZS150,000 - TZS250,000 (US\$ 96 - US\$ 160)

The Cost of Formalization in TZ

- The situation opens the opportunity to experimentally vary subsidy for the titling and identify its impact
- Large formalizations lead to economies of scale
- The authors collaborate with local authorities and contracted a pvt. surveying co.
 - Cadastral survey of 1100 parcels
 - Hired a town planner
- Final cost/parcel, approx. TZS 70,000 = USD\$ 45
- The authors also collaborate with a trusted micro-finance NGO called “Women Advancement Trust” - WAT
 - WAT, among other things redeemed the vouchers and collecting contributions from treatment households

Sample Area

- Survey area: Kinondoni district (one of the 3 districts in Dar, the others, Ilala, and Tmeke)
- Two adjacent communities: Mburahati Barafu and Kigolo Kati - unplanned informal settlements
- Lower levels of female land ownership, 17% and 22% respectively (municipal average = 25%)
- Thus, the authors aim to induce hhs in both communities to purchase certificates of right of occupancy (CRO) and study their impact

Randomization

- 1 Cadastral survey and repayment program
 - Treatment parcels get cadastral surveying and a CRO for 100,000 TZS
 - Control group left to buy cadastral surveying at full government price (over 500,000)
- 2 Random price variation within treatment blocks
 - Treatment hhs were randomly allocated up to two vouchers redeemable for different levels of subsidy (0-80%) on the final price of a CRO
 - Enables estimation of price elasticity of demand for the CRO
- 3 Random voucher conditionality
 - Half of the vouchers were made conditional- redeemable only if a female household member was included as an owner in the CRO application

Take-up

- To measure the impact of conditional v_C and unconditional v_G price discounts on take-up, estimate

$$q_i = \beta_0 + \beta_G v_{Gi} + \beta_C v_{Ci} + \beta_x x + \varepsilon_i \quad (10)$$

- Where x is a vector of baseline household characteristics
- Key empirical test the authors are interested in is:

$$H_0 : \beta_G = \beta_C$$

- If H_0 is rejected \implies there is a disutility (mainly male) household heads who would prefer not to include their spouses as their co-owners male households
- They would require a larger price subsidy in order to offset the prospect of, e.g., diminished bargaining power (See table 4)

Randomization - Application

Ali et al. 2016 - Results

Table 4

The effect of price discounts on take-up of land titles.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
General voucher value (% of TSh 100k)	0.288*** (0.0590)	0.288*** (0.0627)	0.291*** (0.0632)				
Conditional voucher value (% of TSh 100k)	0.288*** (0.0555)	0.307*** (0.0578)	0.295*** (0.0573)				
Price (TSh 100k)				-0.298*** (0.0516)	-0.293*** (0.0514)	-0.293*** (0.0515)	-0.292*** (0.0514)
Price x income							0.0385 (0.0578)
Price x assets						-0.0310 (0.0559)	
HH monthly income (std)			0.0000131 (0.0110)		0.0000187 (0.0110)	0.000380 (0.0110)	-0.0185 (0.0270)
HH asset stock (std)			0.0175 (0.0143)		0.0176 (0.0142)	0.0330 (0.0280)	0.0171 (0.0143)
Baseline controls	No	No	Yes	No	Yes	Yes	Yes
Restricted sample?	No	Yes	Yes	Yes	Yes	Yes	Yes
Test: $\beta_C = \beta_C$	0.993	0.757	0.947				
R ²	0.210	0.234	0.272	0.234	0.272	0.272	0.272
Obs	1144**	1028	1028	1028	1028	1028	1028

Notes: Linear probability model. Dependent variable = 1 if household has fully paid for a CRO. Columns (2)–(7) restrict experimental sample to observations with all available baseline data. Robust standard errors *p < 0.10, **p < 0.05, ***p < 0.01.

Randomization - Application

Ali et al. 2016 - Results

- The authors couldn't reject $H_0 : \beta_G = \beta_C \implies$
- They combine the two variables to get statistical power to shed light on the slope of the demand curve for land titles

$$q_i = \beta_0 + \beta_p p_i + \beta_x \mathbf{x}_i + \beta_{px}(p_i \times \mathbf{x}_i) \varepsilon_i \quad (11)$$

where $p_i = 1 - v_{Gi} - v_{Ci}$

- Key results in columns 4-7 table 4:
 - Net price has a significant negative effect on take-up (this is in line with economic theory)
 - Land titles appear to be a normal good
 - Sensitivity to price does not vary by income

Co-titling

- Results show that applying conditionality does not deter hhs from purchasing land titles
 - Reducing concerns that it might be less-effective in improving women's ownership along the extensive margin
- This does not however imply that hhs are more likely to register a female owner
- The authors attempt to show that price conditionality does encourage hhs to include women as landowners
 - Improvement of women's status along the intensive margin as well

Co-titling

- To do this, the authors estimate the following regression

$$cotitle_i = \gamma_0 + \gamma_G v_{Gi} + \gamma_C v_{Ci} + \gamma_x \mathbf{x} + \varepsilon_i \quad (12)$$

- Key results (see Table 5): No impact of the general voucher, but the conditional voucher has a positive and significant (at one percent) impact on co-titling
 - A 10,000 TZS subsidy (10% of the default price) increase in the predicted probability that a woman is included by 3.0-3.33 percentage point
 - \implies offering the largest conditional subsidy of 80% of the purchase price guarantees the household will co-title!
- The authors also explore robustness of their results (see columns 5-7 of Table 5) & heterogeneous effects based on bargaining power of women (see Table 6)

Randomization - Application

Ali et al. 2016 - Results

Table 5
Effect of voucher distribution on co-titling decision and net women titled.

	Co-titling				Net women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
General voucher value (% of TSh 100k)	-0.0861 (0.129)	-0.157 (0.130)	-0.155 (0.141)	-0.116 (0.189)	0.112** (0.0484)	0.112** (0.0517)	0.114** (0.0521)	0.118 (0.0768)
Conditional voucher value (% of TSh 100k)	0.333*** (0.103)	0.305*** (0.105)	0.330*** (0.119)	0.0390 (0.113)	0.225*** (0.0468)	0.228*** (0.0493)	0.215*** (0.0495)	0.231*** (0.0734)
Any general voucher?				-0.0143 (0.0779)				-0.00290 (0.0377)
Any conditional voucher?				0.259*** (0.0772)				-0.0121 (0.0371)
Constant	0.753*** (0.0674)	0.776*** (0.0678)	0.734*** (0.158)	0.639*** (0.165)	0.309*** (0.0304)	0.338*** (0.0323)	0.420*** (0.0744)	0.424*** (0.0747)
Baseline controls	No	No	Yes	Yes	No	No	Yes	Yes
Restricted sample?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Test: $\beta_c = \beta_c$	0.0000579	0.0000140	0.0000142	0.422	0.0204	0.0256	0.0529	0.261
R ²	0.0958	0.1000	0.143	0.183	0.193	0.220	0.246	0.247
Obs	278*	263	263	263	1144	1028	1028	1028

Notes: Linear probability model. Columns 1–4: dep var = 1 if household has included a woman as co-owner on CRO application, conditional on taking up and filling out application filling one out. Columns 5–8: dep var = 1 if household has included woman on CRO application, zero if any other action taken. Test displays the p-value from a linear test of the hypothesis that general conditional voucher coefficients are equal. Columns (1) and (5) use the entire experimental sample, all other columns restrict the sample to obs with baseline data. Robust standard errors: * p < 0.10, ** p < 0.05, *** p < 0.01.

Randomization - Application

Ali et al. 2016 - Results

Table 6
Interaction effects between price incentives and baseline indicators of female bargaining power.

	X =				Default female owner			Women's share of income			Hypothetically cotite		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)			
	Take up	Take up	Co-titling	Net women	Take up	Co-titling	Net women	Take up	Co-titling	Net women			
General voucher (% of TSh 100k)	0.355*** (0.0858)	0.339*** (0.0930)	-0.0813 (0.203)	0.151* (0.0772)	0.424*** (0.109)	0.0136 (0.230)	0.165* (0.0921)	0.398*** (0.0960)	0.141 (0.222)	0.213*** (0.0788)			
Conditional voucher (% of TSh 100k)	0.335*** (0.0777)	0.300*** (0.0849)	0.514*** (0.170)	0.299*** (0.0732)	0.293*** (0.0966)	0.694*** (0.209)	0.295*** (0.0823)	0.366*** (0.0911)	0.702*** (0.189)	0.340*** (0.0765)			
X		-0.185* (0.104)	0.494** (0.198)	-0.0649 (0.0924)	0.00393 (0.145)	0.717** (0.335)	-0.0180 (0.116)	-0.0108 (0.0991)	0.766** (0.179)	0.0789 (0.0905)			
General × X		0.100 (0.210)	-0.409 (0.374)	0.186 (0.205)	-0.0458 (0.330)	-1.527** (0.609)	-0.0696 (0.223)	-0.143 (0.196)	-1.093** (0.433)	-0.129 (0.179)			
Conditional × X		0.261 (0.199)	-0.958** (0.419)	-0.0354 (0.171)	0.278 (0.292)	-1.124** (0.524)	0.150 (0.238)	-0.0814 (0.176)	-1.100*** (0.289)	-0.165 (0.157)			
Constant	0.546*** (0.0999)	0.571*** (0.103)	0.642*** (0.228)	0.386*** (0.100)	0.579*** (0.112)	0.517** (0.233)	0.368*** (0.106)	0.524*** (0.103)	0.507** (0.227)	0.343*** (0.0998)			
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Test 1: $\beta_C = \beta_{C \times X}$	0.818	0.663	0.0000305	0.0478	0.210	0.0000253	0.131	0.746	0.000161	0.111			
Test 2: $\beta_C \times X = \beta_{C \times X}$		0.469	0.122	0.289	0.332	0.323	0.333	0.738	0.986	0.829			
Test 3: $\beta_C + \beta_{C \times X} = \beta_C + \beta_{C \times X}$		0.555	0.887	0.708	0.514	0.00316	0.0691	0.853	0.0719	0.535			
R ²	0.276	0.281	0.222	0.269	0.288	0.294	0.275	0.281	0.270	0.267			
Obs	601	601	169	601	517	144	517	595	166	595			

Notes: All columns show a separate linear probability model. For each section of the sidewaystable (columns 2–4, 5–7, 8–10), X is defined by the column group heading. In columns labeled take-up, the dependent variable = 1 if household has fully paid for a CRO. In co-titling columns, the dependent variable = 1 if a woman is listed as owner or co-owner on a CRO application. In net women columns, dependent variable measures the total number of women in the household listed on a title combining take-up and co-titling effects. Robust standard errors *p < 0.10, **p < 0.05, ***p < 0.01.

Randomization - Application

Ali et al. 2016 - Conclusions

- Conditional subsidies to titling costs the same positive impact on demand as general subsidies
- WTP for land titles is \$40-\$50
- Conditional on purchasing a CRO, hhs which were allocated a conditional voucher were much more likely to include a woman on their title application
 - Small price incentives are an effective means to encourage de jure empowerment of women in land titling implementation
- The results however provide mixed news for policymakers
 - On the one hand, small price incentives induce women's access to land titles
 - However, there remain challenges in delivery and enforcement of these rights by the state

- End of Lecture