

Poverty and Landownership: quasi-experimental evidence from South Africa

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Introduction

- Wealth and asset inequality can prevent the poor from fully engaging in productive activities, by restricting the types of contracts and exchanges open to them, thereby perpetuating the cycle of poverty.
- Theoretical work that shows that non-market transfers of assets from the wealthy to the less wealthy might have positive efficiency and poverty reducing effects because of these incentive effects (Bardhan, Bowles and Gintis (2000), Legros and Newman (1997), Moene (1992), Mookherjee (1997), Shetty (1987), Banerjee, Gertler and Ghatak (2002))
- But what is the evidence on this?
- Recent land transfers in South Africa allows us to study whether such transfers can make a difference.

- Land policy in South Africa has been through several phases encompassing a wide range of reforms in the period since 1995.
- But the underlying structure of these reforms have remained unchanged in that they have usually been undertaken through a once-off grant made to beneficiaries followed by voluntary market transactions.
- The sole purpose of the grant generally is to facilitate the purchase of land.
- The state's role is to lubricate the bargaining process between the prospective beneficiaries and the seller.
- Key question: have land transfers made an impact on poverty status of beneficiaries?

Programs of Focus

Land Redistribution for Agricultural Development (LRAD): introduced in 2001; targeted at the individual level; awarded to beneficiaries on a sliding scale, depending on own contributions.

Settlement Land Acquisition Grant (SLAG): targeted to entire families; slowly being phased out at the time the survey; no matched contribution.

Extension of Security of Tenure Act (ESTA): farm workers

Labour Tenants Act (LTA): protection to labour tenants

Restitution of Land Rights Act: dispossession under Apartheid

Study Design

- Land reform is a messy business!
- Many factors, unrelated to producer characteristics, can influence the types of comparisons we make.
- Key identification problem: can we figure out some way of holding these factors constant (because they are hard to measure in a survey) thereby removing the confound induced by this category of unobservables?
- Design of the study:
 - 1 Quasi-experimental survey design (control/treatment type structure)
 - 2 Iterative approach to fieldwork and data collection

Control Group: grant applications still in the process of being approved, usually beyond the stage of approval of a “planning grant”, but where the funds for the actual transfer have not yet been approved.

Treatment Group: households to whom land has already been transferred. Three main programs: restitution (rights-based), redistribution (out-right transfer), security of tenure (prevention of evictions)

- Applications that are in the pipeline to become beneficiaries have to pass through several key milestones before final approval of the grant is obtained.
 - 1 Project Registration
 - 2 Approval of Planning Grant
 - 3 Preparation of Project Identification Report
 - 4 Approval of District Screening Committee
 - 5 Approval of Provincial Government
- At each milestone, projects are either approved to pass on to subsequent stages, referred back to the the government appointed planner for further development, or rejected altogether.
- Failure to reach a required milestone is therefore measurable, and such information could therefore be used in principle as an indicator of the likelihood of eventual selection into the treatment group.

- Ultimately we need to make sure that we compare like with like. This has implications for how we sample.
- Cluster sampling was necessary because projects are community-based.
- We therefore needed a large number of clusters (projects), our expectation being that most of the variation in outcomes would be generated by variation *across* clusters.
- Heterogeneity among the control group is greater than the treatment group by dint of the application process.
- It therefore helps to have a larger than needed control group.

- Needed a census of all land reform projects to serve as sampling frame
- From this census, clusters of households or “projects” were selected
- Then had to track beneficiary information
- List of beneficiaries had to be obtained from paper records
- Extremely messy exercise particularly with the control group
- Once lists were obtained, drew random sample of households

Data Description

Table: Mean Per Capita Consumption

Program	Total	Treatment	Control	N	p-val	Δ
All	459.17	453.26	465.99	3666	0.58	0
LRAD	497.52	547.76	472.61	1925	0.05	+
SLAG	375.51	373.55	386.93	456	0.84	0
Restitution	487.30	471.29	550.18	596	0.16	0
Tenure Reform	307.22	280.66	365.12	493	0.07	-
All	5.67	5.65	5.69	3665	0.22	0
LRAD	5.73	5.78	5.70	1940	0.05	+
SLAG	5.50	5.48	5.62	460	0.24	0
Restitution	5.41	5.37	5.49	498	0.09	0
Tenure Reform	5.77	5.74	5.91	606	0.05	-

Table: Test of Difference in Means for Covariates

Variables	Total	Treatment	Control	N	p-val
<i>Number employed in agriculture</i>	0.54	0.77	0.44	1725	0.00
<i>Log days in pipeline</i>	6.74	5.94	7.08	1725	0.00
<i>Days in pipeline (DoseIV)</i>	1423.26	844.27	1666.97	1725	0.00
<i>Days since treatment (Doserec)</i>	352.01	1188.30	0.00	1725	0.00
<i>Household head is male</i>	0.69	0.76	0.67	1725	0.00
<i>Education of household head (yrs)</i>	5.98	6.31	5.85	1725	0.06
<i>Mean farming experience (yrs)</i>	1.51	1.62	1.46	1725	0.40
<i>Number plots accessed pre-95</i>	1.15	0.65	1.34	1663	0.00
<i>Distance to DLRO (100 km)</i>	0.93	0.94	0.92	1718	0.54
<i>Area plots accessed pre-95 (hectares)</i>	51.55	31.60	59.18	1663	0.26
<i>Land allocated by municipality (post-94)</i>	0.13	0.03	0.21	916	0.00
<i>Land allocated by other farmer (post-94)</i>	0.09	0.00	0.15	916	0.00
<i>Land allocated by tribal authority (post-94)</i>	0.06	0.00	0.09	916	0.00

Table: Propensity Score Regressions

Variable	(1)	(2)
Number employed in agriculture	.375 (.055)***	.645 (.107)***
Log days in pipeline	-.761 (.063)***	-.844 (.104)***
Household head is male	.302 (.132)**	.616 (.200)***
Education of household head (yrs)	-.004 (.013)	-.069 (.020)***
Number plots accessed pre-95		1.017 (.181)***
Distance to DLRO in 100 Km		.238 (.146)
Size of plots accessed pre-95 (Hectares)		.00004 (.0004)
Ever been allocated land by the municipality (post-94)?		-2.180 (.351)***
Ever been allocated land by other farmer (post-94)?		-4.915 (1.094)***
Ever been allocated land by the tribal authority (post-94)?		-4.649 (1.084)***
Const.	3.813 (.457)***	4.993 (.760)***

Table: Propensity Score Balance

Block	$\min \hat{p}(\mathbf{x})$	N_0	N_1	$\bar{p}_0(\mathbf{x}) - \bar{p}_1(\mathbf{x})$	SE	t	t_{cv}
1.00	0.03	133.00	11.00	-0.01	0.16	-0.66	2.58
2.00	0.20	64.00	23.00	-0.02	0.01	-2.50	2.64
3.00	0.30	48.00	29.00	-0.01	0.01	-1.81	2.64
4.00	0.40	80.00	73.00	-0.01	0.01	-1.55	2.58
5.00	0.60	38.00	119.00	0.00	0.01	-0.32	2.58
6.00	0.80	19.00	139.00	-0.03	0.02	-2.19	2.58

“Block” refers to an interval placeholder from among 6 mutually exclusive intervals of the propensity score distribution. These intervals are defined by the cut-off points given by $\min \hat{p}(\mathbf{x})$. The fifth column in the table reports on the magnitude of the difference in means for the propensity score between treatment and control for each block. t refers to the t-statistic for testing that the reported difference in column 5 is significant.

Table: Covariate Balance

Variable	1	2	3	4	5	6
<i>onfarmemp</i>	-0.01	0.88	-0.30	-0.42	-0.05	-0.90
<i>ldoseIV</i>	1.81	2.55	-0.55	-0.40	-0.63	1.79
<i>sexhhead</i>	-0.46	0.68	1.41	-0.59	0.12	-0.40
<i>hheadeduc</i>	-1.01	0.92	0.98	0.11	-0.80	0.40
<i>farmerexper</i>	-1.53	0.44	1.88	-0.89	0.67	-0.19
<i>pre95sum</i>	0.75	-0.70	0.53	-1.40	-1.02	-0.07
<i>dist100</i>	-0.90	-1.53	0.87	-0.19	0.74	0.53
<i>pre95size</i>	0.29	1.05	0.77	-1.83	-0.72	-0.60
<i>MUNpl</i>	0.21	-1.81	1.37	0.96	1.78	-0.37
<i>FARMERpl</i>	-0.68	—	—	—	—	—
<i>TRIBALpl</i>	0.58	—	—	-1.05	—	-0.37

The entries report the t -statistic for an equality of means test of each regressor by treatment status within the 6 intervals of the balanced propensity score distribution.

- Key idea is to match each treated household to a control household that most closely resembles it, without throwing away data.
- Formally, we can define the set of potential control group matches (based on the propensity score) for the i th household in the treatment group with characteristics \mathbf{x}_i as $A_i(p(\mathbf{x})) = \{p_j \mid \min_j |p_i - p_j|\}$
- The average treatment effect is then:

$$ATT = (N_1)^{-1} \sum_{i \in \{T=1\}} (y_{1i} - \sum_j \omega(i, j) y_{0j})$$

where j is an element of $A_i(p(\mathbf{x}))$ and $\omega(i, j)$ is the weight given to j .

- When the weight function is $\omega(i, j) = \frac{K(p(\mathbf{x}) - p(\mathbf{x}))}{\sum_{j=1}^{N_{0j}} K(p(\mathbf{x}) - p(\mathbf{x}))}$ and where

$$K = \frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{p(\mathbf{x})^2}{2\sigma^2}}, \text{ we have a Kernel estimator of ATT}$$

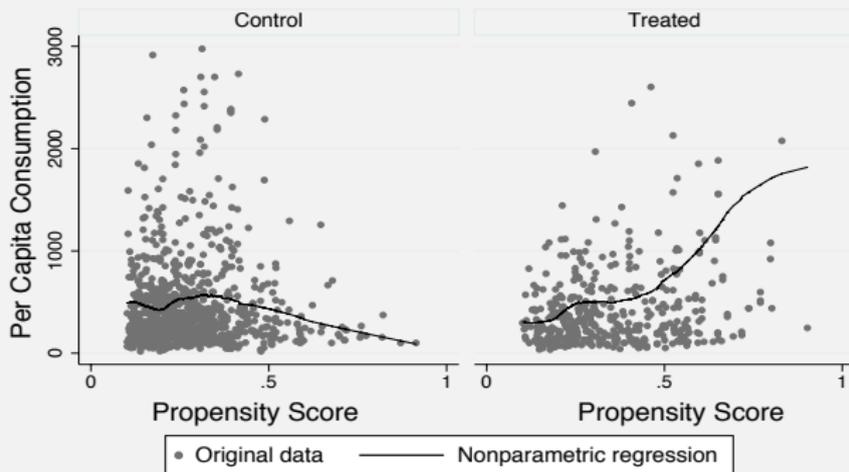
- K is the Gaussian kernel.

Table: Summary of Treatment Effects

Program	Method	Definition	$T = 1$	$T = 0$	ATET	SE	t
LRAD	Single Difference	Per capita			75.18	37.97	1.98
LRAD	Stratification*	Per capita	511	2154	143.93	56.43	2.55
LRAD	Stratification	Per capita	394	1047	149.87	84.42	1.78
LRAD	Kernel*	Per capita	511	1063	134.24	54.88	2.45
LRAD	Kernel	Per capita	394	382	169.18	77.55	2.18
LRAD	Stratification*	Household	511	1063	592.87	208.76	2.84
LRAD	Stratification	Household	394	1047	765.55	329.79	2.32
LRAD	Kernel*	Household	511	1063	545.17	202.24	2.70
LRAD	Kernel	Household	394	382	867.15	276.94	3.13

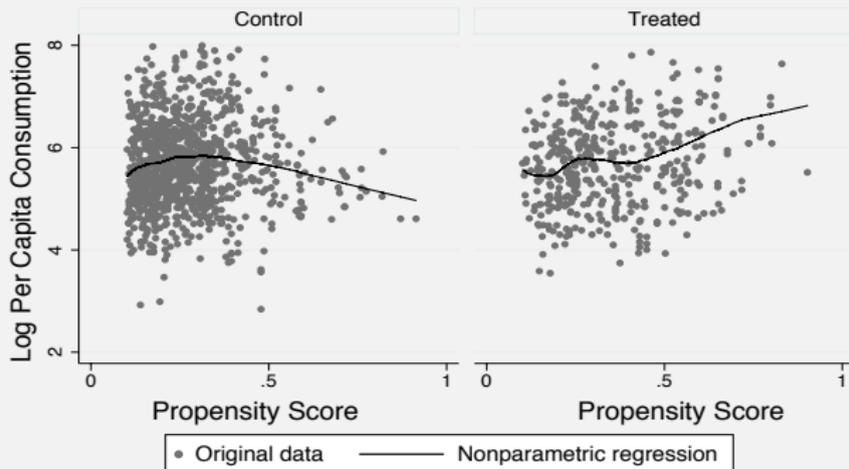
The starred results are based on specification 1 of the propensity score regression. Differences in sample sizes are the result of the combined effect of matching and trimming. Stratification matching is based directly on the blocking used in the tests for balance.

Post-treatment Consumption against Propensity Score



Graphs by Treatment Status

Post-treatment Consumption against Propensity Score



Graphs by Treatment Status

Table: Average Treatment Effects for LRAD (IV)

Dependent Variable	1	2	3
Treatment	PCE	PCE	PCE
Instrumental Variable	None	<i>DoseIV</i>	<i>DoseIV</i>
<i>Distance to DLRO (km)</i>	-0.698 (.31)**	-0.749 (.314)**	
<i>DoseIV</i>	-0.03 (.018)*		
<i>Treated</i>	85.764 (47.458)*	370.036 (166.103)**	386.936 (166.455)**
<i>D (Distance to Nearest DLRO ≤ 50 km)</i>			172.769 (47.937)***
<i>N</i>	1651	1651	1658

- Impact of LRAD on per capita consumption is positive, and remains positive and significant even once we have controlled for selection bias.
- Woolard and Leibbrandt (2007) report that the lower bound poverty line for RSA is R416.99. Another line widely used sets the threshold at R555.55.
- We conservatively take the average of these two lines: R486.27 per capita.
- Average PCE for our control group is R472.61.
- Our estimated impact of the land transfer is R134.24 - enough to bump households out of poverty in the short term.
- Several caveats to these calculations: see paper for more details.