Journal of Development Economics 110 (2014) 250–261

Contents lists available at ScienceDirect

Journal of Development Economics

journal homepage: www.elsevier.com/locate/devec

Poverty and land redistribution

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ARTICLE INFO

Article history:
Received 4 November 2011
Received in revised form 23 September 2013
Accepted 2 October 2013
Available online 18 October 2013

JEL classification:
O10
O12
O13

Keywords:
Land reform
Poverty
Impact evaluation

ABSTRACT

Despite a theoretical literature that promises that land transfers will have large impacts on the well-being of poor households, well-identified empirical evidence on the efficacy of land redistribution is scarce. In an effort to fill this gap, this paper examines South Africa's Land Redistribution for Agricultural Development (LRAD) program. We exploit features of LRAD program implementation to extract exogenous variation in whether, and for how long, applicant households enjoyed land transfers. Binary treatment estimates, which compare treated with untreated households, show that beneficiary households on average experienced a 25% increase in per-capita consumption. Our preferred continuous treatment estimates, which analyze only the subset of treated households, identify the impact time path of land transfers on consumption. These estimates show that living standards initially drop and then, after 3–4 years, rise to 150% of their pre-transfer level. These results are statistically significant and robust to a statistically more conservative identification strategy.

1. Introduction

A rich theoretical literature suggests that asset inequality can be economically detrimental if it leaves large numbers of low wealth agents so poor that they are unable to fully utilize their endowments and productive opportunities.[2] From this perspective, a discrete jump in the productive wealth of poor agents may enable them to unlock their unrealized potential, and generate significant economic gains. It is precisely this connection to incentives and market opportunities that make asset transfer programs a potentially attractive policy option in the fight against rural poverty. This perspective also suggests that the impacts of land and other asset transfer programs will not be instantaneous, but will instead build up over time as beneficiaries respond to, and invest in, these new opportunities.

Despite the attractiveness of these theoretical arguments for land transfers, there has been little empirical demonstration of their effectiveness, especially relative to the outpouring of empirical evidence on cash transfer programs. This paper provides some of the first well-identified estimates of the poverty reduction impacts of land redistribution. Our preferred continuous impact estimates of a South African land reform program, which analyze only households that received transfers, reveal that these transfers increase medium-term household living standards by 50%. The estimated temporal impact pattern also mimics that predicted by the theoretical literature. By way of comparison, Behrman et al. (2005) indicate that Mexico's well-studied cash transfer program will only boost per-capita living standards by less than 5% in the long-term.

The relative scarcity of empirical evidence on land transfers is understandable. While, the last century has seen any number of land redistribution schemes, they are hard to evaluate because the events giving rise to them are decidedly non-random, and options for credibly identifying their impacts are few. At the macro-level, much of the related evidence on egalitarian land ownership derives from cross-country

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[1] We especially thank Klaus Deininger, Rogier van den Brink, Julian May, and Aluwani Matsila, whose efforts greatly facilitated the completion of this study. We also would like to thank Pranab Bardhan, Hans Binswanger, Stephen Boucher, Ben Cousins, Alain de Janvry, Michael Kirk, Heinz Klug, Marcus Goldstein, Michael Lipton, Dilip Mookherjee, Mark Rosenzweig, Jolyne Sanjak, Chris Udry and Francis Wilson for helpful comments on earlier versions of this work, as well as seminar participants at Stellenbosch, UCT, UJ, Wisconsin, Yale, the Inter-American Development Bank, the BASIS CRSP conference on "Escaping Poverty Traps", the World Bank, and the UNU-WIDER conference on "Land Inequality and Decentralized Governance in LDCs". We gratefully acknowledge funding from the World Bank and the South African Department of Land Affairs. Tim Brophy, Susan Godlonton, Simon Halliday, Ronelle Ogle, Victor Orozco, and Heather Warren provided excellent research assistance.

[2] Foundations for this perspective are found in static models such as Bardhan et al. (2000), Dasgupta and Ray (1986), Eswaran and Kotwal (1986), and Mookherjee and Ray (2002). Subsequent theory has shown that overall economic performance can remain sensitive to asset inequality even in the context of dynamic models in which agents have time (and optimal savings plans) as another degree of freedom to work around missing contracts and financial markets. For example, see Carter and Zimmerman (2000), Galor and Zeira (1993), and Ray and Streufert (1993).

0304-3878/$ – see front matter
http://dx.doi.org/10.1016/j.jdeveco.2013.10.003

growth regressions that show a positive relationship between egalitarian land ownership and aggregate economic growth.[3] These studies suffer the usual questions concerning data comparability and differences in historical legacies across societies that may confound causal interpretation of the impact of greater land ownership equality.

At the micro-level, there is an abundant literature that suggests that small-scale farms are more productive than large-scale farms, seemingly indicating that land redistribution can boost living standards and aggregate productivity.[4] While linked to the traditional "economic case for land reform,"[5] few, if any, of the studies in this literature explicitly concern redistributive reforms. Most explore data generated by historical farm size distributions, raising a plethora of identification concerns (e.g., are "naturally occurring" small farms intrinsically more productive either because they have better soils, or because they are operated by better farmers).

The absence of studies that directly explore the causal impact of land redistribution in part reflects the shift of land policy over the last 25 years away from redistribution and towards land titling and tenancy reform programs. But it also reflects the political and economic complexities of implementing and evaluating land reforms that entail an outright transfer of ownership from the wealthy to the poor. While randomized controlled trials (RCTs) have been used to study modest in-kind asset transfers,[6] no country to date has implemented land redistribution by randomly distributing land to some, but not other, potential beneficiaries.

This paper studies the impact of South Africa's Land Redistribution for Agricultural Development (LRAD) program. LRAD makes land purchase grants to landless farm workers and labor tenants. The program does not mandate redistribution of land from rich to poor, but rather operates through markets on a willing buyer–willing seller basis. While the "helping hand" of the state is clearly visible in this process, the market basis of the LRAD program can make it less contentious, and more amenable to evaluation, than state-mandated redistributive reforms.

To identify the impacts of land transfers, this paper exploits features of LRAD implementation to extract exogenous variation in treatment duration (treatment duration is the period of time that beneficiaries had to work with the transferred land up until the time of this study's survey). Individuals self-select into the LRAD applicant pool, and are then subjected to a screening process that encompasses multiple stages. This screening process leads to a homogenization of sorts among the applicant pool. Applicants with similarly high chances of succeeding as farmers are kept in the applicant pool, while applicants with little chance of success are dropped from the pool. This administratively filtered subset of the applicant pool ends up receiving land transfers at different points in time leading to a variation in the duration of treatment. We study the effect of the program by exploiting the plausibly exogenous component of this variation.

To achieve identification we employ generalized propensity score (GPS) methods that allow us to match transfer beneficiaries based on observable characteristics that are likely to influence both treatment duration and its impact. We analyze beneficiary data under two alternative statistical strategies. Under our core strategy, we match individuals based on their date of entry to the pipeline and other conventional human capital characteristics. Identification under this strategy assumes that time in pipeline (both to approval and to final receipt of the land transfer) is random. While this seems reasonable given that all individuals in the study were approved and certified to be worthy, there might be some concern that some part of the time to approval might be related to unobserved, productivity-relevant characteristics. To assuage these fears, we implement a second identification strategy that further matches transfer recipients on the entire time they spent in the pipeline up until the signing of the sales contract for the land (after which further, random, delays occurred before the land could be occupied by the beneficiary). While this second, statistically conservative strategy potentially discards random variation in treatment duration that might otherwise be used to identify the impact of treatment duration, it gives similar results to the core strategy.

Under both identification strategies, we estimate a duration response function by following Hirano and Imbens (2004) approach of mapping the generalized propensity score into outcomes and then averaging outcomes by duration level. Both estimated duration response functions show that consumption levels initially dip (implying negative treatment effects) for households with less than 1 year of treatment, but then rise dramatically to peak at 50% after about three years of exposure. Impacts of this magnitude lift the typical beneficiary household from a $2 per-day poverty line standard of living to an almost $3 per-day living standard. Impacts of this magnitude are sufficiently large that they offset the direct costs of the asset transfer grant within three to four years.

In addition to the continuous treatment estimates, we also use standard binary treatment methods to compare all treated applicants with a pipeline control group of approved applicants who were still stuck in the pipeline at the time of the survey. The validity of these estimates relies on the same assumption as the core identification strategy used in the continuous treatment analysis. As would be expected given the distribution of treatment durations, these estimated binary impacts are roughly half the magnitude of the long-run continuous treatment estimates. The contrast between the binary and continuous estimates indicates that it is important to recover the temporal impact pattern if we are to properly understand the long-term, policy relevant treatment effects for programs like LRAD that expand opportunity for poor families.

The remainder of this paper is structured as follows. Section 2 describes the LRAD program and builds up the logic of the two identification strategies. Section 3 describes the data and derives binary treatment estimates employing the core identification strategy. Section 4 then presents the continuous treatment methodology and results. In this section, we also implement the more conservative identification strategy mentioned above, confirming that the core continuous treatment results are robust. Finally, Section 5 concludes the paper with reflections on the efficacy of land reform as a poverty reduction program.

2. Using LRAD program implementation to identify the impacts of redistribution

The dispossession of the land rights of South Africa's majority population was a cornerstone of colonial policy in South Africa and the apartheid era that followed. Against this backdrop, the first democratically elected government brought forward a land reform agenda in 1994. Ironically, the very thoroughness of dispossession had all but eliminated the peasant smallholder class that has been the focal point of land reform in other countries. In South Africa of the mid-1990s, land reform was initially pursued as a restitution of legal rights, with lesser attention to securing the economic benefits typically associated with land reform efforts. The result, perhaps predictably, was an ineffective program that met with a sluggish

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3 Deininger and Olinto's (2000) analysis is perhaps the most convincing analysis of this genre because of their use of fixed effects methods to control for intrinsic country heterogeneity.

4 See for example Berry and Cline (1979), Binswanger et al. (1995), Carter (1984), Lipton, Eastwood, and Newell (2009), Rosenzweig and Binswanger (1993), and Shaban (1987). See also Lipton et al. (1996) and Zyl et al. (1996) for the case of South Africa.

5 Dorner (1970) and Kanel (1968) articulate the classic instrumental economic case for land redistribution, arguing that agency costs in agricultural labor markets create an inverse relationship between farm productivity and size such that aggregate output and economic performance improve with land redistribution.

6 See de Mel et al. (2008) and Fafchamps et al. (2011) for examples of RCTs that transfer non-land productive assets (e.g., sewing machines) worth a few hundred dollars to randomly selected beneficiaries. These studies largely find large marginal returns to these asset transfers.

uptake. Reacting to this reality, the South African government overhauled its land reform approach in 2001, creating the LRAD program.

Styled on the willing buyer–willing seller market-assisted land reform model (see Deininger (1999)), LRAD is intended to provide land to black South Africans with an interest in farming, especially women. The program requires applicants to live on or near the land they wish to acquire through the program. Individuals who hold public office, civil servants, or relatives of such individuals are ineligible.

The program works on the basis of a grant that is awarded to beneficiaries on a sliding scale. The minimum grant of 20,000 South African rands requires a matching contribution of 5000 rand (in cash or in-kind). The maximum grant of 100,000 rand requires a matching contribution amount of 40,000 rand. In practice, grants are pooled into a fund that is administered on behalf of a small group of beneficiaries. These funds are then used to purchase land, which becomes the property of the beneficiaries.

As with other market-assisted programs, LRAD was intended to rely on beneficiary self-selection to ensure that resources would be channeled to those most interested in farming, and best able to benefit from the redistribution. While this feature is attractive from a targeting perspective, this heavy reliance on self-selection makes it more difficult to evaluate the program.

Our identification strategy rests upon a detailed understanding of the mechanics of LRAD implementation. We will argue that LRAD implementation creates exogenous variation in whether and for how long an individual is treated (receives land). The basic idea is similar in spirit to studies such as Lavy (2002) that rely on arbitrary discontinuities in treatment eligibility to derive plausible counterfactuals for non-randomly assigned treatments. While the LRAD assignment mechanism is not based on arbitrary eligibility cutoffs as in conventional discontinuity designs, we will show that the administrative screening of applicants to the LRAD program presents options to proxy its assignment mechanism in a way that is analogous to the Lavy study. That is, otherwise similar households that could have been treated sooner are denied immediate benefits for arbitrary reasons that are uncorrelated with their characteristics and their expected gains from the program.

2.1. The application and approval process

Fig. 1 illustrates the key implementation stages in the LRAD land redistribution program. There are four main stages, each of which generates variation in whether any potential individual beneficiary was treated with a land transfer, and if so, for how long prior to this study's survey date:

1. Beneficiary self-selection and application (t_1)

Beneficiaries must choose to apply for an LRAD land grant and all of our control group strategies will rely only on the population that self-selected into the program. Moreover, the date by which the individual applied for an LRAD grant will also influence whether the individual had been granted land by the study date, t_5, and for how long (treatment duration, d). As this timing decision is likely to be related to both observable and unobservable productivity relevant characteristics of the individual, all of our identification strategies will employ matching methods that control for application delay, d_1. In the continuous treatment analysis, early applicants with modest treatment durations will function as controls for early applicants with long treatment durations, while late applicants with modest treatments will function as controls for late applicants with longer treatments.

2. Administrative approval (t_2)

LRAD administrative processes are quite complex and involve the following steps:

(1) *Project registration*

The first step in the LRAD approval process is registration of applications to the program. Once an application is submitted, a state planner does an assessment of the site on which the applicants live as well as the land they have applied to purchase. The purpose of this initial assessment is both to verify the information of the applicant against the eligibility criteria and to form an initial impression of the resources available to the applicants as well as the constraints under which they operate. If the application appears to be a serious prospect, then it is "registered" as candidate land redistribution project.

(2) *Planning grant approval*

Approval step 2b begins when the planner requests the district land affairs department to release a nominal sum of money to develop a proposal on behalf of the applicant. The funds pay for various specialized studies that the planner uses to substantiate the beneficiaries' business plan and to negotiate a purchase price for the land. Examples of such studies are property valuations, soil assessments, land surveys and whether public infrastructure is adequate for the project.

(3) *Preparation of project identification report*

Once these commissioned studies materialize, the planner works with the applicants to create a final business plan and proposal that is ultimately submitted to the state to justify the LRAD transfer. This proposal preparation step is an important process that is handled through a series of workshops between relevant stakeholders and culminates in the preparation of a document called the project identification report (PIR), which summarizes the merits of the application. The existence of this document is an important milestone in the approval process as it signals that the applicants have a strong enough interest and background in farming to have warranted the release and expenditure of state resources to begin making the case for the grant.

(4) *District Screening Committee approval*

In approval step 2d, the planner submits the PIR document to a District-level Screening Committee (DSC) of the land affairs department. This body has a broad representation from all stakeholders including officials from the agriculture department, the surveyor general's office and local municipalities. The role of the DSC is to screen applications before they are passed on for final approval by the provincial committee.

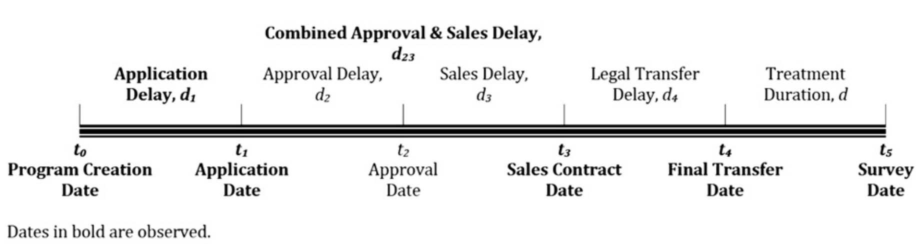

Fig. 1. LRAD implementation timeline.

Once an application has been approved by the DSC, a formal request to designate the land for redistributive purposes is made. At this stage the "designation memo" is prepared. This document contains a summary of the merits of each LRAD application. The specialist reports generated in approval step 2c are used to assess the feasibility of the proposed activities of the applicants. The provincial committee reviews this document before making the final decision about whether or not to approve the application.

As the complexity of this process makes clear, delays in applicant approval could occur for a variety of reasons. Some of these reasons (e.g., a bad proposal) are of course endogenous to applicant characteristics, and expected gains from a transfer, while others are the more innocuous sort of bad luck and bureaucratic delays that crop up during any complex administrative procedure. In the analysis to follow, we will deal with the endogenous portion of this delay in two ways. First, we will restrict our attention only to the population that successfully navigated approval step 2d. This restriction by definition eliminates the weaker proposals and applicants from our study population.

However, one might still worry that the best of the better proposals got approved more quickly, thus making treatment, and a longer duration of treatment, more likely. Ideally, we would like therefore to be able to match on approval delay time, d_2 in Fig. 1, as well as application delay. However, we lack data on the date at which the designation memo was signed. As a secondary strategy we will therefore employ a statistically more conservative matching strategy, as we now explain.

3. Signing of sales contract with land seller (t_3)

Once approved by the district committee, the self-selected, administratively approved potential LRAD beneficiaries face two additional sources of delay. Interviews with program administrators in the field revealed that in practice administratively approved applications often become mired down, facing considerable and highly variable delays in receiving their land, if they receive it at all. One reason for further delay is that the initially willing land-seller (whose assent was obtained in stage 2b) might renege at the last minute, perhaps as a strategy to renegotiate the selling price or simply to avoid new neighbors.[7] While these delays, which seem mostly related to seller characteristics, are likely to be exogenous to expected impacts of transfers, we might worry that such delays are more likely when the land to be transferred is of a different quality. Fortunately, we observe the date on which the sale agreement was signed (t_3 in Fig. 1) and thus have the option to employ propensity score methods that control for the combined approval and sales delay, d_{23}. Note that controlling for this variable takes care of concerns about endogeneity in both stages 2 and 3. It will also throw away substantial exogenous variation (meaning those delays of bad luck that had nothing to do with beneficiary or project characteristics). Given that it cannot be determined how much, if any, of the d_{23} delay is endogenous, we will refer to our identification strategies that control for this delay as "statistically conservative."[8]

4. Legal transfer of land to beneficiary (t_4)

The final delay potentially faced by LRAD beneficiaries is the formal titling and transfer of the land after a signed sales contract is in hand. While this would seem to be a routine administrative process, in fact such was not the case. Interviews with LRAD staff reveal that given the history of apartheid, it was not infrequent that an LRAD sale was delayed or blocked by the discovery of a pending legal claim for the land offered up by the seller. Such claims typically resulted from legal actions of the descendants of individuals who had been previously dispossessed under Apartheid and who could demand return of the land under a Mandela-era land restitution program. While new applications to that program were terminated in 1999, it left a substantial overhang of unresolved land claims, making it legally impossible in some cases to transfer land title deeds to approved LRAD beneficiaries many years later. In the analysis to follow, we will treat such variation as exogenous to expected impacts. Section 4.4 below provides evidence corroborating the claim that these d_4 delays are exogenous and respond to legal-bureaucratic forces.

2.2. Filtered pipeline identification strategies

As the preceding discussion makes clear, the LRAD application process is subject to an array of forces and delays. Such delays not only determine whether an individual is treated at all, it also determines the duration of treatment defined as in Fig. 1 as the time between final land transfer and the date of the research survey.[9] While subsequent sections will detail the precise estimation methods to be employed, we are now positioned to summarize the two fundamental identification strategies that will be employed:

- *Core identification strategy*

The core strategy builds both treatment and control groups from the population that has (i) self-selected into the program; and (ii) been administratively filtered and approved (i.e., cleared stage 2 above). In addition, both the binary and continuous treatment analyses that will be constructed on this basis will use propensity score techniques to control for application date or delay d_1 in Fig. 1. For the continuous treatment analysis, only households that had also exited the pipeline and received their land prior to the survey date will be considered.

- *Statistically conservative identification strategy*

The conservative strategy augments the core strategy by also controlling for the full approval and sales delay (d_{23} in Fig. 1). This strategy is conservative in that it clearly throws out exogenous variation that could aid and strengthen identification in order to purge the analysis of suspected sources of delay that are endogenous to applicant and project characteristics that may be correlated with expected impacts. Because this strategy requires information on the date on which the sale agreement was eventually signed, it is only available for the subset of survey respondents who had in fact received land transfers (i.e., the treatment group). This strategy therefore cannot be adopted when looking at binary impacts that compare treated with untreated households as the untreated households for the most part do not yet have a signed sales contract.

3. Land redistribution impacts: descriptive statistics and binary treatment impact estimates

As described in the prior section, LRAD is implemented at the farm or project level, which typically brings together several beneficiary families. Our sampling followed a multi-level design in which projects were first randomly selected from the universe of all projects (approved or not) in the study regions, with probability of selection proportional to the number of beneficiary or applicant households who were part of the project. This random sample of LRAD projects was then subjected to screening to get rid of weak projects and applicants. As explained above, the existence of a signed "designation memo" (step 2d described in the prior section) indicates that a project and its applicants successfully

[7] A reason less frequently cited for delays in final approval of an application concerns a failure by local councils to meet their targeted roll-out of basic infrastructure in a district affected by an impending redistribution of land.

[8] It also cannot be determined what the bias of any endogenous delay elements would be as some delays may signal lower quality projects, while others may signal higher quality projects.

[9] While the survey date is similar for most respondents, delays due to a hold-up of fieldwork implementation in some districts resulted in further exogenous variation in the duration of exposure to the land transfer.

Table 1
Descriptive statistics.

Variables	Treated	Pipeline control	Duration of treatment		
			>0–2 Years	2–3 Years	3–5 Years
Per-capita consumption (2005 Rands)	594.5**	466.4	486.6	548.1	749.1
	(1098.1)	(692.0)	(906.2)	(713.1)	(1485.7)
Days since application	1803.1*	1675.7	1775.6	1717.6	1908.0
	(761.1)	(1302.1)	(865.9)	(714.9)	(670.2)
Family labor	0.787**	0.436	1.156	0.496	0.658
	(1.291)	(0.942)	(1.789)	(0.791)	(0.870)
Gender of household head (1 if male)	0.754**	0.667	0.725	0.719	0.816
	(0.431)	(0.472)	(0.448)	(0.451)	(0.389)
Education of household head (years)	6.447*	5.843	5.088	7.193	7.217
	(4.880)	(4.496)	(4.058)	(5.084)	(5.198)
Farming experience (mean of household in years)	1.594	1.464	1.361	1.292	2.108
	(3.706)	(3.784)	(3.754)	(2.213)	(4.581)
Household size	6.060	6.138	6.675	5.852	5.599
	(3.532)	(3.732)	(4.013)	(3.192)	(3.192)
Household relocated to participate	0.208**	0.0727	0.206	0.126	0.283
	(0.406)	(0.260)	(0.406)	(0.333)	(0.452)
Distance to nearest neighbor (km)	−0.440	−1.149	−0.768	−0.0627	−0.370
	(2.744)	(2.328)	(2.841)	(2.717)	(2.628)
Restitution overlap (1 if application overlaps with restitution program)	0.248	0.258	0.265	0.238	0.237
	(0.432)	(0.438)	(0.442)	(0.428)	(0.427)
Entry time (months elapsed from program start date to decision to apply)	26.76	18.54	23.94	28.21	28.86
	(33.93)	(29.49)	(30.03)	(25.22)	(43.13)
Pipeline time to sales contract (months)	25.37	.	30.94	22.18	22.34
	(29.20)	(.)	(29.34)	(20.51)	(34.02)
Final pipeline delay time (months)	3.810	.	4.384	3.263	3.673
	(8.128)	(.)	(8.303)	(4.966)	(9.830)
n	448	1202	176	139	163

Notes: The stars in column 1 indicate whether the differences in means for the relevant variables between the treated and pipeline control groups are statistically significant: ** = significant at1 %level; * = significant at 5%level.

navigated the administrative approval process. We therefore checked for the existence of this document for sampled projects, screening out those for which the designation memo did not exist. Projects screened out at this level were replaced by newly randomly selected projects and the process was repeated until the desired sample of stage 2d administratively-approved projects was obtained. In the remainder of this section, we first look at descriptive statistics from this sample, examining characteristics of beneficiaries and non-beneficiaries. We further break down the beneficiary group by treatment duration, as variation in duration is the center of our primary identification strategy. The section closes by using a conventional binary treatment estimator to gauge the average impact of land on beneficiaries relative to approved applicants still stuck in the pipeline at the time of the survey.

3.1. Descriptive statistics

Table 1 shows the means and standard deviations of the key variables used in our analysis. Since we are interested in measuring the impact of LRAD on living standards, our primary outcome variable is monthly per capita consumption expenditures.[10] Table 1 shows that mean per-capita consumption in treatment households is 128 rands (or 28%) higher than mean consumption for the untreated, pipeline control group. This difference is significant at the 1% level.

As already mentioned, our primary analysis will focus on those LRAD applicants who had already exited the pipeline and had received land for some period of time prior to the survey. Table 1 disaggregates these treated households into treatment terciles based on the duration of time since they had received their land transfers. As can be seen, mean per-capita consumption for treated households that had enjoyed their land transfer for less than two years is a modest 4% higher than that of the untreated pipeline control group. Households observed 2–3 years after the transfer have 18% higher consumption than pipeline control households, while households with more than 3 years experience with the transferred lands have expenditures that are 61% higher.

These data were not generated by a controlled experiment, and we need to be wary of possible confounding factors that invalidate these unconditional inter-group differences as causal impact estimates. One possible confound is that pipeline control households or households with low treatment duration applied later to the program. Application time may proxy for eagerness and expected gains from the program. Table 1 displays information on the timing of entry into the pipeline (d_1). As can be seen, treated households on average applied about 8.2 months earlier than did control households. This difference is statistically significant. The differences in application date are less pronounced amongst the treated population. Households with less than 2 years of treatment applied about 4.5 months later than households with higher treatment durations. This difference is statistically significant. The entry time delays for households with intermediate treatment levels (2–3 years) are nearly identical to that of households with more than 3 years of treatment. Together, these results suggest that any statistical approach needs to control for these differences.

Another possible confound is that better applicants may traverse the full transfer process more quickly. As mentioned in the prior section, we lack data on the time it took each applicant to receive full District Screening Committee approval. For most treated applicants, we do have the full pipeline time from application to signing of the sales contract (which subsumes the delay to full District Screening approval). As can be seen in Table 1, the pipeline time to sales contract measure is

[10] Results are unaffected if we normalize consumption by adult equivalents instead of by the number of household members. We also do not normalize expenditures by a poverty line, because there is some controversy in the case of South Africans to which is the most appropriate line to use (Woolard and Leibbrandt, 2007).

about 9 months longer for applicants with less than 2 years of treatment compared to the other treated groups. This pipeline measure is almost identical (22 months) for treated applicants with 2–3 years and more than years of treatment duration. It is of course possible that the longer delays experienced by applicants with less than 2 years of treatment reflects administrative congestion (as these applicants applied later) rather than applicant weakness. Nonetheless, in our more conservative identification strategy, we will also match on this total pipeline delay time as well as on the application delay.

Other differences among treated households are also apparent in Table 1; namely gender (terciles 1 and 2 and terciles 1 and 3), geographic mobility (terciles 2 and 3) and spatial connectedness proxied by the geographical distance to the households nearest neighbor (terciles 1 and 2). Similar patterns hold for the overall treatment-control comparison with the exception that education also emerges as a statistically significant difference. We now consider propensity score estimation techniques that allow us to control for these potentially confounding factors.

3.2. Binary impact estimates: average treatment effects on the treated

Before turning to the continuous treatment estimates that are the centerpiece of this paper, we begin with methodologically more familiar binary ATT impact estimates. For this analysis, we focus on LRAD applicant households that had received District Screening Committee approval (step 2d), denoting those who had received land by the survey date as treated ($D_i = 1$) and those who were still waiting in the pipeline as untreated ($D_i = 0$). Because LRAD land grants were not distributed through a controlled experiment, we need to be mindful of possible differences between the treated and the untreated groups, including date of application to the program, as discussed above. To control for these differences, we employ binary propensity score estimation procedures. The first two columns of Table 2 display the results from two different logit propensity score models for this binary treatment model. In addition to application date, the control variables for the propensity score regression include standard human capital variables.

Table 2
Determinants of treatment.

	Binary treatment[a]		Continuous treatment[b]	
	Model 1	Model 2	Core	Conservative
Gender of household head (1 if male)	0.3533*	0.3711**	0.0555*	0.0445
	(0.146)	(0.144)	(0.023)	(0.024)
Education of household head (years)	−0.8671***	−0.7465**	−0.0137	0.0050
	(0.259)	(0.254)	(0.012)	(0.009)
Education squared	0.0063	0.0061	0.0013*	0.0002
	(0.004)	(0.004)	(0.001)	(0.001)
Farming experience (mean of household in years)	−1.8195**	−1.7313**	0.0534**	0.0250**
	(0.587)	(0.568)	(0.019)	(0.009)
Farming experience squared	−0.0024	−0.0028	−0.0001	0.0000
	(0.002)	(0.002)	(0.000)	(0.000)
Education × farming experience	0.1462	0.1456	−0.0055*	−0.0032**
	(0.081)	(0.079)	(0.003)	(0.001)
Household size	0.0012	0.0212	−0.0057	−0.0073*
	(0.020)	(0.019)	(0.003)	(0.003)
Household relocated to participate	1.3212***	1.3681***	0.0325	0.0378
	(0.205)	(0.200)	(0.025)	(0.025)
Days since application	44.7874***	43.9662***	0.0005***	0.0001***
	(3.745)	(3.656)	(0.000)	(0.000)
Days squared	−3.0578***	−2.9925***	−0.0000***	
	(0.253)	(0.247)	(0.000)	
Days × education	0.1076**	0.0906**	0.0000	
	(0.034)	(0.033)	(0.000)	
Days × farming experience	0.2433**	0.2335**	−0.0000*	
	(0.078)	(0.076)	(0.000)	
Days × farming experience × education	−0.0188	−0.0186	0.0000*	
	(0.011)	(0.011)	(0.000)	
Family labor	0.3424***			
	(0.064)			
Pipeline time to sales contract (months)				−0.0042***
				(0.001)
Pipeline time squared				0.0000***
				(0.000)
Pipeline time × farming experience				−0.0009**
				(0.000)
Pipeline time × education				−0.0002*
				(0.000)
Pipeline time × education × farming experience				0.0002***
				(0.000)
Distance to nearest neighbor (km)				−0.0006
				(0.001)
Constant	−164.4611***	−161.9010***	−0.2611*	0.3123***
	(13.878)	(13.563)	(0.127)	(0.053)
Observations	1650	1650	438	379
Log-likelihood	−732.5	−747.6	73.93	89.81
x^2	464.7	434.5	92.53	74.67

Notes: Standard errors are reported in parentheses and are robust to clustering.
[a] Dependent variable equals 1 if treated, zero if pipeline control.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

Table 3
Average treatment effect on treated: percentage change in per-capita consumption.

Kernel	Bandwidth	ATT	*t*-Ratio Analytical	*t*-Ratio Bootstrapped[a]
Gaussian (fixed bandwidth)	0.05	28.36	2.04	2.41
Gaussian (optimal bandwidth)	0.26	22.77	1.72	1.84
Epanechnikov (optimal bandwidth)	0.59	22.34	1.69	1.88
Quartic (optimal bandwidth)	0.69	22.45	1.69	1.69
Rectangular (optimal bandwidth)	0.23	25.16	1.85	1.96
Tricube (optimal bandwidth)	0.50	24.39	1.82	1.76

Notes:
[a] Bootstrapped standard errors are calculated over 250 replications.

Letting p_i denote the propensity score, define S_p as the region of common support of p_i between the $D = 1$ and $D = 0$ distributions, and let N_1 denote the set of households that have already received land through LRAD, and N_0 denote the set of households still in the pipeline. Further denote n_1 as the number of treated households falling into the common support region of the estimated propensity score density; i.e., the number of households falling into the set $N_1 \cap S_p$. Our matching estimator is then given by

$$\delta = (n_1)^{-1} \sum_{i \in N_1 \cap S_p} \left(y_{1i} - \hat{\mathrm{E}}(y_{0i} | D_i = 1, p_i) \right) \tag{1}$$
$$= (n_1)^{-1} \sum_{i \in N_1 \cap S_p} \left(y_{1i} - \sum_{j \in N_0} \omega(i,j) y_{0j} \right)$$

where $i \in N_1 \cap S_p$ denotes the ith treated household from the set of households with common support on p_i. The second term in this expression serves as a matched substitute for the outcomes of a randomized-out household of the treatment group, where the imputed counterfactual outcome $\sum_{j \in N_0} \omega(i,j) y_{0j}$ is a kernel-weighted average over the set of possible matches, with weight function:

$$\omega(i,j) = K\left(\frac{\mathbf{x}_j' \beta - \mathbf{x}_i' \beta}{h_n} \right) / \sum_{k \in N_0} K\left(\frac{\mathbf{x}_k' \beta - \mathbf{x}_i' \beta}{h_n} \right) \tag{2}$$

where K is a kernel function, h_n is a bandwidth parameter and $\mathbf{x}_i' \beta$ is the log of odds ratio.[11]

Looking more closely at the propensity score estimates shown in Table 2 we see that the intended targeting of women by the LRAD program does not seem to be borne out by the data, as approved female-headed households have a lower probability than male-headed households of finally gaining access to LRAD grants than do male-headed households. The estimated coefficients also indicate that farming experience and education appear to combine non-linearly with the timing of entry into the applicant pool to significantly affect the treatment probability. While the rationale behind these interactions is not apparent, the reported specifications dominate a more parsimonious model without the interaction terms (not reported here), and achieve a correct prediction rate of 76%. The reported specifications also adequately balance the data by conventional tests (results available from the authors). The second propensity score specification is identical to the first except that it eliminates the family labor stock variable. While matching on this variable is arguably warranted as family labor is an important complementary resource to land, it is arguably itself affected by the treatment. Reliance on this second propensity score model has no impact on the estimated treatment effects.

[11] Under our logit specification, $p_i = e^{\mathbf{x}'\beta} / \left(1 + e^{\mathbf{x}'\beta}\right)$, and $1 - p_i = 1 / \left(1 + e^{\mathbf{x}_i'\beta}\right)$ and thus the log odds ratio is given by $\ln(p/(1-p)) = \mathbf{x}_i'\beta$. While it is usual to match directly on p_i, we here follow Heckman and Todd (2009) and match on log odds as this procedure is robust for samples in which the true sampling weights are unknown, as they are in our study.

Based on the model 1 propensity score estimates, Table 3 reports the estimated impacts of treatment on household per-capita consumption for five different kernel functions.[12] The first estimate, based on a standard Gaussian kernel with a fixed global bandwidth of 0.05, yields an estimate average treatment effect of 28%. The other estimates tall employ optimal bandwidths computed according to the approach of Silverman (1986). For the Gaussian kernel, the estimated treatment effect declines to 23% when an optimal bandwidth is employed. Results for the other kernels shown in the table range from 22% to 25%.

Contingent on the identifying assumption that untreated, but administratively approved households, are a valid control group for treated LRAD beneficiaries once matched on application date and routine human capital variables, these estimates indicated that on average LRAD land transfer grants boost household living standards by at least 20%. By way of comparison, Behrman et al. (2005) estimate that 7 years of monthly cash transfers in Mexico's Progresa program[13] would increase *total future* income by only 8%. Of course in the short-run (while receiving transfers), cash transfer beneficiaries experience substantial total income increases, as much as 20% in the case of Progresa program, an amount still below the estimated binary treatment effect of the LRAD asset transfer program. While these average impacts are sizeable, they tell us little about whether and how these impacts change over time as beneficiary households gain more experience exploiting the productive assets made available to them by the LRAD program.

4. The impact dynamics of land transfers: duration analysis of treated households only

Given the quasi-experimental nature of our study design, beneficiary households received land grants at different points in time. Within our sample, some treated households had received land five years before the survey date, while others had only recently received transfers. The impact on the level of economic well-being of the latter group might well be negligible if no income had yet been generated by the newly acquired land. Indeed, if these households were simultaneously co-investing in the newly acquired land, the short run impact on household income and consumption could even be negative. In this case, the Section 3 estimated average treatment effect on the treated would present a data-weighted average of zero or negative impacts for recent beneficiaries and perhaps positive impacts for beneficiaries with longer land access.

In addition to this possible first year dip in living standards, there are two other reasons why the impact of an asset transfer may change over time. First, the beneficiary may experience a learning effect with technical and entrepreneurial efficiencies improving over time. Second, and consistent with the theoretical literature on asset inequality and poverty discussed earlier, the asset transfer may create a crowding-in effect if the beneficiary further invests in the new enterprises made possible by the LRAD grant. Whether this additional investment occurs because of improved access to financial markets, or because of learning effects, the overall impacts on beneficiary well-being are likely to be large in the long run especially if LRAD transfers suffice to lift households over the sort of critical threshold asset level that figures prominently in the poverty traps literature.[14] It is these second round or multiplier effects that are likely to distinguish asset transfer programs from other anti-poverty policy instruments.

[12] Matching occurs over the region of common support of the log–odds ratio, with a further 2% trimming rule imposed.

[13] Excluding administrative costs, the direct cost of these cash transfers would be approximately $US4600, an amount similar to the direct costs of an LRAD grant.

[14] Contributions to this literature that focus on critical minimum asset thresholds and thus suggest a strong role for asset redistributions a la LRAD include Galor, Carter and Barrett (2006), Carter and Lybbert (2012), Moav and Vollrath (2009), and Mookherjee and Ray (2002).

If these observations are correct, then the duration response function—meaning the relationship between program impact and duration of time since the asset was transferred—is unlikely to be a simple step function that can be approximated with a binary treatment estimate. The binary treatment estimates presented in the prior section indicate that land redistribution boosts per-capita living standards by an average of 22% to 28% relative to the pipeline control group. But given that some of the treated population had enjoyed access to their new land for less than a year, and others for more than 5 years, it is not at all clear whether the 22% to 28% range is the long-run, policy relevant treatment effect or simply some mix of short and long-run impacts. It is possible that long-run impacts could be higher for the reasons just discussed. It is also possible that the impacts could dissipate, as they would in the case if the land is abandoned by their beneficiaries, as has happened elsewhere (Barham and Childress, 1992). Our main goal in this paper is to estimate the impact dynamics and duration response function and recover both the long-run impact of land transfers and their time path. Both are of particular relevance from a policy perspective.

4.1. Continuous treatment estimator

A natural starting point for this analysis ordinarily would be to consider a random coefficients model (Heckman and Robb, 1985). However, if treatment status is non-linear in beneficiary characteristics, then the minimum mean square error approximation to the underlying conditional expectation function (CEF) does not have a straightforward interpretation, because the regression coefficients in such a model would actually represent a matrix-weighted average of the gradient of the CEF (Chamberlain and Leamer, 1976). This problem can be overcome if the CEF of our treatment duration variable were restricted to be linear, as in Angrist (1998), but then we would still require a large number of observations for each value of treatment duration to justify this approach. An alternative approach that does not necessitate such an assumption is the extension of the propensity score approach developed by Hirano and Imbens (2004).

We begin by restricting attention to the sample of households in the treatment group, $i \in N_1$. We then postulate a duration response function $y_i(d)$ for all $d \in D$ given that $i \in N_1$; i.e., each household could have any potential outcome from the set D depending on its duration of treatment. When treatment status is binary, we have $D = \{0,1\}$, but here we let $D = \{d_0, d_1\}$. In the empirical implementation, we measure duration as the number of days between the final land transfer and the date of the survey.

The evaluation problem of course results from the fact that each household realizes exactly one outcome, that associated with its actual duration in the LRAD program $y_i = y_i(D_i)$, where $D_i \in [d_0, d_1]$. However, under the continuous treatment case, the problem is further complicated by the fact that there is more than one possible counterfactual duration. We therefore define the impact of LRAD in this continuous case in terms of an average duration response function, $\mu(d) = E[y_i(d)]$. Our goal then is to uncover non-constant treatment effects by taking the difference between this average and some benchmark level of treatment:

$$\theta(d) = \mu(d) - \mu\left(\tilde{d}\right) = E[y(d)] - E\left[y\left(\tilde{d}\right)\right] \qquad \tilde{d}, d \in \mathcal{D} \tag{3}$$

where $\tilde{d}$ serves as the benchmark duration.[15] In our empirical estimates, we set $\tilde{d}$ to be the lowest length of exposure observed in the data.

As in the binary approach, valid identification depends on an independence assumption regarding treatment assignment. Following Imbens (2000), we define weak unconfoundedness[16]:

$$y(d) \perp D | \mathbf{x} \qquad \forall d \in D.$$

To fix ideas, define $r(d,x)$, as the conditional density of treatment duration given the covariates.

$$r(d,x) = f_{D|\mathbf{x}}(d,x) \tag{4}$$

and define a generalized propensity score (GPS) $R = r(D,X)$. Using this framework, Hirano and Imbens (2004) then show that assignment to treatment duration (or equivalently, the timing of treatment), is unconfounded when $f_D(d|r(d,x), Y(d)) = f_D(d|r(d,X))$.

Under the assumption of unconfoundedness, Hirano and Imbens (2004) then show how the GPS can be used to identify $\mu(d)$. Two steps are involved in this proof. First, using Bayes rule and their Theorem 1, they show that in estimating the conditional expectation of the outcomes, all relevant information about the conditional density of the treatment is controlled for by directly conditioning on the treatment level D and the generalized propensity score $\hat{R}_i$. Second, to estimate the duration response function, $\beta(d,r(d,X))$, at a particular level of the treatment they average this conditional expectation over the GPS at that particular level of the treatment, $\mu(d) = \mathrm{E}[\beta(d, r(d,X))]$ and then by iterated expectations, $\mathrm{E}[\beta(d, r(d,\mathbf{x}))] = \mathrm{E}[\mathrm{E}[y(d)|r(d,\mathbf{x})]] = \mathrm{E}[y(d)]$ obtains.[17] Thus knowledge of $\beta(D,R)$ will identify the average duration response function, under weak unconfoundedness conditional on the GPS.

To implement this estimator, we follow Hirano and Imbens (2004) and assume that the conditional density of the duration of treatment is normally distributed with

$$D_i|\mathbf{x}_i \sim N\left(\mathbf{x}_i\beta, \sigma^2\right). \tag{5}$$

These parameters can be estimated by maximum-likelihood, and the estimated GPS recovered as:

$$\hat{R}_i = \frac{1}{\sqrt{2\pi\hat{\sigma}^2}} exp\left\{-\frac{1}{2\hat{\sigma}^2}\left(D_i - \widehat{\mathbf{x}_i\beta}\right)^2\right\}. \tag{6}$$

To estimate the duration response function, we model the conditional expectation of y_i, as a flexible function of D_i and R_i

$$\begin{aligned}\beta(D_i, R_i) = E\left[Y_i|D_i, \hat{R}_i\right] = \; & \alpha_0 + \alpha_1 D_i + \alpha_2 D_i^2 + \alpha_3 \hat{R}_i \\ & + |\alpha_4 \hat{R}_i^2 + \alpha_5 D_i \hat{R}_i\end{aligned} \tag{7}$$

Eq. 7 is then estimated by OLS.[18] Once we have estimated the parameter vector α, we can then recover the average duration response function $\mathrm{E}[y(d)]$. Recall that $\mathrm{E}[y(d)]$ is identified for particular levels of duration, so the average must be taken over all households (irrespective of their realized treatment duration) at each duration level d. By

[15] To simplify the notation, we drop the i subscripting when making reference to realized outcomes or treatment levels.

[16] This is essentially a weaker version of the Rosenbaum and Rubin's (1983) "strong ignorability" assumption, generalized to multi-valued treatments. Hirano and Imbens (2004) refer to this assumption as weak unconfoundedness as it does not require joint independence of all potential outcomes, $\{y(d)\}_{t \in [t_0, t_1]}$ but rather that conditional independence holds for each value of D. For alternative approaches to non-binary treatments, see Behrman et al. (2004) and Imai and van Dyk (2004).

[17] Importantly, note that under this approach, the averaging that is used to construct $\mu(d)$ takes places over the GPS score evaluated at the treatment level of interest, $r(d,x)$, and not over the GPS itself.

[18] The regression function $\beta(d,r)$ does not have a causal interpretation. In particular, the derivative with respect to the treatment level d does not represent an average effect of changing the level of treatment for any particularly subpopulation. We also experimented with various specifications for this regression and conclude that not much additional explanatory power is added by including higher than second-order polynomials in D and $\hat{R}$.

changing the treatment level at which the averaging takes place, we recover an estimate of the entire duration response function.[19] This procedure gives a treatment effect estimator of the form:

$$\hat{\psi}(d) = E[\widehat{Y(d)}] = \frac{1}{n_1}\sum_{i=1}^{n_1}(\hat{\alpha}_0 + \hat{\alpha}_1 \cdot d + \hat{\alpha}_2 \cdot d^2 + \hat{\alpha}_3 \cdot \hat{r}(d, \mathbf{x}_i) + \hat{\alpha}_4 \cdot \hat{r}(d, \mathbf{x}_i)^2 + \hat{\alpha}_5 \cdot d \cdot \hat{r}(d, \mathbf{x}_i)) \quad (8)$$

Finally, to compute a non-constant effect of treatment on treated, we estimate.

$$\hat{\theta}(d) = \hat{\psi}(d) - \hat{\psi}\left(\tilde{d}\right) \qquad \forall d \in D, \quad (9)$$

where $\tilde{d}$ has been fixed at the benchmark level discussed earlier.

4.2. Generalized propensity score estimates and balancing tests

As noted above, the continuous treatment variable is the duration of exposure to the LRAD program, measured as the normalized number of days elapsed since the date of transfer of the land.[20] Table 2 presents maximum likelihood estimates of the conditional distribution of this treatment variable under both the core and statistically conservative identification. In addition to the human capital variables, the core strategy controls for application delay, whereas the conservative strategy further controls for total pipeline time up to the signing of the land sales contract. Both models satisfy the normality assumption as the Kolmogorov–Smirnov test of normality of the conditional errors is passed at the 1% level.

The core model results in Table 2 not surprisingly show that application date is correlated with duration of treatment. Including it, and the other variables (such as farming experience, which is also a significant determinant of treatment) allows us to match these variables in the continuous treatment impact estimates. Similarly, the conservative model estimates show that, as expected, longer pipeline delays reduce the duration of treatment. Before turning to the impact results themselves, we first test the ability of the GPS estimates to balance the data.

To test the GPS balancing property, we follow Hirano and Imbens (2004) and Bia and Mattei (2008). We first partition the support of D into three mutually exclusive intervals, denoted as $G_1,\ldots,G_3$. Within each treatment interval G_k, we compute the GPS $r\left(d_{G_k}, \mathbf{x}_i\right)$ at the mean of the interval $d_{G_k} \in G_k$. Then, for each of the three intervals we estimate the GPS at these treatment interval means d_{G_k} and then discretize the distribution of the GPS evaluated at this representative point. In our model, we chose six mutually exclusive blocks, denoted by $B_1^{(k)}, \ldots, B_6^{(k)}$. Within each interval $B_j^{(k)}$ for $j = 1,\ldots,6$ and $k = 1,2,3$, we compute the difference in means for each covariate across different treatment intervals, but in the same GPS interval (i.e., j is held constant while k is varied). This results in six mean differences for each $d_{G_k} \in G_k$. This information is then collapsed into a single metric, by taking a weighted average of the differences at each representative point, where the weights are equivalent to the number of observations within each block $B_j^{(k)}$. This procedure is repeated for each covariate. In a final step, these weighted averages are then used to construct test statistics. We conducted this test of each of the specifications of the GPS reported in Table 2. Table 4 reports Bayes factors that are computed off of the weighted averages that are based on the conservative core model. Decisive rejection of the null that the data are balanced requires a minimum Bayes factor of less than 0.01. Overall, the model is well balanced as the lowest Bayes factor of 0.3364 falls within an acceptable range of Jefferys' order of magnitude criterion. In particular, time since application and total time in pipeline (from application to sales contract) is balanced by the model.

Table 4
Bayes factor tests of equality of conditional covariate means.

Variable	Normalized treatment intervals		
	[0,0.37]	[0.37,0.47]	[0.47,1]
Gender of household head (1 if male)	6.1213	1.4979	6.8357
Education of household head (years)	4.4726	2.2034	3.9089
Education squared	2.4537	1.5833	4.6867
Farming experience (mean of household in years)	6.7236	3.8165	6.6694
Farming experience squared	6.637	3.0129	6.5379
Education × farming experience	6.2594	4.8765	5.9455
Household size	2.55	6.203	5.0907
Household relocated to participate	5.7877	0.9285	2.683
Days since application	2.1685	6.2083	1.4708
Pipeline time to sales contract (months)	6.6155	1.8842	3.745
Pipeline time to sales squared	6.0723	1.0302	0.7976
Pipeline time × farming experience	6.0176	3.5222	6.5296
Pipeline time × education	2.4063	6.0603	6.02
Pipeline time × education × farming experience	5.3198	5.6462	6.5704
Distance to nearest neighbor (km)	6.4032	6.1707	6.3807

4.3. Estimated impact dynamics

The solid line in Fig. 2 graphs the duration response function, $E[\widehat{Y(d)}]$, estimated using the core model parameters reported in Table 5. Eq. 7 was estimated using the natural logarithm of per-capita monthly expenditures as the dependent variable.[21] The corresponding 95% confidence interval (calculated using bootstrap re-sampling) is shown by dotted lines. The dashed horizontal line is the benchmark, null treatment estimate, $\hat{\mu}\left(\tilde{d}\right)$. By coincidence, this line also marks the level of the two-dollar a day international poverty line converted to South African rand using the PPP exchange rate for 2006. This two-dollar a day poverty line (which is equivalent to about 250 rand per-person, per-month at the PPP exchange rate) is substantially lower than the national poverty line of 430 rand per-person, per-month which has been suggested by the South African Treasury.[22]

As is apparent from Fig. 2, the continuous treatment impact estimator is quite different from the simple step function that would indicate binary treatment estimates to capture all the relevant dimensions of program impact. Noticeable is the sharp drop in consumption over the first year of exposure, and then a period of quick recovery over the second year. Beyond the second year, the estimated average treatment effect rises further to levels that are approximately 50% higher than the international poverty line and the null treatment benchmark. As can be seen, increases of this magnitude take households that are in the vicinity of the poverty line and lift them well above it. In addition, with an average of 5 individuals per-family, the estimated increase in per-capita monthly expenditures implies a total annual household expenditure increase of some 7200 rand per-year. With a typical grant of 20,000 rand, it takes only a few years for these total returns to swamp the direct monetary value of the grant. Before considering further the meaning of these results, we first check their robustness under more conservative identification assumptions.

4.4. Robustness check using conservative identification strategy

Under our basic identification strategy, both binary and continuous treatment estimators reveal that land transfers had significant impacts

[19] We estimate standard errors and confidence intervals for each point along the duration response function using bootstrap methods.

[20] We normalize the treatment variable dividing days treated by the maximum number of days a household could have been exposed to the program.

[21] We also estimated Eq. 7 in levels, implying that each treatment duration has the same absolute effect on per-capita expenditure. These results were qualitatively the same as the log estimates shown here, except that absolute average effects were somewhat larger than those implied by the log changes.

[22] See the comprehensive benchmarking exercises reported in Woolard and Leibbrandt (2007) for further background information on the measurement and reporting of poverty in South Africa.

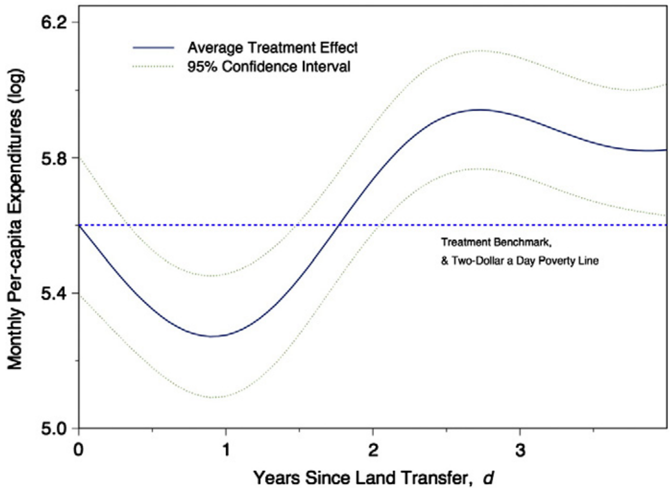

Fig. 2. Duration response function.

on family economic well-being. While both the binary and continuous treatment methods rely on a filtered pipeline strategy and also match on time of entry to the pipeline, we might still worry that at least some of the delay in exiting the pipeline may be related to beneficiary or project characteristics and to expected impacts.

As described in Section 2 above, we can check the validity of the basic results by employing a statistically more conservative strategy that further controls for time in the pipeline up until the signing of the final sales contract. This additional control cuts into the variation available to identify the impact of receiving land, leaving only a variation in what Fig. 1 calls "legal transfer delay time", d_4 (and variation in the survey date) to identify program impacts. While our review of LRAD implementation suggests that this additional matching is throwing away valid, exogenous variation in program treatment, this section looks to see if our findings are robust to the more statistically conservative strategy that identifies based only on legal transfer delays.

As a prelude to that robustness check, we first establish that legal transfer delays are unrelated to things to which they should not be (e.g., characteristics like education that are predictors of individual productivity), and are related to things that they should be (e.g., administrative peculiarities that are the result of the apartheid history of expropriation). Table 6 reports the regression of legal transfer delay on a suite of individual characteristics plus an indicator variable that signals when a particular project was more likely to have been delayed at the last minute by a competing restitution claim.[23] As can be seen, none of the individual characteristics statistically predict legal transfer delays, whereas the dummy variable indicating that a property was more likely to be subject to a competing land restitution claim is highly significant, increasing the legal transfer delay time by 4.7 months. While this simple test cannot conclusively show that legal delay meets the conditional independence assumption needed for identification in our impact model (as it of course cannot test for independence from unobserved factors), it is consistent with that story and illustrates that unexpected restitution claims generated some portion of the identifying variation.[24]

Table 2 presents the estimation results for this more conservative estimation strategy. Using these new estimates, the continuous treatment

Table 5
OLS estimates of the conditional expectation of consumption given treatment duration and generalized propensity score.

	Core	Conservative
	b/se	b/se
Treatment duration, D_i	−0.6591	−1.4524
	(0.866)	(1.455)
D_i^2	0.4629	1.2549
	(0.920)	(1.520)
GPS, $\hat{R}_i$	−1.7366***	−1.0461
	(0.449)	(0.557)
$\hat{R}_i^2$	0.5788***	0.2930
	(0.169)	(0.197)
$D_i \times \hat{R}_i$	1.4317***	1.2599**
	(0.334)	(0.400)
Constant	6.1941***	6.1637***
	(0.211)	(0.230)

Notes:
Std. errors reported in parenthesis, robust to individual clustering.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

Table 6
OLS estimates of determinants of final delay time.

	Legal transfer
	Delay, d_4
Pipeline time to sales contract (months)	−0.0172
	(0.042)
Entry time (months elapsed from program start date to decision to apply)	−0.0187
	(0.013)
Gender of the household head (1 if male)	0.4159
	(1.028)
Education of the household head (years)	0.3345
	(0.382)
Farming experience (mean of household in years)	0.1452
	(0.396)
Household relocated to participate	0.7632
	(1.055)
Distance to nearest neighbor (km)	0.0532
	(0.032)
Education squared	−0.0192
	(0.025)
Farming experience squared	−0.0261
	(0.014)*
Education × farming experience	0.0907
	(0.053)**
Household size	0.1445
	(0.138)
Pipeline time to sales contract (months) squared	0.0002
	(0.000)
Pipeline time × farming experience	0.0203
	(0.015)
Pipeline time × education	−0.0009
	(0.004)
Pipeline time × education × farming experience	−0.0041
	(0.002)
Restitution overlap (1 if application overlaps with restitution program)	4.6920***
	(1.107)
Constant	0.0894
	(1.703)
Observations	378
R-squared	
BIC	2730.0

Notes:
Standard errors in parentheses.
* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

[23] The indicator takes on the value of 1 for LRAD applications initiated prior to the closing of the land restitution window, as discussed in Section 2 above.

[24] One could make the assumption that unobservable variables that are likely to predict treatment duration can always be proxied for by the inclusion of d_{23}. Though this would be a strong assumption, it is probably quite plausible: for example, latent farming ability would undoubtedly affect the speed with which an applicant traverses the approval process. Since the variable *Approval time* is also insignificant in Table 6, any other omitted variable likely to be important for predicting treatment duration will likely be insignificant too, if it is correlated with d_{23} (which it should be). Ultimately however, this must remain as an assumption so we remain agnostic about the extent to which d_{23} can proxy for unobserved variables that might affect *Delay time* and note merely that the robustness check reported in Table 6 is the best we can do with the available data.

effects under generalized propensity score matching are again calculated using Eq. (8). A graph of the estimated duration response function using the results from the more conservative strategy is virtually indistinguishable from Fig. 2, except that, as expected, the confidence bands are a bit wider (figure available from the authors). However, even under this conservative strategy the long run impacts remain significantly different from the benchmark treatment duration.

5. Discussion

Under both identification strategies, our continuous treatment estimator indicates that after an initial dip, the impacts of land transfer rise steadily and after 3 to 4 years plateau at a level that implies long-term increases in per-capita expenditures of approximately 50%.[25] This temporal pattern is consistent with the theory of costly asset inequality, and measured impacts of this magnitude imply substantial co-investment and improved returns to other beneficiary resources. While impact evaluation of agricultural projects is often criticized for focusing on intermediate outcomes (investment, yields or crop income) and ignoring bottom line impacts on living standards, we have something of the opposite problem here. Because LRAD beneficiaries are spread across the country and pursue a wide variety of activities, there is no common crop that can be easily used as an outcome variable to shed further light on the learning, productivity and investment patterns that underlie the estimated living standard dynamic.[26] Future work to unpack the impacts of asset transfers would certainly be warranted.

While there is still much to learn about how asset transfer programs work, comparison with estimated income increases predicted to result from cash transfer programs is again instructive. As discussed above, Behrman, Sengupta and Todd calculate that cash transfers raise earnings by 8% in the long-run, implying an approximately 2% increase in per-capita consumption. Similarly, the Agüero, Carter, and Woolard (2011) study of South Africa's Child Support Grant calculates that the early childhood nutritional impacts of that cash grant would increase adult earnings by some 3% in the long-run. Where it is possible to make these comparisons, the long-run impacts of these cash transfer programs, though not insignificant, are one to two orders of magnitude smaller than the estimated long-run effects of LRAD transfers. It is of course possible that monthly cash grants could also crowd-in additional investment and intermediate income growth. The only evidence we know on this point is the work by Gertler et al. (2012) who find that Progresa cash transfers had long-run investment impacts on the order of about 18%.

In summary, the estimated duration response function accords with what one would expect of an asset transfer program like LRAD. More generally, as King and Behrman (2009) have argued, a variety of factors might account for why there could be a lag, or more gradual realization of the benefits of anti-poverty programs. This is especially the case for a program like LRAD, which is aimed not only at affecting rural livelihoods directly, by also changing learning opportunities and investment incentives.

6. Conclusion

The largely theoretical literature on asset inequality has long suggested that asset transfers—such as land redistribution—can be an effective anti-poverty tool.[27] Asset transfer programs are confronted by fewer questions about work disincentives than are cash transfer programs. In principal, these programs also have the potential to generate very high rates of return if they succeed in unlocking the productive potential of the poor by improving their market access and perhaps getting them over the sort of critical minimum threshold hypothesized by the poverty traps literature.

Despite this promise, well-identified empirical evidence on efficacy of land redistribution has been scarce, in no small part because most reforms arise endogenously, either as a response to, or as a cause of, conflict. This fact places limits on the uses of experimental approaches to identify impacts. For this paper, we have been able to explore a low conflict situation (land redistribution in post-apartheid South Africa) and exploit the fact that the implementation of its market-assisted land reform program generated something akin to a natural experiment, allowing identification of the impact of land transfers on the economic well-being of poor and near poor households.

Binary treatment effect estimates, which compare treated with approved but untreated households stuck in the administrative pipeline, show that land transfers boost household living standards by 25%. More interestingly, our continuous treatment estimates, which are based on exploiting different treatment durations among the sets of LRAD applicants who actually received land transfers, show that living standards initially dip with the land transfers, but then after three years rise to levels that imply a 50% increase in living standards of the treated households who entered the program with poverty line standards of living. Both the temporal patterns of this impact, and its magnitude, are consistent with the theoretical literature on asset transfers and their potential to crowd-in investment, learning and income increases beyond what would be expected from the direct transfer alone. These results are robust to a statistically more conservative identification strategy.

So does land redistribution make for good public policy if the goal is to reduce rural poverty? Compared to cash transfers, where it is possible to “just give the poor the money” (Hanlon et al., 2010), asset transfers are clearly more complicated and have a more limited scope as not everyone can be a successful small-scale farmer. It also remains to be determined if the increases in family well-being detected by the South African land redistribution program spill over into the kinds of investment in child human capital detected in conditional and unconditional cash transfer programs. Subject to these limitations, the impacts detected here would seem to motivate further experimentation with asset transfer programs.

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[25] A similar temporal impact pattern is found in the Tjernström et al.'s (2014) study of a small farm development program in Nicaragua.

[26] There are very few crops grown consistently across the sample. Extracting the most widely grown crop (maize) results in a very small matched sample that lacks the power to detect significant effects on yields. However, a simple aggregation of total yields of agricultural output within households does have power and shows an increasing pattern that is broadly consistent with the estimated per-capita expenditure dynamic, but it is unclear what interpretation to give to this finding. We therefore have not interrogated these results in any depth but would be happy to provide them upon request. In terms of investment indicators, the available survey data includes capital stock measures, but lacks the price information to aggregate them into an indicator that could be analyzed.

[27] While some models focus on inequality per se, our focus here is on asset increments to low wealth households rather than aggregate inequality. While LRAD unambiguously reduced land ownership inequality as it transferred land from wealthy white commercial farmers to less wealthy black farmers, it is fair to be skeptical about the degree to which it reduced inequality given that its rigorous selection process may have eliminated the least wealthy of its applicants.

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