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Economic Benefits and Costs of Land Redistribution in Zimbabwe in the Early 1980s

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Summary. — Evidence on the performance of Zimbabwe's early land reform effort is mixed. In this paper we use a unique micro data set with information on land reform beneficiaries and a control group of nonbeneficiaries and employ propensity score matching to examine household and per capita benefits associated with land reform. Combining this information with estimates on the cost of land reform we find that the economic return is positive but modest. Its magnitude depends on assumptions about the opportunity costs of land and the reasons for the observed increase in the size of resettled households.

Key words — Zimbabwe, land reform, economic returns, propensity-score matching

1. INTRODUCTION

The land question is one of the central themes in Zimbabwean politics. This has been true for over a century. The issue has its roots in the colonial period, when Rudd, in 1888, persuaded Lobengula, King of the Ndebele, to sign a land concession. It was further aggravated by the British South Africa Company, which, after 1893—when hopes faded for striking gold in what was to become Rhodesia—turned its attention to land, initially as payment to soldiers and later for sale to white settlers for a profit. Land was at the heart of the first, 1896–97, liberation struggle, the second liberation struggle which gave birth to the independent Zimbabwe in 1980 and plays a pivotal role in the current political turmoil.

Between the advent of European settler occupation and independence, Zimbabwe's indigenous population has been systematically deprived of most of its fertile land. At independence, about 15 million hectares of predominantly good quality land was owned by about 6,100 families of European decent, and 16.4 million hectares of less fertile land was occupied by a little less than 800,000 indigenous families.

At independence, the government of Zimbabwe was committed to redress this historical injustice. Bound by the Lancaster House Agreements to acquiring farmland on a willing-buyer, willing-seller basis, it embarked on an ambitious program of land reform. Land acquisition was facilitated by the fact that many farmers of European decent had abandoned their land during the independence

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war. A three-year drought during 1982–84 and the resulting loan defaults made additional land available for the program. As a result, over 1982–84, some 25,000 households could be resettled. After 1984, the pace of land acquisition and resettlement slowed markedly. By the end of 1989, 3.5 million hectares of land had been acquired and 71,000 households had been resettled—more than the original target of 18,000 but far from the subsequently revised target of 162,000 households.

The constitutional obligation to proceed on a willing-buyer willing-seller basis expired in 1990 and two years later the Land Acquisition Act was passed, which made possible the compulsory acquisition of farms. Meanwhile land reform proceeded—albeit at a very slow pace, until the late 1990s, when a new chapter to the land saga began. In November 1997, 1,471 farms were designated for compulsory acquisition. This was followed, starting in June 1998, by a period of copycat farm invasions, which with time became the Fast Track Land Resettlement Program in July 2000. [1]

This brief historical account highlights the centrality of land to the policy debate in Zimbabwe. It also suggests a preoccupation with the quantity of land being redistributed rather than with the performance of the land reform program. In this paper we focus on the latter, in particular on the costs and benefits associated to land reform under the first "willing-seller willing-buyer" phase of the land reform program.

The assessment of the performance of the land reform program, or for that matter, its beneficiaries, depends largely on whether one takes a per household or per capita perspective. Those taking a household perspective consider it highly beneficial. Kinsey, Burger, and Gunning (1998), for instance, report how land reform households perform well as they accumulated large amounts of (livestock) assets. Gunning, Hoddinott, Kinsey, and Owens (2000) show how, relative to 1983–84, settler households increased their productivity tremendously. But if one considers per capita performance criteria such as nutritional status or per capita expenditure, land reform beneficiaries do not outperform nonbeneficiaries (Hoogeveen & Kinsey, 2001; Kinsey, 1999), a finding echoed by Alwang, Ersado, and Taruvinga (2001) who report, based on the nationally representative ICES surveys, that land reform households are almost as poor as communal [2] households.

This paper employs yet another performance criterion, namely whether in terms of economic returns land reform has been satisfactory. We ask whether the benefits accruing to land reform beneficiaries are at least as large as the opportunity costs of the resources expended on them. To put it differently, the paper addresses the question of whether the monetary outlays made during the first phase of willing-buyer willing-seller land reform were well spent. Do the returns to the beneficiaries of the land reform program exceed those that would have been obtained had the resources expended on them been put in an interest-bearing bank account?

This paper is not the first to pose such question. Analyses by Cusworth and Walker (1988) and Robilliard, Sukume, Yanoma, and Löfgren (2001) study the benefits and the costs of the land reform program in Zimbabwe as well. These studies have reported an internal rate of return to the program that exceeds 20%. In the analysis provided here, use is made of the cost estimates presented by these authors. In determining the benefits from land reform, however, our approach differs in at least four respects.

First, we explicitly take into account the opportunity costs of land reform by accounting for the fact that in the absence of land reform, a beneficiary would have earned an income as well. Second, instead of focusing on income—a notoriously difficult to measure concept, we consider benefits in terms of consumption. Third, we do not make a comparison between land reform and nonreform households but instead compare land reform beneficiaries to a control group of households that were eligible for land reform, applied for it, but were not selected to participate in the program Finally, we explicitly distinguish between household and per capita outcomes.

To determine benefits, use is made of a unique panel data set, collected by one of us (Kinsey). This micro-data set comprises information on land reform households resettled during 1980–82 and a group of nonland reform beneficiaries. For our analysis, we use primarily expenditure information collected during 1997–99, so that we obtain an estimate of the long run benefits to land reform. Other information, on crop incomes, yields, acreage, asset accumulation and household size is also explored.

The main conclusion from this paper is that assessment of the performance of the early land

reform efforts largely depends on the assumptions regarding the (opportunity) costs of land and the rationale for the observed increase in household size by land reform beneficiaries. If one uses the price of land as the indicator for opportunity costs, and thanks to the relatively low cost of land in the 1980s, the return to land reform is acceptable (about 8% per annum). But, given the distorted nature of Zimbabwe's land market, prices paid for land are unlikely to be a good reflection of its true opportunity cost. Moreover, the reason for the observed increase in household size matters for the assessment of benefits. If one regards the increase in household size as the result of an influx of mainly unproductive relatives attracted by the benefits generated by the original settlers, then the reported returns are, in fact, lower bounds.

The remainder of the paper is organized as follows. Section 2 discusses the data set and explains how it has been constructed and notes certain caveats about the data. Section 3 describes the resettled farmers and the control group of farmers whose request for resettlement was not honored. This discussion allows appreciation of the differences that exist between these two groups of farmers. In Section 4, the benefits of land reform are determined, and use is made of the propensity-score matching method. In Section 5, various cost figures of the program are presented. Following the presentation of the cost estimates, the return to land reform is calculated. The final section comprises concluding remarks.

2. THE DATA

Starting in 1983, Bill Kinsey collected information about 400 then recently (i.e. for approximately two years) resettled households on a wide range of topics, including: crop production, yields and sales, agricultural practices and inputs, agricultural equipment, livestock inventories, sales and revenues, household composition and labor hiring arrangements, credit, extension services, child health, food and asset expenditures, anthropometrics, education etc. Since the initial survey in 1983, the households were revisited in 1987 and in every year during 1992–2001. Starting in 1997 an additional 150 nonland reform households were added to the survey.

Data have been collected in three agro-ecological zones. In Zimbabwe, five such zones are distinguished. These so-called national regions (NRs) are ranked I–V. NR I comprises a small area with very high potential for specialized agriculture; NR V is the area with least potential for agriculture. Initially, it was intended to select one resettlement scheme in each natural region, but this turned out not to be possible. By the time surveying started in 1983, no households had been resettled in NR I, while households in NR V could not be included because of the poor security situation in Matabeleland (in southwestern Zimbabwe, where this agro-ecological zone is mostly located) at that time. Thus, neither of Zimbabwe's regions of highest and lowest agricultural potential is represented in this exercise. The major characteristics of the three zones included in the survey are summarized below (adapted from Moyo, 1995):

Natural region II—Characterized by intensive farming, this region receives moderately high rainfall (750–1,000 mm) confined to the summer months (October–April). The region is suitable for intensive crop or livestock farming systems. A drier sub-region (IIb) is subject to severe dry spells in some seasons. Crop yields are affected in certain years, but not frequently enough to justify shifting cropping practices away from intensive farming systems.

Natural region III—Semi-intensive farming is practiced in this region. Precipitation is moderate (650–800 mm), but severe mid-season dry spells and high temperatures limit the effectiveness of rainfall. Conditions for growing maize, tobacco and cotton production are marginal. Livestock production, fodder crop farming and the farming of cash crops in soils with good moisture retention are suitable farming systems in the regions.

Natural region IV—This is a semi-extensive farming region. Rainfall is relatively low (450–600 mm) and is subject to periodic seasonal droughts and severe dry spells during the rains. Low and uncertain rainfall make cash cropping risky except for drought-resistant crops on soils with better water retention. Farming systems are suited to livestock production with some intensification possible with drought-resistant fodder crops.

Random sampling was used to select schemes and villages within each selected scheme; in each selected village, a census was attempted of all resident households. In Mupfurudzi, in NR II, 230 households located in nine villages were interviewed. In Sengezi, in NR III, 100

households in six villages were selected. The sample comprises 70 households in Mutanda, in NR IV, located in seven villages. The group of communal households added to the panel study in 1997 was selected in such a way that communal villages were included that had supplied the largest number of households to the existing survey. For each resettlement area, two communal villages were chosen; and in each selected village, 25 households were interviewed. [3]

The data have been collected exclusively among farmers living in model A schemes that were part of the intensive settlement program. [4] This modality made use of a centralized planning and implementation sequence and relied on large amounts of specialist inputs. Under the program, land was acquired by the government and a wide range of infrastructure and supporting services was provided to the schemes. Resettlement schemes were provided with depots for seeds and fertilizer, dip tanks for cattle, schools and clinics, and—where possible—clean domestic water sources. Model A schemes allow for family farming and are by far the most popular mode of resettlement.

Approximately 90% of all households resettled in the first 10 years of the program were resettled under this mode. The implication of selecting only model A schemes is that no information is available on farmers in model B—the collective mode of production, in model C—with individual farming centered on a core estate, and model D—which focuses on extensive ranching. The analysis is therefore restricted to resettlement farmers on Model A schemes in the natural regions II, III and IV that were resettled in the early 1980s (Table 1 and Figure 1).

Data have been collected on a broad number of issues, but for the determinants of benefits, income and expenditure information is most relevant. There are several caveats associated with the variables in this part of the data set that hinder comparisons between land reform and communal households. Household income, for instance, is likely to be underestimated for communal households. As an instrument to monitor the dynamics of land reform households—which initially were not allowed to seek employment elsewhere, the survey was designed to estimate household income from agriculture. With regard to incomes earned off the farm, through self-employment or through remittances, the survey is much less comprehensive, so that it is possible that a substantial fraction of income is not recorded. This problem affects communal households disproportionately as they obtain a much larger fraction of their income from nonagricultural sources (see Table 3 in the next section). The implication is that household income as measured in the survey is not the measure best suited for comparisons between land reform and communal households. Gross crop income is a better measure for comparison. It may be used to assess productivity differences between land reform and communal households, but is only a partial measure of income, and therefore less suited for our purposes.

Consumption is a better indicator of overall performance, although consumption has its own set of problems. The list of consumption items used in the survey is limited, potentially leading to an underestimation. Although consumption may be underestimated—which, in turn, hampers any comparisons with income, there is no reason to assume the consumption information for communal households is systematically mismeasured in any greater degree than that for resettled households. The remainder of the paper uses mostly consumption.

Table 1. *Selected characteristics of the three resettlement schemes included in the data set*

Scheme	Mupfurudzi	Sengezi	Mutanda
Province	Mashonaland Central	Mashonaland East	Manicaland
District	Shamva	Hwedza	Makoni
Natural region	IIb/III	IIb/III	III/IV
Area (km^2)	345	84	439
Year settlement officially began	1980	1981	1981
Number of settlement villages	18	8	29
Number of settler households	563	289	575
Mean area available per household (ha)	61	29	76

Source: Adapted from Kinsey (1999).

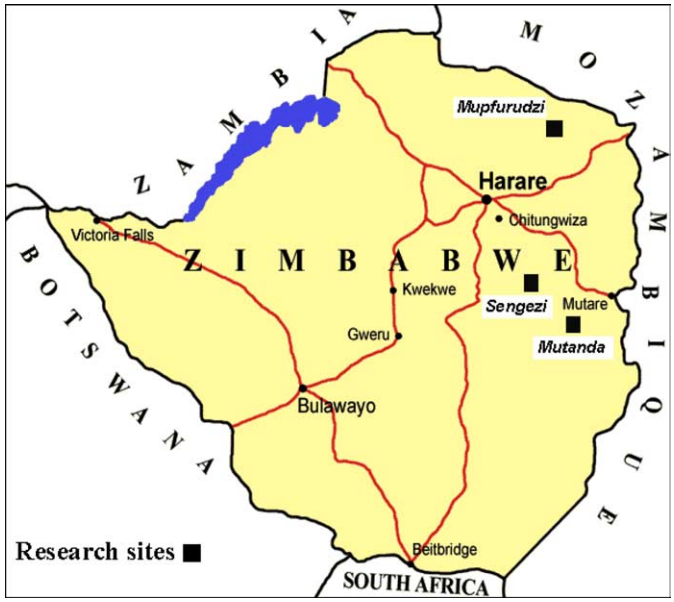

Figure 1. *The location of the survey sites in Zimbabwe.*

3. INDICATORS OF PERFORMANCE OF RESETTLED HOUSEHOLDS

To qualify for land reform, several categories of potential beneficiaries were distinguished. Eligible for settlement were: (a) refugees and other persons displaced by war; (b) those residing in communal areas who were landless; and (c) those who had insufficient land to maintain themselves and their families. In addition, to be eligible, household heads were supposed to be married or widowed, aged 25–55 and not in formal employment. Broadly speaking, these criteria seem to have been followed. Some 90% of households settled in the early 1980s had been adversely affected by the war for independence in some form or another. Before being resettled, most (66%) had been peasant farmers with most of the remainder being landless laborers on commercial farms, workers in the rural informal sector or wage earners in the urban sector.

Any comparison between land reform beneficiaries and a random control group of communal households, comprising applicant and nonapplicant households, is likely to provide biased estimates of the benefits of land reform. Including nonapplicants in the control group seems less appropriate because, out of choice, these households decided not to participate in the program, presumably because they were not eligible, were already doing relatively well or were apprehensive about a new and untested government scheme. If, however, those who were eligible but not accepted in the program could be regarded as control group, a more reliable comparison between beneficiaries and nonbeneficiaries would be attained. After all, these farmers would have liked to become land reform beneficiaries, were eligible for the program, applied for it, but they were prevented from entering it. The data set allows us to distinguish among: (i) those who applied for land reform and were accepted into the program; (ii) those who applied for the program but were rejected and (iii) those who did not apply at all, making it possible to assess benefits by comparing the outcomes for groups (i) and (ii).

Table 2 illustrates the differences among the three groups of farmers. Interestingly, in 1980–81 beneficiaries possessed substantially more draft animals than those whose applications were rejected. [5] Comparing those who applied for the program but were rejected with the group of nonapplicants, it can be seen that the latter were older and less-educated but were better equipped for farming, as they had more land available and possessed more draft animals.

Now that the control group has been defined, it is possible to compare the performance of

Table 2. *Selected initial conditions for those resettling and not resettling, 1980–81*

	Selected for resettlement	Household did not apply	Application was rejected
Land available (acres)	7.1	5.9	4.3
Trained oxen owned	1.5	1.8	0.6
Age of head of household	41.3	44.9	36.5
Education of head of household (years)	5.2	3.9	5.6

Source: Kinsey surveys, various years.

Table 3. *Household characteristics for land reform beneficiaries and the comparison group of rejected farmers, 1997–99*[a]

	Land reform beneficiary	Rejected for resettlement
Household income (Z$ 1995)	7,904	5,337
... of which		
Crop income	66.5%	38.4%
Off farm business revenues	9.9%	16.7%
Livestock produce	1.4%	0.4%
Livestock growth	10.7%	8.2%
Remittances	3.7%	18.5%
Female income	4.4%	4.9%
Off farm income	3.4%	12.8%
Acreage planted	8.1	4.6
... of which		
Maize	56.5%	55.0%
Cotton	15.2%	17.5%
Groundnuts	12.4%	13.0%
Other crops	15.8%	14.5%
Maize yield per acre (kg)	846	554
Cotton yield per acre (kg)	169	167
Groundnut yield per acre (kg)	463	215
Household expenditures (Z$ 1995)	7,561	4,926
Value of livestock (Z$ 1995)	13,764	8,198
Value of capital stock (Z$ 1995)	3,279	1,571
Household size	9.6	6.7
Real expenditures per capita (Z$ 1995)	879	793

Source: Kinsey surveys, 1997 and 1999.
[a] Observations are weighted to correct for the fact that resettled households in NR II and III are over-represented. Excluded are households whose per capita expenditures exceeded mean expenditure plus three standard deviations. The cut-off point was Z$5,244.

both groups of farmers. This is done in Table 3, which presents various outcome measures for 1997–99. The information for each of these years has been expressed in 1995 Zimbabwe dollars.

Table 3 shows that the methods of generating income differ substantially between land reform households and the control group. Whereas for land-reform beneficiaries crop income and livestock constitute about 80% of total income, for the control group this is at most 50%. This latter figure is not particularly low. Reardon (1997) reports in his review of studies on rural household income in Africa that 30–50% of total household income is earned in the non-farm sector. The rejected applicants therefore appear to be no exception to this rule. Remittance income, off-farm income and business revenues are important sources of support for this group. Comparing the levels of income between both groups of farmers shows clearly that the mean household income of resettled farmers is much higher than that of the control

group. As indicated before, however, household income may not be the best basis for comparison.

In line with household income, crop income is also much higher for the resettled farmers. Where land reform beneficiaries earn approximately Z$5,200 in gross crop income, the comparable figure for those rejected is Z$2,050. This comparison suggests that land reform beneficiaries are more involved in farming than the control group of rejected communal households. This conclusion is also supported by the acreage planted, which for land reform beneficiaries is almost double that of the rejected applicant farmers. The same relationship holds for yields, which are substantially higher for settlers. This finding is in accordance with the results presented by Gunning *et al.* (2000), who suggest that land reform households do well in generating crop income.

Not only do land reform beneficiaries do well in generating crop income, they also possess substantially more farm equipment and own more than 50% more livestock than their communal counterparts do. The latter suggests a confirmation of the work of Kinsey *et al.* (1998), who reported a substantial increase in livestock ownership among resettled farmers. The results here also indicate that settlers possess substantially more livestock than communal farmers.

In terms of expenditure levels and on a per household basis, resettled farmers do substantially better than those in the control group. Much of this advantage disappears, however, when expenditures are expressed in per capita terms. This outcome should be attributed to the fact that settler households are substantially larger than nonsettler households are (9.6 as opposed to 6.7 family members). This disparity in household size is such that, in per capita terms, the difference in expenditure levels becomes negligible: the hypothesis of no difference in per capita expenditure between nonbeneficiaries and settlers could not be rejected (p-value of 0.18). This finding suggests that in terms of welfare both types of households are equally well off and confirms the existing evidence on poverty levels (Alwang *et al.*, 2001) and nutritional indicators (Kinsey, 1999).

4. BENEFITS OF LAND REFORM

The information in Table 3, while enlightening about the differences between land reform households and the control group, does not account for differences in initial conditions. This needs to be addressed especially as Table 2 indicates that the differences are substantial and as it is likely that differences in original endowments are correlated with current outcomes.

There are various ways to deal with differences in initial conditions. Ideally, no such differences should exist because the control group of randomly selected, eligible and interested households will have been identified before the start of the program. If—for both the control group and the program participants—information on the outcome variables of interest is collected both before and during the program, a so-called difference-of-differences approach can be followed. Such an approach allows for a straightforward determination of program benefits, as one only has to compare the outcome variables before and during the program. After all, the random selection of a control group guarantees that the calculated benefits are not biased due to differences in initial conditions. A control group of eligible and interested households has been identified in the data set used here. Still a difference-of-differences approach is not possible because information on outcomes was not collected before the start of the program among the control group of rejected land reform applicants. We therefore have to rely on a comparison in levels between land reform households and the control group, while controlling for differences in initial conditions.

One way to do so is to rely on propensity-score matching and to relate land-reform households to (rejected) communal households with the same pre-intervention characteristics (Rosenbaum & Rubin, 1983, 1985). The propensity score is defined here as the probability of participation in land reform, conditional on covariates. In this case, the propensity-score method associates with land-reform households comparable rejected applicant households based on household characteristics at the start of the land-reform program. Obviously, such associations are difficult when there are numerous pre-intervention variables, as is the case here (see Table 2). The innovation of Rosenbaum and Rubin is that they showed that the propensity score—which is determined based on a participation model, usually a logit analysis explaining participation in the program according to pre-intervention characteristics—summarizes the various pre-intervention variables. This allows matching of land reform and

rejected communal households based on the closeness of their propensity scores. An important assumption underlying this method is that participation in land reform is associated only with observable pre-intervention variables. This is the ignorable treatment assignment assumption (Heckman & Robb, 1986; Holland, 1986). [6] Given the importance of this assumption, we implement a test proposed by Jalan and Ravallion (1999) which checks for the presence of remaining selectivity bias after matching.

The data at hand are well suited for propensity-score matching. First, because identical surveys have been administered to both groups of farmers. Second, because these farmers live in similar economic environments and, third, because assignment to the program was not random. There was selection based on observable household characteristics, several of which have been recorded in the questionnaires (see Table 2).

The propensity score is determined by estimating a participation equation on the probability of inclusion in the program. In this case, a simple logit model is used in which the determinants are: whether the household had been staying in a ''protected village'' during the war of independence, the land available to the household at independence, the level of education of the head of household and the natural region in which the household is located. Because of the peculiar set-up of the survey in which about 400 program participants are interviewed and 150 control households are available, it was decided to match the rejected applicants to participants. Hence, the treatment group are the rejected households. The results, therefore, give an indication of the difference in outcome levels had the household not been included in the land-reform exercise.

Predicted values of the propensity score of being rejected for land reform were used to match those in the treatment group to the control group of rejected farmers. To ensure that observations included for matching are taken over common values, the (predicted) propensity scores were trimmed at 2.5% from the top and the bottom. [7] The adjustment is of importance because Heckman, Ichimura, and Todd (1997, 1998) have found that, in determining program benefits using matching techniques, failure to compare participants and matched households at common values of the matching variables is the single most important source of bias. Subsequently, each rejected applicant household was matched to the land reform household with the closest propensity score (measured by the absolute difference in the scores). Only the closest match was taken, thus we followed the method of the nearest neighbor. Results are presented in Table 4.

The results here indicate that controlling for differences in initial conditions matters but the differences between Tables 3 and 4 are not large. [8] After controlling for initial differences, the difference in per capita expenditures between matched land-reform beneficiary households and rejected communal households is Z$66 (down from the Z$86 in Table 3).

The figure of Z$66 can be used to inform about the benefits of land reform. To do so, a correction has first to be made for the underreporting of consumption. The degree of underreporting can be established by comparing the per capita expenditures in the survey used here with those reported by Alwang *et al.* (2001). These authors use the 1995–96 nationally representative Income Consumption and Expenditure Survey (ICES) and report, for land-reform beneficiaries, mean yearly per capita consumption expenditures of Z$1,620 with a median of Z$1,363 (in 1995 prices). In the panel survey, the corresponding figures for the 1995–96 survey year are Z$643 with a median of Z$554. [9] It follows that the expenditure data are underestimated by a factor of about 2.5.

Applying this factor to the difference in per capita expenditures between rejected and beneficiary households suggests that being a land reform beneficiary increases per capita expenditure by Z$165 per annum. At an exchange rate of 9.5 Zimbabwe dollars to the US dollar (in 1995 values and prices), the per capita benefit to land reform beneficiaries is approximately US$17 per annum.

As the matches are chosen based on similarities in observed characteristics, the possibility remains that there are unobservables that jointly affect per capita expenditures and selection into the land-reform program. One way to test for this is to look for a partial correlation between per capita expenditure and the residuals from the participation model. Jalan and Ravallion (1999) suggest such a test. It is an application of the standard Sargan–Wu–Hausman test in which, for a combined sample of the rejected applicant households and the matched land-reform beneficiaries, a regression is carried out with per capita expenditures as the dependent variable and with, as control var-

Table 4. *Benefits from acceptance in the land reform program, 1999*[a]

	Matched resettled households	Household was rejected for resettlement
Household income (Z$ 1995)	9,255	5,625
... of which		
Crop income	65.0%	35.4%
Off-farm business revenues	11.4%	19.8%
Livestock produce	0.6%	0.5%
Livestock growth	9.9%	9.3%
Remittances	5.4%	16.0%
Female income	5.9%	4.4%
Off-farm income	1.8%	14.6%
Acreage planted	8.5	4.9
... of which		
Maize	51.7%	56.1%
Cotton	18.8%	17.5%
Groundnuts	12.9%	12.1%
Other crops	16.5%	14.2%
Maize yield per acre	977	535
Cotton yield per acre	209	159
Groundnut yield per acre	422	229
Household expenditures (Z$ 1995)	7,518	5,125
Value of livestock (Z$ 1995)	13,577	9,687
Value of capital stock (Z$ 1995)	2,670	1,730
Household size	9.7	7.4
Real expenditures per capita (Z$ 1995)	832	766

Source: Kinsey, 1999.

[a] A total of 42 households in the data set were rejected for land reform. Eleven observations were lost because of missing information on initial conditions, and four observations were lost because no common support with the land reform households was found. Hence a match could be made eventually for 27 households. One household was subsequently dropped because of inconsistent answers about off-farm income. To determine benefits from land reform in US dollars, row differences should be multiplied by 2.5 to correct for the under-reporting of expenditure. The 1995 Z$/US$ exchange rate was 9.4.

iables: the propensity score, the residual from the participation model, and the control variables from the participation model. [10] The residual variable controls for heterogeneity in the expenditure outcomes. Selection bias is indicated if the coefficient for the residual is significantly different from zero. In the results presented, this is not the case. The *t*-value of the coefficient on the residuals is −0.55 (Table 5).

Since we find no evidence to support the notion that unobserved variables affect the estimated benefit, but bearing in mind that the number of observations is small, US$17 is our preferred estimate for the (annual) *per capita* return to land reform.

Table 5. *Sargan–Wu–Hausman test on selection bias in the matching estimator*

	Coefficient	*T*-statistics
Propensity score	1,147.3	1.44
Residual	−13.2	−0.55
Years of education of head of households	20.3	1.29
D-Household stayed in a protected village	60.9	0.41
D-Natural Region III	322.5	2.57
D-Natural Region IV	19.4	0.21
Constant	464.4	2.75

5. THE COST OF LAND REFORM

By linking the cost of the land reform program to its benefits, the rate of return to the program can be calculated. No costs could be obtained for the households represented in the sample, but Cusworth (1990) reports that the cost per settler household of the 1980s program of land reform was approximately Z$22,000 (in 1996 prices) or US$2,000 (in 1995 prices). Unfortunately, a breakdown of Cusworth's costing could not be obtained. In a more recent analysis, Robilliard *et al.* (2001) present a detailed breakdown of the estimated costs of the current land reform program for farmers in three classes of natural regions. These costs are presented in Table 6.

Depending on the natural region, it is assumed that an average farm comprises 30 ha in NR II, 45 ha in NR III and 90 ha in NR IV and V. These assumptions correspond reasonably well with our data, where depending on the natural region, total area per resettled farm household varies between 29 and 76 ha (see Table 1). The detailed budget items speak for themselves. The credit program reflects the government assisted credit program for land reform beneficiaries. The amount of the credit support covers the cost of purchasing animals and is US$600 for the second year and US$300 for the third year. Half of the loan is subsidized and the other half is repaid over a 10-year period at a 10% interest rate. Borrowers start repaying after a two-year grace period.

It follows from Robilliard *et al.* (2001) that the cost of resettlement should be put at least at US$4,000, a figure that departs substantially from the cost estimate of Cusworth (1990). Much of the difference should be attributed to changes in the price of land. Consider Figure 2, which presents the cost per hectare of land purchased by the government for resettlement.

Table 6. *Summary of costs per family (US$), 1994–95*

	Natural region		
	I & II	III	IV & V
Land allocation/beneficiary	30	45	90
Farm acquisition/ha	79	53	32
Land assessment/ha			
Transport	0.14	0.14	0.14
Land valuation	0.075	0.075	0.075
Land distribution			
Land use plan/ha	5	5	5
Demarcation/beneficiary	26	26	26
Title survey/beneficiary	132	132	132
Farmer support			
Training/beneficiary	18	18	18
Inputs/beneficiary	53	53	53
Land prep/beneficiary	22	22	22
Extension/beneficiary	26	26	26
Credit support/beneficiary	478	478	478
Infrastructure support			
Water/beneficiary	95	95	95
School/beneficiary	592	592	592
Clinic/beneficiary	19	19	19
Dip tanks/beneficiary	39	39	39
Roads/beneficiary	18	18	18
All costs			
Program cost/beneficiary	4,044	4,137	4,867

Source: Robilliard *et al.* (2001).

In conjunction with the land-cost estimate used by Robilliard *et al.*, we find for 1995 a price per hectare of US$54. In the early 1980s, however, land was much cheaper—about US$18 a hectare, implying that during 1980–95 land prices tripled. If the lower land price is used to recalculate the costs of land reform as reported in Table 6, then the total cost per participating household becomes approximately $2,500, reasonably close to Cusworth's (1990) estimate of

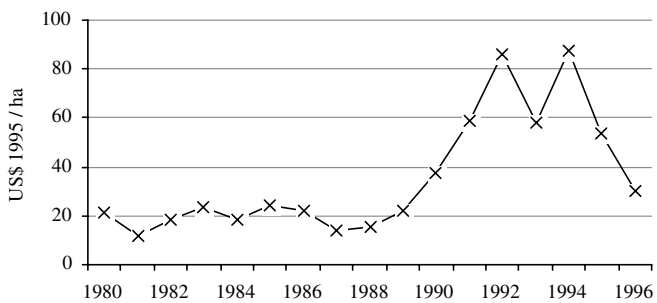

Figure 2. *Prices paid per hectare for land purchased for resettlement, 1980–96, Adams et al. (1996).*

$2,000. The remainder of the difference should be attributed to increases in the costs of infrastructure and service provision.

The large increase in land prices points toward an important proviso in the analysis here, namely the use of market prices as the indicator for the opportunity costs of land. Zimbabwe's land markets are highly distorted. Given these distortions, prices are unlikely to be a reflection of the actual value of land. But, what a realistic estimate of the opportunity cost of land would be is not obvious. As much land is not utilized at all—an early estimate by the World Bank (1991) suggested that as much as three million hectares of commercial farmland remained idle in the 1980s, the opportunity cost might be put at close to zero. On the other hand, if one takes the profits from farming as starting point for the opportunity cost of land, then the estimate would be much higher. For instance, Sukume, Makudze, Mabeza-Chimedza, and Zitsanza (2000) report that, for the 1995–96 season the gross profit (i.e., net profits including the reward for the use of land) from growing one hectare of burley tobacco for a commercial farm in NR I was US$1,612. If one assumes that one-third of this gross profit is spent on land (with the remainder being the remuneration for the entrepreneur's labor and risk-taking), a 10% real rate of interest and a land utilization rate of 50%, then the net present value of land may be as high as US$2,687 per hectare. Tobacco is extremely profitable, however. The corresponding figure for a hectare of maize would be just US$248.

If we nevertheless adhere to using the price of land as the measure of the opportunity cost of land, then the cost and benefit figures can be used to calculate a rate of return to the program. Based on a per capita return of US$17 and a per capita outlay of US$206 per land-reform beneficiary (US$2,000 divided by 9.7 beneficiary household members) a return of 8% is found. This seems reasonable. If, however, the increased land prices are in fact a reflection of the true opportunity costs of land, then a return of US$17 per annum would be less acceptable. At an (opportunity) cost of about $4,000 per household, a return of $17 per capita is insufficient for the program to be considered economically sustainable as the per capita rate of return is reduced to only about 5%.

One reason (apart from land prices being inflated) for the less than satisfactory rate of return is the increase in size of resettled households. According to Gunning *et al.* (2000), in the early 1980s land reform households were of about the same size as communal households. [11] The increase in household size is associated with the resettlement process, but the reason behind it is unclear. Additional household members may have arrived to ensure that the original settlers, who had received large tracts of land, could attain their productive potential through additional labor to help in tilling the land. But in the face of few economic opportunities elsewhere in the economy, they may also have been attracted by the relatively high consumption levels attained by the settler households. If the additional household members are mainly unproductive (i.e. their opportunity costs are low), then the return to land reform is underestimated. If it is assumed that the same return (in terms of household consumption) could have been obtained with the initial number of household members of 7.4 persons—i.e. the additional household members are unproductive and their opportunity costs are zero, then the benefits from land reform become US$85 per capita. Even at resettlement cost of $4,000, such a benefit implies a very reasonable annual rate of return per beneficiary of 15%.

6. CONCLUSION

In this paper, an overview of the benefits and costs of Zimbabwe's land reform program has been presented. Using propensity-score matching to deal with differences in initial conditions between land reform households and a control group of households that were eligible for the land reform program, applied for it but were rejected, we find that the return is modest, US$17 per capita per annum. This outcome has to be interpreted with care because the number of observations in the matching process is small and because various other benefits to land reform—such as reduced pressure on land in communal areas or rectification of an historical injustice—are not valued. Moreover, if the observed increase in household size among settlers is attributed to nonproductive individuals seeking to share in the high income attained by resettled households, then the benefits to resettlement are underestimated because the opportunity costs of these unproductive household members would be zero. A further complicating factor in determining the rate of return to resettlement is uncertainty about the correct opportunity cost of land. In this paper, we use the price of land as the indicator, but given the many distortions in the Zimbabwean land

market the undistorted market price is most likely to be different from the price paid by the government. If despite this caveat, we take the price of land to reflect its opportunity cost, then regardless of the modest benefit of US$17 but due to the low cost of land in the early 1980s, the return on investment in land reform is acceptable (about 8% per annum). If more recent market prices for land were used, however, the rate a return would be lower and less acceptable (about 5%). If the increase in household size is a reflection of the lack of opportunities elsewhere in the economy, and if the additional household members are largely unproductive so that their presence taxes the returns generated by the original settlers, then the per capita benefits of land reform may have been underestimated by as much as a factor of four.

In this context, various issues should be considered. First, a better understanding is needed of why household size has increased among resettled households. Second, consideration needs to be given to whether the incentives for land reform beneficiaries can be improved. Apart from the potential arrival of unproductive family members, the literature suggests various other obstacles of possible significance. Among these are (a) insecurity of tenure—a consequence of the fact that settlers are not given title deeds to the land they farm; instead they receive permits that can be revoked at any time; and (b) the various rules and regulations applying to the settlers (such as the prohibition to build on land used for cultivation or the (largely ineffective) prohibition to have more than 10 head of cattle). Third, it should be considered whether land reform households require the quantity of land they are allocated. For example, beneficiaries under the dominant model A approach have received 5 ha of land for cultivation, implying that, depending on the natural region, 25–85 ha per family are set aside for grazing. Given that at most 20% of household income is directly earned from livestock (although livestock also contribute indirectly to agricultural income by providing draft power and manure), these large areas seem inefficient and unnecessarily costly. Finally, more clarity is needed about the opportunity cost of land in Zimbabwe.

NOTES

1. This historical overview draws extensively from Moyo (1995, 1998) and Zimbabwe (2003).

2. In Zimbabwe nonland reform smallholder farmers are referred to as communal farmers. In this paper we follow the same convention.

3. In 1999, the survey was expanded further to include all households in all the villages covered at two of the sites. In this way, an extra 180 households were added. These households are not included in any of the analyses here.

4. The *intensive* description relates to the major expenditure on the provision of infrastructure that accompanied the resettlement of the smallholders and not the farming systems that the farmers pursue (Mhishi, 1995).

5. The amount of land owned by beneficiaries may be inflated because some households reported as land owned in 1980, land received as part of the resettlement program between October and December 1980. There is one household that owned 165 ha in Zambia. More information on those selected for the land reform program can be found in Kinsey (2002).

6. The assumption underlying the propensity-score method is that assignment to the program is associated only with observable pre-intervention variables (see Heckman & Robb, 1986; Holland, 1986; Rubin, 1977). Bias due to unobservables cannot therefore be ruled out.

7. To be precise, trimming was at 2.5% from the bottom (top) for both the participant and nonparticipant groups, whichever was higher (lower). Ignoring the trimming increased the number of matched households by three. Including these three households in the determination of the program benefits altered the results significantly.

8. This is in accordance with Gunning *et al.* (2000), who report for resettled farmers that initial conditions do not explain current performance in agriculture.

9. Information on communal households was first collected in 1997, thus no information is available for 1995–96.

10. In a linear model, such a specification would not be possible and one would have to omit at least one control variable from the participation equation. The nonlinearity of the propensity scores in the control variables means that this condition is not essential here (Jalan & Ravallion, 1999).

11. The increase in household size is not a peculiarity of the data set used. The 1988–89 monitoring survey (Zimbabwe, 1992), for instance, also reports an average household size among land reform households of about 10 persons, as contrasted with six persons for communal households.

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