Investing in development or investing in relief: quantifying the poverty tradeoffs using Zimbabwe household panel data

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Abstract: This study examines the consequences of alternative relief and development interventions on the well being of households in rural Zimbabwe. It does so by: a) establishing a framework that links household resources to levels of poverty; b) validating the quantitative data with group wealth rankings by the households in the study; c) estimating key parameters within this framework, namely: the determinants of net crop income; the determinants of private transfers; and the links between increased incomes and the accumulation of capital stock; and d) conducting a counterfactual exercise in which relief assistance is reduced and reallocating these funds to improve access to agricultural extension and increased holdings of capital stock. Under these counterfactuals, the incidence and severity of poverty in non-drought years fall significantly. The best performing counterfactual, improving access to extension and increasing capital stock reduces the incidence of food poverty by 11 per cent. Under the most basic scenario, the increased income generated by transforming relief aid into agricultural capital is sufficient to fund an adequate diet for each person in each beneficiary household for six months. Further, such improvements in well being are achieved without households necessarily being made worse off during a drought year. These results suggest that for the households in this sample, there is a significant opportunity cost associated with the shift in external aid resources from development to emergency assistance.

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1. Introduction

The objective of this DFID funded study is to analyse the distributional consequences of drought relief and development assistance on rural households. The intention is to increase the understanding of the impact of relief and development aid on the poor, with the goal of improving policy design and implementation.

The last ten years have witnessed an increasing amount of bilateral and multilateral aid devoted to emergency and distress relief. For example, approximately 12 per cent of DFID bilateral aid is now spent on emergency and distress relief compared to about 2 percent in the mid 1980s. Between 1991/92 and 1994/95 the average amount of DFID bilateral aid spent on emergency aid in countries south of the Sahara was 21 percent. DFID is not the only development agency experiencing such shifts. Across all countries reporting to the OECD's Development Assistance Committee (DAC), about 10 per cent of DAC bilateral aid went on emergency relief in the mid-1990s, up from about 3 per cent ten years earlier. The amount of aid spent on non-emergency activities has remained broadly unchanged over this period. The most spectacular example comes from the World Food Program. In the mid-1980s, about 80 per cent of WFP assistance was spent on development projects with the remainder being allocated to emergency relief. In the mid-1990s, these proportions were reversed (Hoddinott, 1998).

Although these trends have attracted comment (Borton, 1993; Buchanan-Smith and Maxwell, 1994) and expressions of concern at the donor level (Holden, 1994), the consequences of this change are poorly understood. In particular, one might suspect that this may be at the expense of longer-term development efforts to reduce poverty, undernutrition and other measures of well being. To date, the absence of any detailed empirical analysis of the impact of these changes makes it impossible to determine whether such concerns are justified. In this paper, we use the conceptual framework outlined in Hoddinott (1998), together with a unique four year household panel data set from Zimbabwe to quantify the opportunity cost - in terms of foregone poverty reduction - of this shift in aid spending. In one of the four years of this panel, our surveyed households received drought relief. We examine the consequences of reducing the funds allocated to this relief and using the monies saved to increase both physical and human capital of poor households. We find that the incidence and severity of poverty in non-drought years fall significantly. Further, such improvements in well being are made without households necessarily being made worse off during a drought year.

The paper is organised in the following fashion. In section 2, we briefly outline the conceptual framework we will use to examine these trade-offs. Section 3 outlines our data sources. In section 4, we explain how these different relationships are estimated. Section 5 presents our counter-factual simulations and section 6 concludes.

2 . A conceptual framework for examining the impact of changes in the purpose of aid flows

Our conceptual framework is outlined in Figure 1. It is surrounded by three frames denoting the natural, policy and social environment in which the household exists. Such framing emphasises that all interventions are situated within a broader environment and that this environment will have an important impact on the efficacy of any intervention.

Within these environments, consider poverty to be a consequence of an inadequate asset base (broadly defined) and/or low returns to these assets relative to the goods that the

poor wish to purchase. Accordingly, at the top of Figure 1, there are two thickly-lined black boxes. The right-hand box represents labour income, obtained by combining labour supply - defined in terms of both quantity of labour used and the human capital embodied in that labour - with returns to that labour. The left-hand box represents capital income, obtained from combining non-human capital with returns to capital. These, together with private transfers (located in the smaller box between these) produce total household income. One could disaggregate household income in a number of other ways. For example, one could distinguish different sources of income: subsistence cropping; cash cropping; livestock; wage employment; transfers; and other income sources. The attraction of the approach used here is that it makes it relatively straightforward to consider the impact of emergency and development interventions.

Total household income, together with the set of prices faced by the household, determines the set of feasible consumption bundles available to the household. It is all possible combinations of food, shelter, clothing and other goods that a household could purchase. The value of this consumption set can be compared against a defined poverty line. If the purchasing power of the household is less than this line, the household is considered poor. What the household actually consumes will depend on tastes, knowledge (for example, knowledge of which foods are most nutritious) and rules regarding how food and other goods should be distributed within the household.

Next consider the impact of interventions funded by emergency and development assistance. The former include: Cash Transfers, Humanitarian & Supplementary Feeding, School Feeding and Food for Work. The latter include: Infrastructure, Credit, Land Resettlement, Agricultural Extension, Business Extension, New Agricultural Technologies, Ration Shops, Infrastructure and Food Subsidy. As discussed in Hoddinott (1998), the dichotomy between emergency and development aid interventions should not be overemphasised. Infrastructure development, though denoted here as a "development intervention" can greatly enhance the efficacy of emergency operations. Mother and child health programs are supported by both emergency and development interventions.

These interventions affect household well being through four pathways, by: (a) directly augmenting households assets - such as those that increase human, physical or financial capital; (b) increasing the returns to those assets; (c) by increasing incomes without directly altering either the level or return on endowments; and (d) changing the prices faced by households as they turn their income into consumption. All these interventions are placed above the box denoting the set of feasible consumption bundles. By doing so, it is possible to compare how these interventions alter this set and thus determine how they affect the incidence and severity of poverty.[1]

Finally, there are second round or feedback effects, denoted by the dashed black lines in Figure 1. Consider the impact of a development aid intervention designed to improve the provision of agricultural extension. Through the pathways described above, this should lead to a better consumption bundle in the sense that either the amount, number or the quality of goods consumed increases. But note that decisions regarding actual consumption will affect

[1]. It is worth noting that this schematic diagram is silent on whom within the household should be the recipient of these interventions. It has been argued (Alderman et. al., 1995) that this issue is an important factor in the determining the success of these interventions. In this diagram, specifying the identity of the recipient does not affect the impact on the feasible consumption set, but instead would work through the 'intrahousehold allocation rules' and thus the actual consumption bundle chosen.

holdings of physical capital via the decision to save some fraction of household income. Allocations of food, expenditures on education and health will all affect the level and distribution of human capital within the household. These investments have an effect on the household's ability to generate income subsequently. But not all these feedback effects are benign. The provision of a public transfer, say food aid during a drought, may induce an offsetting reduction in assistance provided to the household by family or neighbours.

Figure 1 provides a useful organising framework for considering the effects associated with shifts from spending on development to spending on emergency relief. It emphasises the need to understand the magnitudes of the linkages between various interventions and outcomes of interests. For example, which interventions generate the largest increases in incomes? To what extent is the impact of increased transfers on poverty mitigated by the reductions in private transfers that they may generate? To what extent does the achievement of very immediate goals - such as providing households with incomes during droughts - come at the expense of the longer-term objectives such as poverty reduction? Further, Figure 1 is drawn for a single household. Shifts in aid from development to emergency assistance may alter the composition of the pool of beneficiaries. This could occur in geographical terms - where, for example, resources are shifted out of support to farmers in a high potential region and redirected to drought-afflicted households in a low potential area. If re-allocation occurs across households of different income or demographic classes, it will affect the distribution of observed outcomes as well as their levels.

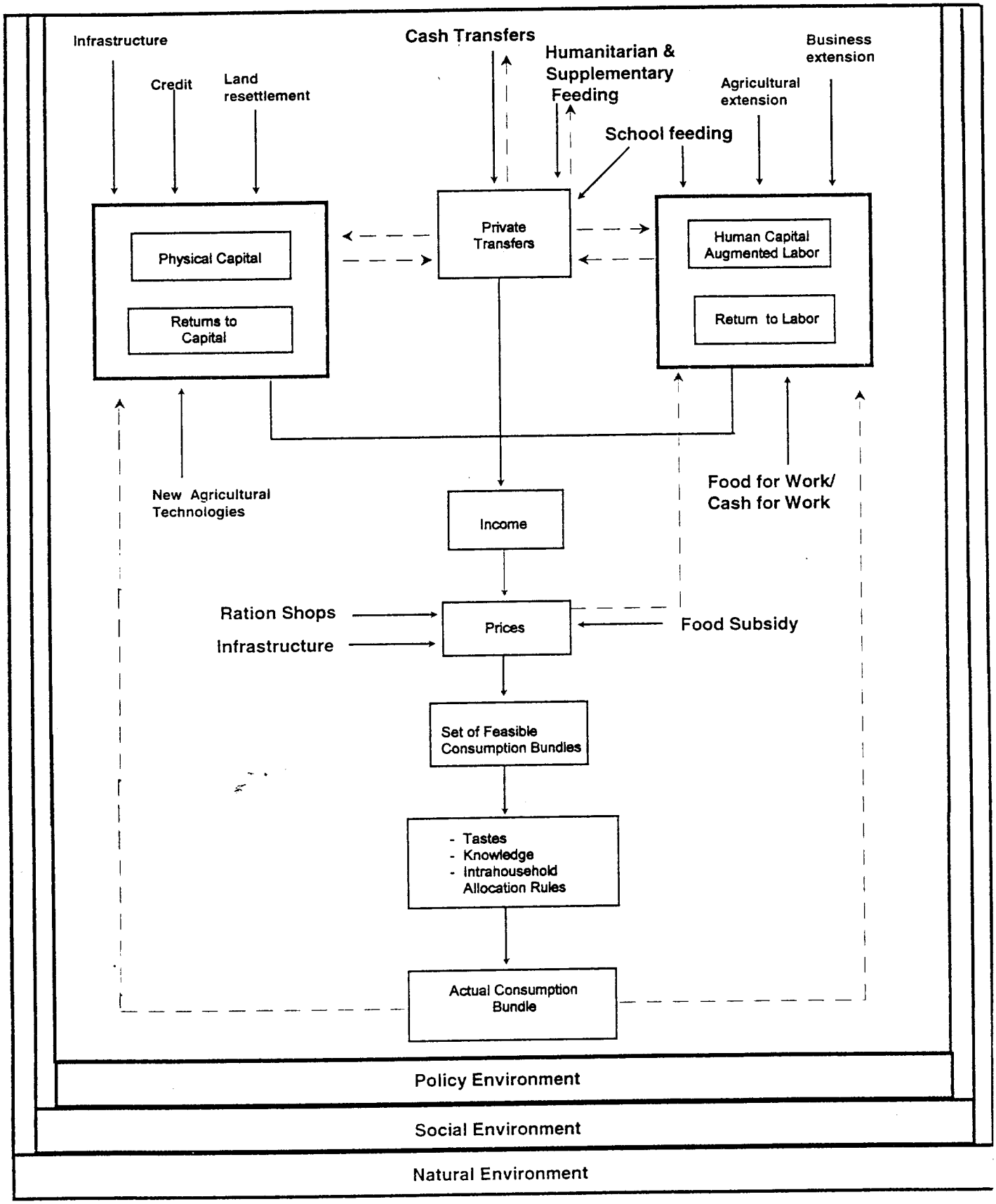
3. The Zimbabwe household panel data set[2]

(i) Background

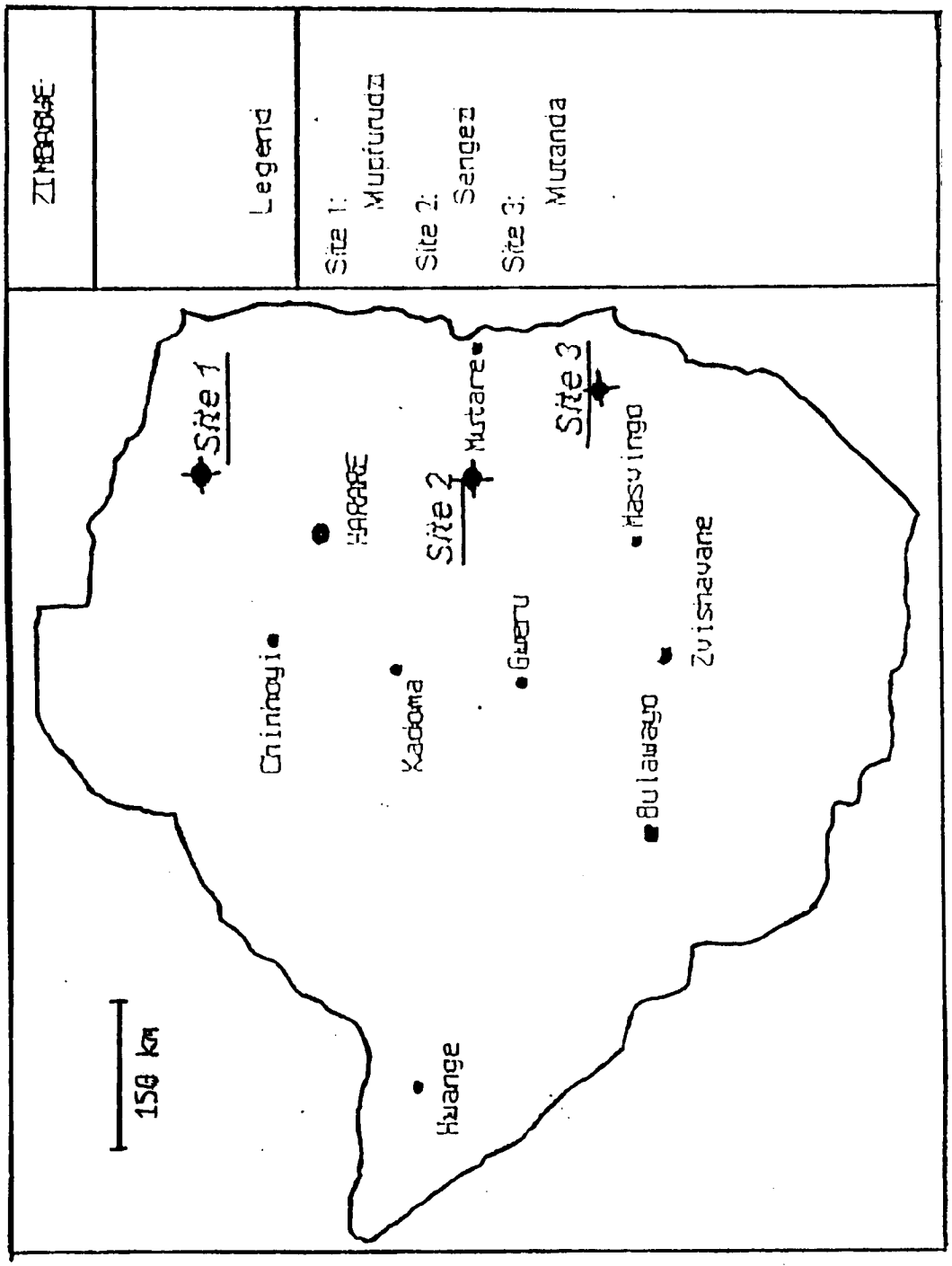
Before we describe our data in detail, it is helpful to have a little knowledge regarding the history of the households in our sample. It is well known that access to land has long been an issue of major economic and political importance in Zimbabwe. Anger at the gross disparities in land ownership between blacks and whites was a major factor motivating armed rebellion against White minority rule. Upon gaining Independence, the Government of Zimbabwe committed itself to a wide ranging programme of land reform, designed to redress these severe inequalities. A component of the land reform programme was the resettlement of households on farms previously occupied by white commercial farmers. The supply of land for resettlement was determined by the availability of areas which had been abandoned during the liberation war and also the general insecurity of European farmers in peripheral areas who were willing to sell. In most cases, these were the commercial farms contiguous or generally bordering the communal areas. The majority of land tended to be in the worst agro-ecological zones, 78 percent of resettlement occurred in zones III, IV and V, and only 22 percent in the more climatically favoured zones I and II.

[2]. These data were collected under the supervision of our colleague, Dr. Bill Kinsey (Free University, Amsterdam and University of Zimbabwe). This work has been supported in Harare by the British Development Division in Central Africa, UNICEF and the former Ministry of Lands, Resettlement and Rural Development. Additional support was provided by grants in the United Kingdom from the Nuffield Foundation, the Overseas Development Institute, and the Department for International Development (formerly ODA). Assistance has also been received from the International Food Policy Research Institute, the Centre for the Study of African Economies at the University of Oxford, the Free University, Amsterdam, and the Research Board of the University of Zimbabwe.

Figure 1 : Interventions and their impact on household welfare



Map 1. Zimbabwe: Location of three resettlement schemes



Our data are drawn from a random sample of 370 such households located in three different resettlement schemes. These schemes were chosen so as to ensure representation in each of the major agro-ecological zones in Zimbabwe suited to cropping: Mupfurudzi in Mashonaland Central (which lies to the north of Harare in Zone II), Sengezi in Mashonaland East (which lies south east of Harare in Zone III) and Mutanda in Manicaland (which lies south east of Harare, but farther away than Sengezi and in Zone IV).

The criteria for selection into these schemes were, in descending order of priority: (i) refugees or other persons displaced by war, including extra-territorial refugees, urban refugees and former inhabitants of protected villages; (ii) those who were residing in Communal areas but were landless; and (iii) those who had insufficient land to maintain themselves and their families (Kinsey, 1982). At the time of settlement, the household heads were also supposed to be married or widowed, aged 25 to 50 and not in formal employment. Families selected for resettlement were assigned to these schemes, and the nucleated villages within them, largely on a random basis. Individuals from similar social, geographical and kin backgrounds were often deliberately separated. Generally, these criteria seem to have been followed. In our sample, some 90 percent 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 had been peasant farmers (66 percent) with the remainder being landless labourers on commercial farms, workers in the rural informal sector or wage earners in the urban sector.

Individuals settled on these schemes were required to renounce any claim to land elsewhere in Zimbabwe. They were not given ownership of the land on which they were settled but instead were given permits covering occupancy of homes and for cultivation. Each household was allocated 5 hectares of arable land for cultivation, with the remaining area in each resettlement site being devoted to communal grazing land. Households were also allocated a residential plot within a planned village. Indeed, these settlements are more akin to the villages of south Asia than they are to the homestead pattern of settlement typical of much of rural Zimbabwe. In return for this allocation of land, the Zimbabwean government expected male heads of households to be farmers. Until 1992, male household heads were not permitted to work on other farms, nor could they migrate to cities, leaving their wives to work these plots.

Approximately 400 households were first interviewed over the period July-September 1983 to January- March 1984. These households are located in 20 different villages (two additional villages were added to the sample in 1993). Just over half (57 percent) are found in Mupfurudzi with 18 percent located in Mutanda and 25 percent found in Sengezi. They were re-interviewed in the first quarter of 1987 and annually, in February and March, from 1992 to 1998. There is remarkably little sample attrition. Approximately 85 percent of households interviewed in 1983/84 are still in the sample. There is no systematic pattern to the few households that drop out. Some were inadvertently dropped during the re-surveys, a few disintegrated (such as those where all adults died) and a small number were evicted by government officials responsible for overseeing these schemes.

These particular characteristics of the sample provide it with properties that are desirable from the point of view of evaluating trade-offs between emergency and development relief. First, there is no requirement to address biases brought about by sample attrition. Second, as Rosenzweig and Wolpin (1988) have argued, examination of the impact of any public intervention is hampered by considerations of selective migration and endogenous program placement. There are strong *a priori* grounds for believing that neither

of these will affect our results. Relocation of these households preceded, by a significant period of time, the drought relief that was provided in the 1990s and which, as discussed below, is the principal focus of the study. The availability of repeated observations makes it possible to control for any correlation between program placement and fixed, unobserved characteristics.

In this paper, we use the 1994 to 1997 rounds, giving us a four year continuous panel. The data were collected at the height of the hungry season and use a 12 month recall period. Our respondents organise their activities around the agricultural cycle. Planting occurs in October, with harvests occurring in May and June and so the work reported below is based on the following cropping (October-September) years: 1992/93; 1993/94; 1994/95; and 1995/96. Data were collected on a variety of variables including: household demographics and changes in composition; asset levels, sales and acquisition; inputs into agricultural production; crop output and sales; livestock; sources and levels of non-agricultural income; child and adult anthropometry and; morbidity. A limited amount of expenditure data is available from 1993/94 onwards. Importantly, the data set includes detailed, long-term information on households' access to various forms of drought relief, including direct food aid, supplementary feeding of children, food for work and in 1994/95, the grain loans scheme.

(ii) Rainfall and incomes: 1992/93 - 1995/96

Figure 2 outlines levels of rainfall by agricultural year and settlement scheme from 1980/81 to 1995/96.[3] The dominant feature of this period has been two droughts. The first, in 1991/92 , is commonly regarded as the worst experienced this century. Although it was followed by two years of better rainfall, both 1992/93 and 1993/94 had below long-run average precipitation. A further drought occurred in 1994/95, followed by a recovery in 1995/96. Figure 2 also brings out the agro-climatic differences that exist across these different resettlement areas. Mutanda has consistently received less rainfall than either Mupfurudzi or Sengezi.

The prohibitions on outside employment that existed right up to the beginning of our four year panel, together with these observed patterns of rainfall, suggest that household incomes will be dominated by agriculture and strongly affected by the weather. Table 1 illustrates the former argument, Figures 3a and 3b the latter.[4]

[3]. These data were supplied by the Zimbabwe Meteorological Services and are based on monthly reports for weather stations in closest proximity to our survey sites. For the period 1991/92 to 1996/96, we take the average of the fours stations closest to Mupfurudzi and the three stations nearest Mutanda and Sengezi.

[4]. The derivation of the figures on crop income are outlined in section 4.ii..

Figure 2. Average Annual Rainfall by Resettlement Scheme

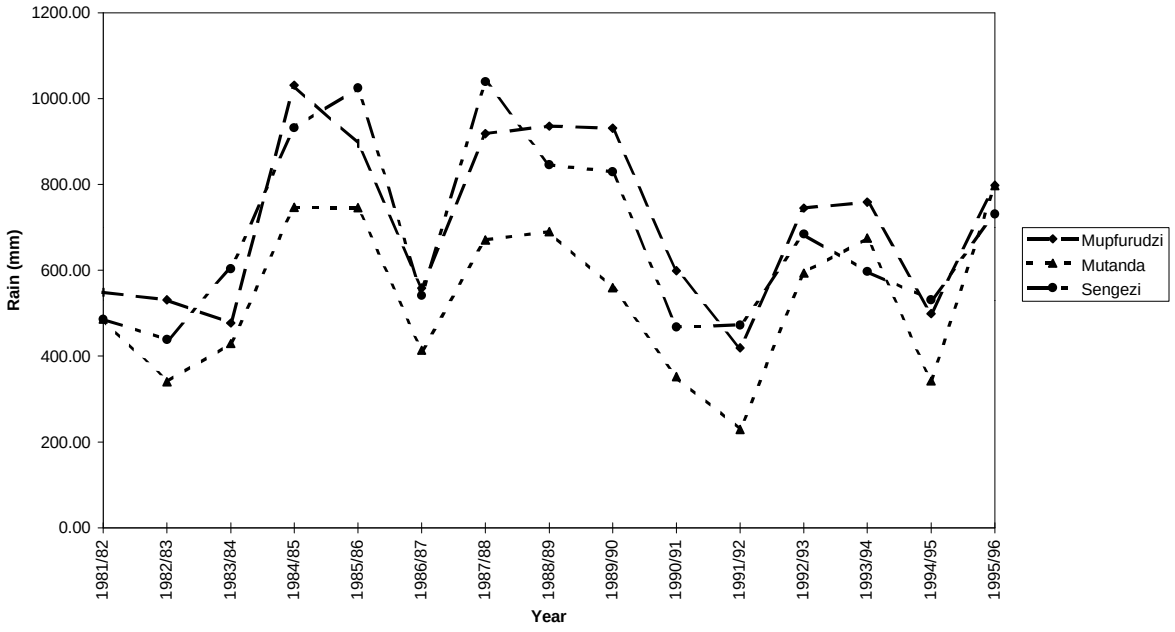


Table 1: Sources of income by year

Source of Income	***1992/93***	***1993/94***	***1994/95***	***1995/96***
For poorest households				
Crop production	79	72	31	70
	71	*66*	*28*	*63*
Livestock	9	12	22	12
	12	*12*	*20*	*13*
Off-farm work	7	10	15	12
	8	*12*	*12*	*15*
Private transfers	0.3	2	1	3
	0.4	*2*	*1*	*3*
Public transfers	4.7	4	31	3
	8.6	*6*	*39*	*6*

Figure 3a shows average real per capita household income by scheme over this four year period. (The CPI is used as the deflator.) As one would expect, the pattern is strongly influenced by the rainfall patterns described above. A second influence, depicted in figure 3b, is the increase in average household size, from 7.2 to 8.4 persons, over this period. This increase is driven by the situation in Mupfurudzi where household size increased by 21 percent, compared to 11 percent in Mutanda and 3 percent in Sengezi. The drop in income adjusted for household size in 1994/95 is less dramatic for Mutanda, as household size temporarily declines from 8.4 to 7.8.

Figure 3a. Average Income

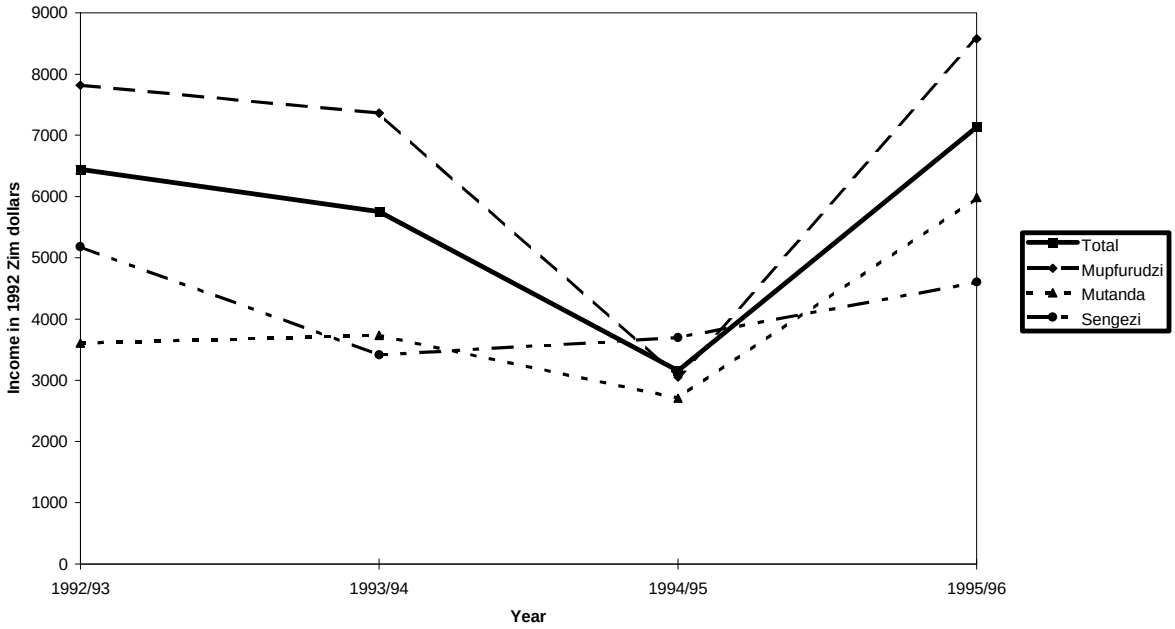
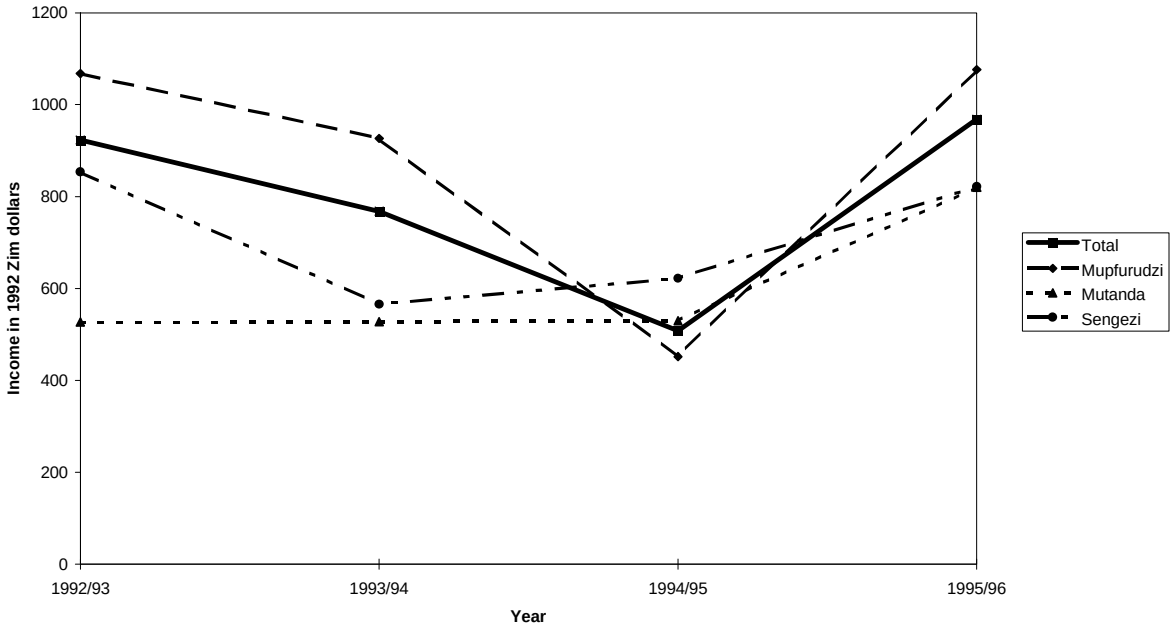


Figure 3b. Average Income Adjusted for Household Size



Mupfurudzi, situated in the most favourable agro-climatic zone, with the highest mean levels of rainfall and best quality soil, has on average higher levels of income from crop production than the other two schemes. Conversely, Mutanda which is situated in the worst agro-climatic region, with the lowest mean rainfall and poorer quality soils, reports consistently the worst income from crop production.

(iii) Poverty: 1992/93 - 1995/96

Our discussion of the levels and changes in per capita income provide some information on these households. However, to use this information to comment on standards of living requires a benchmark - such as a poverty line - to which these income levels can be compared. The construction of such poverty lines is seldom unproblematic, requiring estimates to be made of the cost of acquiring sufficient food to lead an active and healthy life and those expenditures on nonfood goods necessary to attain some minimum standard of living. Ravallion (1992) and Lipton and Ravallion (1995) outline these issues in more detail.

In 1990/91, Zimbabwe undertook a large, detailed, nationally representative survey of incomes, consumption and expenditures (ICES). These data were used to generate two poverty lines for rural areas: a lower or food poverty line of Z$209 per person - based on the need to meet minimum nutritional requirements and the food preferences and choices made by the poorest 30percent (in relative terms) of sampled households - and an upper or consumption poverty line of Z$340 that made an allowance for expenditures on housing, clothing, education, health and transport. Making an allowance for inflation, this poverty line rises to Z$596 in 1993 (Government of Zimbabwe, 1994, World Bank, 1995). A second poverty line has been constructed from data collected as part of the Poverty Assessment Study Survey (PASS) conducted in 1995. This also produced a food poverty and consumption poverty lines for rural areas of Z$1180 and Z$1924 respectively (Government of Zimbabwe, 1996). Deflating these to 1993 levels yields a food poverty line of Z$574 and a consumption poverty line of Z$939, levels roughly 40 per cent higher than those generated by the ICES.

Accordingly, the first issue to face is whether to use the ICES poverty lines, the PASS poverty lines or some combination of the two. Both surveys had shortcomings. Although the ICES collected very detailed information on incomes and consumption, it collected no price data and used one month recall periods which thus overlook seasonality considerations (Jenkins and Prinsloo, 1995). The PASS survey was conducted much more quickly - typically taking less than an hour to complete- covered a much more ambitious range of topics and thus obtained far less detailed information on income or expenditures. As noted in the PASS preliminary report, "the nature of the study itself being a poverty study, there is a general tendency to under-report incomes and to exaggerate poverty in anticipation of expected assistance" (Government of Zimbabwe, 1996, p. 85). In our view, and one echoed in other recent studies of poverty in rural Zimbabwe such as Cavendish (1998), the PASS poverty lines are implausibly high. For example, applying the PASS food poverty line to the 1992/93 income data yields an incidence of food poverty of around 35 per cent. This occurs in a year of good rainfall and where the vast majority of households report that they were self-provisioning in terms of food (Kinsey, Burger and Gunning, 1998). By contrast, the ICES food poverty line yields a food poverty incidence of only 15 per cent. Consequently, we use the ICES poverty lines as the basis for our poverty estimates and counterfactuals.

Three additional issues must also be addressed. First, we need to take account of regional differences in prices. Both the ICES and PASS provide this information by province. Normalising prices in all rural areas to unity, the ICES gives prices for our resettlement schemes of 0.94 in Mupfurudzi (Mashonaland Central), 0.86 in Mutanda (Manicaland) and 1.00 in Sengezi (Mashonaland East). The PASS gives 0.93 for Mupfurudzi, 1.02 for Mutanda and 1.04 for Sengezi. Clearly, using either the ICES or PASS deflators will make little difference in the case of Mupfurudzi or Sengezi, but a substantial difference for Mutanda. Although we have argued that the ICES was the superior survey, we have also noted that it did not collect price data. Further our own observations of prices of

retail goods in shops in all three schemes for items such as cooking oil and salt suggest that in general prices are slightly higher in Mutanda than in the other two schemes. Consequently, we take the average of weights provided by these two surveys as our price deflator weights.

Second, Zimbabwe has experienced very substantial inflation during this period. Our poverty lines are, therefore, updated yearly using the rural CPI. Note that we are assuming that increases in prices over time do not differ across our three survey sites.

Finally, note that the ICES reports that the value of imputed rents in resettlement areas is about 12 per cent of total household income or about 10 per cent of total household consumption (expenditures plus imputed values of consumption in kind). In areas such as resettlement schemes where a rental market does not exist, this is calculated by taking "the value of construction multiplied by a normal interest rate on savings plus depreciation" (Government of Zimbabwe, 1994, p. 52). Further, housing costs are a component of the consumption poverty line, though its exact contribution is not specified. Virtually all our survey respondents live in dwellings that were built for them as part of the resettlement program. Although households were supposed to repay the costs of construction, in practice this did not occur. Further, our households do not own the land on which their houses sit. These considerations, together with the difficulty of obtaining sensible estimates of 'normal interest rates' and 'dwelling depreciation' make it impossible to estimate the imputed values of rents for our surveyed households. Consequently, a direct comparison of our income figures with the ICES consumption poverty line will overstate poverty. Our solution, therefore, is to modestly reduce (by 10 per cent) the non-food component when calculating the consumption poverty line.

We use standard Foster-Greer-Thorbecke (1984) measures of poverty.[5] Let y be per capita income with density function f(y) and a cumulative distribution function $F(y) = \int_0^y f(x)dx$. For a given poverty line denoted as z, poverty is given by p(y, z). The value of aggregate poverty - the total amount of poverty within the population is $P(z) = \int_0^y p(y,z) f(y)dy$. Define p(y, z) as $[(z-y)/y]^a$ where $a \geq 0$. The a is a measure of how sensitive the index is to transfers between poor units. Note that setting $a = 0$ - the P0 poverty measure - is equivalent to setting p(y,z) =1 so P0 is the headcount measure. Now suppose that $a = 1$, a P1 measure. This captures the depth of poverty in that it is affected by both poor households' distance from the poverty line as well as the number of poor households. The P1 poverty measure captures both the incidence and severity of poverty. However, under certain circumstances, it may not adequately capture differences in the severity of poverty. This can be resolved by setting a equal to some value greater than 1. Doing so puts greater weight on the severity of poverty amongst the poor. Table 2 reports the incidence and severity of poverty by year and scheme using both the consumption and food poverty lines.

[5]. This paragraph draws heavily on Kakwani (1993), Kanbur (1987) and Ravallion (1993).

Table 2. The incidence and severity of poverty by scheme and year

	1992/93	1993/94	1994/95	1995/96
P0 consumption poverty *P0 food poverty*				
All households	0.35 *0.19*	0.48 *0.30*	0.65 *0.35*	0.32 *0.15*
By scheme				
Mupfurudzi	0.27 *0.13*	0.40 *0.23*	0.68 *0.39*	0.26 *0.11*
Mutanda	0.60 *0.41*	0.63 *0.44*	0.67 *0.25*	0.41 *0.19*
Sengezi	0.37 *0.19*	0.54 *0.37*	0.54 *0.31*	0.40 *0.21*
P2 consumption poverty *P2 food poverty*				
All households	0.080 *0.034*	0.118 *0.051*	0.127 *0.048*	0.060 *0.025*
By scheme				
Mupfurudzi	0.053 *0.020*	0.088 *0.036*	0.152 *0.063*	0.042 *0.013*
Mutanda	0.183 *0.097*	0.174 *0.078*	0.078 *0.016*	0.100 *0.059*
Sengezi	0.071 *0.023*	0.149 *0.067*	0.103 *0.035*	0.075 *0.031*

These data are consistent with the data reported for household per capita incomes. Mutanda, with poor soil quality, difficult climate conditions and a disparate population, has consistently high levels of poverty compared to Mupfuruzdi and Sengezi. However, Mupfurudzi, the scheme most dependent on crop production for income, experiences significant increases in poverty in drought years. Sengezi is slightly buffered from the 1994/95 drought by its income from non-agricultural activities. After the 1991/92 drought Sengezi has had consistently higher levels of income from non-agricultural activities than the other two schemes - 11 percent in 1992/93, 17 percent in 1994/95, 20 percent in 1995/96, and 22 percent in 1995/96. Although both Mutanda and Mupfurudzi had similarly high levels in 1995/96 in other years they earned very little of their income from these sources.

(iv) Participatory rural appraisal: Poverty and wealth ranking

There is increasing concern amongst development practitioners that formal household questionnaires may not converge with villagers' perceptions of poverty. Many argue, (Mukherjee, 1992, Adams et al. 1997) that in order to improve the selection of beneficiaries for poverty alleviation programmes, and hence the success of such programmes, the gap between standardised household interviews and villagers' perceptions must be bridged. Participatory rural appraisal and related approaches have been developed to provide alternative qualitative measures of socio-economic welfare. Within these approaches wealth ranking, which involves individuals or groups in a village categorising households into

wealth piles according to their own selection criteria, has become one of the most widely used techniques.

Before carrying out detailed quantitative analysis, therefore, we test whether our survey data is a valid measure of economic welfare in comparison with villagers' own perceptions. To triangulate the relationship we conducted a broad participatory rural appraisal with wealth ranking as a key component in June 1997. Drawing on the PRA literature we followed the standard technique described in PRA Notes (1992) and Bevan (1997). In May 1992 IIED published a paper on 'Special Issues on Applications of Wealth Ranking' which was the outcome of a series of seminars at IDS, Sussex. Participants exchanged experiences with wealth ranking which were then documented in the PRA Notes. Subsequently, Bevan (1997) conducted a more recent and practical implementation of the guidelines.

We decided to select a random sample of 17 of the 22 villages surveyed each year[6]. The idea was that those villages not selected could act as controls for future surveys. The 1997 questionnaire had included some pilot qualitative questions. If we were to build on this in future it would be interesting to note whether the answers of villagers who had gone through the group discussions of the PRA exercise were different from those that had not.

In accordance with standard practice we chose three groups of informants to carry out the exercise. We conducted separate rankings by groups of men and women, and where available, the agricultural extension worker. The extension worker was a natural choice given, first the importance of crop income for the households, second, the importance of extension advice in determining crop income, and finally because the extension worker knows the farmers well. In Mutanda, however, the extension worker we had previously been dealing with had left. His replacement had only just been appointed, and did not feel he knew the villagers well enough to carry out a ranking. The chairman of the village, who had been elected by the villagers, was chosen as a suitable replacement. In Sengezi, we have rankings by both extension worker and chairman, because the extension worker was absent for most of the period of the appraisal. On his return we used the extension worker to carry out a second set of rankings. Both rankings are shown in tables 3 and 4.

The process of wealth ranking started by asking villagers how they defined wealth and poverty. Having already prepared a card for each household, villagers were asked to sort the cards into as many piles as they liked indicating the different wealth groups. Once the cards were sorted into wealth piles villagers were asked to review the piles and make any changes they felt necessary. Villagers were then asked to describe the key characteristics of each household and explain why it had been placed in its particular pile. This lead to a general discussion describing the key features of each pile.

In Mupfurudzi we selected 6 of the 9 villages surveyed each year. We later added a seventh village, Tongogara, which the other villagers continually referred to as the wealthiest in the scheme. We thought a ranking in this village would be useful, but on visiting found that most people were busy working in their fields. Instead of group rankings we conducted an individual ranking with the head of one of the wealthier households which we could compare with the extension worker and our income variable.

The number of households in each of the villages in this scheme ranged from 13 to 43. In most cases all households were surveyed and ranked. As mentioned this resettlement scheme is the wealthiest of the three surveyed and responded best to the wealth ranking

[6] The random sample was chosen by drawing cards out of a hat.

exercise. Generally, the meetings were well attended and generated lively discussion. A notable exception was the women's ranking in Mudzinge, a village of apostolics characterised by polygamous marriages. There was a strong feeling of disunity between the women and a distinct distrust of outsiders. One house had the slogan 'come as a visitor, not as a spy' painted on the wall.

In Mutanda we selected 5 of the 7 villages. The average number of households in these villages was 30, but due to the set up of the survey only 10 households had been regularly interviewed. We limited the ranking to these 10 households, although allowed other households to be ranked if they wished. This proved to be the most difficult scheme. Attendance was low, people seemed very suspicious, and responses to the exercise were hesitant. There was a pervasive atmosphere of distrust and disunity amongst the participants. An example of this lack of unity showed itself in the naming of villages. Whereas villagers in other schemes gave their villages names, those in Mutanda were unable to agree and continued to refer to their village by the government assigned numbers. The reason for the disharmony seemed to lie in a pattern of resettlement which was noticeably different from that in the other schemes. The settlers in Mupfurudzi and Sengezi had strong political awareness having been moved as part of a planned Government strategy of resettlement. In Mutanda, by contrast, the settlers were squatters, with very little political solidarity, who were simply allocated the land they were squatting on by the Government.

A ranking that did go well, however, was the men's group in Village 24, although it did transpire that they had a clear objective in mind. We were later informed that they had had a meeting before our arrival where it had decided they would ask us for a dam and fencing. All discussion came back to the need for these items.

In Sengezi we selected 4 of the 6 villages. The number of households in these villages ranged from 35-55 stands, with on average half the village being surveyed each year.

One method of wealth ranking is to identify individuals from different wealth groups and do individual rankings. In Goto we asked the chairman to rank households in the village. Then using his identification we did individual rankings with the wealthiest and poorest households.

In general the meetings in Sengezi started slowly, although attendance increased to quite large numbers throughout the meeting. Rankings tended to centre around the general rather than specific case, perhaps due to the large number of villagers who attended the meetings. The men's rankings were quite lively, while the women's were mixed. In Mongo the women seemed bored and unresponsive, whereas in Mawiri West they were very 'upfront'.

In line with other papers that have compared the technique of wealth ranking and household surveys (Scoones, 1988 and Adams et al. 1997), this paper finds that there is remarkable correlation between the results of these two methods.

At the group level villagers identified poor households on a number of criteria, most of which were visible characteristics. Ownership of cattle, farming equipment, quality of housing and health/education of children were noted in almost every village. When prompted for individual household characteristics villagers listed attributes such as the skill of the farmer, whether the household had a bank account, and the variety of food eaten within the household. While men tended to focus on farming assets and quality of farming, women often included ownership of household items, condition of clothing, and co-operation within a household as key factors determining a households ranking. The agricultural extension workers focused on farming skills, in particular whether the head had a farming certificate or

attended extension meetings. The extension workers also tended to rank households engaged in off-farm activities as poor, irrespective of their income from other activities.

To compare villagers' wealth rankings with the survey data we first, compared the men, women, and extension worker rankings with each other, and then calculated an average of the three rankings. These groups and group average rankings were then compared with a number of income and capital variables. Table 3 reports the results of rank correlations between average household income over the period 1992/93 to 1995/96 with the villagers' own wealth rankings[7]. Table 4 similarly compares capital ownership over the same period with villagers' rankings[8].

Before commenting on the tables it is worth noting that we ran rank correlations on a number of quantitative variables, including: current income, current capital ownership, total crop income and current crop income. Broadly the total average income and capital ownership variables correlated best. They are also the variables used in the quantitative analysis, and therefore are the results we present and discuss.

Table 3. Rank correlation between villagers perceptions and total average household income over the period 1992/93 -1995/96

Village	Men	Women	Extension Worker	Average of 3 rankings	Other	No. of households
Mupfurudzi						
11 Chitepo	0.738***	0.755***	0.723***	0.828***		22
12 Mudzinge	0.758***	0.446	0.774***	0.793***		19
13 Muringamombe	0.396	0.464	0.522*	0.580**		25
14 Mutoramhepo	0.482	0.651	0.257	0.707*		13
15 Pedzanhamo	0.693	0.693	0.326	0.715		11
17 Tongogara			0.463**	0.547***	0.561***	37
19 Zvomanyanga	0.596**	0.767***	0.497	0.669***		23
Mutanda						
21 Mt Zonwe	0.802	0.791	0.257	0.795	0.784	7
23 Village 14	0.353	0.465		0.484	0.566	10
24 Village 10	0.875**	0.529		0.720	0.526	8
26 Village 8	0.650	0.625		0.698	0.503	9
27 Village 6	0.791	0.798*		0.811*	0.767	9
Sengezi						
31 Mongo	0.736***	0.456	0.618	0.644	0.438	16
32 Goto	0.486	0.540	0.398	0.519	0.406	21
33 Rundu	0.544	0.329	0.343	0.451	0.145	20
36 Mawiri East	0.645	0.696	0.644	0.737*	0.679	12
37 Mawiri West	0.754**	0.931***	0.546	0.863***	0.708*	12

In this and subsequent table: *, **, and *** denotes significant at the 10% , 5% and 1% levels respectively;
Village 17- other refers to household 1701; Village 21 - other refers to household 2105. Not chairman, but 'top guy' in village; Village 23 - other refers to two latecomers, households 2308 and 2303;
Village 24, 26, 27, 31, 33, 36 and 37 - other refers to village chairman;
Village 32 - men refers to rich household, women refers to poor household, other refers to village chairman.

[7] Average household income comprises of crop income, livestock income, income from off-farm activities and public and private transfers.
[8] Capital refers to the ownership of agricultural tools, equipment and trained oxen.

Table 4. Rank correlation between villagers perceptions and average capital ownership over the period 1992/93 -1995/96

Village	Men	Women	Extension Worker	Average of 3 rankings	Other	No. of households
Mupfurudzi						
11 Chitepo	0.487	0.397	0.405	0.488		22
12 Mudzinge	0.512	0.654**	0.454	0.621**		19
13 Muringamombe	0.585**	0.362	0.313	0.529*		25
14 Mutoramhepo	0.174	0.234	0.411	0.422		13
15 Pedzanhamo	0.866***	0.722	0.363	0.821**		11
17 Tongogara			0.184	0.320	0.553***	37
19 Zvomanyanga	0.723***	0.742***	0.397	0.648***		23
Mutanda						
21 Mt Zonwe	0.267	0.474	-0.086	0.265	0.294	7
23 Village 14	-0.082	0.140		0.076	0.174	10
24 Village 10	0.895**	0.858*		0.922***	0.724	8
26 Village 8	0.765	0.417		0.624	0.626	9
27 Village 6	0.791	0.798*		0.811*	0.767	9
Sengezi						
31 Mongo	0.883***	0.737***	0.576	0.790***	0.909***	16
32 Goto	0.739***	0.780***	0.570*	0.763***	0.662***	21
33 Rundu	0.765***	0.750***	0.642**	0.791***	0.748***	20
36 Mawiri East	0.797**	0.676	0.529	0.730*	0.582	12
37 Mawiri West	0.401	0.665	0.854***	0.747**	0.466	12

The overall conclusion is positive. There is generally a striking correlation between villagers' identification of wealth groups and levels of income and capital ownership. It should be noted that some of the coefficients are high but not significant, especially in Mutanda. This is because the samples sizes in Mutanda are so small in a statistical sense that we lack the power to find a correlation. In the larger samples the relationships come across more powerfully. The success of the correlations centre on two key features; first, how the villagers defined poverty; and second, the unity or cohesion within the village.

Although the general notion of poverty is the same across villages there are slight differences in emphasis. These differences are important in determining which quantitative variable the wealth rankings correlate with, and are useful in explaining discrepancies. The agricultural extension worker ranked households according to farming ability, irrespective of income earned from non-farm activities. Given the importance of agriculture as a source of income, this meant that in many cases the rankings of the extension worker were correlated with the income variable. Where off-farm income was important, however, there could be discrepancies. In Mutoramhepo the extension worker ranked a household with the third highest level of total income as the poorest. His reasoning was that 'Madawu tries his level best, but has problems with management which results in poor yields. He does engage in other projects like poultry which he is good at'. In Sengezi where households earn a greater proportion of their income from off-farm activities the extension workers' rankings were less significant. More specifically, in Mawiri West the extension worker identified those households where members worked on commercial farms as poor because they did not use their own land properly. Although not presented we also ran correlations with crop income. In this village the rankings of the extension worker were highly correlated with the crop income figure.

Similarly, the women tended to define wealth in terms of ownership of assets, including agricultural tools and household items, clothing and other visual characteristics. This was reflected in the high correlation of their rankings with the capital variables. They also tended to rank households higher than the men and extension workers, a finding also found in the study by Scoones (1988). An interesting example is Mudzinge where the rankings of the women were not correlated with those of the men, extension worker or the income variable. However, they were correlated with the ownership of capital. This suggests that their rankings were based on visual success rather than well-being. If there was as much distrust amongst the women that was portrayed during the meeting, then this result is not surprising. If there is limited information sharing then the only way that they could rank each other would be on the basis of visual assets. This criteria for ranking seems to identify wealthy households accurately, but is less useful for ranking the middle and lower income households. Consequently there was correlation between the women's rankings of the wealthier households and those of the men, extension worker and income variable. Rankings of the other households, however, was very poor.

The men tended to define wealth in terms of income from farming and assets owned for farming, but acknowledged the importance of off-farm income for some households. Their rankings were highly correlated with both total income earned over the period and capital ownership. In Pedzanhamo the men specifically said they ranked according to ownership of cattle and implements used for farming which explains the high correlation with the capital variable.

Another feature to note was the importance of harmony within a village in determining the correlation with the quantitative variables. Where a village was united and worked well together there appeared to be a high correlation with the total income variable. Where there was less cohesion in the village the rankings tended to correlate more with the visual variables, such as capital ownership or current income. There was a tendency for the small villages to be more united and have a feeling of greater solidarity, which resulted in their rankings being more in line with the quantitative variables.

Finally the ease with which the villagers responded to the ranking exercise was important. Where there was reluctance, the rankings were poorly correlated. In village 23 in Mutanda there was negative correlation with the capital variable. This was one of the most remote villages we visited with no bus route or local shops. The women were particularly uninterested in the exercise, while the men were hesitant about ranking the poorest households. While there was correlation between those ranked as the wealthiest, the ranking of the middle and poorer income households was more disparate. In contrast, where there was lively discussion there appeared to be high correlation, for example, Zvomanyanga where participants turned up over 15 minutes early for the meeting.

There were drawbacks in the wealth ranking method. It was apparent that where villagers wanted to they were capable of accurately differentiating between wealth groups. However, this differentiation was done on a broad base of criteria of which it was never clear which feature dominated, or if some unspecified criteria was implicitly considered. Nor did the exercise allow for quantifying the levels of wealth which made it impossible to compare across villages. In some villages there was a deep reluctance to participate which affected the results. A concern, found also by Adams (1997), is that the wealth ranking technique seems to be sensitive to a number of factors such as, the gender of the group doing the exercise, the number of households being ranked, the number of attendees at the meeting, and even the attributes of the facilitators. Nevertheless, it was useful to have discover information on

conflict and harmony within villages that otherwise would not have been available.

The general conclusion is that, in line with similar studies, we found that the correlations between economic indicators measured by the standard household survey and those defined through a wealth ranking exercise, to be positive and significant. The broad consistency of these results therefore supports the results of the survey data.

4. Parameterising the relationships

The next step involves combining the conceptual framework outlined in section 2 with the data described in section 3. In doing so, it is necessary to recognise that we do not have data available to parameterise all the relationships described in Figure 1. It should also be noted that our description of the sample indicates that certain causal relations are more important than others. Specifically:

1. crop income is the single most important source of household income, accounting (in non-drought years) for 75 percent or more of total household income and 70 percent of income for households defined as poor. Accordingly, understanding the determinants of crop income should be an important component of our analysis;
2. the literature on the extent to which public transfers and other sources of household income can reduce, or crowd out, private transfers suggests, in some cases, that crowding out can be on the order of close to 100 percent (Cox and Jimenez, 1995). For this reason, this relationship should also be estimated;
3. non-agricultural income sources, such as wage labouring, the production and sale of handicrafts and beer, are of relatively little importance outside of drought years. In drought years, revenues from such sources are likely to be constrained by insufficient demand rather than labour-leisure choices. Consequently, these are assumed to be exogenous;
4. failing to account for potential second round effects risks missing the cumulative effects of a given intervention. Thus, we consider the relationship between increased incomes and the accumulation of capital stock;
5. we do not have data on expenditures for the full time span of the panel. Consequently, we evaluate the effect of changing relief and development interventions in terms of their impact on poverty relative to income.

Thus, our next step is to parameterise three relations: between crop incomes, and factors of production; private transfers, public transfers and other sources of household income; and the determinants of investment in capital stock. We begin by providing a brief description of our approach to estimating these relationships and then turn to the specific results for each.

(i) Estimation issues

In cross-section regression analysis there is always concern about correlation between regressors and unobservable characteristics giving rise to a problem of omitted variable bias. If, however, it is assumed that these unobservables are constant over time for a given cross-sectional unit or are the same for all cross-sectional units at a given time, or a combination of both, they can be absorbed into the intercept term of a regression model as a means to explicitly allow for the individual and/or time heterogeneity contained in the temporal cross-sectional data. This requires repeated observations on the same unit of observation, panel data.

For example, consider the impact of agricultural extension services on crop production. Birkhaeuser, Evenson and Feder (1991) argue that the observed difference in crop yield between farmers who receive a visit by an extension worker, and those that do not, is largely a result of the extension workers visit, and the knowledge imparted. However the difference in output could be independent of the extension worker, and instead correlated with some other factor, say the quality of the farmers' land. A correlation between unobserved land quality and agricultural extension advice generates the misleading result that extension increases farm output. Assuming that land quality is fixed, it is possible with panel data to distinguish between the two hypotheses by studying changes in yield over time of a farmer who once received extension advice and then did not, or vice versa. If it is accepted that extension services do not increase output, then the farmers yield should not be affected when he stops receiving the advice, independent of the quality of the land. On the other hand, if it is accepted that extension services do increase output, it would be expected that the farmers yield would decrease if he stopped receiving extension advice, again independent of the quality of the land. By following individual farmers over time as they change status, it is possible to construct a proper recursive structure to study the before and after effect.

Following Deaton (1997), in econometric terms consider this relationship between crop income received by the household (y), extension advice (x) and other relevant variables such as land quality which are fixed and unobservable (q). Denoting the subscripts i and t as referring to household i at time t, write:

$$y_{it} = b' x_{it} + q_i + m_t + u_{it}$$

The error term is decomposed into two components: m_t is a time effect that applies to all individuals in the sample at time t, and u_{it} is a white noise disturbance term. Note that if there is correlation between m_t and x, ordinary least squares estimation will yield biased and inconsistent parameter estimates.

With panel data we have more than one observation on each of the sample points which allows us to remove the fixed effects by taking differences, or where we have more than 2 observations by subtracting the individual means. When T is greater than 2 we can write:

$$y_{it} - \bar{y}_i = (m_t - \bar{m}) + b'(x_{i2} - \bar{x}_i) + u_{it} - \bar{u}_i$$

Because the individual fixed effects q have been removed the regression is free of any correlation between the explanatory variables (in our example, extension advice) and the unobserved fixed effects (land quality), and so the parameters can be estimated consistently.

However, eliminating the fixed effects is not costless. The number of observations falls (with T periods one is sacrificed to control for the fixed effects) which could lead to an imprecise or inefficient estimate. This may be worse than a biased estimate. As Deaton argues (1997) the tradeoff between bias and efficiency has to be made on a case-by-case basis. For this reason we include the results of the OLS regressions, the pooled OLS regression, and the fixed effects regression. We also report the results on the F-test which tests if individual household effects have no impact on the dependent variable, (in this example, income), and the Hausman specification test which tests whether the errors are uncorrelated with the variables. If the set of coefficients that are estimated by the fixed effects estimator and the coefficients estimated by the random effects estimator are not significantly different then we can accept that the errors are uncorrelated with the variables.

(ii) Crop Income Function

In this sub-section, we explain how we model the determinants of crop income. We begin with a brief theoretical description before moving on to describe the data we use to estimate this relationship and to present the econometric results based on the estimation methodology described above.

Define profits from producing crops as revenues minus costs. If the household produces n crop outputs (y_1, y_2,..,y_n) and uses m inputs (x_1, x_2,…,x_m), and prices of the output goods are (p_1, p_2,…,p_n) and of the inputs (w_1, w_2,…,w_m), the profits the households receive, p , can be expressed as

$$p = \sum p_i \cdot y_i - \sum w_i \cdot x_i \qquad (4.ii.1)$$

Note that y_i is a function of the application of the inputs (x_1, x_2,…,x_m) so we can re-write this as:

$$p = \sum p_i \cdot f_i(x_1, x_2, ..., x_m) - \sum w_i \cdot x_i \qquad (4.ii.2)$$

Assuming that households are profit maximisers they will combine their factors of production in a way so as to maximise their profit. By partially differentiating 4.ii.2 with respect to various inputs we can obtain the standard result that farmers will use these inputs to the point where their marginal return equals their marginal cost. Our interests are somewhat different. We would like to know how an increase in x will affect real profits p .

To do this, we estimate the relationship between net crop income, and factors of production in real terms. Because we consider profits in real terms we do not have to worry about nominal price effects. As the construction of these variables is not entirely unproblematic, we describe these in detail beginning with the dependent variable, net crop income, then output prices and finally the factors of production.

As discussed in section 4.ii the dependent variable, net crop income, is the most important source of income for households in this sample. Households planted, on average, 2 out of 3 of the following crops: maize, cotton, tobacco, sunflowers, groundnuts, *nyimo, rapoko, mhunga*, (types of grain) and sorghum. Of these maize is the most important source of cash income for the majority of households. Over the period of the sample 99 percent of farmers grew maize. For households in Mupfurudzi cotton is also an important source of cash income.

During each interview, households were asked to report yield, sales and retention, for each crop, for the previous harvest. Gross income from crop production was calculated as total yield (sales plus retention) multiplied by the price. Price was calculated for each household by dividing total sales value by the quantity sold. Where a household did not sell any of the crop, its total yield was multiplied by the median price for the sample. Net income was calculated by subtracting the cost of fertiliser and hired labour from the gross figure[9].

Inputs into crop production include agricultural capital stock, labour, land, fertilisers, human capital and rainfall. We describe the construction of each of these, beginning with a key variable for our analysis, capital stock.

We define agricultural capital stock as consisting of those tools and equipment used in crop production. This includes the following: ox-ploughs, scotch carts, cultivator/harrows, ox-planters, water carts, cotton sprayers, wheelbarrows, tractors and tractor equipment, hoes, axes, spades, machetes and slashers. Information on holdings of these tools is available for

[9] Households were asked how much fertiliser and pesticide they had used, this was then converted to a monetary value by multiplying the quantity by the prices quoted by 'Windmill' the main distributor of fertiliser in Zimbabwe. Households were also asked if they had used hired labour and how much they had paid.

all four years considered here. Valuation of the items, however, is slightly tricky.

As part of the surveys, households were asked about what items they owned, when they were obtained, how much they paid for it, and how much it would cost today. Answers to these questions revealed two problems. First, a number of households did not remember, and therefore report, what they had paid for the item. Second, the prices of virtually identical items were highly variable between households, perhaps due to problems of recall or differences in knowledge regarding current prices. Rather than allowing the price of capital goods to vary across households, we impose a uniform price across households. Specifically, the median purchase price of items both acquired and reported for the crop year 1995/96 were used as a base. (These are assumed to suffer fewer recall problems.) These were then deflated using the consumer price index to derive prices for other survey years. As an example, the median reported purchase price for an ox-plough in 1995/96 was Z$775. Table 7 compares the deflated values of this figure with the median reported buying prices in previous years. The correspondence between these is reasonably close, although our method appears to overstate the value of an ox-plough.

Table 7: Comparison of deflated and actual median ox-plough prices

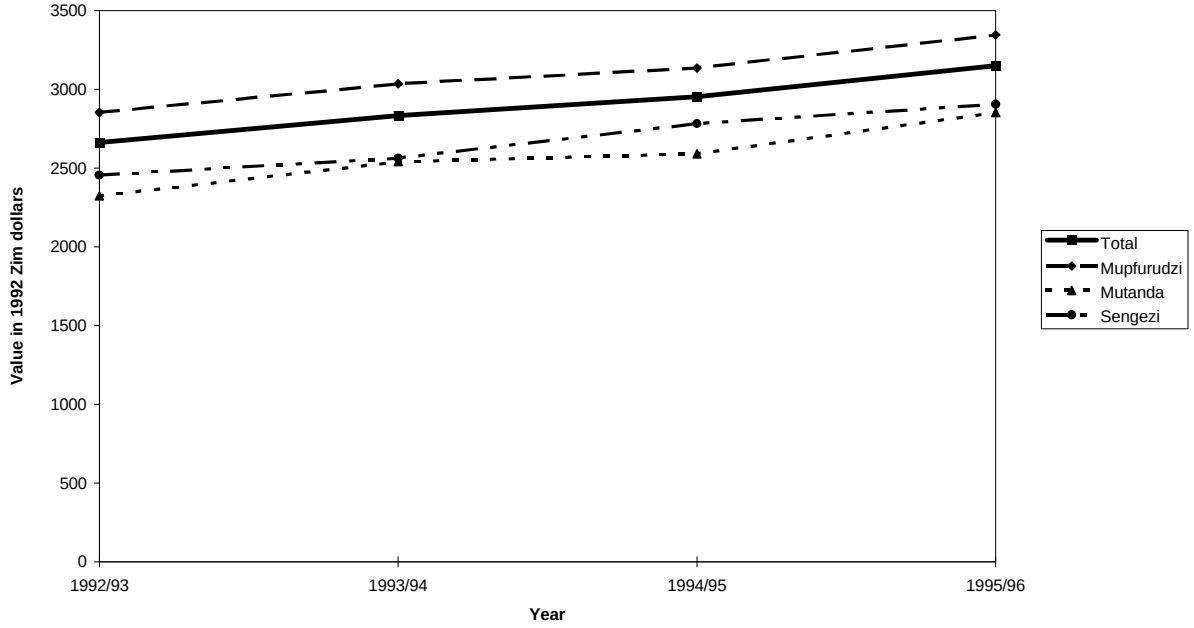
Year	1992/93	1993/94	1994/95	1995/96
(a) median reported purchase price	418	500	550	775
(b) 1995/96 price deflated by CPI	436	533	653	775
(a) / (b)	0.96	0.94	0.84	1

The validity of our approach relies on assumptions regarding the deflator and the treatment of depreciation. Specifically, we deflate the price of agricultural *capital* goods using a *consumer* price index. Implicitly, we are assuming that changes in the prices of the former broadly mimics the latter. Assets typically decline in value as a result of wear and tear. Many argue, for example, Anderson (1991), that this reduction in value must be reflected in the capital stock measure. In other words, depreciation should be deducted from gross investment in order to calculate the increase in capital which is relevant to explaining increases in output. The implication of this view is that each $1 of depreciation reduces output by as much as each $1 of gross investment increases output. Scott (1991), however, argues that a machine that produces the same quantity of output *ceteris paribus*, year-in year-out cannot be said to have experienced any depreciation. He argues that capital only depreciates when it becomes obsolete. For this reason, he argues that capital stock should be measured in gross terms, not net of depreciation. This argument reflects the situation facing households in the sample. Due to the nature of asset ownership within the sample it would lead to a gross underestimate of the contribution of capital to growth if conventional systems of growth accounting were used. Many households own and still use equipment handed down from previous generations. For example, in 1982/83 over 50 percent of households owned and used an ox-plough which was over 10 years old (11 percent of households owned an ox-plough over 30 years old). Therefore to measure the effect of capital under these circumstances the appropriate measure of capital stock is a gross figure, and not a net figure.

Figure 4 shows a steady increase in the ownership of capital in all three schemes. From an average of $2,663 per household, capital grew by 18 percent. The change in capital reflects the changes in income over the period. Starting from the low level in 1992/93 it steadily increased until the 1994/95 drought. During this period the level remained constant,

but increased with the better harvest the following year.

Figure 4. Ownership of Capital



Agricultural production is also dependent on livestock available for ploughing. Households were asked to report the number of trained oxen they owned and their value. The figures on value varied greatly therefore we decided to include the number of pairs of trained oxen as a related capital variable.

The ideal measure of labour usage would be days worked, by person and activity. Unfortunately, this measure is not available for all survey years. As a crude proxy, we take available family labour supply - defined as the number of people in the household between the ages of 15 and 64.

The principal chemical inputs used by the farmers include compound D as an initial fertiliser, and ammonium nitrate as a top dressing. In addition a number of farmers use pesticides. These are all measured in kilograms and added together to create the fertiliser variable.

Recent research has emphasised the importance of human capital as an input into agricultural production. This includes formal schooling, the acquisition of farming experience and access to agricultural extension. Since formal schooling is a fixed characteristic for adults in the household, it drops out when we first difference our variables. Acquisition of farming experience will be highly collinear with other variables that trend upwards and hence was excluded. However, access to extension advice provided by the Department of Agricultural Technical and Extension Services (AGITEX) - on land preparation, the timeliness of operations, crop spacing, plant population sizes, the use of better seeds and varieties, fertiliser use and pest control - varies significantly from farmer to farmer and from year to year. In particular, funding cut-backs are largely responsible for a drop in the percentage of households receiving at least one visit from an AGITEX officer from 76 percent in 1991/92 to 62 percent in 1995/96. In the results reported below, access to extension is measured as a dummy variable equalling one if the household received at least one visit that year and zero otherwise, $extension_2$ is one if the household had one or two

visits, extension$_3$ is one if the household had one to five visits, extension$_4$ is one if the household had one to ten visits, and extension$_5$ is one if the household had one to some indefinite number of visits. Hence beyond one visit the variables will be picking up the incremental effect of additional visits.

The final independent variable included is annual rainfall by scheme which has been described in section three.

Mean values in 1992 Z$, averaged over the four year panel, for these variables are:

Table 8: Mean values, in 1992 Z$, averaged over 1992/93 - 1995/96 for net crop income function

	Mean	Standard Deviation
Dependent variable		
net crop income	$4,285	5076
Independent variables		
capital tools	$2,900	1865
labour	4.97	2.69
land cultivated	3.50 hectares	1.37
fertiliser	744 kilograms	735

Using the estimation method described in section 4.ii, we estimate the following model in ordinary least squares and first differences. We present first, the OLS results for individual years, second, the results from the pooled sample, and third, the fixed effects estimates.

$$\log Y_{it} = a + b_1 \cdot \log K_{it} + b_2 \cdot O + b_3 \cdot \log L_{it} + b_4 \cdot \log A_{it} + b_5 \cdot \log F_{it} + b_6 \cdot X_1 + b_7 \cdot X_2 + b_8 \cdot X_3 + b_9 \cdot X_4 + b_{10} \cdot X_5 + b_{11} \cdot R \qquad (4.ii.3)$$

where: Y is net crop income; K is capital;
O is pair of trained oxen; L is labour;
A is land; F is fertiliser;
X_1 is at least 1 extension visit; X_2 is at least 2 extension visits;
X_3 is at least 3 extension visits; X_4 is at least 4 extension visits;
X_5 is at least 5 extension visits; R is rainfall;
a, b are parameters to be estimated;
and i denote households and t time.

Table 9: OLS estimates of the determinants of net crop income

	1992/93 n = 373		**1993/94 n = 392**		**1994/95 n = 230**		**1995/96 n = 386**	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff	t-stat.	Coeff	t-stat.
log of capital	0.310	2.948***	0.352	4.327***	0.137	0.930	0.203	2.394**
trained oxen	0.130	2.204**	0.181	2.773***	0.243	2.466**	0.107	2.506**
log of labour	0.168	1.843*	-0.089	-1.060	0.336	1.699*	-0.136	-1.656*
log of land	1.073	9.459***	1.144	9.366***	0.852	3.204***	1.200	10.215***
log of fertiliser	-0.02	-0.710	0.111	3.246***	0.023	0.366	0.054	2.370**
extension 1	0.312	2.144**	-0.062	-0.425	-0.102	-0.339	0.207	1.762*
extension 2	-0.015	-0.115	0.219	1.397	0.174	0.502	-0.023	-0.167
extension 3	0.007	0.060	0.050	0.289	0.082	0.285	-0.034	-0.255
extension 4	-0.040	-0.259	-0.167	-0.709	-0.248	-0.661	0.085	0.157
extension 5	-0.003	-0.021	0.030	0.134	0.706	1.591		
rain	0.006	5.782***	0.004	5.662***	0.004	2.325**	-0.004	-7.035***
constant	-0.827	-0.642	-0.653	-0.654	2.822	1.283	7.248	6.570***
R-squared	0.50		0.53		0.21		0.52	
F-test		32.11***		39.80***		6.04***		31.98***

In this and subsequent tables: *, **, and *** denotes significant at the 10% , 5% and 1% levels respectively;
T-statistic calculated using White's heteroscedasticity consistent standard errors.

Table 10: Pooled and fixed effects estimates of the determinants of net crop income

	Pooled n = 1381		**Fixed Effects n = 1381**	
	Coeff.	t statistic	Coeff.	t statistic
log capital tools	0.285	5.649***	0.324	3.093***
trained oxen	0.117	3.544***	0.010	0.203
log of labour	0.168	2.846***	0.168	1.576
log of land	1.091	14.430***	0.662	6.505***
log of fertiliser	0.119	6.756***	0.027	0.930
extension 1	0.381	4.410***	0.253	2.565***
extension 2	-0.104	-1.158	-0.007	-0.066
extension 3	-0.019	-0.215	-0.055	-0.563
extension 4	-0.085	-0.640	0.033	0.207
extension 5	0.042	0.297	0.009	0.048
rain	0.015	4.679***	0.022	8.603***
rain squared	-9.9e-6	-3.825***	-1.6e-5	-7.488***
constant	-2.828	-2.382**	-2.648	0.059
R-squared	0.42		0.55	
F-test	F(12,1368)	74.59***	F(398,970)	1.499***

Including year dummies did not significantly alter the coefficients or their standard errors.

The crop income function was also estimated by quantile regression which takes the median of the distribution rather than the mean. Under this method the coefficients and their standard errors did not significantly change.

The F statistic tests the hypothesis that all coefficients excluding the constant are zero. In all cases we can reject the null hypothesis.

The Hausman specification test tests whether the errors are uncorrelated with the variables. In this case we reject the null hypothesis that the differences in the coefficients are not systematic. That is, we accept that there are unobservable differences across households which are not random. Chi-squared (12) = 92.01***.

There are a number of interesting features to note. The coefficient on agricultural capital variable is well measured. Its magnitude is robust across alternative estimation procedures. Standard OLS estimations for each year, and estimations pooling all years, consistently yield coefficients close to 0.3 (although those in drought and flood years tend to be lower). The contribution of trained oxen to net income appears to be correlated with the farmer. When unobserved characteristics are controlled for, as in the fixed effects model, the coefficient on trained oxen falls and is no longer significant. In contrast the coefficient on tools increases and remains significant. Second, the coefficients on the extension variables show that while one extension visit significantly boosts income, additional visits have no impact. This has implications for policy which we will pursue later. Further, if we compare the pooled and fixed effects coefficients we see that when unobserved characteristics are controlled for the coefficient falls from 0.38 to 0.25. This suggests that one-third of the effect of extension on income is correlated with the farmer, but that most of the effect is independent of the farmer.

The coefficients on rainfall are, as expected, positive and significant, in non-drought years, and negative and significant in the flood year. The inclusion of the squared rain variable in the pooled regressions is to pick up this effect. Not surprisingly, up to a certain level of rainfall income will increase, beyond that level the effect on income will be negative.

(iii) The relationship between private transfers, public transfers and other sources of household income

We now turn to a consideration of a potentially important second round effect, namely the potential for public transfers or other sources of household income to induce reductions in the receipt of private transfers, a phenomenon also known as 'crowding out'. As in our discussion of the determinants of crop income, we begin with an explanation of how we justify the inclusion of the variables that appear in our regression results. We describe how these are constructed using our data set before moving onto a summary of our results.

Consider two households, i, where i = d, r.[10] Denote their income as Yi. Household d transfers money, PT, to household r. In addition, governments provide public transfers to these households Ji.

The relationship between private transfers and public transfers can be written as:

$$PT = pt(yd, (yr + J)) \qquad (4.iii.4)$$

The important parameter in an estimated version of (4.iii.4) is on (yr + J). Note that we have the following possible cases. If the parameter on (yr + J) = -1 the donor's motive is altruism and we have complete crowding out. If the parameter is 0 we have an exchange motive. Public transfers either have no effect on public transfers or they crowd-in private transfers. A parameter lying between 0 and -1 could be generated by either altruism, as in the case where public transfers are externally funded, or exchange. However, by imposing a little further structure, it is possible to separate these out. Under exchange, the implicit price of services is a function of the recipients income. There is no plausible reason why the composition of the recipients income would affect this relationship. This may not hold true if altruism is the motivation. Under altruism, the donor might treat income received as public transfers differently from that received as earned income. They could regard the latter as

[10].The theory underlying the response of private transfers to the provision of public transfers is based on work originally undertaken by Becker (1974) and extended by Cox (1987), Cox and Jimenez (1992) and Cox and Jakubson (1995).

being a more virtuous form of income.

The next step is to set out a form of (4.iii.4) that can be estimated with the data available here. The dependent variable is net private transfers received by the household. This is the difference between remittances received from relatives, neighbours and friends living outside of the household less transfers made by the household to these individuals. Where items were received or given in-kind, physical quantities are converted into monetary values using median prices calculated for similar goods. Average, net private transfers were Z$87 per household per annum (standard deviation: 485). Public transfers are a measure of the value of all types of assistance given to households by the state. It includes the monetary value of food aid, supplementary feeding, food-for-work, and in-kind transfers of fertiliser and seeds. In non-drought years, these tend to account for only a small fraction of household income, around 4 per cent. They are much more important during droughts, accounting for 19 per cent in 1994/95. In non-drought years average public transfers were Z$155 per household per annum, including the drought year Z$362 per household per annum (standard deviation: 606).

Our data set does not contain information on incomes of migrants and other individuals making transfers to our respondents. With the data available to us on out-migration: when they left; where they live and their current occupation, we do the following. First, we assume that earnings increase with experience, but these vary by occupation. That is, we might expect that the earnings profile for a low-skill migrant - say a farm labourer - will differ from a high-skill migrant such as a bank manager. Second, from the point of view of estimating the crowding out relationship, it is the total income of all migrants that is important. So what we include as our proxy for this is the total amount of job experience of all migrants by occupation (low, medium and high). Next, we create a second set of variables, the number of migrants living in different localities: local, rural; local, urban; non-local rural or urban; and major urban (Harare, Bulawayo, Mutare and people living in Botswana and S. Africa). This captures the fact that within Zimbabwe wages vary spatially.

In addition to these regressors, it has been argued that altruistically motivated migrants may send greater remittances to households containing dependants such as young children or the elderly (Hoddinott, 1994). To control for this, we include the number of children (by sex), and adults (by age and sex) in our regressions.

The estimated model is that described previously:

$$PT_{it} = b_1 t_{it} + b_2 yr_{it} + gX_{it} + (h_i + v_{it}) \qquad \text{(4.iii.5)}$$

where: PT is private transfers received by the household;
t public transfers;
yr other sources of household income;
$\mathbf{X_{it}}$ is a vector of other migrant and household characteristics (as described above), including location, job type and years of migration of migrants and the number of children and adults (by sex) in the recipient household; and
$b_1, b_2 and g$ are parameters to be estimated.

Our concern in this paper is the extent to which public transfers crowd out net private transfers. To save space only the results on the income variables are reported. Full results are available on request.

Table 11: Pooled and fixed effects estimates of the determinants of net private transfers

	public transfers		**other income**	
	Coefficient	t.statistic	Coefficient	t.statistic
1992/93	0.005	0.477	-0.0001	-0.121
1993/94	-0.010	-0.624	0.002	0.869
1994/95	0.033	0.663	0.022	1.067
1995/96	0.0002	0.004	0.001	0.225
pooled				
without time dummies	-0.006	-0.233	0.002	1.228
F-test : 2.57***				
with time dummies	0.017	0.641	0.001	0.728
F-test : 3.70***				
fixed effects[11]				
without time dummies	-0.008	-0.390	-0.002	-0.544
F-test : 1.314***				
with time dummies	0.041	1.490	-0.004	-1.421
F-test : 1.323***				

The striking result from these regressions is that there appears to be no crowding out of private transfers with the provision of aid. This is not surprising given the very small amounts of private transfers received in comparison to public transfers. It is therefore safe to assume no crowding out in the simulation model to follow.

(iv) Investment

We now turn to a second potentially important feedback effect, namely the link between increased incomes and investment. This can either be estimated or derived from the production function described in section 4.ii.. Due to difficulties in estimating an investment function the second approach is adopted here[12].

Following Branson (1979), the first step is to note that equilibrium capital stock is the level at which the marginal revenue of an addition to this stock equals its marginal cost. Denote R as revenue, P as price, C as cost. Marginal revenue is: $\partial R / \partial K = \partial(Py)/ \partial K = P\, \partial y / \partial K$. Marginal cost is $\partial C / \partial K = C$ and so under this equilibrium condition, $C = P\, \partial y / \partial K$. Next, recall that in section 4.ii, that the relationship between y and K is estimated using a Cobb-Douglas production function (equation 4.ii.3). Partially differentiating this with respect to K yields $\partial y / \partial K = \alpha \cdot (y / K)$ where α is the coefficient on the capital stock. Note that $\partial y / \partial K$ appears in both the marginal cost and marginal revenue equations; a little manipulation yields:

$$\partial y / \partial K = \alpha\, y / K = C / P \qquad (4.iv.6)$$

As this describes an equilibrium condition, the equilibrium capital stock can be written as:

$$K^E = \alpha \cdot y\, (P / C) \qquad (4.iv.7)$$

Investment is the change in capital stock from one period to the next. Assume that

[11] Hausman test: chi-squared (17)=39.18***.

[12] To estimate an investment function we need to consider the change in income and change in capital from one year to the next. This immediately drops the panel element to three years. Also those households that had zero income during the drought year would fall out of the estimation. Both these factors bring the sample size below that needed to estimate efficient and unbiased parameters.

farmers adjust from actual to equilibrium capital stock in one period (this is relaxed below). Accordingly, investment will equal change in the equilibrium capital stock, $I = \Delta K^E$. Therefore:

$$\Delta K^E = \Delta (\alpha y) (P/C) \quad (4.iv.8)$$

Recall that the coefficient on capital stock, α is stable over the period estimated here and so equation (4.iv.8) is re-written as:

$$\Delta K^E = \alpha (\Delta y) (\Delta P/ \Delta C) \quad (4.iv.9)$$

It is assumed that $(\Delta P/ \Delta C) = 1$. The Government of Zimbabwe's Producer Price Index (Government of Zimbabwe, 1998) shows that over the period 1988- 1996, producer prices for foodstuffs (a proxy for P) have risen at an annual rate of about 27 per cent, whereas producer prices for metal products - the closest proxy available for agricultural capital goods, have risen by about 24 per cent per year. Assuming $(\Delta P/ \Delta C) = 1$ is therefore, if anything, a conservative assumption. Accordingly, $\Delta K^E = \alpha (\Delta y)$. Table 10 indicates that α takes on a value of 0.325, and so the relationship between changes in income and changes in the capital stock is given by:

$$\Delta K^E = 0.325 \ (\Delta y)$$

Finally, note that this approach assumes that there are no adjustment lags in moving from current to equilibrium capital stock. Although this is clearly an unrealistic assumption, it can be easily relaxed by assuming an adjustment process (for example, assuming that half the adjustment takes place in the first year etc.) This is incorporated into the simulation models presented in the next chapter.

5. Counterfactuals: Quantifying the trade-offs between relief and development assistance

The next step is to use these estimated relationships to simulate the effects of alternative relief and development interventions on incomes and the incidence and severity of poverty. The purpose of the counterfactuals is to present a 'what if' scenario to give a sense of what the opportunity costs are of switching from development aid to relief aid, rather than a prescription for action at the household level. The interventions we choose in these counterfactuals are driven by the results found in section 4.ii on the determinants of crop income. We begin with a general overview of our counterfactual analysis, followed by a description of the counterfactuals we run. We then turn to a discussion of the results.

(i) General outline of the counterfactual

The intuition behind our counterfactuals is the following. Recall that we observe households over the period 1992/93 to 1995/96. There is one drought year in 1994/95 and three non-drought years. It should be noted that in 1991/92, immediately prior to the period we consider, there was the extraordinary drought of the century that gripped all of southern Africa and for which Zimbabwe received much external assistance. The drought in 1995/96 received less international publicity and was addressed primarily by domestic resources. The principal response of the Government of Zimbabwe was a targeted grain loans scheme (there was also a small supplementary feeding program.) Rather than provide households with gifts of maize, the intention was that these allocations would be in-kind loans, repayable in kind following the next (1995/96) harvest. In practice, however, the scheme seems to have been largely untargeted. In our survey areas, 87 percent of households received these loans. The value of grain allocated was remarkably similar across households regardless of their income

levels. Further, repayment of these loans has been, at best, sporadic. Our survey data suggest that a generous estimate would be that 25 per cent of recipients had repaid their loans in full.

Table 12: Value of per capita grain loan received by per capita income quintiles

Per capita income quintiles	Value of per capita grain loan received (standard deviation)
1 (poorest)	120 (83)
2	111 (77)
3	125 (95)
4	103 (99)
5 (richest)	106 (107)

In light of this evidence, we consider the grain loans scheme to have been largely an untargeted transfer scheme. Our counterfactual analysis is based on the following. Suppose that instead of waiting for the next drought to occur, either external donors or the Government of Zimbabwe had, in addition to the drought relief supplied in 1991/92, provided some or all of our households with additional capital stock at the beginning of the 1992/93 agricultural season, and/or agricultural extension advice throughout the period. The funding for this is obtained by reallocating the resources used for the 1994/95 grain loans scheme[13].

Five features of such a counterfactual should be noted.

First, our counterfactual analysis is not based on increasing external transfers, but rather reallocating existing transfers across time and across households.

Second, the allocation of capital occurs at the beginning of the 1992/93 agricultural season, after the 1991/92 drought year, and the extension advice in each year where there was no visit. One concern may be that households will consume rather than retain the allocation of capital. We know that the relief aid given in 1994/95 is consumed. This comment is based on data we have that indicates that despite the drought, households were dissaving in the aftermath of the drought (Kinsey, Burger and Gunning, 1997). The concern therefore revolves around the notion of whether these households would consume a 'windfall' of capital. Bevan, Collier and Gunning (1989) on the coffee boom in Kenya argue that African households generally save a large proportion of such windfalls (82 percent). We have evidence that when these households received such 'windfall' assistance in the form of free fertiliser and seeds at the beginning of the crop year 1992/93, this was actually used for production rather than consumed. We therefore assume no consumption of the initial allocation of capital stock. Any relaxation of this assumption effectively lowers the reduction in poverty. Hence we acknowledge that the counterfactual estimates may reflect an upper bound of the extent of poverty reduction.

Third, our 'development' allocation is the provision of additional capital stock and extension advice. This follows from the finding, reported in section 4.ii, that capital stock

[13] In this simulation model, we transfer the funds from 1994/95 to 1992/93, without discounting. We do so for two reasons. First, if we discounted using the real Zimbabwe rate of interest - for example, the rate on the 91 day Government treasury bills, or the rediscount rate less inflation - we get a slightly negative rate. So the discounting would yield a figure slightly higher than the value of the relief aid. Another way round this would be to use the long term interest rate on gilts or US treasury bonds, which historically have been about 3 percent. Discounting using such a figure would reduce the value of the development assistance by only a small amount (just over 12 percent) and so it is not clear that such a discounting would have a significant impact on the results.

and a visit by an extension worker both have a large, positive effect on increasing household crop income. From section 4.ii we know that the provision of one extension visit per year dramatically increases income, but that subsequent visits have little or no impact. We therefore base the counterfactuals on providing one visit per year to households that had not previously received a visit. The number of households that did not receive a visit in: 1992/93 was 59, 144 in 1993/94; 171 in 1994/95; and 141 in 1995/96.

Fourth, the cost of an extension visit is calculated from data supplied by the Ministry of Agriculture in Zimbabwe. Taking into account salary and mileage costs an estimate of $10 per visit was calculated. To be conservative we doubled this figure[14].

Fifth, we do not re-allocate any of the funds associated with the supplementary feeding programs that were put in place in 1994/95.

(ii) Mechanics of the counterfactual

To fix precisely how these counterfactuals operate, recall that in section 4.ii, we estimated the following relationship: ln(net crop income) = 0.324 * ln(capital) + 0.288 * extension1 + other regressors. With respect to capital, in this log-log specification, the elasticity of crop income with respect to capital is the coefficient on log capital. That is, 0.324 = (dy/dk) / (y/k) which implies dy = 0.324 * (y/k) * dk. For a given increase in capital stock, income rises by the product of that change multiplied by the crop income-capital ratio multiplied by 0.324. In the semilog relationship between extension and income the interpretation is slightly different. Because the coefficient on the extension dummy measures the discontinuous effect on income, the appropriate interpretation is $g = \exp(b_6)-1$ (Halvorsen and Palmquist, 1979). In our case, g = exp (0.254) - 1, g = 0.289. Based on this interpretation gaining access to extension will increase farm income by 29 percent. So the change in income is calculated as dy = y * 0.29.

Next consider possible second round effects. The first is the possibility that this increase in private income will lead to a reduction in private transfers. But from the results presented in section 4.iii, we know that virtually no such crowding out will occur. As explained above, we assume that income from all other sources - such as wage labour, handicrafts and beer brewing - is exogenous. Accordingly, the remaining second round effect to consider is the relationship between capital stock accumulation and changes in income. As outlined in section 4.iv, this is given by I = 0.324 * dy + other regressors. An increase in income of $1 will increase capital stock by $0.324. As noted in section 4.iv., however, households are unlikely to immediately invest in capital in response to an increase in income. If we assume a lag of 0.5, this reduces the coefficient to 0.162. To be cautious we use this reduced coefficient in the simulations[15].

Now consider the case of the following hypothetical household. Initial income in 1992/93 is $2000, comprising $1500 of net crop income, $100 of public transfers, $200 of

[14] The Ministry of Agriculture supplied data on extension workers salaries, mileage allowances, per diems, and housing costs by scheme for: 1984 when the schemes were first resettled, 1992 and 1996. From Kinsey (1982) we have information on the number of villages and households each extension worker was responsible for covering. From the survey we know the number of visits the extension workers made to the households in the sample in each year. Assuming housing costs are fixed we calculated the salary and mileage allowance costs per extension worker. We then calculated the number of visits made and from this derived a per visit cost. The cost per visit in Mutanda and Mupfurudzi was $10, and Sengezi $8. To be conservative we took the highest figure and doubled it.

[15] It should be noted that running the simulations with 0.324 or 0.162 makes little or no difference to the poverty levels.

private transfers and $200 from all other (exogenous) sources. The household has an initial capital stock of $3000. If the household receives a capital stock transfer at the beginning of 1992/93 of $250, it will generate an additional $40 (= $250 * (1500/3000) * 0.324) in crop income. Recall that none of this additional crop income is crowded out by a reduction in private transfers, and other income sources are exogenous. So the increase in total household income in 1992/93 is $40. Next, recall the relationship between changes in income and changes in capital stock. Higher incomes in 1992/93 increase capital stock by $7 (= $40 * 0.162) and so the household begins the 1993/94 season with an additional $257 (the initial allocation of $250 *plus* the $7 of additional investment). Consequently, in 1993/94 crop incomes rise as a result of both the initial transfer of capital stock and also the additional investment made by the household.

We now carry out the same exercise for extension. If a household receives a visit by an extension worker at the beginning of 1992/93 this will generate additional income of $435 (= 1500 * 0.29). Higher income in 1992/93 will increase capital stock by $70 (= 435 * 0.162). Consequently, in 1993/94 crop income rises as a result of both the additional income from the extension visit and also the additional investment made by the household.

(iii) Counterfactuals

Now we consider a number of alternative relief and development interventions and their impact on incomes and incidence and severity of poverty. The counterfactuals are going to appear along two dimensions, first variations on development interventions, and second alternative targeting strategies. Development interventions can be divided into two cases: where the grain loan is redistributed in the form of capital equipment, and where the loan is used to fund extension visits and some capital equipment.

We chose to target the aid in three ways. The first is called ‘like for like’. In this counterfactual each household is allocated capital stock at the beginning of 1992/93 agricultural year exactly the value of assistance received in 1994/95. From section 5.i. we know that the grain loan was largely untargeted so this counterfactual reflects the most likely transfer of development aid. This counterfactual is the least demanding in terms of information needed.

The second targeting mechanism is geographic: it re-allocates aid given to wealthy villages to poor villages. This is seen as a relatively straight forward way of identifying the poor and requires only limited information. We rank the twenty-two villages on the basis of average per capita incomes over the four year period. Drought relief given in 1994/95 to the five best off villages over the four year period is reallocated in 1992/93 to the poorest five villages according to this ranking. However, those taking decisions on the allocation of aid may not have more than a ‘snapshot’ view of which villages are poor or rich. Using the ‘snapshot’ variant therefore, we redistribute drought relief given to the five wealthiest villages in 1994/95 to the five poorest villages identified by their per capita income in 1992/93.

A final method of targeting is to take the grain loan allocated to the rich and give it to the poor, hence the name ‘robin hood’. In this case assistance is only provided to those households whose average per capita incomes over the four year period are in the bottom third of the distribution. Relief that previously went to households in the top third of this distribution is reallocated at the beginning of the 1992/93 agricultural year. In this case the grain loan allocated to the bottom two-thirds of the distribution is left untouched. A variant is again the ‘snapshot’ method whereby the poorest third in 1992/93 are given the grain loan which had been allocated to the top third in 1994/95, both identified by their per capita

incomes in 1992/93 and 1994/95 respectively[16].

Table 13 shows how much aid is reallocated per recipient household for each counterfactual. The allocation of aid for the capital counterfactuals is straight forward. The value of the total grain loan to be redistributed is divided amongst the intended beneficiaries. The average total cost for extension services over all four years is $35. The final column gives an average of aid received by any beneficiary household who receives either capital or extension, or a combination of the two.

Table 13: Average allocation of aid per recipient household

Counterfactual	**Capital**		**Extension**		**Capital &/or Exten.**	
	No. of recipients	Value of aid	No. of recipients	Value of aid	No. of recipients	Value of aid
Like for Like	320	$995				
Geographic (1)	55	$1,870				
Geographic (2)	47	$1,917				
Robin Hood (1)	123	$701				
Robin Hood (2)	123	$806				
Extension			287	$35		
Extension & Like for Like	320	$963	287	$35	362	$851
Extension & Geographic	55	$1,683	287	$35	307	$301
Extension & Robin Hood	123	$617	287	$35	313	$243

Tables 15, 16 and 17 provide the results of running, first, the capital counterfactuals based on the first three targeting criteria, and second, the extension advice and capital counterfactuals based on a sub-set of the first three targeting criteria. We begin with a detailed discussion of the 'like for like' capital counterfactual and a comparative analysis of the impact of alternative targeting strategies. We then discuss these alternative targeting strategies in relation to the extension and capital development interventions.

The 'like for like' counterfactual is the simplest of the five capital counterfactuals we consider. All that is done is change the timing of the intervention without changing either the identity of the recipients or the amount they receive. Doing so at the beginning of 1992/93 raises recipient incomes in that year by an average of 6 percent per person. P0 consumption poverty falls by 5 per cent from 0.41 to 0.39. P0 food poverty falls by about 8 percent, from 0.26 to 0.24. The percentage reductions in P2 poverty are even larger, with the severity of consumption poverty falling by 11 per cent and food poverty by nearly 16 per cent. Poverty falls further in 1993/94, but appears to rise sharply in the drought year of 1994/95 as these households no longer receive the in-kind income in the form of maize grain. Poverty then falls relative to actual outcomes in the post-drought year of 1995/96.

These results suggest that the direct substitution of relief with development assistance embodies a trade-off between reducing poverty in non-drought years and increasing poverty in drought years. The increased poverty in 1994/95 is quite substantial. P0 food poverty rises by nearly 47 per cent and nearly three quarters of all households fall below the consumption poverty line. However, this overlooks an important fact, namely that the capital stock available to these households is higher because they have not consumed the original aid allocation, and because they have used some of the additional incomes generated to

[16] We experimented with different allocation rules, such as taking from the top 50 percent of the distribution and giving to the bottom 20 percent. The poverty levels remained basically unchanged.

accumulate further capital. Suppose we make the reasonable assumption that in 1994/95, households who received assistance at the beginning of the simulation dissave this accumulated capital to the point where their counterfactual incomes in 1994/95 equal their actual incomes. Under this variant of our counterfactual, there is only a 1 percent increase in poverty in this drought year. Further, food poverty still falls in the subsequent year because the capital stock accumulated as a consequence of the earlier transfer of aid is still available to the household.

Our next step is to compare our 'like for like' counterfactual with the results from the other targeted counterfactuals. In the first two years of these simulations, the 'like for like' counterfactual dominates the others in terms of poverty reduction. In the third (drought) year, 1994/95, with dissaving, the 'like for like' simulation performs no better than the actual outcome and all other measures register an increase in poverty. These results emerge despite the fact that under the targeted interventions, more capital per person is provided to recipient households in the geographic allocation, and two-thirds of the grain loan is untouched in the 'robin hood' allocation. Table 15 shows that the geographic targeting increases cumulative per capita income for beneficiaries by greater amounts than the 'like for like' counterfactual (by $341), hence the P2 measure improves with these interventions. However, the number of beneficiaries is very small, 55 in the case of geographic targeting. If we consider the whole sample the additional income is significantly less ($51).

The results of the 'robin hood' targeting lie between the other two allocations. This is because the 'robin hood' allocation targets 123 beneficiaries, more than double targeted under the geographic allocation, but provides them with less capital. In the first two years the results mirror the geographic targeting. However, during the drought the results worsen, particularly the 'robin hood' targeting based on the 'snapshot' approach where more of the grain loan is reallocated. Because more capital is provided to more than twice the number of beneficiaries under the geographic targeting, and still two-thirds of the sample continue to receive their grain loan, the P2 measure outperforms any other counterfactual. This reduction in the severity of poverty, however, seems to come at the cost of penalising the top third of households.

Thus the explanation for the difference in the counterfactual results can be split into four parts: (1) both the 'robin hood' and geographically targeted interventions reach a relatively small number of households. Not all targeted households are lifted out of poverty and poor households who are not recipients of assistance will obviously not be lifted out of poverty either. Further, the effect of some of the assistance is wasted in that it provides more income than is necessary for some households to be lifted out of poverty; (2) in the drought year, recipient household incomes are higher as a result of their higher capital stocks. However, this is achieved at the expense of some households who lose their aid, with the result that the reduction in poverty amongst recipient households is slightly more than offset by the increase in poverty amongst some of the non-recipients; (3) our dissavings assumptions apply only to those households who receive aid. It turns out that for these households under the 'robin hood' and geographic targeting, only a few tend to need to dissave in order to restore their incomes to the level we actually observe.; and (4) as a consequence of (3), capital stock levels tend to be higher under the geographic counterfactuals at the end of 1994/95, with the result *ceteris paribus* that incomes are higher in the post-drought year that follows. The obvious conclusion that follows from this is that the current counterfactuals either do a poor job in targeting poor households (as in the case of geographic (2)) or they either penalise too many households at the top of the distribution

and/or benefit too few households at the bottom, as in the 'robin hood' counterfactuals.

Next we consider the second set of interventions which focus on the provision of extension advice and, in three cases, capital equipment. The first counterfactual considers the provision of extension advice for households who had previously not received a visit in 1992/93. The visits are funded by reallocating a small portion of the grain loan, the remaining grain loan is distributed as before. The initial impact in 1992/93 is not large which is not surprising given only 59 households benefited from the allocation. The effect, however, in 1993/94 when a further 144 households receive a visit, is greater. The P0 measure falls by 10 percent from 0.52 to 0.46. No-one is made worse off in the drought of 1994/95, and poverty continues to fall the following year by 8 percent. In itself this seems a significant drop in poverty at very little expense. Poverty falls in non-drought years and no-one is made worse off in the drought year by simply reallocating 3 percent of the relief aid given during the drought.

However, the impact is even greater if we combine extension advice with the allocation of capital equipment. Starting with the best capital counterfactual 'like for like' and combining this with extension advice we see even further falls in poverty - by 7 percent in 1992/93 and 15 percent in 1993/94. Again, no-one is made worse off during the drought and in fact P0 food poverty falls by 7 percent. Poverty then falls a further 8 percent in 1995/96. The P2 measure in 1995/96 without dissaving falls by 22 percent, and with dissaving still falls by 15 percent.

Thus, we have the striking result that, under this counterfactual, no one is ever made worse off in the drought year and welfare is improved in non-drought years. On average, each person living in a beneficiary household has an additional $Z198 (1992 $Zim) available to her, an amount that would fund an adequate diet (defined by the expenditures necessary to reach the food poverty line) for about six months. A further measure of this improvement is the number of people lifted out of food poverty. We calculate this by multiplying the difference between the actual and counterfactual food poverty P0 by the number of households in the sample (370) by mean size of food poor households. This yields the following: 130 in 1992/93; 135 in 1993/94; 99 in 1994/95; and 104 in 1995/96.

As in the capital counterfactuals, 'robin hood' and geographic targeting do not appear to be as effective as the 'like for like' counterfactual. Although the geographic and 'robin hood' counterfactuals see a large boost in income it is not translated into poverty reduction because there are far fewer beneficiaries. In terms of poverty reduction the 'like for like' counterfactual dominates all others for the reasons explained when comparing the capital counterfactuals previously.

To conclude, table 14, provides a summary of the average poverty rates of the best, worst and median counterfactuals.

Table 14:Average poverty rates for P0 and P2 of best, worst and median counterfactuals

Average poverty rate for 1992/93-1995/96 (with dissaving)				
	P0	% change	P2	% change
Actual Outcome	*0.503*		*0.128*	
Best: 6. Extension & Like for Like	0.468	7	0.111	13
Median: 7. Extension and Geographic	0.475	6	0.124	3
Worst: 5. Robin Hood (2)	0.520	-3	0.125	2

Table 15: Comparison of actual and counterfactual mean per capita incomes and household capital stock

	% change in mean per capita income under counterfactual, beneficiaries *% change in mean per capita income under counterfactual, all households*				Cumulative per capita income increase	Number of recipient households
	1992/93	1993/94	1994/95	1995/96		
1. Like for Like	8 *7*	11 *10*	-32 *-28*	10 *9*	213 *184*	320
2. Geographic (1)	21 *3*	26 *4*	10 *-7*	28 *4*	341 *51*	55
3. Geographic (2)	14 *2*	24 *3*	6 *-7*	31 *4*	360 *46*	47
4. Robin Hood (1)	7 *2*	9 *3*	2 *-6*	9 *3*	86 *24*	123
5. Robin Hood (2)	8 *3*	10 *3*	1 *-5*	10 *3*	111 *33*	123
6. Extension	4 *3*	10 *8*	3 *2*	11 *8*	20 *16*	287
7. Extension & Like for Like	10 *10*	18 *18*	4 *-4*	16 *15*	198 *193*	362
8. Extension & Geographic	7 *6*	13 *11*	2 *-5*	15 *12*	71 *58*	307
9. Extension & Robin Hood	7 *6*	12 *10*	-1 *-3*	12 *10*	45 *37*	313

If households had previously not received an extension visit and were provided with advice and capital stock distributed according to the same distribution as the grain loan, average poverty would have fallen by 7 percent. If the worst case counterfactual had been adopted poverty would have increased by 3 percent, however, the severity of poverty would have fallen by 2 percent. Finally, if we took the median case, poverty as measured by P0, would have fallen by 6 percent.

Table 16: Comparison of actual and counterfactual consumption and food poverty (P0) estimates

	P0 consumption poverty *P0 food poverty*							
	1992/93	1993/94		No dissaving of aid supplied capital stock			Dissaving of aid supplied capital stock	
				1994/95	1995/96		1994/95	1995/96
Actual Outcome	0.41 *0.26*	0.52 *0.35*		0.70 *0.43*	0.38 *0.21*		0.70 *0.43*	0.38 *0.21*
Capital Counterfactuals								
1. Like for Like	0.39 *0.24*	0.48 *0.33*		0.78 *0.63*	0.36 *0.19*		0.71 *0.48*	0.38 *0.19*
2. Geographic (1)	0.41 *0.25*	0.51 *0.34*		0.73 *0.48*	0.36 *0.19*		0.73 *0.48*	0.36 *0.19*
3. Geographic (2)	0.41 *0.25*	0.51 *0.33*		0.72 *0.49*	0.36 *0.19*		0.72 *0.49*	0.36 *0.19*
4. Robin Hood (1)	0.41 *0.24*	0.51 *0.34*		0.74 *0.47*	0.37 *0.19*		0.74 *0.47*	0.37 *0.19*
5. Robin Hood (2)	0.41 *0.23*	0.51 *0.34*		0.79 *0.45*	0.37 *0.20*		0.79 *0.45*	0.37 *0.20*
Extension and Capital Counterfactuals								
6. Extension	0.40 *0.24*	0.47 *0.33*		0.70 *0.42*	0.35 *0.19*		0.70 *0.42*	0.35 *0.19*
7. Extension and Like for Like	0.38 *0.22*	0.44 *0.31*		0.69 *0.40*	0.33 *0.17*		0.69 *0.40*	0.36 *0.18*
8. Extension and Geographic	0.39 *0.23*	0.46 *0.31*		0.72 *0.46*	0.33 *0.17*		0.72 *0.46*	0.33 *0.18*
9. Extension and Robin Hood	0.39 *0.22*	0.47 *0.32*		0.74 *0.45*	0.35 *0.18*		0.74 *0.45*	0.35 *0.19*

Table 17: Comparison of actual and counterfactual consumption and food poverty (P2) estimates

	P2 consumption poverty *P2 food poverty*						
	1992/93	1993/94		No dissaving of aid supplied capital stock		Dissaving of aid supplied capital stock	
				1994/95	1995/96	1994/95	1995/96
Actual Outcome	0.105 *0.051*	0.153 *0.078*		0.165 *0.069*	0.089 *0.042*	0.165 *0.069*	0.089 *0.042*
Capital Counterfactuals							
1. Like for Like	0.093 *0.043*	0.133 *0.064*		0.306 *0.190*	0.077 *0.034*	0.223 *0.129*	0.083 *0.038*
2. Geographic (1)	0.098 *0.046*	0.143 *0.070*		0.202 *0.101*	0.081 *0.036*	0.202 *0.101*	0.081 *0.036*
3. Geographic (2)	0.101 *0.047*	0.145 *0.072*		0.200 *0.100*	0.084 *0.039*	0.200 *0.100*	0.084 *0.039*
4. Robin Hood (1)	0.098 *0.046*	0.142 *0.069*		0.186 *0.083*	0.080 *0.036*	0.186 *0.083*	0.081 *0.036*
3. Robin Hood (2)	0.095 *0.044*	0.143 *0.070*		0.174 *0.071*	0.081 *0.036*	0.170 *0.068*	0.082 *0.037*
Extension and Capital Counterfactuals							
6. Extension	0.101 *0.048*	0.141 *0.070*		0.163 *0.069*	0.080 *0.037*	0.163 *0.069*	0.079 *0.037*
7. Extension and Like for Like	0.089 *0.041*	0.124 *0.058*		0.158 *0.067*	0.069 *0.031*	0.155 *0.065*	0.076 *0.035*
8. Extension and Geographic	0.094 *0.044*	0.132 *0.064*		0.195 *0.098*	0.073 *0.033*	0.195 *0.098*	0.075 *0.034*
9. Extension and Robin Hood	0.094 *0.044*	0.132 *0.063*		0.179 *0.081*	0.073 *0.033*	0.179 *0.081*	0.079 *0.036*

Two conclusions emerge. First, the provision of both extension advice and capital equipment have the greatest impact on household incomes and the severity and incidence of poverty in these resettlement schemes. Second, the most effective form of targeting appears to be not to target at all. The provision of untargeted aid reaches a larger population and hence has a greater impact on reducing the incidence of poverty. Targeted interventions reach a small number of people who are lifted further out of poverty than is needed, hence having a greater impact on the severity of poverty for these households. With limited resources the more cost-effective policy would be to improve conditions for the larger population by reducing the overall incidence of poverty.

(iv) Qualitative method of targeting

A final targeting method is to use the results from the wealth ranking exercise. In this set of counterfactuals we run the actual poverty levels for the smaller sample (274) (recall only 17 of the 22 villages were included in the exercise).

Whereas the first three sets of targeting tools are applied to both the provision of capital and extension advice, because the wealth ranking is conducted on a 'wealth' criteria, the aid in these counterfactuals is given only in the form of capital equipment.

Table 18 presents results on four counterfactuals. The actual outcome for the reduced sample is presented first. The results are in line with those presented for the larger sample, although the P0 measure tends to be about 2 percentage points higher than the measure based on the per capita income. For comparison the first 2 counterfactuals are exactly the same capital counterfactuals run in the previous section - 'like for like' and 'robin hood' based on per capita income. Again, the results are in line with those presented for the larger sample, the 'like for like' counterfactual performs best. The P0 measure falls by 7 percent. With dissaving only 1 percent are made worse off during the drought year. In the following year, 1995/96, poverty declines again.

The third counterfactual takes all the grain loan allocated in 1994/95 and redistributes it evenly amongst the bottom third of households identified as poor from the ranking conducted by the villagers. This reduces the P0 measure by 9 percent in both 1992/93 and in 1994/95, but dramatically increases poverty in the drought year by 14 percent. Even with dissaving the level of poverty remains high. The reason for this large increase in poverty is because all the grain loan is taken from all 1995/96 recipients and re-distributed to only 96 households.

The final counterfactual uses the 'robin hood' method of taking from the rich and giving to the poor, as identified by the rankings of the villagers. The poverty levels are almost identical to those reported for the 'robin hood' counterfactual based on per capita income, although perhaps marginally outperforming them. Given the level of correlation between the quantitative and qualitative rankings this is not surprising.

Similar to the previous set of other counterfactuals the direct targeting interventions, in particular both the 'robin hood' counterfactuals, outperform the 'like for like' counterfactual on the P2 measure. However, overall the 'like for like' counterfactual sees a larger average fall in poverty as measured by the P0 measure than any other targeting mechanism.

Table 18: Comparison of actual and counterfactual consumption and food poverty (P0 and P2) estimates for PRA Sample

	P0 consumption poverty *P0 food poverty*						
	1992/93	1993/94		No dissaving of aid supplied capital stock		Dissaving of aid supplied capital stock	
				1994/95	1995/96	1994/95	1995/96
Actual outcome	0.45 *0.27*	0.54 *0.38*		0.72 *0.43*	0.40 *0.22*	0.72 *0.43*	0.40 *0.22*
Capital Counterfactuals							
10. Like for Like	0.42 *0.24*	0.50 *0.36*		0.81 *0.65*	0.38 *0.20*	0.73 *0.49*	0.39 *0.21*
11. Robin Hood	0.44 *0.24*	0.53 *0.36*		0.80 *0.65*	0.38 *0.21*	0.80 *0.49*	0.38 *0.22*
12. PRA (1)	0.40 *0.23*	0.49 *0.33*		0.81 *0.63*	0.37 *0.19*	0.80 *0.61*	0.37 *0.20*
13. PRA (2) R.H.	0.42 *0.24*	0.51 *0.35*		0.79 *0.54*	0.38 *0.20*	0.79 *0.54*	0.38 *0.20*

	P2 consumption poverty *P2 food poverty*						
	1992/93	1993/94		No dissaving of aid supplied capital stock		Dissaving of aid supplied capital stock	
				1994/95	1995/96	1994/95	1995/96
Actual Outcome	0.110 *0.052*	0.162 *0.083*		0.166 *0.067*	0.096 *0.044*	0.166 *0.067*	0.096 *0.044*
Capital Counterfactuals							
10. Like for Like	0.097 *0.045*	0.142 *0.069*		0.316 *0.197*	0.082 *0.034*	0.229 *0.132*	0.089 *0.040*
11. Robin Hood	0.096 *0.043*	0.143 *0.067*		0.216 *0.103*	0.078 *0.032*	0.216 *0.103*	0.078 *0.032*
12. PRA (1)	0.095 *0.046*	0.135 *0.065*		0.312 *0.194*	0.079 *0.034*	0.288 *0.172*	0.081 *0.035*
13. PRA (2) R.H.	0.098 *0.047*	0.142 *0.069*		0.233 *0.121*	0.082 *0.035*	0.233 *0.121*	0.082 *0.035*

6. Conclusions

This study has examined the consequences of alternative relief and development interventions on the well being of households in rural Zimbabwe. It does so by: a) establishing a framework that links household resources to levels of poverty; b) validating the quantitative data with group wealth rankings by the households in the study; c) estimating key parameters within this framework, namely: the determinants of net crop income; the determinants of private transfers; and the links between increased incomes and the accumulation of capital stock; and d) conducting a counterfactual exercise in which relief assistance is reduced and reallocating these funds to improve access to agricultural extension and increased holdings of capital stock. Under these counterfactuals, the incidence and severity of poverty in non-drought years fall significantly. The best performing counterfactual, improving access to extension and increasing capital stock (#7), reduces the incidence of food poverty by 11 per cent. Under the most basic scenario, the increased income generated by transforming relief aid into agricultural capital stock (the 'like for like' counterfactual) is sufficient to fund an adequate diet for each person in each beneficiary household for six months. Further, such improvements in well being are achieved without households necessarily being made worse off during a drought year. These results suggest that for the households in this sample, there is a significant opportunity cost associated with the shift in external aid resources from development to emergency assistance.

There are a number of caveats, however, that should be attached to these findings. First, these counterfactual results are only as robust as the assumptions that underlie them. These include estimates of key parameters of the simulation model - the impact of agricultural extension and capital stock on net crop income; the absence of crowding out of private transfers by public transfers or other household income; the relationship between changes in crop income and additions to the capital stock - and the assumption that a 'development' transfer of capital is saved, not consumed. For the reasons discussed in the substantive sections of the study, a reasonable degree of confidence can be ascribed to these estimates. Even if one were to halve their impact, these counterfactuals would still produce a modest improvement in household incomes (for example, a five per cent reduction in the incidence of food poverty).

Second, these results pertain to only one welfare measure, income. It could be argued that the improvements observed here are offset by a deterioration in some other measure of well being. Two companion papers (Hoddinott and Kinsey, 1998a; 1998b) find no effect of the grain loans scheme on either child (measured in terms of growth velocity of height) or adult health (measured in terms of BMI), suggesting that along these dimensions, no offsetting effect will be found. However, expressing well being in utility rather than income/poverty terms might cause a modification of these findings. Specifically, some households might attach a value to the presence of public insurance as a means of dampening income volatility, and this is not accounted for here.

The final caveat is the most important one. The sample used here is a representative sample of households resettled in three areas of Zimbabwe. These farmers are not typical Zimbabwean farmers, let alone typical African farmers. In particular, these households have, on average, been successful in accumulating assets, both as a mechanism for accumulation and as a means of buffering income shocks. For this reason, it would be incorrect to use these results to argue that donors should reduce emergency spending and redirect these funds to development assistance. Current recipients of emergency aid elsewhere in Africa may not have the same ability to withstand shocks; proposed recipients of

development assistance may not be as skilled as are these farmers in using human and physical capital to generate income.

The correct policy conclusions are twofold: a) when emergency and disaster relief requirements increase, their funding requirements should be met by supplementing the aid budget, not by reducing development assistance. The current policy of funding emergency relief by reducing development budgets is likely to have an opportunity cost in terms of foregone poverty reduction; and b) emergency relief should provide more than merely the means to keep people alive. Its objective should be enhancing livelihoods, as well as protecting lives, through the provision of physical and human capital. By doing so, it can reduce the need for emergency assistance in the future.

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