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Proxy Means Tests for Targeting Social Programs

Simulations and Speculation

Margaret E. Grosh
Judy L. Baker

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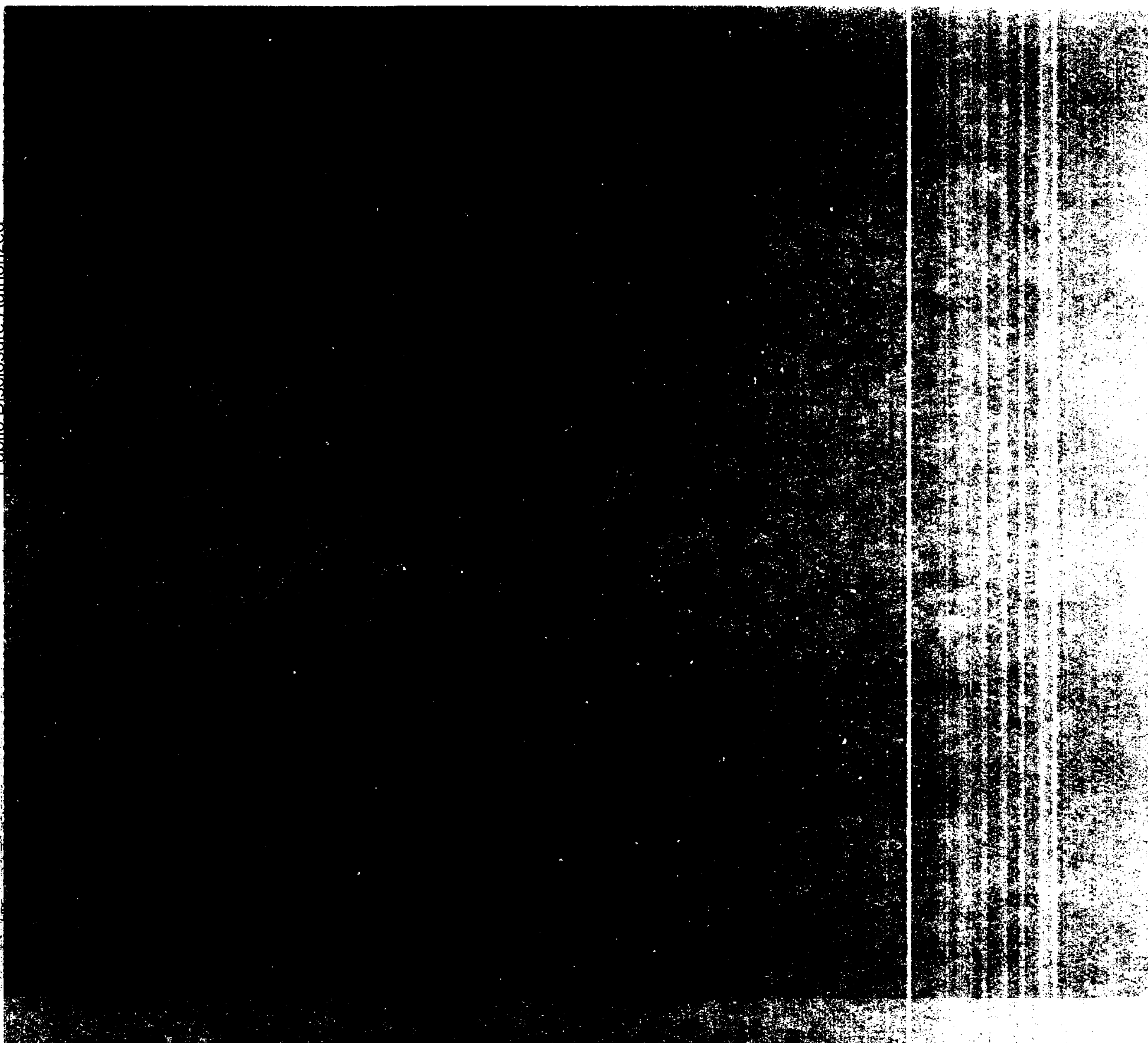

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The World Bank
Washington, D.C.

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FOREWORD

In an attempt to reduce the impact of economic adjustment on the poor, many governments in Latin America and the Caribbean have introduced or reformed targeted social programs. These programs aim to transfer benefits to the poorest groups in the most efficient way possible. Among the targeting options gaining more attention are variants on means testing. This paper explores one of them — the proxy means test.

The paper is part of a broader program of research in the Policy Research Department on the extent of poverty in developing countries and on policies to reduce poverty. Aside from the policy implications for conducting proxy means tests, the work also demonstrates the need for and usefulness of household data collection efforts such as the Living Standards Measurement Study (LSMS) surveys in developing countries. The paper is unusual in two respects: it combines extensive manipulation of household data sets with qualitative case study material on program implementation, and it uses data from three different LSMS surveys.

Lyn Squire

Lyn Squire, Director
Policy Research Department

ABSTRACT

This paper examines how a proxy means test might work in targeting social programs. The term "proxy means test" is used to describe a situation where information on household or individual characteristics correlated with welfare levels is used in a formal algorithm to proxy household income, welfare or need. Given the administrative difficulties associated with sophisticated means tests and the inaccuracy of simple means tests, the idea of using other household characteristics as proxies for income is appealing.

Chapter II carries out simulations on data sets from Jamaica, Bolivia and Peru to explore what kind of information can best be used in a proxy means test and how accurate such tests might be expected to be. The results show that household characteristics can serve as reasonable proxies for information on income in assessing eligibility for social programs. More information is generally better than less, though there are diminishing returns. The proxy systems all have significant errors of undercoverage, but they cut down leakage so much that the impact on poverty is better with imperfect targeting than with none. Some fine-tuning of the basic system, such as calibrating for the poorest half of the population, improves results considerably. In Jamaica, calibrating separately for rural and urban areas did not improve results. An assumed 25 percent level of distortion of information had no effect at all on targeting outcomes.

Chapter III describes the practical experience with Chile's Ficha CAS system, one of the oldest and best-known proxy means tests in the developing world. Chapter IV discusses in qualitative terms some of the strategic choices and implications in setting up proxy means tests of different sorts. The amount of staff time, the amount of training required for staff at different levels, the number of computers, and the transport and communications links required will vary greatly depending on the decisions regarding how the system should be set up.

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I. The Need for a Proxy Means Test

Targeting benefits to the poor, however simple in concept, is an inexact art in practice.[1] Rigorous targeting requires a precise definition of the target group, which in turn may require a political consensus that is hard to solidify. Once the target group is established, a means must be found of identifying individuals or households that are in that group and of excluding others. This can be quite difficult technically, as well as being costly. In practice, targeting reflects the tradeoffs between the advantages of focusing program benefits on those who need them the most and the political, technical and financial difficulties of doing so.

A comparative study of targeting in Latin America has found that, among all targeting mechanisms, proxy means tests produce the best incidence outcomes (Grosh 1994). Proxy means tests use household or individual characteristics to proxy a means test, thus avoiding the problems involved in relying on reported income. In this paper, we explore the use of proxy means tests empirically by carrying out a series of simulation experiments using household level data from Jamaica, Bolivia and Peru and by evaluating two programs that actually use this targeting mechanism.

In the remainder of Chapter I, we will discuss why proxy means tests are needed. In Chapter II, we will explain the simulation exercise in detail, and address the array of technical issues faced when carrying out such a targeting scheme through alternating the basic simulation. These include differences in the number of independent variables, definitions of the dependent variables, eligibility cut-off points, distortion effects, population sub-samples and cross-country comparisons. Chapter III describes the field experience from the two programs in Chile and Costa Rica that use proxy means tests. Chapter IV discusses the design issues of implementing such a program.

Means Tests

In Theory. In targeting a program to the poor, being able to make a precise judgment about the level of welfare or the means of the recipient makes it possible to categorize that individual accurately as being either eligible or ineligible for the program. In principle, conducting a means test is the best way to determine eligibility. In practice, however, means tests suffer from several problems. First, applicants have an incentive to understate their welfare level so as to qualify for program benefits. Verifying that welfare level is difficult to do, particularly in developing countries, as reliable records needed for verification usually do not exist. Although social security records, government payrolls, and income tax records are starting points, they may cover only a small share of the population, and almost by definition

1. Targeting is the process by which benefits are channelled to members of the high priority group that a program aims to serve. In nutrition programs, for example, the target group may be malnourished children. In medical programs, the target group may be those at particular bio-medical risk or with particular medical conditions. In this paper, we assume that the poor are the target group.

are more likely to cover the rich than the poor. They may also be incomplete, inaccurate, or hard to use.

Income is also not always considered an accurate measure of welfare. It does not usually include the imputed value of owner-occupied housing or durable goods or the value of home-grown crops. No adjustments are made for the seasonal income of agricultural workers or the uneven income of the sporadically employed, thus distorting actual welfare.

In the light of these difficulties, rigorous means tests are largely reserved for industrialized economies where a well-educated labor force is concentrated in jobs in which cash is paid regularly and payments are reported to tax or welfare authorities. Means testing is rarely used in poor countries. Where it is used, it is greatly simplified, thus reducing its accuracy from the perfect standard that is theoretically possible.

In Practice. Simple means tests are performed as part of the food stamp programs in Jamaica, Honduras, Sri Lanka and Zambia. In Jamaica, household income is reported by the head of the household. A social worker visits the household to check whether the visible living standard is grossly inconsistent with the reported income. However, this evaluation is largely subjective. No form containing any systematic examination or weighting of certain factors has to be filled out by the social worker (Grosh 1992). In Sri Lanka, income is taken as reported (Edirisinghe 1987). In Honduras, teachers are responsible for identifying students from households that have incomes below a threshold level. The teachers do visit the homes, but with no guidelines about what to look for or whether or how to evaluate non-salary income, although some try to do so anyway (Grosh 1994). In Zambia, formal sector workers register for food stamps through their employers, which ought to make it possible to verify their income, but in practice, some employers seem to underreport their employees' incomes or at least fail to report bonuses and some kinds of emoluments. The income of the spouses of the employees is requested as part of the food stamp application process, but this is not verified (Zambian Prices and Incomes Commission, undated).

Means tests are also performed in the course of waiving fees for hospital care in a number of countries. In the Dominican Republic, for example, each hospital does its means tests somewhat differently. Broadly, a social worker interviews the patient and asks questions about, for example, their income, employment status and family size. Most patients requesting a discount receive a 25 to 50 percent reduction in fees. The process is rather chaotic, and the increased waiting time, sometimes of several hours, and unpleasantness of the interview (which is sometimes conducted with a crowd listening in and participating) may be as effective a means of screening out the less needy than the actual content of the means test (Laforgia 1992b). In Belize, clerks are supposed to assign fees from a sliding scale based on the patient's report of the employment status and earnings of all adult household members. In fact, nearly all patients are assigned to Category II, which corresponds to an income level so low that few households with one working member have incomes that low. The thresholds have not been raised since 1967, even though inflation since that time has been considerable (Laforgia 1992b).

Of these programs that use means testing we know the incidence only of the Jamaican and Sri Lankan food stamp programs. These deliver 56 percent and 57 percent of their benefits, respectively, to those in the poorest 40 percent of the population. Clearly the means tests that they use are not very accurate, yet they do produce a progressive incidence of benefits and are administratively simple. Are they good enough? This can only be answered by comparing them to an alternative.

Proxy Means Tests

Given the administrative difficulties associated with sophisticated means tests and the inaccuracy of simple means tests, the idea of using other household characteristics as proxies for income is appealing, and the purpose of this paper is to explore this alternative. We begin by considering briefly what we know about proxy means tests from other authors' simulations and from program experience in Chile.

In Theory. Ravallion and Chao (1989) limited themselves to just two or three variables to develop a transfer scheme. Their algorithm made optimal use of the available information by explicitly minimizing poverty given an information set and budget. The targeting scheme allowed equal transfers to all individuals within a group, but different transfers among groups of different characteristics. Three studies — Datt and Ravallion (1993) using regional targeting in India, Ravallion (1993) using regional targeting in Indonesia and Ravallion (1989) using landholding classes in Bangladesh — applied the algorithm to quantify the potential impact on poverty. The results are sobering, in that they showed that single indicators at the macro level are of limited use, especially when constraints are put on the transfers to restrict the optimal solutions to something that might be politically feasible.

Haddad, Sullivan and Kennedy (1991) used household survey data from Ghana, the Philippines, Mexico and Brazil to address an issue that is conceptually identical to proxying a means test. They were interested in finding alternative variables that would be easier to measure and that would predict food and nutrition security. They used simple overlaps to judge predictive accuracy. For example, they showed that in rural Brazil, 55 percent of households in the upper tercile in terms of household size were in the lower tercile for food security. Most of the work in the study was devoted to considering each one of an exhaustive list of variables for each of seven data sets and two or three variable combinations. The paper suggested that some variables that would be very simple to collect could serve as good proxies for the measures of caloric adequacy that are usually used as the standard measures of food and nutrition security which are harder to collect as they rely on the memory of individuals and on the anthropometric indicators of pre-school children. This paper also has excellent conceptual chapter fully applicable to targeting to poor households.

Glewwe and Kanaan (1989) have gone furthest in showing how household characteristics can be used to proxy a means test. Using data from Cote d'Ivoire, they used regression analysis to predict welfare levels based on several combinations of variables that are fairly easy to measure. They then assigned to each individual a transfer equal to the difference between their predicted welfare level and the poverty line. They started at the bottom of the distribution of predicted welfare and used up a fixed budget. They then used the Foster-Greer-Thorbecke family of poverty measures to compare the outcomes of various targeting schemes with untargeted and perfectly targeted transfers. Holding the budget constant, they quantified how having more information on beneficiaries can make targeting more accurate and thus reduce poverty more for a given budget. In an alternative scenario, they quantified the budget savings that accrued when poverty was reduced to a given level due to better targeting. The paper demonstrated that simple regression predictions can improve targeting markedly over untargeted transfers. Glewwe (1990) took the same basic approach. Instead of using regressions to predict welfare, he solved a poverty minimization problem to derive weights for each household variable. While theoretically more appropriate, the poverty minimization technique is much more difficult to compute, and produces results not dissimilar from those based on regression analysis.

In Practice. These academic papers all tentatively point towards a system that has been in practice in Chile for a number of years and has recently been introduced in Costa Rica and Colombia. Since 1980, Chile has used the Ficha CAS system to determine eligibility for a number of social programs. The Ficha CAS is a form filled out by a social worker that collects information on household characteristics such as location, housing quality, household composition and education and the work done by the household members. Scores are assigned based on the answers to each question. These scores are then used to determine whether the household is eligible for two large cash transfer programs and for water and housing subsidies, and if so, for what level of subsidy.

All of the means tests (mentioned in the "Means Tests" section on pages one to three) gather information on household characteristics other than income, but none of them make systematic use of that information. Thus, it would be fairly easy for these programs and a host of others that already use some kind of social worker evaluation to apply a proxy means test based on household characteristics other than income. A more systematic use of the information that they already collect may improve targeting outcomes and increase the fairness and transparency of the programs.

As both the academic literature and program experience suggest that non-income household indicators might be used to proxy a means test, the rest of this paper explores this idea further. Chapter II presents a number of simulations similar to those presented by Glewwe and Kanaan to see how well different proxy models perform in targeted programs in Jamaica, Bolivia and Peru.

II. Exploring the Algorithm through Simulation Experiments

The simulation experiment in this paper is designed to predict welfare through an Ordinary Least Squares (OLS) regression based on a set of household information closely correlated to welfare. Program benefits are then distributed to those households that fall below the poverty line based on their *predicted* income level. Targeting accuracy is measured by "leakage" and "undercoverage" rates, and the impact on poverty is measured by the overall reduction in poverty following the implementation of the targeted transfer scheme.

For illustrative purposes, the basic simulations are carried out on the Jamaica household survey data set. Variations on the original model are performed to determine how sensitive the results are to the kinds of changes that might be desirable in adapting the basic model to actual use in social welfare programs. The following sections address the questions: Which variables are good proxies for welfare? Are the outcomes different for rural and urban populations? How sensitive are the results to the poverty line? Do the results vary across countries? and What effects do distorted survey responses have on targeting outcomes?

Data and Country Descriptions

Data. The data used are from the Living Standards Measurement Study (LSMS) surveys in Jamaica, Peru and Bolivia. The surveys are multi-sectoral with modules on income, consumption, savings, employment and unemployment, health, education, nutrition, housing and migration, all designed to evaluate the effect of various government policies on the living conditions of the population. Although there are some differences in the number and size of the modules and in how the modules are adapted to fit country-specific circumstances, the surveys are broadly similar.[2] The questions from each survey used in this paper are almost identical, with the exception of a few country-specific variations.

The Jamaica data set comes from a nationwide survey carried out in November 1989, which included approximately 4,000 households (see Grosh 1990). The data set for Peru comes from a survey that was done in Lima only in 1990 covering an estimated 1,500 households (see Glewwe and Hall 1991). Bolivia's data were collected in a survey covering urban areas during 1990 (third round) from a sample of approximately 7,300 households (see Republica de Bolivia 1993).

Country Descriptions. These countries have been chosen largely because of the availability of data, but also because they exhibit important differences in size, heterogeneity, income and social development. Some basic indicators are presented in Table 1. The implications of these differences will be discussed when the country results are contrasted in Chapter II, in the section titled, "Do the Outcomes Improve Using Only the Poorest 50 Percent of the Population?" beginning on page 18.

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2. See Glewwe (1990) for a more detailed description of the LSMS.

Table 1: Welfare Indicators

	Bolivia	*Jamaica*	*Peru*
Basic Statistics			
GNP per capita (1990 US$)	$630	$1,500	$1,160
Population (1990 millions)	7.2	2.4	21.7
Area (thousands sq. km.)	1,099	11	1,285
Welfare Outcomes			
Infant Mortality (percent)	92	16	69
Life Expectancy (percent)	54	73	62
Literacy (percent)	74	--	85
Coverage of Social Services			
Percent with Access to Health Services	63	90	75
Primary Enrollment (percent)	81	123	105
Secondary Enrollment (percent)	34	67	61
Percent with Access to Safe Water	44	96	55

Source: World Bank (1992).

Methodology

Measuring "True" Welfare. The data sets contain detailed expenditure information and a large number of household and individual characteristics that may be correlated with welfare. We chose per capita household consumption expenditure as the welfare measure. The consumption variable includes all consumption expenditures as well as the value of food and household items received as gifts or produced in the household and the imputed value of the use of durable goods and owner-occupied housing.

Expenditure is generally presumed to be a more accurate measure of welfare than income for several reasons. First, survey respondents who may under-report their income to avoid taxation or other negative repercussions are less likely to do so when reporting their consumption behavior. Second, the comprehensive nature of the expenditure measure makes it easier to compare households that have a high proportion of in-kind income with those that do not. Third, because expenditures tend to be less variable than income over time, the comprehensive expenditure measure also helps to compare between households with seasonal or sporadic incomes and those with more regular incomes.

Predicting Welfare. Varying sets of explanatory variables that are closely correlated with the consumption measure are then used in an Ordinary Least Squares (OLS) regression to predict welfare. A stepwise function is used because it is designed to eliminate from the regression those variables that are not statistically significant and that do not increase the model's overall explanatory power.

Strictly speaking, OLS is inappropriate for predicting poverty for two reasons. First, the technique minimizes the squared errors between the "true" and the predicted levels of welfare. The minimization of least squared errors is a different theoretical problem than that of the minimization of poverty. Ravallion and Chao's (1989) algorithm directly minimizes poverty. Therefore, it produces better poverty results, and could be a better tool for designing a transfer scheme. However, we do not use it throughout this paper because it is cumbersome to use when a large number of predictive variables are available, it does not efficiently use continuous variables and it is unfamiliar to policymakers. OLS may not be the best algorithm to use for a small number of discrete variables, but it is convenient and useful when the large numbers of variables and continuous variables are available. (See Annex I for a fuller discussion of this issue.)

The second problem with using an OLS model is that many of the variables we use on the right hand side of the regression are, strictly speaking, endogenous. In other words, the household makes decisions about them that are not independent of the decisions that determine household welfare, which is the variable on the left hand side of the regression. OLS is nevertheless sufficient in this targeting simulation because we are only concerned with identifying the poor and not with explaining why they are poor.

Selection of Variables. In selecting which variables to use in predicting welfare, it is important to choose those that are closely correlated with welfare to ensure accuracy in prediction. Targeting accuracy is also dependent on the verifiability of the variables. Therefore, for the simulation experiment, we use four classes of independent variables — location, housing quality, family characteristics, and ownership of durable goods.

Location variables are the most easily verifiable. Indeed, they need not be verified at all for some programs. Rather, the program can operate only in poor neighborhoods so that no individual household certification is needed. Examples of such programs include subsidized health clinics, schools or ration shops. For other programs that do require individual household certification, the location of a residence is easily verified by a social worker. **Housing quality** may also be easily verified by a social worker visiting the home. **Family characteristics**, such as the level of education, occupation and number of members, may be difficult to verify, but are frequently assumed by program managers to be reported accurately. **Ownership of durable goods** can be distorted by removing the goods from the home during an expected visit by the social worker, which is easier to do with small or mobile items such as radios, fans, bicycles or cars than for items such as stoves or refrigerators. The presumption is also that people are more willing to lie about ownership of such items than they are about household characteristics.

For the simulations, the variables on the questionnaires for each of the four groups of questions (location, dwelling, family and durable goods) are chosen. Dichotomous variables are then created for some of the continuous variables in order to identify those characteristics that discriminate between poor and rich households.

Models. Five sets of the proxy variables are explored. Model I uses only location variables as predictors. Model II uses location and dwelling characteristics. Model III adds information on family characteristics to the variables used in Model II. Model IV adds information on the ownership of durable goods to that used in Model III. Model V uses only the variables in each category that are the best predictors within their class. Within each model, the precise variable and its definition varies somewhat according to the specific characteristics of the data sets used.

Eligibility Decision. We use the predicted welfare levels to assign individuals to the eligible or ineligible groups. For most of the simulations, we set the poverty line or eligibility cut-off point at the welfare level of the thirtieth percentile of the individual welfare distribution using "true" welfare. The selection of the cut-off point is essentially an arbitrary decision, though the value we have selected is in the range frequently used in the literature on poverty that uses relative measures and, in the case of Jamaica, it is close to the absolute poverty line calculated by Gordon (1989). We perform some sensitivity analysis using other cut-off points.

Targeting Accuracy. To assess how well the alternate targeting formulae work, we look at targeting accuracy using Type I and II errors, and calculate rates of undercoverage and leakage. Individuals are categorized in four groups according to whether their "true" and their predicted welfare levels fall above or below the eligibility cut-off point. Individuals whose "true" and predicted welfare measures put them on the same side of the cut-off line are targeting successes. Those who should not and do not receive benefits under the targeting scheme are likewise a targeting success.

When "true" and predicted welfare levels fall on different sides of the eligibility cut-off point, a targeting error has occurred. A person whose "true" welfare level is below the cut-off but whose predicted welfare is above it will be incorrectly identified as ineligible for program benefits. This kind of error is called a false negative, a Type I error or an error of exclusion in statistical jargon.

"Undercoverage", which is calculated by dividing the Type I error by the total number who should get benefits, is the percentage of those whom the program is meant to cover who are not covered. This undercoverage makes the program ineffective in changing the welfare level of the intended beneficiaries, but it carries no budgetary cost. Since those who are not covered receive no benefits, the program does not incur the costs of delivering those benefits.

The other case of targeting error occurs when a person's "true" welfare level is above the cut-off but his/her predicted welfare is below it. These individuals are thus incorrectly identified as being eligible for program benefits. This kind of error is called a false positive, a Type II error or an error of inclusion, and leads to the "leakage" of program benefits.

"Leakage" is the percentage of program benefits that are received by people who are not eligible to receive them. It is calculated by dividing the number in the Type II error category by the number of persons served by the program. Leakage increases program costs by giving benefits to those who are not the intended recipients, thereby rendering the program inefficient.

Lower rates of undercoverage and leakage are preferable to higher rates. Comparing undercoverage and leakage to each other, however, is more difficult. In general, the higher the priority assigned to raising the welfare of the poor, the more important it is to eliminate undercoverage. With a Rawlsian social welfare function, undercoverage is the only consideration. Conversely, the higher the priority assigned to saving limited budget funds, the more important it is to eliminate leakage.

In practice, poverty and social programs aim to raise the welfare of the poor as much as possible within their budget constraints. Both kinds of error are, therefore, important, and a firm preference for one over the other is rarely stated. It is perhaps interesting to note that minimizing undercoverage has been a traditional argument in favor of universal subsidies, especially of food prices. However, with the advent of tight budgetary constraints in the 1980s and 1990s, many governments have moved away from universal subsidies towards targeted programs that presumably lower leakage but that introduce the risk of undercoverage.

Impact on Poverty. We choose between different rates of undercoverage and leakage by looking at the impact each rate has on poverty. The mechanism that reduces poverty the most for a given transfer budget is preferred.

We use the Foster-Greer-Thorbecke (FGT) class of poverty measures because it has all the axiomatically desirable properties and contains common and easily understood variables (see Box 1 for a brief explanation of the measure and Foster, Greer and Thorbecke 1984 for details).

In testing how alternative targeting mechanisms affect poverty, it is necessary to specify a transfer formula. Here we assume a constant budget of J$1 million, or about 1 percent of the total welfare of the sample. This budget is divided equally among all those who are identified as eligible according to the different models. Thus, the amount received by each recipient varies among models.

Following the transfer, the post-transfer welfare of the recipients is calculated, and the poverty index is then determined based on this distribution of welfare.

Which Variables Are Good Proxies for Income?

From the classes of variables described above, we try to determine which variables are good proxies for income. The simulation experiment, referred to as Simulation A, is carried out on the Jamaica data set. A stepwise regression is performed for the five models described above. Table 2 presents the exact variables used in each model, as well as the R^2 value. Full regression results with beta weights and T statistics are reported in Annex II. As more variables are added to the equation, the R^2 rises, indicating that the ability of the model to predict welfare increases. The real test of the simulation for our purposes, however, is not the R^2 but targeting accuracy and the impact on poverty.

Box 1: Foster-Greer-Thorbecke Poverty Measures

The formula for the FGT poverty index is:

$$P_\alpha = \frac{1}{n}\sum_{i=1}^{q}\left(\frac{Z-y_i}{Z}\right)^\alpha$$

where Z = poverty line
y_i = income of the ith person
q = the number of poor
n = the total population

The n people in the population are ranked by welfare from poorest to richest: i = (1, 2... q..n). the parameter α represents the sensitivity to the income distribution among the poor. When $\alpha = 0$, the FGT measure collapses to the Headcount ratio or the percentage of the population that is below the poverty line. This measure can give estimates of how many of the poor should be served by poverty programs, but is insensitive to differences in the depth of poverty. Suppose the poverty line is \$100. there are ten people in the economy and two are poor. The Headcount index will give the same result ($P_0 = .2$) if there are two poor people with incomes of \$95 as it would with two incomes of \$5, yet clearly, in the latter case poverty is more severe.

When $\alpha = 1$, the FGT index becomes the Poverty Gap, a measure of the depth of poverty. This measures the total income shortfall as a percentage of the poverty line. Thus, in the case of the two poor people with incomes of \$95, $P_1 = 0.01$. With two poor people earning \$5, P_1 would be 0.19.

The drawback to the Poverty Gap measure is that it will estimate the poverty to be the same when one poor person has an income of \$90 and the other an income of \$10 as it would when both have an income of \$50. Yet most people would agree that the suffering of the extremely poor person with only \$10 is worse than that of the poor person with \$50 or \$90. This is overcome for $\alpha > 1$. Let us use $\alpha=2$. Then the first case gives $P_2 = 0.082$ and the second gives 0.025. The drawback to using $\alpha = 2$ is that the measure is hard to interpret.

Source: from Grosh (1994), Box 3.1, p. 25.

Targeting Accuracy. Model I uses only geographic characteristics — Kingston/other towns/rural areas, and a parish dummy. It identifies no-one as poor, because the predicted welfare level in each area is above the eligibility level used (J\$2,875). While this result might prompt the conclusion that geographic targeting is absolutely useless in Jamaica, this is not quite the case. Geographical location, when used in a different type of simulation experiment, proved to be an effective targeting mechanism (see Baker and Grosh 1994). In this simulation, the small number of variables in the regression (which are largely dichotomous) limit the models' ability to predict income accurately.

Model II uses both location and dwelling characteristics. Characteristics such as building material and method of sewage disposal are easily verifiable by a social worker. Because the

Table 2: Variables Used in Simulation A, Jamaica

Model I: LOCATION — R^2=.11
Area where household is located (1=Kingston, 2=other urban, 3=rural)
Parish where household is located (dichotomous variable based on poor areas)

Model II: LOCATION AND HOUSING — R^2=.21
All variables in Model I plus:
Type of dwelling (0 if separate; 1 if other)
Type of walls (0 if wood; 1 if other material)
Electricity (0 if household has it; 1 if not)
Water source
Toilet facilities (0 if pit; 1 if other)
Kitchen (0 if shared; 1 if used exclusively by household)

Model III: LOCATION, DWELLING AND FAMILY — R^2=.36
All variables in Model I and II plus:
Number of people in the household
Level of education attained
Age of the head of household
Sex of the head of household
Number of children under the age of five in the household
Employment (0 if not, 1 if yes)
Type of employment (0 if agriculture, 1 if informal sector, 2 if formal sector)

Model IV: LOC., DWELL., FAM. & OWNERSHIP OF DURABLES — R^2=.41
All variables in Models I, II and III plus:
Ownership of home
Ownership of a telephone
Ownership of a gas stove
Ownership of an electric stove
Ownership of a refrigerator
Ownership of an air conditioner
Ownership of a fan
Ownership of a radio/cassette player
Ownership of a stereo
Ownership of a motorbike

Model V: SUB-SAMPLE OF HIGHLY SIGNIFICANT VARIABLES --R^2=.34
Area
Household size
Electricity
Toilet facilities
Ownership of a telephone

number of permutations of variables is still low and the average welfare level of most of the groups is above the eligibility cut-off point, Model II suffers from the same problems as Model I. The results are better than those in Model I, but are still poor. An extremely low portion of those who are eligible receive benefits, with undercoverage at 92 percent. On the other hand, leakage is quite low, at 31.4 percent. Results on targeting accuracy are presented in Table 3.

Table 3: Results for Simulation A, Jamaica - Basic Simulation, J$2,875 Poverty Line/J$2,875 Eligibility Threshold

	Targeting Accuracy		Poverty Impact			
Model	Under-coverage	Leakage	Transfer per Benefic.	Post-transfer $\alpha=0$	Post-Transfer $\alpha=1$	Post-Transfer $\alpha=2$
Model I	100.0	0	0	-	-	-
Model II	92.0	31.4	J$2110	.277 (-7.83)	.091 (-13.57)	.041 (-18.76)
Model III	41.3	33.7	J$294	.279 (-6.76)	.084 (-26.30)	.033 (-42.24)
Model IV	41.0	34.2	J$286	.277 (-7.69)	.083 (-24.81)	.033 (-44.46)
Model V	47.7	36.1	J$297	.284 (-5.24)	.088 (-17.79)	.036 (-32.12)
Uniform Transfer	0	70.0	J$ 73	.288 (-3.68)	.096 (-7.77)	.043 (-11.93)

Note: The poverty line is J$2,875 or the 30th percentile of "true" welfare. The eligibility threshold is a predicted welfare of J$2,875. Under the post-transfer columns, the first number is the poverty index and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. The initial poverty indices were: $\alpha=0$,.298; for $\alpha=1$,.103; and for $\alpha=2$,.047.

Models III and IV use many more and continuous variables, so the advantages of using OLS rather than a poverty minimization algorithm begin to come to the fore. However, these variables also introduce the problems of using indicators that are difficult to verify. They perform better in terms of undercoverage than the previous models, with rates falling to 41.3 percent and 41.0 percent respectively. They identify about a quarter of the population as being eligible for benefits, with program costs commensurate with covering 26.1 percent and 26.9 percent of the population respectively. Leakage rates are respectively 33.7 percent and 34.2 percent of the benefits assigned.

In this case, the information on durable goods adds very little, although in other simulations the information will prove more useful. Because the information on ownership of durable goods is relatively difficult to verify, an improvement in targeting accuracy must be weighed against the likelihood of misrepresentation and the costs of verification. If these problems are important, it may be just as satisfactory to use Model III.

Model V uses only those five variables that produce the highest t values in the regression model. The variables come from all four classes of information. The model therefore suffers from the same problems of verification that Models III and IV do, but using so few variables would reduce information processing costs. The main disadvantage of Model V is that undercoverage is somewhat higher than for Model IV.

Who Is Missed. While it is unsatisfactory to fail to cover those who fall below the poverty line, the error is less grave if the people who are excluded fall only just below the poverty line rather than at the very bottom of the welfare distribution. Figure 1 shows the relative position of those below the poverty line whom the simulation models identify as being ineligible. As the information used in the model increases, not only does the total occurrence of Type I errors fall but also they are more heavily concentrated among those just under the poverty line. Thus, Model IV, which has the lowest Type I error (41 percent of the eligible population), also performs best in terms of who was excluded. Of those incorrectly excluded, fully half are in the decile just under the poverty line. Only 17 percent of those incorrectly excluded are from the poorest decile.

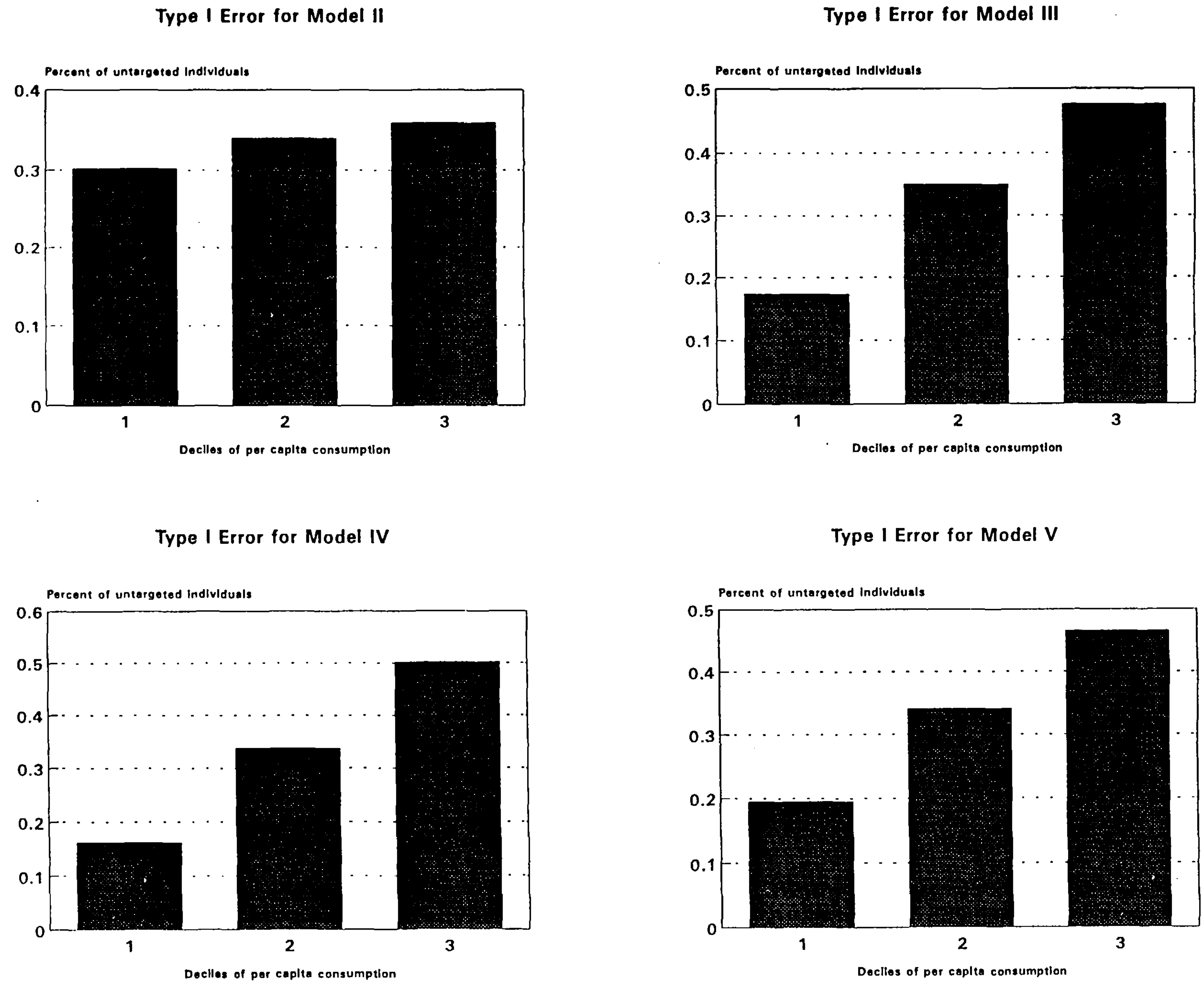
Poverty Reduction with Fixed Transfer Budget. The best way to judge whether the levels and tradeoffs between undercoverage and leakage are acceptable is to calculate the changes in the poverty indices that result from the different models. The model that reduces poverty the most given a fixed budget is the most acceptable.

We take a fixed budget for program benefits equal to J$1 million (or about 1 percent of the welfare level of our sample — grossed up to the national level this would be about US$22 million). We distribute this evenly among those identified by the respective models as being eligible. Thus, the amount transferred varies as the number of persons identified as being eligible varies. We also make a uniform transfer to all members of the population for the sake of comparison. The results are presented in the Poverty Impact section of Table 3.

The change in poverty measures under the targeted models is better than that of a uniform transfer. With J$1 million to spread uniformly among the whole of our sample, each individual would receive J$73.15. This would lower the Poverty Index for $\alpha=0$ from .298 to .288, that is, by 3.68 percent. Using Models II-IV, the Poverty Index for $\alpha=0$ falls by about twice that relative amount. For Model IV, the index falls by 7.69 percent to .277.

When the importance of inequality is deemed to be high, then the Poverty Index with $\alpha=2$ will be more relevant. For $\alpha=2$, the change in the poverty index under any of the targeting schemes is proportionately greater, and the improvement from the more sophisticated models is greater. A uniform transfer to the whole population changes the Poverty Index with $\alpha=2$ from .047 to .043, a decrease of 11.93 percent. Model IV lowers the index to .033, a decrease of 44.46 percent.

Figure 1: Type I Errors



Raising the Eligibility Cut-off Point. One way to reduce undercoverage is to use the same models to predict welfare, but to use a higher cut-off point for establishing eligibility. To try this, we raise the cut-off point to J$3,647 (the 40th percentile of the "true" welfare distribution). We keep the same criterion as previously for judging the outcome — J$2,875. This procedure should lower undercoverage while raising leakage, as is shown in the results in Table 4. In Model IV, the higher cut-off point lowers undercoverage from 41 percent previously to 32 percent. Leakage increases from about 34 percent of benefits to 38 percent of benefits, and program costs rise from 26.9 to 33.1.

Given a fixed transfer budget of J$1 million, poverty indices improve less with the higher eligibility threshold than with the lower threshold. Thus, the increase in leakage outweighs the decrease in undercoverage.

Discussion. In general, the degree of undercoverage is disappointingly high. Models I and II, with verifiable indicators, correctly identify only a small fraction of the individuals that should receive benefits. Models III and IV, with less verifiable indicators, correctly identify only about half of those who should receive benefits.

Of the five OLS models, Model IV reduces poverty the most. Models using more variables that are more difficult to verify perform better than simpler, verifiable models. The improvement in poverty outcome using the proxy method of predicting welfare levels and then assigning program benefits on that basis is large when compared with a uniform transfer scheme. This makes the method a potentially interesting policy tool. The next sections of this paper will, therefore, discuss variations on the modeling procedures to determine how sensitive they are to changes in the poverty line, in population subgroups and in countries. Only the full model, Model IV, will be presented in the text.

Table 4: Results for Jamaica, Simulation B: Raised Eligibility Threshold J$2,875 Poverty Line/J$3,647 Eligibility Threshold Full Model (IV)

	Targeting Accuracy		*Poverty Impact*			
Model	*Under-coverage*	*Leakage*	*Transfer per Benefic.*	*Post-transfer* $\alpha=0$	*Post-Transfer* $\alpha=1$	*Post-Transfer* $\alpha=2$
Model IV	31.0	39.3	J$227	.277 (-7.59)	.084 (-22.83)	.034 (-39.99)
Uniform transfer	0.0	60.0	J$ 73	.288 (-3.68)	.096 (-7.77)	.043 (-11.93)

Note: The poverty line is J$2,875 or the 30th percentile of "true" welfare. The eligibility threshold is a predicted welfare of J$3,647 (or the 40th percent of "true" welfare). Under the post-transfer columns, the first number is the poverty index and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. The initial poverty indices were: α=0,.298; for α=1,.103; and for α=2,.047.

How Sensitive Are the Results to the Poverty Line?

In this section, we examine the sensitivity of the proxy means testing procedure to the choice of poverty line. When the poverty line changes, the proportion of people that the program should cover changes. The interpretation of the undercoverage, leakage and program costs remains the same.

The results of the simulations are presented in Table 5 for the 10th and 40th percentile poverty lines. The degree of undercoverage is not very sensitive to the poverty line. Under Model IV, undercoverage ranges from 35.8 percent for the 10th percentile poverty line to 38.6 percent for the 40th percentile line. Full results for all five models are presented in Annex II.

The problem of leakage becomes more severe the lower the poverty line is set. For Model IV, it rises from 24.8 percent for the 40th percentile poverty line to 66.7 percent for the 10th percentile poverty line.

For the higher poverty lines, the better models identify and serve fewer persons in total than the target. For the 40th percentile poverty line and Model IV, the program actually serves 33 percent of the population. For the 10th percentile poverty line, the opposite is true. It means to serve 10 percent, but in fact identifies 18 percent of the population.

Table 5: Results for Simulation C, Jamaica, Comparison of Poverty Lines, Full Model (IV)

	Targeting Accuracy		*Poverty Impact*			
Model IV	*Under-coverage*	*Leakage*	*Transfer per benefic.*	*Post-transfer* $\alpha=0$	*Post-transfer* $\alpha=1$	*Post-transfer* $\alpha=2$
10 percent Poverty Line (J$1,593)	35.8	66.7	J$405	.069 (-53.50)	.092 (-154.3)	.001 (-321.7)
30 percent Poverty Line (J$2,875)	41.0	34.2	J$286	.277 (-7.69)	.083 (-24.81)	.033 (-44.46)
40 percent Poverty Line (J$3,647)	38.6	24.8	J$227	.392 (-3.40)	.137 (-13.68)	.063 (-24.7)

Note: The eligibility threshold for each row is the same as the poverty line. Under the post-transfer columns, the first number is the poverty index under the given poverty line and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. Initial poverty indices were: for the 10 percent poverty line, $\alpha=0$,.106; $\alpha=1$,.023; $\alpha=2$,.007; for the 30 percent poverty line, $\alpha=10$,.298; $\alpha=1$,.103; $\alpha=2$,.047; and for the 40 percent poverty line, $\alpha=0$, .405, $\alpha=1$, .156, and $\alpha=2$, .079.

Are the Outcomes Different for Rural and Urban Populations?

The same basic simulation is carried out separately for rural and urban subsets of the population. The motivation for this is that the infrastructure and housing characteristics of the two populations may differ to the extent that models should be developed for each group individually. For example, even relatively wealthy rural residents may not be served by a sewerage disposal system, nor have their water piped from a city system. Urban residents not connected to water and sewerage are likely to be poor, whereas, in rural areas, this correlation is less strong.

We first split the population into rural and urban subsets, and for each in turn, we repeat the procedure outlined above. We thus allow both the variables that enter the equations and their coefficients to differ. The variables that enter the equations for rural and urban areas separately are presented in Table 6. They differ little from the whole population models, though their coefficients do differ. Using the separate models those with predicted welfare below J$2,875 are regarded as eligible, and the population is recombined to analyze targeting accuracy.

Fine-tuning the simulation models to differences between rural and urban areas makes relatively little difference in the outcomes for Jamaica. For most of the models, the under-

Table 6: Variables in Rural and Urban Model IV: Location, Housing, Dwelling and Family Characteristics

Urban: R^2=.36	*Rural: R^2=.36*
Area	Area
Parish	Parish
Type of dwelling	Type of dwelling
Type of walls	Type of walls
Electricity	Electricity
Toilet facilities	Toilet facilities
Kitchen	Kitchen
Number of people in the household	Water source
Level of education	Number of people in the household
Age of the head of household	Level of education
Sex of the head of household	Age
Children under the age of five	Sex
Employment	Children under the age of five
Ownership of a telephone	Employment
Ownership of a gas stove	Ownership of a telephone
Ownership of an electric stove	Ownership of a gas stove
Ownership of a refrigerator	Ownership of an electric stove
Ownership of an air conditioner	Ownership of a refrigerator
Ownership of a radio/cassette	Ownership of an air conditioner
Ownership of a stereo	Ownership of a radio/cassette
Ownership of a motorbike	Ownership of a stereo
Ownership of a bicycle	Ownership of a fan

Table 7: Results for Simulation D, Jamaica, Separate Urban and Rural Weights, J$2,875 Poverty Line/J$2,875 Eligibility Threshold, Full Model (IV)

	Targeting Accuracy		*Poverty Impact*			
Model	*Under-coverage*	*Leakage*	*Transfer per Beneficiary*	*Post-transfer* $\alpha=0$	*Post-transfer* $\alpha=1$	*Post-transfer* $\alpha=2$
Model IV	49.8	31.8	J$347	.274 (-8.73)	.082 (-25.77)	.033 (-45.78)
Uniform	0.0	70.0	J$75.00	.288 (-3.68)	.096 (-7.77)	.042 (-11.93)

Note: The poverty line is J$2,875 or the 30th percentile of "true" welfare. The eligibility threshold is a predicted welfare of J$2,875. Under the post-transfer columns, the first number is the poverty index and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. The initial poverty indices were: $\alpha=0$,.298; for $\alpha=1$,.103; and for $\alpha=2$,.047.

coverage rates are slightly worse, and leakage slightly better (see Table 7). The change in the poverty index from transfers assigned under the two simulation schemes are very similar, and the direction of the change depends upon the model and the α value.

The difference in outcome is too small to justify the administrative complexity of using two separate algorithms, one for rural areas and one for urban areas. The simple, unified algorithm will serve as well.

Do the Outcomes Improve Using Only the Poorest 50 Percent of the Population?

This section uses only the poorest half of the population as the basis for building the targeting models. This puts more emphasis on accurately predicting the welfare of those near the bottom of the distribution, where improvements are most relevant when reducing poverty is the key concern.

Using only the bottom half of the welfare distribution may also be more realistic than using the whole population. For most social welfare programs, the recipient must take some action to apply. Members of the middle and upper class may not bother to do so. Only those who think they stand a good chance of qualifying or for whom the benefit is worth the nuisance or stigma of applying will actually put themselves forward as candidates. Thus, the proxy means test might in practice only be applied to those who are near the bottom of the welfare distribution.

The variables that enter the regressions as good predictors and their coefficients are determined using only the poorest half of the sample. The variables that are used in Model IV are shown in Table 8.

Once again, let us set the standards of comparison appropriate for this case before proceeding to the results. The poverty line used is J$2,875, still that of the 30th percentile of the whole population. We exclude the top half of the distribution from this case, so that, of the bottom half, we know that 60 percent will be poor (30 percent of the whole).

The targeting accuracy of this simulation in which only the poor are considered is dramatically better than when the whole population is included. Undercoverage for all models is under 20 percent, and as low as 13 percent for Model IV (see Table 9). This contrasts sharply with the undercoverage rate of 41 percent for Simulation A (Model IV). Nearly all of the poor are identified and covered in this scenario. Leakage is also slightly lower than in previous simulations. For Model IV, leakage is as low as 28.2 percent, whereas in Simulation A, leakage for Model IV was 34.2 percent. Program costs are, of course, somewhat higher because so many of the poor are reached. Program costs are commensurate with covering 36.8 percent of the population for Model IV.

Table 8: Variables for the Poorest 50 Percent Only, Model IV: Location, Housing, Dwelling and Family

Model IV: R^2=.28
Area
Type of walls
Parish
Electricity
Toilet facilities
Kitchen
Number of people in the household
Level of education
Age of the head of household
Ownership of home
Ownership of a gas stove
Ownership of a refrigerator
Ownership of a radio/cassette
Ownership of a sewing machine
Ownership of a bicycle
Ownership of a television
Ownership of a fan

A final set of simulations is presented that compares the results of the procedure used on the urban populations of Jamaica, Bolivia and Peru (Lima only). In order to present comparable results, the basic simulation is rerun for Jamaica, using only the urban population, and assigning a poverty line congruent with the 30th decile of the urban welfare distribution. In all cases, a transfer budget equal to 2.5 percent of the poverty line times the population size is used. Those variables that are entered into the model are included in Annex II.

Table 9: Testing the Poorest 50 Percent in Jamaica, J$2,875 Poverty Line/J$2,875 Eligibility Threshold, Model IV

	Targeting Accuracy		*Poverty Impact*			
Model	*Under-coverage*	*Leakage*	*Transfer per beneficiary*	*Post-transfer $\alpha=0$*	*Post-transfer $\alpha=1$*	*Post-transfer $\alpha=2$*
Model IV	13.1	28.2	J$193	.560 (-7.06)	.178 (-17.66)	.771 (-28.84)
Uniform transfer	0		J$73.00	.396 (-2.43)	.148 (-5.41)	.073 (-8.36)

Note: The poverty line is J$2,875 or the 30th percentile of "true" welfare. The eligibility threshold is a predicted welfare of J$2,875. Under the post-transfer columns, the first number is the poverty index and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. The initial poverty indices were: α=0,.298; for α=1,.103; and for α=2,.047.

Do the Results Vary across Countries?

Table 10 presents comparative results for the three countries. For urban Jamaica, the pattern of results is much as before, although undercoverage is somewhat higher and leakage is lower.

The results for Bolivia are very comparable to those for Jamaica. This holds for the changes between models, the level and mix of errors and the impact on poverty.

For Peru, the location and housing quality variables are insufficient to identify beneficiaries (results not shown here). When family characteristics and durable goods ownership are added, Model IV performs reasonably well for leakage, with 35.1 percent of benefits leaking. It, however, performs worse than in Jamaica and Bolivia with respect to undercoverage — excluding 53.8 percent of those eligible. Changes in poverty levels are less than found for Jamaica. Model IV, for example, reduces poverty by 6 percent for $\alpha=0$ and by 36 percent of $\alpha=2$ for Peru, and by 11 percent and 36 percent for Jamaica.

The relatively poor performance of the models in Peru may be due to a number of factors. The poor performance of housing variables is partly due to the fact that electricity, water and sewerage systems are quite extensive, covering 90, 70 and 72 percent of the population respectively (Glewwe and Hall 1991). These systems have been functioning very inadequately since Peru's economic crisis. It could be that questions about number of hours of service per day would be better predictors than the existing questions that ask only about access to the service.

Even Model III, which includes variables covering family size and the education, sex and age of the head of the household, performed less well in Peru than in the other two

Table 10: Cross-Country Simulations for Urban Areas, 30 Percent Poverty Line/30 Percent Eligibility Threshold, Full Model (IV)

	Targeting Accuracy		*Poverty Impact*			
Model	*Under-coverage*	*Leakage*	*Transfer per beneficiary*	*Post-transfer $\alpha=0$*	*Post-transfer $\alpha=1$*	*Post-transfer $\alpha=2$*
Jamaica	43.0	26.1	J$459.70	.281 (-6.66)	.077 (-20.78)	.028 (-37.10)
Bolivia	39.3	23.5	8.32 Bs	.281 (-6.29)	.076 (-24.36)	.027 (-46.21)
Peru	53.8	35.1	114.30 Intis	.291 (-5.96)	.079 (-19.78)	.029 (-35.86)

Note: The poverty lines and eligibility thresholds are as follows: Jamaica, J$4,268, Bolivia, 77 Bs, and Peru, 955 intis, all equivalent to the 30th percent of "true" urban welfare. Under the post-transfer columns, the first number is the poverty index, and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. Initial poverty indices were: for Jamaica, $\alpha=0$,.300; $\alpha=1$,.094; $\alpha=2$,.039; for Bolivia, $\alpha=0$,.298; $\alpha=1$,.094; $\alpha=2$,.039; and for Peru, $\alpha=0$,.308; $\alpha=1$,.096; $\alpha=2$,.039.

countries. This may be due in part to the fact that the economic crisis has led to massive new cyclical poverty on top of the structural poverty that already existed. Previously those people who were able to get educated were able to get ahead, but since the crisis even many of these educated people are now poor (see Instituto Cuanto 1991 for a discussion of new and structural poverty). Finally, since the data come only from Lima, they are less heterogeneous than the data for Bolivia and Jamaica.

What Effect Does Distortion Have on Outcomes?

Included in the simulations are two classes of information that are difficult to verify — family characteristics and the ownership of durable goods. An individual may not report the truth in answering these two classes of questions if they believe that the distorted answer will enable them to benefit from a transfer program. In order to assess what impact this may have on targeting outcomes, we perform a simulation using distorted information for some of the proxy variables.

For the simulation, we assume that 25 percent of households will exaggerate or conceal certain information, although this may be an over-estimation of the actual distortion. We assume that 25 percent of randomly chosen households exaggerate their household size by one more person, report only primary-level school attainment even if some secondary schooling was received and do not report ownership of a radio, fan, stereo or motorbike. These particular durable goods were chosen because they are easy to hide if a social worker makes a home visit.

The original simulation using a 30 percent poverty line was carried out with the distorted information, producing outcomes that were almost identical to the original simulation. Undercoverage was 40.2 percent, and leakage was 34.3 percent. The reductions in poverty following a transfer of J$233 per beneficiary were also almost identical to the original information (see Table 11).

Table 11: Comparison of True and Distorted Information, J$2,875 Poverty Line/J$2,875 Eligibility Threshold, Model IV

	Targeting Accuracy		*Poverty Impact*			
Model	*Under-coverage*	*Leakage*	*Transfer per beneficiary*	*Post-transfer* $\alpha=0$	*Post-transfer* $\alpha=1$	*Post-transfer* $\alpha=2$
Original Information	41.0	34.2	J$286	.277 (-7.69)	.083 (-24.81)	.033 (-44.46)
Distorted Information	40.2	34.3	J$266	.269 (-10.50)	.082 (-25.54)	.033 (-42.21)

Note: The poverty line is J$2,875 or the 30th percentile of "true" welfare. The eligibility threshold is a predicted welfare of J$2,875. Under the post-transfer columns, the first number is the poverty index, and the number in parentheses is the percentage change from the initial, pre-transfer poverty index value. The initial poverty indices were: $\alpha=0$,.298; for $\alpha=1$,.103; and for $\alpha=2$,.047.

These results show that even with an exaggerated assumption of distortion by 25 percent of households, outcomes would be the same as with the undistorted information. Thus, we can infer that some distortion in reporting will not affect targeting accuracy greatly.

Summary and Conclusions

In this part of the paper, we have seen that household characteristics can serve as reasonable proxies for information on income in assessing eligibility for social programs. More information is generally better than less, though there are diminishing returns. The proxy systems all have significant errors of undercoverage, but cut down leakage so much that the impact on poverty is better with imperfect targeting than with none. Some fine-tuning of the basic system, such as calibrating for the poorest half of the population, improve results considerably. In Jamaica, calibrating separately for rural and urban areas did not improve results. An assumed 25 percent level of distortion of information had no effect at all on targeting outcomes. In the next part of the paper, we examine a program that actually uses this type of system.

III. Chile's Program Experience with Proxy Means Tests

Basic Description

Chile's Ficha CAS system began as part of a sweeping effort to target social services to the poor.[3] Information on household characteristics is gathered through household surveys and a weighting system is applied to the answers to determine eligibility for Chile's social programs. Two large cash transfer programs — the family subsidy and old age pensions for the poor — are assigned in this way, as are housing subsidies and, more recently, water subsidies. The information is gathered by social workers during visits to the home, which allows them to verify many of the variables. A widespread outreach effort was made when the system was first implemented. Social workers tried to reach near census-level coverage in the areas of the country where previous poverty maps had showed the poor to be concentrated.

The basic concept of the Ficha CAS has remained constant, but the implementation of the system has changed as experience has been gained in the field. There have been two phases of implementation so far — CAS-I and CAS-II. The term "CAS-I" refers to the system from its inception in 1980 until 1987. During this period, a very simple form was used to collect information on fourteen variables on the location and housing characteristics of the household and the educational attainment and labor activity of household members. The form was constructed so that a score was assigned to each possible answer. For example, Question 6 asked what kind of cooking fuel was used in the home. An answer of gas or electricity got a score of 4, coal or paraffin got a score of 2 and wood or other got a score of 0. These scores were marked by the social worker in a column on the right side of the card. At the bottom of the card, the social worker totalled the score. At the end of the interview, therefore, the social worker could tell the household whether they qualified for certain social programs. The system was highly decentralized. Each municipality carried out the interviews, classified beneficiaries and sent lists of eligible beneficiaries to the central offices of those programs that were participating in the system. Aggregate results were also used in the planning process in sectoral ministries and social programs and in the Office of Planning itself, particularly to determine where to locate facilities and programs.

An evaluation of CAS-I pointed up several flaws. The variables used and the weighting scheme overestimated poverty in rural areas, did not allow evaluation of families doubled up in one dwelling due to poverty and did not contain an adequate amount of information on health and education to guide major social expenditure programs. In terms of administration, it was found that there had been inadequate training of staff at the municipal level, and, thus, municipalities did not have sufficient qualified personnel to run the system. Furthermore, the fact that the simple formula used to weight information was publicly known made it easy for the candidates to know how to bribe the social workers to falsify information on the form.

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3. This chapter is largely based on Sancho (1992) with supplemental information from Raczynski (1991) and Vial et al (1992a and b). CAS stands for Comunales de Asistencia Social.

The term "CAS-II" refers to the system from 1987 to the present. The survey form has been lengthened to include questions on income and wealth, on participation in or use of social programs and on health and education. Now, rather than having the interviewers figure the scores manually in the field, the answers are entered into computer databases that calculate the score and determine eligibility for programs. The management of the system is still largely decentralized in that municipalities are responsible for most of the administration. The individual social workers, however, no longer determine scores. In some municipalities data entry and weighting is done in-house; in many others it is contracted out to private firms. The central Secretariat of Social Assistance and Development is also taking a more active role in training and supervision than was the case under CAS-I.

Technical Issues

The CAS-I formula was derived as a simplified version of a socio-economic index that had previously been used. The original socio-economic index was the result of a principal components analysis of the Continuous Nutrition Status Survey (ECEN) that contained a good deal of socio-economic information. Using the socio-economic index as the dependent variable, multiple classification techniques were used to determine the ten variables that were easiest to collect and the weights that would best predict the socio-economic index. The formula used produced a correlation of 90 percent.

The CAS-I scores ranged from 3 to 133 points, and were then summarized in five groups. Those with scores from 3 to 50 points were assigned to index 1, which accounted for the poorest 5 percent of the population. Those with scores from 50 to 63 were assigned to index 2, which comprised the next 10 percent of the population. Scores from 64 to 75 were assigned to index 3, which comprised the next 15 percent of the population. In general, those who were assigned to indexes 4 (scores of 76 to 87) or 5 (scores of 88 to 133) were not eligible for benefits from any of the programs for which the Ficha CAS system served as the means of determining eligibility.

The CAS-II system is the result of two years of work and of "expert negotiations" among professionals in various planning offices. The range of scores is from 314 to 780 points, but the system of grouping scores is similar. The exact formula is no longer public. There are different formulae for urban and rural areas. In each formula, housing variables, household per capita income and the occupation of the head of household each account for about a quarter of the score. In urban areas, education accounts for about 10 percent of the score, and wealth about 20 percent. In rural areas, education accounts for 20 percent and wealth for 5 percent. Within the housing variables, sanitation gets about 40 percent of the score, crowding 20 percent and comfort 50 percent. Within the wealth variable, real estate gets 80 percent of the score and equipment 20 percent.

Cost and Administrative Requirements

The Secretariat of Social Assistance and Development (SEDAS) has run the Ficha CAS system since 1987. They have a team of four or five full-time professionals who write the operational manuals, train the personnel at various levels and travel throughout the country supervising the system. The Regional Superintendencies also play a role in supervising the way the system operates in each of the 13 regions. This is done with existing personnel on a limited basis.

The municipalities carry out the bulk of the work, using a team consisting of several people. The project officer is generally in charge of the system. The chief of the municipality's Social Department may also spend part of his/her time managing the system. A field chief is an optional member of the team; s/he helps to plan and direct the fieldwork. A reviewer is responsible for the office-based quality review of forms that have been filled out in the field. A supervisor supervises the field work. Interviewers do the field work. Initially, government public employment programs provided interviewers at low cost, but when these programs closed, many municipalities contracted out the fieldwork. Finally, data entry clerks and computers are needed. Data entry is sometimes done in-house and is sometimes contracted out, either by individual municipalities or by all of the municipalities in a region together.

About 1,100 people are involved in implementing the Ficha CAS system. Except for the central team of four or five in SEDAS, most of these people work with the system only part-time. Their full cost cannot be ascribed to the system, but they must be trained and supervised. The cost of the system is approximately US$5 per assessment.

Program Experience with CAS

Three programs in Chile use the CAS system now. The CAS system costs these programs from 0.5 to 5.4 percent of their costs (figured as though the whole of the CAS cost were a cost to each program). For the two programs that rely most on the CAS score, 72 and 62 percent of program benefits reach the poorest 30 percent of the population, which is the goal (see Table 12). This is an excellent record.

The **Family Subsidy** program tries to help those who do not receive the family transfer benefits that are provided to formal sector workers linked with social security systems. The Family Subsidy provides monthly cash payments to children under the age of fifteen and pregnant women. The targeting criteria is that the household have a CAS score of 1 or 2, children under the age of five must have up-to-date health care and children over the age of six must be enrolled in school. The municipality determines the household's CAS score, and passes the list of eligible households to the institution that pays out the Family Subsidy. As a fraction of program benefits, the cost of targeting is 5.4 percent (counting the full cost of the CAS system to the program).

Assistance Pensions are aimed at the poor, the handicapped and the elderly. Program eligibility is determined by the Ficha CAS and, for the handicapped, by medical certificate. Targeting costs as a fraction of program benefits are 0.5 percent.

The Housing Subsidy programs consists of a single payment to poor families in very poor housing situations. To be selected to receive a subsidy, a family has to meet a number of criteria including that they already have a place on the waiting list, they have savings for the purchase of a home, they have the ability to service the mortgage payment with less than 20 percent of the household's income, their CAS score is sufficient, and the family composition meets defined criteria. Program benefits per family are over US$4,000, so the cost of the CAS is 0.1 percent of program costs. The targeting is, however, less progressive than for the other programs, because of the requirements regarding savings and mortgage payments.

Two other programs tried to use the CAS-I and rejected it, largely because it was found to be administratively impractical for their purposes. Some of the problems these programs experienced were the same as those that led to the reform of the system from CAS-I to CAS-II and may to some degree have now been solved or minimized. The two programs that rejected the CAS system, however, have more candidates needing assessment in a short time than the programs that continue to use it.

The two programs that stopped using the Ficha CAS to target their beneficiaries were the school lunch and the complimentary feeding programs. In the case of the school lunch program, the system was used in its early days from 1980 to 1982. Four problems were found. The Ficha CAS was only applied to about 55 percent of students. There were also logistical problems in the system, which led to the falsification of information on households' cards. This, therefore, made the scores poor targeting indicators. The information was frequently slow in being passed from the CAS system to the schools and clinics. Finally, the scoring system overestimated rural poverty.

In the case of the complimentary feeding program, from 1983 to 1985, supplemental benefits were assigned to children at socio-economic risk of malnutrition as defined by a CAS score of 1 or 2. Three problems were found. Because the system was not computerized, it was slow and difficult to get lists of eligible beneficiaries from the municipal office to the clinics. Because there was a backlog of families to be assessed, clinic nurses sometimes carried out the assessment in the clinic rather than in the home, which led to less reliable data and erroneous decisions. In general, the system was not capable of classifying 1.2 million children in a short period of time.

Table 12: Incidence of Programs with CAS Targeting

Decile	*Family Subsidy*	*Assistance Pensions*	*Housing Subsidies*
1	34.0	35.2	28
2	23.3	15.2	
3	14.5	12.0	22
4	10.1	11.4	
5	7.4	9.6	20
6	4.7	6.8	
7	2.5	4.8	18
8	1.9	2.0	
9	0.9	1.7	12
10	0.7	1.3	

IV. Design Issues in Using Proxy Means Tests

In considering whether and how a proxy means testing system similar to that developed in this paper should be developed in practice, a number of issues must be considered depending on the characteristics of each country and program in question. This section discusses some general points, prefaced by two alternative scenarios to illustrate how proxy means tests can be applied to a wide range of different programs.

Contrasting Scenarios

The first hypothetical scenario involves hospital fee waivers. Either when patients are admitted or at some time during their stay in hospital, the hospital's social worker or cashier would fill out a form that collects information for the proxy means test. The social worker would then input the data into a user friendly "means test" computer program provided by the central planning agency responsible for devising the proxy system. The computer program's output would provide the answer as to whether the individual is eligible for a fee waiver or not. The hospital then charges for its services accordingly.

The second hypothetical scenario would apply to a series of broad-based welfare programs. Social workers would be sent out to interview all households in poor areas. During their visit, they would verify whether the members of the household are giving accurate answers to as many of the questions as possible. The social workers would use a simple form that would allow them to determine eligibility by simple arithmetical calculations. The social workers would then explain to the eligible households that they qualify for one or more programs, and direct them to the relevant welfare offices to enroll.

These two scenarios differ in several important aspects — whether information is verified, whether the formula is secret or publicly known, whether it is simple or complicated, whether it is the same for several programs or different, whether the population is captive, as in the case of hospital patients seeking fee waivers, or whether social workers must seek out candidates.

Factors to Consider

Program Type. Whether the proxy means test is appropriate is determined not so much by the nature of the benefit to be transferred as by certain aspects of the program goals, structure and costs. The proxy means test could be used for a variety of social programs. Food stamps, hospital fee waivers, university scholarships, general welfare programs, housing subsidies, school feeding or food commodity distribution schemes are all programs that could be targeted using the proxy means test.

Cost/Benefit. Perhaps the overriding factor in whether to use a proxy means test is how much it would cost relative to how much it would save. The cost, of course, depends on how the system is set up (see below for a qualitative discussion) and on local price levels. How much the system would save depends on how large are the benefits per beneficiary of the programs to be targeted. For programs that provide low benefit levels, the gain from reducing leakage is much less than for programs with high benefit levels. If the same evaluation process is used to determine entry into several programs as in Chile, then there are obvious economies of scale. Let us run through some back-of-the-envelope calculations to get an idea of the possible ranges of costs and benefits.

In Chile, the administration of the CAS-II system costs about US$5 per household. The system has some sophisticated elements, such as the outreach campaigns, that are costly. It also has some cost savings elements that other countries might not use, such as the fact that it contracts out the data entry to specialist firms who are able to realize economies of scale. We will, therefore, consider three cost levels of $2.50, $5 and $10 per candidate.

A further important dimension of costs is the proportion of the population that is to be assessed. The extreme case is that all households would be assessed. This is quite unlikely in practice, though, if every child were offered a school lunch or a textbook with a sliding payment scale for reimbursement, it might begin to approach this. The more likely scenario is that only a fraction of all households would ever apply for a program because the rich would know they were unlikely to qualify or because, for others, the time or stigma associated with applying is too great relative to the benefit. We thus consider three percentages of the households that might have to be evaluated — 100, 50 and 25.

We will consider low-, medium- and high-cost programs of $10, $50 and $100 worth of benefits per year. The low value might represent the cost of free textbooks or preventive primary health care of minimal standard. The middle value is approximately the value of a typical school lunch program. The higher value might represent a cash transfer program. Very much higher benefits per candidate are possible in housing and university scholarship programs, say $1,000 per year.

Finally, we need to know by how much leakage would be reduced by the targeting program. We assume two initial leakage rates — the 70 percent level might represent a universal benefit, while the 50 percent level would represent some program that is already progressive due to the nature of the good or how it is provided — and several plausible final leakage rates.

Table 13 combines these different costs and benefits. For example, if the proxy means test could be expected to reduce leakage to 50 percent when it would be 70 percent in an untargeted program, and each beneficiary receives a benefit worth $50 per year, the savings implied would be $10 million. If half of all households (100,000 in this example) were tested, and the cost per proxy means test were $5 per household, the cost of the system would be $0.5 million.

For a wide range of costs and saving scenarios, the savings could outweigh the costs of a proxy means testing system of this kind. For the programs with high benefits, even the most expensive kind of means testing would save money. For the programs with the lowest benefits,

Table 13: An Estimated Calculation of Costs and Benefits

	PROGRAM SAVINGS		
	PROGRAM BENEFITS PER YEAR		
Reduction in Leakage	*$10*	*$50*	*$100*
from 70% to 50%	$2 million	$10 million	$20 million
from 70% to 30%	$4 million	$20 million	$40 million
from 50% to 30%	$2 million	$10 million	$20 million
from 50% to 20%	$3 million	$15 million	$30 million
	TARGETING COSTS		
	COST PER CANDIDATE ASSESSMENT		
% of Households Assessed	*$2.50*	*$5.00*	*$10.00*
100%	$.50 million	$1.0 million	$2 million
50%	$.25 million	$ .5 million	$1 million
25%	$.125 million	$ .25 million	$ .5 million

Note: Population is assumed to be 1 million in 200,000 households.

the targeting costs would have to be minimal before the targeting system would be justified. The benefit/cost ratio ranges from 320 ($40 million in program savings/$125,000 in means testing costs) to 1 ($2 million in program savings/$2 million in means testing costs). These calculations are all notional and do not take into account the details of any specific program or the way it is proposed that the targeting system will be administered, but they do demonstrate that in some cases a proxy means testing system could be useful.

Information Collection — Household or Office. The information can be collected on a visit to the household, in the place where the service is delivered or at a welfare office. A household visit makes it possible to verify the location and housing quality variables and, partially, the family variable and the durable goods ownership variable. Household visits are, however, time consuming and involve transport costs. Collecting the information in an office reduces the time and travel costs of the social workers. It can also speed up the application process considerably by allowing the social worker to enroll the individual directly at the time of the interview. However, the costs savings of collecting information in this way should be weighed against the likely degree of misreporting and the costs of leakage.

An intermediate solution might be to collect the information in the office, and to make household visits and verifications for a sample of beneficiaries. Households that had given inaccurate information would then have to pay some sort of penalty. To encourage households to report information accurately, the probability of being caught would have to be high and the penalty severe, while the public awareness of both of these facts would have to be widespread, enough so that households would take them seriously. This voluntary compliance prompted by the individual's fear of getting caught, is similar to the principle on which the US income tax system works. Few programs in the Latin America and Caribbean region have developed a credible penalty system so this option may be more useful in theory than in practice for now.

Another factor to consider is whether or not to let households know of the proposed visits in advance. Announced visits should ensure that the respondent is home, which should lower administrative costs by reducing the need for repeat visits. Surprise visits, of course, may lead to a more truthful representation of the circumstances of the household as they will have been unable to make any arrangements to alter their living conditions. In practice, prior announcements are difficult when the poor receive no mail service and have no access to a telephone and must rely on information passed on at civic gatherings or through third parties. On the other hand, the arrival of the social worker in the village or neighborhood is likely to be noticed by all the local people, so that only the first household visited that day would be surprised.

Who Collects Information. Many programs that could be targeted using a proxy means test are piggy-backed onto other existing programs in order to save administrative costs. For example, nurses or teachers may be given the responsibility for determining the eligibility of applicants for food supplements, school lunches, free textbooks, or cash transfers. This is done for two reasons. First, the extra work involved is judged to be no more than a small addition to their existing duties. Second, the add-on program does not have to budget for the salaries of the staff who determine eligibility, which amounts to a considerable saving. The nurse or teacher may also be assumed to know the local population well, thus minimizing the likelihood that program candidates would be able to get away with falsely reporting information.

Although the advantages of this piggy-backing can be great, there are limitations. For example, there can only be savings in staff costs if the nurses or teachers in question were not already working at full capacity. If their time was fully occupied by their existing jobs then they would have to stop doing something else that was important when the demands of the new program are added to their job descriptions. Thus, if the nurses or teachers are unable to do less of their original jobs, there is a loss even if it is not measured. Also, there is a risk that if the patient or parent is displeased with the nurse or teacher's decision as to eligibility for the program, then the working relationship between nurse and patient or between teacher and parent may be damaged in a way that can harm the fundamental role of the nurse or teacher. For example, if the patient believes the nurse is not concerned with his welfare, the patient might then disregard what the nurse tells him or her about health education, follow-up visits or directions for medications.

Passive versus Active Candidate Identification. The proxy means test can be used for programs in which candidates are expected to come of their own accord and ask to be enrolled. Hospital fee waiver programs are a good example of this kind of program. All the people to be evaluated are present in a few hospitals and are easily identified. Alternatively, likely candidates for a particular program can be identified by means of an outreach effort and they can then be encouraged to apply for the program, as in Chile.

Approval Rates. If the vast majority of candidates who apply are judged to be eligible, then there is little benefit to be derived from screening candidates.

Secrecy of the Algorithm. The algorithm for calculating eligibility or priority for the social program can be secret or public. If the algorithm is both simple and publicly known, the form the social worker fills out for each candidate household can assign weights to each possible answer, and the score can be calculated by the social worker on the spot. If the social worker can classify the household immediately, s/he can immediately enroll the applicant in the program or counsel him/her on how to enroll in the program or claim the benefit. This can reduce the administrative costs of the program, the transaction cost to the participant and the risk that eligible candidates will not be served. Also, the community can judge whether it is appropriate for candidates to apply or not. This could help to discourage those people who are unlikely to be eligible from applying, thereby saving the cost of having to evaluate their applications. It may also help to encourage eligible applicants to apply. Finally, a publicly-known algorithm may help the public to judge whether a program is being administered fairly or not. The community becomes a monitoring agent, thus preventing the program from getting an unjustifiably bad press.

The disadvantage of using a publicly-known algorithm is that it becomes easy for an applicant to know how to change his behavior or to lie about it in order to qualify for the benefit, or to bribe the social worker to report his situation incorrectly. If this happens often, then the accuracy of targeting will be impaired, possibly to the point it is no longer worth using the proxy means test at all.

Complexity of the Algorithm. The algorithm can be more or less complex depending on the number of variables, the techniques used for deriving and weighing them and whether the algorithm is differentiated by region. In general, more complex algorithms will be more accurate, but will have higher computational costs and may preclude some ways of decentralizing responsibility for the program.

Local or Central Processing. Depending on the secrecy and complexity of the algorithm, and the availability of computers, either the individual's eligibility can be determined at the time the information is collected or it may be necessary to send the information to a central office for processing.

Processing eligibility information locally reduces time lags between the collection of information and the determination of eligibility. It may also be consistent with the general management of the program if much of the program administration is decentralized. Good training and supervision from the central level will be needed to ensure that local processing is carried out to uniformly high standards. If computers are available for the use of local program managers, then processing may not be difficult. Again, a good example is the case of hospital fee waivers in hospitals that already have a computerized information system. If computers are not available at the local level, the eligibility formula will have to be simple and publicly-known, in order for local processing to be feasible. This is more likely to be the case in situations where social workers visit households, although, with the increasing availability of cheap laptop computers, direct data entry and evaluation during the site visit may soon be feasible.

Processing eligibility information in a central office, on the other hand, can ensure uniform quality and the availability of adequate computers and of appropriately-trained personnel. It can also take advantage of economies of scale in information processing. In the political economy of targeting, central processing shifts the responsibility for the decision on whether a candidate is eligible for a program from the individual who collects the information to an impersonal, unknown bureaucracy. This is good if it safeguards the reputation of the individual responsible for collecting the information and his ability to work with the candidate in the future. However, it would be undesirable if this makes the program seem arbitrary or unfair, or gives the impression that administrators at the central level are making decisions on eligibility based on some criteria unrelated to need.

Supervision. There should be some supervision built into the administration of the proxy means test. Supervisors would provide guidance on the correct way to apply the test, and help workers to deal with problems. They would also serve as auditors for detecting abuse of the system. Theoretically, an optimal level of supervision exists that depends on how much it costs to carry out compared with how much it saves by improving targeting. In practice, the error rate found in cases reviewed will serve as the guideline for whether supervision is adequate in the following way. Assume that a supervisor can review 75 applications in a month. The average program benefit is $10 per month. The supervisor's salary is $75 per month. If the supervisor finds that in 10 percent or more of the cases reviewed the program beneficiary was ineligible, then the supervisor's salary has been saved. The calculation is simplistic because it does not value the gain to the program of identifying Type I errors as well as Type II errors, nor does it assess the general improvement in the reputation of the program.

In practice, program planners may opt for less than optimal supervision because it is a cost that leads to better quality service (which is hard to measure), rather than to increased coverage or service delivery (which is easier to measure).

Developing a good proxy means testing system probably takes several years rather than several months. In Chile, designing and implementing CAS-I and CAS-II took about three years in each case, and it is too early to say whether a further evolution might take place later. The design and testing of the algorithms is likely to take several months in places where a useful household survey data set already exists, and longer if a special survey is required. Then administrative planning, writing the operational manuals and training staff takes several months. Finally, it is wise to make the first period of implementation short and/or slow so that difficulties can be ironed out before the next phase of implementation begins. It might, for example, be sensible to start with a small number of beneficiaries (for example, taking only one region or only one small program such as university scholarships) rather than more (in larger programs like national school feeding or supplemental foods for all children under the age of five).

What It Really Takes. The logistics of how much staff time, the amount of training required for staff at different levels of the system, the number of computers and the transport and communications links required will vary greatly depending on what is decided about how the system should be set up.

There will be a need for a small central core of economists and computer programmers to devise and evaluate a proxy means testing system regardless of how the system is set up. They would probably be located in a planning office, either in the Ministry of Planning or in the relevant sectoral ministry in situations where only one social program is to be targeted. Beyond this, the requirements vary greatly. The central office could do all data entry and eligibility determination or this could be done at the municipality, clinic or school level. The system could be applied to a few thousand applicants for university scholarships, government housing or expensive hospital care, or it could be applied to tens of thousands of cash transfer program applicants.

We can, therefore, only generalize that the more widespread applications of the proxy means test will be more feasible when the civil service staff required to run the program are well-educated and have low turn-over and some are computer literate. The feasibility will also be affected by the existence or absence of reliable electrical and telephone services. Where staff are minimally literate, change every few months and have few support services, it is likely to be possible to employ a proxy means test to target a large number of applicants, though simple versions might still be useful in specific programs with high levels of benefits.

ANNEX I. ALGORITHMS COMPARED

There are several ways that the same information can be used to determine which household characteristics to use in targeting social programs to the poor. The different ways of combining the information are called algorithms. This annex will explore the advantages and disadvantages of four different algorithms.

Single Indicator Targeting

In single indicator targeting, the mean welfare level of two different population sub-groups are compared. All members of the poorer sub-group are then assigned to receive the program benefits.

The algorithm is very easy to use, because only information on the mean welfare of two sub-groups is needed. The method is useful in situations where only such limited information exists. It cannot, however, make use of more complete information even when it may be available. If, for example, the distribution of poverty within each of the sub-groups is known, it cannot be used in this algorithm. Nor can it take account of information on more than one characteristic.

Ravallion and Chao's Poverty Minimization Algorithm

Ravallion and Chao (1989) have made publicly available an algorithm for helping policymakers to design poverty transfer programs. The algorithm minimizes poverty as characterized by the Foster-Greer-Thorbecke measure, subject to a budget constraint, by calculating the optimal transfer to mutually exclusive population sub-groups. The computer program requires the input of information on the income distribution within each subgroup (if only discrete points on the distribution function of income are available, the program interpolates between them). The output is the optimal transfer for each sub-group. The program can use any number of sub-groups, any poverty line and any budget. The user may constrain the optimal transfers so that all are positive (everyone gets some transfer) or so that some are negative (implying that some groups are taxed), and the largest negative value is permitted for different groups.

The program is designed to be applied to policy problems where the policymaker may have information about the correlation between poverty and a limited number of household attributes such as location of residence, sector of employment or landholding class from small household surveys but does not have such information for each potential transfer candidate. The algorithm is designed to solve a problem very akin to that addressed in this paper — "What transfer do you give to whom to minimize poverty?" It is an important new tool in the empirical welfare literature.

While Ravallion and Chao's algorithm is the solution to the appropriate theoretical problem, the computer program currently available has certain drawbacks when used for the work in this paper. First, this paper is concerned more with identifying of who should a receive a transfer that will be of uniform size to all recipients, so the question posed is slightly different. It is possible within the computer program to constrain certain sub-groups to receive allocations of equal size, but this requires setting *a priori* which groups will benefit and which will not. Thus, it is necessary to run an unconstrained program first to determine which groups would get what benefit without the constraint, then to make a somewhat *ad hoc* decision as to which groups to include in the constraint of equal benefits, and finally to rerun the newly constrained program. Second, the information to be collected in our hypothetical social worker interview can include a large number of variables, some of which may have continuous values. The continuous variables have to be grouped into dichotomous variables to use the computer program, in the process of which information is lost. Furthermore, with many permutations of variables and their values, the number of discrete groups becomes very large. This increases computation time and results in very finely differing levels of optimal transfer, which again is a diversion from the problem of determining who should benefit in a uniform program. Third, this paper is aimed at the policymaker and program analyst with meager mathematical background and limited computer skills. This audience is unlikely to be able to obtain, use or absorb the Ravallion and Chao method without extensive training.

Ordinary Least Squares

Ordinary Least Squares (OLS) regressions minimize the squared errors between the "true" and predicted levels of welfare. The minimization of least squared errors is a different theoretical problem than that of the minimization of poverty. For example, OLS stresses minimizing the errors between "true" and predicted welfare at the top of the welfare distribution, which is not of interest in poverty work. OLS techniques are, therefore, not strictly appropriate to the problem posed in this paper. They are, however, simple, fast and familiar. They also make efficient use of the continuous variables.

Glewwe's Application of Non-Linear Mathematical Programming Algorithms

Recognizing the shortcomings of both OLS and Ravallion and Chao's algorithms, Glewwe (1990) goes one step further. He solves a poverty minimization problem similar to Ravallion and Chao's. He, however, allows for an unlimited number of variables, and for continuous variables. The transfers selected are customized for each individual. He formulates the problem as a non-linear mathematical programming exercise and uses grid search algorithms to solve the problem. Again, this technique solves the right theoretical problem and overcomes the limits on variable class and number in Ravallion and Chao's algorithm. The difficulty in the computer programming is, however, magnified and Glewwe has not transformed his routines into a tool kit available to the public.

The Four Methods Compared

Of these four methods, we chose to use OLS for three reasons: (i) its computational simplicity; (ii) its capacity which allows the use of large numbers of variables and continuous variables; and (iii) the fact that it makes it possible for uniform transfers to be easily investigated. It is, nonetheless, useful to know how much better our results would have been if we had chosen to use another algorithm. Three sets of comparisons shed light on the issue. The results are shown in Table AI.1.

First, with the Jamaican data used in Chapter II of the paper we used Ravallion and Chao and OLS using only the rural/urban variable as our information set. OLS failed utterly in this case because the average welfare level in rural areas was above the cut-off point. The method thus identified no-one as being eligible for a transfer. Ravallion and Chao, however, produced undercoverage of 21 percent, and leakage of 57 percent. Poverty was reduced from 16 percent for $\alpha=0$ to 35 percent for $\alpha=2$ with a transfer budget of J$1 million. It should be noted here that single indicator targeting produced the identical outcome to Ravallion and Chao.

Second, we uses the information in Model V and computed results for Ravallion and Chao and OLS. Model V is the abbreviated model with only five variables, one of which is continuous. Thus, to apply Ravallion and Chao, we constructed a dichotomous variable from the continuous variable. There were 60 possible permutations of the five variables. In fact, only 15 permutations had enough observations to be useful for our purposes (we used 10 at minimum). We further simplified the use of Ravallion and Chao by allowing transfers of different levels to different groups, which we did not allow in the case of OLS. This customizing should have allowed Ravallion and Chao to reduce poverty by more than OLS. We did two permutations of the OLS technique — one with the continuous variable, and one with it made dichotomous as for the Ravallion and Chao test.

When comparing Ravallion and Chao and OLS with dichotomous variables — the directly parallel cases — Ravallion and Chao did perform substantially better in terms of undercoverage, but slightly worse on leakage. The impact on poverty was similar for $\alpha=0$, but was substantial for $\alpha=2$. It is noteworthy that, with the continuous variable formulation, OLS actually compared well. OLS produced undercoverage of 48 percent compared with 42 percent for Ravallion and Chao. OLS produced leakage of 36 percent compared with 40 percent for Ravallion and Chao. The changes in poverty levels were nearly identical. Indeed, the results from OLS are slightly better. Even though it was minimizing least squared error rather than poverty and was used here with uniform rather than customized transfers, the ability of OLS to deal with the continuous family size variable was very important in its performance.

The conclusion we drew is that, with a large number of variables or continuous variables, OLS performs quite satisfactorily when compared to Ravallion and Chao.

We drew on Glewwe and Kanaan (1989) and Glewwe (1990) for a third comparison. The papers used the same data set, poverty line and transfer budget. Both customized the transfer for each individual. The first used OLS, the second non-linear mathematical programming. As can be seen by comparing Table AI.2, the results are very difficult to distinguish. Again, we have concluded that OLS performs satisfactorily.

Table AI.1: Comparisons of Poverty Minimization and OLS Methods

Information: Rural/Urban Residence

	Model I		*Model V*		
Targeting Accuracy	Ravallion and Chao	OLS	Ravallion and Chao	OLS	OLS (dummys)
Those who are eligible and do receive benefits	23.9	0.0	17.3	15.8	12.7
Those who are not eligible and do not receive benefits	38.4	69.5	58.4	61.0	62.7
Type I error	6.2	30.5	12.7	14.3	17.3
Type II error	31.5	0.0	11.6	8.9	7.3
Undercoverage	20.5	100.0	42.3	47.7	57.6
Leakage	56.9	0.0	40.1	36.1	36.5
Program cost	55.4	0.0	28.9	24.7	20.0
Welfare Outcome					
$\alpha = 0$	.257		.285	.284	.290
	(- 4.24)		(- 4.20)	(- 5.24)	(- 4.53)
$\alpha = 1$	.082		.089	.088	.922
	(-11.21)		(-16.83)	(-17.80)	(-13.72)
$\alpha = 2$	.035		.037	.036	.038
	(-16.50)		(-30.00)	(-32.12)	(-23.79)

Table AI.2: Comparison of Non-Linear Mathematical Programming and OLS Methods

	FGT Poverty Index		
	$\alpha=1$	$\alpha=2$	$\alpha=3$
Initial Poverty Level	*0.0757*	*0.0296*	*0.0142*
Glewwe (OLS)			
T = 20,000,000			
Untargeted	0.0688	0.0260	0.0121
	(-9.1%)	(-12.2%)	(-14.8%)
Imperfect Targeting:			
Model 1	0.0617	0.0219	0.0095
	(-18.5%)	(-26.0%)	(-32.8%)
Model 2	0.0606	0.0215	0.0094
	(-19.9%)	(-27.5%)	(-33.8%)
Model 3	0.0621	.0223	0.0088
	(-17.9%)	(-24.8%)	(-37.8%)
Model 4	0.0581	0.0206	0.0088
	(-23.3%)	(-30.6)	(-37.8%)
Model 5	0.0574	0.0196	0.0080
	(-24.2%)	(-33.7%)	(-43.2%)
Perfect Targeting	0.0508	0.0107	0.0024
	(-32.9%)	(-63.8%)	(-83.2%)
Glewwe (Non-Linear Mathematical Programming Algorithm)			
T = 20,000,000			
Untargeted	0.0688	0.0260	0.0121
	(-9.1%)	(-12.2%)	(-14.8%)
Imperfect Targeting:			
Model 1	0.0612	0.0216	0.0091
	(-19.2%)	(-27.0%)	(-35.9%)
Model 2	0.0607	0.0213	0.0088
	(-19.8%)	(-28.0)	(-38.0%)
Model 3	0.0586	0.0201	0.0084
	(-22.6%)	(-32.1%)	(-40.8%)
Model 4	0.0583	0.0193	0.0079
	(-23.0%)	(-34.8%)	(-44.4%)
Perfect Targeting	0.0508	0.0107	0.0024
	(-32.9%)	(-63.8%)	(-83.2%)

Notes:
1. Poverty line = 148,690 CFAF/capita per year.
2. Figures in parentheses show percent reduction in poverty, expressed as a negative number given various targeting methods.
3. 10 million CFA Francs over the sample used is equivalent to 7.83 billion CFA Francs (about $20 million) for all Côte d'Ivoire.

Source: Glewwe (1990), pp.23 and 43.

Table AII.1: Full Regression Results for Jamaica, Reported in Text Table 3

VARIABLE	Model I Adj. R^2=.11	Model II Adj. R^2=.21	Model III Adj. R^2=.36	Model IV Adj. R^2=.41	Model V Adj. R^2=.34
AREA	-.28 (-15.50)	-.10 (-4.89)	-.13 (-7.15)	-.09 (-5.12)	-.14 (-8.61)
PARISH	.13 (6.99)	.09 (5.19)	.10 (6.03)	.10 (6.26)	
WALLS		.05 (2.94)	.07 (4.58)	.05 (3.45)	
HOUSE TYPE		.08 (4.13)		.03 (1.51)	
TOILET		.18 (8.94)	.13 (7.32)	.09 (4.80)	.16 (8.93)
ELECTRICITY		.15 (8.07)	.74 (10.08)	.09 (4.54)	.19 (11.45)
WATER		-.06 (-3.14)		-.02 (-1.22)	
KITCHEN		-.03 (-1.45)	-.07 (-4.52)	-0.4 (-2.45)	
SIZE			-.32 (-17.94)	-.33 (-19.11)	-.35 (-24.03)
SEX OF THE HH			-.11 (-7.19)	-.10 (-6.86)	
AGE OF THE HH				-.03 (-1.77)	
TYPE SCHOOLING			.11 (5.59)	.09 (4.57)	
GRADE COMPLETED			-.02 (-1.09)	-.03 (-1.77)	
CHILDREN <5			-.06 (-3.73)	-.05 (-3.18)	
EMPLOYMENT			-.05 (-1.76)	-.03 (-1.15)	
TYPE OF EMPLOYMENT			.10 (3.66)	.07 (2.73)	
OWNERSHIP OF HOUSE				-.03 (-1.90)	
TELEPHONE				-.14 (-9.37)	-.17 (-11.34)
ELECTRIC STOVE				.05 (3.52)	
GAS STOVE				.05 (2.41)	
REFRIGERATOR				.08 (3.74)	
FAN				.06 (3.39)	
STEREO				-.04 (2.94)	
TELEVISION				-.02 (-1.00)	
MOTORBIKE				.014 (1.00)	
AIR CONDITIONER				.06 (4.26)	
RADIO/CASSETTE				.03 (2.244)	

Table AII.2: Full Regression Results for Rural Jamaica, Summarized in Text Table 7

VARIABLE	*Model I* Adj. R^2 = .04	*Model II* Adj. R^2 = .17	*Model III* Adj. R^2 = .32	*Model IV* Adj. R^2 = .40	*Model V* Adj. R^2 = .30
PARISH	.19 (8.28)	.14 (6.31)	.15 (7.34)	.15 (7.44)	.17 (8.45)
WALLS		.04 (1.56)	.06 (2.61)	.05 (2.45)	
HOUSE TYPE		.10 (4.44)	.04 (2.11)	.06 (2.74)	
TOILET		.22 (8.94)	.18 (8.16)	.13 (6.15)	.28 (13.73)
ELECTRICITY		.16 (6.65)	.19 (8.44)	.06 (2.13)	
WATER		-.07 (-3.28)			
KITCHEN			-0.07 (-3.04)	-0.5 (-2.20)	
SIZE			-.33 (-1.34)	-.34 (-14.67)	-.33 (-16.43)
SEX OF THE HH			-.11 (-4.74)	-.10 (-4.65)	-.11 (-5.37)
AGE OF THE HH			-.029 (-1.17)	-.04 (-1.59)	
TYPE SCHOOLING			-.080 (3.41)	.06 (2.89)	
CHILDREN < 5			.05 (-2.12)	-.04 (-1.92)	
EMPLOYMENT			-.06 (-1.78)	-.04 (-1.41)	
TYPE OF EMPLOYMENT			.09 (3.10)	.07 (2.37)	
TELEPHONE				-.07 (-3.76)	
ELECTRIC STOVE				.17 (8.58)	.21 (10.23)
GAS STOVE				.03 (1.04)	
REFRIGERATOR				.12 (4.32)	
FAN				.08 (3.78)	
RADIO				.06 (2.95)	
STEREO				.02 (1.08)	
AIR CONDITIONER				.08 (4.32)	

Table AII.3: Full Regression Results for Urban Jamaica, Summarized in Text Tables 7 and 10

VARIABLE	Model I Adj. R^2 = .01	Model II Adj. R^2 = .10	Model III Adj. R^2 = .30	Model IV Adj. R^2 = .36	Model V Adj. R^2 = .31
AREA	-.10	.03		-.38	
	(-3.77)	(1.13)		(-13.79)	
PARISH	.05	.04	.04	.03	
	(1.84)	(1.46)	(1.58)	(1.56)	
WALLS		.09	.10	.08	.11
		(3.38)	(4.11)	(3.21)	(4.74)
HOUSE TYPE		.03			
		(1.39)			
TOILET		.12	.07	.05	
		(3.89)	(2.66)	(2.06)	
ELECTRICITY		.18	.17	.11	.21
		(6.11)	(6.48)	(3.89)	(9.13)
WATER		-.04	-.04		
		(-1.45)	(-1.74)		
KITCHEN			-.08	-.03	
			(-3.49)	(-1.10)	
SIZE			-.35		-.38
			(-12.44)		(-16.81)
SEX OF THE HH			-.13	-.11	-.15
			(-5.51)	(-4.87)	(-6.81)
AGE OF THE HH				-.05	
				(-1.75)	
TYPE SCHOOLING			.10	.10	
			(4.44)	(2.95)	
GRADE COMPLETED				-.05	
				(-1.48)	
CHILDREN < 5			-.08	-.06	
			(-3.09)	(-2.43)	
EMPLOYMENT			.060	.05	
			(2.50)	(2.00)	
OWNERSHIP OF HOUSE				-.03	
				(-1.36)	
TELEPHONE				-.17	-.21
				(-7.08)	(-8.98)
ELECTRIC STOVE				-.03	
				(-1.22)	
GAS STOVE				.07	
				(2.36)	
REFRIGERATOR				.05	
				(1.51)	
FAN				.04	
				(1.44)	
STEREO				.06	
				(2.39)	
BICYCLE				.03	
				(1.19)	
MOTORBIKE				.04	
				(1.71)	
AIR CONDITIONER				.05	
				(2.38)	

Table AII.4: Full Regression Results for Poor Jamaica, Summarized in Text Table 9

VARIABLE	Model I Adj. R^2 = .07	Model II Adj. R^2 = .13	Model III Adj. R^2 = .22	Model IV Adj. R^2 = .28	Model V Adj. R^2 = .22
AREA	-.21 (-7.54)	-.14 (-4.45)	-.19 (-6.32)	-.18 (-6.29)	-.18 (-6.84)
PARISH	.11 (3.86)	.10 (3.52)	.15 (5.25)	.14 (5.21)	.13 (4.76)
WALLS		.04 (1.34)	.06 (2.19)	.05 (1.87)	
TOILET		.06 (2.30)	.05 (1.84)	.03 (1.26)	
ELECTRICITY		.21 (7.54)	.26 (9.62)	.11 (3.40)	.26 (10.02)
WATER		-.05 (-1.72)			
KITCHEN		-.03 (-1.22)	-.08 (-3.08)	-.04 (-1.40)	
SIZE			-.27 (-8.85)	-.34 (-11.21)	-.30 (-11.59)
SEX OF HH			-.03 (-1.15)		
AGE OF THE HH			-.08 (-2.60)	-.09 (-2.98)	
TYPE SCHOOLING			-.08 (-2.25)	-.09 (-2.72)	
GRADE COMPLETED			.07 (1.92)	.05 (1.47)	
CHILDREN < 5			-.06 (-2.03)	-.05 (-1.74)	
OWNERSHIP OF: HOUSE				-.04 (-1.36)	
GAS STOVE				.10 (3.21)	
REFRIGERATOR				.09 (2.80)	
FAN				.03 (1.03)	
RADIO/CASSETTE				.116 (4.54)	.15 (5.88)
TELEVISION				.05 (1.57)	
BICYCLE				.08 (3.04)	
SEWING MACHINE				.06 (2.20)	

Table AII.5: Full Regression Results for Urban Bolivia, Summarized in Text Table 10

VARIABLE	*Model I* Adj. R^2 = .04	*Model II* Adj. R^2 = .16	*Model III* Adj. R^2 = .27	*Model IV* Adj. R^2 = .35	*Model V* Adj. R^2 = .29
CITY	.09 (6.87)	.04 (3.47)			
DEPARTMENT	.17 (12.61)	.12 (9.69)	.11 (9.05)	.10 (9.05)	.14 (12.34)
ZONE	.06 (4.24)	.05 (4.22)	.04 (3.83)		
FLOOR		-.09 (-6.54)	-.07 (-5.21)		
WALLS		.05 (4.06)			
HOUSE TYPE			.05 (3.94)		
DRAINAGE		.16 (-11.63)	.09 (7.20)	.04 (3.28)	
WATER		.10 (9.10)	.08 (6.81)	.05 (4.80)	
NUMBER OF ROOMS		.15 (11.21)	.16 (11.61)	.09 (6.97)	
SIZE			-.31 (-25.80)	-.32 (-22.44)	-.32 (-27.48)
AGE OF HH			.13 (9.43)	.09 (7.16)	.11 (9.10)
LANGUAGE			.10 (8.05)		
GRADE COMPLETED			.12 (9.43)	.05 (4.51)	
OWNERSHIP OF: HOUSE				-.15 (-11.04)	
URBAN LAND				-.04 (-3.29)	
TELEPHONE				-.15 (-11.04)	-.27 (-21.40)
STOVE				-.07 (-6.37)	
REFRIGERATOR				-.07 (-5.06)	
TELEVISION				-.05 (3.75)	
CAR				.08 (-7.37)	
SPECIAL CAR				-.14 (-11.32)	-.20 (-16.58)

Table AII.6: Full Regression Results for Urban Peru, Summarized in Text Table 10

VARIABLE	Model I Adj. R^2 = .01	Model II Adj. R^2 = .09	Model III Adj. R^2 = .23	Model IV Adj. R^2 = .31	Model V Adj. R^2 = .30
AREA	.03 (1.07)	.03 (1.11)	.07 (2.69)	.06 (2.21)	
ZONE	-.05 (-1.73)				
SEGMENT	-.14 (-4.34)		-.05 (-1.34)		
WALLS		-.09 (-2.64)	-.09 (-2.53)	-.06 (-2.13)	
ROOF		.11 (2.80)	.10 (2.63)		
FLOOR		.24 (7.15)	.17 (5.31)	.07 (2.23)	
ELECTRICITY		.04 (1.30)	.03 (1.03)		
SEWAGE		-.03 (-1.00)			
KITCHEN		.04 (1.27)			
SIZE			-.35 (-13.25)	-.35 (-14.00)	-.34 (-13.81)
SEX OF THE HH			-.06 (-2.17)	-.03 (-1.34)	
AGE OF THE HH			.06 (1.84)		
TYPE SCHOOLING			.14 (4.67)	.08 (2.98)	.08 (3.08)
OWNERSHIP OF: TELEPHONE				.11 (3.58)	.15 (4.93)
REFRIGERATOR				.05 (1.60)	
RADIO				.06 (2.46)	
TELEVISION				.18 (5.92)	.21 (7.27)
CAR				.12 (4.35)	.14 (5.02)

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