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Focused Transfer Targetin against Poverty Evidence from Tunisia

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Focused Transfer Targeting against Poverty
Evidence from Tunisia

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Abstract

This paper introduces a new methodology to target direct transfers against poverty. Our method is based on estimation methods that *focus on the poor*. Using data from Tunisia, we estimate 'focused' transfer schemes that highly improve anti-poverty targeting performances. Post-transfer poverty can be substantially reduced with the new estimation method. In terms of P_2, the most popular axiomatically valid poverty indicator, a 30 percent reduction in poverty from transfer schemes based on OLS method to focused transfer schemes, requires only a few hours of computer work based on methods available on popular statistical packages. Finally, the obtained levels of under-coverage of the poor is so low that reforms based on 'proxy-means' focused transfer schemes are likely to avoid social unrest.

Key Words: Poverty; Targeting; Transfers. *JEL classification*: D12; D63; H53; I32; I38.

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1. Introduction

1.1. The issue

Transfer schemes are among the main policy tools to alleviate poverty. Cash transfers are the provision of assistance in cash to the poor or to those who face a risk of falling into poverty. The schemes (also called ‘proxy means tests’) are based on predictions of household living standards used to calculate the transfers. Such predictions are obtained by using household living standard survey data for regressing the living standard variable on household characteristics easy to observe.

Many countries have been using proxy means testing to target transfers, particularly in (1) Latin America and the Caribbean, such as Chile for many years under the *Ficha CAS* system, Columbia under *SISBEN*, Mexico under the *Oportunidades* Program, Nicaragua, Jamaica, etc; and (2) Asia, such as India, Indonesia, China, Thailand and Philippines. In these countries, many theoretical and practical issues related to proxy means testing have been studied. The performance of the estimated transfer schemes is very variable (Coady, Grosh and Hoddinot, 2004). Raising their impact on poverty is of paramount importance as stressed in de Janvry and Sadoulet (2006b). However, the statistical foundations of these programs have not received the attention that it deserves. We fill this gap in this paper.

In this paper, we propose an estimation method of anti-poverty transfer schemes that focus on the poor and the near poor, thereby dramatically improving the scheme performance. We apply our new method to Tunisia. Our aim is to improve anti-poverty schemes and our methodological procedure is a part of the answer on which we concentrate in this paper.

1.2. What is targeting?

Although living standards are measured with household surveys, they are generally badly known for the households that are not surveyed. Many authors have studied or discussed assistance to poor people based on targeting when some characteristics of individuals can be observed, but not income.[3] Recently, Coady, Grosh and Hoddinot (2004) review 122 targeted antipoverty programs in 48 countries. Cash transfers based on proxy means tests are generally found to provide the best results, although there is an enormous variation in targeting performances. They also find that targeting performance is better in rich countries and where governments are accountable. Lindert, Skoufias and Shapiro (2005) measure the redistributive power of 56 transfer programs in 8 countries. They find that public transfers can be an efficient way of redistributing income, but often fail to do so. Moreover, the coverage of the poor is found far from 100 percent for the studied cash transfer programs. Some transfer programs are conditional on pre-specified behavior by beneficiaries (e.g., child school attendance or child vaccination). We do not deal with these programs in this paper. The interested reader can consult de Janvry, Finan, Sadoulet and Vakis (2006) and de Janvry and Sadoulet (2006a, b) for comparisons of conditional and unconditional cash transfers.

In Latin America and Caribbean countries at least, there is little evidence of labor disincentives from public transfers. Ravallion (2005) argues that the tradeoffs equity/efficiency and insurance/efficiency restraining the scope for attacking poverty using transfers have been much exaggerated, and may not even be binding because of market failures. So, concentrating on simple optimization programs omitting these trade-offs and incentive problems makes sense.

Ravallion and Chao (1989) model the targeting problem as one of minimizing some specific poverty measures subject to a given anti-poverty budget by using geographical

[3] For instance, see Ravallion (1991), Besley and Coate (1992), Glewwe (1992), Besley and Kanbur (1993), Datt and Ravallion (1994), Slesnick (1996), Chakravarty and Mukherjee (1998), Ahmed and Bouis (2002), Coady et al. (2002), Schady (2002), Tabor (2002a,b), Coady, Grosh and Hoddinot (2004), Coady and Skoufias (2004), Skoufias and Coady (2004), Datt and Joliffe (2005), Lindert et al. (2005), Africa Focus (2006), DFID (2006), Gassman and Notten (2006), Weiss (2005).

groups of individuals. Additional correlates of household living standards can also be used (Glewwe, 1992).

1.3. Implementation difficulties in targeting

What are the issues arising when designing and implementing targeting methods? In this paper we deal with design issues, while implementation issues can be as important in practice. Let us briefly mention some of these.

Beyond the targeting difficulties, which we shall discuss later, the administration of cash transfer programs can be complex. Eligibility is often very general, and not precisely defined. One permanent unresolved concern is how to distinguish the permanently poor population from the transient poor who may need different type of support. Even when the entitlement conditions are accurate and clear, eligibility lists are often infrequently updated. In particular, many newly poor families may not reached by the transfer schemes for a long time, or be left on waiting list for a long time. Updating the eligibility lists is also costly and subject to political and social bias (as in Park, Wang and Wu, 2002).

The temporal lag shortcomings do not stop here. The living standard predictions are based on household living standard survey data that is not collected every years in most LDCs. Using living standard prediction equation, even based on a short list of correlates easy to observed, implies that the estimated parameters are reasonably stable over time. However, in practice quality of prediction may degrade over relatively short periods of time (3 to 5 years). In that situation, the efficiency gap between different types of proxy-mean cash transfers, or with price subsidies, would decline, blurred by data quality issues. Also, the risk of social unrest due to incorrect targeting would be heightened. This situation suggests that frequent household living standard surveys should be conducted when social policy much relies on proxy-means tests. These issues, as these related to measurement errors, should not be seen as a reason to abandon any use of statistical techniques altogether, but rather as indicating possible directions of progress in surveys.

Although costs are likely to be much reduced when using proxy-means cash transfer as compared to price subsidies, the financial constraints remain important, and ultimately depend on the political support obtained by the cash transfer policy. When financial constraints are too stringent, needy families may generally be kept below the subsistence thresholds, even after transfers.

The costs should include not only the total amount of monetary transfers to implement, but also administrative costs that may be non-negligible.

In the literature, most measured administrative costs of transfer schemes range from less than 5 percent to about 15 percent of the targeting budget (See Grosh and Baker, 1995, Alderman and Lindert, 1998, Coady, Grosh and Hoddinot, 2002). Therefore, the conclusions of our study are unlikely to be offset by administrative costs only.[4] The fact that there already exists in Tunisia small systems of direct transfers to: the elderly, the handicapped, schoolchildren, and needy families, suggest that administrative implementation on a larger scale is doable.

Moreover, overlap between different assistance programs may make their management difficult and redundant. All this could be dealt with imposing the administrative implementation of these programs. Notably, relying on decentralized administrations may be more efficient, as was found in Bangladesh (Galasso and Ravallion, 2005), although more ambiguous results are found to West Bengal by Bardhan and Mookherjee (2006).

Another issue is that some households may change some of their characteristics by which they are targeted or hide their true characteristics in an attempt to receive a larger

[4] Besley (1990) discusses the theoretical consequences of such costs and other costs of means testing. Other types of costs would come from the demeaning nature of transfers, as had been observed in the US with food stamps. However, monetary transfers, such as pensions are generally not considered demeaning, and the poor in Tunisia are generally needier than most of the poor in the US, and thus may not afford to be excessively proud.

transfer. Though, it is unlikely that the net benefit of such strategies will be non-negative for many characteristics, like location and dwelling types. In our results, the characteristics that can reasonably be modified or hidden by households (education and occupation variables) are precisely the ones that do not add much to the performance of the scheme.

1.4. Targeting performance

Returning to design issues, two indicators, leakage and coverage, are popular for measuring targeting performance.

With imperfect targeting, only poor people who are predicted as poor can benefit from poverty alleviation. On the other hand, non-poor people predicted as non-poor or with their predicted living standard beyond the chosen threshold consistent with the program budget, receive transfers. Thus, two types of errors characterize imperfect targeting. The *Type I error* (*undercoverage*), central in Ravallion (1991), is that of failing to reach some members of the targeted group. As Atkinson (1995) noted, this failure generates horizontal inefficiency when compared with perfect targeting. The *Type II error* arises where benefits are awarded to ineligible people under perfect targeting. The *leakage* of program benefits is a monetary assessment of this error, obtained by adding (1) the transfers given to those whose pre-transfer income is above the poverty line, and (2) the transfers received by pre-transfer poor that are unnecessary because the post-transfer living standards are raised above the poverty line.[5] The *leakage ratio* is obtained by dividing the leakage with the available budget. A final measure of the program efficiency is the reduction in poverty measures due to the transfer scheme.[6]

[5] Grosh and Baker (1995) and Cornia and Stewart (1995) do not consider the second component of the leakage cost. Creedy (1996) distinguishes between vertical expenditure inefficiency, equal to the leakage ratio as estimated by Grosh and Baker (1995) and by Cornia and Stewart (1995), and poverty reduction efficiency equal to our leakage ratio.

[6] Other measures of transfer efficiency have been proposed, while we concentrate on the main indicators related to our concerns, in part to avoid drowning the reader under figures for a paper which already contains a lot of them. Bibi and Duclos (2006) propose indicators of horizontal inequity, Coady, Grosh and Hoddinott (2004) and Lindert, Skoufias and Shapiro (2005) propose to use the Distribution Characteristic Indicator, which shows the change in social welfare marginal benefit achieved by transferring a standardized budget to the program, and the

1.5. Living standard predictions

In practice, anti-poverty targeting or poverty simulations can be based on predictions of household living standards, generally obtained from ordinary least squares (OLS) regressions on observed characteristics.

For example, using data from the 1997 Egypt household survey Datt and Joliffe (2005) estimate models of household consumption in a first stage, and exploit the estimates to simulate poverty rates obtained from changes in policy variables by assuming lognormality of consumption to predict expected consumption and expected individual poverty. They find that improving education, parental background, land redistribution, and access to health facilities lead to poverty alleviation. In contrast, we do not rely on lognormality assumption, often rejected by the data. Instead, we use predictions of conditional quantiles of living standards that do not depend on parametric distribution assumptions on errors, as quantile regression estimators are nonparametric in that sense.

However, the OLS method is centered on the mean of the dependent variable (e.g., household living standard) and should provide accurate predictions around this mean mostly, which is often far from the poverty line. Then, the predicted living standards of the poor and near poor may be inaccurate. This explains why significant undercoverage of the poor is common (as in Grosh and Baker, 1995). This is the case when the mechanisms explaining the living standards of the non-poor differ from those of the poor. The latter is expected because poor households differ from other households not only by their capital and skills, but also by their access to social networks and credit possibilities, and by their economic activities.

Undercoverage and poverty indicators are not the only possible performance indicators of anti-poverty welfare programs. The 'distributional characteristics', which measure the gain

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Coady-Grosh-Hoddinott index, which allows the comparison of the actual performance to the outcome that would result from neutral targeting. Many inequality, concentration and progressivity indices could also be used.

in some social welfare function of a marginal increase in the transfer budget is another indicator, although mostly useful for small transfer changes. Coady and Skoufias (2004) decompose the distributional characteristics in a component capturing the target efficiency of the transfer program and another one describing its redistributive efficiency. Their simulations based on Mexican data show that understanding transfer performance implies to use various performance indicators.

In this situation, using OLS predictions may be sub-optimal. In this paper, we use estimation methods that 'focus' on the poor. This allows us to improve the predictions of the living standards of the poor and near poor. The method we propose can also be adapted to any social program based on 'household assessment', that is: predictions of household characteristics (as in Case and Deaton, 1996, or Hanmer, Bijlmakers, Basset, Sanders and Chapman, 1998). Thus, health policies directed to ill persons, education policies directed towards underperforming students, pensions to the elderly, and any policy associated to specific intervals for a social variable that is imperfectly observed could benefit from focused targeting.

Various estimation methods are possible for this purpose. For example, a semi-non-parametric estimation of the income distribution could be implemented by using kernel estimation methods in which correlates are parametrically incorporated (e.g., Pudney, 1999). Even full non-parametric estimation of conditional distributions of living standards could be adapted to the problem at hand. However, nonparametric methods suffer from slow consistency, inaccurately estimate the distribution tail, and are subject to the 'multidimensional curse' requiring unavailable large information because of many correlates included in proxy means tests. Moreover, analysts operating in statistical institutes in LDCs favor simpler estimation methods. Accordingly, Deaton (1997) emphasizes methods that can be actually implemented in the relevant institutions.

For these reasons we investigate two simple restrictions of the predictive regressions: (i) censoring the dependent variable to eliminate the influence of observations located far from the poverty line; (ii) using quantile regressions. The knowledge of the quantile regressions centered on all observed quantiles is equivalent to the knowledge of the empirical conditional distribution. Of course, there are too many quantiles to consider for a practical procedure, while good results may be obtained by just trying one quantile around the poverty line. Then, *focusing on the poor* means that the predictions are calculated by defining the quantile regression or the censorship threshold in terms of living standard levels judged representative of the poor or the near poor.

Assume that the equation used to predict living standards has the form $y_i = X_i b + u_i$, where y_i is the living standard of household i, X_i are exogenous correlates of living standard for household i, u_i is an error term, b is a vector of parameter to estimate. OLS estimates corresponds to imposing the restriction $E(y_i /X) = Xb$, which implies $E(u_i /X) = 0$. Quantile regression estimates centered in quantile θ correspond instead to the restriction $q_\theta(y_i /X) = Xb$, where function q_θ denotes the conditional quantile function of order θ, conditional on the value of the variables X. This restriction implies $q_\theta(u_i /X) = 0$. That is: the quantile on which a quantile regression is centered relates to error quantiles and not to the initial living standard measure.

Then, what we predict is a chosen quantile of the distribution of the living standards *conditionally on the correlates*. This method has two shortcomings. Firstly, if the error terms are approximately normal, some efficiency may be lost as compared with OLS. Secondly, the focus is conditional on the set of correlates. That is, the chosen quantile is not that of the dependent variable, but the quantile of the error term in the estimated equation. However, that is precisely the quantile of the error that may matter most if one is interested in the prediction error that affects the transfer scheme performance.

Quantile regression, centered on the poverty line should improve targeting, as compared to OLS, precisely because they are centered on the distribution location that identifies the poor, i.e. the poverty line threshold. Indeed, typically in regression methods, the prediction error is minimal at the central tendency used to define the regression method (mean for OLS regression, median for Least-Absolute Deviation regression, a given quantile for quantile regression), while it increases quadratically with the distance of the data from the chosen central tendency. As the living standards of the poor are usually quite different from the mean living standard in a population, OLS prediction errors are large for the living standards of the poor. In contrast, if the chosen quantile is close to the poverty line, the quadratic increase in prediction error does not occur for quantile regressions centered near the poverty line.

Another important issue is that OLS estimates for anti-poverty schemes are sensitive to the presence of outliers, to the non-normality of error terms with finite sample size, to heteroscedasticity and other misspecifications. Quantile regressions deal with these concerns for robustness (Koenker and Bassett, 1978), crucial in poverty analysis because of measurement errors in consumption surveys and the non-robustness of many poverty measures (Cowell and Victoria-Feser, 1996). Nonetheless, using quantile regressions deals with non-normal errors and error outliers but not with other measurement errors. Censored quantile regressions have been found useful to obtain robust explanations of chronic and seasonal-transient poverty (Muller, 2002).

As mentioned above, a better focus of the scheme can also be obtained by eliminating part of the income distribution (the richest households for example) from the prediction. This suggests using Tobit regressions and censored quantile regressions instead of respectively OLS and quantile regressions.

Another interest of focused targeting is that it is logically related to the theoretically optimal transfer schemes with the transfers concentrated towards the poorest of the poor, the richest of the poor, or both (Bourguignon and Fields, 1997). From this theoretical perspective what need to be determined are the transfers to these sub-populations. Then, focused predictions of the living standards of the poor and near poor may generate more efficient transfers.

1.6. Comparison with Elbers, Lanjouw and Lanjouw

Another field where living standard regression predictions are obtained in a first stage are used in a second stage for poverty simulation is the small area literature. Thus, other attempts to improve the focus on the poor could be based on combining census data and household survey data, although Bigman and Srinivasan (2002) and Schady (2002) found that the improvements in targeting in India and Peru are small. More recently, Elbers, Lanjouw and Lanjouw (2003) provide encouraging results for poverty estimation. We do not deal in details with this approach in this paper as it raises additional and specific difficulties.

However, we now spell out a few differences in the ELL techniques and our focusing approach. Although transfer programs based on proxy-means tests rely on observable household characteristics, in contrast with ELL they use neither census data, nor the information on the precise location of households. Should they use such information? Perhaps, but they are reasons to doubt it. First, information on many household characteristics from census data is infamously known as being generally of mediocre quality. This justifies basing many analyses on specific survey data including this information rather than on the exhaustive census data. Highly contaminated census data look like a poor basis for establishing such a sensitive policy as income transfers. Second, using accurate location for designing transfer scheme may lead to short-distance or long-distance migrations from households attempting to capture the transfers. Third, in the household living standard survey used to estimate the predicted incomes, only very few local areas are observed, which constitutes a poor basis to introduce information on precise household location in the living standard prediction procedure. Four, since we do not use census data, most statistics estimated at cluster level would have very large sampling standard errors, which is not the case for ELL.

On the whole, the non-use of census data and cluster dummies constitutes a major difference of our approach with that of ELL. This justifies that we do not attempt to estimate an error component model at cluster level. In these conditions we need not use simulation techniques for our estimation procedure. All our estimates are grounded on explicit calculations instead. Another simplification is that because we use quantile regressions, a method that is nonparametric with respect to errors, we need not impose arbitrary distribution assumptions. Also, without error decomposition, we need not impose ELL orthogonality identification restrictions between the different components and the income correlates. This is important because not all analysts agree on the validity of these restrictions.

Note that we deal with model error and sampling error in different ways than ELL. Namely, they are interested in the decomposition of the global estimation error of poverty measures into these components (and a simulation error component which does not exist in our case). In contrast, we are interested in model error in that it determines the level of transfers provided to each household type, but not for the estimation of the accuracy of poverty estimators or transfer performance estimators. For the later stage, what we used is only the sampling standard error of these estimators as the transfer schemes to compare are considered as given policies.

Finally, a complication arising in our case, but not in ELL, is the presence of a stage of transfer calculation from the sample of predicted living standards. This stage would much complicate using ELL small area framework because the calculation of the transfer levels make intervene the whole sample of living standard predictions and not only the observable variables restricted to each small sample area.

In footnote 5 of ELL, the authors claim that using quantile regressions give results non-significantly different from using OLS. As quantile regressions of income or living standard dependent variables have routinely been found to significantly vary across the quantiles used to center the regressions, we presume that they found this result using least-absolute deviations (ie., median quantile) estimators or other arbitrary quantile. There is no mention of selection a given quantile to *focus* the quantile regression in their paper.

Is it possible to improve anti-poverty targeting by using living standard predictors that focus on the poor or near poor? The aim of the paper is to explore this question. However, our intention is not to propose a detailed reform of the anti-poverty policy in Tunisia, nor to deal with all the practical implementation difficulties of such policy. Section 2 presents the anti-poverty transfer schemes. In Section 3, we apply our new method to the 1990 Tunisian household survey. In Section 4, we discuss program efficiency results. We find that: (1) focused targeting would reduce poverty much more than targeting based on OLS, and (2) undercoverage of the poor can be massively reduced. Finally, Section 5 concludes this paper.

2. Anti-Poverty Cash Transfers

This paper is based on the following popular poverty measures of the FGT class (Foster et al., 1984) because of their attractive axiomatic properties: $\mathrm{P}_\alpha(z,Y)=\int_0^z\left(\frac{z-y}{z}\right)^\alpha \mathrm{f}(y)\,\mathrm{d}y$, where z is a pre-specified poverty line, $f(.)$ is the c.d.f. of household income y (or household living standard) and α is a poverty aversion parameter.[7] Naturally, our approach could be extended to other poverty measures. Given an anti-poverty budget, one must design transfers that optimally allocate this budget across households.

Let us first consider the situation when Y (the vector of incomes in a population before applying the vector of transfers $T=\{t^i,\ i=1,\dots,N\}$ is perfectly observed. In that case, the optimal transfer allocation is the solution to:

$$\underset{\{t^i\}}{\text{Min}}\ \mathrm{P}_\alpha(z,Y+T)\equiv\frac{1}{N}\sum_{i=1}^{N}\left(\frac{z-(y^i+t^i)}{z}\right)^\alpha\ I_{[y^i+t^i<z]}$$
$$\text{subject to}$$
$$\sum_{i=1}^{N}t^i=B,\qquad \text{with}\ \ t^i\geq 0,\forall\, i,$$

where N is the population size, B is the budget to allocate, t^i is the non-negative cash transfer to household i and y^i is pre-transfer income. The objective function can be weighed by the household size (or some equivalent-scale) in each household to deal with poverty at the individual level rather than the household level. However, for expositional simplicity, we neglect for the moment the possibility that households may include several members. We do

[7] The $\mathrm{P}_\alpha(.)$ is the head-count ratio if $\alpha = 0$, the poverty gap index if $\alpha = 1$, and the poverty severity index if $\alpha = 2$. The FGT poverty measures satisfy the transfer axiom if and only if $\alpha > 1$, and the transfer sensitivity axiom if and only if $\alpha > 2$. All these measures satisfy the focus axiom and are decomposable.

not consider how the budget B is funded. When Y is perfectly observable, the solution to this problem is referred to as 'perfect targeting' and denoted t^i for household i.

Bourguignon and Fields (1990, 1997) show that perfect targeting minimizing the headcount ratio would start awarding transfers so as to lift the richest of the poor out of poverty:

$$t^i = z - y^i \text{ if } y^i < z,\ t^i = 0 \text{ otherwise}$$

(in a decreasing order of income until all the budget is exhausted, 'r-type transfer'). In contrast, if the aim is to minimize a FGT poverty measure satisfying the transfer axiom ($\alpha > 1$), it is optimal to start allocating the anti-poverty budget to the poorest of the poor ('p-type transfer'). In that case, the transfer scheme would be:

$$t^i = y_{max} - y^i \text{ if } y^i < y_{max};\ t^i = 0 \text{ otherwise,}$$

where $y_{\max}$ is the highest cut-off income allowed by the budget. As the anti-poverty budget rises, $y_{\max}$ increases up to the poverty line, z, and perfect targeting would permit to lift all the poor out of poverty.

Unfortunately, perfect targeting is not feasible because incomes cannot be perfectly observed. Nevertheless, since the household living standards are correlated with some observable characteristics, it is possible, as in Glewwe (1992), to minimize an expected poverty measure subject to the available budget for transfers and conditioning on these characteristics. In practice, the approach followed in the literature or by practitioners for designing the transfer scheme is to replace unobserved living standards by predictions based on observed variables.

Let us first recall the standard procedure used in the literature for such predictions. Several empirical articles on anti-poverty targeting have appeared in the literature.[8] They generally follow a two-step procedure. First, the expectation of y^i conditional on x^i (the vector

[8] Glewwe and Kanaan (1989), Glewwe (1992), Grosh and Baker (1995), Ravallion and Datt (1995), Bigman and Srinivasan (2002), Park et al. (2002), Schady (2002), Tabor (2002a,b).

of living standard correlates for household *i*) is parametrically estimated by *OLS*. Then, if the budget allows it, each predicted poor household receives the difference between its predicted income and the poverty line. Other dependent variables, or even composite measures of welfare such as principal components extracted from multivariate analysis could be used in such regressions, sometimes with a change in the meaning of the objective function. Our method can be easily adapted to these cases.

Some variables could be easily modified by the households, raising moral hazard problems. We deal with this issue by avoiding as much as possible endogenous regressors, and by considering alternative sets of correlates, defined by their increasing presumed sensitivity to moral hazard.

What matters for anti-poverty targeting is the ability to identify the poor and predict their living standards. Our strategy is to focus on the poor and the near poor when predicting living standards. Grosh and Baker (1995) improve targeting accuracy when using only the poorest 50 percent of the population. However, we prefer to keep the information on the proposition of the non-poor. Indeed, econometric models based on censored variables are likely to yield more efficient results than those based on sample truncation since they do not throw away valuable information about the identification of the poor and of the non-poor.

In this situation, if the error term in the latent equation of this model is normal, living standard predictions can be obtained by using a Tobit model, conditional upon some household characteristics. However, several issues may cause Tobit estimates to be inconsistent. First, the normality assumption on which the Tobit model is based is often rejected even for logarithm of living standards. Second, heteroscedasticity is likely to arise from household heterogeneity. Finally, the threshold y_{max} may be unknown. We deal with these difficulties by also using censored quantile regressions that are little sensitive to them.

We now turn to the estimation results. We start by presenting the data used for the estimations.

3. Data and Methodology

3.1. The data

In Tunisia, targeting transfers to poor people has become increasingly urgent because structural adjustment programs have imposed cuts in food subsidies, traditionally the main way to fight poverty. This is all the more so that the leakage from food subsidies to non-poor people is considerable, while failure to substantially serve all in the target group is common. The Tunisian Universal Food Subsidies Program (TUFSP) is the main policy for alleviating poverty in Tunisia. Since 1970, basic foodstuffs have been under subsidy to protect the purchasing power and the nutritional status of the poor. Even if beneficial to the poor, this program was inefficient and costly. Indeed, about 2.9 percent of GDP was spent in subsidies by 1990 (still slightly less than two percent nowadays). Furthermore, the richer households received much more from the program than the poor. Improvement of this subsidy program has been limited by preference patterns, income inequality and the size of individual subsidies (Alderman and Lindert, 1998). In such situation, transfer schemes might alleviate poverty at a lower budgetary cost, provided that the method used to design the scheme performs well, as argued by Alderman and Lindert. This is consistent with one of the key challenges identified in Tunisia by the World Bank to meet the goals of the 10th Economic Development Plan: to strengthen the performance of social programs while maintaining budget balances (The World Bank, 2004). Meanwhile, maintaining social stability through a better safety net is still a major challenge in Tunisia (Hassan, 2006). A former substitution of food subsidies with direct cash transfers to the poor ended in riots in the 1980s because the proposed transfer system was perceived as leaving aside a large proportion of the poor. Other issues about social welfare, inequality and horizontal inequity could be raised about such policies in Tunisia (as in Bibi and Duclos, 2006). In this paper we focus on poverty.

We use data from the 1990 Tunisian consumption survey conducted by the INS (National Statistical Institute of Tunisia). Unfortunately, this is the most recent complete national consumption survey data available in Tunisia, where official data dissemination rules are stringent. The survey provides information on expenditures and quantities for food and non-food items for 7734 households. Usual other information from household surveys is available such as the consumption of own production, education, housing, region of residence, demographic information, and economic activities.

Because the estimation of equivalence scales based on cross-section data has often been criticized,[9] and in order to concentrate on the issue of imperfect targeting, we assume that per capita consumption expenditure is an adequate indicator of each household member's welfare. Other equivalence scales have been tried and provide results qualitatively similar.

We define in Table 1 the correlates of living standards used for the predictions. The correlates are grouped according to increasing difficulties of observation by the administration and increasing ease of modification or hiding by households. Set I contains regional dummies. Using it along with OLS corresponds to 'regional targeting' and the regional poverty profile estimated in Muller (2007).[10] Set II includes regional and demographic information on households and characteristics of the household's dwelling. Set III adds information on the occupation and the education of the household's head to that in Set II. The variables in Set II are unlikely to be manipulated by households and could be cheaply observed, yet those added in Set III are easier to conceal. So, Set II is the set to include in the regression analyses based on the need for these to be verifiable by program offices and not easily manipulated by households.

[9] Pollak and Wales (1979), Blundell and Lewbel (1991).

[10] For more information about regional targeting, see Kanbur (1987), Ravallion (1992), Datt and Ravallion (1993), Baker and Grosh (1994), Besley and Kanbur (1988), and Bigman and Fofack (2000).

It has been found that price differences across households may affect poverty measurement (Muller, 2002). In order to correct for this, account for substitution effects caused by the elimination of price subsidies (which is the source of the budget for cash transfers) and control for spatial price dispersion, we estimate the equivalent-gain from food subsidies, Γ. The calculus of Γ is explained in the working paper Muller and Bibi (2006) and is derived from the estimation of a quadratic almost ideal demand system (QAIDS) and based on a modified Blundell-Robin estimator. Both income and poverty line are converted into equivalent-income. Our reference price system is the one without subsidies since the subsidies budget is assumed to be reallocated to cash transfers.

Then, they are four stages of estimation. (1) the estimation of a demand system to infer equivalent-incomes that enter the definition of living standard variable; (2) the prediction of living standards from observed characteristics; (3) the calculus of the optimal transfers corresponding to the predicted living standards, using perfect targeting optimization; (4) the simulation of the welfare effects of the transfer scheme. Let us turn to the living standard predictions.

3.2. Results for living standard predictions

Table 2 shows the descriptive statistics of the main variables used in the estimation. Mean total expenditure per capita is TD 804 (Tunisian Dinars). Tables 3 presents the results of OLS regressions, Tobit regressions (censored at 10%), quantile regressions (anchored on the first decile) and censored quantile regressions (censored at 50% and based on the first decile) of the logarithm of the household consumption per capita, on Sets I, II and III of explanatory variables.[11] The regression predictions are applied to the whole sample, here and throughout the study. Other conventions, for censorships and quantiles lead to results in

[11] Other estimation methods could be used such as Probit models of the probability of being poor, or non-linear specifications for the right-hand-side variables. We tried a variety of such methods. However, to limit the length of the paper, we only show some of the better performing and more relevant estimates.

agreement.[12] We use for the dependent variable the logarithm of the equivalent income (i.e. with living standards corrected with true price indices inferred from the estimated demand system).[13] Alternative results of this paper without adjustment or corrected by Laspeyres price indices are in agreement.

The censored quantile regression estimator for dependent variable y_i and quantile θ is obtained as the solution to the minimisation of

$$1/N \sum_i \rho_\theta[y_i - \max(0, X_i'\gamma)],$$

where $\rho_\theta[u] = \{\theta - I_{[u<0]}\} |u|$, X_i is a matrix of regressors, γ is a vector of parameters, N is the sample size.

Quantile regressions correspond to replacing $\max(0, X_i'\gamma)$ with $X_i'\gamma$. Powell (1986) and Buchinsky and Hahn (1998) analyse these estimators. The estimation is obtained by a combination of a linear programming algorithm and sub-sample selection at each iteration of the optimisation. We estimate the confidence intervals of the censored quantile regression estimates by using the bootstrap method proposed by Hahn (1995) with 1000 bootstrap iterations.

It has been argued that quantile regressions could help poverty analysts by choosing quantiles corresponding to the poor (Buchinsky, 1994). The argument is overstated since the quantile is that of the conditional distribution, i.e. of the error term, and not of the living standard. However, for predicting the living standards of the poor or near poor, since the prediction errors mostly stem from the error terms in the living standard equations, quantile regressions anchored on small quantiles should improve the predictions for these sub-populations. Then, our choice of the quantile in the quantile regressions is motivated by the

[12] The censorship at quantile 50 percent of the censored quantile regression is chosen because of two requirements. First, censored quantile regression estimates are inconsistent if too few observations are present in the uncensored subsample (a condition is needed which is unlikely with a too small sample). Second, excessive censoring leads to disastrous loss of accuracy in the estimation.

[13] To remain close to common practices we did not weigh the estimation by the sampling scheme. However, we checked that using sampling weights in this case yields similar results, in part because the sampling probability at each sampling stage of this survey are almost proportional to population sizes.

focus. This approach corresponds to specifying quantiles close to the poverty line in the living standard regressions.

Let us take a look in Table 4 at the ratios of the variance of the prediction errors over the variance of the logarithm of the living standards.[14] These ratios are measures of the prediction performance of the estimation methods for the mean of the logarithms of living standards. They are provided for three subpopulations: the whole population of households, the households in the first quintile of the living standards, the households in the first and second quintiles. For the OLS, the considered ratio is equal to $1-R^2$.

The results show that quantiles regressions (anchored at quantile 0.1) generally perform much better than the other methods for predicting the logarithms of living standards *of the poor* (here defined as belonging to the first or second decile of the living standard distribution), to the exception of censored quantile regressions that are better for the poor under the first quintile. In contrast, the best method for predicting the mean of the logarithms of living standards in the whole population is the OLS method. Predicting the logarithms of living standards by using Tobit regressions (with censorship at 10 or 30 percent) does not improve on OLS predictions for the whole population in this data set. Moreover, Tobit predictions for the poor remain much inferior to the predictions obtained with quantile regressions, and censored quantile regressions. Finally, the predicting performance of the censored quantile regressions is disappointing for the whole population, and dominated for the poor in the second quintile by that of the quantile regressions. This is worrying since realistic poverty lines in Tunisia lie between the first and second quintile. An additional difficulty with censored quantile regressions is that they rely on estimation algorithms difficult to implement in most national statistical institutes of less developed countries.

[14] The interpretation of the R^2 as a percentage of variation explained is dependent on the use of OLS to compute the fitted values. This is why we use instead the ratio of variances as our prediction performance indicator.

Then, if our business is predicting the logarithms of living standards of the poor or near poor, the quantile regressions look like the most promising method. In contrast, censoring living standards with Tobit models does not provide improved predictions for the poor.

Our approach consists in exploiting the better predictions from quantile regressions for the living standards of the poor to improve the performance of anti-poverty transfers. Note that using such predictions, whatever the estimation method, for directly estimating poverty, would lead to very inaccurate poverty estimates. However, we shall show that using the predictions based on quantile regressions is useful if the aim is to improve transfer schemes. Appropriate assessment will be obtained by estimating the scheme with different methods and examining the results. We now turn to the results of the prediction equations in Table 3, which, as a by-product, provide us with estimates of living standard explanations in Tunisia. The signs of most coefficient estimates (significant at 5 percent level) correspond to the expected effects of variables and are consistent across all estimation methods.

In the next step in the analysis, the predicted household living standards are used to simulate poverty levels resulting from the targeting scheme, first by using poverty curves.

4. Program Efficiency Results

The calculation of the transfer $T_{\alpha}(.)$ in the simulations, according to the Bourguignon and Fields' rule, requires the determination of the cut-off income, $y_{\max}$, beyond which no transfer takes place. The r-type transfer is: $y_{\max}$ minus the predicted income, for each household predicted poor. Under perfect targeting, the $y_{\max}$ permitted by the budget currently devoted to food subsidies is TD 358 (Tunisian Dinars), greater than poverty lines estimated for Tunisia.[15] However, even if the budget is sufficient to eliminate poverty under perfect targeting, under imperfect targeting additional resources are necessary and the budget is exhausted.

We use a poverty line equal to TD 250 to estimate targeting efficiency measures, consistently with the most credible poverty line in The World Bank (1995), corresponding to a head-count index of 14.1 percent. This poverty line corresponds to an *equivalent poverty line of TD 280* without subsidies. However, the qualitative results of this paper go through with poverty lines at reasonable levels, as is illustrated in the poverty curves corresponding to the stochastic dominance analyses shown in the working paper Muller and Bibi (2006).

The better performance of quantile regressions may be attributed to the focus properties of this method. However, an alternative interpretation could be that the robustness of the quantile regressions is what matters in practice. To control for this we run Huber robust regressions. Huber regressions yield almost the same results than OLS whether for the estimated coefficients or for the poverty curves. So, using Huber estimates does not modify the coefficients obtained with OLS-based predictions, and is therefore of no interest to improve the quality of predictions in that case. The better performance of the quantile regressions for anti-poverty targeting schemes is therefore not due to robustness. However, poverty curves provide only qualitative insights. We now turn to quantitative estimates of targeting efficiency.

4.1. Estimates of targeting performance

Table 5 presents simulation results for: (1) two measures of targeting accuracy (leakage and undercoverage), and (2) the levels of poverty reached with the transfer schemes

[15] The poverty line estimated by the National Statistic Institute and the World Bank (1995) – see also Ravallion and van der Walle (1993) - on the basis of needs in food energy corresponds to TD 196, the poverty lines by Ayadi and Matoussi (1999) vary between TD 213 and 262, and the poverty lines by Bibi (2003) vary between TD 227 and 295. Poverty lines calculated by the World Bank for 1995 (The World Bank, 2000) are between TD 252 to TD 344.

and with price subsidies. As mentioned above, a poverty line of TD 280 per capita per year without subsidies is used, consistently with The World Bank (1995). An individual having an income of TD 280 without subsidies has the same welfare level with TD 250 and subsidized prices. We also show qualitatively similar conclusions for two other poverty lines in Appendix 3. To concentrate the discussion on targeting performance, we discuss the poverty results for P_2 only. Results for other poverty indices are in the working paper Muller and Bibi (2006).

We emphasize in our comments the comparison amongst transfer methods. The standard errors suggest that the estimated targeting indicators significantly vary with the prediction methods. This is indeed generally the case when tests of differences are implemented, as found with bootstrap confidence intervals. The results based on regressor Set I, corresponding to regional targeting, show that this typical regional targeting scheme, based on OLS, already improves on food subsidies in terms of poverty remaining after the policy. However, if the aim is to reduce poverty measured by the axiomatically valid poverty severity measure P_2, quantile regressions anchored on the first decile are best. Moreover, leakage and undercoverage are also lower with this method.

However, the picture slightly changes when we extend the set of regressors. With regressor Set II, which adds information on dwelling and demographic characteristics to the information on regional dummies of Set I, substantial improvements can be reached whether in terms of poverty statistics, leakage or undercoverage. Remember also that Set II is our chosen set of correlates for actual program offices. With Set II, the quantile regression based on the first quantile remains the best approach for reducing P_2 and undercoverage. On the other hand, undercoverage is related to probably indispensable political conditions since policies leaving aside a large proportion of the poor are unlikely to be implementable in

Tunisia. Censored quantile regressions allow us even larger reduction of undercoverage, although they are less straightforward to implement.

Using information on educational level or occupation of household head gains little ground. The quantile regressions based on the first decile (and sometimes the censored quantile regressions) remain preferable if the aim is to alleviate P_2, while OLS are better if the aim is to cut the number of the poor down. Using censored quantile regressions anchored on the first decile would lead to the lowest undercoverage. Meanwhile, quantile regressions based on the first decile, which are simpler to implement, still yield low undercoverage of 8.7 percent, a remarkably low result. The other methods may produce disastrous outcomes for undercoverage.

However, if the aim is to reduce leakage, while quantile regressions based on the first decile perform better than OLS, using censored quantile regressions or Tobit regressions may be very slightly preferable. As a matter of fact, no prediction method provides substantial fund savings through leakage reduction. Leakage always remains very high (above 68 percent) whatever the used method.

Omitting price correction or deflating with household Laspeyre price indices gives similar results. On the whole, the quantile regression based on the first decile is best for diminishing P_2 and perhaps undercoverage. Often, the censored quantile regressions anchored on the first decile with a 50 percent censorship dominate the quantile regressions based on the first decile for reducing undercoverage, but they seem unlikely to be used in most applied contexts since this method is not available in standard statistical packages.[16]

Three important points may be noted. First, the gaps between the estimated reductions in P_2 with different prediction methods are considerable. The statistical method used to design

[16] Note that a characteristic of the censored regression method is that it may coincide with quantile regression estimates for low quantile. This comes from the fact that both estimators are derived from solving linear programming problems that may yield the same optimal kink. Such situation occurred several times in our results.

the transfer scheme is a crucial ingredient of the performance of the scheme. When compared with other cash transfer methods, substantial improvement of the poverty situation measured by P_2 can be obtained (from 3.85 percent with the best OLS method to 2.72 percent with the best quantile regression method – centered in the first decile). The percentage of excluded poor households from the scheme dramatically falls (to 8.6 percent) as compared with what is obtained with OLS predictions based on geographical dummies (for which it is 41.6 percent). Second, the usually employed method, based on OLS estimates, appears as the least performing approach compared to ways of focusing the predictions on the poor. However, when considering only the number of the poor, the OLS provide acceptable predictions for the richest of the poor that are not discounted when compared with the poorest. With limited budget, one could push still further the transfer performance by using quantile regressions centered about the poverty line for r-type transfers and centered on small quantiles for p-type transfers, consistently with the theoretical definitions of these transfer types.

The censorship of the richer half of the sample is statistically too crude to make much impact on the performance of anti-poverty schemes through Tobit predictions even if they may slightly improve on OLS. Besides, Tobit regressions yield inconsistent estimates if the error terms in predicting equations are not strictly normal. Getting rid of the normality assumption by using censored quantile regressions generally yields worse results than what can be obtained with quantile regressions, except for undercoverage.

On the whole, using prediction methods focusing on the relevant part of the living standard distribution provides a way to substantially raise transfer efficiency. Quantile regressions are natural to carry out this task, as our results illustrates, since they can be centered on any chosen location of the conditional distribution of living standards. Even better results could be reached by trying a large set of quantiles instead of just using arbitrarily the first and second deciles to center the regressions. However, we did not want to 'force' the results by implementing these extensive tries, akin to data mining. For example it is likely that centering on quantile 0.14, corresponding to the actual percentage of the poor, would be a good way of improve quantile predictions around the poverty line. Systematic search of the centering quantile, although time consuming, could be implemented in any context where a household living standard survey is available in order to optimize the transfer performance.

As shown in Appendix 2, robustness checks based on two other poverty lines yield similar qualitative results. In Muller and Bibi (2006), stochastic dominance tests show that the qualitative results for poverty measures can be extended to a broad range of poverty lines.

4.2. Uniform transfers and graphs of targeting errors

Results shown in Table 5 also indicate the performances of uniform transfer to the poor, respectively based on OLS predictions and (first decile) quantile regression predictions based on the largest set of regressors. The performances are disastrous with OLS-based uniform transfers yielding to worst reached levels of P_2 and Undercoverage. They are better for quantile regression-based uniform transfers, while with mediocre reached level for P_2 (although only slightly less good than with optimal transfer based on OLS). However, the lowest level of Undercoverage can be obtained. This is because all the identified poor received transfers, whereas with optimal transfers some well identified poor are not covered for lack of sufficient funds.

Note that leakage statistics should not include useless transfers that would raise households above the poverty line. If this correction is included in the leakage statistics, then even under uniform transfers, Leakage and Undercoverage are not mirror images.

Figure 1 shows graphs of targeting errors against initial living standard levels for z = TD280, following Coady and Skoufias (2004). On the left of the poverty line, the curves shows the percentage of the initial poor not reached by transfers. On the right of the poverty line, it shows the percentage of the non-poor unduly receiving transfers.

One can see that OLS and quantile regressions essentially differ by their capacity to calculate well transfers for the extremely poor households, while their performances are closer for households around the poverty lines. On the other hand, the OLS would better target non-poor households if it were necessary, These features are apparent whether optimal transfer amounts are calculated when using the predictions (in graphs 1 and 2) or if pro-poor uniform transfers are used (in graphs 3 and 4). Graphs 3 and 4 also correspond on the left of the poverty line to the percentage of ex-post poor for each level of initial living standard. This is because with uniform transfers and the chosen poverty line there is enough budget to lift all the poor who can be identified above the poverty line.

These graphs allow the visual separation of the performances of the pure targeting transfer schemes (graphs 3 and 4) from optimized transfer schemes (graphs 1 and 2). Additional ex-post targeting errors could be caused by adjusting the transfer levels to the predicted living standards. Indeed, with optimized transfers and the available budget, not all households can be served by the transfer scheme. In contrast, with uniform transfers all households identified as poor are served but they receive amounts that are not related to their living standard level.

For uniform transfers, the bulk of targeting errors from OLS are below the poverty line and substantial. They are much less substantial for optimized OLS transfers, for which the errors elicit a smooth peak at the center of the graph. In contrast, decile-regression targeting errors are much smaller at the left of the poverty line, whether for optimized or uniform transfers. Meanwhile, in the right of the poverty line these errors are larger than than from OLS. All these features fit well with the predicted statistics of Table 4. However, decile-regression targeting errors do not differ very much when considering optimized and uniform transfers. This is because with the considered transfer budget and poverty line, only about 3.5 percent of households are simultaneously identified as poor (using quantile regressions based on the larger set of variables) and cannot be served because of budget exhaustion. It appears that the main gain obtained from moving from uniform to optimized transfers, as far as targeting based on decile regressions is concerned, occurs around the tried poverty line in Tunisia. The graphs make clear that the use of quantile regressions is important for better targeting of the poor, whether for uniform or optimized transfers.

4.3. Policy consequences

What are the policy consequences of our new method of focused transfer schemes? Clearly, highly improved performances can be attained by adapting the statistical method used for the prediction of living standards. Lower poverty levels, smaller leakage and undercoverage statistics can be obtained by focusing the estimation of transfer schemes. In Tunisia, the gain of efficiency, notably in terms of undercoverage, is so large that it should deserve serious policy consideration. In terms of P_2, the most popular axiomatically valid poverty indicator, 3.9 percent is the level reached with the best OLS method. An additional reduction down to 2.7 percent, that is another half reduction in poverty, requires only a few hours of simple statistical work easy to do with common package (e.g., Eviews or Stata). Moreover, this reduction is much larger than that obtained by adding education and occupation variables to the list of regressors in OLS regressions.

The econometric results have shown that decisive progress can be reached in the design of the scheme. The choice of the econometric method for predicting living standards is crucial for the performance of the transfer scheme. Adopting an econometric method that focus on the poor improves the efficiency of the transfer scheme. In our data, the method of quantile regression centered on a quantile close to the expected poverty line provides the best results.

There is already a small transfer scheme in operations in Tunisia: the 'Programme des Familles Nécessiteuses' (République Tunisienne, 1991). However, to implement a large transfer program would necessitate raising large funds. A logical consequence of our analysis is to make possible the transfer of some of the public funds allocated to price subsidies towards a national focused transfer scheme.

But growth is not everything. Previous attempts at eliminating subsidies in Tunisia ended in riots. Indeed, since all the poor, and other population categories, benefit from price subsidies, an economically better aid system to the poor based on direct cash transfers may alleviate poverty, but may also leave aside a large proportion of the poor. If this risk is perceived as high by the population, social unrest may follow, especially because the Tunisian society is very aware of social policies. Therefore, replacing subsidies by OLS-based transfers is likely to be impossible. Indeed, our results show that about between one quarter and one fifth of the poor would be excluded from the benefits of such transfers and would simultaneously lose the benefits they extract from subsidies. Another possibility would be to replace food subsidies with targeted food subsidies based on proxy-means programs. However, this seems difficult since it would imply to be able to administrate many expenditure transactions by targeted households.

However, using focused transfers, would allow the government to reduce the undercoverage of the scheme to such a level (at most 8.6 percent of the poor in our estimates, which could still be improved), that: (1) the reform should be politically viable, and (2) the reform would not generate severe risks for a large proportion of the poor. As a matter of fact, it seems exceptional that such a limited proportion of the population would suffer from a large social reform. Moreover, considering the gain in efficiency caused by the elimination of price distortions, and the saving of public funds, the actual percentage of the poor suffering from the reform may even turn out to be negligible.

5. Conclusion

Leakage to the non-poor is often substantial from universal food price subsidy programs directed to the poor. Because of their large budgetary cost, many governments have moved away from them towards better targeted methods, such as proxy-means cash transfer. Indeed, benefits can be awarded to the poor contingently on their characteristics. However, transfer schemes may be inaccurate because the statistical predictions involved in their design are centered on the mean of the living standard distribution and not enough oriented towards the potentially poor.

This paper improves on past methods by focusing on the poor and near poor for the design of transfer schemes based on estimated living standard equations.[17] This is achieved by using quantile regressions and censorship for the prediction of living standards. This is not the object of the paper to delve into detailed practical analysis of the Tunisian anti-poverty policy or to deal with all the implementation difficulties of this policy.

Our estimation results based on data from Tunisia reveal considerable potentialities for poverty alleviation with our new approach. The improvement is also substantial as compared to usual targeting schemes based on OLS predictions: with our method based on quantile regressions poverty could be massively reduced in Tunisia. Moreover, large reduction in undercoverage is possible, even when compared with the best OLS-based transfers. In contrast, censoring the living standard distribution does not improve the performance of transfer schemes, except for reducing undercoverage.

[17] Therefore, not for food subsidies for which distinguishing among households for eligibility of benefits is not feasible.

Targeting by indicators may be relatively cheap to implement, when compared to the huge financial burden of price subsidies. This is notably the case when it can be carried out just after a national census since the variables contributing to the efficacy of the transfer scheme are then easy to observe from a census. Moreover, in such situation the scheme could be improved by using the methods in Elbers, Lanjouw and Lanjouw (2003), taking full advantage of the census information for small area targeting.[18]

Other econometric ways of focusing on the poor are possible, for example by using non-parametric regressions, shadowing the shape of the living standard distribution. It is unclear what the optimal econometric techniques to use to implement this focus concern are and we conjecture that they may depend on the data at hand. On the whole, the important point in our approach is the adaptation of the estimation method for household living standard predictions in order to improve the performance of the anti-poverty targeting scheme. Using quantile regression improves this performance dramatically in the case of Tunisia. However, other variants and improvement are probably possible and left for future work.

[18] It is likely that poverty mapping can be improved by estimating methods focusing on the poor. We leave this question for future work. Finally, the assessment of the welfare impact of public spending (van de Walle, 1998) could be based on focusing statistical approaches.

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Appendix 1: Tables

Table 1: Definition of the variables

Set I: Area	
Great Tunis	1 if household lives in Great Tunis, 0 otherwise.
Northeast	1 if household lives in Region Northeast, 0 otherwise.
Northwest	1 if household lives in Region Northwest, 0 otherwise.
Middle East	1 if household lives in Region Middle east, 0 otherwise.
Middle west	1 if household lives in Region Middle west, 0 otherwise.
Sfax	1 if household lives in Sfax, 0 otherwise.
Southeast	1 if household lives in Region Southeast, 0 otherwise.
Southwest	1 if household lives in Region Southwest, 0 otherwise.
Complement for Set II:	
Demographic information	
Nc2	Number of children in household old less than 2 years old.
Nc3-6	Number of children aged between 3 and 6 years.
Nc7-11	Number of children aged between 7 and 11 years.
Na12-18	Number of adults aged between 12 and 18 years.
Na19p	Number of adults old more than 19 years.
Age	Age of the household head (HH).
Age2	Squared age of the HH.
Type of house	
Nbroompc	Number of rooms per capita
Detached House	1 if household lives in a detached house, 0 otherwise.
Flat	1 if household lives in a flat, 0 otherwise.
Arab house	1 if household lives in an Arab house, 0 otherwise.
Hovel	1 if household lives in a hovel, 0 otherwise.
Accommodation Mode	
Owner	1 if household is owner of the house.
Rent	1 if household is renting a house.
Locvte	1 if household has a leasing agreement for his house
Free	1 if household lives in a free of charge house.

Complement for Set III:	
Occupation of HH	
Unemp	Dummy variable for HH is unemployed.
Agrilab-se	Dummy variable for HH living in the Southeast and agricultural labourer.
Agrilab-sw	Dummy variable for if HH living in the Southwest and agricultural labourer.
Agrilab-an	Dummy variable for if HH living in another region and agricultural labourer.
Nonagrilab	Dummy variable for if HH is an industry worker.
Agrifar	Dummy variable for if HH is a farmer.
Agrifar-nw	Dummy variable for if HH living in the Northwest and agricultural farmer.
Sms	Dummy variable for if HH is self-employed or manager.
Another	Dummy variable for if HH has another type of job.
Nbbud	Number of participants in the household's budget.
Nactiff	Number of female workers.
Nactifm	Number of male workers.
Schooling level of HH	
Illiterate	Dummy variable for HH is illiterate.
Prim	Dummy variable for HH has a primary schooling level.
Sec-J	Dummy variable for HH has a junior secondary schooling level.
Sec-S	Dummy variable for HH has a senior secondary schooling level.
Higher	Dummy variable for HH has a higher educational level.
Nbetud	Number of students.
Nbelspv	Number of children in private secondary school.
Nbelspu	Number of children in public secondary school.
Nbelppv	Number of children in private primary school.
Nbelppu	Number of children in public primary school.

HH = 'household head'. Zone 1 corresponds to Greater Tunis, the most prosperous region and largest industrial center. Zone 5 corresponds to the Middle East (Sousse, Monastir, Mahdia), which is the second economic region of Tunisia. It is reputed for its thriving tourist industry. Since Zones 1 and 5 are omitted, the sign of the coefficients of the other zones should be negative in the prediction equation of living standards. Zone 2 is the Northeast (Nabeul, Bizerte, Zaghouen), which is the third most important economic region of Tunisia. We expect the coefficient of this variable to have the smallest magnitude among the zone coefficients in the prediction equation. Zone 3 corresponds to the Northwest where the highest poverty incidence is. Its coefficient should have the largest magnitude among the zone coefficients. Zone 4 is the Middle West which is also very poor. Zone 6 is the Sfax area, which is economically prosperous as one the main industrial center after Tunis and the Middle East. Zone7 is the Southwest where Tozeur oasis stands as an important producing area of dates. It is also an increasingly prosperous tourism center. Other important towns in this area are Gafsa (with a declining production of phosphates) and Kbelli. Zone 8 is the Southeast, which includes Gabes (relatively wealthy although less than Sfax), Mednine and Tataouine. Its coefficient in the prediction equation should be negative.

As for the housing characteristics, the number of rooms per capita should be correlated with living standards. The omitted category for the housing type is 'villa'. Therefore, the coefficients of the remaining categories should have negative signs, especially for 'arab house' and 'hovel'. Arab's houses are traditional houses that do not satisfy standard requirements of modern houses. Walls may not be straight. Construction materials used for roof, walls and floor are often of poor quality.

The activities of members are likely to matter for living standards. The number of participants in the household budget (nbbud) and the number of male and female active members (respectively actifm, actiff) should be positively correlated with the living standard. The categories for professionals, managers, industrials and traders are omited in the prediction equations. Then, except for the category Agrifar (farmer), the included professional categories should have negative coefficients. The sign of the coefficient for farmer may be ambiguous because the questionnaire does not distinguish small and large producers. Moreover, no information on the cultivated areas or on the agricultural activity is available.

Education variables are often correlated with living standards. We omit the categories corresponding to university or the second cycle of the secondary level (at least 4 years of secondary education beyond the 6 years of primary education) for the education of the household head. The remaining categories are denoted: Illiterate (no education); Prim (6 years of primary education or less); Sec1 (3 years of secondary education or less). The coefficients of these dummy variables should be negative. Nbetud denotes the variable indicating the number of students in the household. Since education is likely to be a normal good, we expect its coefficient to be positively correlated with the household living standard.

Table 2: Descriptive Statistics (7734 observations)

Variables	Mean	Std. Deviation	Minimum	Maximum
Yearly total expenditure	4066	3456	99	54234
Yearly total expend. p.c.	804	809	47	20531
Great Tunis	0.216	0.412	0	1
Northeast	0.138	0.345	0	1
Northwest	0.152	0.359	0	1
Middle East	0.127	0.333	0	1
Middle west	0.134	0.341	0	1
Sfax	0.088	0.283	0	1
Southeast	0.089	0.284	0	1
Southwest	0.055	0.228	0	1
Nc2	0.322	0.565	0	4
Nc3-6	0.612	0.824	0	5
Nc7-11	0.748	0.933	0	5
Na12-18	0.995	1.167	0	7
Na19p	3.001	1.433	0	11
Age	48.27	13.79	16	99
Nbroompc	0.544	0.366	0.05	4.5
Detached House	0.185	0.388	0	1
Flat	0.048	0.214	0	1
Arab house	0.733	0.442	0	1
Hovel	0.033	0.179	0	1
Owner	0.801	0.399	0	1
Rent	0.079	0.269	0	1
Locvte	0.061	0.239	0	1
Free	0.059	0.235	0	1
Unemp	0.014	0.117	0	1
Agrilab-se	0.009	0.096	0	1
Agrilab-sw	0.006	0.077	0	1
Agrilab-an	0.076	0.265	0	1
Nonagrilab	0.309	0.462	0	1
Agrifar	0.137	0.344	0	1
Agrifar-nw	0.031	0.173	0	1
Sms	0.132	0.339	0	1
Another				
Nbbud	0.518	1.116	0	8
Nactiff	0.303	0.621	0	5
Nactim	1.209	0.866	0	7
Illiterate	0.476	0.499	0	1
Prim	0.289	0.453	0	1
Sec-J	0.072	0.258	0	1
Sec-S	0.091	0.287	0	1
Higher	0.041	0.197	0	1
Nbetud	0.045	0.243	0	4
Nbelspv	0.052	0.245	0	3
Nbelspu	0.403	0.789	0	5
Nbelppv	0.006	0.093	0	3
Nbelppu	1.007	1.198	0	7

Table 3: Prediction Equations

Variables	OLS V1	OLS V2	OLS V3	Tobit V1	Tobit V2	Tobit V3	UQ01 V1	UQ01 V2	UQ01 V3	CQ01 V1	CQ01 V2	CQ01 V3
Constant	6.631	6.38	6.567	6.574	6.135	6.363	5.779	5.832	6.000	5.779	5.992	6.04
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Northeast	-0. 197	-0.061	-0.054	-0.245	-0.116	-0.102	-0.243	-0.069	-0.048	-0.243	-0.063	-0.037
	(0.000)	(0.004)	(0.006)	(0.000)	(0.012)	(0.025)	(0.000)	(0.040)	(0.133)	(0.000)	(0.014)	(0.149)
Northwest	-0. 557	-0. 364	-0.314	-0.545	-0.398	-0.340	-0.574	-0.398	-0.333	-0.574	-0.344	-0.288
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mid. west	-0. 496	-0. 223	-0.19	-0.472	-0.272	-0.241	-0.534	-0.287	-0.261	-0.534	-0.294	-0.236
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Sfax	-0. 336	-0. 306	-0.274	-0.337	-0.356	-0.329	-0.390	-0.320	-0.288	-0.390	-0.240	-0.158
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Southeast	-0. 350	-0. 194	-0.151	-0.098	-0.003^{-}	0.048^{-}	-0.223	-0.041	-0.042	-0.223	0.005	0.041
	(0.000)	(0.000)	(0.000)	(0.077)	(0.957)	(0.411)	(0.000)	(0.256)	(0.254)	(0.000)	(0.851)	(0.159)
Southwest	-0. 47	-0. 273	-0.208	-0.381	-0.263	-0.176	-0.420	-0.239	-0.169	-0.420	-0.151	-0.088
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)
Age		0.009	0.009		0.007	0.009		0.011	0.008		0.006	0.003
		(0.002)	(0.003)		(0.259)	(0.116)		(0.027)	(0.143)		(0.099)	(0.479)
Age2		-0.0001	-0.0001		-0.0001	-0.0001		-0.0001	-0.0001		-0.0001	-0.0000
		(0.000)	(0.003)		(0.079)	(0.084)		(0.003)	(0.190)		(0.024)	(0.573)
Nc2		-0.082	-0.084		-0.068	-0.074		-0.101	-0.077		-0.113	-0.075
		(0.000)	(0.000)		(0.001)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Nc3-6		-0.115	-0.122		-0.083	-0.098		-0.104	-0.116		-0.110	-0.120
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Nc7-11		-0.087	-0.122		-0.062	-0.087		-0.092	-0.108		-0.100	-0.118
		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Na12-18		-0.055	-0.116		-0.033	-0.093		-0.056	-0.114		-0.052	-0.114
		(0.000)	(0.000)		(0.003)	(0.000)		(0.000)	(0.000)		(0.000)	(0.000)
Na19p		0.04	-0.050		063	-0.024		0.036	-0.05		0.022	-0.057
		(0.000)	(0.000)		(0.000)	(0.039)		(0.000)	(0.000)		(0.000)	(0.000)

Nbroompc	0.653	0.542	118	0.856	0.526	0.453	0.129	0.133
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Flat	0.103	0.072			0.055	0.107	-0.017	-0.013
	(0.008)	(0.050)			(0.374)	(0.067)	(0.720)	(0.785)
Arab	-0.341	-0.175	-0.339	-0.219	-0.43	-0.243	-0.322	-0.127
house	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
	-0.68	-0.448	-0.665	-0.488	-0.871	-0.581	-0.792	-0.496
Hovel	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	0.021	-0.003	0.036	0.003	-0.027	-0.013	0.015	0.015
Free	(0.426)	(0.903)	(0.453)	(0.955)	(0.544)	(0.754)	(0.659)	(0.661)
	0.154	0.080	0.231	0.130	0.160	0.057	0.086	0.056
Rent	(0.000)	(0.001)	(0.003)	(0.084)	(0.000)	(0.162)	(0.005)	(0.079)
	0.213	0.151	0.247	0.178	0.244	0.189	0.137	0.086
Locvte	(0.000)	(0.000)	(0.003)	(0.028)	(0.000)	(0.000)	(0.000)	(0.009)
		0.027		0.049		0.022		0.015
Nbbud		(0.000)		(0.001)		(0.039)		(0.071)
		0.125		0.049		0.121		0.066
Nactiff		(0.000)		(0.032)		(0.000)		(0.000)
		0.168		0.185		0.176		0.143
Nactim		(0.000)		(0.000)		(0.000)		(0.000)
		-0.342		-0.312		-0.443		-0.433
Unemp		(0.000)		(0.000)		(0.000)		(0.000)
		-0.226		-0.182		-0.209		-0.208
Agrilab-an		(0.000)		(0.000)		(0.000)		(0.000)
		-0.331		-0.321		-0.223		-0.34
Agrilab-		(0.000)		(0.000)		(0.027)		(0.000)
sw		-0.197		-0.197		-0.074		-0.119
		(0.000)		(0.061)		(0.414)		(0.102)
Agrilab-se		-0.121		-0.066		-0.102		-0.051
		(0.000)		(0.045)		(0.000)		(0.011)
Notagrilab		-0.037		0.019		0.016		0.043

			(0.093)			(0.681)			(0.656)			(0.138)
Agrifar			-0.032			-0.128			-0.098			-0.152
			(0.426)			(0.052)			(0.141)			(0.004)
Agrifar-nw			-0.374			-0.413			-0.381			-0.245
			(0.000)			(0.000)			(0.000)			(0.000)
			-0.224			-0.243			-0.203			-0.099
Illiterate			(0.000)			(0.001)			(0.000)			(0.000)
			-0.055			-0.207			-0.049			0.021
Prim			(0.042)			(0.025)			(0.276)			(0.543)
Sec-J			0.111			0.022			0.013			0.032
			(0.000)			(0.783)			(0.782)			(0.391)
			0.158			0.303			0.182			0.157
Nbetud			(0.000)			(0.000)			(0.000)			(0.000)
			0.074			0.113			0.105			0.106
Nbelspv			(0.000)			(0.000)			(0.000)			(0.000)
			0.213			0.051			0.249			0.084
Nbelspu			(0.002)			(0.756)			(0.006)			(0.239)
			0.04			0.023			0.038			0.049
Nbelppv			(0.000)			(0.135)			(0.025)			(0.000)
Nbelppu	7734	7734	7734	7734	7734	7734	7734	7734	7734	7734	7734	7734
Nb. Obs.												

The living standard variable is the equivalent income.
V1 : Version 1 estimation using Set I variables (regional variables).
V2 : Version 2 estimation using Set II variables (Set I + demographic and dwelling variables).
V3 : Version 3 estimation using Set III variables (Set II + occupation and schooling level of household head).
Tobit : Censored (10)
UQ01 : Uncensored quantile (0.1) regression.
CQ01 : Censored (50) quantile (0.1) regression.
P-value in parentheses. 7734 observations.

Table 4: Variance of the Prediction Errors over the Variance of the Logarithms of Living Standards

Whole population

	OLS	Tobit Threshold 10%	Tobit Threshold 30%	Quantile Regressions (Quantile 10%)	Quantile Regressions (Quantile 30%)	Censored Quantile Regressions Threshold 50% (Quantile 10%)
Set I	0.897	0.908	0.900	2.291	1.146	3.251
Set II	0.551	0.635	0.568	1.413	0.693	2.259
Set III	0.473	0.546	0.490	1.223	0.589	1.991

The poor under the first quintile

	OLS	Tobit Threshold 10%	Tobit Threshold 30%	Quantile Regressions (Quantile 10%)	Quantile Regressions (Quantile 30%)	Censored Quantile Regressions Threshold 50% (Quantile 10%)
Set I	0.832	0.806	0.814	0.105	0.410	0.059
Set II	0.420	0.408	0.406	0.080	0.210	0.062
Set III	0.338	0.333	0.326	0.080	0.177	0.066

The poor under the second quintile

	OLS	Tobit Threshold 10%	Tobit Threshold 30%	Quantile Regressions (Quantile 10%)	Quantile Regressions (Quantile 30%)	Censored Quantile Regressions Threshold 50% (Quantile 10%)
Set I	0.845	0.826	0.825	0.120	0.370	0.134
Set II	0.428	0.448	0.423	0.147	0.211	0.158
Set III	0.350	0.373	0.344	0.152	0.185	0.155

Table 5: Measures of Targeting Efficiency for z = TD 280

	P_2 (in %)	Leakage	Undercoverage
OLS 1	**.758**	**84.5**	**41.6**
	(0.10)	(4.34)	(2.88)
OLS 2	**.439**	**72.4**	**21.6**
	(0.05)	(3.67)	(1.58)
OLS 3	**.385**	**72.5**	**18.5**
	(0.04)	(3.60)	(1.37)
TB10 1	**.842**	**73.7**	**41.6**
	(0.09)	(4.43)	(3.24)
TB10 2	**.430**	**69.3**	**24.2**
	(0.04)	(3.98)	(1.67)
TB10 3	**.364**	**68.2**	**21.1**
	(0.03)	(3.88)	(1.51)
TB30 1	**.759**	**85.1**	**41.6**
	(0.10)	(4.51)	(3.24)
TB30 2	**.418**	**70.9**	**21.7**
	(0.04)	(3.98)	(1.67)
TB30 3	**.356**	**71.1**	**17.5**
	(0.03)	(3.63)	(1.34)
QR10 1	**.739**	**75.6**	**13.2**
	(0.08)	(3.41)	(1.97)
QR10 2	**.344**	**70.0**	**10.2**
	(0.04)	(3.11)	(1.00)
QR10 3	**.272**	**69.5**	**8.67**
	(0.03)	(3.07)	(0.91)
QR30 1	**.776**	**78.3**	**33.2**
	(0.09)	(3.88)	(2.88)
QR30 2	**.376**	**70.5**	**15.4**
	(0.04)	(3.31)	(1.32)
QR30 3	**.312**	**73.0**	**13.1**
	(0.03)	(3.35)	(1.16)
QRC01 1	**.739**	**75.6**	**13.2**
	(0.08)	(3.42)	(1.97)
QRC01 2	**.404**	**68.9**	**9.92**
	(0.04)	(3.02)	(0.95)
QRC01 3	**.298**	**70.9**	**6.92**
	(0.03)	(3.09)	(0.76)
Pro-poor Uniform OLS3	**0.977**	**83.8**	**64.4**
Pro-poor Uniform QR10 3	**0.444**	**75.4**	**6.72**

The living standard variable is the equivalent income.
Set I of independent variables includes only regional variables. Set II includes in addition to Set I, demographic and dwelling variables. Set III includes in addition to Set II, occupation and schooling level of household head.
OLS 1: Transfers based on OLS 1 : Set I variables.
OLS 2: Transfers based on OLS 2 : Set II variables.
OLS 3: Transfers based on OLS 3 : Set III variables.
TB10 1: Transfers based on Tobit censured at 10 percent with Set I variables.
TB10 2: Transfers based on Tobit censured at 10 percent with Set II variables.
TB10 3: Transfers based on Tobit censured at 10 percent with Set III variables.
TB30 1: Transfers based on Tobit censured at 30 percent with Set I variables.
TB30 2: Transfers based on Tobit censured at 30 percent with Set II variables.
TB30 3: Transfers based on Tobit censured at 30 percent with Set 3 variables.
QR10 1: Transfers based on quantile regressions centered on quantile 0.1 with Set I variables.
QR10 2: Transfers based on quantile regressions centered on quantile 0.1 with Set II variables.
QR10 3: Transfers based on quantile regressions centered on quantile 0.1 with Set III variables.
QR30 1: Transfers based on quantile regressions centered on quantile 0.3 with Set 1 variables.
QR30 2: Transfers based on quantile regressions centered on quantile 0.3with Set II variables.
QR30 3: Transfers based on quantile regressions centered on quantile 0.3with Set I variables.
QRC01 1: Transfers based on censored quantile regressions centered on quantile 0.3, censored at quantile 0.5, with Set I variables.
QRC01 2: Transfers based on censored quantile regressions centered on quantile 0.3, censored at quantile 0.5, with Set II variables.
QRC01 3: Transfers based on censored quantile regressions centered on quantile 0.3, censored at quantile 0.5, with Set III variables.
Pro-Poor Uniform OLS3: Uniform transfers based on OLS 3 : Set III variables.
Pro-Poor Uniform QR10 3: Uniform transfers based on quantile regressions centered on quantile 0.1 with Set III variables.

Each of measures presented in this table has been multiplied by 100 for easy interpretation.
7734 observations. Sampling errors in parentheses.

Appendix 2: Robustness checks with two other poverty lines

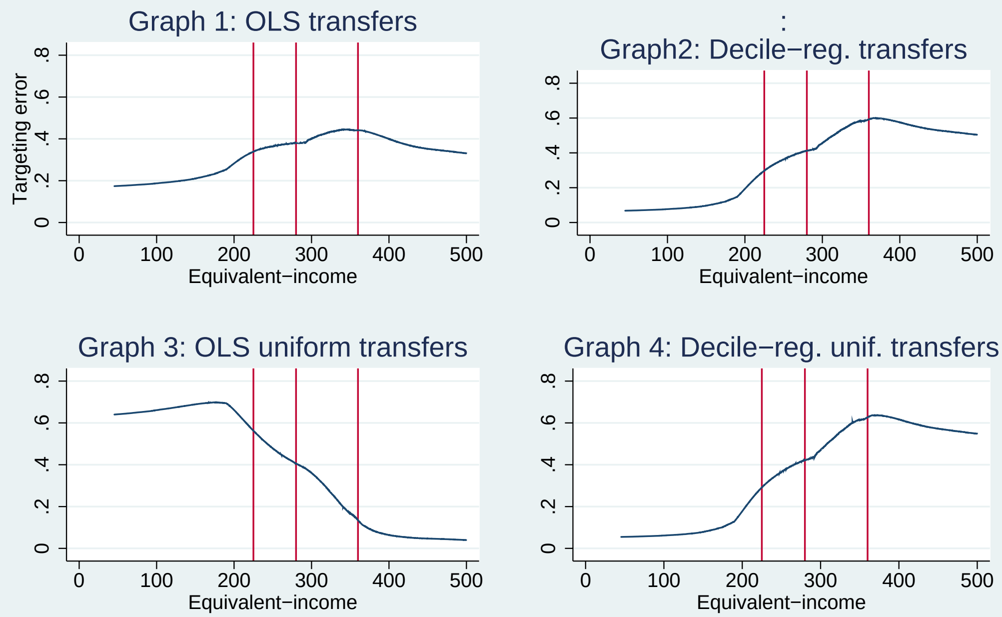
Table for z = TD 360

	P_2 (in %)	Leakage	Under-coverage
OLS 1	**1.93**	**69.0**	**47.3**
OLS 2	**1.31**	**50.4**	**29.9**
OLS 3	**1.17**	**48.1**	**27.3**
TB10 1	**2.10**	**60.0**	**47.3**
TB10 2	**1.32**	**47.7**	**33.2**
TB10 3	**1.17**	**44.9**	**31.4**
TB30 1	**1.93**	**69.5**	**47.3**
TB30 2	**1.28**	**48.6**	**29.7**
TB30 3	**1.12**	**46.6**	**26.6**
QR10 1	**1.98**	**61.9**	**16.9**
QR10 2	**1.26**	**50.1**	**16.6**
QR10 3	**1.07**	**47.3**	**15.3**
QR30 1	**1.98**	**63.7**	**37.6**
QR30 2	**1.24**	**49.0**	**23.1**
QR30 3	**1.05**	**48.3**	**20.7**
QRC01 1	**1.98**	**61.9**	**16.9**
QRC01 2	**1.39**	**50.5**	**15.9**
QRC01 3	**1.13**	**49.6**	**14.0**
Pro-poor Uniform OLS3	**1.29**	**50.8**	**45.0**
Pro-poor Uniform QR10 3	**1.82**	**63.7**	**3.84**

Table for z = TD 225

	P_2 (in %)	Leakage	Under-coverage
OLS 1	.311	93.8	38.2
OLS 2	.154	85.4	17.5
OLS 3	.134	86.5	16.0
TB10 1	.344	82.0	38.2
TB10 2	.141	82.0	18.1
TB10 3	.116	82.0	15.6
TB30 1	.312	94.5	38.2
TB30 2	.140	83.9	16.8
TB30 3	.118	85.3	14.3
QR10 1	.272	84.3	12.6
QR10 2	.092	83.0	6.76
QR10 3	.071	84.0	7.05
QR30 1	.312	87.3	32.9
QR30 2	.118	83.9	11.2
QR30 3	.098	87.8	10.1
QRC01 1	.272	84.3	12.6
QRC01 2	.112	81.0	7.14
QRC01 3	.080	85.0	5.47
Pro-poor Uniform OLS3	.688	24.4	82.5
Pro-poor Uniform QR10 3	.098	86.4	16.1

Figure 1: Ex Post and Ex Ante Targeting Errors



The vertical lines are poverty lines at 225, 280 and 360 TD