Manipulation of Social Program Eligibility: Detection, Explanations, and Consequences for Empirical Research

Adriana Camacho* Emily Conover†

January, 2008

Abstract

We document manipulation of a targeting system which used a poverty index score to determine eligibility for several social welfare programs in Colombia, including publicly provided health insurance. We explain how some politicians may have abused the system by strategically timing the household interviews necessary for determining the poverty index score, and by changing scores. Before diffusion of the poverty index score algorithm, the number of interviews conducted increased around local election periods. After the algorithm was made public, the score density exhibited a sharp discontinuity *exactly* at the eligibility threshold. There are large differences across municipalities. Using mayoral election data we find that in municipalities with more competitive elections the discontinuity at the threshold is larger. We also find suggestive evidence that municipalities with more community organizations and higher newspaper circulation had less manipulation.

JEL No. D72, I32, I38.

*Department of Economics, Universidad de Los Andes. adcamach@uniandes.edu.co. Thanks to the Brown University micro lunch participants. Research assistance from Alejandro Hoyos and Paula Mejía is gratefully acknowledged. We thank *Departamento Nacional de Planeación* (DNP) for providing the Census of the Poor dataset, Andrés Rosas and CEDE for municipality level data and *El Tiempo* for the newspaper circulation data.

†Department of Economics, University of California, Berkeley. econover@econ.berkeley.edu. I am grateful to Chang-Tai Hsieh and Edward Miguel for their advice, and to Pranab Bardhan, Bryan Graham, David Levine, Enrico Moretti and Gérard Roland for helpful comments and suggestions. Special thanks also to Cristine Pinto, Daniel Mejía, Suresh Naidu, Paulina Oliva, Dean Scrimgeour, participants in the UC Berkeley Student Development Workshop and the NEUDC 2007 conference. Comments welcome. All errors are ours.

1 Introduction

Poverty alleviation is a goal of many developing country governments. Faced with scarce resources, targeting methods have increasingly become an attractive way to maximize the impact of government social program spending by directly providing assistance to the poor.[1] Developing countries are also often marked by fragile political institutions and corruption. Although many studies have focused on documenting corruption,[2] little attention has been paid to the interaction between targeting methods and political incentives in developing countries. In this paper we use a comprehensive dataset of a poverty index score used in Colombia to identify potential beneficiaries for a variety of social welfare programs. We document the emergence of a sharp discontinuity in the poverty index score density *exactly* at the eligibility threshold, and we find other unusual patterns in the data which suggest manipulation. We explain how some politicians may have abused the targeting system by strategically timing the household interviews necessary for determining the poverty index score, and by changing scores.

In the early 1990s the Colombian government made targeted social program spending a priority. During this period an unprecedented proxy-means testing targeting system was put in place. To identify the poor population the government designed the Census of the Poor (known as the SISBEN in Colombia).[3] This census collects comprehensive information on dwelling characteristics, demographics, income, and employment at the individual and household level and uses it to assign a poverty index score to each family which goes from 0 (poorest) to 100 (richest). This score was designed to measure long term living conditions, not transitory income shocks, and thus to identify properly the population most in need of assistance. Eligibility rules for several social welfare programs use a specific and known threshold from the poverty index score. The most common threshold was a score of 47 for urban families. People with scores at or below 47 could apply for a

[1] See Coady et al. (2004), and Castañeda et al. (2005) for extensive evaluations of different targeting interventions used around the world.

[2] See Reinikka and Svensson (2004), Bertrand et al. (2006), Hsieh and Moretti, 2006, and Olken (2007).

[3] SISBEN in Spanish stands for: System of Beneficiary Selection. See Castañeda (2005b) for a detailed description of the SISBEN system.

broad range of programs including unemployment benefits, housing improvement programs, food aid to the elderly, educational subsidies, and a publicly provided health insurance program, the largest program (Cárdenas, 2006).[4]

The central government provided municipal officials with instructions on how to target the population of interest for the Census of the Poor with door-to-door interviews, but allowed municipalities discretion over the administration and timing of the interviews. Some safeguards built into the system included the creation and distribution by the central government of the questionnaire and computer program used to calculate the scores for each family interviewed. Nevertheless, information from the interviews was processed within each municipality. In this paper we use the dataset corresponding to the original urban Census of the Poor, implemented from 1994 to 2003.[5] This dataset includes approximately 18 million individual observations with all the responses to the questions in the census, as well as the poverty index score given to each family.

Despite the safeguards in the system, we see unusual patterns in the data suggesting that the recorded poverty index scores are not all accurate. In Figure 1 we document the emergence of a sharp discontinuity of the score density *exactly* at the eligibility threshold. Additionally, in the spirit of studies that use statistics to uncover evidence of cheating,[6] we identified municipalities with relatively high proportions of families that had almost identical interview answers in a given month. We found that 97% of these families had scores below 47, the eligibility threshold. In addition, 91% of these families with suspicious scores were interviewed after 1997, when the score algorithm became well-known to municipal officials. We also use the answers to the questions and the score algorithm, to check that the coded score corresponds to the score the algorithm should have calculated. The coded score and the score generated by the algorithm match in most cases, indicating that most of the manipulation was not due to overwriting the final score. Yet, we

[4] Papers that evaluate programs which use information from the Census of the Poor include Attanasio et al. (2006) and Barrera-Osorio et al. (2007). The first evaluates a conditional cash transfer program known as *Familias en Acción* which uses a lower eligibility threshold score than the one we focus on. The second uses information from a revised Census of the Poor introduced after the one we study here.

[5] A revised Census of the Poor was introduced in 2003.

[6] See, for example, Jacob and Levitt, 2003; Wolfers, 2006.

find that for a few municipalities there is a high concentration of scores at zero, even though the interview answers should not generate a zero score.

Newspaper articles suggest that manipulation took place at the local government level.[7] Besides documenting manipulation of the poverty index scores, using an electoral framework we explain an incumbent politician's relative costs and benefits for abusing the system. A basic assumption underlying the model is that expanding program coverage yields additional votes for political incumbents because inclusion in these welfare programs is something voters want. Before diffusion of the score algorithm to municipal officials (sometime after July 1997), the benefits of surveying for local politicians were high since there was confusion among the general population about the eligibility criteria for the programs. Many people thought that having an interview was a sufficient condition for eligibility. Although, there is variation across municipalities, during this period many local politicians were conducting a relatively high number of surveys around election months. Over time, people became aware that instead of interviews, a score at or below 47 was the necessary condition for program eligibility. After the score algorithm was released, a sharp discontinuity of the score density emerged *exactly* at 47.

We test some of the predictions implied by the model using data from mayoral elections. As predicted, we find that when the elections are more competitive, and thus the benefits to the incumbent of an additional vote are higher, the discontinuity at the poverty threshold is larger. Conversely, using the number of community organizations and newspaper circulation as proxies for the monitoring of politicians, we see suggestive evidence that the discontinuity at the threshold is smaller in municipalities where there is better monitoring, and thus the marginal costs of cheating higher. These findings relate to the growing literature explaining how politicians in developing countries use pre-electoral policies to influence election outcomes (Khemani, 2004; Drazen and Eslava, 2005; Ferraz, 2007).

[7] For example, in the newspaper *El Pais* the titles of articles dated November 28, 1997 and October 13, 2000 translate to "I did not exchange Census of the Poor interviews for votes" (quoting a government official) and "Politicians offer Census of the Poor interviews in exchange for votes".

We also assess whether alternative explanations could generate the observed patterns in the poverty index score distribution. To rule out the possibility that the changes in the distribution are due to changes in macro-economic conditions, we use data from household surveys at three points in time and find that the score distribution from these other data, where there was no incentive for manipulation, is smooth. Finally, since municipalities had discretion over the timing of the interviews, we address the possibility of municipal officials getting better at targeting the poor by looking at the number of interviews conducted, within a municipality and over time, using a geographical targeting mechanism. We find that the number of interviews conducted within poorer and richer neighborhoods remains relatively constant, so again this cannot explain the sharp discontinuity at 47.

Government social program spending in Colombia increased from 8% of GDP in the early nineties to 16.7% in 1996 (Cárdenas, 2006). Most of these social programs use the Census of the Poor to identify beneficiaries. Whereas the poverty index score is intended to identify the population in most need, cheating along the lines documented here, takes resources away from them, and is thus costly for society. Ecuador (with the SelBen) and Chile (with the CAS program) are examples of other developing countries which have similar systems to identify the poor. These countries, and others, can benefit from Colombia's experience when designing and implementing their own programs, in particular in devising ways to overcome politicians' manipulation in determining eligibility.

From a methodological perspective, we emphasize the importance of taking into account the possibility of sorting when evaluating programs that use proxy-means tested targeting.[8] Similarly to studies in the US and the UK that have looked at bunching behavior when the threshold for eligibility for transfer programs or tax payment schedules is known (Friedberg, 2000; Hoynes and Blundell, 2001; Saez, 2002), evaluations of programs that use the Census of the Poor should consider behavioral responses from individuals and politicians. Specifically, the design of the Census of the

[8] See McCrary (2008) for a formal and general test of manipulation of the running variable density function.

Poor could, in theory, allow the use of regression discontinuity (RD) by exploiting the discontinuous assignment of treatment based on the score. However, the results presented show evidence of sorting at the 47 threshold invalidating the use of RD as an appropriate methodology.[9]

The paper is structured as follows: in section 2 we describe the Census of the Poor dataset, the survey data, the election data and other data used in the study. In section 3 we present evidence in support of the manipulation hypothesis. In section 4 we use a political economy model to explain what could be generating the poverty index score discontinuity taking into account politicians' incentives for manipulation. We also test some of the predictions of the model with election data. In section 5 we present results showing that the changes in the distribution are most likely not driven by alternative explanations such as the score algorithm, changes in economic conditions, or selection. We conclude in section 6.

2 Data

2.1 Census of the Poor Data

The original Census of the Poor data was conducted by each municipality between 1994 and 2003. Including urban and rural households, the dataset contains 25.8 million individual records. In our working sample we exclude the rural population because the eligibility thresholds are different for rural areas. Besides, approximately 70% of Colombia's population is urban.

Colombia's neighborhoods are geographically stratified into six levels (strata), with stratum level 1 the poorest and level 6 the wealthiest. There is also an unofficial strata level 0 which corresponds to neighborhoods without access to any type of utilities, domestic workers or people who rent a room from another household. Since the objective of the Census of the Poor was to identify the poor, municipal officials were instructed to conduct door-to-door interviews in neighborhoods of strata below level four, though people living in richer neighborhoods could request an interview. Because in this paper we focus on identifying manipulation from local officials, we also exclude from our working

[9] Related studies which have cautioned against using regression discontinuity designs in different contexts include Lee (2006), McCrary (2008), Urquiola and Verhoogen (2007).

sample people living in neighborhood strata level four or above.[10] This leaves approximately 18 million individuals that represents roughly 40% of the total Colombian population. Of 1120 municipalities, 785 have Census of the Poor records, and these municipalities account for 86.5% of the Colombian population.

The Census of the Poor dataset is not a panel dataset despite the fact that it spans a 10 year period. Generally, each household was interviewed only once. Implementation dates varied by municipality, and most municipalities conducted more than one round of interviews.

Panel A in Table 1 shows summary statistics for the Census of the Poor and a 10% sample of the 1993 Population Census from IPUMS international (IPUMS, 2007). The 1993 Population Census includes all urban socio-economic strata levels, while the Census of the Poor includes only below level 4 (i.e. the left-side of the distribution according to socio-economic strata characterization). The table shows that as expected, people from the Census of the Poor are slightly younger, with larger households, smaller dwellings, and generally less educated. Additional information from the Census of the Poor presented in panel B shows that a very small percentage of the households owns a washer, but more than half owns a television set.

The poverty index score used is a weighted average of answers to the Census of the Poor. The score is calculated at the family level. It uses information from the unit of residence, the family and individuals. The poverty index score has four components: utilities, housing, demographics and education. These components are divided into subcomponents that are added to calculate the overall score. Appendix Table A1 show the algorithm for calculating the poverty index score.

2.2 Household Survey Data

We use household survey data as an alternative data source to verify whether score discontinuities emerge in these surveys. Survey data for 1993 come from the *Socio-economic Characterization Survey* implemented by Colombia's National Planning Agency (DNP), the same agency that designed the Census of the Poor. This survey includes approximately 20,000 households in urban

[10] Our main findings do not change when we include people in all strata levels.

areas. Survey data for 1997 and 2003 come from the *Quality of Life Surveys*, collected by the Colombian National Administrative Department of Statistics (DANE).[11] The 1997 survey includes approximately 9,000 households and the 2003 survey includes approximately 18,500 households in urban areas. The surveys are representative at the national level. In our analysis we restricted the sample to people living in urban areas and strata levels below four to make it comparable with our working dataset of the Census of the Poor.

To get a sense of some demographic characteristics of people with a 47 score, we use information from the 1993 survey.[12] On average, people with a threshold score of 47, and older than 18 years, have 4.8 years of schooling, while people with scores from 0 to 25 have 2.4 years, and richer people with scores from 75 to 100 have 12.6 years of schooling. The average normalized per capita income for someone with a threshold score of 47 is .15. This translates into each person in the family receiving .15 (US$49) of the monthly minimum wage equivalent in that year. The corresponding number for someone with a score between 0 and 25 is 0.11 (US$36), and 2.08 (US$682)[13] for people with scores from 75 to 100. Table 2 shows more detail on these figures, as well as the equivalent values using information from the Census of the Poor.

2.3 Election Data

Mayoral election data were provided by Colombia's Electoral Agency. For the period we study, mayoral elections occurred in 1994, 1997, 2000 and 2003. There is information for the number of votes every candidate in each municipality received only after 1997, thus we created a measure of political competition for these years. We define the intensity of political competition as:

$$political\ competition \equiv 1 - \left(\frac{votes(winner) - votes(runner\ up)}{Total\ votes}\right) \tag{1}$$

We define *political competition* this way so that higher values represent more competitive elec-

[11] The 1993 survey is known in Colombia as the CASEN survey. The 1997 and 2003 surveys are known in Colombia as *Encuestas de Calidad de Vida* (ECV).

[12] We use survey data because we want a representative sample of people in all stratum levels, including level four and above.

[13] All US dollar amounts reported in this section are calculated using purchasing power parity exchange rates for 1998 from the Penn World Tables.

tions. This variable takes values that could go from 0 to 1. The closer to 1 the more competitive the election. Table 3 shows summary statistics for the variables used in the empirical analysis. The mean value for the *political competition* variable is 0.821, which translates into a difference in the fraction of votes the winner received relative to the runner-up of 0.179.

2.4 Other Data

Previous studies have found that institutions are stronger when there is more community oversight or when citizens are better informed (Fiszbein, 1995; Besley and Burgess, 2002; Rosas and Mendoza, 2005). One measure for institution quality that we use is the number of community organizations in each municipality in 1998. Rosas and Mendoza (2005) describe community organizations as neighborhood level government accountability and conflict resolution entities sometimes involved in local infrastructure projects. These data come from a non-profit civil foundation, the Social Foundation (*Fundación Social*).

We also use newspaper circulation data, with the idea that it is harder to cheat in municipalities where the citizens are better informed about public affairs. Newspaper circulation corresponds to certified daily average circulation data by municipality for 2004 from Colombia's main national newspaper, *El Tiempo*.

Other cross section data that we use include: an alternative measure for poverty in a municipality which is the proportion of people with unsatisfied basic needs constructed using information from the 1993 and 2005 Population Census; the distance from the municipality to the largest city in the state measured in kilometers; and the size of the municipality in square kilometers. These data come from DANE. Summary statistics are provided in Table 3.

3 Manipulation of Poverty Index Scores and Timing of Interviews

3.1 Patterns in the Data

The poverty index score could have been manipulated at different stages and by different agents: during the interview by the respondent or the enumerator, at the data entry point or after by someone with access to the data, such as a municipal official. Although we cannot rule out the existence of manipulation at the individual level, it is difficult to detect patterns in the data that indicate the respondent was lying given that there are no right or wrong answers *per se*, but rather a combination of answers that yield a score, and each respondent could have randomly misrepresented himself. If the enumerator is the one who is changing the answers though, we can check whether a particular combination of answers appears with a higher than expected frequency for a given enumerator. Manipulation during or after the data entry stage involves changes to the answers in the questionnaire, to a specific component, or to the final score. In this section we show information in support of the claims that the Census of the Poor was manipulated, and in particular we find problems likely to come during or after the data entry stages. Later, in section 5 we explore whether alternative explanations could be generating the trends we observe in the data.

Some suspicious patterns in the data are shown in Figures 1 and 2. Figure 1 shows that from 1998 to 2003 the score distribution exhibits an increasing discontinuity of the density *exactly* at the eligibility threshold of 47.[14] Figure 2 shows that there are spikes in the number of interviews conducted during periods of mayoral elections from 1994 to 1997.

Figure 3 shows the Census of the Poor distribution for all years and the 1993 household survey data distribution, which is representative at the national level. If the 1993 household survey data distribution is a good approximation of what the Census of the Poor distribution would look like without manipulation, then this figure indicates that one way in which manipulation occurred was to have some scores lowered. The differences between the distributions can guide as to where the people who had their scores changed come from.

It is important to note that the algorithm for the score was made available to the municipal administrators sometime after July 1997 in an instructional presentation that was also distributed as a pamphlet (DNP, 1997). The timing of this release coincides almost exactly with the appearance

[14] Since the Census of the Poor was implemented at the municipal level, we also look at the size of the discontinuity at the threshold allowing for municipality fixed-effects, consistent with Figure 1, we found that from 1994 to 1998 the discontinuity at the threshold is noisy, and we cannot reject the hypothesis that it is centered around zero. From 1998 onward, however, the discontinuity at the threshold increases over time and it is not centered around zero.

of the poverty index score discontinuity at the 47 threshold in 1998.

3.2 Evidence of Manipulation

One way to change the poverty index score is by simply overwriting the real score with a hard coded score below the threshold. Using the score algorithm and the individual answers from the survey we reconstructed the poverty index score and compared it to the one recorded in the data. By doing this we were able to identify, whether the given overall score, or a specific component, is different from what the algorithm should have generated. Panel A in Table 4 shows that the housing, utility and education components match almost perfectly. The observations that did not match in the housing and utility components came mostly from four municipalities where the total given score for a component was zero, despite the fact that the constructed score was non-zero (not reported in the table).

Approximately 11% of individuals do not match in the demographic component. By using 1008 possible combinations for this component we were able to determine where the differences for the non-matching families come from. Panel B in Table 4 shows a break-down of the non-matching families. Most of the discrepancies come from the income per capita in minimum wage units subcomponent, where 720 municipalities have a difference. An explanation for the discrepancy is that at a certain point in the data entry stage the program used to calculate the score asked the data entry person to enter a value for that year's minimum wage. If the municipality entered (by accident or on purpose) the wrong minimum wage, then our minimum wage component is different.

Fifty percent of the difference in the minimum wage units subcomponent comes from one municipality, where in approximately 47% of the cases the reconstructed score is higher than the given score. The second highest concentration comes from a municipality with 12% of the differences, in which approximately 58% of the cases the reconstructed score is higher than the given score. Across all municipalities in 45% of the cases the reconstructed score is higher than the given score. The overall results are presented in Figure 4. This figure shows the given poverty index score distribution and the reconstructed score at the individual level and for people living below strata level four. The figure shows that, with some exceptions at the zero score, the reconstructed score follows closely the given score distribution. Importantly, at the aggregate level, the reconstructed score also changes discontinuously at the threshold, indicating that for most of the municipalities the manipulation did not occur at the point of overwriting the true score for a new score, but it must have occurred at a different stage in the process.

In the data we also identify values of the score that do not exist. Appendix Table A1 shows that most of the subcomponents of the poverty index score have four decimal digits. Across components, the score algorithm generates only two possible combinations that can take whole number values, all other combinations have at least two decimal places. We find that 14 municipalities within a *departamento* (state) have whole number values which the score algorithm could not have generated. Moreover the average of these scores is 20 and all of them are below the eligibility threshold. We also identified the highly unlikely cases that all components sum to zero. We found that the majority of these cases appear in 8 municipalities for 14,354 families and after 1998.

Another way to change the scores, besides hard coding different answers, would be to learn a combination of answers that yields a score below the threshold and use this combination repeatedly. To investigate this possibility, we first selected the families that have almost exactly the same answers as at least one other family interviewed in a given municipality and month.[15] We counted the number of families that we saw with shared answers and we divided that by the total number of families interviewed in that municipality and month. This gives us a ratio between 0 and 1. If, for example, everyone in that municipality and month had the same answers, the ratio would be 1. We ranked that ratio and flagged everyone above the 80th percentile.[16]

With this methodology we were able to identify for example, a municipality that on a single day in 2002 interviewed approximately 45,000 individuals from different neighborhoods, but who all

[15]We write "almost exactly" because the condition we used is that the value for the four components of the score (education, housing, demographics, and utilities) should be exactly the same.

[16]The 80th percentile is an arbitrary value that we chose because we think it is sufficiently high. We are in the process of trying to obtain the complete population census data to calculate the probability of choosing someone with almost exactly the same characteristics within a municipality using as many variables that coincide with the Census of the Poor. Once we obtain this probability we will use it as a guide for determining the threshold.

had a score of 31. These individuals had the same answers for schooling, earnings and possessions, the same survey supervisor, coordinator and data entry person, and very little variation in dwelling characteristics. Overall we identified around 415,000 individuals with highly suspicious similarities in their answers. The distribution of their scores is shown in Figure 5. It is worth noting that 97% of the people identified with unusual answers, fall below the 47 threshold, in contrast to only 50% of all respondents falling below this threshold when using data from the 1993 nationally representative household survey; and 91% percent of them were interviewed after 1997. Furthermore, there is a high concentration of people with scores between 35 and 47 in this group.

To summarize, in this section we showed patterns in the data that suggest there was manipulation in the implementation of the Census of the Poor. We also found some evidence of manipulation by identifying non-matching answers between the score the algorithm would have generated and the given scores. The largest number of suspicious scores comes from looking within municipalities and in each month, where we found 415,000 individuals with repeated answers.

4 Mechanisms for Manipulation of Poverty Index Score and Timing of Interviews

4.1 A Simple Theoretical Framework

In this section we provide a simple framework where a local politician has two tools to increase his electoral support: conduct a high number of surveys before an election, and cheat by lowering some scores. The framework presented here shows that the mechanism through which politicians misused the program, either by conducting a high number of surveys before elections or by changing people's scores, depends on the relative costs and benefits of each at a particular point in time, and these change with their information set.

Using a probabilistic voting model framework (see Lindbeck and Weibull, 1987, and Persson and Tabellini, 2000), let the cumulative density of the poverty index score s be given by F(s).[17] Let the

[17] See Robinson and Verdier (2002), Robinson (2005) and Shaffer (2006) for related literature on vote buying, patronage and clientelism in Colombia respectively.

exogenous poverty index score threshold for program eligibility be denoted s_0, and $0 < F(s_0) < 1$ so that some people fall above and below the poverty index score threshold. Assume that the politician chooses to lower scores for a constant fraction p of people above the s_0 threshold. We will call this "cheating". Cheating does not require being surveyed at that particular point in time since people surveyed in the past could have their scores changed. Assume that the politician never cheats by raising anyone's score. Voters support the incumbent, I, if the expected utility they get from him winning exceeds the expected utility they would get from the challenger C:

$$G^C < G^I + n^I b_{s_i} \mathbb{I}[s \leq s_0] + p b_{s_i} \mathbb{I}[s > s_0] + \delta_i + \theta \tag{2}$$

G represents a vector of public goods proposed by each candidate (for example: taxes or government expenditures), assume it is exogenous. n^I is the number of surveys conducted before the election divided by the total number of surveys conducted, thus $0 \leq n^I \leq 1$. b_{s_i} represents the benefit to the voter of being surveyed. $n^I b_{s_i} \mathbb{I}[s_i \leq s_0]$ represents the expected benefit to the voter if the incumbent conducts a relatively large number of surveys before the election. From the voter's perspective, this term is only beneficial if his score is below the official threshold s_0. p is the proportion of people with scores above s_0 for whom the politician lowers the score to some score below s_0. b_{s_i} is the benefit to the individual from having his score lowered. So $p b_{s_i} \mathbb{I}[s_i > s_0]$ represents the expected benefits to a voter with a score above the threshold. From the perspective of the voter, cheating is only beneficial if his score is above the threshold s_0. δ_i is an individual specific measure of the voter's political bias toward the candidate. $\delta_i \sim U\left[\frac{-1}{2\phi}, \frac{1}{2\phi}\right]$ and for simplicity is distributed independently of the poverty index score. θ is an aggregate shock to the population's preferences, realized after the parties commit to policies. $\theta \sim U\left[\frac{-1}{2\psi}, \frac{1}{2\psi}\right]$.

Re-writing equation 2 for the swing voter (i.e. the indifferent voter between the incumbent or challenger) we get:

$$G^I - G^C + n^I b_{s_i} \mathbb{I}[s \leq s_0] + p b_{s_i} \mathbb{I}[s > s_0] + \theta = -\delta_i \tag{3}$$

From equation 3 and using the fact that $\delta_i \sim U\left[\frac{-1}{2\phi}, \frac{1}{2\phi}\right]$ we derive the expected vote share for the incumbent:

$$V_I = \tfrac{1}{2} + \phi[(G^I - G^C + n^I b_{s_i} + \theta)(F(s_0)) + (G^I - G^C + p b_{s_i} + \theta)(1 - F(s_0))] \tag{4}$$

The first term represents the fraction of people who benefit from the number of surveys conducted. The second terms corresponds to the fraction of people who benefit from cheating. The incumbent wants to maximize the probability of winning the next election $P^I \equiv Pr(V_I > .5)$. Using equation equation 4 and information on the density of θ we get the incumbent's problem:

$$\max_{p,n^I} P^I R - c(p, n^I) \tag{5}$$

Where R are the rents to the incumbent of winning the election. This is equivalent to:

$$\max_{p,n^I} \quad \frac{1}{2} + \psi\phi R[(G^I - G^C + n^I b_{s_i})(F(s_0)) + (G^I - G^C + p b_{s_i})(1 - F(s_0))] - c(p, n^I) \tag{6}$$

In contrast, the challenger cannot conduct surveys or cheat before the election.[18]

Let $c(p, n^I) = \frac{\eta}{2}(n^I)^2 + \frac{c}{2}p^2$ be the costs incurred by the politician, these are increasing in the number of surveys conducted and in the amount of cheating, since the probability of being caught cheating is likely to increase with the amount of cheating, and public awareness of opportunistic behavior by the incumbent from timing the surveys right before elections is also likely to increase with n^I. Solving for the fraction of people for whom the politician lowers the score p, and for the fraction of surveys conducted before the election n^I respectively:

$$p = \frac{\psi\phi R b_{s_i}[1 - F(s^0)]}{c} \tag{7}$$

$$n^I = \frac{\psi\phi R b_{s_i} F(s^0)}{\eta} \tag{8}$$

[18] A question that arises here is whether this is a credible way to buy votes since in a secret ballot system voters could renege on their promise. Other studies have looked at this question, for example, Stokes (2005) explains and illustrates how clientelistic parties are able to circumvent the secret ballot system through "deep insertion into voters' social networks" and repeated interactions between the parties and voters.

Some of the results we obtain from this set-up include an inverse relationship between the costs and the amount of cheating, $\frac{\partial p}{\partial c} < 0$. There is also a direct relationship between the level of political competition ψ, and the amount of cheating, $\frac{\partial p}{\partial \psi} > 0$. In municipalities with a higher proportion of poorer people we should see less cheating, $\frac{\partial p}{\partial F(s^0)} < 0$. And by taking the ratio of equations 7 and 8, we see that there will be an increase in the relative amount of cheating when the costs of surveying rise.

These findings explain that the patterns observed in Figures 1 and 2 are the results of a relative costs and benefit trade off between conducting surveys before an election or cheating. People value surveys because in order to determine eligibility to many social programs they need to be surveyed first. When the program started, there was confusion among the population as to whether being surveyed was a sufficient enough condition for eligibility. At this point, the optimal strategy for the incumbent was to almost exclusively conduct surveys since the costs of surveying relative to cheating were low. The release of the exact poverty index score formula greatly reduced the costs of cheating after 1998. Over time people were also becoming increasingly aware that in addition to being surveyed they needed a score below the threshold, s_0. These factors contributed to a change in the optimal strategy for the incumbent, which became cheating after 1998.[19]

4.2 Empirical Results

Having provided a framework for the patterns documented in Figures 1 and 2, in this section we test whether the extent of cheating in the data responds to incumbents' costs and benefits.

The administration of the Census of the Poor is controlled by the executive branch of local government, thus we use election data for mayors. We regress measures of the discontinuity at the threshold for each municipality on competitiveness of the election. The regression equation has the

[19] The model assumes that poor people in Colombia vote. The election data that we have is at the municipal level, we checked for turnout as a function of the proportion of poor at the municipal level. We found that municipalities with a higher proportion of poor people vote between 0.09-0.13 less than municipalities with lower proportion of poor people.

following form:

$$discontinuity_{jt} = \alpha + \beta_1 political\_competition_{jt-1} + \beta_2 controls_{jt} + \eta_t + \gamma_j + \epsilon_{jt} \quad (9)$$

Where the dependent variable *discontinuity* serves as a proxy for the amount of cheating in a municipality. We construct this variable using data for 6 months before the election. This variable is defined as the difference in the fraction of interviews 3 and 5 points below the threshold relative to the same number of points above the threshold of 47, divided by the number of points (3 or 5). If there were no surveys conducted in this range in a municipality in a given year then the variable *discontinuity* has a missing value. *discontinuity* could go from -1 to 1, but most of the values are positive. The closer this variable is to 0 the smaller the discontinuity at the threshold.

We define *political competition* as specified in equation 1. This variable could go from 0 to 1. The closer the value is to 1 the more competitive the election. Since we only have information for all candidates starting in 1997, we estimate the results for election years 1997, 2000 and 2003. We used lagged political competition as a proxy for anticipated political competition because using the value from the same year is likely to be endogenous since it is a function of anticipated and manipulated political competition.[20]

The variable *controls* includes population and the ratio of urban to total population in each municipality for each year. η is the municipality fixed effect, and γ a year effect. A positive coefficient on *political competition* indicates that more competitive elections are associated with more cheating by incumbents.

Results are displayed in Table 5. Consistent with the model the table shows that when the benefits of an additional vote are higher, the discontinuity at the threshold is in fact larger. Columns (1) and (4) do not include any controls, all other columns include population controls. A standard deviation increase in the amount of political competition (s.d.= 0.17) evaluated at the mean value, increases the percent of interviews three points below the threshold relative to three points above

[20] Mayors in Colombia can not be re-elected for consecutive terms. However Drazen and Eslava (2005) explain that "manipulation of fiscal policy is regarded as a usual political practice" mainly because the politician's decisions "affect his party's re-election chances (or those of the incumbent's preferred candidate)".

the threshold from .031 to .036. The magnitude of the effect remains constant after including population controls.

The available data that proxies for the cost of cheating do not vary over time. We use number of community organizations and number of the main newspaper in circulation as measures for the costs of manipulation in a given municipality. The equation we use to determine whether the size of the discontinuity is smaller when the costs of cheating are higher has the following form:

$$discontinuity_{jt} = \alpha + \beta_1 costs_j + \beta_2 lnpop_{jt} + \beta_3 demography_{jt} + \beta_4 geography_j + \eta_t + \epsilon_{jt} \quad (10)$$

Where the dependent variable *discontinuity*, again proxies for the amount of cheating in a municipality. *costs* is either the number of community organizations or the daily average newspaper circulation from May to October in each municipality.[21]

A concern about running a cross section regression is that the mode of the score distribution is centered at a different point for each municipality depending on its wealth level. In the variable *demography* we control for differences in poverty rates across municipalities by including a measure of the proportion of people with unsatisfied basic needs calculated from the 1993 and 2005 population census. We also included in all regressions a measure of the size of the population *lnpop*, and the proportion of urban population in each municipality. To control for the possibility that more remote areas could have more cheating because of weaker presence of the state, we included the distance to the largest city in the *departamento* (state). Also in the *geography* variable we included the surface area of the municipality. We expect to see that municipalities with better monitoring institutions have less cheating. For these regressions we used the same years that we used in the previous table: 1997, 2000 and 2003.

We find that the coefficients have the expected signs, consistent with the idea that better monitoring is associated with less cheating in municipalities around election times. Columns (1)-(4) use the fraction of surveys three points below and above the threshold, while columns (5)-(8)

[21] We use data from May to October because this would correspond to the period six months before the mayoral elections. We divided the number of community organizations by 1000 and the newspaper circulation by 100,000 to ease interpretation

use the fraction of surveys 5 points below and above the threshold. The results indicate that a standard deviation increase in the number of community organizations or newspaper circulation (s.d. = 0.325 and 0.032 respectively) evaluated at the mean value, are associated with a lower percent of interviews three points below the threshold relative to three points above the threshold from .031 to .030. The effect is similar for newspaper circulation. Also, consistent with the model prediction we see that there is an inverse relation between the discontinuity at the threshold and the proportion of poor in a municipality. In the next section we rule out alternative explanations for the score manipulation we observe.

5 Alternative Explanations for Pattern in Score Distribution

We first rule out that the score algorithm is mechanically generating a higher number of combinations for scores below the eligibility threshold. The score algorithm takes information from approximately 24 questions. The answers are then used to compute sub-scores for each of the four components. There are 384 possible combinations in the education component, 1008 in the demographic, 90 in utilities, and 480 in the dwelling component for a total of approximately 16 billion possible combinations of answers. We calculated the number of possible combinations to generate each score and plotted the distribution. The maximum number of combinations is around 600 million for a score of 50. The minimum is 1 for a score of 100. Figure 6 shows that the simulated distribution does not exhibit a discontinuity at the eligibility threshold or anywhere else.[22]

Another explanation for what could be generating the pattern in the score distribution over time could be changes in general macroeconomic conditions. In fact, in 1999 Colombia experienced a recession. During that year, according to figures from the National Statistics Agency (DANE), real GDP fell by 4.2%. The recession is likely to have increased the proportion of poor in the

[22] Here we assumed that all combinations are equally likely. In reality however, we expect the covariance between certain answers to be different from zero and not to see some combinations in the population. The score distribution depicted in Figure 7 uses survey data from representative samples of the Colombian population, which we restrict to strata levels below 4 to make it comparable to the population in the Census of the Poor, this distribution does not exhibit discontinuities at the threshold.

population, and thus could have affected the shape in the aggregate score distribution. To address this concern, we took alternative data from nationally representative household surveys for 1993, 1997 and 2003. If the unusual patterns in the poverty index score data are genuine, not due to manipulation, we would expect to see them in an alternative dataset. Using these surveys and the score algorithm, we constructed the poverty index score to see how the distribution behaves over time. We recognize that survey data has shortcomings some of which include: the wording of questions might be different from the Census of the Poor; the surveys by design have smaller sample sizes; and the surveys provide a "snapshot" of the population in a given year. We tried to overcome the first shortcoming by using the 1993 household survey. This survey was conducted during the summer of 1993, prior to the Census of the Poor, was used as a pilot survey in the design of the Census of the Poor, and it has almost identical question wording.[23] We believe that people answering the 1993 household survey had no incentives to provide false information because prior to the Census of the Poor, eligibility for social programs in Colombia was not directly determined using this type of survey information. There is nothing we can do to overcome the second shortcoming, but in general, after restricting the sample to strata level below four, the surveys we used are representative of the population of interest, the urban poor. The third shortcoming we addressed by using survey data for 1997 and 2003 which provide information on changes of the distribution over time.

Even though we do not have survey data for 1999, the year of the recession, we expect that if the effects of the recession went beyond 1999 then the 2003 survey data distribution should also exhibit a discontinuity at the threshold, such as the one observed in the Census of the Poor. The first graph in Figure 7 shows that the lowess 1993 household survey distribution and the Census of the Poor distribution for 1994 look very similar.[24] The Census of the Poor distribution lies slightly to the right of the 1993 household survey distribution. The second and third graphs in

[23]One exception is in the income question, where the household survey provides more detailed and extensive questions on income sources.

[24]We use lowess because of limitations in sample size. Even if we did not smooth the distributions the survey data do not exhibit a discontinuity at the threshold.

Figure 7 show the poverty index score distribution and the Quality of Life surveys for 1997 and 2003 respectively. In 1997 the Census of the Poor distribution is to the left of the survey distribution, but we do not observe a discontinuity at the eligibility threshold. In 2003 however the two distributions differ greatly. The mode of the distribution of the Census of the Poor is to the left and there is a discontinuity at the eligibility threshold, which does not appear in the survey data distribution.

To summarize, from Figure 7 we can see that if a random sample of interviews was drawn each year, then the distribution would not exhibit a discontinuity at the eligibility threshold and, consistent with the overall growth in the Colombian economy during this 10 year period, the distribution would be moving to the right over time. However, instead what we see is that the mode of the Census of the Poor distribution moves left over time, and that after 1997 the distribution shows a discontinuity at the eligibility threshold.

One objection to Figure 7 is that the survey data that we use is a representative sample of the population at a given point in time. Comparisons with these data assume that a random sample of neighborhoods was interviewed in a given year across and within municipalities. In fact, municipalities had discretion on the timing of the surveys, and not all municipalities interviewed all people in strata level below four at once. Thus, it could be possible that the pattern we see at the aggregate level is driven by selection. Specifically, richer municipalities could have conducted interviews first, and within a municipality richer neighborhoods could have been surveyed first.[25] This is worrisome since one explanation for the pattern in the score distribution could be that over time municipalities became better at identifying the poor neighborhoods, or that the municipalities which conducted the interviews later were poorer and thus had a higher concentration to the left of the threshold.

Since implementation was done at the municipal level, and to the extent possible, our analysis is at this level, one way to check for selection is by comparing the number of surveys conducted by stratum level over time within a municipality. We did this because we knew that the central

[25] This however goes against information provided by some municipal officials in charge of the implementation who told us that poorer neighborhoods were prioritized for surveys.

government instructed municipal officials to use strata levels in targeting. We should be concerned about selection if, for instance, we see that within a municipality strata level 1 (poorer) interviews are increasing over time while in strata level three (richer) interviews are decreasing. The equation that we use to calculate the number of interviews within a municipality over time is:

$$surveys\_stratumx_{jt} = \alpha + \eta_t + \gamma_j + \epsilon_{jt} \tag{11}$$

Where $surveys\_stratumx$ corresponds to the number of surveys conducted in stratum level x. In Figure 8 we plot the coefficients for η which correspond to each year month combination from January 1994 to September 2003, using January 1994 as the reference month. The Figure shows that, excluding the peaks in 1995 and 1997 which correspond to electoral periods previously discussed, for strata one to three the number of interviews remains relatively constant over time, and they have a slight upward trend after 2000 for strata 0.[26] Overall, the results presented here do not indicate that the score algorithm, changes in economic conditions or selection explain why after 1998 we start seeing a discontinuity exactly at the eligibility threshold.

6 Conclusion

In this paper we documented patterns in the data that indicate there was manipulation during the implementation of the Census of the Poor in Colombia. We also developed a theoretical framework to illustrate a mechanism through which manipulation by politicians may have occurred. We tested some of the predictions of this framework with electoral data and found that the amount of manipulation in a municipality is positively associated with political competition. We also found suggestive evidence of less cheating during electoral periods when there is a stronger presence of monitoring institutions.

In a "back of the envelope" calculation we estimate that from 1994 to 2003 approximately three million people were not properly assigned a poverty index score. Considering that during the period

[26] To further look into the possibility of selection in the data as an explanation for the patterns we see over time, we are in the process of trying to obtain the complete population census data for 1993 to see if the characteristics of neighborhoods interviewed earlier within a municipality vary greatly from those that were interviewed later.

studied the total population of Colombia was approximately 40 million, the misallocation of three million of the poorest segment of the population is noteworthy.

Most of the paper has focused on documenting and explaining motivations for manipulation, yet the findings presented here raise an important normative question: Given that we find evidence that the poverty index scores were lowered, but not raised, more people became eligible for social programs, so was the manipulation observed necessarily "bad" from a social welfare perspective? Some factors that should be considered when answering this question include: Assuming that the design of the proxy-means testing instrument is properly identifying the population most in need of social program benefits, then the resources used by people who had their scores lowered could have instead been used to provide additional social programs for people truly below the poverty eligibility threshold. Furthermore, in anthropology and political science "clientelism" is known as a relation between a politician who gives patronage in exchange for the vote or support of a 'client' (Robinson and Verdier, p.3, 2006). If the people who had their scores lowered were able to become eligible for different social programs because of their political connections rather than their need, then it is likely that this redistribution of benefits was socially wasteful, as is usually the case in clientelistic relations.[27] If, on the other hand, the people who had their scores lowered were truly in need, then this type of manipulation was welfare enhancing, in which case, the need for a better mechanism to identify the poor arises.

Whether or not the manipulation documented here reduced welfare, the findings in this paper highlight the importance of adopting changes to improve the system. Developing countries that have implemented similar systems to identify the poor can benefit from Colombia's experience when designing or implementing their own programs. The Colombian government already made some changes that help reduce manipulation in the implementation of the "new" Census of the Poor which started in 2003. The new census has a different questionnaire and a new score algorithm which has been kept secret. The government has also set guidelines that limit conducting interviews

[27]See Robinson, 2005, for information on the historical presence of clientelistic relationships in Colombia.

or assigning social benefits in pre-electoral periods in certain municipalities.[28] Further efforts and controls like increasing the penalties for cheating, improving detection of cheaters, and more forcefully restricting to non-electoral periods the selection of the people eligible for the program should be considered as ways in which future duplicity can be limited.

Finally, from a methodological perspective, we want to highlight the importance of checking whether the necessary assumptions for identification hold when conducting empirical research.[29] This should be done before any efforts are made to evaluate the effectiveness of social programs since ignoring the possibility of sorting can lead to biased results. For instance, in evaluating the effect on health outcomes of publicly-provided health insurance for the poor in Colombia, preliminary findings show that disregarding the possibility of sorting at the threshold overestimates the effectiveness of welfare health insurance (Camacho and Conover, 2007).

[28] As reported in *El Tiempo*, September 2, 2003.

[29] See also McCrary (2007), Urquiola and Verhoogen (2007).

References

Attanasio, Orazio, Emla Fitzsimons, Ana Gomez, Diana Lopez, Costas Meghir, and Alice Mesnard, "Child Education and Work Choices in the Presence of a Conditional Cash Transfer Programme in Rural Colombia," CEPR Discussion Papers 5792, C.E.P.R. Discussion Papers August 2006.

Barrera-Osorio, Felipe, Leigh Linden, and Miguel Urquiola, "The Effects of User Fee Reductions on Enrollment: Evidence from a Quasi-Experiment," working paper 2007.

Bertrand, Marianne, Simeon Djankov, Rema Hanna, and Sendhil Mullainathan, "Does Corruption Produce Unsafe Drivers?," NBER Working Papers 12274, National Bureau of Economic Research 2006.

Besley, Timothy and Robin Burgess, "The Political Economy Of Government Responsiveness: Theory And Evidence From India," *Quarterly Journal of Economics*, November 2002, *117* (4), 1415–1451.

Camacho, Adriana and Emily Conover, "Evaluating the Effects of Public Health Insurance on Child Health in Colombia," working paper 2007.

Cárdenas, Mauricio, *Introducción a la Economía Colombiana*, Alfaomega-Fedesarrollo, Bogotá Colombia, 2006.

Castañeda, T., K. Lindert, B. de la Brière, L. Fernandez, C. Hubert, O. Larrañaga, M. Orozco, and R. Viquez, "Designing and Implementing Household Targeting Systems: Lessons from Latin America and the United States," Social Protection Discussion Paper Series 0529, The World Bank, Social Protection Unit, Washington DC 2005.

Castañeda, Tarsicio, "Targeting Social Spending to the Poor with Proxy Means Testing: Colombia's SISBEN System," Social Protection Discussion Paper Series 0526, The World Bank, Social Protection Unit, Washington DC 2005.

Coady, D., M.E. Grosh, and J. Hoddinott, *The Targeting of Transfers in Developing Countries: Review of Lessons and Experience*, World Bank Publications, 2004.

Departamento Nacional de Planeación, "SISBEN: Una Herramienta para la Equidad," July 1997.

Drazen, Allan and Marcela Eslava, "Electoral Manipulation via Expenditure Composition: Theory and Evidence," NBER Working Papers 11085, National Bureau of Economic Research January 2005.

Ferraz, Claudio, "Electoral Politics and Bureaucratic Discretion: Evidence from Environmental Licenses and Local Elections in Brazil," Working Paper, Instituto de Pesquisa Economica Aplicada January 2007.

Fiszbein, A., "Local Government Capacity in Colombia: Beyond Technical Assistance," *World Bank Country Study*, 1995.

Friedberg, L., "The Labor Supply Effects of the Social Security Earnings Test," *The Review of Economics and Statistics*, 2000, *82* (1), 48–63.

Hoynes, Hilary and Richard Blundell, "Has "In-Work" Benefit Reform Helped the Labour Market?," NBER Working Papers 8546, National Bureau of Economic Research October 2001.

Hsieh, C.T. and E. Moretti, "Did Iraq Cheat the United Nations? Underpricing, Bribes, and the Oil for Food Program," *Quarterly Journal of Economics*, November 2006, *121* (4), 1211–1248.

Jacob, Brian A. and Steven D. Levitt, "Rotten Apples: An Investigation of The Prevalence and Predictors of Teacher Cheating," *Quarterly Journal of Economics*, August 2003, *118* (3), 843–877.

Khemani, S., "Political Cycles in a Developing Economy: Effect of Elections in the Indian States," *Journal of Development Economics*, 2004, *73* (1), 125–154.

Lee, David, "Randomized Experiments from non-Random Selection in U.S. House Elections."

Lindbeck, Assar and Jorgen Weibull, "Balanced-Budget Redistribution as the Outcome of Political Competition," *Public Choice*, January 1987, *52* (3), "272 – 297".

McCrary, Justin, "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics*, February 2008, *127* (2), 698–714.

Minnesota Population Center, "Integrated Public Use Microdata Series (IPUMS) International: Version 3.0.," 2007.

Olken, Benjamin A., "Monitoring Corruption: Evidence from a Field Experiment in Indonesia," *Journal of Political Economy*, April 2007, *115* (2), 200–249.

Persson, Torsten and Guido Tabellini, *Political Economics: Explaining Economic Policy*, MIT Press, Cambridge MA, 2000.

Reinikka, Ritva and Jakob Svensson, "Local Capture: Evidence From a Central Government Transfer Program in Uganda," *Quarterly Journal of Economics*, May 2004, *119* (2), 678–704.

Robinson, J.A. and T. Verdier, *The Political Economy of Clientelism*, Centre for Economic Policy Research, 2002.

Robinson, James A., "A Normal Latin American Country? A Perspective on Colombian Development," Working Paper, Harvard University Department of Government 2005.

Rosas, Andres and Juan Mendoza, "The Economic Effects of Geography: Colombia as a Case Study," Documentos de Economía 003584, Universidad Javeriana May 2005.

Saez, E., "Do Taxpayers Bunch at Kink Points?," NBER Working Papers, National Bureau of Economic Research 2002.

Schaffer, F.C., *Elections for Sale: The Causes and Consequences of Vote Buying*, Lynne Rienner Publishers, 2006.

Stokes, S.C., "Perverse Accountability: A Formal Model of Machine Politics with Evidence from Argentina," *American Political Science Review*, 2005, *99* (03), 315–325.

Urquiola, Miguel and Eric A. Verhoogen, "Class Size and Sorting in Market Equilibrium: Theory and Evidence," CEPR Discussion Papers 6425, C.E.P.R. Discussion Papers August 2007.

Wolfers, Justin, "Point Shaving: Corruption in NCAA Basketball," *American Economic Review*, May 2006, *96* (2), 279–283.

Table 1: Summary Statistics: Census of the Poor and 1993 Population Census

	Census of the Poor		Population Census	
Panel A: Description	Mean	Std. Dev	Mean	Std. Dev
Demographics				
Age	25.67	19.47	26.37	18.88
Household size	4.50	2.36	4.17	2.14
Proportion male	0.48	–	0.48	–
Not disabled	0.98	–	0.98	–
Dwelling characteristics				
Number of rooms	2.10	1.38	3.77	1.93
Brick, rock or block walls	0.75	–	0.86	–
Dirt floors	0.12	–	0.07	–
Utilities				
Access to electricity	0.98	–	0.96	–
Access to sewage	0.75	–	0.87	–
Trash disposal service	0.80	–	0.83	–
Highest Schooling (age>18)				
None	0.12	–	0.06	–
Primary	0.52	–	0.38	–
Secondary	0.34	–	0.42	–
College	0.02	–	0.13	–
Post-college	0.00	–	0.01	–

	Census of the Poor
Panel B: Description	% of People
Possessions	
Own a TV	0.58
Own a refrigerator	0.36
Own a blender	0.42
Own a washer	0.04

Note: Panel A includes information available both in the Census of the Poor and the Population Census. Panel B includes only information available in the Census of the Poor. The Census of the Poor has 18,176,019 observations. For the 1993 Population Census we use a 10% random sample from IPUMS-international with 2,325,747 observations. We restrict both to urban areas only. The 1993 Population Census includes all socio-economic strata levels, while the Census of the Poor includes only levels below 4 (i.e. the left-side of the distribution according to socio-economic strata geographical characterization). – indicates the standard deviation is omitted in binary variables, the mean value in this case corresponds to the proportion of people with the reported characteristics.

Table 2: Education and Income by Poverty Index Score Groups

Poverty Index Score	Years of Schooling (age>18)		Normalized HH income per capita	
(groups)	Survey data	Census of the Poor	Survey data	Census of the Poor
0-25	2.35	2.78	0.11	0.08
25-50	4.47	4.35	0.15	0.12
50-75	7.96	7.66	0.60	0.50
75-100	12.57	12.22	2.08	1.69
Mean	7.23	5.36	0.56	0.25
Median	7	5	0.31	0.13

Note: The survey data comes from the 1993 Socio-economic Characterization Survey, representative at the national level. The Census of the Poor includes only levels below 4 (i.e. the left-side of the distribution according to socio-economic strata geographical characterization).

Table 3: Summary Statistics: Election and Control Variables

Description	Mean	Std. Dev	Min	Max
Political competition	0.821	0.170	0.065	1.000
Discontinuity +/- 3 points	0.031	0.026	-0.111	0.148
Discontinuity +/- 5 points	0.036	0.024	-0.067	0.122
Log population	10.045	1.176	7.731	15.678
Ratio of urban to total population	0.475	0.248	0.058	0.998
Proportion of poor (NBI)	0.380	0.169	0.001	0.929
Number of community organizations	56	325	2	5944
Newspaper circulation	434	3154	1	51574
Distance to largest city in state (km)	101	83	0	548
Surface area of municipality (km2)	796	1889	15	17873

Note: Discontinuity +/- x points is the difference in the fraction of interviews x=3,5 points before the threshold relative to the same points after the threshold, using data for the 6 months prior to the election. The closer to 0 the smaller the discontinuity at the threshold. Political competition is one plus the negative of the difference in the fraction of votes the winner received relative to the runner-up in the previous election (see equation 1). The closer to 1 the more competitive the election. Proportion of poor (NBI) is a measure for unsatisfied basic needs constructed using information from the 1993 and 2005 Population Census. Community organizations are the number of neighborhood level civil institutions in each municipality. Newspaper circulation corresponds to certified daily average circulation data by municipality for 2004 from Colombia's main national newspaper, *El Tiempo* (both are in units). A municipality in Colombia is the jurisdiction most similar to a county in the U.S.

Table 4: Reconstructed vs. Recorded Poverty Index Score

Panel A: Component	Match	Individuals	HH	% HH
Housing	Yes	18,223,521	5,341,261	99.67
	No	60,344	16,869	0.33
Utilities	Yes	18,183,770	5,331,420	99.45
	No	100,095	26,710	0.55
Education	Yes	17,826,330	5,229,323	97.50
	No	457,535	116,501	2.50
Demographic	Yes	16,145,135	4,747,080	88.60
	No	2,138,730	611,050	11.40
Source of demographic component differences				
Panel B: Demographics	Match	Individuals	HH	% HH
Age	No	46,130	9,516	1.56
Employment	No	1,906	308	0.05
Number of Rooms	No	450,758	120,764	19.76
Minimum wage	No	1,529,315	446,368	73.05
Household size	No	92,020	29,223	4.78
Value not found	No	18,601	4,871	0.80
Total	No	2,138,730	611,050	

Note: The Census of the Poor includes individuals in urban areas and all socio-economic strata levels. In Panel A "Match" indicates all individuals and households where the reconstructed score (calculated using the score algorithm and answers to each question) agrees with the score given in the database. Panel B reports the main source of demographic component differences between the given score and the reconstructed score.

Table 5: Discontinuity at the Threshold and Political Competition

Dependent variable:	Discontinuity +/- 3 points			Discontinuity +/- 5 points		
	(1)	(2)	(3)	(4)	(5)	(6)
Political competition	0.030***	0.029**	0.029**	0.022**	0.022*	0.022*
	[0.011]	[0.012]	[0.012]	[0.010]	[0.011]	[0.011]
Log population		0.273***	0.265***		0.162**	0.164*
		[0.087]	[0.098]		[0.069]	[0.084]
Ratio of urban to total			-0.173			0.024
population			[1.019]			[0.792]
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	468	468	468	468	468	468
R-squared	0.92	0.93	0.93	0.94	0.95	0.95

Note: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include an intercept term. The dependent variable is the difference in the fraction of interviews 3 and 5 points before the threshold relative to the same points after the threshold divided by the number of points, using data for the 6 months prior to the election. The closer to 0 the smaller the discontinuity at the threshold. Political competition is defined as one plus the negative of the difference in the fraction of votes the winner received relative to the runner-up in the previous election (see equation 1), thus scores closer to 1 denote more competitive elections.

Table 6: Discontinuity at the Threshold and Costs of Cheating (Cross Section)

Dependent variable:	discontinuity +/- 3 points				discontinuity +/- 5 points			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of community	-0.004**	-0.003**			-0.006***	-0.005***		
organizations	[0.001]	[0.002]			[0.002]	[0.002]		
Newspaper circulation			-0.037**	-0.035**			-0.068***	-0.067***
			[0.016]	[0.017]			[0.016]	[0.016]
Proportion of poor	-0.039***	-0.046***	-0.038***	-0.043***	-0.034***	-0.039***	-0.031***	-0.032***
	[0.006]	[0.007]	[0.007]	[0.008]	[0.006]	[0.006]	[0.007]	[0.008]
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demography controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography controls		Yes		Yes		Yes		Yes
Observations	790	790	587	587	790	790	587	587
R-squared	0.07	0.08	0.09	0.10	0.08	0.09	0.09	0.10

Note: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include an intercept term. The dependent variable is the difference in the fraction of interviews 3 and 5 points before the threshold relative to the same points after the threshold divided by the number of points, using data for the 6 months prior to the election. The closer to 0 the smaller the discontinuity at the threshold. The number of community organizations is divided by 1000. The number of newspaper circulation is divided by 100,000. Demography controls include proportion urban population and proportion poor population using a measure for unsatisfied basic needs from the 1993 and 2005 Population Census. Geography controls include distance to the largest city (capital) in the *departamento* (state) and municipality surface area.

Figure 1: 1994-2003 Poverty Index Score Distribution

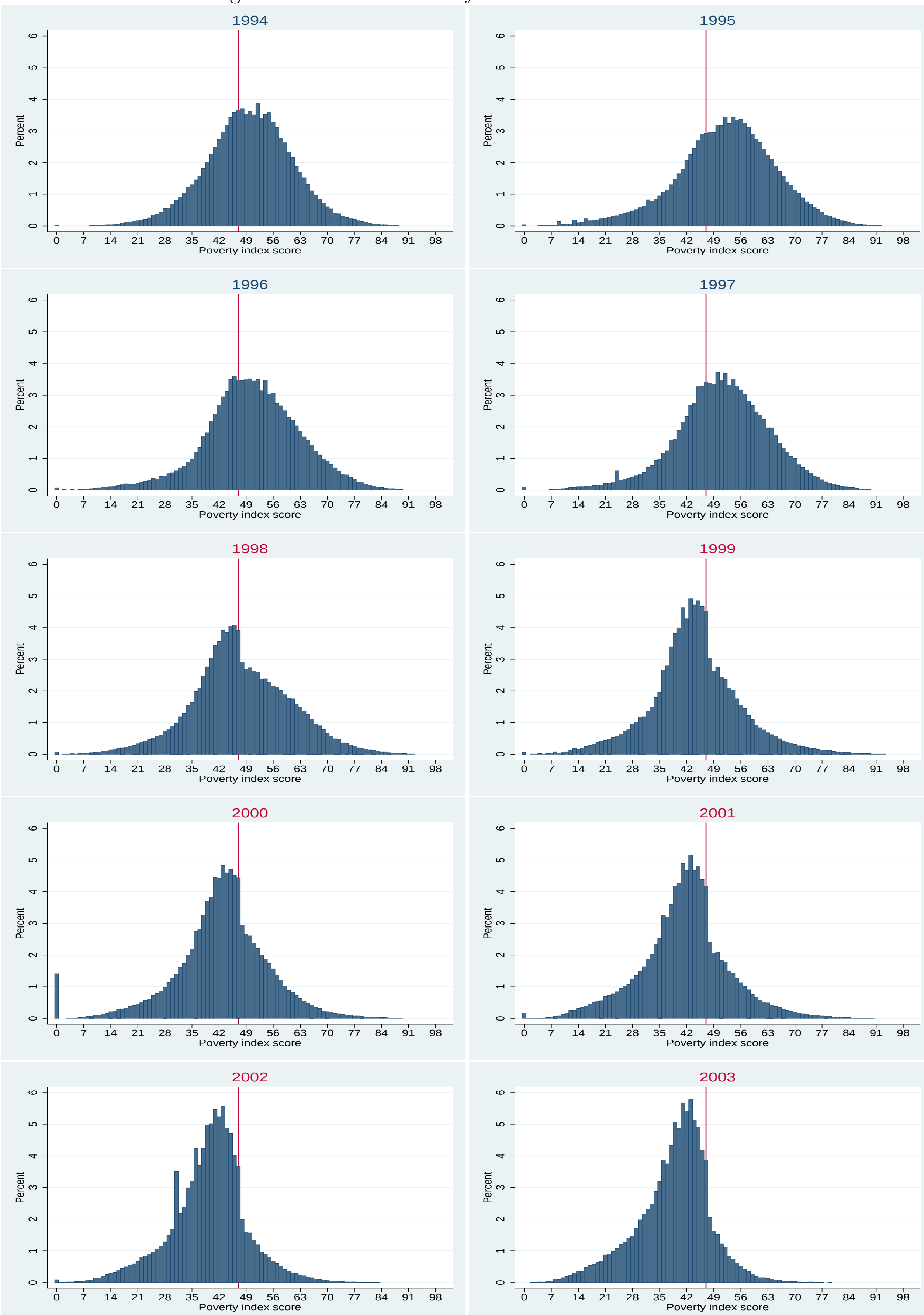

Figure 2: Number of Census of the Poor Interviews, controlling for Municipality and Strata

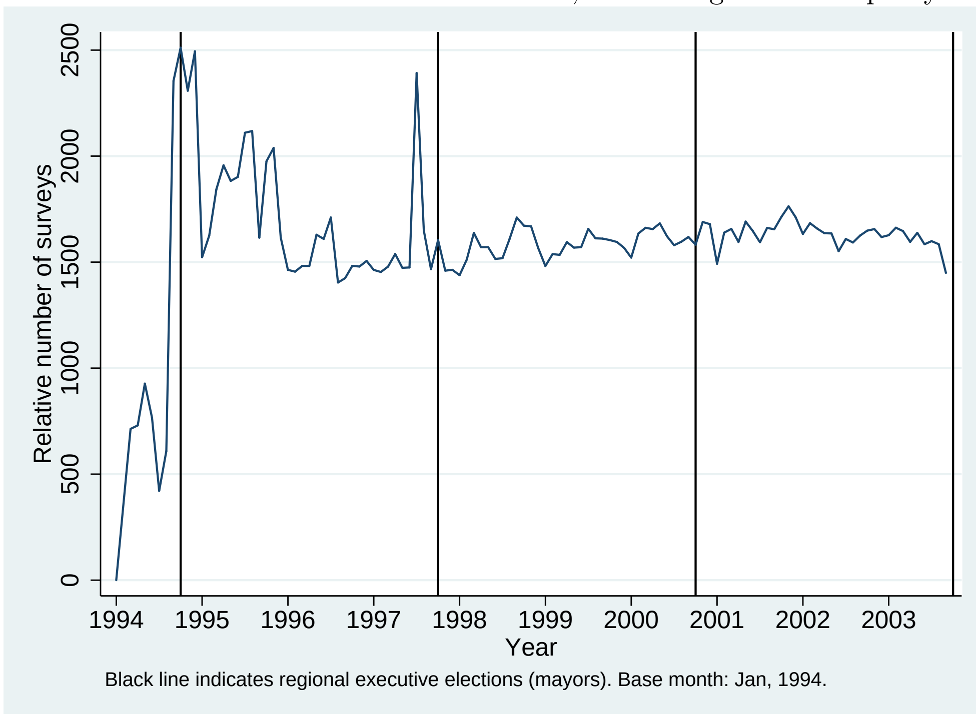

Figure 3: Census of the Poor and 1993 Survey Data Score Distribution

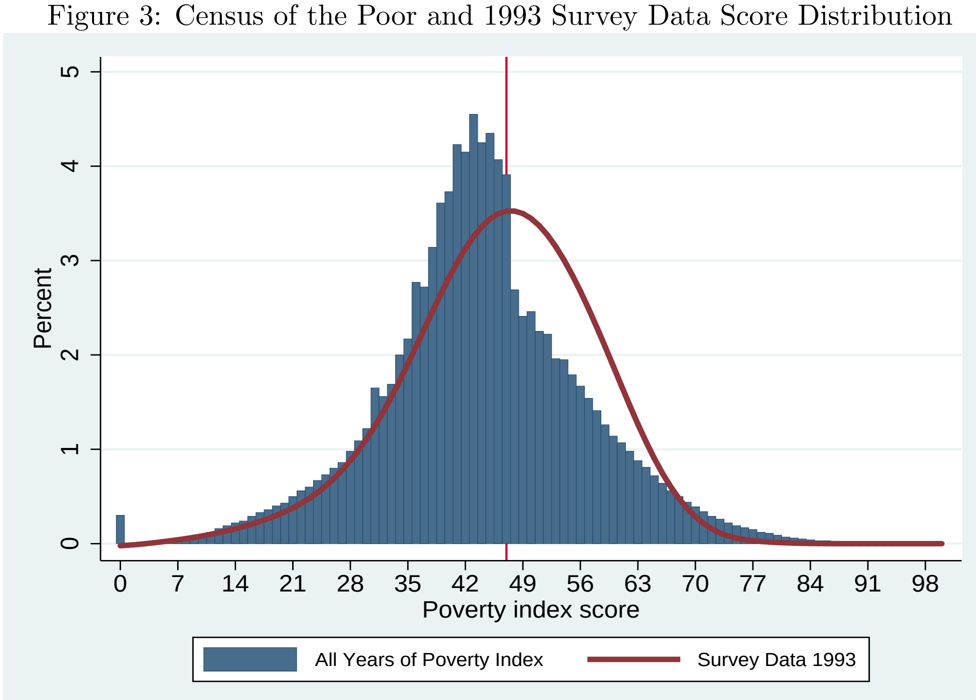

Figure 4: Poverty Index Score and Reconstructed Score

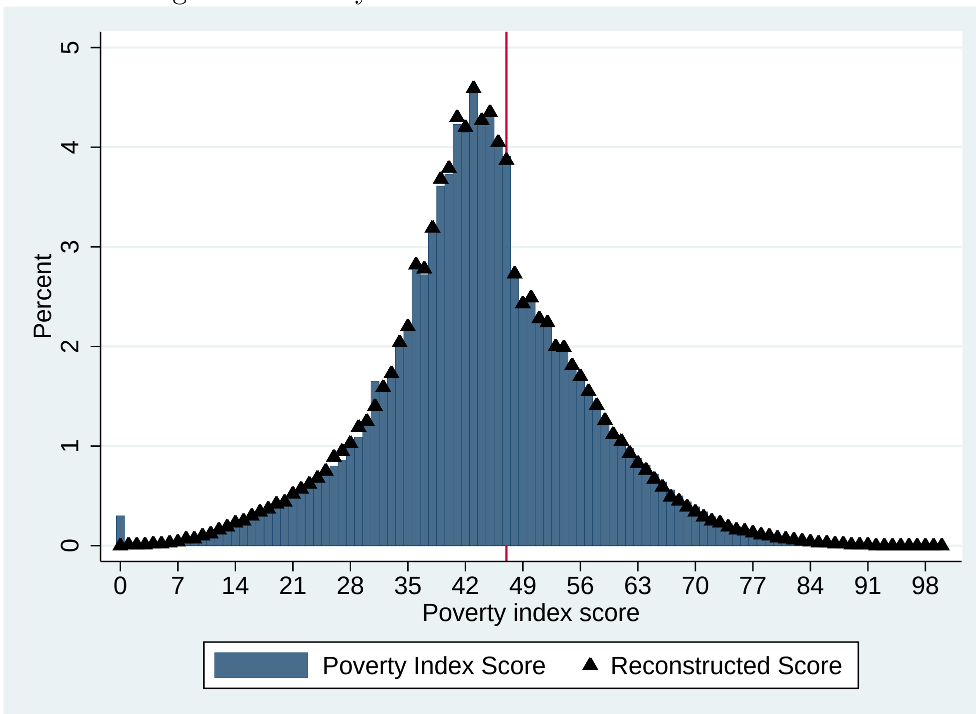

Figure 5: Poverty Index Scores of Individuals with Repeated Answers within a Municipality and Month

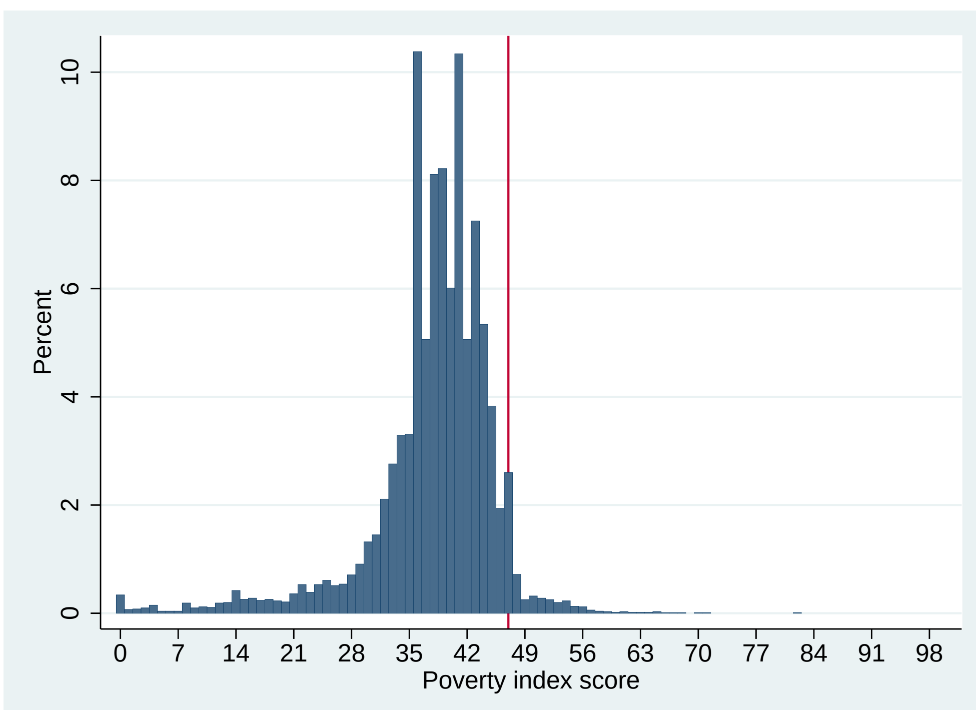

Figure 6: Simulated Distribution of Combinations by Score Generated by the Algorithm

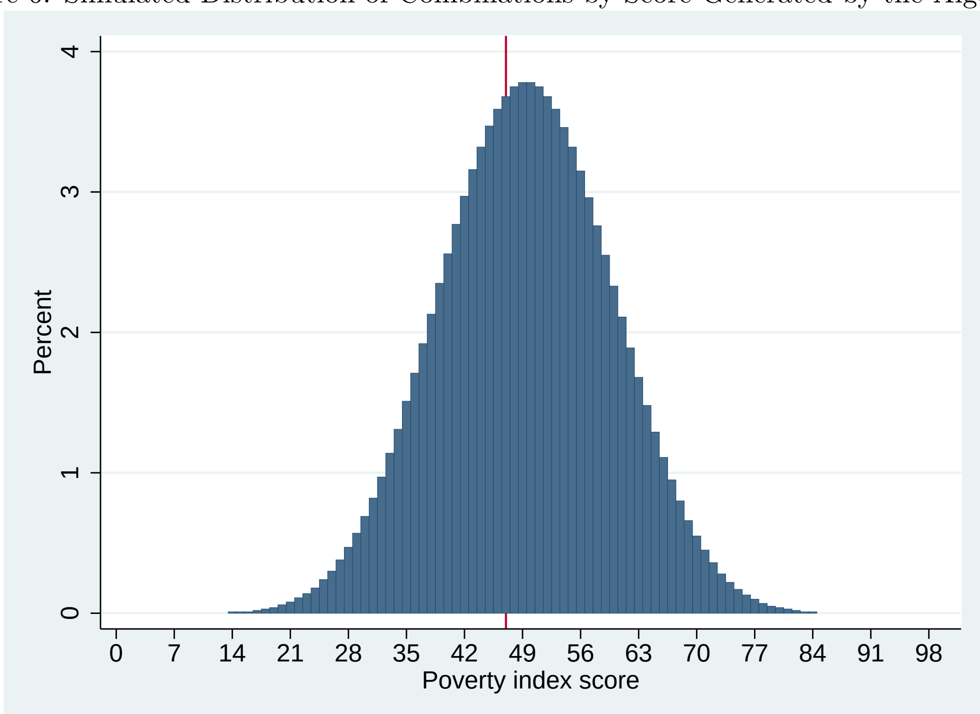

Figure 7: Poverty Index and Survey Data Score Distributions

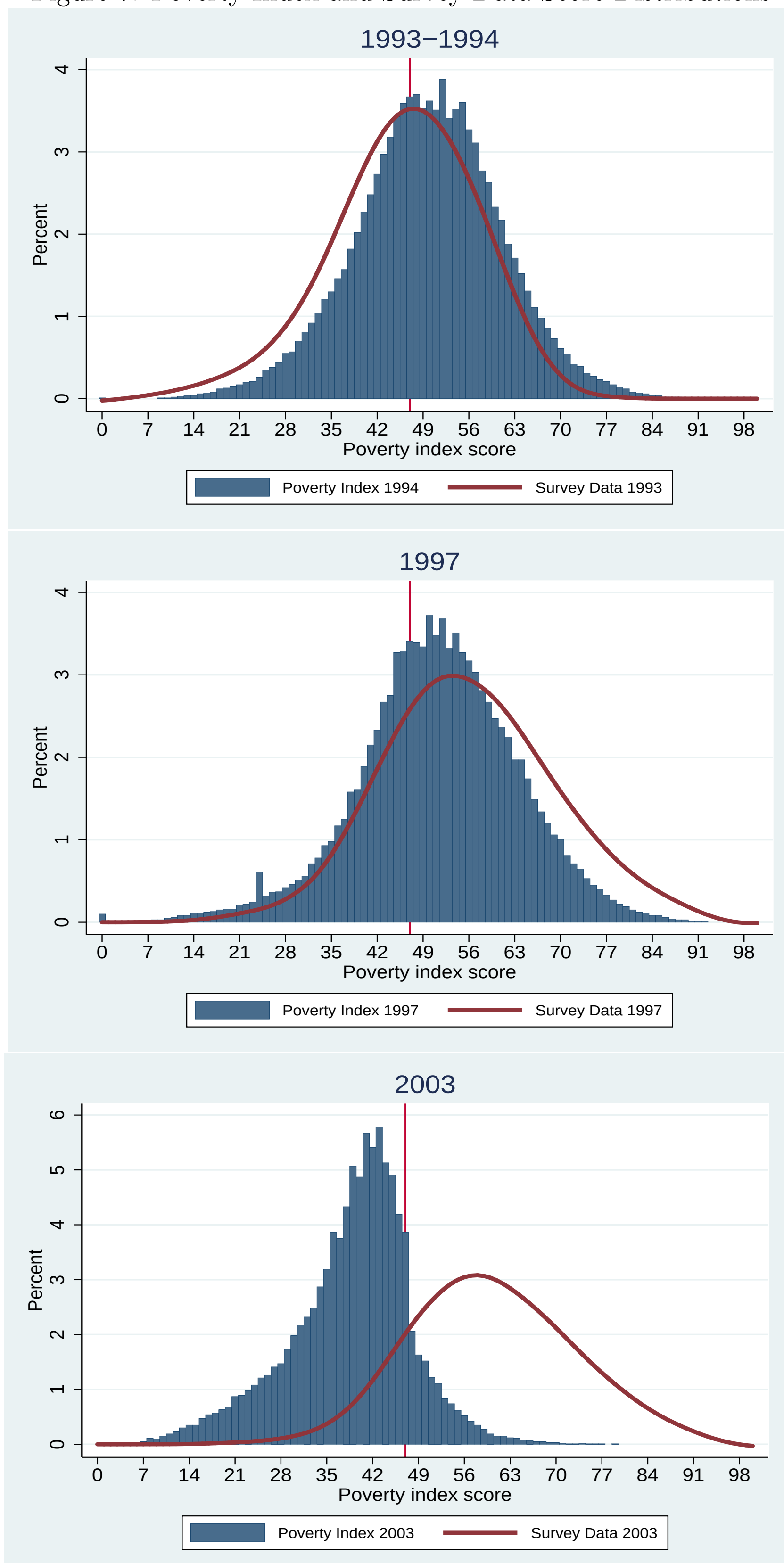

Figure 8: Poverty Index Surveys by Stratum, controlling for Municipality

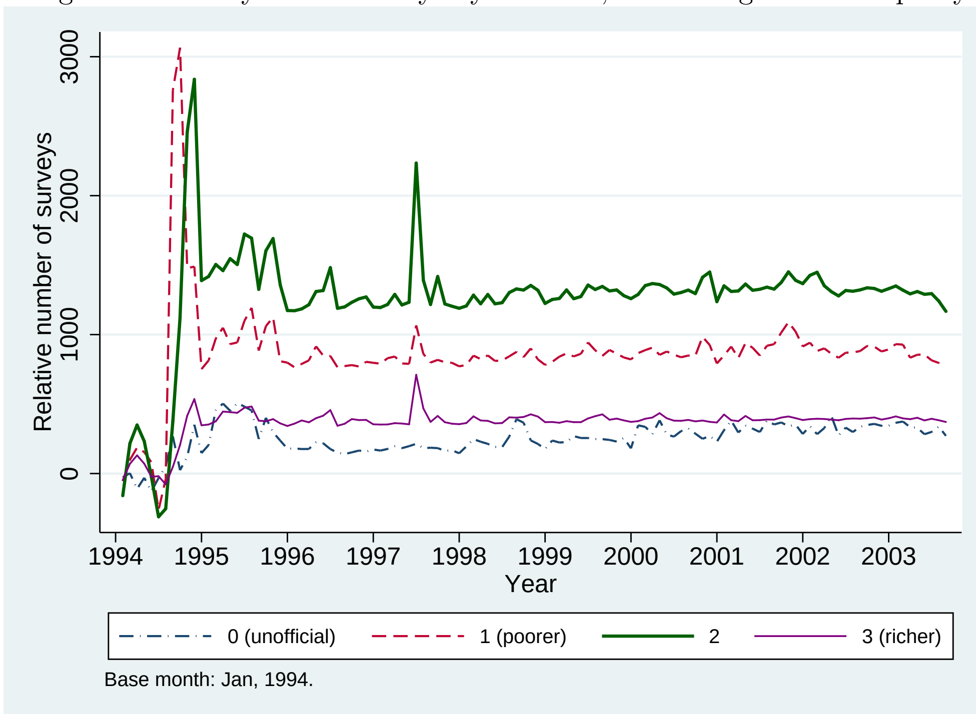

Table A1: Appendix. Poverty Index Score Algorithm

Description	Weight
Education	
Education of the highest wage earner	
Without education	0
Incomplete primary school	1.6239
Complete primary school	3.4435
Incomplete secondary school	5.0039
Complete secondary school	7.3434
Incomplete college	9.7833
Completed college	11.546
Post-graduate	12.4806
Avg. education of household members older than 11 years	
Without education	0
(0, 4]	1.657
(4, 5]	2.9947
(5, 10]	4.969
(10, 11]	7.6387
(11, 15]	9.4425
(15, 16]	10.69
More than 16 of schooling	11.1396
Social security of the highest wage earner	
No social security and self-employed or not working	0
No social security and works in firm of 2-9 workers	1.166
No social security and works in firm of 10 or more workers	2.6545
With social security and self-employed or not working	3.9539
With social security and works in firm of 2-9 workers	5.8427
With social security and works in firm of 10 or more workers	6.9718
Housing	
Wall materials	
No walls, bamboo	0
Zinc, cloth, cardboard, metal etc.	0.2473
Unpolished wood	2.0207
Mud	4.8586
Adobe	6.2845
Rock, bricks or blocks	7.7321
Roof materials	
Straw	0
Recycled materials (cardboard, metal, etc)	2.1043
Tiles, zinc (without a ceiling)	3.7779
Tiles, zinc (with a ceiling)	5.0973
Floor materials	
Dirt	0
Unpolished wood	2.9037
Cement	3.6967
Tiles, vinyl or bricks	5.8712
Rugs, polished wood, marble	6.8915
Number of appliances that the household owns	
None	0
Up to 3 basic appliances	2.1435
4 basic appliances without a washer	3.0763
3 to 4 basic appliances with a washer	4.7194

Source: Colombia's National Planning Agency, (DNP).

Table A1: Appendix. Poverty Index Score Algorithm (Cont.)

Description	Weight
Demographics	
Children to family size ratio	
More than 0.65	0
(0.0, .65]	0.2237
No children	1.4761
Employed to family size ratio	
Less than 0.30	0
(0.30, 0.60]	0.6717
(0.60, 0.90]	1.739
More than 0.90	4.0149
Room crowdedness	
Less than 0.20	0
(0.20, 0.30]	0.5584
(0.30, 0.40]	1.6535
(0.40, 0.70]	2.5727
(0.70, 1.00]	4.3886
(1.00, 4.00]	6.0042
More than 4.0	8.3828
Income percapita relative to the minimum wage	
Less than 0.15	0
(0.15, 0.25]	0.8476
(0.25, 0.35]	2.1828
(0.35, 0.50]	3.5362
(0.50, 0.75]	5.3636
(0.75, 1.00]	7.0827
(1.00, 1.25]	8.2489
(1.25, 1.50]	9.4853
(1.50, 2.00]	10.2098
(2.00, 3.00]	11.3999
(3.00, 4.00]	13.0872
More than 4.0	13.7378
Utilities	
Water source	
River or spring	0
Public well/pool or other source	1.1601
Well without a pump	2.6497
Well with a pump	4.6037
Truck	6.1693
Water/sewage system	7.2554
Type of toilet facilities	
No toilet facilities	0
Latrine	2.4519
Toilet without connection to water source	3.3323
Toilet connected to a well	3.9615
Toilet connected to sewage	6.8306
Waste collection and disposal	
Throw it to a lot	0
Take it to a container	2.1291
Picked by garbage collection services	3.2701

Source: Colombia's National Planning Agency, (DNP).