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

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Abstract

We document manipulation of a targeting system which used a poverty index score to determine eligibility for social welfare programs in Colombia, including health insurance. We show strategic behavior in the timing of the household interviews around local elections, and direct manipulation when some households had their eligibility scores lowered. Initially the number of interviews increased around local elections. After the algorithm was made public to local officials, the score density exhibited a sharp discontinuity *exactly* at the eligibility threshold. The discontinuity at the threshold is larger where mayoral elections are more competitive; and smaller in municipalities with less competitive elections, more community organizations and higher newspaper circulation.

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1 Introduction

Faced with scarce resources, developing countries have increasingly used targeting methods to maximize the impact of their programs for the poor.[1] Developing countries are also often marked by fragile political institutions and corruption. Although many studies documented corruption (Reinikka and Svensson, 2004, Bertrand et al. 2006, Hsieh and Moretti, 2006, and Olken, 2007), there are no papers that focus on the interaction between targeting methods and political manipulation in developing countries. That is what we do in this paper. We contribute to the literature by identifying manipulation of a targeting tool used to identify beneficiaries of social programs, and relate this manipulation to the political process. Specifically, we use a comprehensive dataset of a poverty index score that identifies potential beneficiaries for a variety of social welfare programs in Colombia. 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 used 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. An unprecedented proxy-means testing targeting system was put in place. To identify the poor population the government carried out its Census of the Poor (known as the SISBEN I in Colombia).[2] 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 (least poor). This score was designed to measure long term living conditions, not transitory

[1] See Coady et al. (2004), Castañeda et al. (2005), and Paxson and Schady (2002) for their evaluations of different targeting interventions used around the world.

[2] SISBEN in Spanish stands for: System of Beneficiary Selection. See Castañeda (2005b) and Vélez et al. (1999) for a detailed description of the SISBEN system.

income shocks, and thus to properly identify the population most in need of assistance. The score is mapped into six different levels (level 1 is the poorest and level 6 the richest). Eligibility rules for several social welfare programs use a specific and known threshold from the poverty index score. The most common threshold was level 2, which maps to a score of 47 for urban families. Until 2002 the poverty index score was mainly used for health insurance subsidies (RS). People with scores at or below 47 were eligible to receive subsidized public health insurance. After 2002 for a broad range of publicly provided programs: housing improvement, food for the elderly, educational subsidies, but health insurance was still the largest program (Cárdenas, 2006).

The central government instructed municipal officials on how to target the population for the Census of the Poor with door-to-door interviews, but allowed municipalities discretion over the administration and timing of the interviews. Safeguards built into the system included the creation and distribution by the central government of the questionnaire and computer program that calculates the scores. 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, commonly known as the "old" or "first" SISBEN, implemented between 1994 and 2003. This dataset includes approximately 18 million individual observations in urban areas, with all the responses to the questions in the census, as well as the poverty index score of each family.

Despite the safeguards in the system, we see unusual patterns in the data suggesting that the 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. In the spirit of studies that use statistics to uncover evidence of cheating (Jacob and Levitt, 2003; Wolfers, 2006), we identified municipalities with relatively high proportions of families that had almost identical 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 whether 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. Nevertheless, we find that for a few municipalities there is a high concentration of scores at zero, even though the interview answers cannot generate a zero score.

There are several ways in which the manipulation of the Census of the Poor may occur: people being surveyed can lie, the enumerators or data entry person can change the answers, or a person in a position of power (e.g. a politician) can instruct someone to change the score. One type of manipulation does not exclude another, and there is anecdotal evidence that different types of manipulation occurred.[3] Although each type of manipulation can undermine the system, in this paper we focus on political manipulation because uncovering it has concrete policy implications. Newspaper articles suggest that manipulation took place at the local government level.[4] 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 or his/her party because it increases the probability of voters' inclusion in these welfare programs, something that 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

[3]For instance there are stories of people moving or hiding their assets, or of borrowing and lending children.

[4]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". See appendix A for other press articles on electoral manipulation.

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 time. This behavior is not necessarily corrupt, but it is strategic. Over time, however, people became aware that instead of interviews, a score at or below level 2 (or score 47) was necessary for program eligibility. After the score algorithm was released, a sharp discontinuity of the score density emerged *exactly* at 47, the cutoff threshold for level 2.

We test some of the predictions implied by the model using data from mayoral elections. As predicted, we find that in municipalities with more competitive elections, and thus with higher benefits to the incumbent of an additional vote, 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. We also find that, as the model predicts, more monitoring is negatively correlated to the proportion of surveys conducted in pre-electoral periods. These findings contribute to the growing literature explaining how politicians in developing countries use pre-electoral manipulation to influence election outcomes (Khemani, 2004; Drazen and Eslava, 2005; Ferraz, 2007). And in addition, this paper is unique in relating pre-electoral manipulation with targeting systems for social programs.

We 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 is no incentive for manipulation, is smooth. Finally, since municipalities had discretion over when they make 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. In addition, we estimate a weighted average of a municipal level poverty index and find that over time the composition of the proportion of poor, in the municipalities that conducted surveys, did not decline, indicating that the pattern is not driven by the composition of municipalities over time.

Government social program spending in Colombia doubled from 1992 to 1996[5]. Most of these social programs use the poverty index score from the Census of the Poor to identify beneficiaries. Whereas the poverty index score is intended to identify the population most in need, cheating along the lines documented here actually takes resources from them, and is costly for society. Ecuador (with the SelBen) and Chile (with the CASEN) 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, by providing a real and wide-spread case we add to the literature that emphasizes the importance of taking into account the possibility of sorting when evaluating programs that use proxy-means tested targeting.[6] 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 targeting tools should consider behavioral responses from individuals and politicians. Specifically, the design of the Census of the Poor could, in theory, allow the use of regression discontinuity (RD) by exploiting the

[5]Data from Colombia's Statistical Agency DANE and Colombia's National Planning Agency DNP.

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

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 when this sorting occurs but is ignored.[7]

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, and 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

[7]Related studies which have cautioned against using regression discontinuity designs in different contexts include Lee (2006), McCrary (2008), Urquiola and Verhoogen (2007). In the Colombian case see Camacho and Conover (2010) for an analysis of the effects of public health insurance (SR) on newborn health before and after there is evidence of manipulation in the poverty index score.

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. We exclude from our working sample people living in neighborhood strata level four or above.[8] This leaves approximately 18 million individuals that represent 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, have smaller dwellings, and generally less education. 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 is a weighted average of answers to the Census of the Poor. Appendix Table A1 shows the algorithm for calculating the poverty index score. 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

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

to calculate the overall 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 areas. Survey data for 1997 and 2003 come from the *Quality of Life Surveys*, collected by the Colombian National Administrative Department of Statistics (DANE).[9] 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.[10] 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 0.15. This translates into each person in the family receiving 0.15 (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)[11] for people with scores from 75 to 100. Table

[9]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).

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

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

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 are able to create a measure of political competition for 1997, 2000 and 2003. 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 elections. 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. An article by Chávez (2006) explains the key role that community organizations play in improving and monitoring

the transparency of public resources. 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.[12] 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 (NBI in Spanish) 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 all types of manipulation could be detrimental to the system, we focus on political manipulation because of its implications on undermining the political process and weakening democratic institutions.[13] Manipulation during or after the data entry stages involves changes to the

[12] See Schulhofer-Wohl and Garrido (2009) for a recent article on how a decrease in newspaper circulation affected the number of candidates that ran for office, the probability of the incumbent winning an election and voter turnout in Cincinnati.

[13] Bottia et al. (2008) document manipulation by individuals using the second Census of the Poor.

answers in the questionnaire, in a specific component, or in 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, which is consistent with manipulation occurring in a "centralized" way. 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 show 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. In particular, the spike is more noticeable prior to the 1997 election. There are no obvious spikes in the number of surveys conducted after 1998.

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

[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. Additionally, as reported in the note of Figure 1 we estimate the discontinuity at the threshold using local linear regressions with a rule-of-thumb bandwidth suggested by Fan and Gijbels (1996).

3.2 Evidence of Manipulation

One way to change the poverty index score is simply by 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 breakdown 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.

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 almost all 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 4 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.

With this methodology we were able to identify for example, a municipality that on a single day interviewed approximately 45,000 individuals from different neighborhoods, but who all 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. Having the same supervisor and coordinator is consistent with centralized manipulation and not manipulation from individuals copying

[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.

answers from their neighbors,[16] or enumerators "selling" answers to the households.[17]

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.

[16]This is because the answers came from different neighborhoods, and the scores are exactly the same. To get the same scores it is necessary that the demographics of the household including household composition, and age structure are (almost) the same, and the observable dwelling characteristics would also need to be (almost) identical. This is highly unlikely and if respondents are dishonest enumerators can detect lies during the interview for observable dwelling characteristics. Additionally because respondents needed to provide national id cards, birth certificates or other forms of documentation, demographic information is also corroborated.

[17]Supervisors, coordinators, data entry people or someone higher up is likely to notice that 45,000 people are ending up with almost exactly the same answers for a detailed questionnaire with more than 30 questions.

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

4.1 Theoretical Framework

In this section we provide a theoretical framework to motivate our empirical findings, 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).[18] Let the 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:

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

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

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

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. This applies only during the first time the voter is 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 $pb_{s_i} \mathbb{I}[s_i > s_0]$ 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. ϕ is a parameter that indicates the relative dispersion of the individual preferences for the candidate. θ is an aggregate shock to the population's preferences, realized after the parties commit to policies. $\theta \sim U\left[\frac{-1}{2\psi}, \frac{1}{2\psi}\right]$. ψ is a parameter that indicates the relative dispersion of the population's preferences for the candidate.

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

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

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:

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

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 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:

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

In contrast, the challenger cannot conduct surveys or cheat before the election.[19]

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

[19] 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. In Colombia, creative ways that have been used include a system know as the "carousel." Electoral officials at each voting table sign each ballot when the voter first comes to the table. A ballot which is not signed by the table official is considered invalid. To get the carousel going, a person needs to insert in the ballot box an unsigned ballot and keep the signed ballot. This signed ballot is then given to a vote-buyer-coordinator. The vote-buyer-coordinator marks the ballot and asks the next person to deposit the marked ballot and return the signed ballot that they will get at the table to the voter-buyer-coordinator, the carousel goes on, and the contracts are enforced. For a description of this system see "How to buy a vote in Colombia" *Cómo se compra un Voto en Colombia* El Tiempo. 20 June 1998. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-790679#. Accessed 29 July 2009. Another way in which the contract can be enforced is by exploiting the timing of enrollment into the SR. Households first need to get surveyed, then get an id card, and finally enroll in the SR. The delivery of the id card or enrollment into the SR could be jeopardized if the households renege on their promise to vote for the incumbent's candidate of choice.

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:

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

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

Some of the results we obtain from this set-up include an inverse relationship between the costs and the amount of cheating, $\frac{\partial p}{\partial c} < 0$; and between the costs and the amount of conducting surveys $\frac{\partial n^I}{\partial c} < 0$. 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. And although timing the surveys around election periods is not in itself corrupt, it does correspond to strategic behavior. 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.[20]

Before proceeding to the empirical results we should note that mayors in Colombia can not be re-elected for consecutive terms. However mayoral electoral manipulation was widely documented in the press during the period we study.[21] In addition, Drazen and Eslava (p.18, 2005) explain that "there are two reasons why an incumbent mayor who cannot run for re-election has incentives to manipulate fiscal policy at the end of his term of office. First, an incumbent knows that his decisions affect his party's re-election chances (or those of the incumbent's preferred candidate). Second, officials usually run for election to other posts in later years, or for re-election to the same post in the future, and their actions while in office are used by voters in future elections to assess their preferences and competence."[22]

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. We exploit variation both within and across municipalities. A challenge encountered by scholars studying corruption is how to measure it. We contribute to this literature by developing a measure of manipulation which uses the size of the discontinuity at the threshold.

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 standardized measures of the discontinuity at the threshold for each municipality on a standardized measure of

[20]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.

[21]See appendix B for a list of articles that document electoral manipulation.

[22]In another study we are looking at whether "corruption pays," or whether politicians who cheat are more or less likely to get a political position in the future.

competitiveness of the election. The regression equation has the 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 (May-October) because in Colombia political campaigns con only be conducted during the 6 months prior to the elections. 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).[23] 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. Our regression results report standardized values for all variables. 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. Following the literature, 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.

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.

[23] We use 3 and 5 points from the threshold because we want some values sufficiently closed to the threshold where there is data for many municipalities. As robustness, we also used a different definition of "cheating" by looking at the proportion of poor below the threshold relative to what the Unsatisfied Basic Needs Index (a different measure of poverty) would predict. The results (not reported but available upon request) that we obtained are consistent to those reported in the paper. The limitation of this alternative measured of "cheating" is that it does not vary over time since the Unsatisfied Basic Needs Index is calculated from the population Census which is conducted every 10 years.

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 additional controls to the municipality and year effects, all other columns include population controls. Columns (1)-(4) of Table 5 use the fraction of surveys three points below and above the threshold, while columns (5)-(8) use the fraction of surveys 5 points below and above the threshold. A standard deviation increase in the amount of political competition (s.d.= 0.178) increases the percent of interviews three points below the threshold relative to three points above the threshold by 0.18 of a standard deviation, and it increases the percent of interviews five points below the threshold relative to three points above the threshold by 0.17 of a standard deviation. The magnitude of the effects remain constant after including population controls.

If politicians are using the Census of the Poor to influence the election outcomes, then we expect manipulation to be more prevalent just before the elections. As a "placebo" test we explore whether the competitiveness of the election influences the size of the discontinuity on non-electoral periods. We construct the variable $discontinuity_{jt}$ using data for: (1) months 12 to 6 prior to the election (November of the previous year to April of the election year), and (2) using the same six months of the year (May-October) but one year before the election. Results are reported in Table 6. We find that unlike the results reported in Table 5 which use data for 6 months prior to the election, the political competition does not influence the size of the discontinuity at the threshold.

Next we exploit variation across municipalities. 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 proportion of surveys is smaller when the costs of cheating are higher has the following form:

$$\begin{aligned} proportion\_surveys_{jt} &= \alpha + \beta_1 costs_j + \beta_2 lnpop_{jt} + \beta_3 demography_{jt} \\ &+ \beta_4 geography_j + \eta_t + \epsilon_{jt} \end{aligned} \tag{10}$$

Where the dependent variable *proportion_surveys*, is the fraction of annual surveys conducted in the 6 months prior to the election. *costs* is either the number of community organizations or the daily average newspaper circulation from May to October in each municipality.[24]

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:

$$\begin{aligned} discontinuity_{jt} &= \alpha + \beta_1 costs_j + \beta_2 lnpop_{jt} + \beta_3 demography_{jt} \\ &+ \beta_4 geography_j + \eta_t + \epsilon_{jt} \end{aligned} \tag{11}$$

Where the dependent variable *discontinuity*, again proxies for the amount of cheating in a municipality. *costs* are defined as in equation 10.

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*

[24]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

(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 report standardized results. We find that the coefficients have the expected signs, consistent with the idea that better monitoring is associated with a lower fraction of surveys in the 6 months before the election and less cheating in municipalities around election times. Columns (1) and (2) of Table 7 indicate that a standard deviation increase in the number of community organizations or newspaper circulation (s.d. = 0.325 and 0.032 respectively) are associated with a lower fraction of surveys conducted before the elections of 0.03 of a standard deviation. Columns (1)-(4) of Table 8 use the fraction of surveys three points below and above the threshold, while columns (5)-(8) 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 are associated with a lower percent of interviews three points below the threshold relative to three points above the threshold of 0.04 of a standard deviation. The corresponding numbers for five points below the threshold are 0.07 and 0.08 for community organization and newspaper circulation respectively. 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 Table 8. Larger municipalities have a smaller discontinuity at the threshold. This could indicate that smaller, less technically advanced municipalities tend to have more corruption. In the next section we rule out alternative explanations for the observed score manipulation.

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.[25]

Another explanation for what could be generating the pattern in the score distribution over time could be changes in general macroeconomic or labor market 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 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

[25]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.

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 address 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.[26] 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 in 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.[27] The Census of the Poor distribution lies slightly to the right of the 1993 household survey distribution. The second and third graphs in 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

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

[27] 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.

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.[28] 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.

We first check for the possibility that municipalities conducting surveys are poorer over time. We do this by using a measure of poverty at the municipal level called the Unsatisfied Basic Needs Index (NBI in Spanish). This index is provided by the Colombian National Statistical Agency (DANE) and takes a value between 0 and 100. The higher the value,

[28]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.

the larger the fraction of poor in the municipality. We estimate a weighted average of this index, by taking the proportion of surveys conducted in each municipality in a given month, and multiplying this value by the Unsatisfied Basic Needs Index for that municipality and year. The results are presented in Figure 8. The figure shows a declining proportion of poor over time, this relationship however is not significant, indicating that the composition of the proportion of poor in the municipalities conducting surveys did not decline over time.

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.[29] 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:

$$surveys\_stratumx_{jt} = \alpha + \eta_t + \gamma_j + \epsilon_{jt} \tag{12}$$

Where $surveys\_stratumx$ corresponds to the number of surveys conducted in stratum level x. In Figure 9 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.

An alternative way to test for selection within a municipality and over time, is to use the geographical information from the 1993 Population Census (prior to the implementation of the Census of the Poor) and see how the characteristics of people in neighborhood blocks interviewed earlier compare to those interviewed later. If the characteristics of people in neighborhood blocks interviewed later are worse than those interviewed earlier then we should

[29] We did this because the central government instructed municipal officials to use strata levels in targeting.

expect a left-ward shift in the distribution and be concerned about the possibility of selection. In the appendix, we do this for one municipality, which we call municipality X, with different patterns in the poverty score distribution before and after 1998.[30] Municipality X exhibits a sharp discontinuity at the eligibility threshold in the post 1998 period, see appendix Figure A1. Using the Census of the Poor we identify neighborhood blocks where interviews were conducted before 1998 and blocks where interviews were conducted after 1998. We take this information to the 1993 Population Census to see if the average characteristics of people interviewed after 1998 are worse than those interviewed before 1998. Accounting for the number of households interviewed in each block we graph the mean characteristics of the people interviewed in the pre and post 1998 period. Overall, the neighborhoods interviewed later do not look worse than the neighborhoods interviewed earlier as seen in appendix Figure A2. Even if this was the case, we would expect a leftward shift in the distribution and not necessarily a sharp discontinuity exactly at the threshold.

Overall the results presented in this section indicate that the score algorithm, changes in economic conditions or selection do not explain why after 1998 we see a discontinuity exactly at the eligibility threshold. Although alternative explanations not explored in this section due to space or data constraints could be proposed for the pattern observed in the Poverty Index Score distribution, in order for these explanations to be relevant, they would need to address not only the leftward shift in the distribution, but also the timing of the emergence of the discontinuity after the release of the score algorithm, and the sharp drop in the density of the distribution exactly at the eligibility threshold.[31]

[30] We checked this for 4 large municipalities, where we can rely on the geo-coded data used both in Census 1993 and Census of the Poor 1994-2003. This is a data-intensive exercise, and we are unable to conduct it for all municipalities given that the quality of the neighborhood coding information in the Census of the Poor varies greatly across municipalities, and it is often omitted.

[31] For instance, alternative explanations such as individuals misrepresenting themselves to reduce their score, enumerators "helping" out, or changes in the minimum wage might explain a leftward shift in the score distribution, but do not explain the timing of the emergence of the discontinuity at the threshold in

6 Summary and Discussion

In this paper we documented patterns in the data that indicate strategic behavior and manipulation during the implementation of the first Census of the Poor in Colombia. Not ruling out the possibility of individual manipulation, we identify mass manipulation following the data entry stages after the score algorithm was made available to local officials. In a "back of the envelope" calculation we estimate that from 1994 to 2003 approximately three million people had their scores changed, this corresponds to approximately 33% of what the survey data indicates should be the actual number of beneficiaries. Considering that during the period studied the total population of Colombia was approximately 40 million, the misallocation of three million of the poorest segment of the population is noteworthy. The corresponding financial impact is approximately 7% of the Health and Social Security budget.

We motivate our empirical findings with a theoretical framework that indicate how manipulation by politicians may have occurred. We tested 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.

Our contribution is that by using administrative data we identify manipulation of a large scale targeting system used to determine eligibility for social programs. In addition, we link this manipulation to the political process. This is relevant to officials in countries which use similar targeting systems to identify beneficiaries of social programs. We contribute to the corruption literature by developing a measure of manipulation which uses the size of the discontinuity in the poverty index score distribution at the threshold. And, we provide a real world and large scale case of the violation of a necessary assumption for identification when

1998, and the discontinuity emerging *exactly* at the threshold.

applying a regression discontinuity design methodology. Which, as Camacho and Conover (2010) show when estimating the effects of public health insurance on newborn health, can change the results of RD studies that do not take into account manipulation of the running variable.[32]

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, 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.[33] 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

[32]On this point, see related papers by McCrary (2007), Urquiola and Verhoogen (2007).

[33]See Robinson, 2005, for information on the historical presence of clientelistic relationships in Colombia. See Acemoglu et al (2009) for a paper on elections, violence and government policies in Colombia.

experience when designing or implementing their own programs. The Colombian government has already made important changes that help reduce manipulation in the implementation of the second Census of the Poor which started in 2003. The second 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 or assigning social benefits in pre-electoral periods in certain municipalities.[34] 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.

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

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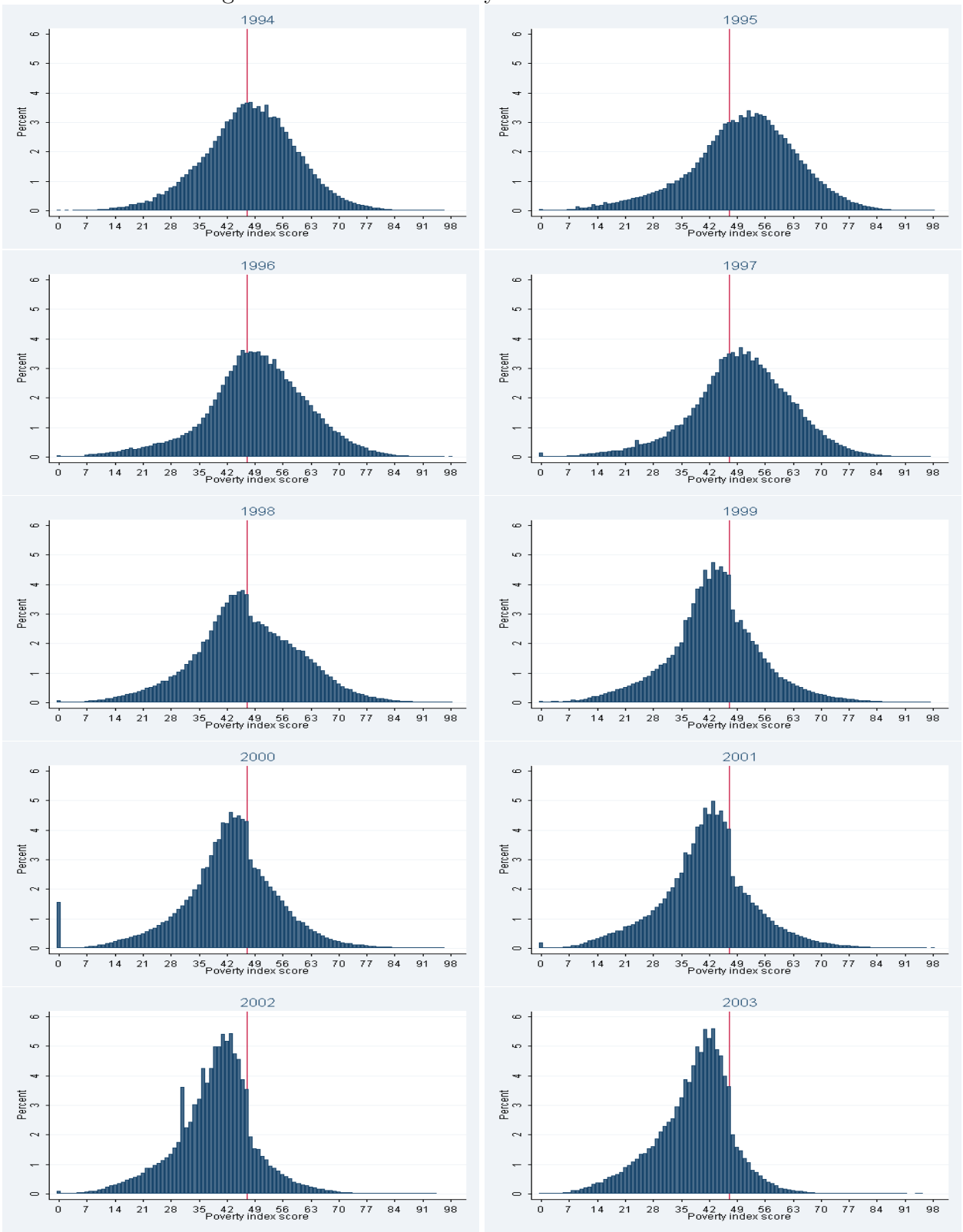
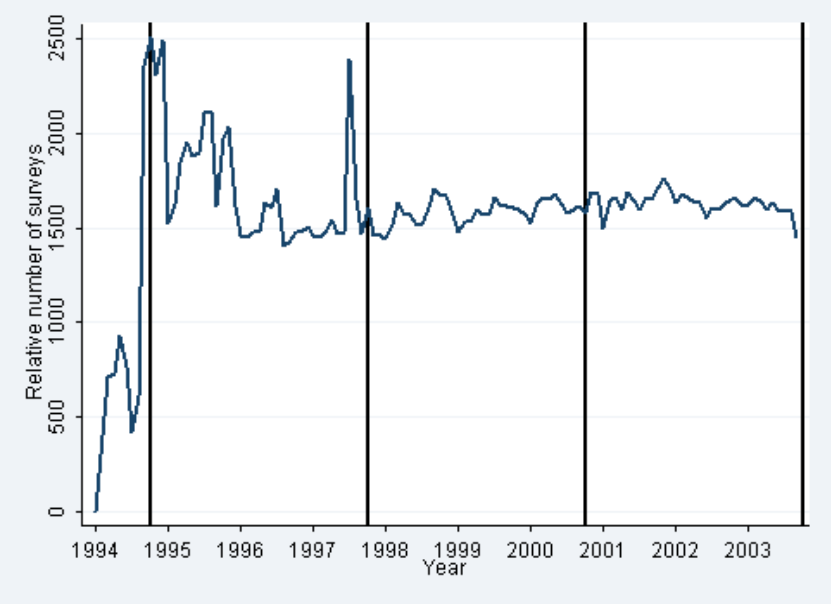
Figure 1: 1994-2003 Poverty Index Score Distribution



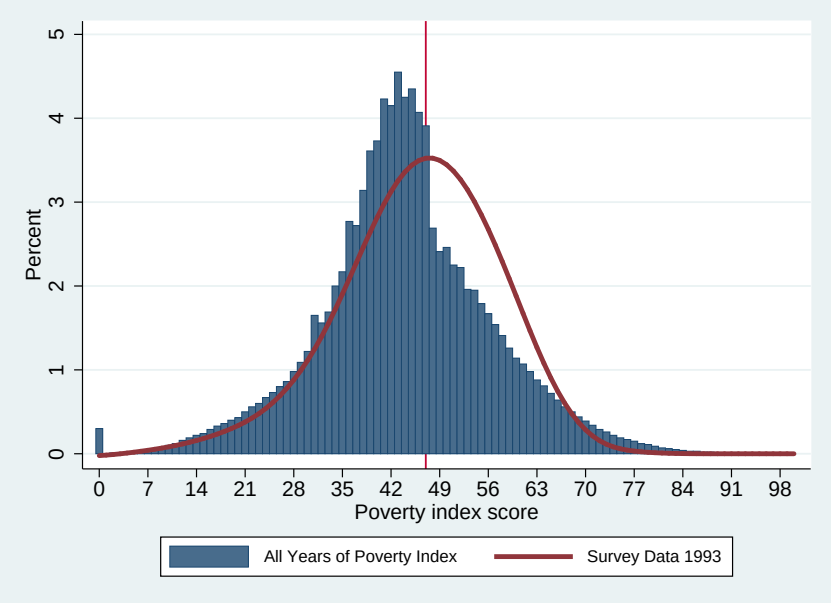
Note: Each figure corresponds to the interviews conducted in a given year, restricting the sample to households living in strata levels below four. Using local linear regressions and an optimal bandwidth algorithm we estimate the size of the discontinuity for each year as follows: 0.033 (1994); 0.080 (1995); 0.08 (1996); 0.024 (1997); 0.868***(1998); 1.209***(1999); 1.422***(2000); 1.683***(2001); 1.565***(2002); 1.547***(2003), where *** indicates significance at 1%.

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



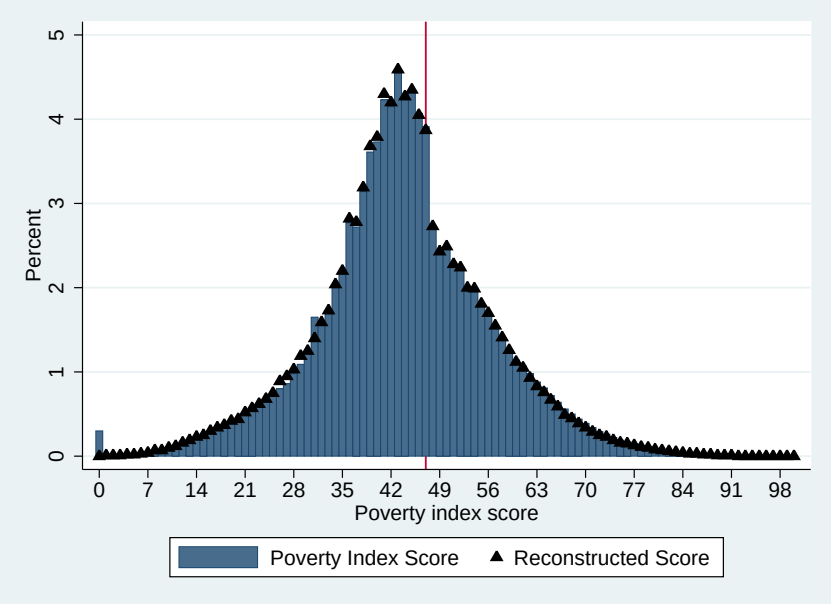
Note: Black line indicates regional mayoral elections. Results from coefficients of a regression of number of surveys per year month, on an indicator for each year month, controlling for municipality and strata level. Base month: January 1994.

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



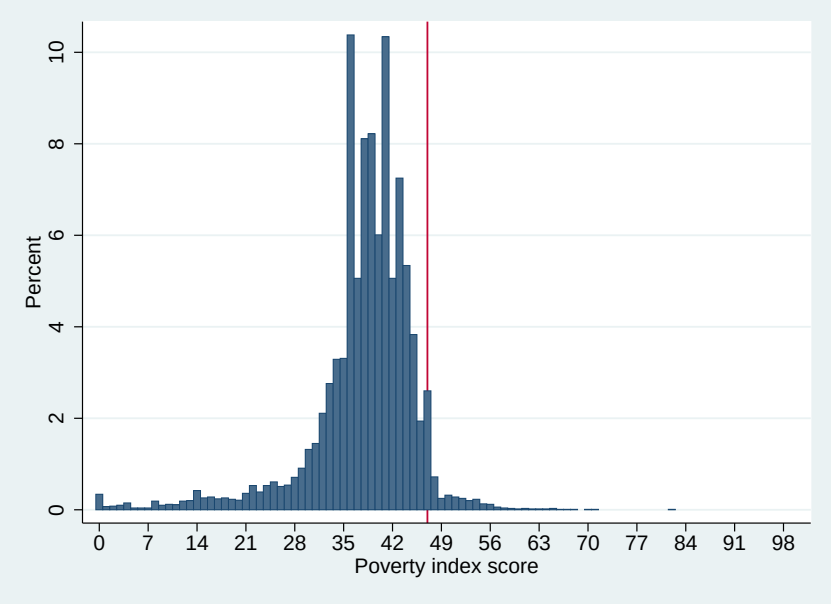
Note: Census of the Poor interviews for all years. Both the Census of the Poor and the survey data use only households living in strata levels below four.

Figure 4: Poverty Index Score and Reconstructed Score



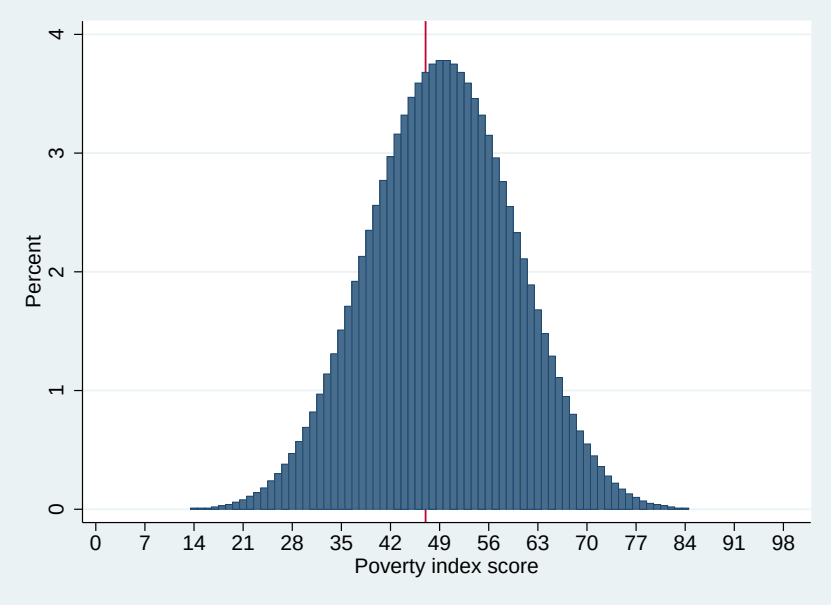
Note: Black triangles indicate the reconstructed Poverty Index Score using the score algorithm in Appendix table A1. Bars indicate the Poverty Index Score distribution as it appears in the Census of the Poor database.

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



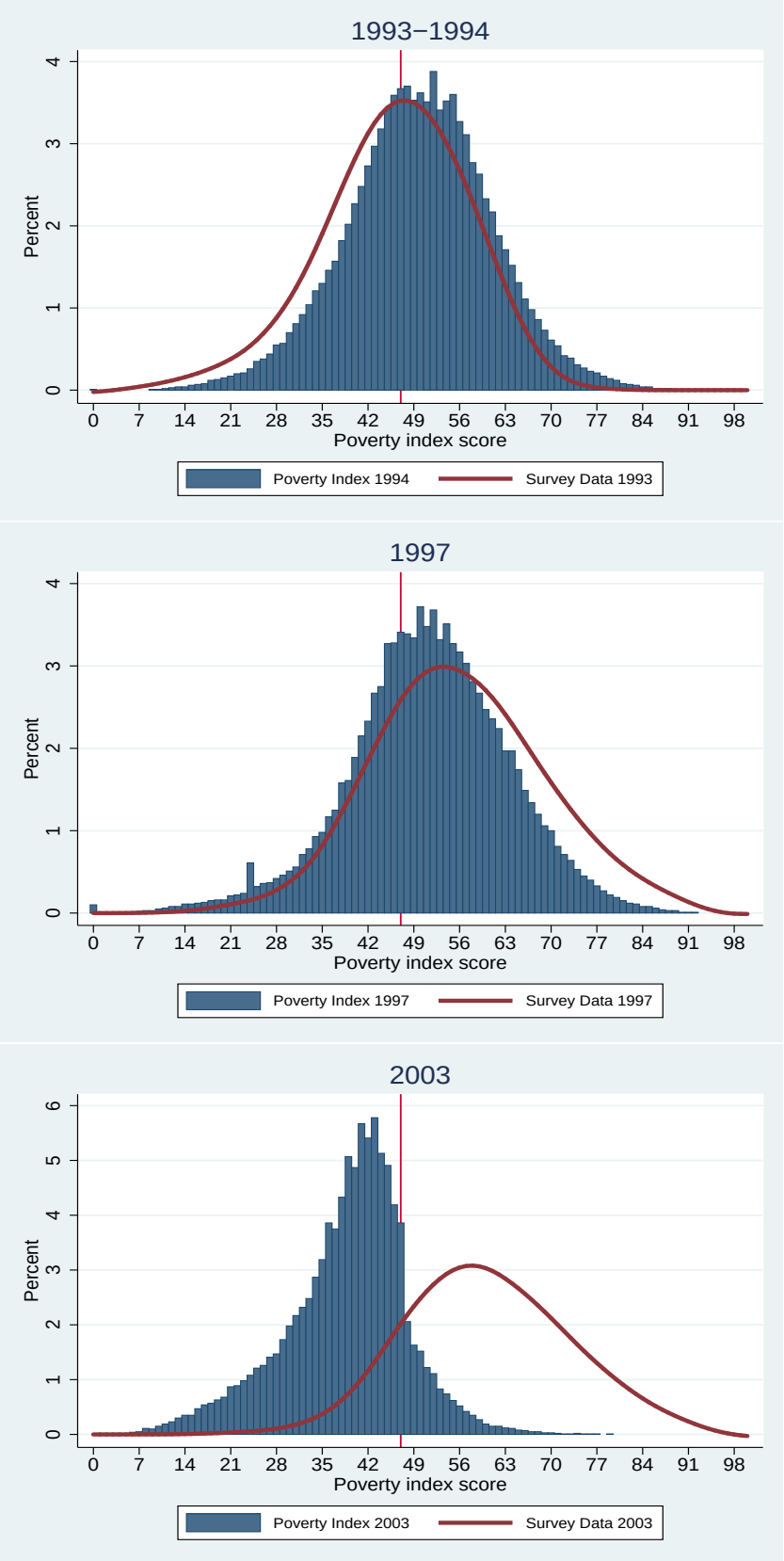
Note: Repeated answers corresponds to number of families with the exact same component score for all four components within a municipality and month.

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



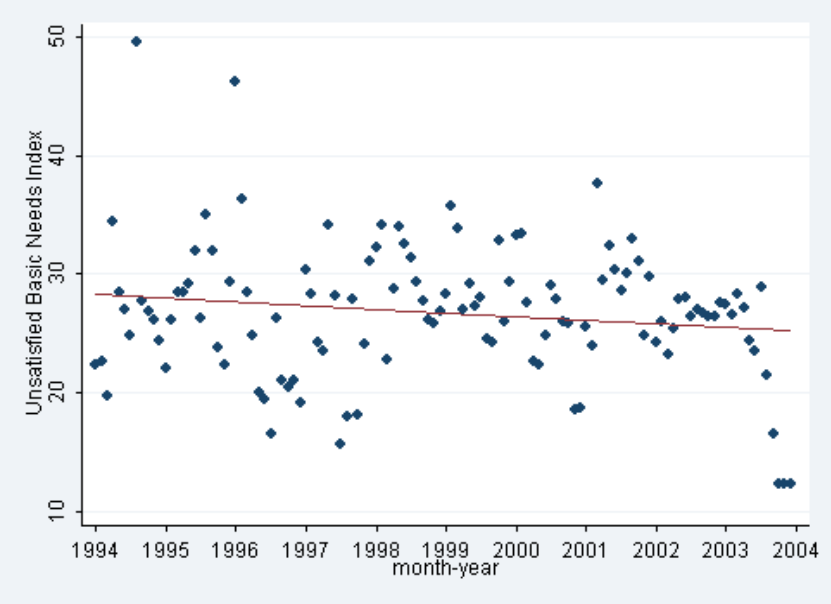
Note: Distribution of number of possible combinations to generate each score. Overall there are approximately 16 billion possible combinations to generate all scores. See section 5 for details.

Figure 7: Poverty Index and Survey Data Score Distributions



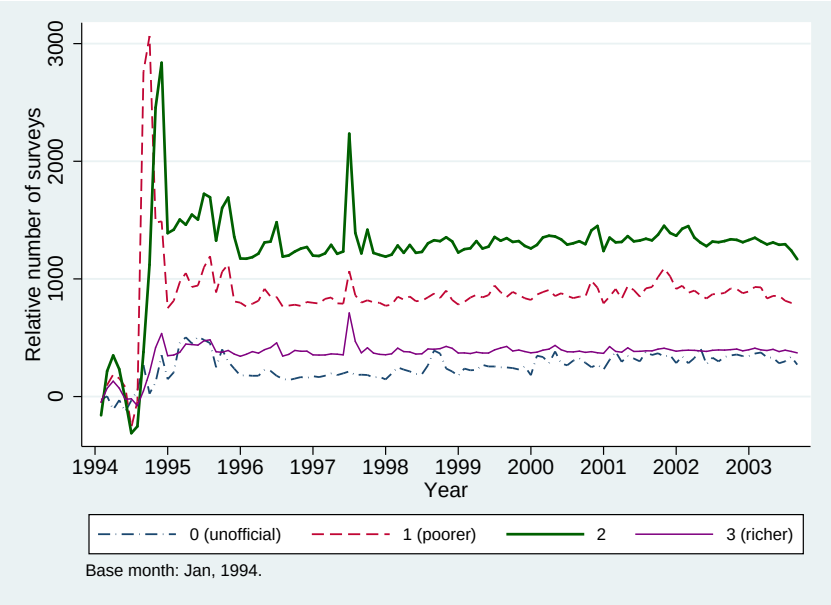
Note: Both the Census of the Poor and the survey data use only households living in strata levels below four.

Figure 8: Weighted Unsatisfied Basic Needs Index Over Time



Note: Each dot depicts a monthly weighted value for the Unsatisfied Basic Needs Index. The index captures the proportion of poor in a municipality, and it takes the values between 0 (richer) and 100 (poorer). The fitted line has a negative and insignificant coefficient.

Figure 9: Poverty Index Surveys by Stratum, controlling for Municipality



Note: Results from coefficients of a regression of number of surveys in each strata per year month, on an indicator for each year month, controlling for municipality. Base month: January 1994. See equation 12 in section 5 for details.

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

	Census of the Poor		Population Census	
Panel A	Mean or %	Obs.	Mean or %	Obs.
Individual Characteristics				
Age	25.67	17,946,125	26.37	2,325,747
% male	0.48	17,946,125	0.48	2,325,747
% not disabled	0.98	17,946,125	0.98	2,325,747
Highest Schooling (age>18)				
% None	0.12	1,190,407	0.06	77,850
% Primary	0.52	5,253,254	0.38	516,254
% Secondary	0.34	3,453,972	0.42	569,317
% College	0.02	248,601	0.13	172,703
% Post-college	0.00	11,260	0.01	19,226
Household Characteristics				
Household size**	3.42	5,241,212	4.17	537,317
Number of rooms in HH	1.90	5,241,208	3.56	537,317
Brick, rock or blocks walls	0.76	39,954	0.86	462,446
Dirt floors	0.11	5,571	0.06	33,324
Access to electricity*	0.98	51,350	0.96	513,655
Access to sewage	0.77	40,494	0.89	475,839
Trash disposal service	0.82	43,147	0.84	452,385
	Census of the Poor			
Panel B	% of HH			
Possessions				
Own a TV	0.52			
Own a refrigerator	0.33			
Own a blender	0.37			
Own a washer	0.04			
Observations	5,241,212			

Note: Panel A includes information available both in the Census of the Poor and the 1993 Population Census. Panel B includes only information available in the Census of the Poor. 1993 Population Census is a 10% random sample from IPUMS-international. We restrict both to urban areas only. *Different wording of question. **Different definitions. 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).

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	0.825	0.178	0.109	0.999
Discontinuity +/- 3 points	0.033	0.028	-0.111	0.148
Discontinuity +/- 5 points	0.037	0.023	-0.033	0.121
Proportion of surveys	0.511	0.252	0.000	1.000
Log population	10.045	1.136	7.731	15.678
Ratio of urban to total population	0.470	0.246	0.058	0.998
Unsatisfied Basic Needs Index (NBI)	0.380	0.169	0.001	0.929
Number of community organizations	0.056	0.325	0.002	5.944
Newspaper circulation	0.004	0.032	0.000	0.516
Distance to largest city in state (km)	101	83	0	548
Surface area of municipality (km2)	796	1,889	15	17,873

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. NBI (in Spanish) is a measure for the proportion of people in a municipality with 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. The number of community organizations is divided by 1000. Newspaper circulation corresponds to certified daily average circulation data by municipality for 2004 from Colombia's main national newspaper, *El Tiempo*. The number of newspaper circulation is divided by 100,000. 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	0.189**	0.184**	0.184**	0.177**	0.174*	0.174*
	[0.073]	[0.079]	[0.079]	[0.082]	[0.091]	[0.091]
Log population		10.862***	10.545***		8.104**	8.160*
		[3.524]	[3.965]		[3.494]	[4.245]
Ratio of urban to total			-1.42			0.249
population			[8.495]			[8.284]
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	132	132	132	132	132	132
R-squared	0.14	0.23	0.23	0.09	0.15	0.15

Note: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include an intercept term, and report standardized results. 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: Robustness: Discontinuity at the Threshold using Information months 12-6 prior to the election and 1 Year Prior to Mayoral Election

Dependent variable:	Discontinuity +/- 3 points			Discontinuity +/- 5 points		
	(1)	(2)	(3)	(4)	(5)	(6)
Political competition, calculating discontinuity	-0.024	-0.026	-0.027	0.048	0.045	0.044
using months 12-6 prior to the election (Nov-April)	[0.051]	[0.052]	[0.052]	[0.049]	[0.049]	[0.050]
Observations	470	470	470	470	470	470
R-squared	0.00	0.01	0.02	0.06	0.07	0.07
Political competition, calculating discontinuity	0.073	0.073	0.073	0.065	0.066	0.065
using 1 yr prior to election	[0.052]	[0.052]	[0.052]	[0.054]	[0.054]	[0.054]
Observations	480	480	480	480	480	480
R-squared	0.01	0.01	0.01	0.03	0.03	0.04
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes
Log population		Yes	Yes		Yes	Yes
Ratio of urban to rural			Yes			Yes

Note: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include an intercept term and report standardized results. 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 (1) months 12 to 6 prior to the election (November of the previous year to April of the election year), and (2) using the same six months of the year (May-October) but one year before 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. Each cell and row represents results from a different regression.

Table 7: Number of Surveys and Costs of Cheating (Cross Section)

Dependent variable:	Proportion of surveys			
	(1)	(2)	(3)	(4)
Number of community organizations	-0.029*** [0.009]	-0.030*** [0.009]		
Newspaper circulation			-0.033*** [0.012]	-0.034*** [0.012]
Proportion of poor	0.034 [0.035]	0.054 [0.041]	0.024 [0.044]	0.051 [0.051]
Log Population	0.051 [0.036]	0.060* [0.037]	0.046 [0.046]	0.048 [0.048]
Year effects	Yes	Yes	Yes	Yes
Demography controls	Yes	Yes	Yes	Yes
Geography controls		Yes		Yes
Observations	1295	1295	853	853
R-squared	0.07	0.07	0.07	0.07

Note: Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include an intercept term and report standardized results. The dependent variable is the fraction of annual surveys conducted in the 6 months prior to the election during election years. 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.

Table 8: 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	-0.042**	-0.038**			-0.075***	-0.071***		
organizations	[0.017]	[0.018]			[0.019]	[0.021]		
Newspaper circulation			-0.040**	-0.038**			-0.085***	-0.083***
			[0.018]	[0.019]			[0.020]	[0.020]
Proportion of poor	-0.227***	-0.264***	-0.222***	-0.247***	-0.228***	-0.262***	-0.205***	-0.212***
	[0.034]	[0.039]	[0.041]	[0.047]	[0.039]	[0.043]	[0.049]	[0.056]
Log Population	-0.160***	-0.155***	-0.188***	-0.175***	-0.111**	-0.103*	-0.129**	-0.114*
	[0.050]	[0.051]	[0.056]	[0.057]	[0.052]	[0.054]	[0.060]	[0.062]
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 and report standardized results. 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 during election years. 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.

A Appendix: Press Articles on Electoral Manipulation (not for publication)

"Arrestadas 49 Personas En Comicios" El Tiempo. 20 June 1994. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-155377. Accessed 7 August 2009.

"Denuncian Posible Fraude Electoral" El Tiempo. 11 March 1994. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-71134. Accessed 8 August 2009.

"Obispos Advierten Sobre Clientelismo" El Tiempo. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-226942. Accessed 8 August 2009.

"Ramírez, Alcalde Electo De Soacha" El Tiempo. 9 November 1994. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-247901. Accessed 7 August 2009.

"Demandadas Elecciones De 26 Alcaldes Populares" El Tiempo. 2 December 1994. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-258336. Accessed 7 August 2009.

"Alcaldes Investigados" El Tiempo. 22 March 1997. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-556887. Accessed 8 August 2009

"Demandarán Elecciones En Codazzi". El Tiempo. 4 November 1997. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-667574. Accessed 8 August 2009

"No cambié Carnets del Sisben por Votos" El Pais. Newspaper. 28 November 1997.

"Irregularidades en la Jornada Electoral" El Tiempo. 4 November 1997. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-694231. Accessed 7 August 2009.

"Juego de Manos" Semana. 30 March 1998. Magazine on-line. Available from

http://www.semana.com/noticias-nacion/juegos-manos/37073.aspx. Accessed 8 August 2009.

"Cómo se Compra un Voto en Colombia." El Tiempo. 20 June 1998. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-790679#. Accessed 29 July 2009.

"Piden Vigilancia para Elecciones en Cartagena" El Tiempo. 24 January 2000. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-1280701. Accessed 8 August 2009.

"Políticos Ofrecen Cupos en el Sisben a Cambio de Votos" El Pais. Newspaper. 13 October 2000.

"Procuraduría Alerta en Elecciones" El Tiempo. 27 October 2000. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-1259284. Accessed 8 August 2009.

"Denuncian Anomalías en las Pasadas Elecciones" El Tiempo. 23 November 2000. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-1231260. Accessed 8 August 2009.

"La Trampa Electoral" Semana. 8 July 2002. Magazine on-line. Available from http://www.semana.com/noticias-nacion/trampa-electoral/1273.aspx. Accessed 8 August 2009.

"Enriquecimiento a Costa de la Salud" El Tiempo. 30 December 2003. Newspaper on-line. Available from http://www.eltiempo.com/archivo/documento/MAM-1045041. Accessed 8 August 2009.

Table A1: Appendix (Not for publication). 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).

Figure A1: Poverty Index Score Distribution Pre and Post 1998, Municipality X

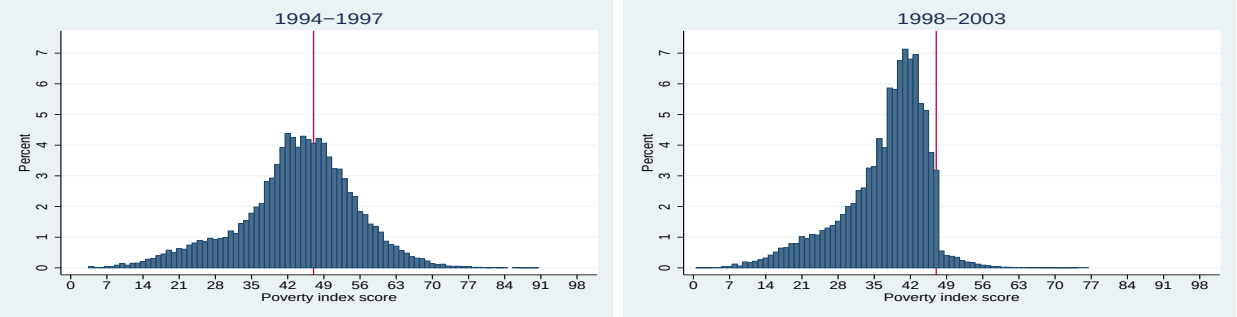


Figure A2: Characteristics of Neighborhoods using the 1993 Population Census, Muni. X