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Manipulation of Social Program Eligibility[†]

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We document how manipulation of a targeting system for social welfare programs evolves over time. First, there was strategic behavior of some local politicians in the timing of the household interviews around local elections. Then, there was corrupt behavior with the sudden emergence of a sharp discontinuity in the score density, exactly at the eligibility threshold, which coincided with the release of the score algorithm to local officials. The discontinuity at the threshold is larger where mayoral elections are more competitive. While cultural forces are surely relevant for corruption, our results also highlight the importance of information and incentives. (*JEL* D72, I32, I38, O15, O17).

Due to the high costs to society in terms of development and growth, addressing corruption has become a priority of governments and international institutions.[1] To effectively combat corruption it is necessary to understand its causes. Although causes are debated, culture is often identified as a key factor.[2] While cultural forces are surely relevant for corruption, our findings suggest that policy makers should also consider information, timing, and political incentives when designing instruments to allocate public subsidies.

Our paper documents the sudden emergence of a sharp discontinuity exactly at the eligibility threshold of a targeting instrument used to identify potential beneficiaries for a variety of social welfare programs in Colombia. The sudden emergence appears to be a consequence of the diffusion of information about the mechanism for resource allocation. In the four years following the introduction of the instrument, there was no apparent manipulation but, rather, strategic behavior by some politicians who timed

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[†] To comment on this article in the online discussion forum, or to view additional materials, visit the article page at http://www.aeaweb.org/articles.php?doi=10.1257/pol.3.2.41.

[1] For related work on corruption and development see Paolo Mauro (1995) and Pranab Bardhan (1997).

[2] See Mauro (2004), Johann Graf Lambsdorff (2006), Raymond Fisman and Edward Miguel (2007), and Abigail Barr and Danila Serra (2010).

the surveys right before elections. After the information was released, corruption, manifested by lowering poverty index scores, made some households eligible for subsidies. The cost of the corrupt behavior documented here is non-trivial, corresponding roughly to 7 percent of the National Health and Social Security budget.

In the early 1990s, when the Colombian government initiated an unprecedented proxy-means testing system, targeted social program spending became a priority. To identify the poor population the government carried out its Census of the Poor (known as the SISBEN I 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 (least poor). This score was designed to measure long term living conditions, not transitory income shocks, and thus to properly identify the population most in need of assistance. Eligibility rules for several social welfare programs use specific thresholds from the poverty index score. The most common threshold was a score of 47 for urban families.

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 the computer program that calculates the scores. 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 covers approximately 18 million individual observations in urban areas, with the responses to all the questions in the census, as well as the poverty index score recorded for each family.

Despite the safeguards in the system, we found unusual patterns in the data suggesting that not all the scores are accurate. In Figure 1, we document the sudden emergence in 1998 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,[4] we identified municipalities with relatively high proportions of families that had almost identical answers in a given month. Using the answers to the questions and the score algorithm, we also found that most of the manipulation was not due to overwriting the final score.

The Census of the Poor can be manipulated in several ways: the enumerators can change the answers, a person in a position of power (e.g., a politician) can instruct someone to change the score, or respondents can lie.[5] One type of manipulation does not exclude another. Although each type of manipulation can undermine the system, in this paper we focus on political manipulation. Newspaper articles suggest that manipulation took place at the local government level.[6]

[3] See Tarsicio Castañeda (2005), and Carlos E. Vélez, Elkin Castaño, and Ruthanne Deutsch (1999) for a detailed description of the SISBEN.

[4] See Brian A. Jacob and Steven D. Levitt (2003), Justin Wolfers (2006).

[5] There is anecdotal evidence of people moving or hiding their assets, or of borrowing and lending children.

[6] For example, in the newspaper *El País* the title of an article dated October 13, 2000 translates to "Politicians offer Census of the Poor interviews in exchange for votes." Other references for press articles on electoral manipulation are available in the online Appendix C.

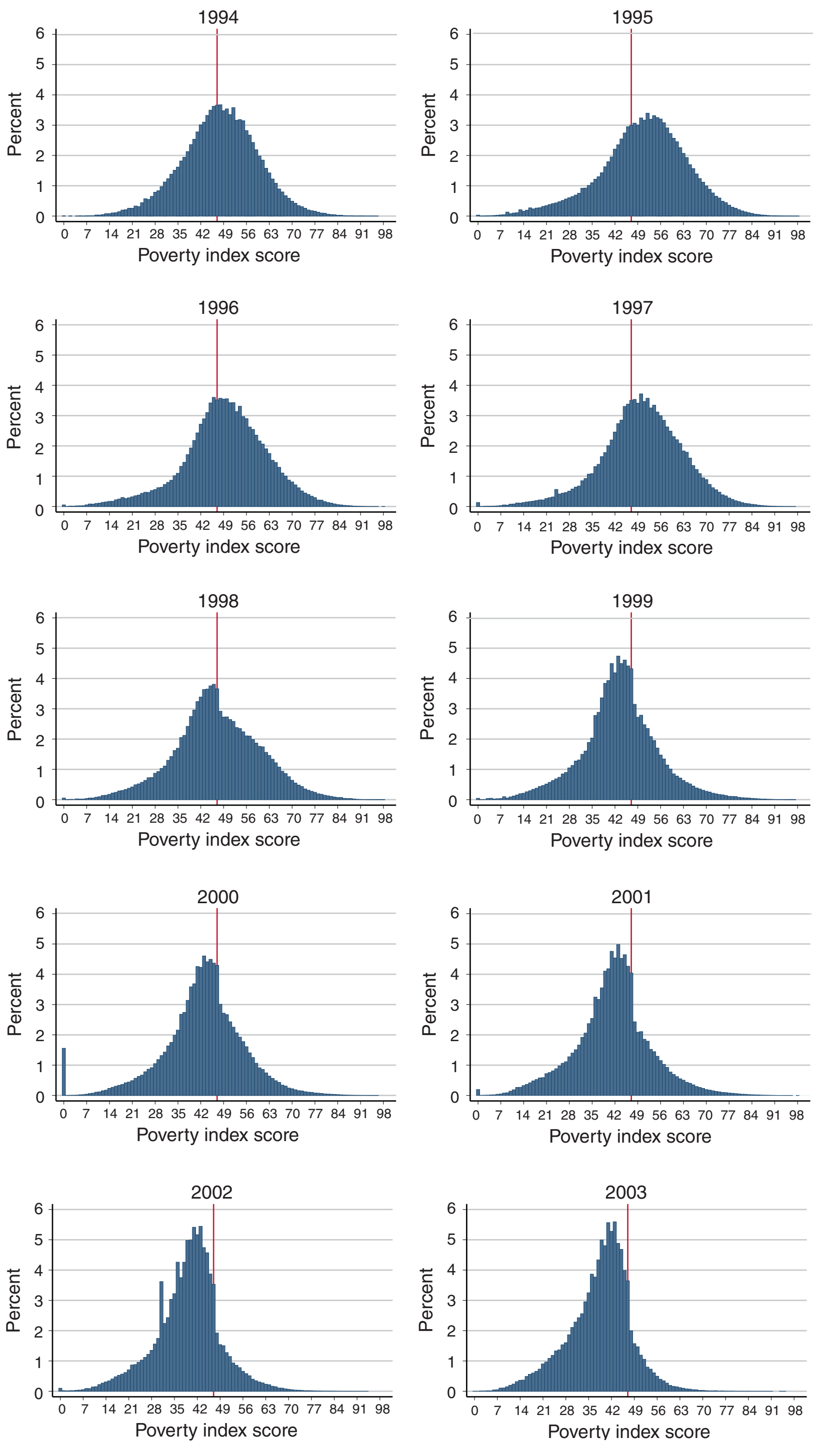

Figure 1. Poverty Index Score Distribution 1994–2003, Algorithm Disclosed in 1997

Notes: Each figure corresponds to the interviews conducted in a given year, restricting the sample to urban households living in strata levels below four. The vertical line indicates the eligibility threshold of 47 for many social programs.

The algorithm for the score was made available to municipal administrators sometime after July 1997, in an instructional presentation that was also distributed as a pamphlet.[7] Before diffusion of the score algorithm, the benefits of surveying for local politicians were high since people were confused about the eligibility criteria for the programs. They thought that having an interview was a sufficient condition for eligibility (Misión Social, Departmento Nacional de Planeación Ministerio de Salud, and Programa Naciones Unidas para el Desarrollo (UNDP) 2003, 153). Although there is variation across municipalities, during this period many local politicians were conducting a relatively large 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 below a certain cutoff was necessary for program eligibility. The timing of the release of the score algorithm to local officials coincides almost exactly with the appearance, in 1998, of a sharp discontinuity of the score density at 47, the cutoff threshold. After the score algorithm was released, we find that in municipalities with more competitive elections, and thus with higher benefits to the incumbent for each additional vote, the discontinuity at the poverty threshold is larger.

We assess whether alternative explanations could generate the observed patterns in the score distribution. To ensure that the changes in the distribution are not due to changes in macroeconomic conditions, we use other data where there is no incentive for manipulation, and find that the score distribution does not exhibit a clear jump at the eligibility threshold. We rule out the possibility of municipal officials getting better at targeting the poor by looking at the number of interviews conducted within poorer and richer neighborhoods over time and find that it remains relatively constant. We estimate a weighted average of a municipal level poverty index and find that over time the proportion of poor, in the municipalities that conducted surveys, did not increase, indicating that the pattern is not driven by the composition of municipalities.

Government social program spending in Colombia doubled from 1992 to 1996.[8] Most of these social programs use the poverty index score from the Census of the Poor to identify beneficiaries. There are few studies that quantify the costs of corruption;[9] we contribute to this literature by estimating the costs of corruption for a nationwide intervention and observed that these costs are significant. We estimate that approximately 3 million people (8 percent of the Colombian population at that time) had their scores lowered.

Our findings also add to the growing literature explaining how politicians in developing countries use pre-electoral manipulation to influence electoral outcomes.[10] Moreover, this paper is unique in relating pre-electoral manipulation with targeting systems for social programs. 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

[7] Colombia's National Planning Agency (DNP), July 1997 *"SISBEN: Una Herramienta Para la Equidad,"* PowerPoint presentation and pamphlet.

[8] Data from Colombia's National Administrative Department of Statistics (DANE) and DNP.

[9] See for example, Benjamin A. Olken (2006); Ritva Reinikka and Jakob Svensson (2004).

[10] Stuti Khemani (2004); Allan Drazen and Marcela Eslava (2010); Claudio Ferraz (2007).

proxy-means tested targeting.[11] Similarly to studies in the United States and the United Kingdom that, although not about corruption, have shown bunching behavior due to kinked budget sets,[12] evaluations of programs that use targeting tools should consider behavioral responses from individuals and politicians.

The paper is structured as follows: in section I we describe the datasets used in the study. In Section II we present evidence in support of the manipulation hypothesis. In Section III we use a political economy model to explain what could be generating the poverty index score discontinuity and test some of the predictions of the model with election data. In Section IV we present results showing that the changes in the distribution are most likely not driven by alternative explanations. We conclude in Section V.

I. Data

A. *Census of the Poor Data*

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

Colombia's neighborhoods are geographically stratified into six levels (strata), with 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 is 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 sample people living in neighborhood strata level four or above.[13] This leaves approximately 18 million individuals that represent roughly 40 percent of the total Colombian population. Of 1,102 municipalities, 785 have Census of the Poor records, and these municipalities account for 86.5 percent of the Colombian population.

Using the Socio-Economic Characterization Survey, representative at the national level, we estimate that the response rate for the Census of the Poor is approximately between 77 percent and 89 percent. This high response rate is supported by the way in which the surveys were conducted, which followed the same process as the population Census. Each enumerator was assigned specified blocks, and they were instructed to conduct the interviews in a clockwise fashion, house by house without skipping any residence. Supervisors then randomly checked to ensure that the interviews were completed appropriately.

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

[12] Leora Friedberg (2000); Richard Blundell and Hilary W. Hoynes (2004); Emmanuel Saez (2010).

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

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

	Census of the Poor		Population Census	
Panel A	Mean or percent	Obs.	Mean or percent	Obs.
Individual characteristics				
Age	25.69	18,176,019	26.37	2,325,747
Male	0.48	18,176,019	0.48	2,325,747
Not disabled	0.98	18,175,663	0.98	2,325,747
Highest schooling (*age* > 18)				
None	0.11	1,222,950	0.06	77,850
Primary	0.51	5,478,766	0.38	516,254
Secondary	0.35	3,711,856	0.42	569,317
College	0.02	256,427	0.13	172,703
Post-college	0.00	11,305	0.01	19,226
Household characteristics				
Household size**	3.44	5,310,308	4.17	537,317
Number of rooms in HH	1.89	5,310,304	3.56	537,317
Brick, rock or blocks walls	0.84	4,436,999	0.86	462,446
Dirt floors	0.11	562,147	0.06	33,324
Access to electricity*	0.98	5,203,646	0.96	513,655
Access to sewage	0.90	4,801,232	0.89	475,839
Trash disposal service	0.87	4,619,680	0.84	452,385

	Census of the Poor
Panel B	Percent of households
Possessions	
Own a TV	0.52
Own a refrigerator	0.33
Own a blender	0.37
Own a washer	0.04
Observations	5,310,308

Notes: 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 percent random sample from the Minnesota Population Center (Minessota Population Center 2007). 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).

** Different definitions.

* Different wording of question.

The Census of the Poor 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 to cover the poor population.

Panel A in Table 1 shows summary statistics for the Census of the Poor and a 10 percent sample of the 1993 Population Census from the Minnesota Population Center (Minnesota Population Center 2007). The 1993 Population Census includes all urban socio-economic strata levels, while the Census of the Poor includes only those below level 4 (i.e., the left-side of the distribution according to socio-economic strata characterization).[14] The table shows that, as expected, people in the Census of the Poor are slightly younger, have smaller dwellings, and generally less education.

[14] Information about the strata level is not available in the Population Census.

The poverty index score is a weighted average of answers to the Census of the Poor.[15] The Census of the Poor has 62 questions, which took approximately 10–15 minutes to complete. The score is calculated at the family level. It uses information about 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. The algorithm used to calculate the score is complex, and many of the answers for demographic and education questions do not enter directly into the calculation but are derived (e.g., average education of household members older than 11 years old; social security of the highest wage earner), making it very difficult for anyone to predict whether a family would have a score below 47.

B. *Socio-Economic Characterization and Quality of Life Surveys*

We use alternative data sources to verify whether score discontinuities emerged in these other data. Data for 1993 come from the *Socio-Economic Characterization Survey* implemented by DNP, the same agency that designed the Census of the Poor. This survey includes approximately 20,000 households in urban areas. Data for 1997 and 2003 come from the *Quality of Life Surveys*, collected by DANE.[16] The 1997 and 2003 Quality of Life Surveys include approximately 9,000 and 18,500 urban households respectively. 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.

C. *Election Data*

Data for mayoral elections 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:

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

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 2 shows summary statistics for the variables used in the empirical analysis. The mean value for the *political competition* variable is 0.829, which translates into a difference in the fraction of votes the winner received relative to the runner-up of 0.171.

[15] The algorithm for calculating the poverty index score is available in online Appendix D.

[16] 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).

Table 2—Summary Statistics: Election and Control Variables

Description	Mean	SD	Min	Max
Political competition	0.829	0.168	0.109	0.999
Discontinuity+/− 3 points	0.022	0.031	−0.136	0.152
Discontinuity+/− 5 points	0.026	0.036	−0.076	0.209
Log population	10.517	1.136	8.771	14.656
Ratio of urban to total population	0.534	0.233	0.188	0.988
Proportion of poor (NBI)	0.304	0.143	0.005	0.691
Proportion of surveys	0.511	0.252	0.000	1.000
Number of community organizations	56	325	2	5,944
Newspaper circulation	434	3,154	1	51,574
Distance to largest city in state (km)	101	83	0	548
Surface area of municipality (km2)	796	1,889	15	17,873

Notes: 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 and accounting for the fact that a continuous non-manipulated distribution would also yield a non-zero discontinuity as described in Section IIIB. 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 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. Newspaper circulation corresponds to certified daily average circulation data by municipality for 2004 from Colombia's main national newspaper, *El Tiempo*. A municipality in Colombia is the jurisdiction most similar to a county in the United States.

II. Manipulation of Poverty Index Scores and Timing of Interviews

A. *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 later 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. Manipulation during or after the data entry stages involves changes to the answers in the questionnaire, in a specific component, or changing 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 IV 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, indicated by the vertical line in the figure. Table 3 shows the estimate of the discontinuity at the threshold using local linear regressions with a rule-of-thumb bandwidth suggested by J. Fan and I. Gijbels (1996). The timing of the appearance of the poverty index score discontinuity in 1998 at the 47 threshold coincides almost exactly with the release of the score algorithm to municipal administrators (sometime after July 1997). Before the score algorithm was made available to municipal officials, some local politicians behaved

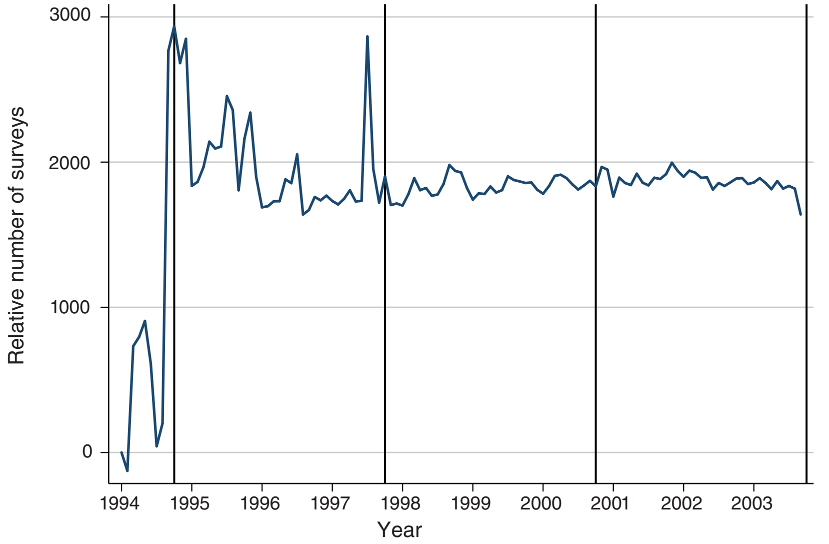

FIGURE 2. NUMBER OF CENSUS OF THE POOR INTERVIEWS, CONTROLLING FOR MUNICIPALITY AND STRATA

Notes: Vertical lines indicate 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.

strategically by timing the household interviews around local elections. This is illustrated in Figure 2 which 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 from 1998 to 2003 and the 1993 Socio-Economic Characterization Survey data distribution, which is representative at the national level. If the 1993 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.

B. *Evidence of Manipulation*

We were able to identify, whether the given overall score, or a specific component, is different from what the algorithm should have generated by using the score algorithm and the individual answers from the survey. Table 4 shows that the housing, utility and education components match almost perfectly. But approximately 11 percent of individuals do not match in the demographic component. Most of the discrepancies come from the income subcomponent. This could happen because at the data entry stage the program used to calculate the score required the data entry

Table 3—Size of the Discontinuity at the Eligibility Threshold

Year	Estimator	SE
1994	0.033	[0.086]
1995	0.080	[0.083]
1996	0.008	[0.121]
1997	0.024	[0.097]
1998	0.868***	[0.119]
1999	1.209***	[0.145]
2000	1.422***	[0.154]
2001	1.683***	[0.150]
2002	1.565***	[0.132]
2003	1.547***	[0.132]

Note: Estimation done using local linear regressions and an optimal bandwidth algorithm.
***Significant at the 1 percent level.

Table 4—Reconstructed versus Recorded Poverty Index Score

Component	Match	Individuals	Households	Percent of households
Housing	Yes	18,107,888	5,288,141	99.68
	No	60,165	16,806	0.32
Utilities	Yes	18,068,140	5,278,296	99.50
	No	99,915	26,651	0.50
Education	Yes	17,721,184	5,194,450	97.92
	No	446,871	110,497	2.08
Demographic	Yes	16,052,471	4,700,355	88.60
	No	2,115,583	604,592	11.40

Notes: The Census of the Poor includes individuals in urban areas and all socio-economic strata levels. "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.

person to type a value for that year's minimum wage. If someone in the municipality entered (by accident or on purpose) the wrong minimum wage, then the income subcomponent generated by the algorithm would be different.

Figure 4 shows the overall results of the given poverty index score distribution and the reconstructed score at the individual level 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 by overwriting the true score with a new score, but it must have occurred at a different stage in the process.

In the data, we identify values of the score that cannot exist. 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.

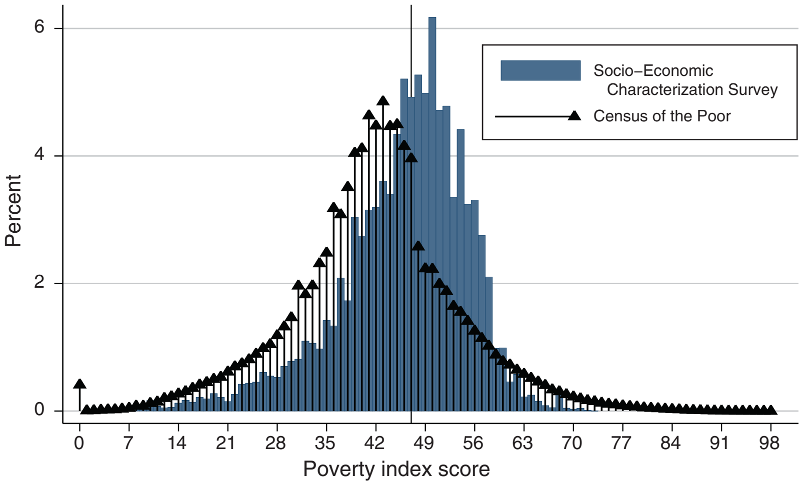

Figure 3. 1998–2003 Census of the Poor and 1993 Socio-Economic Characterization Survey Score Distribution

Notes: The Census of the Poor and the Socio-Economic Characterization Survey use only urban households living in strata levels below four. The vertical line indicates the eligibility threshold of 47 for many social programs.

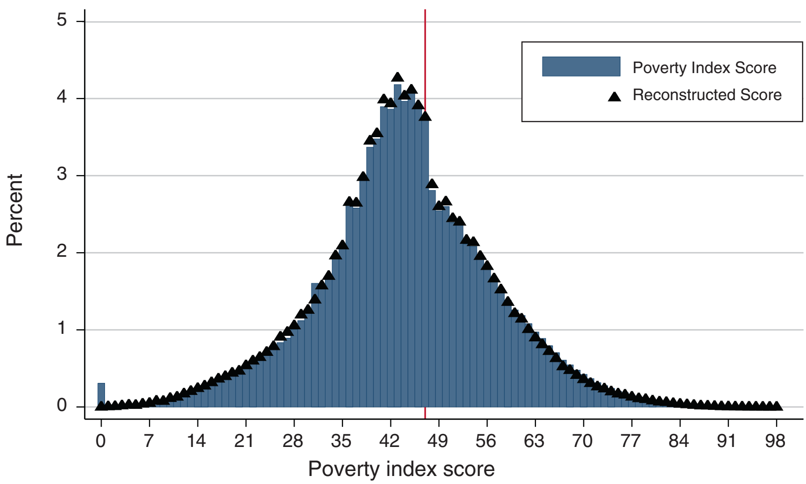

Figure 4. Poverty Index Score and Reconstructed Score

Notes: Triangles indicate the reconstructed Poverty Index Score using the score algorithm. Bars indicate the Poverty Index Score distribution as it appears in the Census of the Poor database. The vertical line indicates the eligibility threshold of 47 for many social programs.

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 follow two approaches. We selected the families that have almost exactly the same answers as at least one other family interviewed in a given municipality and month.[17] In the first approach, we counted the number of families with shared answers and 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. Since we do not observe manipulation in the score distribution before 1998, we treat the pre-1998 data as a sample from the true data generating process for repeated answers. Using the pre-1998 data, we estimate local linear quantile regressions of the proportion of repeated answers on the total number of interviews conducted in each municipality and month.[18] We use the predicted values from these regressions to flag those in the post-1998 period above the ninetieth percentile.

In a second and more restrictive approach, we identified the number of families with the most common repeated answers in each municipality and month. We divided that number by the total number of families interviewed in that municipality and month. This gives us a ratio between 0 and 1. If, for example, in a municipality there were 1,000 households interviewed in a given month, and 10 had shared answers, while another 500 households also had shared answers that yield a different score, we identify only those 500 households and divide that number by 1,000. Then, using the pre-1998 data, we estimate local linear quantile regressions of the proportion of the most common repeated answers on the total number of interviews conducted in each municipality and month. We use the predicted values from these regressions to flag as suspicious those in the post-1998 period above the ninetieth percentile.

With these methodologies we were able to identify, for example, a municipality that on a single day interviewed approximately 45,000 individuals from different neighborhoods, but who each 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 answers from their neighbors, or enumerators "selling" answers to the households.[19]

Overall, using the first approach, we identified around 819,000 households (approximately 2.8 million people) with highly suspicious similarities in their answers. With the second approach, we identified around 50,000 households (approximately 178,000 people) with highly suspicious similarities in their answers. In Figure 5, we show how with the first approach 77 percent of the households

[17] 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.

[18] We estimate local linear quantile regressions because both the mean, and the variance of the proportion of repeated answers, changes with the total number of interviews. Thus, we need a flexible functional form to identify the upper quantiles.

[19] 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 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. Supervisors, coordinators, data entry people or someone higher up are likely to notice that 45,000 people are ending up with almost exactly the same answers for a detailed questionnaire with 62 questions.

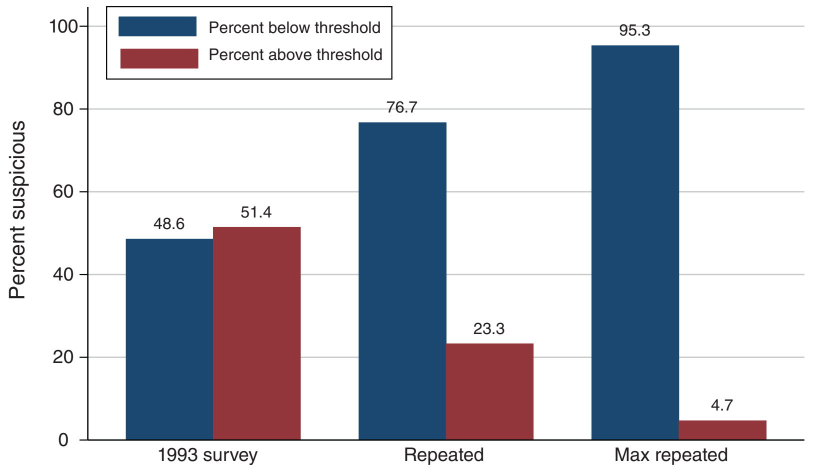

FIGURE 5. PERCENT OF SUSPICIOUS SCORES BELOW AND ABOVE THE THRESHOLD

Notes: Repeated answers corresponds to number of families with the exact same score for all four components within a municipality and month. 1993 survey uses data from the 1993 Socio-Economic Characterization Survey (SEC).

identified with unusual answers, fall below the 47 threshold. While with the second approach 95 percent do. This is in contrast to only 48 percent of all respondents falling below this threshold when using data from the 1993 nationally representative household survey. Furthermore, in both approaches there is a high concentration of people with scores between 35 and 47.

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 approximately 2.8 million people with repeated answers. We summarize our findings in Table 5.

III. Mechanisms for Manipulation of Poverty Index Score and Timing of Interviews

A. *Theoretical Framework*

In this section we provide a brief theoretical framework to motivate our empirical findings. We show two mechanisms through which politicians misused the program, either by conducting a large number of surveys before elections or by changing people's scores. The instrument used to increase their electoral support depends on the relative costs and benefits of each at a particular point in time.[20]

[20] See James A. Robinson and Thierry Verdier (2002), Robinson (2005) and Frederic Charles Schaffer (2007) for related literature on vote buying, patronage, and clientelism.

Table 5—Summary of Suspicious and Changed Poverty Index Scores

Due to:	Number of households	Percent of households
Suspicious hard coding a different score from what the algorithm would generate	22,532	0.42
Suspiciously repeated answers in a municipality-month	819,384	15.43
Total cheating detected	841,916	15.85
Estimated undetected cheating	35,277	0.66

Notes: Hard coding a different score includes: hard coding a component score that cannot exist, hard coding the component scores as zeros, changing the final score to zero or another score. Suspiciously repeated answers consists of finding combination of answers for households within a municipality and month that are repeated beyond what the ninetieth percentile of the pre-1998 data would indicate. See Section IIB for details.

Using a probabilistic voting model framework,[21] let the cumulative density of the poverty index score s be given by $F(s)$. 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. 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:

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

G represents a vector of public goods proposed by each candidate, assume it is exogenous. n^I is the number of surveys conducted before the election divided by the total number of surveys conducted. 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 threshold for whom the politician chooses to lower the score to some score below s_0. We will call this "cheating." b_{s_i} is the benefit to the individual from having his score lowered. Thus, $p b_{s_i} \mathbb{I}[s_i > s_0]$ represents the expected benefits to a voter with a score above the threshold of getting a score below s_0. δ_i and θ correspond to an individual specific measure of the voter's political bias toward the candidate and an aggregate shock to the population's preferences respectively. Both are uniformly distributed and inversely related to ϕ and ψ, which respectively indicate the relative dispersion of the individual and population's preferences for the candidate.

The incumbent wants to maximize the probability of winning the next election. Unlike the incumbent, the challenger cannot conduct surveys or cheat before the election.[22]

[21] See Assar Lindbeck and Jörgen W. Weibull (1987); and Torsten Persson and Guido Tabellini (2000).

[22] Studies that have looked at whether it is possible to buy votes in a secret ballot system include Susan C. Stokes (2005) who explains how clientelistic parties are able to circumvent the secret ballot through "deep insertion into

Assuming increasing costs in the number of surveys conducted and in the amount of cheating, $c(p, n^I) = (\eta/2)(n^I)^2 + (c/2)p^2$, we can solve the incumbent's problem 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:

$$p = \frac{\psi \phi b_{s_i}[1 - F(s^0)]}{c} \tag{3}$$

$$n^I = \frac{\psi \phi b_{s_i} F(s^0)}{\eta}. \tag{4}$$

Some of the results we obtain from this set-up include: a direct relationship between the level of political competition ψ, and the amount of cheating, $\partial p/\partial \psi > 0$; an inverse relationship between the costs and the amount of cheating, $\partial p/\partial c < 0$; and between the costs and the amount of surveys conducted $\partial n^I/\partial c < 0$. In municipalities with a higher proportion of poor people we should see less cheating, $\partial p/\partial F(s^0) < 0$. These results will be tested in the empirical section and in the online Appendix A.

These findings explain that the patterns observed in Figures 1 and 2 are consistent with a relative costs and benefit tradeoff between conducting surveys before an election or cheating. People value surveys because to determine eligibility for many social programs they first need to be surveyed. When the program started, there was confusion among the population as to whether being surveyed was a sufficient condition for eligibility. This enabled politicians to use surveys as a way to influence the electoral outcomes. 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 because the score algorithm was still secret. 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.[23]

voters' social networks" and repeated interactions between the parties and voters. In Colombia, a way in which the contract can be enforced is by exploiting the timing of enrollment into social programs. Households first need to get surveyed, then get an id card, and finally enroll. Another way is a system know as the "carousel" (see *El Tiempo*, "How to Buy a Vote in Colombia," June 20, 1998, http://www.eltiempo.com/archivo/documento/MAM-790679#). Electoral officials at a voting table sign each ballot when the voter first comes to the table, or else the ballot is considered invalid. To get the carousel going, a voter needs to insert an unsigned ballot in the box and keep the signed ballot. The vote-buyer-coordinator marks the signed ballot with his preferred candidate and asks the next voter to deposit the ballot and return an unmarked signed ballot.

[23] Mayors in Colombia cannot be re-elected for consecutive terms. However, mayoral electoral manipulation was widely documented in the press during the period we study. In addition, Drazen and Eslava (2010, 46) explain that even if incumbent mayors cannot be re-elected immediately he has incentives to manipulate because "his decisions affect his party's re-election chances (or those of the incumbent's preferred candidate)," and in the future he may run for re-election to the same (or a different) office.

B. *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 develop a measure of manipulation at the municipal level 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 the discontinuity at the threshold for each municipality on competitiveness of the election. The regression equation has the following form:

$$(5)\ discontinuity_{jt} = \alpha + \beta_1 political\_competition_{jt-1} + \beta \mathbf{X}_{jt} + \eta_t + \gamma_j + \epsilon_{jt},$$

where the dependent variable *discontinuity* serves as a proxy for the amount of cheating in a municipality j at time t. We construct this variable using data from municipalities that conducted interviews 6 months before the election (May–October) because in Colombia political campaigns can 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).[24] To account for the fact that a continuous non-manipulated distribution would also yield a non-zero discontinuity, we subtract the discontinuity of the distribution observed in each municipality for the period without manipulation from 1994 to 1997. 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. Our regression results report standardized coefficients for all variables. 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.[25]

The variable $\mathbf{X}$ includes population and the ratio of urban to total population in each municipality for each year. η is a year effect, and γ the municipality fixed

[24] We use 3 and 5 points from the threshold because we want values sufficiently closed to the threshold where there is data for many municipalities.

[25] In addition, there is a statistically significant and positive correlation (0.04) between contemporaneous and lagged political competition.

Table 6—Discontinuity at the Threshold and Political Competition

	Discontinuity +/− 3 points			Discontinuity +/− 5 points		
Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
Political competition	0.174**	0.176**	0.177**	0.112*	0.113*	0.114*
	[0.074]	[0.080]	[0.080]	[0.056]	[0.062]	[0.062]
Log population		11.181***	10.283***		5.577**	5.136*
		[3.445]	[3.667]		[2.345]	[2.688]
Ratio of urban to total population			−4.305			−2.113
			[8.149]			[5.268]
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	112	112	112	112	112	112
R^2	0.18	0.29	0.30	0.14	0.21	0.21

Notes: Robust standard errors in brackets. All regressions include an intercept term, and report standardized coefficients. 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 and accounting for the fact that a continuous non-manipulated distribution would also yield a non-zero discontinuity as described in Section IIIB. 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.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

effects. A positive coefficient on *political competition* indicates that more competitive elections are associated with more cheating by incumbents.

Results are displayed in Table 6. Consistent with the model the table shows that when the benefits of an additional vote are higher, the discontinuity at the threshold is larger. Columns 1 and 4 do not include additional controls to the municipality and year effects, all other columns include population controls. Columns 1–3 of Table 6 use the fraction of surveys 3 points below and above the threshold, while columns 4–6 use the fraction of surveys 5 points below and above the threshold. A standard deviation increase in the amount of political competition (s.d. $= 0.168$) increases the percent of interviews three points below the threshold relative to three points above the threshold by 0.17 of a standard deviation, and it increases the percent of interviews five points below the threshold relative to three points above the threshold by 0.11 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 falsification exercise 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: months 12–6 prior to the election (November of the previous year to April of the election year), and using the same 6 months of the year (May–October) but 1 year before the election. Results are reported in Table 7. We find that, unlike the results reported in Table 6 which use data for 6 months prior to the election, the political competition does not influence the size of the discontinuity at the threshold.

Table 7—Robustness: Discontinuity at the Threshold using Information months 12–6 prior to the election and 1 Year Prior to Mayoral Election

	Discontinuity +/− 3 points			Discontinuity +/− 5 points		
Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
Political competition, discontinuity in months 12–6 prior to election	−0.009	−0.009	−0.010	0.030	0.030	0.029
	[0.031]	[0.032]	[0.032]	[0.036]	[0.036]	[0.036]
Observations	328	328	328	328	328	328
R^2	0.12	0.13	0.13	0.08	0.09	0.09
Political competition, discontinuity in 1 yr prior to election	0.028	0.028	0.028	0.045	0.044	0.043
	[0.040]	[0.040]	[0.040]	[0.041]	[0.040]	[0.040]
Observations	384	384	384	384	384	384
R^2	0.00	0.00	0.00	0.02	0.03	0.03
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

Notes: Robust standard errors in brackets. All regressions include an intercept term and report standardized coefficients. 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 and accounting for the fact that a continuous non-manipulated distribution would also yield a non-zero discontinuity as described in Section IIIB, using data for (1) months 12–6 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.

In online Appendix A we exploit variation across municipalities. 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. We estimate cross section regressions because the available data that proxies for the cost of cheating do not vary over time. We find that consistent with the model's predictions, 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.

IV. 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 by calculating the number of possible combinations to generate each score. We plotted this simulated distribution, available in online Appendix B Figure B1, and found that it does not exhibit a discontinuity at the eligibility threshold or anywhere else.

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 DANE, real GDP fell by 4.2 percent. 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 data from the Socio-Economic Characterization and Quality of Life 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.[26]

Even though we do not have Quality of Life 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 6 shows that the 1993 Socio-Economic Characterization Survey distribution and the Census of the Poor distribution for 1994 are centered around a similar point. The second and third graphs in Figure 6 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 centered to the left of the Quality of Life 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 Quality of Life Survey data distribution.

To summarize, from Figure 6 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 6 is that the Socio-Economic Characterization and Quality of Life 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. However, 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. 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.

To rule out the possibility that richer municipalities were conducting surveys earlier and poorer municipalities later, we 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 DANE and takes a value between 0 and 100. The higher

[26] Most of the questions necessary to construct the score algorithm are available in the Socio-Economic Characterization and Quality of Life Surveys with a few exceptions like the income question, where the Socio-Economic Characterization Survey provides more detailed and extensive questions on income sources.

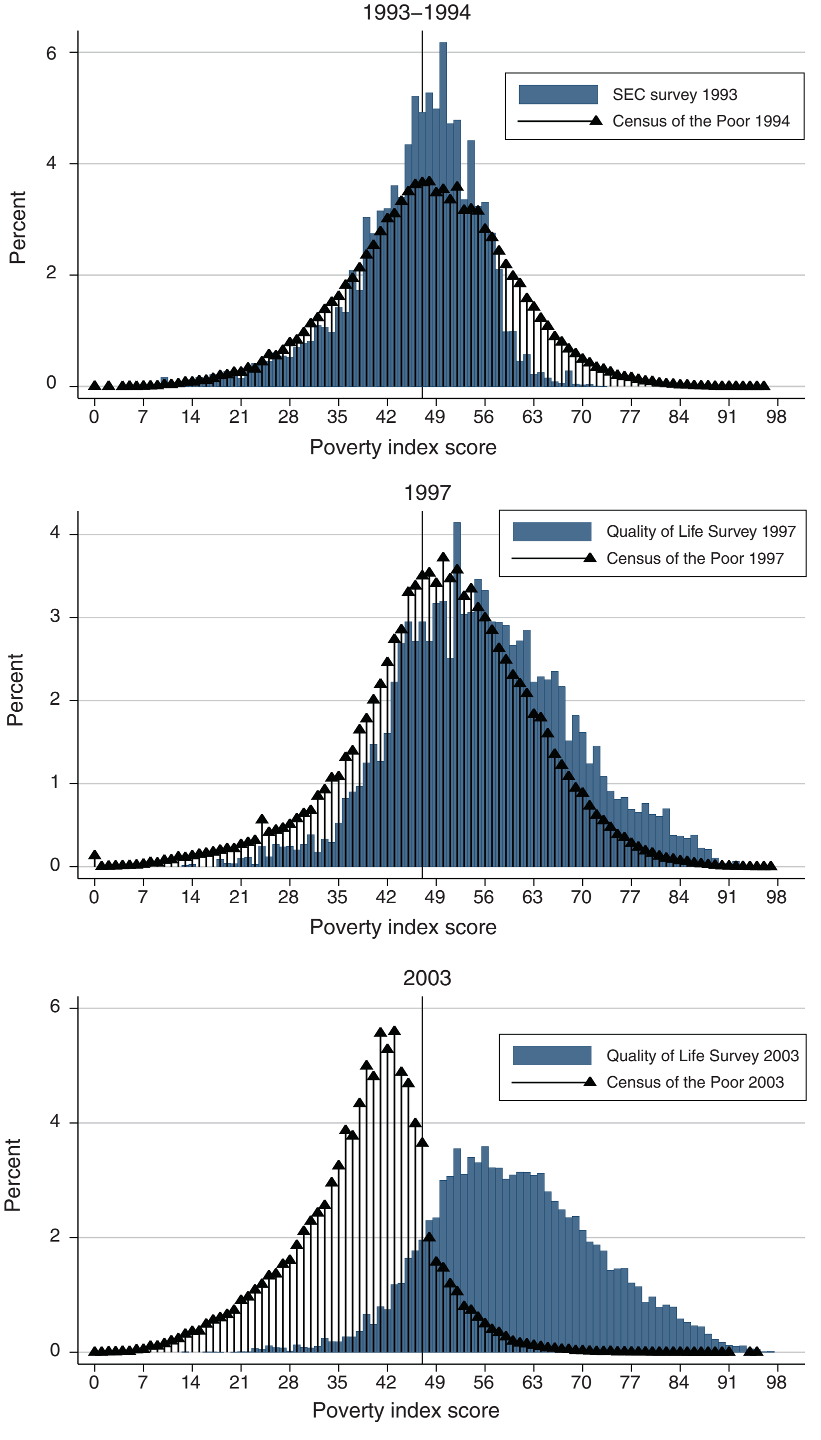

Figure 6. Poverty Index and 1993 Socio-Economic Characterization (SEC) and 1997 and 2003 Quality of Life Surveys Score Distributions

Notes: The Census of the Poor, the Socio-Economic Characterization, and Quality of Life Surveys use only urban households living in strata levels below four. The vertical line indicates the eligibility threshold of 47 for many social programs.

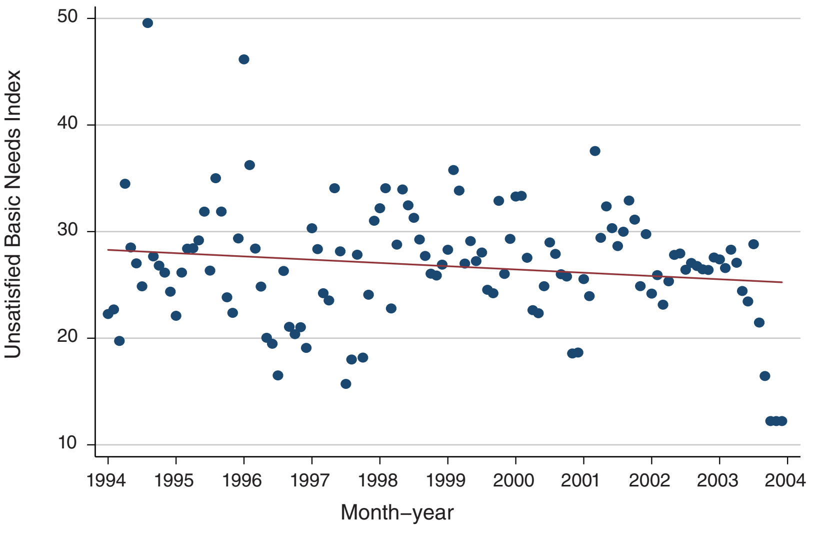

FIGURE 7. WEIGHTED UNSATISFIED BASIC NEEDS INDEX OVER TIME

Notes: 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.

the value, 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 7. 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 increase 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.[27] 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:

$$surveys\_stratumx_{jt} = \alpha + \eta_t + \gamma_j + \epsilon_{jt}, \tag{6}$$

where *surveys_stratumx* corresponds to the number of surveys conducted in stratum level x in municipality j at time t. 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

[27] We did this because the central government instructed municipal officials to use strata levels in the surveying process.

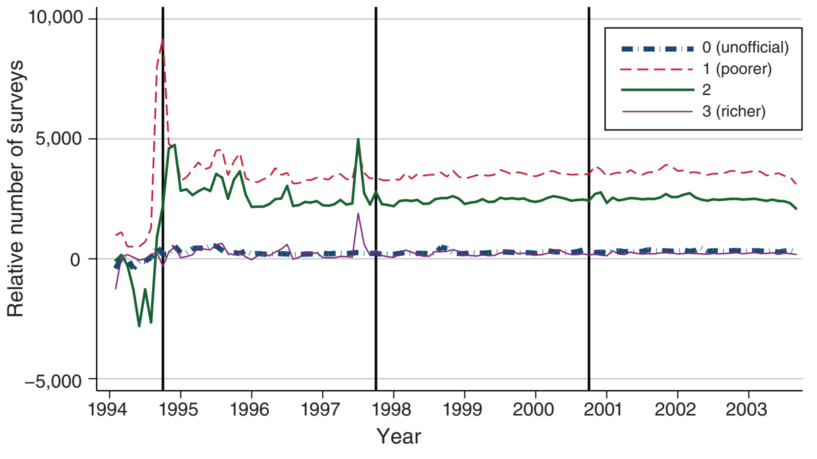

Figure 8. Number of Census of the Poor Interviews by Strata Level, Controlling for Municipality

Notes: Vertical lines indicate regional mayoral elections. 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 (6) in Section IV for details.

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.[28]

Overall the results presented in this section and in the online Appendix B 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.[29]

V. 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. We motivate our empirical findings with a

[28] See online Appendix B for additional information on alternative possible explanations for the patterns observed in the score distribution.

[29] 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 1998, and the discontinuity emerging *exactly* at the threshold.

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 some municipalities is positively associated with political competition.

By using administrative data we are able to identify manipulation of a large scale targeting system that determines eligibility for social programs. In a "back of the envelope" calculation we estimate that approximately three million people had their scores changed, this corresponds to roughly 33 percent of what the Socio-Economic Characterization 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. We link this manipulation to the political process and show that it can take time for corruption to emerge. The sudden emergence of the discontinuity argues against the idea that corrupt behavior is due solely to social norms and culture, and that it is inherent in the system or the population. Instead it supports the view that corruption can be enabled by a change in information, and become more pronounced possibly due to political incentives.

Most of the paper has focused on documenting and explaining motivations for manipulation, yet the findings presented here raise two important questions: First, was the manipulation observed necessarily bad from a social welfare perspective? Factors that should be considered when answering this include: if 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. The possibility of clientelism, in which resources are directed to those with political connections rather than real need, often involves socially wasteful rent-seeking.[30]

Second, is the design of the proxy-means testing instrument properly identifying the population most in need? If the people who had their scores lowered were truly in need, then this type of manipulation could be welfare enhancing, in which case, the need for a mechanism that does not use a discontinuous rule to identify the poor arises. For instance: a system that uses an observable and hard to manipulate characteristic might not as carefully identify individuals, but would be less costly to administer and present less opportunity for cheating; or redesigning the Poverty Index Score to reduce the possibility of excluding some people who are truly in need.

Whether or not the manipulation documented here reduced welfare, the findings in this paper highlight the importance of adopting changes to improve the system. 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

[30] See Robinson (2005) for information on the historical presence of clientelistic relationships in Colombia. Daron Acemoglu, Robinson, and Rafael J. Santos (2009) discuss elections, violence, and government policies in Colombia.

certain municipalities.[31] Further efforts and controls like increasing the penalties for cheating, improving detection of cheaters, updating the information or introducing changes to the system, 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.

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

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