Election Aggregates and the Choice of Electoral Manipulation Strategies*

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Work in progress. Comments welcome

Abstract

Corrupt politicians use a variety of manipulation strategies to win elections. How do politicians choose among them? Using a new dataset of reports of electoral crimes and survey data from Colombia, I identify the level of aggregation of electoral results, and the electorate size as important determinants of the relative incidence between vote buying, turnout suppression, and fraud. The data reveal a robust negative correlation between the size of the average polling station and vote buying. I provide evidence that such correlation can be attributed to the increased ability of brokers to sustain compliance of bribed voters when electoral results of small groups are available. I also find a negative association between electorate size and each method of manipulation that is stronger for vote buying than for turnout suppression and fraud. The result is consistent with vote buying having larger marginal costs of implementation than other methods.

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On October 31 of 2011 in the town of Yopal, Colombia, around three hundred people armed with stones, machetes and other farm tools went to the local offices of the *Registraduría Nacional* (National Registrar's office) to protest what they believed was the illegitimate victory of their elected governor.[1] As stated by some of them, people had been given pre-marked ballots favoring the new governor on election day. This was not the only anomaly. There were ten official reports of vote buying, four reports of polling place staff members favoring a particular candidate, one of a person voting without being registered, and one of abstention induced by threats. Like in Yopal, all over the world politicians use a variety of strategies to irregularly capture elections. What explains the choice among strategies? As with any other choice, this one is determined by the available strategies' costs and benefits, which in turn are affected by a potentially long list of election-specific factors. Using a new dataset of reports of electoral crimes and survey data from Colombia, this paper identifies two of those factors, the level of aggregation of published electoral results and the electorate size, as important determinants of the relative incidence between vote buying, turnout suppression, and fraud.[2]

The monitoring of groups' voting behavior has been used to control electorates at least since the days of the powerful political machines of Chicago (Gosnell 1937). Collective monitoring has been noted to enforce clientelistic transactions (Chandra 2004; Birch 2011; Fox 2012), to be more efficient than monitoring individuals' votes (Kitschelt and Wilkinson 2007, 17), and its effectiveness seems to increase when results are counted at the precinct level (Schaffer 2007*a*, 24). Birch highlights the point, "Such techniques [collective monitoring techniques] of course rely on the use of small units of vote aggregation that allow for political patrons to ascertain how relatively small groups of people vote" (Birch 2011, 98). Although higher levels of aggregation appear to have a negative effect on manipulation, we still do not know how important, if at all, this effect is, and we do not clearly understand the mechanisms by which it works. Consider for example vote buying in secret ballot elections in which results are available at the polling station level. Why would polling places where few people vote—which are those having more disaggregated results—facilitate vote buying? It is generally the case that not everyone voting in a polling station is bribed, even if the polling place is small; therefore, the broker still does not know whether those who accepted the payments are voting as instructed. This paper provides a simple mechanism that explains why small units of aggregation help to sustain clientelistic transactions even when the vote totals of those who receive the payments are not observed. The paper proceeds by presenting the first systematic empirical analysis that shows the importance of the aggregation of results in influencing manipulation.

A larger electorate also seems to reduce the ability of politicians to engage in electoral

[1] See "Denuncias de fraude alteraron elecciones en el Casanare" (Reports of fraud disrupt elections in Casanare) in *Semana*, October 31, 2011.

[2] Vote buying is defined as the exchange between individual voters and party operatives of material benefits for votes, turnout suppression refers to activities that inhibit voting like threats inducing abstention, or buying national identification cards from voters before the elections, and fraud is defined as any action carried by people involved in the counting process aimed at altering vote totals.

malpractice. Stokes, for example, discusses how the industrialization process and the growth of the electorate contributed to the rapid decline of vote buying in Britain in the 19th century. A large electorate—the argument says—increases the cost of bribery, making programmatic campaigning more attractive relative to buying electoral support (Cox 1987; Stokes 2011). The logic is consistent with the negative correlation found between the number of registered voters and a measure of vote broker dominance in Brazil (Gingerich and Medina 2012) and it could also explain why there are more bribing attempts in small and rural populations in Argentina and Nigeria (Stokes 2005; Bratton 2008).[3] There are several questions, however, for which we do not have good answers. Do all manipulation methods respond equally to increases in the electorate size? Is the documented negative impact of electorate size on manipulation explained by the mechanical increase in costs of influencing more voters, or rather, by the changes in the social structure, cultural norms, or levels of aggregation of results that are brought with urbanization and population growth? Here, I provide evidence of a differential effect of electoral size on different strategies that remains after accounting for those potential confounders. I also offer an explanation for these associations that relies on differences on how each strategy is implemented.

The paper documents a robust negative correlation between vote buying and the size of the average polling station and a negative association between the size of the electorate and vote buying, turnout suppression, and fraud that is stronger for vote buying than for the other strategies. I argue that the first finding is explained by the effect that disaggregated results have on the incentives of bribed voters when future payments are contingent upon reaching certain vote totals. The logic for this is simple. Only when brokers have access to results of small groups do voters feel that their vote is important to reach the number of votes that the broker considers appropriate to grant further payments. The differential effect of the electorate size, on the other hand, is explained by the larger marginal cost of buying votes relative to the costs incurred by parties with other strategies. How do corrupt politicians choose between vote buying, turnout suppression, and fraud? What the findings suggest is that politicians engaging in manipulation have more incentives to choose vote buying over other methods in places where few voters vote in each polling station. The evidence also indicates that when the election that is being manipulated has a large electorate, politicians avoid using strategies with high costs per irregular vote.

There are several challenges that must be addressed when assessing the relevance of our hypotheses through statistical analysis. The first is a measurement problem. Electoral crimes are not perfectly observed, and even if they were, those who observe them might have reasons not to report them. In the analysis that follows, I use two alternative estimation techniques to address misreporting.[4] One technique exploits differences in election monitors' and citizens' crime

[3]The argument put forward in Gingerich and Medina (2012) emphasizes how risk averse brokers prefer not to engage in vote buying with large electorates due to the greater uncertainty in the outcome of the election.

[4]Empirical work in clientelism has used list-experiment techniques to account for misreporting and the resulting biases that it generates in statistical analysis (Gonzalez-Ocantos et al. 2012; Corstange 2010). These methodologies

reports. The second incorporates a model of misreporting directly in the statistical estimation of the parameters of interest. As a byproduct of the main analysis, the empirical strategy allows us to identify differences in patterns of underreporting, overreporting, and correct reporting across manipulation methods.

A separate challenge to the empirical analysis is to account for factors that provide alternative explanations for correlations that support our expectations. One example is the concentration in rural isolated areas of voters that are more likely to cooperate with brokers in vote buying transactions. Vote buying is facilitated when brokers target poor and uninformed voters who care little about politics, who are altruistic and think they are helping the broker by following her instructions, or those who trust others and believe the broker when she promises future rewards after the elections. If it is the case that smaller electorates and fewer people voting per polling station are characteristic of rural isolated areas that concentrate these voters, this could explain why we observe a strong negative relationship between vote buying and both the electorate size and the polling place size. The Colombian data, however, offers us a complete set of controls that allows us to rule out this and other alternative interpretations. Moreover, the dataset covers two local and two national elections per municipality, which permit the use of panel data techniques to directly account for unobserved time-invariant factors at the municipality level. Lastly, I reexamine the main hypotheses using a separate dataset with survey information from the Latin American Public Opinion Project (LAPOP 2013) for which we have an even larger set of controls.

After estimating models with specifications that control for many potential confounders, accounting for misreporting with two different methodologies, and using aggregated and individual-level measures of manipulation in the analysis, both the negative correlation between polling station size and vote buying, and the negative one between all methods and electorate size—which is strongest for vote buying—are maintained. The paper also shows that these associations do not seem to be explained by reverse causality and that they are robust to different modeling assumptions regarding independence in the implementation across methods of manipulation.

In addition to the literature already cited, this paper is part of a rapidly growing literature on the factors that influence electoral malpractice. Other determinants of manipulation in the post-secret ballot era have been identified to be poverty, uneducated citizens, and the presence of electoral rules that increase intra-party competition (Lehoucq and Molina 2002; Hicken 2007; Bratton 2008).[5] Brusco, Nazareno and Stokes (2004), Stokes (2005) and Nichter (2008) turn their attention away from economic and institutional factors and examine how voters' political preferences affect reported vote buying.[6] This paper contributes to the previous literature by developing and testing new hypotheses on the variation of manipulation that focus on intrinsic differences in implementation

are not amenable to aggregated data.

[5]For a complete review of the literature on pre-secret-ballot electoral misconduct see Lehoucq (2003).

[6]Other papers that try to account for the observed targeting of both opponents and supporters in vote buying transactions are Dunning and Stokes (2008) and Dunning and Stokes (2010).

across methods and not on voters' preferences, socioeconomic status, or electoral rules. The paper uses an original dataset that allows us to examine different manipulation methods used in the same time and place, which gives information on how parties choose their optimal manipulation mix.

The ideas developed in this article are also connected to those of a small group of papers that are interested in how parties combine strategies. Gans-Morse, Mazzuca and Nichter (2009) and Morgan and Vardy (2010) formally explore how institutional changes, like the introduction of the secret ballot and compulsory voting, alter the incentives of political machines to choose between different strategies.[7] We know, however, very little about what determines the relative use of manipulation techniques under fixed electoral rules, a gap that I attempt to fill here. On the empirical front, cross-country studies have shown the prevalence of manipulation of electoral rules and vote choice over manipulation of administrative aspects of elections and the irrelevance of international monitors in the choice of strategies (Birch 2011; Kelley 2012). No previous empirical work has documented the role of election aggregates determining the observed relative incidence of different strategies.

The theoretical mechanisms highlighted here are not directly tied to the idiosyncracies of the Colombian democratic experience and are consistent with previous observations that link units of aggregation and the electorate size with manipulation in places like Chicago (Gosnell 1937), Brazil (Gingerich and Medina 2012), Germany and Great Britain in the 19th Century (Anderson 2000; Stokes 2011), and Singapore (Hicken 2011, 295). Nonetheless, it is important to be cautious when making claims about external validity. The Colombian case also shares with many developing democracies a disconnect between formal rules and perceived democratic outcomes. The country has a long tradition of democratic institutions by Latin American standards, but distrust in electoral processes is common. In 2002, 86.7% of respondents of the Latinobarometer survey thought that with or without democracy the same people would be running the country and between 1996 and 2010 an average of 77% of respondents thought that elections were not clean and fair. Those numbers contrast with an average Polity score of 7 since 1956.[8] Recent scandals provide a solid base for those perceptions. In July of 2012, the police arrested six former employees of the Registraduría Nacional for running a "counting and computing center" where they intended to change the results of the governor's race in Valle.[9] More telling are the numerous reports of right-wing paramilitary forces known as the *Autodefensas Unidas de Colombia* (AUC—United Self-Defense Organization of

[7] For a complete discussion of other institutional factors that affect clientelism see Hicken (2007) and Hicken (2011).

[8] The Index of Political Rights calculated by the Freedom House also gives Colombia a fairly good rating. The average score for Colombia for the period 1973-2013 is 2.75 (the scale goes from 1 to 7 with lower scores being associated to more democratic regimes).

[9] Later that month, new evidence showed that it was not the first time that this counting center had been used. See "Capturan 6 personas por supuesto intento de fraude electoral en Valle" (6 people captured for attempting to change the electoral results in Valle) in *El Tiempo*, July 1, 2012, and "Investigan fraude electoral in cinco departamentos" (Investigation of potential fraud in five departments) in *El Tiempo*, July 7, 2012.

Colombia) committing fraud and more frequently, intimidation of candidates and voters.[10] What the Colombian case illustrates is that politicians adapt to institutional changes, and with different degrees of success, are able to capture public office. By now it is well understood that the adoption of democratic institutions does not guarantee improvements in accountability and responsiveness in the short term, and sometimes, like in Colombia, not even after several decades. Only through a clear understanding of the manipulation techniques can we close the gap between democratic ideals and practices.

The paper proceeds as follows. The next section lays down in detail the mechanisms that connect election aggregates to the relative incidence among strategies. The third section presents the empirical strategy and it is followed by a brief overview of the political and institutional context of Colombia in the period of analysis. The fifth section describes the data. The sixth presents the main results, robustness tests, and a description of estimated misreporting patterns of electoral crimes. The seventh concludes.

The Mechanisms

When the secret ballot was first introduced in Australia in 1856, it was supposed to counteract the rampant vote buying that was characteristic of elections. More than 150 years later, elections all over the world use the secret ballot and yet, vote buying is still widespread (Schaffer 2007*b*). How do parties sustain compliance of bribed voters even though their choices are protected by the secret ballot? There are many answers. Vote brokers might target reciprocal individuals (Finan and Schechter 2012), or those who support the party but are unlikely to vote in the absence of inducements (Nichter 2008).[11] They can also engage in direct monitoring by using carbon copies of ballots, pictures of ballots taken with cell phones, or as reported in towns of the north cost of Colombia, by using little kids who do the monitoring accompanying voters to the voting booth while pretending to be the voters' children.[12]

Here, we are interested instead in collective monitoring sustaining vote buying. If the electoral results of groups of voters are available, party operatives can induce compliance by conditioning future bribes on their party reaching pre-determined vote totals. The aggregation levels of results however, determine whether group monitoring can be effectively applied. When only voting results of large groups are available, no bribed voter would think that her vote is important to reach any number of votes that ensure the continuation of payments. Since her choice does not determine whether the interruption of future bribes will occur, the bribed voter would have more incentives to

[10] Left wing guerrillas have also been known to interfere with elections (although not as actively as the paramilitaries).

[11] For more on how social norms and local knowledge sustain clientelistic transactions see White (1965); Scott (1972); Callahan and McCargo (1996), and Stokes (2005).

[12] Interview conducted by the author in Bogotá, Colombia, March 2012.

vote according to her preferences. The opposite occurs if the bribed voter knows that she is voting along with a small group of voters whose results are being watched by the broker. Then, she will vote as instructed as her vote can make a difference on whether she will receive new bribes. This simple intuition, formally modeled in Rueda (2012), gives us an observable implication. Since in Colombia results are available at the polling station level, we should see that places where more people vote at each polling station should have fewer cases of vote buying.

Large numbers of voters at each polling station can also create obstacles for direct fraud. Large polling places where hundreds of people vote are more likely to concentrate the attention of poll-watchers and monitors that oversee the counting process. However, if it is possible for a politician to circumvent this monitoring and to alter the results of a large polling station, the number of irregular votes obtained is larger. Politicians then could have more incentives to concentrate their ballot stuffing efforts in large polling stations. While the expected relationship between number of voters per polling station and vote buying is negative, for fraud, we do not have a strong prior on the direction of the effect of polling station size. As for turnout suppression, the size of the average polling station should not affect the ability of party operatives to induce abstention. When implementing this strategy, party operatives only need to monitor turnout, which is easily observable regardless of the size of the polling station where the targeted person intended to vote. Moreover, those engaging in turnout suppression usually hold the identification cards required for voting of the targeted individuals, which eliminates the need to check for compliance.

The number of voters per polling station is not the only election aggregate that affects manipulation. When there is uncertainty about the preferences of individual voters, a large electorate forces parties to obtain irregularly a large number of votes to influence the results. This leads us to expect a negative effect of increasing the electorate size on all types of manipulation. However, differences in the marginal costs of implementation between vote buying, turnout suppression and fraud create differences in the magnitude of the electorate size's effect across methods. When buying votes, parties offer positive inducements through networks of brokers that monitor small groups of voters for several elections. The more involved in the community brokers are, the easier it is for them to identify good targets—those who value reciprocity, those who are under economic stress, or those who weakly support the party but would not vote in the absence of bribes. Therefore, the cost of an additional irregular vote in vote buying not only captures the value of the bribe, but also part of the costs associated in finding well connected brokers.[13] For turnout suppression, it is also important to have a network of operatives working for the party who induce voters to abstain, but unlike vote buying, operatives' local knowledge is not crucial as turnout is easily monitored. Moreover, turnout suppression does not always involve positive inducements as people are sometimes threatened with violence to force their abstention. Fraud has an even lower marginal cost

[13] In fact, as stated by several campaign operatives in Colombia, the competition for established brokers can be fierce, with parties offering compensation tied to performance in the brokers' areas of influence. Interview conducted by the author in Bogotá, Colombia, March 2012.

per irregular vote than the previous methods. Once party operatives are able to alter the results of a polling station, an additional irregular vote's cost is almost null. While the size of the electorate affects negatively all methods, vote buying has a higher marginal cost and should therefore have a stronger negative relationship with electorate size than that of turnout suppression and fraud.

Empirical Strategy

To examine the previous hypotheses I use models in which the dependent variable is the count of reports of a particular crime. Throughout the paper I model this count as a random Negative Binomial variable $y_{i,t}$ with mean equal to exponent of the linear combination of our explanatory variables of interest and controls,

$$\mathbf{x}_{i,t}\beta = p_{i,t}\ \alpha^p + e_{i,t}\ \alpha^e + x_{i,t}\ \delta.$$

Therefore

$$y_{i,t} \sim NegBin\left(exp(\mathbf{x}_{i,t}\beta), exp(\mathbf{x}_{i,t}\beta)(1+\eta\ exp(\mathbf{x}_{i,t}\beta))\right), \tag{1}$$

where $\mathbf{x}_{i,t} = (p_{i,t}, e_{i,t}, x_{i,t})$, $\beta = (\alpha^p, \alpha^e, \delta)$, and η is the overdispersion parameter. The subindex i denotes a municipality and t an election year. The variable $p_{i,t}$ is the aggregation level of results measured by the voting age population divided by the number of all polling stations in the municipality, $e_{i,t}$ is the average size of the electorate in all elections on that particular year, and the vector $x_{i,t}$ contains control variables. I estimate β for each manipulation method.

To summarize the hypothesized relationships between dependent and independent variables using the introduced notation, we have

1. In vote buying models the coefficient α^p should be negative.

2. For all manipulation methods the coefficient α^e is expected to be negative, but when vote buying reports are the dependent variable, it should have a larger magnitude than the one obtained in models of turnout suppression or fraud.

Maximum likelihood estimation applied to model (1), would give us consistent estimates of β if misreporting of crimes by citizens was not determined by any of the regressors. Unfortunately, this is unlikely. Consider for example the fraction of the population living in poverty which is included in $x_{i,t}$. Low income can facilitate the implementation of all manipulation methods and it can also reduce the reports of crimes. Poor voters have a higher distrust of institutions and they might perceive that the authorities do not take seriously their testimony, or they might lack

information about the procedures to file a report. If that is the case, our estimates of the impact of poverty in all manipulation methods can be underestimated.

This paper addresses the potential problems created by misreporting using two strategies. First, the models are estimated using data on reports filed by election monitors rather than using citizens' reports. The logic behind this strategy is that misreporting is less likely when monitors are independent outside actors, and more importantly, that the factors that affect the true count of electoral crimes do not determine what crimes the monitors decide to report. This approach is not without shortcomings. Election monitors are not present in every municipality, the municipalities that are covered are not randomly chosen, and the data on monitors' reports covers fewer elections.

A potential solution to those shortcomings is to adopt Multiple Imputation (MI) to fill the missing observations of reports where monitors were not present (Rubin 1987; King et al. 2001). MI regards the missing data as random variables and then replaces the missing observations with multiple draws from an assumed distribution. Following this procedure, multiple datasets are created and the econometric analysis is carried out in each of them. The point estimates from each dataset, the $\widehat{\beta}$'s in our case, are averaged to obtain the final results. Finally, standard errors are adjusted to account for error introduced by the imputation.[14]

For MI to give us sensible results we are required to assume that the missing values may depend on observed values in our dataset but not on unobservables (missing at random assumption MAR). This is an strong assumption but it is one that it is likely to hold in this particular case. As stated by the head of *Misión of Observación Electoral* (MOE)—the non-government organization that monitors the elections and collects the reports—the two most important factors that determine whether a municipality has MOE monitors are security conditions, and whether there are already other non-government organizations affiliated with MOE in the municipality.[15] That is, MOE sends monitors to places where their safety is not compromised, and where there are local organizations that can provide logistical support. Larger and more economically developed municipalities tend to have more of those organizations. Our dataset has information on armed groups presence as well as several economic variables which suggest that MAR holds for this analysis. A clear advantage of using multiple imputation to fill missing monitors' reports is that the difference between citizens' and monitors' reports where available, provides additional information that is used at the imputation stage. If it is the case that controlling for security concerns and economic variables, the expectation of crimes still determines where monitors are sent, having information of reported crimes by citizens can proxy for those expectations allowing MAR to hold.

The second strategy to deal with misreporting when using citizens' reports is to explic-

[14] I use Amelia II (Honaker, King and Blackwell 2009) to create 10 complete datasets using all variables included in the analysis. Appropriate transformations are taken to improve the fit of our dataset to a Multivariate Normal distribution assumed by Amelia. It is important to note however, that the Multivariate Normal distribution works well at the imputation stage even when we have ordinal variables as it is our case. For a discussion on the literature on this particular point see (King et al. 2001).

[15] Interview with MOE director Alejandra Barrios was conducted by the author in Bogotá, Colombia, March 2012.

itly model the misreporting process and to incorporate such a model into the estimation of the parameters of interest. For this, I use a generalized Negative Binomial mixture Poisson model proposed by Li, Trivedi and Guo (2003). Under this approach, which I will call *structural*, the true count of electoral crimes, denoted by $y^*_{i,t}$, follows a Negative Binomial distribution with mean $exp(\mathbf{x}_{i,t}\beta)$. Conditional on $y^*_{i,t}$ being zero, $y_{i,t}$, the reported count of crimes, is assumed to be Poisson distributed with mean $exp(\mathbf{z}_{i,t}\phi)$. In a similar way, it is assumed that conditional on $y^*_{i,t}$ being positive, the observed count is distributed Poisson with mean $y^*_{i,t}exp(\mathbf{z}_{i,t}\theta)$. The model allows to estimate the parameters β, ϕ and θ and hence, to identify what factors determine the true count of crimes, over-reporting and underreporting. In the appendix, I present the likelihood function derived from this model and explain how a Simulated Maximum Likelihood estimator is implemented to recover the parameters of interest.

Misreporting is not the only source of concern when testing our hypotheses. One shortcoming of using aggregated municipality-level data is that there are no good measures available for a potential confounder that could affect the estimation of our parameters of interest. Cultural norms like reciprocity or those that promote trust among members of the community might facilitate overcoming the commitment problems of vote buying transactions. Rural areas can have cultural norms that differ from those in urban centers and at the same time they tend to have fewer numbers of voters registered per polling place and smaller electorates in local elections. Not accounting for cultural norms in the regressions could bias our estimations in favor of finding evidence that supports our hypotheses.[16]

Concerns of potential bias caused by the omission of cultural norms is addressed in two separate ways. The panel data structure of the aggregate datasets allows us to estimate fixed effects models, which could account for any time invariant unobserved factor at the municipality level that can affect our estimates of the coefficients of interest. The effectiveness of this approach relies on the common assumption that cultural norms do not change rapidly over time.[17] The second way to account for potential confounders is to replicate the analysis using a different dataset that has more information on social characteristics of voters. For this, I use the LAPOP Colombian survey for the years 2010, 2011, and 2012.

While the LAPOP survey offers a large set of variables that allow us to rule out explanations that are inconsistent with our hypotheses, it also has some limitations. In the available survey there is only information on the incidence of vote buying and turnout suppression but not on fraud, the survey is carried on a small fraction of Colombian municipalities, and we do not have validation data as we do with the aggregate data to account for potential misreporting. For these reasons,

[16] Note that this can be true even after controlling for population as a municipality whose population is more densely concentrated geographically tends to have fewer polling stations than another with the same population that lives more evenly distributed within the territory of the municipality.

[17] For recent articles that present evidence of slow change in culture and beliefs see Alesina (2007) and Hoff, Kshetramade and Fehr (2011).

the paper relies primarily on the results of the aggregate analysis and presents the survey analysis as a separate robustness check of the main results.

When using survey data, the probability of a respondent being the target of a manipulation strategy is modeled using the logistic distribution. The questions in the survey used to build the dependent variables asks whether the respondent was offered a bribe in the last 4 years, and whether she has been threatened to induce her abstention. This last question does not specify how long ago the threat occurred. For this reason, all explanatory variables that are election specific in the individual-level models are the averages of the variables calculated for the most recent local and the national elections. For example, the measure of electoral competitiveness used as a control for the year 2012 is the average of the competitiveness of the local elections of 2011 and that of the national election of 2010.

Given that underreporting of bribes attempts is still a problem that must be addressed, I also follow an structural approach by estimating a logit model that accounts for potential misclassification in the dependent variable proposed by Hausman, Abrevaya and Scott-Morton (1998). In the appendix I present further details on this model and its estimation.[18]

Institutions and Political Environment

Since the analysis uses data from local and national elections, and the construction of several explanatory variables relies on the particular arrangement of Colombian institutions, I now give a brief summary of its main features. As established by the 1991 constitution, Colombia is a presidential democracy where the president is elected by popular vote under plurality with run off. Its territory is divided into 32 departments, and the head of the executive at the department level is the governor. The governor is elected by plurality rule for one four year term. At the municipality level, the executive power is vested in mayors who are also elected by plurality rule. The legislative branch is represented by a bicameral congress that has an upper house with a national constituency represented by 100 senators. In the lower house, there are 32 multi-member districts that correspond to the departments, and their size is determined by population. Members of the Colombian congress are elected by party-list proportional representation with seats allocated by D'Hondt divisors.[19] For the Senate there is a threshold of representation of 2% of valid votes and for the lower house, the threshold is 50% of a Hare quota (valid votes/total seats per district).[20] Members of congress are elected in elections that occur in the same year but in different months of the presidential election. At the regional level, the legislative branch is represented by department

[18] For Monte Carlo experiments evaluating the properties of this estimator see Hug (2010).

[19] Before 2006 however, the seat allocation formula was simple quota and largest remainders applied to personal lists. That is, each party was allowed to present more than one list at every election (for details on the reform and its effects on the party system see Pachón and Shugart 2010).

[20] For districts where there are only two seats the threshold for House of representative elections is 30% of the district quota.

assemblies and municipal councils. Seats for assemblies and councils are also allocated by D'Hondt divisors with a district threshold of 50% of the quota. Regional elections of mayors, governors, and members of department assemblies and councils all occur on the same day.

During the period of analysis the Colombian political landscape was transformed by two processes: the rise and consolidation of non-traditional parties and the involvement of armed groups in politics. Although Colombian politics were dominated by Liberals and Conservatives for most of the country's history, after the constitution of 1991 new parties gained considerable representation. By 2002, the share of seats taken by non-traditional parties in congress was 43.2%. That same year Alvaro Uribe, a candidate from an independent movement, was elected president of Colombia. Around that period, during the regional elections of 2000, 2003 and congress elections of 2002, right-wing paramilitaries exerted direct pressure to influence their results in a scale that had not been seen before. This was the direct result of a series of pacts where AUC leaders and national and regional politicians agreed to engage in mutual cooperation. The Ralito Pact was one of them. According to Miguel de la Espriella, a former Senator who was sentenced to three years in prison, in the year 2001 60 politicians met paramilitaries' leaders Don Berna, Salvatore Mancuso, Diego Vecino and Jorge-40 to sign a document that called for a "new social contract" and for the "refounding of the country." In the Casanare Pact, regional politicians agreed with the paramilitary leader Martin Llano to appoint paramilitary recommended candidates to local government positions once they were elected. The previous pacts are only two examples of the vast efforts that AUC undertook supporting certain politicians in exchange for policies that favored their organization.[21] As of October of 2012 there were 200 congressmen, 94 mayors and 179 municipal council members formally investigated for their links with paramilitaries.[22] Rather than substituting the clientelistic relationships of the past, more violent tactics (intimidation of poll officials, voters, and murder or kidnapping of political opponents) complemented them, increasing the overall effectiveness the manipulative efforts.

Data

Municipality-level data for the dependent variables come from citizens' reports filed in regional offices of the Attorney General of Colombia and election monitors' reports collected by MOE. The citizens' dataset contains the number of reports by type of manipulation from every municipality (1122 in total) per election year for the period from 2002 to 2011. The MOE data have election monitors' reports from 632 municipalities and covers the period from 2006 to 2011.

The MOE reports are chosen for their coverage and independence from regional and na-

[21] For formal statistical evidence that shows how paramilitary presence is associated with changes in voting patterns see Acemoglu, Robinson and Santos (2012).

[22] See "Casi 500 investigados por parapolítica en los últimos cinco años" (Almost 500 public officials investigated for links with paramilitaries) in *Semana* October 25, 2012.

tional governments. The MOE is the largest non-governmental domestic organization monitoring Colombian elections. Other monitor agencies cover fewer municipalities, do not monitor regional elections, or only report incidents occurring few days prior and after the elections. The MOE works with smaller regional organizations that report incidents occurring months previous to the elections, as well as incidents that occur during and after the election. Also, the MOE is financed by several international institutions, which lends more credibility to its political independence.[23]

Table 1: Summary Statistics (Municipality-Level Analysis)

	Observations	Mean	Std. Deviation	Min	Max
Panel A	Dependent Variables				
Fraud (Citizens)	4351	0.075	0.625	0	25
Fraud (Monitors)	1059	0.055	0.362	0	9
Turnout Suppression (Citizens)	4351	0.131	1.175	0	44
Turnout Suppression (Monitors)	1068	0.03	0.477	0	15
Vote Buying (Citizens)	4351	0.223	0.98	0	29
Vote Buying (Monitors)	1068	0.281	1.409	0	27
Panel B	Explanatory Variables				
Armed Group	4351	0.279	0.449	0	1
Closeness CG	3968	0.412	0.433	0	1
Electorate Size	4351	2735500	2522193	1585.75	5565864
Local Revenues	4351	23.705	24.547	0	100
Margin	4351	0.104	0.058	0.01	0.546
Polling Place Size	4351	318.39	84.061	108.046	1110.75
Poverty	4351	42.113	21.031	0.431	100
Total Population	4351	40690.49	248474.5	908	7467804

This table presents summary statistics for the variables used in the municipality-level analysis. The sample is restricted to those observations where there is information on citizens' reports of electoral crimes. For variables' definitions and sources, see Table 8 .

Table 1 has summary statistics for all the variables used in the aggregate-level analysis. Panel A shows that vote buying is the most reported manipulation strategy by both monitors and citizens. We also see that for all methods but vote buying there are more citizens' reports than monitors' reports.[24] This was expected, as different people can end up reporting the same crime as they do not know which crimes have already been reported. As a last observation, we notice that reports on any type of manipulation are rare, as reflected by the low average number of reports in a given municipality per year. The perception among monitors, politicians and voters is that it is underreporting and not lack of criminal activity what explains the low means in the sample for all types of manipulation.[25]

[23] Some of these institutions are: United Nations, European Union, USAID, Konrad Adenauer Stiftung, Oxfam, Global Network of Domestic election Monitors, Ford Foundation.

[24] This is also true once we compare only the municipalities that have in a given year monitors' and citizens' reports.

[25] Interviews conducted by the author in Bogotá, Colombia, March 2012.

The main explanatory variables, the voting age population per polling place and the electorate size, are built with data from the Registraduría Nacional and from the National Statistics Office (DANE). The electorate size is calculated as the average number of valid votes of all elections in a given year. In regional elections years (2003, 2007 and 2011), the electoral size is the average of the votes cast in the mayor election in the municipality, the votes for the local council candidates in the municipality, and the votes in the assembly's and governor's races in the department where the municipality is located. For national election years (2002, 2006, 2010) the size of the electorate is calculated as the average of the votes for representatives in the department and the votes for senators and president in the nation. Table 8 in the appendix has a detailed description of the construction and sources of these and other regressors used in the analysis.

All our econometric specifications include as controls a measure of the competitiveness of the elections, a measure of poverty, an indicator of presence of guerrillas or paramilitary forces and a measure of the local government fiscal autonomy. Given that our dependent variable is the level of manipulation, all models also include the municipality population size.

Whether clientelistic parties target core or competitive districts remains a contested issue in the literature (e.g. Calvo and Murillo 2004; Stokes 2005; Magaloni 2006; Nichter 2008; Calvo and Murillo 2009; Dunning and Stokes 2008, 2010). Our objective is not to further explore this question but rather to account for potential biases caused by omission of competitiveness. Since more competitive elections are thought to increase turnout, and they require less votes to change the results through irregular means, not controlling for competitiveness could bias upwards our coefficient of electorate size. The measure of competitiveness is the average margin of victory in all elections of a given year weighted by the valid votes of each type of election in the municipality. For elections to the national and regional legislative bodies, competitiveness is the gap between the list winning the last seat and the closest loser, while for plurality rule elections it is the gap between the winner and the runner up. The variable is built using data from the Registraduría Nacional accounting for thresholds of representation and vote-seat formulas described earlier.

Poverty has also been hypothesized to increase manipulation. Poor voters are cheaper to bribe, do not have strong political preferences, and have few years of schooling, all characteristics that facilitate the irregular capture of elections. More importantly, the omission of income levels could bias the results in favor of our hypotheses, as richer municipalities are likely to have larger electorates and polling places. A measure of poverty calculated by DANE for each municipality for 1993 and 2005, the Unsatisfied Needs Index, is linearly interpolated and used as control.[26] I also include as a control the share of local revenues in the total revenues of the municipality taken from the National Planning Department. This is also a proxy for economic development as richer municipalities rely less on transfers from the central government.[27]

[26] While it is not ideal to use the interpolated series, all of the results reported in the paper hold if we leave this variable out of the models. These results are available upon request.

[27] This variable can also serve as a proxy for the rewards of holding office. Elected positions that bring more

Given the importance of non-state armed actors in Colombian politics, the control set includes a variable that takes the value of one if there was a combat where either guerrillas or paramilitary forces were involved, or if there was a unilateral military action from any of these groups in the municipality. The concern if we do not control for the presence of armed groups is that they tend to operate in rural areas where the electorate and average polling place sizes are smaller. The source of this information is the Conflict Analysis Resource Center (CERAC).

The individual level data come from the 2010, 2011 and 2012 LAPOP surveys. The survey is based on interviews of 6012 voting age individuals and covers 81 municipalities over the three years. Table 9 in the appendix includes the summary statistics of all the variables used in the individual-level analysis. Panel A shows that just as it was the case with the aggregate data, reported vote buying is much more common than reported turnout suppression. When we compare the first group of variables of Panel B in Tables 9 with Panel B of Table 1 we can see that the average municipality for which we have survey information is in general quite different from the one in the aggregate analysis sample. The average municipality in the individual level analysis is richer, has a larger share of local resources in total revenues, and has a larger population.

Results

A first look at the reported electoral crimes by citizens reveals some patterns that are consistent with our expectations. In Figure 1, the bar graphs divide the Colombian municipalities according to their potential number of voters per polling station by quartiles of that variable, and by the average electorate size.[28] We see that the figures show a negative relationship between the reported per capita levels of manipulation and both polling place and electorate size. The key pattern in the figures is that the two negative relationships are much more stronger for vote buying than for turnout suppression and fraud.

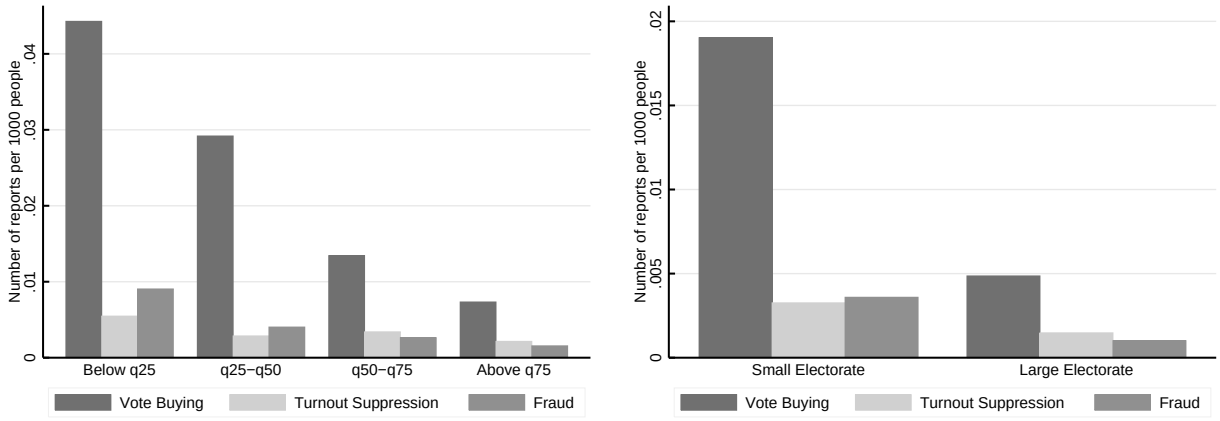
Table 2 confirms these general patterns. Each column has the coefficients of the main explanatory variables for the Negative Binomial models where the dependent variable is the count of reports of a particular method.[29] Columns (1), (3) and (5) present results of models that use citizens' reports as dependent variables. We first note that in these models, for the three methods of manipulation, there is a negative and significant coefficient of polling place size. The same is true for the coefficient of the size of the electorate, with the magnitude of the coefficient in the vote buying model being larger than that of other methods. Columns (2), (4) and (6) present

political power, like those that assign greater control over public resources, should have more politicians willing to engage in manipulation to attain them. In Colombia, local governments receive a large share of its revenues from the central government and those resources are tied to particular expenditures (mainly expenditures in health and education), which reduces the discretion in how they are spent.

[28] Municipalities whose size is below the median electorate size in the sample are in the 'Small Electorate' category and those above the median are in 'Large Electorate'.

[29] Estimated coefficients for all controls are available upon request.

Figure 1: Electoral Crime Reports by Polling Place Size and Electorate Size



results that use election monitors' reports. Assuming that monitors' misreporting, if it exist, is not associated with our variables of interest, those columns would present estimated coefficients that would not be affected by misreporting. What we see in those models is that once again, polling place size is negatively related to all manipulation methods but only for vote buying this relationship is statistically significant. We also see that vote buying and fraud are significantly and negatively related to the average electorate size and that vote buying is more responsive to it than other methods. The magnitude of these effects is large. Given that both variables are in logs, their coefficients can be directly interpreted as elasticities in count models. In the average municipality adding the results of two polling stations before publishing them (doubling the polling place size) would eliminate vote buying (the estimated effect is -105.8%). Doubling the electorate size in the average municipality, which is less of an increase compared to the change in size from local to national elections, would reduce it by 61.4%.

The association between polling place size and vote buying however can still be explained by mechanisms different than our hypothesized one. As noted already, isolated rural areas have different traditions and social norms that could facilitate compliance in vote buying. Since isolated areas have also polling stations that serve fewer voters, our measure of aggregation could be picking up the "social norms effect". A second mechanism, emphasizes how in these isolated areas the population generally lack information about the candidates and could use the bribes as a signal of the candidates' attributes, like their ability to provide future goods or other benefits.[30] This mechanism would explain voters' compliance in vote buying as voters give their support voluntarily to a candidate that appears to be "competent" by displaying her wealth and bribing voters. Again, the coefficient on the number of voters per polling station could be capturing this lack of information.

[30] See Kramon (2009, 2011) for a discussion on the role of signaling in vote buying.

Table 2: Determinants of Electoral Crime Reports

Dep. Variable:	Vote Buying		Turnout Suppression		Fraud	
	(1)	(2)	(3)	(4)	(5)	(6)
Polling Place Size	-1.140***	-1.058**	-0.896**	-1.059	-1.922***	-0.448
	(0.251)	(0.430)	(0.402)	(0.646)	(0.388)	(0.771)
Electorate Size	-0.364***	-0.614***	-0.201***	-0.143	-0.144***	-0.463***
	(0.029)	(0.083)	(0.049)	(0.252)	(0.053)	(0.174)
Observations	4351	1068	4351	1068	4351	1059
Municipalities	1098	632	1098	632	1098	632
Reports	Citizens	Monitors	Citizens	Monitors	Citizens	Monitors

This table presents coefficients of Negative Binomial count models. 'Citizens' indicates that the data for the dependent variable come from citizens' reports. 'Monitors' indicate that the data for the dependent variable come from election monitors' reports. All models include as controls: the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election margin of victory. Polling place size and electorate size of the previous elections are in logs. Standard errors clustered at the municipality level are in parentheses. $***$ Significance at 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

Given that general lack of information about politics and social norms do not change quickly over time, these and similar concerns can be addressed with estimations that account for constant municipality effects. Here, I estimate linear and Poisson fixed effect models.[31]

The results of the fixed effect models that use monitors' reports are in Table 3.[32] The linear models in columns (1), (3), and (5) show that polling place size is negatively and significantly related to vote buying and fraud but not to turnout suppression, and that the coefficient of electorate size for vote buying is negative and significant, while for the other methods is not. The Poisson models in columns (2), (4) and (6) give us moderate support for our hypotheses. We find that the negative effect of electorate size is stronger for vote buying than for turnout suppression and fraud. However, vote buying is not negatively related to polling place size. It is important to note that the sample used by the Poisson models is significantly reduced as the estimator drops all municipalities that have no variation in the levels of reports, which could explain this change in results.

While using monitors' reports might reduce concerns about how misreporting affects our results, this approach might be hampered by the non-random selection of municipalities in the

[31] Although the reported crimes were assumed to have a Negative Binomial distribution in (1), switching to the Poisson distribution presents at least two advantages over the Negative Binomial when including fixed effects. First, with the Poisson model, consistent estimation of β does not require that the count is truly Poisson distributed. A sufficient and less restrictive condition for consistency is that $E[y_{i,t}|\mathbf{x}_{i,1},...,\mathbf{x}_{i,T}] = \gamma_i exp(\mathbf{x}_{i,t}\beta)$, where γ_i is the municipality effect. Second, when using the alternative conditional fixed effects Negative Binomial estimator proposed by Hausman, Hall and Griliches (1984) the municipality effects can not be interpreted as unobserved invariant factors. The reason for this is that the fixed effects not only differentiate the means of the cross section units but they appear as a separate shifter in their variances. The way they do this is different from the way other regressors affect the variance term (see Winkelmann 2008, p. 227). However, results of the fixed effects Negative Binomial estimator are generally consistent with the results of the paper and are available upon request.

[32] Results for fixed effects models with citizens' reports data are consistent with the one presented in Table 3 and are available upon request.

Table 3: Determinants of Monitors' Electoral Crime Reports (Fixed Effects Models)

Dep. Variable:	Vote Buying		Turnout Suppression		Fraud	
	(1)	(2)	(3)	(4)	(5)	(6)
Polling Place Size	-4.553***	0.660	-1.157	-0.584	-1.030**	-7.474*
	(1.614)	(1.464)	(0.935)	(7.157)	(0.478)	(4.014)
Electorate Size	-0.125**	-0.713***	0.015	-0.541	0.001	-0.588*
	(0.055)	(0.159)	(0.016)	(0.656)	(0.021)	(0.305)
Observations	1069	222	1068	48	1059	83
Municipalities	632	82	632	15	632	31
Model	FE	Poisson FE	FE	Poisson FE	FE	Poisson FE

This table presents coefficients of linear fixed effect models in columns (1), (3) and (5), and of Poisson fixed effects models in columns (2), (4) and (6). Data for the dependent variable come from election monitors' reports. All models include as controls: the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election margin of victory. Polling place size and electorate size of the previous election are in logs. Standard errors clustered at the municipality level are in parentheses for the linear models. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

sample. Particularly, based on the selection criteria used by MOE to choose the municipalities that are monitored, we are left with a sample of municipalities where monitors are perceived to be safe, and where there are other organizations providing logistical support to the monitors. These same municipalities are likely to have different levels of manipulation.[33] As explained in the methodological section, our dataset allow us to use multiple imputation to address this potential problem. Table 4 presents estimation results that use 10 completed datasets. We find strong support for the hypotheses that relate polling place size and electorate size to manipulation. Vote buying is consistently negatively associated with polling place size while other methods are not, and the effect of electorate size on vote buying is stronger than that of other methods. As the table shows, those results are maintained even when we account for potential confounders that are invariant over time in each municipality with linear and Poisson models.

It is also important to note that the magnitude of the coefficients for polling place size and electorate size has decreased relative to the estimations that used only available data. The marginal effects of polling place size and electorate size however are still important. Adding the results of two polling stations before publishing them would reduce the number of reports by 50.2% and doubling the electorate size would reduce them by 16.6%.

We further check the robustness of the results using the structural approach proposed by Li, Trivedi and Guo (2003). The vector of variables in the true count equations, $\mathbf{x}_{i,t}$, includes the same variables used up to this point. The variables used to model misreporting, those included in

[33] It is unclear whether we should expect to have more or less manipulation where monitors are sent. On the one hand, safer and larger municipalities have more public resources at their disposal increasing the incentives of politicians to engage in manipulation. On the other, municipalities that are considered as safe are those where non-state armed actors might be less effective at influencing elections.

Table 4: Determinants of Monitors' Crime Reports (Multiple Imputation)

Dep. Variable:	Vote Buying			Turnout Suppression			Fraud		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Polling Place Size	-0.502***	-0.952**	-1.381*	-0.046	-0.164	-1.235	-0.454	-0.197	-1.407
	(0.178)	(0.43)	(0.773)	(0.291)	(0.186)	(2.169)	(0.295)	(0.125)	(1.073)
Electorate Size	-0.166***	-0.091***	-0.175***	0.011	0.001	0.011	-0.054*	-0.007*	-0.052
	(0.021)	(0.012)	(0.017)	(0.034)	(0.004)	(0.032)	(0.033)	(0.004)	(0.032)
Observations	4486	4486	2911	4486	4486	1482	4486	4486	1443
Municipalities	1122	1122	728	1122	1122	371	1122	1122	361
Model	Neg. Bin.	FE	Poisson FE	Neg. Bin.	FE	Poisson FE	Neg. Bin.	FE	Poisson FE

This table presents averages of the estimated multiple imputation coefficients and their standard errors in parenthesis. Columns (1), (4) and (7) present multiple imputation results of a Negative Binomial model, columns (2), (5), (8) present multiple imputation results of a linear fixed effects model, and columns (3), (6) and (9) results of a Poisson fixed effects model. All results are estimated using 10 complete-datasets. All models include as controls: the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election of the same type margin of victory. Polling place size and electorate size of the previous election are in logs. The number of municipalities and observations in Poisson Fixed Effects models reflect averages across the samples used from the imputed datasets. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

$\mathbf{z}_{i,t}$, are the lag of the poverty measure, the previous election margin, the lag of the presence of armed groups, the previous election log of the average electorate size, a measure of 'closeness' of the local government to the central government, and the log of the total population.

The margin of victory in the previous election can affect accurate reporting as follows. If voters perceive that the margin was large because of successful manipulation, they will be less likely to report new manipulation attempts, as experience shows them that the party can get away with such actions. Also, if armed groups are supporting a candidate, it is likely that people would fear the consequences of reporting against them.[34] A separate factor that can induce underreporting is the size of the electorate. In a large election, a voter can think that others are more likely to report any anomaly, which would save her the cost of doing it herself. Lastly, people could perceive that it is less likely that their reports would bring any consequence when the incumbent's party is the one engaging in manipulation, and both, local and central governments are controlled by this party. Therefore, if there is ideological affinity between the central government and local governments, we expect to see more underreporting. Our measure of closeness is the percentage of senators from the mayor's party that voted in favor of key legislation supported by the central government.

Table 5 presents the results of the mixed Negative Binomial-Poisson structural model. Panel A presents the coefficients of the true count of crimes, Panel B, the coefficients of the observed count conditional on not having any true crimes, and the Panel C, the coefficients of the observed count conditional on having at least one true crime. As expected, vote buying continues to be strongly

[34] Reprisals can be facilitated by the fact that people have to give their personal information when they file the reports.

Table 5: Determinants of Manipulation and Misreporting (Citizens' Reports)

	Vote Buying	Turnout Suppression	Fraud
	(1)	(2)	(3)
Panel A	Actual crimes (β)		
Polling Place Size	-1.49***	-0.906**	-2.157***
	(0.385)	(0.442)	(0.77)
Electorate Size	-0.485***	0.263***	0.387**
	(0.108)	(0.078)	(0.232)
Panel B	Misreporting with no crimes (ϕ)		
Poverty	-0.192	1.306*	-0.66
	(0.191)	(0.823)	(0.522)
Armed Group	-0.235	1.181*	-0.697
	(0.313)	(0.923)	(1.11)
Margin	1.157	-3.567	-1.766
	(1.917)	(11.818)	(3.957)
Closeness CG	0.215	4.021***	-0.793
	(0.243)	(1.264)	(0.732)
Electorate Size	-0.362***	-0.879***	-0.584**
	(0.042)	(0.208)	(0.278)
Panel C	Misreporting with crimes (θ)		
Poverty	-0.169	-0.027	0.238
	(0.386)	(0.083)	(0.537)
Armed Group	-0.334*	-0.578**	-1.029
	(0.233)	(0.277)	(1.182)
Margin	-6.405***	-0.683	5.115
	(1.56)	(2.367)	(7.572)
Closeness CG	-0.144	-0.108	0.082
	(0.222)	(0.196)	(0.265)
Electorate Size	0.169**	-0.414***	-0.478*
	(0.092)	(0.055)	(0.306)
Observations	4351	4351	4351
Municipalities	1098	1098	1098

This table presents Simulated Maximum Likelihood estimated parameters of the mixed Negative Binomial Poisson structural model. Coefficients of the Negative Binomial model of true crimes are in Panel A. Coefficients of the Poisson count of reports conditional on not having crimes are in Panel B. Coefficients of the Poisson count of reports conditional on having a positive count of crimes are in Panel C. All true count of crimes equations include as controls: the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election margin of victory. Misreporting equations include as additional control to the ones included in the table the log of the total population. Polling place size and electorate size of the previous election are in logs. Standard errors clustered at the municipality level are in parentheses. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

and negatively related to the level of aggregation of electoral results. Turnout suppression and fraud are also found to be negatively related to the same variable. We again see that the size of the electorate is negatively associated to vote buying. For these models, turnout suppression and fraud are positively associated to the electorate size, perhaps indicating a shift from vote buying to strategies with lower marginal costs.

Individual Level Analysis

As noted before, the presence of omitted factors can explain the consistent negative association between polling place size and vote buying. I have argued that fixed effects panel estimators are likely to address that potential issue if the unobserved factors are social norms or lack of information that facilitate vote buying. In this section, I take an alternative approach by trying to control directly for social attributes of voters that are used to select voters to be bribed. Specifically, the LAPOP survey contains information on whether the person helps others in her community and whether the person finds them trustworthy, as well as measures that capture her general interest and information about politics.

Table 6: Determinants of Electoral Manipulation (Individual-Level Analysis)

Dep. Variable:	Vote Buying		Turnout Suppression	
	(1)	(2)	(3)	(4)
Polling Place Size	-2.540**	-2.652**	-3.805	-3.211
	(1.126)	(1.308)	(11.526)	(11.434)
Electorate Size	-4.440	-4.339	-61.536	-59.799
	(11.184)	(12.275)	(66.876)	(65.191)
Avg. Prob. of Underreporting		0.2404		0.0016
Observations	3655	3515	1569	1513
Municipalities	77	77	21	21
Model	Logit	Missclass. Logit	Logit	Missclass. Logit

This table presents coefficients of Logit models in columns (1) and (3), and coefficients of Logit models that correct for misclassification of the dependent variable in columns (2) and (4). All models include as aggregate controls the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, the average of the second most recent local and national elections margins of victory, and municipality effects. The models also include as individual level controls the respondents' age, education, gender, income level, interest in politics, involvement in community affairs, religiosity, news readership frequency, strength of general trust in the community, whether the person is registered to vote, and a dummy variable for whether the person lives in a rural area. The variables that enter the underreporting equation for models in column (2) and (4) are: age, education, gender, income level, perception of corruption in the public sector, and a dummy variable of whether the person has been a victim of a crime in the previous year. Polling place size and electorate size in the previous election are in logs. 'Avg. Prob. Underreporting' gives the sample average of the predicted probability of reporting not having experienced being the target of manipulation when the person has. Standard errors clustered at the municipality level are in parentheses. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

Table 6 presents the results of the models that include as regressors a set of individual

characteristics, all the municipality-level explanatory variables used in the aggregate analysis, and municipality fixed effects. The individual level characteristics are: age, years of education, gender, whether the voter is registered to vote, a measure of how interested she is in politics, a measure of her involvement in helping others in her community, the importance given to religion, the reported attention given to the news, a measure of the trust in others, and a dummy of whether she lives in a rural area within the municipality. In column (1) we have the results of a logistic regression that models the probability of being offered a bribe, and in column (3) the logit that models the probability of having been pressured into abstaining. We see that while the probability of being offered a bribe is negatively and significantly related to the average polling size in the municipality, turnout suppression does not appear to be affected by that variable.

Considering that social desirability bias could affect the previous results, I also estimate models that accounts for potential misclassification in the dependent variable following Hausman, Abrevaya and Scott-Morton (1998).[35] The results are very similar to the ones of the standard logit. Figure 2 presents the predicted probability of being offered a bribe as a function of the average polling place size for the average citizen of Cartagena, an intermediate city located in the north of Colombia where reports of vote buying are frequent.[36] Increasing the average polling size from 300 to 600 reduces the probability of being offered a bribe from 0.742 to 0.314.

Although we found support for the first hypothesis using the individual-level data, we did not find a different effect of electorate size across manipulation methods. For both of them, the electorate size coefficient is not statistically different from zero. One possible explanation for this is related to the fact that the municipalities included in this sample are markedly different from those in the aggregate dataset. As it was noted before, the survey is conducted in municipalities that are much larger and richer than the average municipality in Colombia. If the changes of electorate size generate larger changes in manipulation when the electorate is small than when it is large, it would be hard to find a significant effect in this sample. Currently, there is no other survey that has information on different manipulation strategies in a larger sample of municipalities to confirm this explanation.

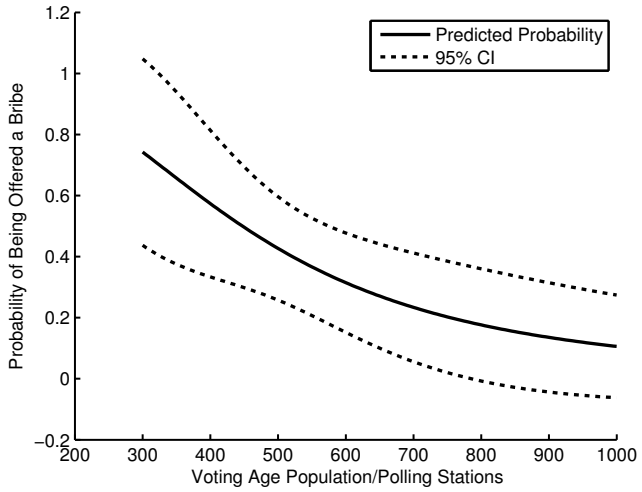
Other Robustness Tests

One concern regarding the test of our first hypothesis is that the negative relationship between polling place size and vote buying is explained by reverse causality. Given that the number of

[35] The underreporting equation includes: age, education, gender, income level, perception of corruption in the public sector, and a dummy variable of whether the person has been a victim of a crime in the previous year.

[36] Cartagena has a margin of victory of 14.12% of the valid votes, 22.92% of its population lives in poverty, 33.67% of its revenues come from local sources, and it has a population of approximately 942,600 people. The average Cartagenean in the sample is 35 years old, has 12 years of education, is a male, is registered to vote, considers religion to be important, follows the news few times a week, considers the people in his community to be trustworthy, is not very interested in politics, and helps at solving community problems one or twice a year. He also thinks that corruption of public officials is very common, and has not been victim of a crime in the last year.

Figure 2: Levels of Aggregation of Results and Vote Buying (Individual-level Analysis)



polling stations is partially determined by the number of registered voters, it is possible that vote buying would increase the number of registered voters by a magnitude that would require increasing the number of polling stations. We can use information on the number of registered voters in a municipality and a rule included in the Colombian electoral law regarding polling place sizes to define a subset of the data in which the vote-buying-to-polling-station mechanism would be very hard to observe. Then we can check whether the previous results still hold using that subset. Polling stations in Colombia are set to serve up to 400 voters. When more than 400 voters are registered to vote in a given poll location, a new polling station is prepared for those additional voters. If vote buying is causing polling stations to increase, we would expect this to be more frequent in municipalities whose numbers of registered voters per polling station falls short of the 400 limit. Columns (2), (4) and (5) of Table 10 in the appendix presents the results of the models where we have restricted the sample to municipalities where the median number of registered voters per polling station in the previous election is less than 250.[37] We still find a strong significant negative relationship between the measure of aggregation levels of results and vote buying even after controlling for time invariant municipality effects. A stronger test based on the same idea is to include in the sample only municipalities whose largest polling place has less than 400 registered voters. Columns (1) and (3) do this for municipalities for which its largest polling station have less than 350 registered voters. This dramatically reduces our sample but we still find a strong negative relationship between polling place size and vote buying. This time however, the coefficient of polling place size is precisely estimated when using citizens' reports but not with monitors' reports.

Reverse causality can also affect our estimated coefficient of electorate size. The three

[37] Results using the mean of registered voters per polling station are similar and are available upon request.

methods of manipulation can potentially alter turnout. Vote buying and fraud are implemented to increase vote totals of the party responsible for the manipulation attempts, possibly increasing observed turnout, while turnout suppression by definition, attempts to reduce it. There are also indirect effects of manipulation. For example, all methods can depress participation by fostering a feeling of futility on potential voters (Simpser 2012). Since it is unclear what the overall effect of manipulation on turnout is for this sample, and whether there are differences of this effect across methods, I reestimated the main count models with a different measure of electorate size. For a local election year, this measure is the average of the voting age population in the municipality and the department. For a national election year, it is the average of the voting age population of the department and that of the country. This is a proxy for the potential size of the electorate that is not affected by manipulation attempts. Table 11 in the appendix shows that for models that use citizens and monitors reports, the main conclusions are maintained.

Regarding the second hypothesis, a shortcoming of the previous analysis is that it does not allow us to test whether the differences in the electorate size's coefficient across manipulation methods are statistically significant. One way to address this issue is to fit a Seemingly Unrelated Linear Equation Model (SUR). The SUR model assumes that the error term for one method of manipulation is related to the error in the equation of a different manipulation method. This is a sensible assumption, considering that a party could be optimally choosing between methods for a given election in the same place. The SUR estimations, shown in the appendix, confirm our previous results. Using the citizens' and monitors' reports, I find that the differences in coefficients of the electorate size are indeed significant across methods, with vote buying having the largest negative coefficient for that variable. We also see that the polling place size is negatively related to vote buying, although for the model that uses the monitors' reports the coefficient is significant at the 12.4% level.[38]

A separate concern is that our coefficients of electorate size could be capturing the effect of unobservable characteristics that differentiate local from national elections, and that those characteristics of local elections facilitate vote buying more than other methods. Consider the amount of information that voters have about candidate platforms. In local elections, the voters are not as exposed to advertising, debates, interviews and other sources of information as they are in national elections, which could decrease the strength of their preferences for a given candidate. While having an electorate that does not have strong political preferences is a great advantage for those engaging in vote buying (as the reservation price of a vote is increasing in the strength of political preferences), it is less so for politicians that engage in fraud or turnout restrictions. Table 13 in the appendix presents the results of models that include as a separate regressor a dummy variable that indicates whether it is a local election year. The table includes coefficients for Negative Binomial

[38] When using the Multiple Imputation completed datasets, I do find support for both of our hypotheses. Results are available upon request.

models that use the monitors' data, and the ones of models that correct for potential selection through multiple imputation. We first see that the magnitude of the coefficient of the electorate size has decreased in all models relative to those presented in Tables 2 and 4, indicating that in fact, there could be unobserved characteristics of local elections that facilitate all kinds of manipulation. However, for most of these models we still find that vote buying is more sensitive to the growth of the electorate than other methods.[39] However, for the models that use the sample that does not correct for potential selection, the coefficient of electorate size is not precisely estimated. We can also see that the coefficient of the local election dummy fails to be significant in all models.

Patterns of Misreporting

Panels B and C of Table 5 show some interesting results regarding the variables that affect misreporting. A large electorate reduces the incentives to denounce false crimes and it increases underreporting of turnout suppression and fraud. This can be explained by the fact that false accusations carry less weight whenever there are more voters, especially if it is expected that not many others will corroborate or denounce similar actions. Also the incentives of witnesses of true crimes to free ride on others' reporting is stronger on large electorates. We also see, as expected, that armed groups presence is associated with underreporting of all methods, although for fraud, the coefficient is not precisely estimated. Finally, less competitive elections measured by the margin of victory in previous elections, only seem to induce underreporting of vote buying.

Besides identifying the determinants of misreporting, our empirical strategy allows us to asses the extent of overreporting, underreporting, and accurate reporting across manipulation methods. I use the estimated coefficients of the Negative Binomial model in Table 4 and those of the true crime equation in Table 5 to simulate the true count of electoral crimes for each observation in the sample.[40] Then, I compare the simulated counts to the one reported by citizens to calculate the proportions of underreporting, overreporting, and accurate reporting in the whole sample.

Table 7 presents the average proportions and their standard errors for 500 simulated accurate counts. There are several observations. The first is that with the exception of the distribution of misreporting of vote buying, multiple imputation and the structural approach give us very similar proportions. This is an encouraging sign that our empirical strategy might be actually capturing the underlying misreporting mechanisms that generates our data. Two completely different methodologies that operate under a different set of assumptions mostly agree in the overall patterns of misreporting. The second is that underreporting seems to be more prevalent than overreporting across manipulation methods. Only the structural approach seems to give a different message for vote buying. Having more underreporting than overreporting is consistent with the general view

[39] The differences in coefficients are also significant in SUR models that use the same samples as the models in the table. Results are available upon request.

[40] For the multiple imputed coefficients I use the average coefficients obtained with the completed datasets.

Table 7: Simulated Proportions of Misreporting and Accurate Reporting

	Vote Buying		Turnout Suppression		Fraud	
	Structural	MI	Structural	MI	Structural	MI
Over	0.108	0.094	0.036	0.047	0.036	0.038
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Under	0.097	0.253	0.137	0.093	0.091	0.093
	(0.004)	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)
Accurate	0.796	0.652	0.827	0.861	0.873	0.869
	(0.004)	(0.007)	(0.005)	(0.004)	(0.004)	(0.005)

This table presents the average proportion of overreporting, underreporting and correct reporting over 500 simulations of the true crime count. The simulated true count is compared with the citizens' reports to calculate the proportions. 'Structural' indicates proportions calculated with simulated counts from a Negative Binomial model with coefficients taken from the misreporting structural model in Table 5. 'MI' indicates proportions calculated with simulated counts from a Negative Binomial with coefficients from the Multiple Imputation estimations in Table 4 columns (1), (4), and (7). Standard errors of the mean proportions are in parentheses.

held by voters, politicians and election monitors in which reporting electoral crimes does not seem to have major consequences. If the chances of a reported crime generating a formal accusation against a party or a politician are very small, people will be less likely to report them when there was a crime and will not even consider reporting when no crime has taken place. This would explain why fewer than 5% of all reports are about fake instances of turnout suppression and fraud. For vote buying however, the fake reports are higher. Misinforming about potential vote buying can create doubts on the electorate and could influence the vote choice of uninformed weak supporters. Since vote buying is perceived by voters to be more common than other forms of manipulation, politicians have more incentives to engage in overreporting of vote buying rather than reporting false attempts of other parties to manipulate with methods that are rarely seen.

Concluding Remarks

This paper shows that brokers who operate in places where there are disaggregated electoral results have an advantage when inducing compliance of bribed voters in secret ballot elections. The magnitude of the effect of increments in the size of the average polling station on vote buying is large. The predicted effect of adding the results of two polling places before publishing them is a reduction in vote buying of at least half. The findings were also consistent with large electorates giving politicians incentives to move away from high marginal cost strategies like vote buying to others like fraud or turnout suppression.

A potential problem with adding the results of several polling stations before publishing them is that this could end up facilitating fraud and miscounting. The results suggest that if anything, there was a negative relationship between the measure of aggregation of results and

reported fraud, which could reduce such concerns. As for the differential effect of the electorate size across strategies, it appears that focusing on monitoring the count process rather than sending monitors to the streets to report vote buying might be more efficient in national constituency elections. The opposite would be true in local elections that have smaller electorates.

This paper has only taken the first step towards understanding how parties combine strategies to irregularly control the electorate. One topic that deserves future examination is whether there are systematic differences between the strategies used by incumbents and those used by the challengers. The fact that incumbents can use public programs, spending, and employment as campaigning tools could reduce their incentives to engage in risky strategies like the ones studied here. There should also be differences in the strategies used in races for different government posts. Politicians in the executive branch are in a better position to use public spending strategies. Members of legislatures have less control over the allocation of resources around elections, which could give them more incentives to engage in risky manipulation. The data used in this paper do not allow us to explore such hypotheses because we do not know the government position that the candidate engaging in manipulation was pursuing, nor her party. Further inquiry on these issues will better inform the formulation of electoral reforms in places where elections are not bringing the desired levels of accountability and representativeness.

Appendix

Modeling Misreporting with Municipality-level Data

Given the model specification discussed in the paper, it is easy to show that the probability mass function of the observed count of crimes is

$$f(y_{i,t} \mid \mathbf{x}_{i,t}, \mathbf{z}_{i,t}, \beta, \phi, \theta) = \frac{e^{-\mu^0_{i,t}} {\mu^0_{i,t}}^{y_{i,t}}}{y_{i,t}!} \left(\frac{\nu}{\nu+\lambda_{i,t}}\right)^{\nu} + \sum_{y^*_{i,t}=1}^{\infty} \frac{e^{-y^*_{i,t}\mu_{i,t}} (y^*_{i,t}\mu_{i,t})^{y_{i,t}}}{y_{i,t}!} \frac{\Gamma(y^*_{i,t}+v)}{\Gamma(v)\Gamma(y^*_{i,t}+1)} \left(\frac{\nu}{\nu+\lambda_{i,t}}\right)^{\nu} \left(\frac{\lambda_{i,t}}{\nu+\lambda_{i,t}}\right)^{y^*_{i,t}}$$

where $\mu^0_{i,t} = exp(\mathbf{z}_{i,t}\phi)$, $\lambda_{i,t} = exp(\mathbf{x}_{i,t}\beta)$ and $\mu_{i,t} = exp(y^*_{i,t}\mathbf{z}_{i,t}\theta)$, and ν is one over the overdispersion parameter.

Maximum likelihood estimation can not be directly implemented for this model given the presence of the infinite series in the above expression. Li, Trivedi and Guo (2003) propose implementing a Simulated Maximum Likelihood estimator. For this, we require an unbiased simulator for the probability mass function of the observed count, $\widetilde{f}(y_{i,t}, \mathbf{x}_{i,t}, \mathbf{z}_{i,t}, u; , \beta, \phi, \theta)$

$$E[\widetilde{f}(y_{i,t}, \mathbf{x}_{i,t}, \mathbf{z}_{i,t}, u; \beta, \phi, \theta) \mid y_{i,t}, \mathbf{x}_{i,t}, \mathbf{z}_{i,t}] = f(y_{i,t} \mid \mathbf{x}_{i,t}, \mathbf{z}_{i,t}, \beta, \phi, \theta)$$

with the expectation taken over an appropriate distribution of u.

The simulator is in our case

$$\widetilde{f}(y_{i,t}, \mathbf{x}_{i,t}, \mathbf{z}_{i,t}, u; \beta, \phi, \theta) = \frac{e^{-\mu^0_{i,t}} {\mu^0_{i,t}}^{y_{i,t}}}{y_{i,t}!} \left(\frac{\nu}{\nu+\lambda_{i,t}}\right)^{\nu} + \frac{\frac{e^{-u\mu_{i,t}} (u\mu_{i,t})^{y_{i,t}}}{y_{i,t}!} \frac{\Gamma(u+v)}{\Gamma(v)\Gamma(u+1)} \left(\frac{\nu}{\nu+\lambda_{i,t}}\right)^{\nu} \left(\frac{\lambda_{i,t}}{\nu+\lambda_{i,t}}\right)^{u}}{p(u|\mathbf{x}_{i,t})},$$

where $p(u \mid \mathbf{x}_{i,t})$ is the truncated at zero Negative Binomial distribution with parameters estimated from a "naive" Negative Binomial model of the observed count. The naive Negative Binomial model is the one that does not account for misreporting.

The Simulated Maximum Likelihood estimates are obtained by maximizing the following expression

$$\sum_{t=1}^{T} \sum_{i=1}^{N} \log \frac{1}{S} \sum_{s=1}^{S} \widetilde{f}(y_{i,t}, \mathbf{x}_{i,t}, \mathbf{z}_{i,t}, u^s_{i,t}; \beta, \phi, \theta),$$

over (β,ϕ,θ). In this expression $u^s_{i,t}$ with $s = 1, ...S$ are random draws taken from the truncated at zero Negative Binomial distribution $p(u \mid \mathbf{x}_{i,t})$. For the estimations in Table 5 I use 300 draws.

Modeling Underreporting with Individual-level Data

The model proposed by Hausman, Abrevaya and Scott-Morton (1998) extends the standard logit model as follows. Let v_j take the value of 1 whenever respondent j reports being a target of a given type of manipulation and zero otherwise, and v_j^* be a unobserved variable that takes the value of 1 only when she in fact has been the target of manipulation. I am interested in learning how certain regressors in $\mathbf{x}_j$ affect the probability of observing v_j^* being 1 and so I assume that $Pr(v_j^* = 1|x_j) = F(\mathbf{x}_j\beta)$ with $F(.)$ being the cumulative logistic distribution. The difference with the standard logit model is that $Pr(v_j = 0|v_j^* = 1)$ is allowed to be positive. This probability is modeled as function of regressors z_j with $Pr(v_j = 0|v_j^* = 1, z_j) = G(z_j\xi)$ where $G(.)$ is also a cumulative logistic function.[41] The probability of a respondent j answering that she has been the target of manipulation is then

$$Pr(v_j = 1|x_j, z_j) = (1 - G(z_j\xi))F(x_j\beta).$$

The MLE estimated parameters are obtained by maximizing the log likelihood function

$$\sum_{j=1}^{M} v_j \ln((1 - G(z_j\xi))F(x_j\beta)) + (1 - v_j)\ln(1 - ((1 - G(z_j\xi))F(x_j\beta)))$$

over (ξ, β).

Complementary Tables

[41] The original model setup also allows for the possibility of false reporting. For this application I assume that the probability of claiming to be the target of manipulation when the person has not is zero as there are no clear benefits of lying in this way when answering the survey. The assumption also ensures that a monotonicity condition needed for consistent estimation of β is automatically satisfied. For more details see (Hausman, Abrevaya and Scott-Morton 1998, p.242).

Table 8: Variable Definitions and Sources

Variable	Description	Source
Armed Group	Dummy variable that takes the value of 1 if there was a combat where either guerrillas or paramilitary forces were involved, or if there was a unilateral military action taken by any of these groups.	CERAC
Closeness Central Govt.	Percentage of senators from same party as the mayor of the municipality that voted in favor of important legislation that the central government supported. If the mayor belongs to a party that has no senators the variable takes the value of zero. Two roll call votes are used. The first from 2009 decided in favor of a referendum for a constitutional change that allowed president Alvaro Uribe to run for office for the third time. The second in 2004, approved the constitutional change that allowed the first reelection of president Alvaro Uribe.	*Gaceta del Senado* and author's calculations
Electorate Size	Average of the total valid votes of all races in that particular year. For a regional election year it is the average of the valid votes in mayor and local council members races at the municipality level, and of assembly and governor races at the department level. For a national election year is the average of the valid votes of lower house members races at the department level and of president and senate races at the national level.	Registraduría Nacional del Estado Civil and author's calculations
Local Revenues	Share of revenues of the local government in the municipality as a share if its total revenues (that include transfers from the central government).	National Planning Department.
Margin	Average of all margins of victory in races of a given year weighted by valid votes of each race in the municipality. Margins for plurality elections (mayor, governor and president) are calculated as the gap between the winner's and the runner-up's votes. For presidential elections results of the first round are used. For proportional representation races (municipal councils, department assemblies, lower house and senate) after 2003, margins are the gap between the electoral quotient of the party winning the final seat and the electoral quotient of the closest loser as in Selb (2009). Before 2003, it is calculated as the gap between the votes of the party winning the final seat and the votes of the closest loser.	Registraduría Nacional del Estado Civil and author's calculations
Polling Place Size	Population 20 years or older divided by the number of polling places in the municipality	DANE, *Registraduría Nacional del Estado Civil*, and author's calculations
Poverty	The Unsatisfied Basic Needs Index of 1993 and 2005 is linearly interpolated for each municipality.	DANE and author's calculations
Total Population	Total population	DANE

Variable	Description	Source
Age	Respondent's age	LAPOP
Corruption Public Sector	The variable is built with answers to the following question: "Taking into account your own experience or what you have heard, corruption among public officials is: very common, common, uncommon, or very uncommon." Numerical values are adjusted to make the variable increasing in corruption perceptions.	LAPOP
Crime Victim	The variable takes the value of 1 if the person has been the victim of a crime in the past year and 0 otherwise	LAPOP
Uninterested Politics	The variable is build with answers to the following question: "How much interest do you have in politics: a lot, some, little or none?" The variable is increasing in lack of interest in politics	LAPOP
Years Education	Number of years of education	LAPOP
Female	Gender variable. It takes the value of 1 if respondent is a female and 0 otherwise	LAPOP
Income Level	The 2011 and 2010 LAPOP surveys ask respondents to choose an income range out of ten in which their monthly income falls into. For the year 2012, LAPOP increased the number of income ranges. For the year 2012 answers were modified to make them compatible with the ones from previous years. The variable is increasing in reported income	LAPOP and author's calculations
Involved Community	Frequency at which the respondent helped to solve a problem in her community or neighborhood in the last year. The answers are originally grouped in four categories (Once a week, once or twice a month, once or twice a year, never) and the values assigned to each category are modified to make the variable increasing in involvement in community affairs	LAPOP and author's calculations
News	Attention given to the news. The variable is build with the answers to the question "About how often do you pay attention to the news, whether on TV, the radio, newspapers or the internet?" The answers are grouped in four categories (daily, a few times a week, a few times a month, rarely, never) and the values of each category are modified to make the variable increasing in attention to the news	LAPOP and author's calculations
Registered Voter	It takes the value of 1 if the person is registered to vote and 0 otherwise	LAPOP
Religiosity	Importance of religion. The variable is increasing in self reported importance of religion	LAPOP and author's calculations
Trust Community	Trust in the community. The variable is built with answers to the question "Would you say that people in this community are very trustworthy, somewhat trustworthy, not very trustworthy or untrustworthy?" Original values assigned to the answers are modified to make the variable increasing in trust	LAPOP and author's calculations

Table 9: Summary Statistics (Individual-level Analysis)

Variable	Observations	Mean	Std. Deviation	Min	Max
Panel A		Dependent Variables			
Turnout Suppression	3647	0.008	0.087	0	1
Vote Buying	3655	0.181	0.385	0	1
Panel B		Explanatory Variables			
Armed Group	3655	0.444	0.497	0	1
Electorate Size	3655	2843577	196594.4	2540350	3260987
Local Revenues	3655	43.914	32.873	1.039	100
Margin	3655	0.116	0.04218	0.0145	0.222
Polling Place Size	3655	644.585	307.855	340.707	1397.086
Poverty	3655	2.799	0.81	1.567	4.605
Total Population	3655	1603814	2673015	2726	7467806
Age	3655	36.522	14.511	17	89
Corruption	3517	3.405	0.789	1	4
Crime Victim	3652	0.203	0.402	0	1
Uninterested Politics	3655	2.862	0.944	1	4
Years Education	3655	9.928	4.563	0	18
Female	3655	0.493	0.5	0	1
Income Level	3655	4.495	1.892	0	10
Involved Community	3655	1.428	0.782	1	4
News	3655	3.613	0.777	0	4
Registered Voter	3655	0.816	0.387	0	1
Religiosity	3655	3.479	0.802	1	4
Trust Community	3655	2.898	0.887	1	4

This table presents summary statistics for the variables used in the individual-level analysis. For variables' definitions and sources, see Table 8.

Table 10: Determinants of Vote Buying (Few Registered Voters per Polling Station)

Dep. Variable:	Vote Buying				
	(1)	(2)	(3)	(4)	(5)
Polling Place Size	-2.701**	-1.315***	-4.146	-2.206***	-3.799***
	(1.333)	(0.478)	(3.544)	(1.151)	(1.179)
Electorate Size	-0.063	-0.307***	-2.943***	-0.442**	-0.082
	(0.169)	(0.067)	(0.804)	(0.216)	(0.064)
Restriction Poll. Place Size	Max. Size≤350	Median Size≤250	Max. Size≤350	Median Size≤250	Median Size≤250
Observations	146	1000	19	189	189
Municipalities	103	346	18	141	141
Model	Neg.Bin	Neg. bin	Neg.Bin	Neg.Bin	FE
Reports	Citizens	Citizens	Monitors	Monitors	Monitors

This table presents coefficients of Negative Binomial count models' and coefficients of a fixed effects linear model. The samples of models in columns (1) and (3) include only municipalities whose maximum number of registered voters per polling station is less than or equal to 350. The samples used by the models in columns (2), (4) and (5) include only municipalities whose median number of registered voters per polling station is less than or equal to 250. 'Citizens' indicates that the data for the dependent variable come from citizens' reports. 'Monitors' indicate that the data for the dependent variable come from election monitors' reports. All models include as controls the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election margin of victory. Polling place size and electorate size of the previous election are in logs. Standard errors clustered at the municipality level are in parentheses. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

Table 11: Determinants of Electoral Crime Reports (Electorate Size Measure based on Voting Age Population)

Dep. Variable:	Vote Buying		Turnout Suppression		Fraud	
	(1)	(2)	(3)	(4)	(5)	(6)
Polling Place Size	-1.100***	-0.920**	-0.872**	-1.253	-1.904***	-0.308
	(0.254)	(0.450)	(0.403)	(0.811)	(0.392)	(0.814)
Electorate Size (Voting age Pop.)	-0.366***	-0.579***	-0.203***	-0.011	-0.144***	-0.395**
	(0.028)	(0.080)	(0.049)	(0.258)	(0.052)	(0.166)
Observations	4351	1068	4351	1068	4351	1059
Municipalities	1098	632	1098	632	1098	632
Reports	Citizens	Monitors	Citizens	Monitors	Citizens	Monitors

This table presents coefficients of Negative Binomial count models. 'Citizens' indicates that the data for the dependent variable come from citizens' reports. 'Monitors' indicate that the data for the dependent variable come from election monitors' reports. All models include as controls: the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election margin of victory. Polling place size and electorate size of the previous election are in logs. Standard errors clustered at the municipality level are in parentheses. $***$ Significance at 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

Table 12: Determinants of Electoral Crime Reports SUR Models

	Citizens' Reports			Monitors' Reports		
Dep. Variable:	Vote Buying	Turnout Suppression	Fraud	Vote Buying	Turnout Suppression	Fraud
Polling Place Size	-0.346***	-0.123	-0.196***	-0.293	0.106	-0.002
	(0.065)	(0.077)	(0.042)	(0.19)	(0.069)	(0.052)
Electorate Size	-0.066***	-0.017	-0.01*	-0.19***	-0.012	-0.021**
	(0.009)	(0.011)	(0.006)	(0.033)	(0.012)	(0.009)
χ^2_2 (H_0: equal α^e's)			77.98			47.91
p-value			0.0			0.0
Observations			4351			1059

This table presents coefficients of Seemingly Unrelated Equation Linear Models. The first three columns present coefficients of the model that uses citizens' reports. The last three columns present results for the model that uses election monitors' reports. All models include the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and the previous election margin of victory as controls. Polling place size and electorate size of the previous election are in logs. At the bottom of the table it is presented a Wald test statistic for the null that the coefficient of electoral size is equal across equations an its corresponding p-value. Standard errors are in parentheses. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see Table 8.

Table 13: Monitors' Crime Reports and Local Elections

Dep. Variable:	Vote Buying				Turnout Suppression				Fraud			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Polling Place Size	-0.793*	-0.485***	-0.907**	-1.308	-1.082	-0.021	-0.155	-1.13	-0.32	-0.448	-0.199	-1.486
	(0.413)	(0.183)	(0.414)	(0.856)	(0.72)	(0.303)	(0.176)	(2.143)	(0.8)	(0.302)	(0.122)	(1.165)
Electorate Size	-0.107	-0.129**	-0.049	-0.133**	0.307	0.093	0.009	0.06	-0.111	-0.033	-0.008	-0.087
	(0.131)	(0.054)	(0.036)	(0.055)	(0.357)	(0.12)	(0.018)	(0.104)	(0.183)	(0.088)	(0.013)	(0.123)
Local Election Year	2.408	0.155	0.166	0.168	1.877	0.33	0.03	0.195	2.124	0.083	-0.004	-0.14
	(0.541)	(0.22)	(0.137)	(0.219)	(1.318)	(0.504)	(0.07)	(0.432)	(1.011)	(0.409)	(0.049)	(0.689)
Observations	1068	4486	4486	4486	1068	4486	4486	4486	1059	4486	4486	4486
Municipalities	632	1122	1122	1122	632	1122	1122	1122	632	1122	1122	1122

This table presents coefficients of a Negative Binomial model that uses monitors reports in columns (1), (5), and (9). Columns (2), (6) and (10) present multiple imputation results of a Negative Binomial model, columns (3), (7), (11) present multiple imputation results of a linear fixed effects model, and columns (4), (8) and (12) results of a Poisson fixed effects model. All multiple imputation results are estimated using 10 complete-datasets. All models include as controls: the log of total population, the lag of the Unsatisfied Basic Needs index, the lag of the share of local revenues in total revenues, and a previous election's margin of victory measure. Polling place size and electorate size of the previous election are in logs. $***$ Significance at the 1% level. $**$ Significance at the 5% level. $*$ Significance at the 10% level. For more detailed data definitions and sources, see the appendix.

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