The Impact of Election Fraud on Government Performance

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Abstract

I examine how election fraud affects the type of government corruption and red tape that inhibits economic growth, as measured by building permit approvals in the Philippines, which are often delayed due to requests for bribes. To identify effects, I exploit a switch to automated elections in 2010 that made committing fraud more difficult. Estimates from a research design comparing changes over time in previously high-fraud and low-fraud towns indicate that automated elections significantly reduced election fraud, and that this led to a sharp and sustained 15 percent increase in the number of building permits approved annually.

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1 Introduction

The promise of democracy is that it allows voters to hold elected officials accountable for their performance (Adsera, Boix, and Payne, 2003; Barro, 1973). However, when election outcomes can be manipulated via fraud, elected officials may no longer have any incentive to perform or to respond to their constituents' needs. Worse still, they may engage in corrupt behavior that is harmful to economic growth, for example by exploiting bureaucratic red tape to exact bribes from firms. A lack of electoral accountability may explain why, despite the rise of democratic institutions around the world, corruption and poor government performance remain persistent problems, especially in developing countries (Olken and Pande, 2012; Svensson, 2003). Poor government performance, involving unnecessary red tape and corruption, is widely believed to inhibit economic development through its effect on discouraging investment (Mauro, 1995; Méon and Sekkat, 2005; Fisman and Svensson, 2007; World Bank Group, 2016). This paper focuses on the question of whether reducing election fraud and restoring electoral accountability results in improved government performance.

Despite the intuitive appeal of linking election fraud to government performance and corruption, little evidence exists demonstrating the causal pathway. This is largely because research on the impact of election fraud has been hampered by a lack of election fraud measures, and of settings in which there is demonstrable election fraud. Important exceptions to the absence of work on elections and corruption include Ferraz and Finan (2008, 2011) and Ofosu (2019). Ferraz and Finan (2008, 2011) exploit data on corruption, constructed from publicly released expenditure audit reports, to identify the effect of reported corruption on electoral outcomes. They also examine the effect of reelection incentives on the corruption practices of incumbent politicians. However, these studies focus on Brazil, a country whose elections are not marred by election fraud (Fujiwara, 2015). Ofosu (2019) uses a field experiment to study the effect of election-day monitoring on politician responsiveness in Ghana. The objective of this paper is to complement this important literature by estimating the impact of a lasting, nationwide election reform on election fraud and on subsequent economic growth-inhibiting behavior by government.

I use data from a period of election reform in the Philippines, a developing country in East Asia whose elections have long been perceived to be fraudulent (Schaffer, 2005). Beginning in 2010, the Philippines switched from manual to automated elections. Compared to the slow and vulnerable manual election system, the automated election system was expected to reduce fraud during counting and canvassing by decreasing the time needed to generate election results from six weeks to one. The automated election system also employed new security and transparency features that made it difficult to get away with fraudulently changing election results (Mugica, 2015).

My empirical strategy exploits the differential impact of the election reform on towns in the Philippines that arises because all towns were assigned to use the automated election system in 2010, regardless of their pre-existing level of fraud. As a result, if the automated election system eliminates election fraud, towns that previously had high levels of election fraud should experience a greater reduction in fraud compared to towns that were already low-fraud. Thus, I can compare how government performance changes in the historically high-fraud towns relative to the low-fraud towns over the same period, due to the reduction in election fraud. This approach allows me to account for time-invariant differences between high-fraud and low-fraud towns, as well as differential shocks by region over time.[1] Because my preferred specification includes region-by-year fixed effects, the identifying assumption is that absent the switch to the automated election system, the historically high-fraud towns would have experienced changes in government performance similar to what the low-fraud towns in the same region experienced. I show empirical evidence in support of this assumption, as government performance changed similarly for high- and low-election fraud towns prior to the switch, and then diverged immediately after.

I base my identification of historically high-fraud towns on a list of consistent election hotspots published before the last manual election in 2007, based on earlier announcements by the Philip-

[1] In Section 2.2, I discuss economy-wide trends in GDP and construction growth in the Philippines. The Philippines avoided a recession during the last global financial crisis, due in part to reforms implemented after the 1997 Asian Financial Crisis that limited bank exposure (Business Mirror, 2018).

pine National Police.[2] Because this list was published several years before the election reform, I can show that high-fraud towns do not diverge from low-fraud towns before the reduction in election fraud. I use digit-based tests developed by Beber and Scacco (2008) to confirm the existence of election fraud in these areas before the election reform, and to determine the effectiveness of the election reform. These tests have been validated against actual election fraud by Weidmann and Callen (2013). Specifically, I examine the uniformity of the last digits of vote totals obtained by each mayoral candidate. Absent election manipulation, each digit from 0 to 9 should occur in vote totals as the last digit with equal frequency. When corrupt election officials make up numbers, they are likely to construct a fraudulent vote total that ends in 0 or 5. Thus, evidence of election fraud would be indicated by these digits – 0 or 5 – occurring as the last digit more frequently than other digits.

Since new construction, repairs, and improvements require permit approval from local building officials, I measure government performance using the number of building permits approved each year. I show descriptive evidence that government officials often ask for bribes during the permitting process, and that this is associated with longer waiting times. Consequently, the performance and integrity of local governments can directly affect the amount of investment activity that each town can attract.

The digit-based tests provide forensic evidence that historically high-fraud towns, identified from election watch lists published by the Philippine National Police, experienced more election fraud than other towns during the manual election period. These tests also show that election fraud in both groups decreased to undetectable levels during the automated election period. My finding is consistent with Crost, Felter, Mansour, and Rees (2013), who find that incumbents no longer were able to manipulate close elections in 2010, as they could in the last manual election in 2007. While the election reform did not appear to increase turnout rates, or hurt the re-election chances

[2]This list was built by journalists who had access to previous announcements of the Philippine National Police of election hotspots before the 2001, 2004, and 2007 elections. Consistent election hotspots are the towns that were in all three prior lists. In Section 3.1, I discuss how election hotspots are identified.

of incumbent politicians, I show that it significantly reduced their victory margins.

My main results indicate that reducing election fraud caused the number of approved building permits to increase by 15 to 17 percent. Since red tape and bribe requests are strongly associated with delays in the processing of building permits, this large increase in the number of approved building permits provides evidence that these obstacles likely decreased. Descriptive evidence based on data from selected Philippine cities surveyed by the World Bank are consistent with this interpretation, as the average waiting time to get a building permit decreased by 26 days. Moreover, there was no accompanying drop in the number of required procedures to get a building permit, indicating that it is unlikely that permitting standards were relaxed. One might reasonably expect that this improvement in government performance would facilitate economic growth, since building permits proxy for greater investment flows into the local economy (Berman, Felter, Kapstein, and Troland, 2013). These results are robust to the inclusion of a time-varying control for population, region-by-year fixed effects, and town-specific linear time trends. Estimates are also robust to the inclusion of a time-varying control for turnout rates, which is important as changes in the effective electorate have been shown in other settings to affect government behavior independently from election fraud (Fujiwara, 2015).

By providing the first evidence that reducing election fraud causes significant improvements in government performance, this paper contributes to related literature on the effects of elections (e.g., Besley and Burgess, 2002; Besley and Case, 1995; Ferraz and Finan, 2011; Repetto, 2017; Khemani, 2004; Klomp and de Haan, 2016; Ofosu, 2019). These studies find that politicians change their behavior in various ways as a result of electoral pressure. Ofosu (2019) in particular finds that politicians randomly exposed to more intense election-day monitoring in a field experiment become more likely to spend their Constituency Development Funds,[3] presumably because committing election fraud became more difficult. Building on these experimental findings by Ofosu (2019), I study the effects of a nationwide election reform in the Philippines, where elec-

[3]A common arrangement in developing countries, Constituency Development Funds are allocated by the central governments to local politicians to support development projects in their electoral constituencies.

tion monitoring has had little effect on election quality, likely because of weaknesses and capacity constraints in the judicial system.[4] I am also able to show that improving the conduct of elections caused improvements along non-spending measures of government behavior, which is important in contexts where spending can be subject to corruption. Overall, my findings complement previous related work, by showing that reducing opportunities for election fraud is an important part of what enables elections to hold government accountable.

I also contribute to an emerging literature that studies the political and economic effects of electronic voting (e.g., Fujiwara, 2015; Hidalgo, 2013; Moraes, 2012). Fujiwara (2015) showed that, by reducing the number of residual or uncounted votes, electronic voting effectively enfranchised less educated Brazilian citizens. This led to increased health spending and better health outcomes among the less educated. I contribute to this literature by focusing on a setting where election fraud has been a major problem. In doing so, I am able to show that technology can also lead to improved economic outcomes through its effect on election fraud, even when it does not change electoral turnout.

The findings of this paper have important policy implications. By demonstrating that automated election technology reduced election fraud, I show that electoral accountability can be improved by preventing candidates from manipulating vote totals in their favor. In contexts where monitoring and sanctions do not appear to suffice, demonstrating that technology can be used to achieve a meaningful reduction in election fraud adds to the arsenal that developing countries can use to improve the integrity of their elections. Perhaps more importantly, my main results demonstrate the positive effects of investing in credible elections on an economic outcome. To the extent that these results generalize to other contexts, they indicate that reducing election fraud can bring about meaningful differences in the type of government performance that directly affects economic development.

[4] A non-partisan election watchdog, The National Citizens' Movement for Free Elections or NAMFREL has monitored Philippine elections since 1986. Despite this, a perception of widespread election fraud continued. On average, less than 1 election-related case per year was tried by the Philippine Supreme Court over the period 1970-2003.

The rest of this paper is organized as follows. The next section presents the institutional background of the Philippines. Section 3 discusses the various data used in the study and describes the identification strategy. Section 4 measures and analyzes the variation in fraud induced by the switch in election systems and the resulting impact on government performance. Section 5 concludes.

2 Institutional Background

2.1 Introduction of Automated Elections

The Philippines is a developing country in Southeast Asia. It is divided into 18 administrative regions, with a total of 81 provinces, and 1,634 towns (Philippine Statistics Authority, 2015). Elections for national and local positions are held every three years; elections for the presidency and vice-presidency are held every six years. Prior to 2010, the Philippines used a manual election system. This provided many fraudulent ways to manipulate election outcomes even after the votes have been cast. I focus on the types of election fraud that can occur during or after Election Day (Mala and Pangilinan, 2011). Some examples include ballot box snatching (intercepting and destroying valid ballot boxes), ballot box stuffing (substituting fake ballots for valid ballots), vote padding or shaving (dagdag-bawas in the Filipino vernacular, which literally translates as "plus-minus"), and outright fabrication of election returns and canvassed results.

Several features of the manual election system made it particularly susceptible to election fraud. First, the ballots had blank spaces for voters to write the names of the candidates they are voting for. Voters could change their choices simply by crossing out the names and replacing them with new ones. While this feature allowed voters to change their mind or correct a mistake, its downside was that it potentially allowed people other than the voters to change the vote after ballots were already cast. Second, compared to the ballots used in the automated election system, the manual election ballots were relatively easy to duplicate (Singson, 2010). This made it easy to commit ballot stuffing, a type of election fraud where fake ballots are stuffed into ballot boxes to add to or even substitute for the valid ballots that actual voters filled out on Election Day. Perhaps most

importantly, the actual counting and canvassing of ballots was done by hand. This process was time-consuming and prone to error and manipulation. While it is difficult to obtain data on exactly how long the counting took prior to the reform, reports indicate it took more than 30 days (Mugica, 2015). That longer time window provided many opportunities to commit election fraud after ballots have already been cast, for example by changing vote totals during the tallying process, or by intercepting and changing the election returns reported by the polling centers (Mala and Pangilinan, 2011).

After a successful piloting of various voting technologies in regional elections in the Autonomous Region of Muslim Mindanao (ARMM),[5] the Commission on Elections chose and deployed the automated election system in the May 2010 national and local elections. Importantly for the purposes of this study, legal challenges meant that the implementation of the new election system was uncertain. In fact, the last legal challenge was dismissed by the Philippine Supreme Court just days before the May 2010 election (Pazzibugan and Bordadora, 2010). This means that it is unlikely for incumbent politicians to change their behavior before 2010 in response to the impending election reform.[6]

The new system addressed many of the vulnerabilities of the manual election system, from the security of ballots to the speed and accuracy of tallying votes. Figure 1 illustrates the differences between the manual and automated election ballots. The new ballots come with security features and are bar-coded in UV ink to their specific precincts. This means that the new ballots cannot be easily duplicated or used in other precincts. In addition, erasures were no longer allowed on the ballot. While this means that voters cannot change their mind, it also means that no one else can change their votes.

[5]The ARMM is an autonomous region in the Philippines that was formed in 1989. In decreasing order of size, the Philippines is divided into regions, provinces, and towns. The ARMM is the only region that has its own government. Only positions for this regional government was up for grabs in 2008, making 2010 also the first local elections in the ARMM to use the automated election system.

[6]I confirm this empirically in Figure 8.

Another important improvement of the automated election system over the manual election system is the deployment of precinct-level optical scanning machines to scan and transmit the votes. This greatly sped up the counting and canvassing process, because the machines report their vote tallies up the aggregation chain and to a transparency server immediately after counting. The transmission chain has the advantage of being fast and transparent, since the vote tallies obtained after aggregation at the central server must match the tallies reported to a transparency server. The speed of the automated counting also meant that the time window in which to commit election fraud was considerably shortened (Reyes, 2013). Randomized manual audits conducted after the 2010 and 2013 elections concluded that the precinct-level optical scanning machines were 99.6 percent accurate in 2010, and 99.9 percent accurate in 2013 (Crisostomo, 2015). In addition, surveys revealed that most Filipinos were satisfied with the conduct of automated elections (Social Weather Stations, 2010).

2.2 Construction in the Philippines

Construction tends to move in the same direction as overall economic growth in the Philippines, making it a useful indicator of economic activity in this setting. Figure 2a shows that growth in gross value of construction exhibited similar, though smaller, variation than overall GDP during the period 2001-2015. Unlike many countries, the Philippines avoided a recession during the global financial crisis, although economic growth slowed for a year in 2009 before picking up again. Government officials attribute this resilience to banking and fiscal reforms undertaken after the 1997 Asian Financial Crisis. Despite the economic slowdown in 2009, it can be seen from Figure 2b that the number of building permits has been growing at the national level since 2007, though the overall trajectory appears to be slightly downward, with near-zero growth rates in 2010 to 2011 and in 2013.

There are significant regulations facing the construction industry in the Philippines. Ostensibly, these regulations exist to ensure public safety. However, red tape and the resulting slow process for complying with these regulations also provide an opportunity for corrupt officials to demand and

receive bribes. Firms may be tempted to pay these bribes in the hopes of speeding up the application process. The quality of local governments thus affects the number of building permits that can be approved each year, through their control over red tape and the likelihood of bribe requests.

New construction, as well as repairs and improvements to existing structures, require building permit approval from the Office of Local Building Officials. Obtaining such approval often also requires ancillary permits from several different agencies, which greatly increases firms' exposure to corruption. World Bank Enterprise Surveys conducted in the Philippines indicate that some firms are asked for bribes in connection with various permit applications. Table 1 describes bribe incidence in the Philippines, by year and type of transaction.[7] In 2009, the last year before the electoral reform, 28 percent of firms that applied for a building permit in the Philippines were asked for bribes. This fraction decreased to 20 percent in 2015, a statistically significant decrease of 8 percentage points.[8]

Firms may be tempted to pay bribes in an effort to speed up the application process, which can be time-consuming and difficult to navigate. Table 2 summarizes data from the World Bank Doing Business Surveys on the number of procedures and waiting time to gain approval in selected towns in the Philippines. There is substantial variation across towns in the number of procedures and waiting time to gain approval. For instance, in 2011 obtaining a building permit took 169 days in the capital city of Manila, while in the adjacent city of Makati the waiting time was only 90 days. There is also variation over time. From 2008 to 2011, the average number of procedures increased from around 28 to 30, but the average waiting time decreased from around 132 days to 106 days.

However, as demonstrated by Freund, Hallward-Driemeier, and Rijkers (2015) using World Bank Enterprise Surveys from many countries, requests for bribes are associated with *longer*, in-

[7]The World Bank Enterprise Surveys gather firm-level data on the business environment in different countries. This includes asking about obstacles to doing business, including waiting time and incidence of bribe requests. The particular question about bribes during the building permitting process is: "In reference to that application for a construction-related permit, was an informal gift or payment expected or requested?"

[8]I am unable to use the data in a difference-in-difference framework because the sample consists of only firms that are in historically low-fraud towns.

stead of shorter, waiting times. I conduct similar analyses using only Enterprise Survey data from the Philippines to show that the same relationship exists. Figure 3 plots the kernel density of how long it takes to get a building permit approved in the Philippines, by whether or not firms were asked for bribes. It shows that while there is some overlap, the kernel density for the firms asked for bribes is shifted to the right of the firms not asked for bribes. Although this is not conclusive evidence of a causal connection, it does show that the descriptive evidence is consistent with bribery incidence being associated with longer wait times for building permit approval.

Table 3 presents estimates of the strength of the relationship between being asked for bribes and waiting time, using various specifications that include controls for important factors such as firm characteristics, managerial experience, worker productivity, and interaction with government officials. The association between bribery incidence and waiting time remains meaningful and significant even after controlling for these variables. These estimates suggest that being asked for a bribe is associated with a 40 to 60 percent delay in the time that it takes to get a building permit approved, which likely reduces the number of building permits that get through the permitting process every year. Taken together, these suggest that there may be room for improvements in government performance to increase the number of building permits by streamlining the permitting process and reducing corruption.

These improvements are expected to have important effects on prospects for economic development. Corruption in general has been found to be one of the most important determinants of investment (Asiedu and Freeman, 2009), and even a one percent increase in bribe incidence is associated with a 3 percent reduction in firm growth (Fisman and Svensson, 2007). Moreover, the waiting time and complexity of application process for building permits has been identified in a survey of firms as the biggest regulatory obstacle to doing business. Research in the U.S. finds that speeding up the permitting process could spur construction spending (as cited in World Bank Group (2016)). Since businesses would prefer to locate in areas where regulatory burdens are lighter, improving government performance in the building permitting process can encourage more investment and consequently spur greater economic growth.

3 Data and Empirical Strategy

3.1 Election Hotspots

To identify historically high-fraud towns, I obtained a list of towns that were consistently declared election hotspots for the 2001, 2004, and 2007 election cycles (Eder and Barrientos, 2007). Election hotspots are towns where the Philippine National Police (PNP) expects election-related incidents to occur. The PNP identifies towns as election hotspots ahead of each election if they satisfy the following criteria: 1) has a history of politically-motivated incidents, and 2) there are armed groups present in the area, such as separatist rebels or private army groups associated with influential politicians. A town is classified as an election hotspot if it meets both criteria (De Jesus, 2015).[9] Before each of the three reference election cycles, the Philippine National Police published a list of election hotspots. In 2007, journalists obtained historical lists and identified 181 towns that had been declared election hotspots in all three elections (Eder and Barrientos, 2007).[10] Because the consistent election hotspot label is not an official designation, it is unlikely for this group of towns to have been targeted by any government reform after its identification.

As shown in Figure 4, these consistent election hotspots are geographically distributed all over the Philippines. This means that my results are unlikely to be driven by just one or two regions of the Philippines that are significantly different from the rest. Also, since consistent election hotspots are located in several regions of the Philippines, I can control for town-specific and region-by-year specific fixed effects.

The implicit assumption in identifying high-fraud towns using the list of consistent election

[9] I also attempted to construct the groups of high-fraud and low-fraud towns empirically by applying the digit-based technique to historical vote totals, and examining which towns' vote totals deviated the most from the uniform distribution. However, because I have data on only three elections prior to the reform, it is difficult for me to distinguish between randomness and fraud using the digit-based technique.

[10] I have so far been unable to recover the raw list of election hotspots, disaggregated by election year, for those three elections on which the consistent elections hotspots list was based.

hotspots is that consistent election hotspots are also the towns that experienced greater election fraud during the manual election period. While there already is cross-country evidence showing a strong correlation between election fraud and violence (Weidmann and Callen, 2013), I also use a forensic measure of election fraud to provide evidence in favor of this assumption. That is, although I do not have information on the election-related incidents that led the Philippine National Police to classify towns as hotspots, I will be able to examine whether these hotspots exhibit more evidence of election fraud than other towns. I also use the forensic measure to show that the election reform reduced election fraud in the consistent election hotspots relative to the other towns. It is important to note that I use this forensic measure to identify election fraud at a group level. Due to data constraints, specifically that the election returns data are only available for three pre-election reform years and are not disaggregated into precinct-level data, I am unable to construct reliable town-level measures of election fraud.

3.2 Measuring Election Fraud from Vote Totals

Data on historical election results come from the Commission on Elections in the Philippines. The data include the final vote tallies obtained by each candidate for local office. In the Philippines, local elections for mayor, vice-mayor and town council are held every three years. The data are available for 2001, 2004, 2007, 2010, and 2013 elections. Thus, there are three elections in the manual election period and two elections in the automated election period. Both 2004 and 2010 were presidential election years. I focus on elections for mayor, as it is the highest and most important executive office in each town.

To forensically assess the level of fraud in each period using only the vote totals, I use the last digit tests proposed by Beber and Scacco (2008) and validated by Weidmann and Callen (2013).[11] The intuition behind these tests is that absent vote manipulation, each digit from zero to nine should be equally likely to appear as the last digit of a vote total. However, research in psychology and statistics (Boland and Hutchinson, 2000; Dlugosz and Müller-Funk, 2009) suggests that people

[11] Weidmann and Callen (2013) used data from the Afghanistan election of 2009 to validate the last digit measure of fraud. They showed that there is a positive relationship between the p-value of the last-digit test and the share of ballot boxes that showed physical signs of fraud.

favor some digits over others when they attempt to come up with random numbers. Specifically, individuals tend to choose rounded numbers that end in 0 or 5. This implies that analyzing the digits that appear in vote totals can provide forensic evidence of election fraud.

I operationalize this idea by examining the actual distribution of last digits of vote totals for mayoral candidates in high-fraud towns and low-fraud towns, before and after automated elections. I use chi-square goodness-of-fit tests to determine whether the observed frequencies of the last digits follow the predicted frequencies from a uniform distribution, as deviations from the uniform distribution indicate the presence of election fraud. In addition, I use Mann-Whitney U tests to determine whether the observed frequencies of last digits differ between high-fraud towns and low-fraud towns.[12]

As I will show in Subsection 4.1, these digit-based tests provide formal empirical evidence that the consistent election hotspots did in fact exhibit greater election fraud than other towns during that time period. However, digit-based tests also suggest that towns adjacent to consistent election hotspots appear to also have experienced greater election fraud than other towns. Institutionally, this may happen because groups that carry out election fraud may be shared between local politicians (Centre for Humanitarian Dialogue, 2011).[13] Since they appear to share the characteristics of high election fraud towns but are not in the list of consistent election hotspots, it is unclear whether they should be considered part of the comparison group or the treatment group.[14] Therefore, I compare consistent election hotspots to other towns that are not adjacent to consistent election hotspots but are located in the provinces that contain the consistent election hotspots. In the Appendices, I show that the main results are similar when all towns are included in the analysis.

Survey evidence from one province in the Philippines also suggest an association with this form of post-election fraud and vote buying, a type of pre-election fraud. Table 4, Panel B reports

[12]The Mann-Whitney U test is a nonparametric hypothesis test where the null hypothesis is that both distributions of last digits are drawn from the same distribution.

[13]Also sourced from phone interviews with Philippine National Police.

[14]Figure A.1 shows the distribution of last digits of vote totals for these towns, and compares them to the distributions for the consistent election hotspots and the comparison group (non-hotspots).

the responses to a survey in Isabela province, by historical fraud incidence in the town of residence of the respondents. While respondents in both high-fraud and low-fraud towns report vote buying, the incidence is significantly higher in the high-fraud towns.[15]

3.3 Measuring Government Performance

Lastly, data on approved building permits come from the Philippine Statistics Authority. Each year, field personnel are deployed to each town to gather data from the Office of Local Building Officials. Building permit data are based on copies of original application forms of approved building permits and from demolition permits. To compare how building permits change over time, and to address the possibility that some towns have no building permits in some years, I construct my dependent variable as the logarithm of the number of building permits plus 1. I complement this by using an inverse hyperbolic sine transformation on the number of building permits, which directly accounts for zeros. Table 4 describes the data for the hotspot (historically high-fraud) towns compared to non-hotspot (historically low-fraud) towns. On average, hotspot towns have fewer and lower- valued building permits approved each year.

3.4 Empirical Strategy

Because all towns adopted the automated election system regardless of their pre-existing level of fraud, there exists plausibly exogenous variation in election fraud at the town level. In particular, towns that previously experienced more election fraud will experience a relatively greater reduction in election fraud. If so, I can employ a research design that examines whether outcomes change more in towns that experience greater reductions in election fraud. As discussed in the previous section, I identify towns that experienced relatively high levels of election fraud by referring to a list of towns that were consistent election hotspots during the 2001, 2004, and 2007 national and local elections. I compare how outcomes change for this group of towns to the change in outcomes observed for towns that are non-adjacent but are still located in the same provinces that contain hotspots.

[15] I am grateful to Cruz (2019) for sharing this survey data.

I employ digit-based tests to formally show that fraud did in fact decrease more in the towns that were consistent election hotspots than in the comparison group. To do so, I first examine whether there was more election fraud, as measured by zeros occurring more often as the last digit, in the consistent election hotspots than in the other towns during the manual election period. I then test whether both groups exhibit any evidence of election fraud during the automated election period.

Next, I estimate a generalized difference-in-differences model to determine the impact of reducing election fraud on how many building permits get approved each year:

$$ln(building\ permit_{it}) = \beta(hotspot_i * automated\ elections_t) + c_i + u_t + \varepsilon_{it} \quad (1)$$

where $ln(building\ permit_{it})$, the log of building permits reported at the town level is the dependent variable; $hotspot_i * automated\ elections_t$ is the treatment variable that takes on a value of 1 for hotspot towns in the automated election period (2010 and later); and c_i and u_t control for town and year fixed effects, respectively. This is a generalized difference-in-differences model; the town fixed effects subsume a time-invariant indicator for being a hotspot, while the year fixed effects subsume an indicator for the automated election period. In other specifications I include region-by-year fixed effects, which account for the effects of regional shocks and allow towns in different administrative regions to follow different trajectories over time, and town-specific linear time trends, which allow each town to follow a different trend over time. Robust standard errors are clustered at the town level. I also conduct a series of placebo treatments to empirically determine how often effects of the magnitude I observe arise by chance, similar to Abadie, Diamond, and Hainmueller (2010).

Because towns in administrative regions of the Philippines are likely to be more similar to each other and be exposed to region-specific shocks than other towns, my preferred specification includes both town fixed effects and region-by-year fixed effects. The identifying assumption is that absent the switch to the automated election system, consistent election hotspots would have experienced changes in building permit approval similar to what other towns in the same administrative

region of the country experienced.

I test and relax this identifying assumption in the following ways. First, I graphically examine whether governance outcomes for hotspots and non-hotspots started diverging before 2010. Then, I formally test this by including an indicator for the year before automated elections were adopted. If hotspots and non-hotspots were not changing similarly during the manual election period, it would suggest that the change experienced by the non-hotspots in the automated election period is not a valid counterfactual for the change the hotspots would have experienced absent the reduction in election fraud. Also, since automated elections only started in 2010, and its successful implementation was uncertain before that year, if hotspots and non-hotspots had begun to diverge even before the 2010 elections, then the improvement in government performance might be due to something other than the reduction in election fraud. Finally, I include town-specific linear time trends, which allow for the possibility that towns followed different trends over time. To the extent that the estimates are robust to these specifications, these tests provide evidence to support the validity of this research design.

4 Results

4.1 Impact of Automated Elections on Election Fraud

I begin by forensically measuring the election fraud present in consistent election hotspots during the manual election years of 2001, 2004, and 2007, and then comparing it to the election fraud measured during the automated election years of 2010 and 2013. I then do the same for the non-hotspots. This allows me to examine how much election fraud levels in hotspots changed after the adoption of automated elections compared to the change experienced by non-hotspots during the same period, and whether being labeled a consistent election hotspot before the 2007 election led to the elimination of election fraud beginning with the 2007 manual election.

Figure 5 summarizes the main results from this exercise. It graphs the distribution of the last digits during the manual and automated election periods for hotspots and non-hotspots, and displays the results of chi-square goodness-of-fit tests that examine whether each digit appears with

equal probability. Both groups of towns appear to have experienced election fraud in the manual period, as evidenced by zeros occurring as the last digit more than 10 percent of the time. However, the election fraud experienced by hotspot towns seems to have been much worse. Zeros occur as the last digit almost 20 percent of the time in the hotspot towns, but only about 12 percent of the time in the other towns. Using a Mann-Whitney U test, I am able to reject the null hypothesis that the last digits of vote totals from hotspot towns are drawn from the same distribution as those from non-hotspot towns during the manual election period (*p-value = 0.0001*). That is, although the last digits from non-hotspot towns are not uniformly distributed in the manual election period, which indicates the presence of some election fraud, the observed frequency of zeros there is still smaller than what is observed in the hotspot towns. Moreoever, Figure 6 shows little evidence that election fraud was eliminated in 2007, after consistent election hotspots were identified by journalists.

Election fraud drops significantly in the automated election period in both hotspots and non-hotspots. Figure 5 shows that each digit becomes equally likely to appear as the last digit of a vote total after the election reform. Formally, chi-square goodness-of-fit tests fail to reject the null hypothesis that the observed frequencies of last digits from both the hotspots and the non-hotspots during the automated election period follow the predicted frequencies from a uniform distribution. In addition, a Mann-Whitney U test is now unable to reject the null hypothesis that the last digits of vote totals from hotspots are drawn from the same distribution as last digits from the non-hotspots (*p-value = 0.5798*). Since hotspots previously experienced greater election fraud than non-hotspots, together these tests indicate that the automated election system reduced election fraud significantly more in the hotspot towns than in the non-hotspot towns.[16]

To assess the likelihood that other groupings of towns may also have exhibited equal or greater amounts of election fraud during the manual election period, I conduct a series of tests on randomized groupings of towns. Figure 7 plots the resulting distribution of 1,000 placebo chi-square test

[16]To provide some evidence against the alternative explanation that hotspots may exhibit more zeros due to benign reasons such as illiteracy or lack of equipment to facilitate counting many ballots, I redo all of these analyses by splitting the hotspots and non-hotspots into below median and above median poverty incidence. As shown in Figure A.2 and Figure A.3, the election fraud results are unchanged when I split the sample in this way.

statistics from goodness-of-fit tests that test the observed distribution of last digits from randomized groupings of towns against the uniform distribution. All placebo chi-square test statistics I observe are smaller than the chi-square test statistic that I obtain when testing the observed distribution of last digits from high-fraud towns I use to define treatment. This helps to validates the use of the list of high-fraud towns to define treatment.

4.2 Impact of Automated Elections on Government Performance

Having shown that automated elections generated plausibly exogenous variation in election fraud, I now turn to the question of whether the large reduction in election fraud in hotspots caused government performance to improve. To do this, I compare the change in the log of total building permits approved in hotspots to the change in the log of total building permits approved in non-hotspots, before and after automated elections. To determine whether outcomes had started diverging even before treatment, in Figure 8 I graph the estimated divergence over time between hotspots and non-hotspots, relative to the difference in the year before the election reform. The coefficients graphed in Figure 8 come from estimating Equation 2, which is a dynamic version of Equation 1. Equation 2 accounts for town fixed effects, c_i, region-by-year fixed effects, λ_{rt}, and the final lag, 2013+, estimates the average effect through the end of the data:

$$ln(building\ permit_{it}) = \sum_{\substack{t=2006 \\ \neq 2009}}^{2013+} \delta_t \cdot (hotspot_i * 1(year_t)) + c_i + \lambda_{rt} + \varepsilon_{it} \tag{2}$$

There are a few things worth noting in Figure 8. First, both sets of towns appear to track each other in terms of changes in total building permits approved in the years before 2010. Notably, there is no evidence of divergence in 2007, the year the consistent election hotspots were identified by journalists. This is consistent with Figure 6 in that since election fraud was still present during the last manual election, politicians faced little electoral pressure. This also suggests that the research design is reasonable, in that if there had been no switch to automated elections, both groups would have continued tracking each other after 2010. In fact, there is a sharp rise in approved building permits starting in 2010 in the hotspots relative to the other towns. Permits issued then stay at that higher level throughout the automated election period. This effect manifests immediately during the first automated election year, giving further evidence that it is the reduction

in election fraud that caused the divergence. This also means that elected governments were able to quickly improve their performance along this dimension, which is plausible since reducing red tape and corruption requires only executive action. Third, the improvement in government performance persists in the succeeding years. This is exactly what one would expect if the improvement is due to the reduction in election fraud. That is, because election fraud decreased throughout the automated election period, we should expect that government performance would continue at a higher level for as long as governments are held accountable in elections.

Estimation results of Equation 1 are shown in Table 5. Panel A reports the effect of reducing election fraud on government performance, measured by the log of building permits. In Column 1, where only town and year fixed effects are included, the estimated effect of reducing election fraud is an increase in the number of building permits approved by about 16.6 percent. This represents a substantial improvement in government performance, and is statistically significant at the 5 percent level. In Column 2, my preferred specification, I add region-by-year fixed effects. The inclusion of region-by-year fixed effects means that I am comparing changes in hotspots to other towns located in the same region of the country, and accounting for region-specific shocks over time. Given that administrative regions in the Philippines tend to consist of similar towns, it is likely that this is the more appropriate comparison to make. The estimate in this preferred specification is 14.8 percent, and is statistically significant at the 5 percent level. In Column 3, I add a time-varying control for town-level population, which results in a similar estimate. Column 4 adds an indicator for hotspots in the year 2009, which formally tests whether the hotspots and other towns began to diverge even before the switch to automated elections. In line with Figure 8, the estimated coefficient on this leading indicator is not statistically significant, indicating that there is little evidence of divergence before automated elections. This also supports the assertion that election hotspots did not start diverging from other towns after being identified as high fraud in 2007. In Column 5, I add town-specific linear time trends, which allow each town to follow a different trend over time, to the preferred specification used in Column 2. This yields a 16.7 percent estimated increase in approved building permits. In Columns 6 and 7, I show that estimates are robust to including a

time-varying control for the turnout rate.[17] For each of these specifications, robust standard errors are clustered at the town level.

Panel B reports estimates using the same specifications, but where the inverse hyperbolic sine transformation is applied to the raw number of building permits. Doing so results in estimates that are similar, if slightly larger than in Panel A. Importantly, all estimates across both panels are significant at the 5 percent level. Taken together, they provide strong evidence that reducing election fraud improved government performance along an economically important dimension, the number of building permits that are approved each year.

Using a series of placebo treatments, I assess the likelihood that effects of this magnitude arise by chance. To do this, I re-assign treatment randomly among towns. In each of the 1,000 permutations, I re-construct my variable of interest as $(placebo\ hotspot_i * automated\ elections_t)$, and re-estimate Equation 1.[18] Results are presented in Figure 9, where the vertical dashed line at 0.148 corresponds to Column 2, Panel A of Table 5. Of the 1,000 placebo estimates, only 11 are larger than 0.148. This corresponds to a one-sided p-value of 0.01, indicating that is unlikely that this effect arises by chance.[19]

4.3 Potential Mechanisms

I now examine potential mechanisms through which reducing election fraud may lead to a significant improvement in government performance. Government performance might have improved because different, higher-performing people were elected to office, or because incumbents now face the threat of being removed from office if they perform poorly. In the Philippines, more than half of candidates are incumbent officials running again for the same office. These incumbents enjoy substantial electoral advantages, such as name recognition and perhaps more importantly, control over local public projects and employees. Incumbent candidates win about 80 percent of

[17] As will be shown in Section 4.3, there is little evidence that the election reform affected turnout rates in this setting.

[18] Since the election reform happened in 2010, it is unnecessary to re-assign a placebo treatment timing.

[19] Appendix Figure A.4 shows the distribution of placebo estimates for the full sample, where only 37 placebo estimates are larger than the true estimate.

the time when they run. Typically, incumbents win by a margin of 30 percentage points over their challengers, reflecting the advantage that they have in elections. Institutional reforms such as term limits have so far failed to reduce this advantage (Querubin, 2012).

If incumbents were cheating during the manual election period, then eliminating election fraud may lead to fewer incumbents getting reelected. In Table 6, I investigate this hypothesis directly by estimating the effect on the probability that incumbents win re-election. Estimates from various specifications show that there is little evidence that incumbents became less likely to win re-election as a result of the reduction in election fraud. I then examine whether incumbents may face more competition, which could induce them to perform better even though their likelihood of getting re-elected has not yet changed. To do so, I examine whether incumbent candidates get a smaller share of the total vote in the automated election period.

Table 7 presents evidence in favor of this mechanism. Across specifications, the estimated effect is approximately a 10 percentage point reduction in the incumbent victory margin, on average. That is, the difference between the vote share obtained by winning incumbents and the vote share obtained by their closest challenger decreased by 10 percentage points. This represents an erosion of about a third of the average victory margin previously experienced by winning incumbents. Since a decrease in the incumbent candidates' victory margins implies an increase in the vote share received by challengers, this means that elections between challengers and incumbents became more competitive in the automated election period. Thus, reducing election fraud appears to increase electoral pressure on re-elected candidates, even though it does not yet appear to decrease the number of incumbents that win re-election.

Previous research has found that voting technology can affect government behavior through de facto enfranchisement of voters (Fujiwara, 2015). Since a change in electoral turnout can affect politician behavior independently of a reduction in the ability to commit election fraud, it would be difficult to disentangle the two mechanisms. However, a change in the effective electorate is unlikely in the setting I study, as the more substantial changes occurred in the counting and

transmission of the ballots.[20] Moreover, in Table 8, I show that there is little evidence that the election reform affected turnout. This alleviates the potential concern that impending election reform increased faith in elections, and led to more people voting in 2010 and 2013. Taken together, these provide evidence that the technology used in the election reform reduced election fraud while not appearing to affect turnout. Thus, I interpret the increase in building permits as being due to the increase in electoral accountability brought about by the reduction in election fraud.

5 Discussion and Conclusion

The fundamental question addressed in this paper is whether election fraud causes poor government performance, as measured by a proxy for the type of government performance widely believed to inhibit economic growth. If so, reducing election fraud should improve government performance and perhaps eventually lead to higher economic growth. The election reforms in the Philippines present a unique opportunity to determine whether reducing election fraud can have such an effect. The switch to an automated election system in 2010 dramatically reduced election fraud, thereby increasing electoral accountability. The reduction in election fraud means that winning elections now requires that candidates actually receive the most votes from actual people, because candidates can no longer manipulate election results after the ballots have been cast. An analysis of vote shares reveals that the reduction in election fraud also appeared to negatively affect the victory margins enjoyed by winning incumbents, suggesting that the reform increased electoral pressure on incumbents, and that effects come from existing politicians choosing to improve their performance.

Importantly, the reduction in election fraud led to an immediate and sustained 15 to 17 percent increase in the number of building permits approved. This result is robust to using various specifications, including adding region-by-year fixed effects and town-specific linear time trends, as well as controlling for population and turnout. Secondary descriptive data from World Bank surveys

[20] In the manual elections, voters had to write the names of the candidates. In the automated elections, voters marked the boxes corresponding to the candidates they wished to vote for. In both election systems, ballots were paper-based.

indicate that the average number of procedures actually increased slightly around the time of election reform, so there is little evidence to suggest that building permit requirements were suddenly relaxed. Rather, it seems to be the case that improvements in government performance, whether by reducing red tape or by reducing bribe requests, are the drivers of this increase. The descriptive data from World Bank surveys are also consistent with this explanation, as the average waiting time decreased by 26 days and bribe requests went down by 8 percentage points. Since building permits are mandatory for any construction to take place, the removal of bottlenecks in the application process can have strong positive effects on the local economy. Of course, it remains an open question whether other, more difficult to measure, functions of government improve as a result of the reduction in election fraud. But what is clear is that at least in this context, reducing election fraud results in a large and sustained improvement in a measure of government performance that captures investment in the local economy.

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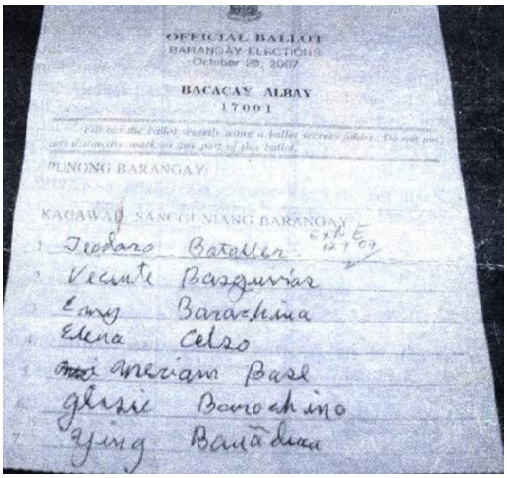
Figures

Figure 1: Examples of Ballots Used in Manual and Automated Elections.



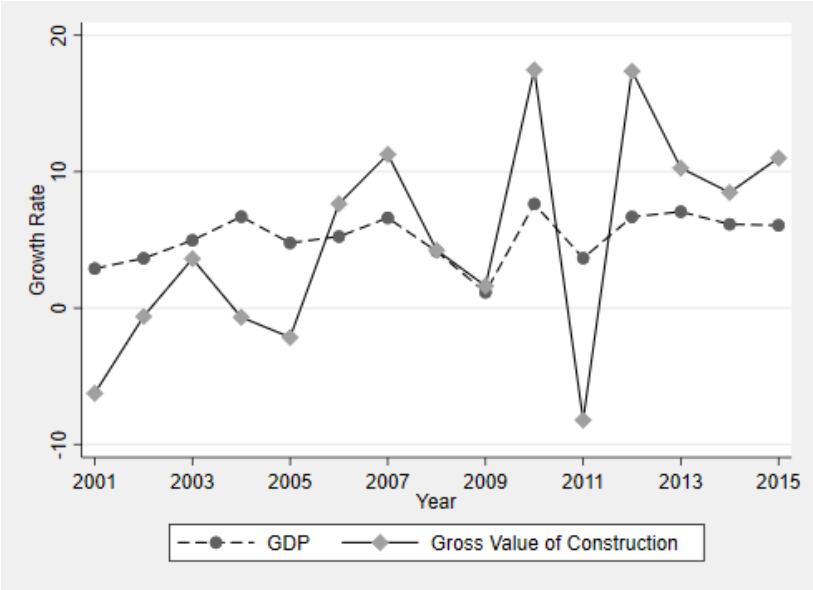
(a) Manual Election Ballot.

(b) Automated Election Ballot.

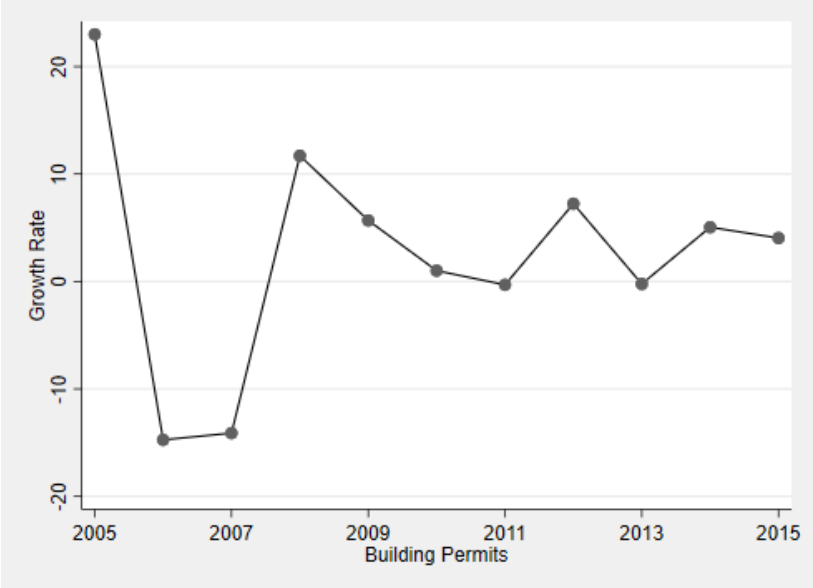
Note: Panel A shows a partially filled-out manual election ballot while Panel B shows part of the official sample ballot released in 2010. The manual election ballot is a piece of paper with blanks for voters to write names of candidates in. Voters are provided with a list of candidates in the voting booth. To use the automated elections ballot, voters must shade the appropriate circles and must use a marking pen. The new ballots also come with more security features to make it harder to duplicate.

Source: Manual election ballot is from G.R. No. 184268, Supreme Court of the Philippines, while the automated election ballot is the official sample ballot released each election by the Commission on Elections.

Figure 2: National Accounts of the Philippines.



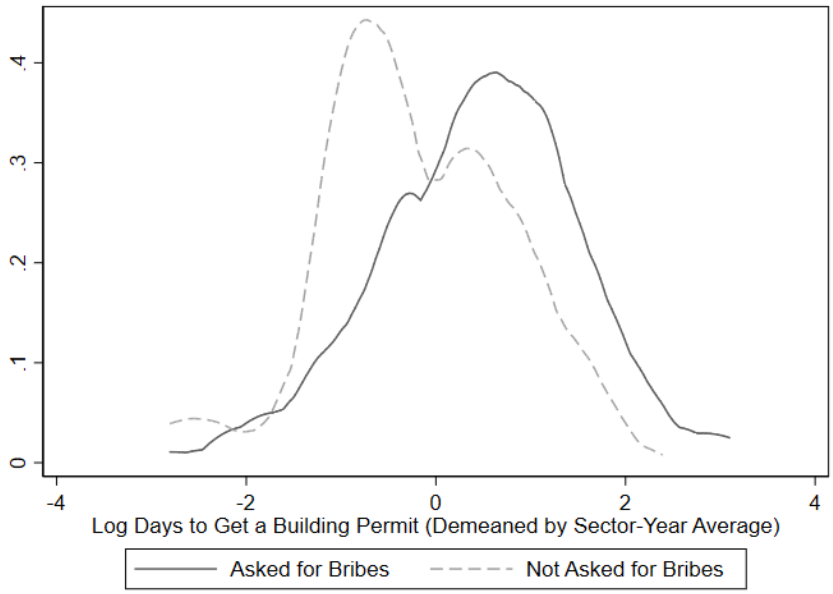
(a) Gross Domestic Product and Gross Value of Construction



(b) Building Permits

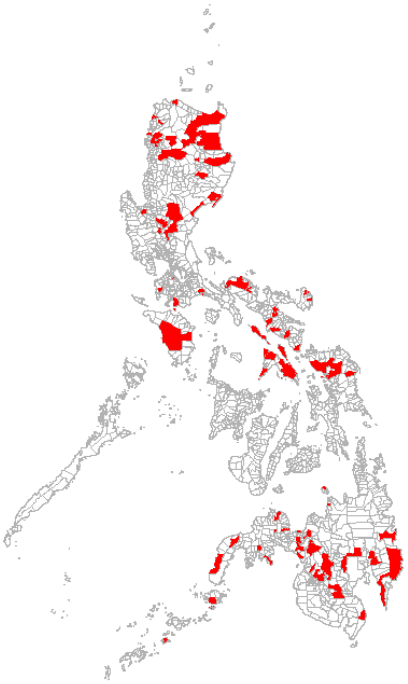
Note: Above is a time-series graph of annual GDP, Gross Value of Construction, and Building Permits growth rates in the Philippines. The latter is based on national data that is only available from 2004 onward. Disaggregated building permits data are only available from 2006 onward.

Figure 3: Kernel Density of the Amount of Time it Takes to Get a Building Permit in the Philippines, by Whether or Not a Firm is Asked for a Bribe.



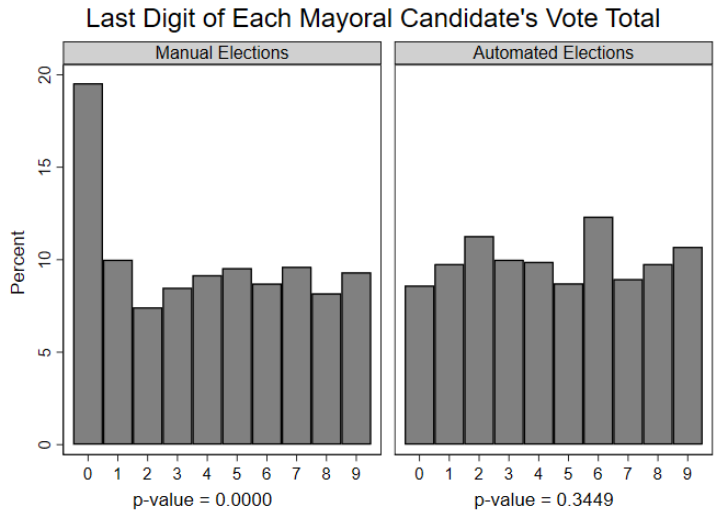
Note: These graphs show the difference in the time it takes to obtain approval for a building permit depending on whether or not a firm is asked for bribes. The unit of observation is an individual firm. Waiting time is measured in log days, and are demeaned by the sector-year average waiting times.

Figure 4: Map of the Philippines, with Consistent Election Hotspots Highlighted.

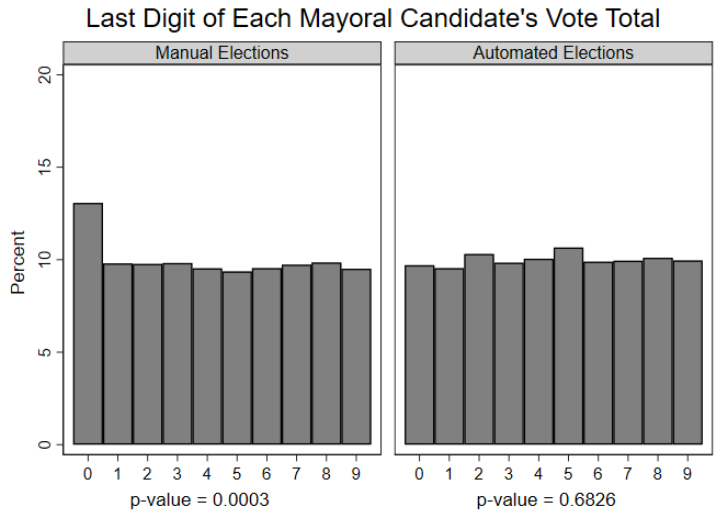


Note: This map shows the municipality-level boundaries in the Philippines. The shaded areas represent the towns that were consistently identified as election hotspots in 2001, 2004, and 2007. The Philippine National Police identifies towns as election hotspots ahead of each election. A town is declared an election hotspot if both of the following are true: 1) history of politically motivated incidents; and 2) presence of threat groups (rebel groups or private armies affiliated with politicians).

Figure 5: Distribution of Last Digits of Vote Totals From Mayoral Races, Manual Election Period vs. Automated Election Period.



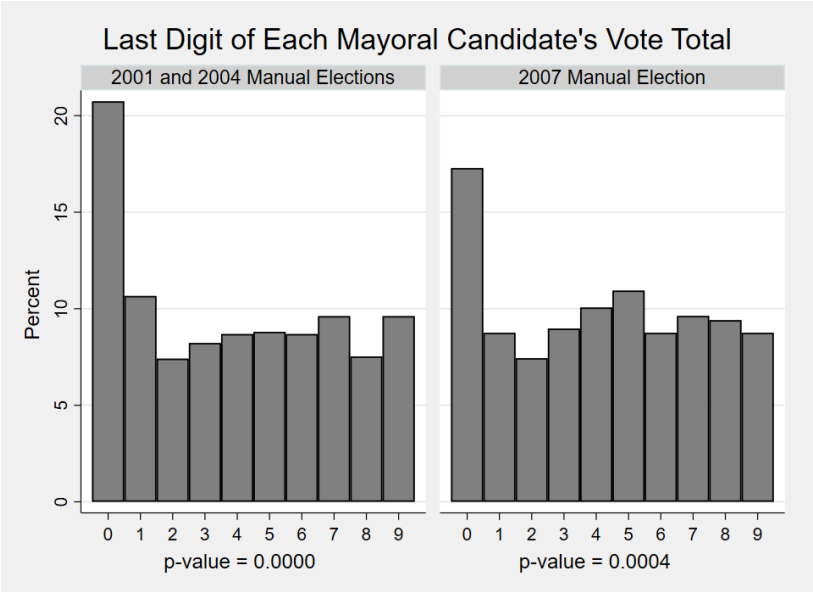
(a) Consistent Election Hotspot Towns.



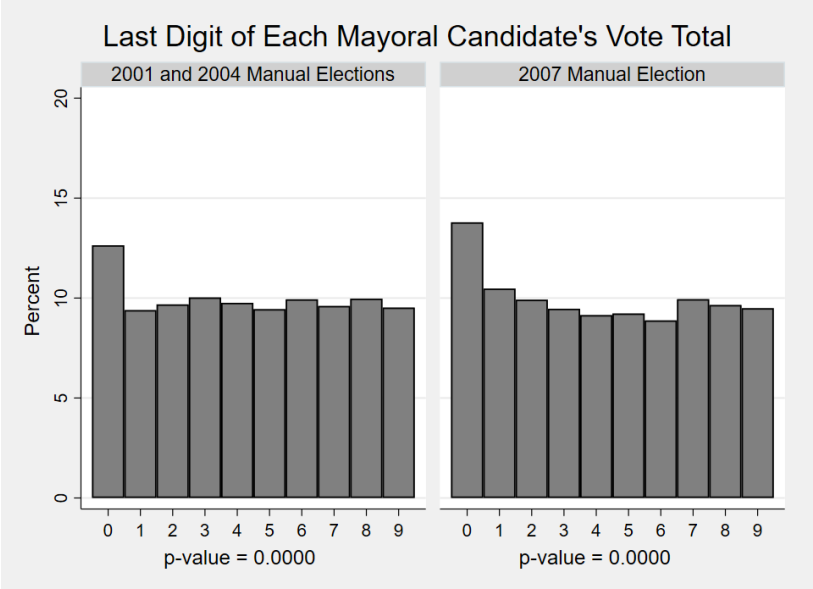
(b) Other Towns.

Note: The above figure shows the distribution of the last digits during the manual and automated election periods for hotspots and non-hotspots, and displays the p-values associated with chi-square goodness of fit tests that examine whether each digit appears with equal probability. The manual election histograms show last digits of vote totals for mayoral elections in 2001, 2004, and 2007. The automated election histograms show data from the 2010 and 2013 mayoral elections. Both hotspots and non-hotspots exhibit non-uniform distributions of last digits in the pre-2010 period, however results from a Mann-Whitney test rejects the null hypothesis that the two samples are drawn from the same population (*p-value = 0.0001*). In the automated election period, both groups of towns exhibit last digit distributions that are indistinguishable from each other (*p-value = 0.5798*) and from the uniform distribution.

Figure 6: Distribution of Last Digits of Vote Totals From Mayoral Races During the Manual Election Period, Before and After Identification of High-Fraud Areas.



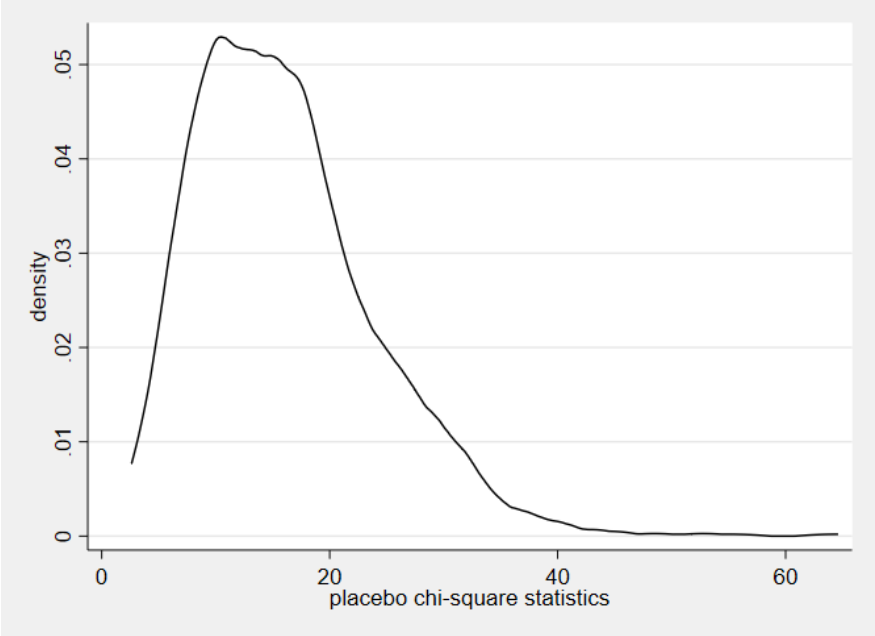
(a) Consistent Election Hotspot Towns.



(b) Other Towns.

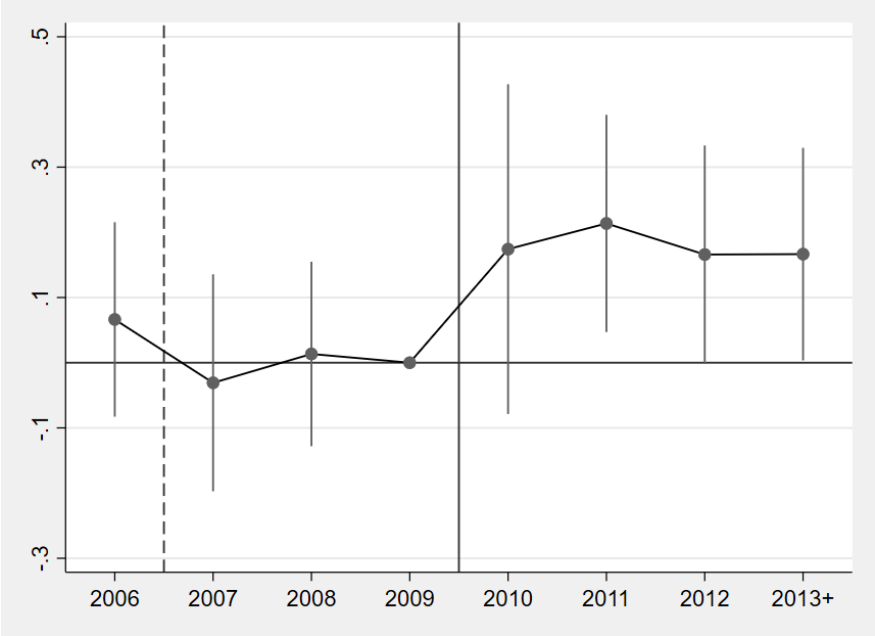
Note: The above figure shows the distribution of the last digits during the manual and automated election periods for hotspots and non-hotspots, and displays the p-values associated with chi-square goodness of fit tests that examine whether each digit appears with equal probability.

Figure 7: Placebo Test Statistics From Chi-Square Goodness of Fit Tests on Randomized Groupings of Towns



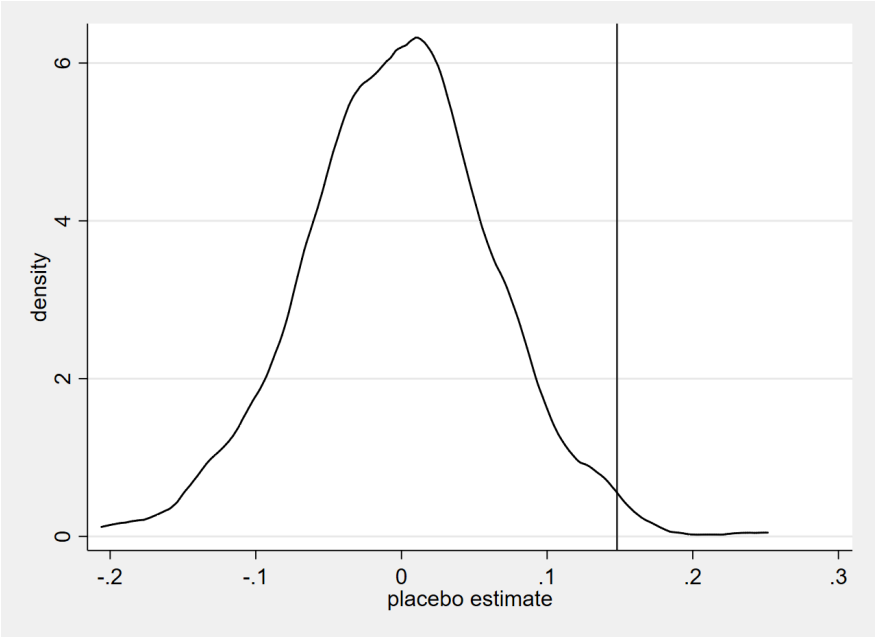
Note: This figure shows the resulting distribution of 1,000 placebo chi-square test statistics from goodness-of-fit tests that test the observed distribution of last digits from randomized groupings of towns against a uniform distribution. The maximum placebo chi-square test statistic I observe is 64.61, which is significantly smaller than the chi-square test statistic of 140.61, observed from the list of high-fraud towns I use to define treatment. This indicates that it is unlikely for other groupings of towns to have experienced higher levels of election fraud during the manual election period than the list of high-fraud towns that I obtained.

Figure 8: Estimated Divergence in Building Permits Before and After the Shift to Automated Elections Between Hotspots and Other Towns



Note: The graph shows the estimated divergence over time between building permits approved in hotspots and non-hotspots, relative to the difference in 2009, the final pre-treatment year. The solid vertical line separates the data into pre- and post-election reform, while the dashed vertical line separates the data into pre- and post-publication of the consistent election hotspots list. The estimates come from a regression that allows for dynamic effects, and includes indicators for town fixed effects as well as region-by-year fixed effects. The final lag, 2013+, estimates the average effect through the end of the data.

Figure 9: Placebo Estimates of the Effect of Reducing Election Fraud



Note: This figure shows the resulting distributions of 1,000 placebo estimates of Equation 1, with region-by-year fixed effects. The dashed line at 0.148 marks the corresponding point estimate from Column 2, Panel A of Table 5. About 1.1 percent of placebo estimates lie to the right of this point estimate.

Tables

Table 1: Bribe Incidence by Type of Transaction, 2009 and 2015.

	2009	2015	Difference
A. Transactions with Municipal Government.			
Electrical Connection	0.155	0.158	0.00301
	(0.364)	(0.367)	
Building Permit	0.282	0.200	-0.0824**
	(0.451)	(0.401)	
Tax Inspection	0.196	0.127	-0.0688***
	(0.397)	(0.334)	
Operating License	0.111	0.0946	-0.0168
	(0.315)	(0.293)	
B. Transactions with Other Government Offices.			
Import License	0.156	0.136	-0.0204
	(0.364)	(0.343)	
Water Connection	0.0938	0.207	0.113**
	(0.293)	(0.409)	
Total Informal Payments (in Philippine Pesos)	468264.7	291942.3	-176322.4
	(1091286.6)	(418248.2)	
Informal Payments as % of Total Sales	0.0110	0.00920	-0.00181
	(0.0489)	(0.0264)	

Note: The above table shows the fraction of firms that were asked for bribes in connection with various permit applications and other dealings with government officials. Panel A presents bribery incidence for transactions that are processed at the municipal level. Panel B presents statistics for transactions that are processed with other government offices outside of municipal governments, and thus not subject to the same electoral incentives. The last column reports the difference between years 2009 and 2015, and the outcome of t-tests. Data are taken from the World Bank Enterprise Surveys, where firms were asked whether "....an informal gift or payment expected or requested?"

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 2: Number of Procedures and Waiting Time Needed to Obtain a Building Permit in Twenty Sampled Cities in the Philippines in 2008 and 2011.

	2008		2011	
	Number of Procedures	Waiting time (in days)	Number of Procedures	Waiting time (in days)
Caloocan	29	135	31	109
Cebu	31	83	36	92
Davao	28	60	27	57
Lapu-Lapu	32	90	34	88
Las Piñas	25	134	27	102
Makati	25	125	26	90
Malabon	29	155	32	112
Mandaluyong	29	155	33	121
Mandaue	33	70	35	72
Manila	24	203	26	169
Marikina	25	121	28	91
Muntinlupa	30	141	31	108
Navotas	27	145	28	107
Parañaque	31	137	30	107
Pasay	27	161	31	121
Pasig	33	173	36	148
Quezon City	28	141	33	120
San Juan	31	175	33	144
Taguig	23	121	25	85
Valenzuela	25	123	28	91
Average	28.25	132.4	30.5	106.7

Note: Twenty cities were sampled by the World Bank for the 2008 and 2011 Doing Business Reports. The average waiting time decreased significantly, by almost 26 days, between 2008 and 2011 ($p= 0.01$). On the other hand, the number of procedures increased by 2 procedures ($p= 0.04$).

Table 3: The Relationship Between Being Asked for a Bribe and the Waiting Time for Approval of Building Permit in the Philippines.

Panel A: All firms	(1)	(2)	(3)	(4)	(5)
Firm is asked for bribe	0.624***	0.622***	0.462**	0.452**	0.428**
	(0.159)	(0.160)	(0.200)	(0.195)	(0.196)
Observations	173	173	119	117	105
Panel B: Subset of firms for which data on all variables are available	(1)	(2)	(3)	(4)	(5)
Firm is asked for bribe	0.335*	0.331	0.433**	0.431**	0.428**
	(0.199)	(0.223)	(0.204)	(0.206)	(0.196)
Observations	105	105	105	105	105
Sector fixed effects		x	x	x	x
Controls for firm and managerial characteristics			x	x	x
Control for worker productivity				x	x
Controls for firm visibility and interaction with government officials					x

Note: Each column in each panel represents a separate regression. The unit of observation is an individual firm in the World Bank Enterprise Survey. Standard errors are heteroscedasticity-robust.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 4: Descriptive Statistics, by Historical Election Fraud Incidence.

Panel A. Town-level summary statistics	All towns	High-fraud towns	Low-fraud towns
Log(number of building permits)	1.85	1.67	1.91
	(1.97)	(1.82)	(2.01)
Log(value of building permits)	5.53	5.32	5.61
	(5.00)	(4.89)	(5.03)
Arcsinh(number of building permits)	2.19	1.99	2.26
	(2.26)	(2.11)	(2.30)
Arcsinh(value of building permits)	5.93	5.71	6.00
	(5.33)	(5.23)	(5.36)
Observations	7070	1760	5310

Panel B. Respondents in Isabela province	All respondents	Respondents in high-fraud towns	Respondents in low-fraud towns
Fraction that report vote buying	0.49	0.62	0.45
	(0.50)	(0.49)	(0.50)
Observations	427	104	323
Fraction that report election-related violence	0.05	0.12	0.03
	0.22	0.33	0.17
Observations	1174	234	940

Note: Each cell contains the mean with the standard deviation in parentheses. In Panel A, the unit of observation is a town-year, and the time period spans 2006-2015. Panel B is based on a household survey conducted in the municipalities of the province of Isabela in 2010 (Cruz, 2019).

Table 5: The Effect of Reducing Election Fraud on the Total Number of Building Permits.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Dependent Variable: Log(1+Building Permits)							
Hotspot*Automated Elections	0.166**	0.148**	0.149**	0.145**	0.167**	0.170**	0.215**
	(0.0706)	(0.0665)	(0.0665)	(0.0737)	(0.0820)	(0.0768)	(0.0943)
Hotspot*Year Before Automated Elections				-0.0163			
				(0.0657)			
B. Dependent Variable: Arcsinh(Building Permits)							
Hotspot*Automated Elections	0.208**	0.186**	0.188**	0.185**	0.209**	0.218**	0.263**
	(0.0831)	(0.0782)	(0.0781)	(0.0863)	(0.0967)	(0.0908)	(0.112)
Hotspot*Year Before Automated Elections				-0.0125			
				(0.0769)			
Observations	7070	7070	7070	7070	7070	6030	6030
Town and Year Fixed Effects	x	x	x	x	x	x	x
Region-by-Year Fixed Effects		x	x	x	x	x	x
Control for Population			x	x	x		x
Control for Turnout						x	x
Town-Specific Time Trends					x		x

Note: Each column represents a separate regression. In Columns 6 and 7, the inclusion of a time-varying control for the turnout rate during elections leads to a smaller sample, due to missing turnout data for some towns. Results are robust to using this subsample. The unit of observation is town-year. Robust standard errors are clustered at the town level.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 6: The Effect of Reducing Election Fraud on the Probability That Incumbent Candidates are Re-elected

Dependent variable: Incumbent Win	(1)	(2)	(3)	(4)
Hotspot*Automated Elections	0.00457	-0.0171	-0.0581	0.109
	(0.0602)	(0.0622)	(0.0640)	(0.178)
Hotspot*Election Before Automation Elections			-0.0971	
			(0.0846)	
Observations	4057	4057	4057	4057
Town fixed effects	x	x	x	x
Region-by-year fixed effects		x	x	x
Town-specific time trends				x

Note: Each column represents a separate regression. Year 2001 is held out because it is used as the reference point for determining incumbents, so the election years included are 2004, 2007, 2010, and 2013. Robust standard errors are clustered at the town level.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 7: The Effect of Reducing Election Fraud on Incumbent Candidates' Victory Margins

Dependent Variable: Incumbent Victory Margin	(1)	(2)	(3)	(4)
Hotspot*Automated Elections	-0.102*	-0.0967*	-0.118**	-0.125
	(0.0513)	(0.0553)	(0.0541)	(0.437)
Hotspot*Election Before Automation Elections			-0.0589	
			(0.0824)	
Observations	2882	2882	2882	2882
Town fixed effects	x	x	x	x
Region-by-year fixed effects		x	x	x
Town-specific time trends				x

Note: Each column represents a separate regression. A candidate's victory margin is calculated by taking the difference between their vote share and the vote share received by the candidate with the second-highest number of votes. Year 2001 is held out because it is used as the reference point for determining incumbents, so the election years included are 2004, 2007, 2010, and 2013. Robust standard errors are clustered at the town level.

* Significant at the 10% level

** Significant at the 5% level

*** Significant at the 1% level

Table 8: Turnout Rates

	(1)	(2)	(3)	(4)
Hotspot*Automated Elections	-0.00841	-0.0106	-0.0125	-0.00265
	(0.00728)	(0.00966)	(0.0108)	(0.0147)
Hotspot*Election Before Automation Elections			-0.00550	
			(0.00843)	
Observations	3021	3021	3021	3021
Town and Year Fixed Effects	x	x	x	x
Region-by-Year Fixed Effects		x	x	x
Town-Specific Time Trends				x

Note: Each column represents a separate regression. The unit of observation is town-year. Robust standard errors are clustered at the town level.

* Significant at the 10% level

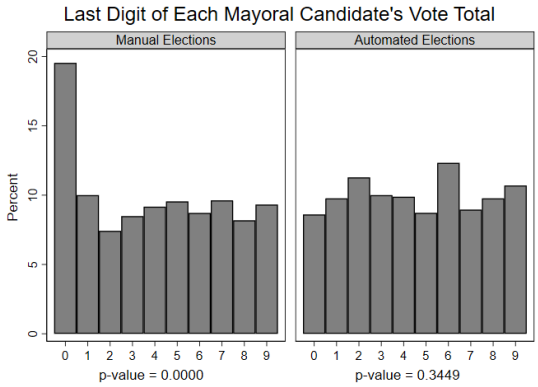
** Significant at the 5% level

*** Significant at the 1% level

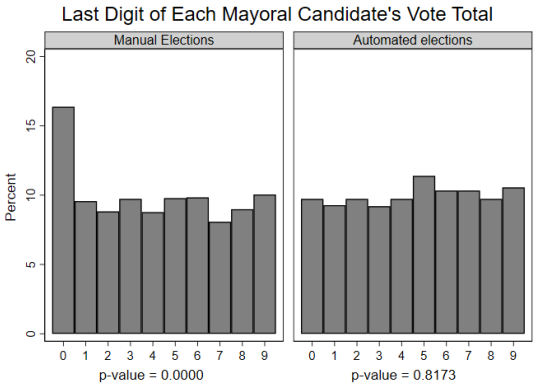
Appendix - For Online Publication

Figure A.1: Distribution of Last Digits of Vote Totals From Mayoral Races, Manual Election Period vs Automated Election Period

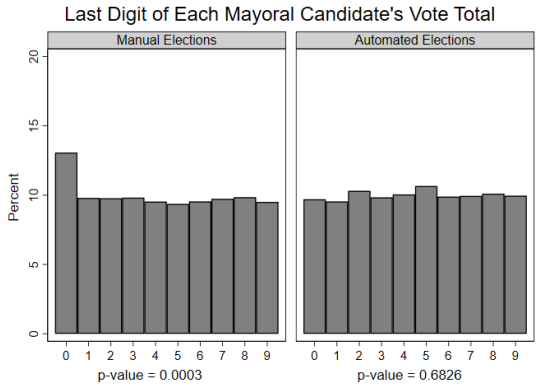
(a) Consistent Election Hotspot Towns.



(b) Towns Adjacent to Consistent Election Hotspots



(c) Other Towns.



Note: Each discrete distribution is tested against the predicted last digit frequencies from a uniform distribution. The manual election histograms show last digits of vote totals for mayoral elections in 2001, 2004, and 2007. The automated election histograms show data from the 2010 and 2013 mayoral elections. All groups of towns exhibit non-uniform distributions of last digits in the pre-2010 period, however results from Mann-Whitney U tests reject the null hypotheses that they are drawn from the same population. In the automated election period, all groups of towns exhibit last digit distributions that are indistinguishable from a uniform distribution.

Table A.1: The Effect of Reducing Election Fraud on the Total Number of Building Permits.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Dependent Variable: Log(1+Building Permits)							
Hotspot*Automated Elections	0.0499	0.120*	0.119*	0.133*	0.113	0.131*	0.141
	(0.0666)	(0.0644)	(0.0642)	(0.0720)	(0.0780)	(0.0730)	(0.0889)
Hotspot*Year Before Automated Elections				0.0563			
				(0.0665)			
B. Dependent Variable: Arcsinh(Building Permits)							
Hotspot*Automated Elections	0.0698	0.153**	0.152**	0.171**	0.150	0.170**	0.180*
	(0.0787)	(0.0761)	(0.0759)	(0.0847)	(0.0923)	(0.0866)	(0.106)
Hotspot*Year Before Automated Elections				0.0765			
				(0.0774)			
Observations	16340	16340	16340	16340	16340	13970	13970
Town and Year Fixed Effects	x	x	x	x	x	x	x
Region-by-Year Fixed Effects		x	x	x	x	x	x
Control for Population			x	x	x		x
Control for Turnout						x	x
Town-Specific Time Trends					x		x

Note: This table presents results when non-hotspot towns adjacent to consistent election hotspots are included in the analysis. Each column represents a separate regression. In Columns 6 and 7, the inclusion of a time-varying control for the turnout rate during elections leads to a smaller sample, due to missing turnout data for some towns. Results are robust to using this subsample. The unit of observation is town-year. Robust standard errors are clustered at the town level.

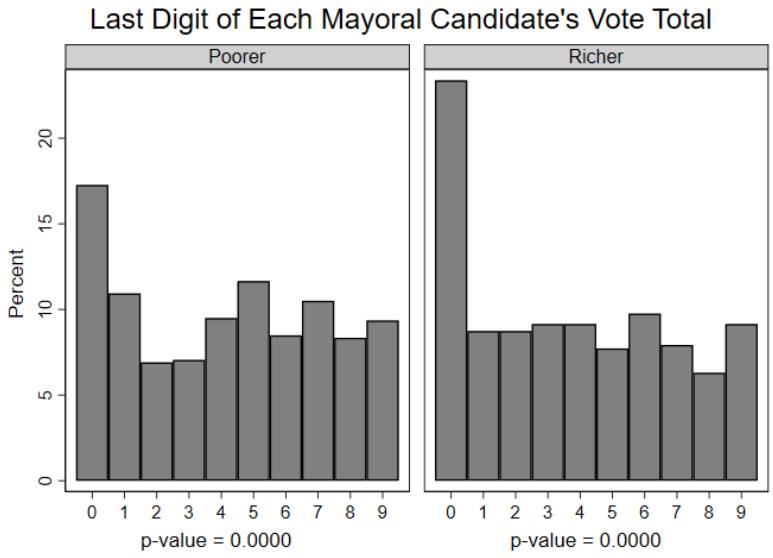
* Significant at the 10% level

** Significant at the 5% level

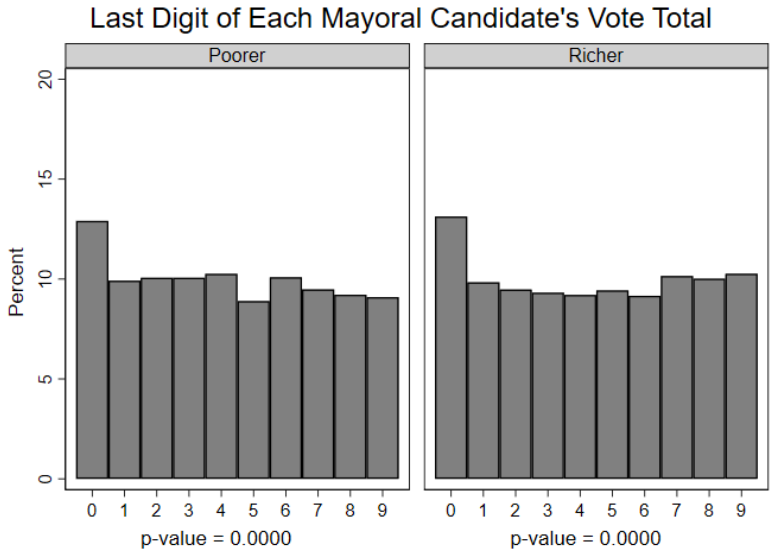
*** Significant at the 1% level

Figure A.2: Distribution of Last Digits of Vote Totals From Mayoral Races During the *Manual Election Period*, by Poverty Incidence

(a) Hotspots, by poverty incidence.



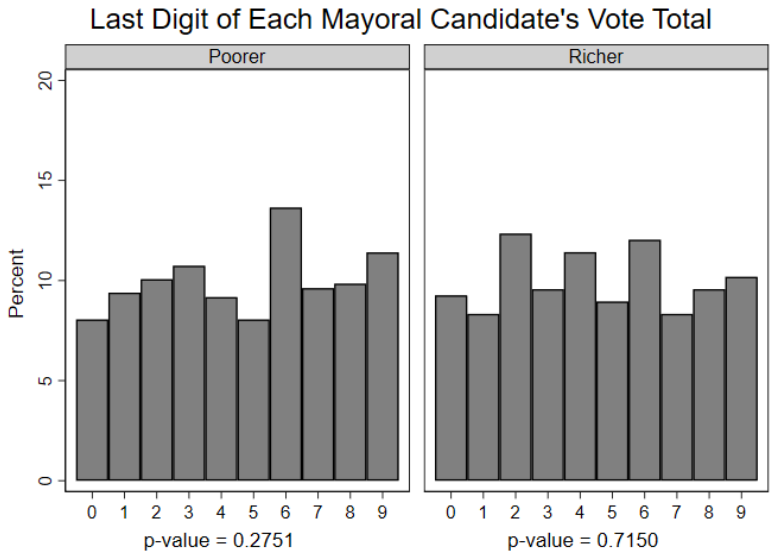
(b) Non-Hotspots, by poverty incidence.



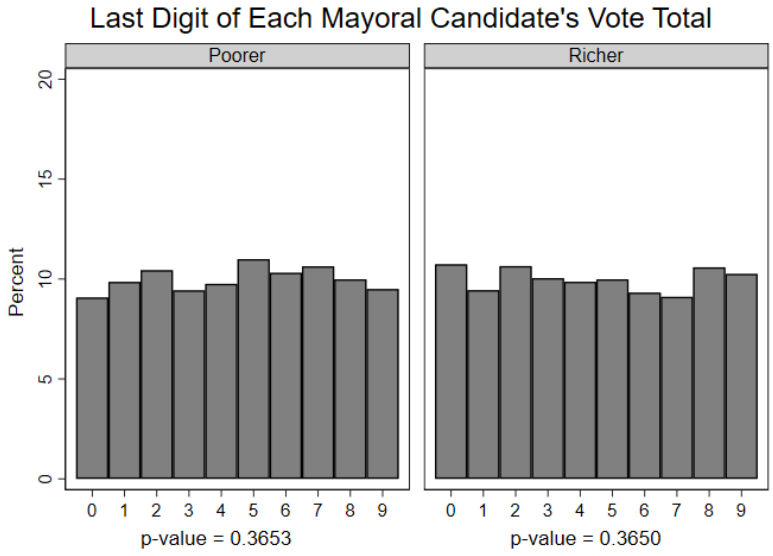
Note: Each discrete distribution is tested against the predicted last digit frequencies from a uniform distribution. The above figure shows the distribution of the last digits for hotspots and non-hotspots during the manual election period, by poverty incidence, and displays the p-values associated with chi-square goodness of fit tests that examine whether each digit appears with equal probability. These graphs show that the excess mass at zero can be found in both poorer and richer municipalities, and the general size of the mass parallels those found when the sample is not split into poorer and richer municipalities.

Figure A.3: Distribution of Last Digits of Vote Totals From Mayoral Races During the *Automated Election Period*, by Poverty Incidence.

(a) Hotspots, by poverty incidence.

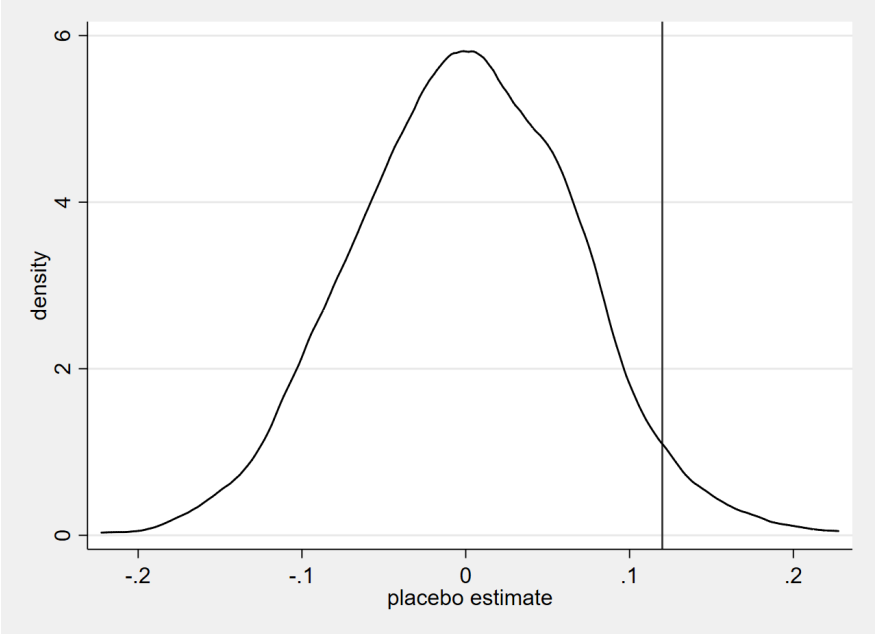


(b) Non-Hotspots, by poverty incidence.



Note: Each discrete distribution is tested against the predicted last digit frequencies from a uniform distribution. The above figure shows the distribution of the last digits for hotspots and non-hotspots during the automated election period, by poverty incidence, and displays the p-values associated with chi-square goodness of fit tests that examine whether each digit appears with equal probability. These graphs show that the distributions of last digits all follow a uniform distribution, even when the hotspot and non-hotspot municipalities are split into richer and poorer municipalities.

Figure A.4: Placebo Estimates



Note: This figure shows the resulting distributions of 1,000 placebo estimates of Equation 1, with region-by-year fixed effects. The dashed line at 0.120 marks the point estimate from Column 2, Panel A of Appendix Table A.1. Less than 4 percent of placebo estimates lie to the right of this point estimate.