THREE ESSAYS ON PUBLIC ECONOMICS


A Dissertation

by

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ABSTRACT

This dissertation introduces three essays on public economics with a focus on the developing country setting. The first two essays present evidence that the accountability generated by electoral institutions positively affects government performance. The last essay shows that exposure to inter-group conflict has lasting effects on the degree to which individuals favor members of their in-group. All three essays use data from the Philippines.

In the first essay, "The impact of election fraud on government performance," I study the effects of an election reform that happened in the Philippines in 2010. Using data on election results for mayor, I show that it reduced election fraud regardless of the pre-existing level of election fraud. I find that the reduction in election fraud caused building permit approvals to increase by 15 percent. Since delays in granting approvals are associated with bribe requests, this increase implies a significant improvement in government performance.

In the second essay, "Does electoral pressure lead to better government performance?," I study whether increased electoral pressure can increase the effort government officials put into their tasks. To measure effort, I use province-level data on the fraction of the affected population evacuated to prepare for tropical cyclones. This isolates effort because resources are pre-positioned and provided by national government agencies. I find that governors who face re-election incentives increase evacuations by twenty percentage points.

In the third essay, "Does conflict exposure increase in-group bias? Evidence from experiments in the Philippines," we exploit community-level exposure to two types of conflict that exist in the Philippines: a Muslim separatist movement and a communist insurgency. We merge data on community-level conflict exposure with information about giving to partners of different religion

or ethnicity measured using experimental games. Our results indicate that Muslims exposed to the inter-religious conflict become more biased against other Muslims, while being exposed to the indiscriminate communist conflict leads to opposite effects.

# DEDICATION

*To my parents, Milagros Momongan Peralta and Engelbert Kasilag Peralta*

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CONTRIBUTORS AND FUNDING SOURCES

**Contributors**

This work was supervised by a dissertation committee consisting of Professor Mark Hoekstra, and Professors Catherine Eckel and Jason Lindo of the Department of Economics and Dr. Bethany DeSalvo of the Texas Research Data Center.

Part of the data analyzed for Section 4 was provided by Professors Katerina Sherstyuk and Patricio Abinales of the University of Hawaii. Section 4 is joint work with Natalia Candelo, Sun-Ki Chai, Debbie Gundaya, Catherine Eckel, Katerina Sherstyuk, and Rick Wilson.

All other work conducted for the dissertation was completed independently by the student.

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# 1. INTRODUCTION

To design effective public policy, it is necessary to obtain both an accurate understanding of existing problems as well as the effectiveness of various policy interventions. In this dissertation, I study the lasting effects of conflict exposure on social norms regarding bias, which helps explain why countries recover differently following civil conflict. I also provide causal evidence that reducing election fraud and increasing electoral pressure both generate significant improvements in government performance.

Section 2 studies the causal effects of an election reform in the Philippines on economic and political outcomes. In 2010, the Philippines switched to using voting machines to count ballots instead of by hand. Because the new system was more secure and faster at generating results, election fraud became much harder to commit. Using newly-developed forensic measures of election fraud, I show that this reform drove election fraud in mayoral elections down to undetectable levels, regardless of the pre-existing level of election fraud. Leveraging this as-good-as random variation in election fraud, I then examine the effect on building permit approvals. This is a useful measure of mayoral performance since the permitting process is directly controlled by mayors and building permit approvals are a leading indicator of economic activity (Berman, Felter, Kapstein, and Troland, 2013). My main result is that the reduction in election fraud caused a 15 percent increase in the number of building permits approved each year. In addition to this, I estimate a substantial erosion in incumbent victory margins, without an accompanying decrease in their likelihood of winning re-election. This pattern of results indicates that the effect on performance may have worked through an increase in the electoral pressure felt by incumbents.

In Section 3, I directly examine this idea by specifically asking whether increases in electoral pressure, due to the salience of upcoming elections, causes governors to exert more effort when conducting mass evacuations in preparation for tropical cyclones. This performance measure directly captures effort, as all the necessary resources are provided by the national government and governors need only to coordinate evacuation efforts within their province. Since governors become ineligible to seek re-election after serving three consecutive terms, I can use variation in electoral pressure generated by these term limits and the time to the next election. Specifically, since upcoming elections do not increase pressure on ineligible governors, I identify the causal pathway by comparing how evacuations change as elections approach in provinces headed by eligible governors relative to provinces headed by ineligible governors. Results indicate that increased pressure from upcoming elections causes a 20 percentage point increase in the fraction of affected population evacuated. Robustness exercises indicate that this effect grows as elections draw closer and fades when elections are farther away.

Section 4 studies the effect of conflict exposure on the institution of fairness to religious in-group counterparts. With Catherine Eckel, Katerina Sherstyuk, Rick Wilson, Natalia Candelo, Sun-Ki Chai, and Debbie Gundaya, I use data gathered from previous studies to examine how conflict exposure affects religious bias in the Philippines. Understanding how conflict can have lasting effects on exposed individuals is important, especially as it affects the type of behavior that matters for economic development. To study this, we exploit as-good-as-random community-level exposure to two types of conflict that exist in the Philippines: a Muslim separatist movement and a communist insurgency. We merge data on community-level conflict exposure with information about giving to partners of different religion or ethnicity. Our results indicate that Muslims exposed to the inter-religious/ethnic conflict become more biased against

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other Muslims, while being exposed to the indiscriminate communist conflict leads to opposite effects. This implies that the nature of the civil conflict, together with the identities of the groups involved, interact with each other to mediate the effect on bias. Public policies designed to foster unity after civil conflict would benefit from taking this into account.

# 2. THE IMPACT OF ELECTION FRAUD ON GOVERNMENT PERFORMANCE

## 2.1    Introduction

The promise of democracy is that it allows voters to hold government accountable for their performance (Adsera, Boix, and Payne, 2003; Barro 1973; Ferejohn, 1986). However, when election outcomes can be manipulated via fraud, government officials may no longer have an incentive to perform or respond to their constituents' needs. Worse, elected officials may engage in corrupt behavior that inhibits economic growth, such as exploiting bureaucratic red tape to exact bribes from firms. This lack of electoral accountability perhaps explains 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 caused by unnecessary red tape and corruption has been shown to significantly inhibit economic development through their effect on discouraging investment (Mauro, 1995; Meon and Sekkat, 2005; Fisman and Svensson, 2007; World Bank Group, 2016). This section 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, to my knowledge there has been no evidence 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 settings in which there is demonstrable election fraud. Important exceptions to the absence of work on elections and its relationship with government performance include Ferraz and Finan (2008 and 2011). They exploit data on corruption constructed from publicly released expenditure audit reports to identify the effect of reported corruption on electoral outcomes, as

well as the effect of reelection incentives on corruption practices of incumbent politicians. Importantly, however, these studies focus on Brazil, a country whose elections are not marred by election fraud (Fujiwara, 2015). The purpose of this section is to complement this small but important literature by being the first to estimate the impact of election fraud on economic growth-inhibiting behavior by government.

It does so by using data from an election reform in the Philippines. The Philippines is 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. Relative to the slow and vulnerable manual election system, which on average required about six weeks to generate official results (Mugica, 2015), the automated election system was expected to reduce election fraud in several ways. Specifically, the reform was designed to reduce election fraud committed during counting and canvassing by decreasing the time needed to generate official results from six weeks to one, and also by introducing security and transparency features that make it difficult to successfully change election results after the fact.

To identify the effect of election fraud on government performance, I exploit the differential impact of the election reform across regions 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, then areas with previously high levels of election fraud will experience a greater reduction in fraud compared to the low-fraud areas. This allows me to compare how government performance changes in the historically high-fraud towns relative to the low-fraud towns over the same period. Since my preferred specification includes region-by-year fixed effects, the specific 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. Importantly, I show that the empirical evidence is consistent with this assumption, as both high- and low-election fraud towns changed similarly in the years prior and then diverged immediately after the switch.

To measure election fraud, I use the digit-based tests outlined by Beber and Scacco (2012), and validated against actual election fraud by Weidmann and Callen (2013), to examine the uniformity of the last digits of vote totals obtained by each mayoral candidate. Evidence of election fraud would be if some digits, such as zero or five, occur as the last digit more frequently than others. Government performance is measured using the number of building permits approved each year at the town level. New construction as well as repairs and improvements require permit approval from local building officials. I show descriptive evidence that government officials often ask for bribes during the permitting process, and that this is associated with longer waiting times. Thus, the performance and integrity of local governments can have a direct impact on the amount of investment activity that each town can attract.

Digit-based tests provide evidence that historically high-fraud towns indeed experienced significantly greater election fraud than other towns during the manual election period. The tests also show that election fraud in both groups was driven to undetectably low levels in the automated election period, a finding that is consistent with Crost, Felter, Mansour, and Rees (2014), who find that incumbents are no longer able to manipulate close elections in 2010 compared to 2007.

Results indicate that reducing election fraud caused the number of approved building permits to increase by 15 to 17 percent. Since red tape and the ensuing bribe requests cause delays in the processing of building permits, this large increase in the number of approved

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building permits provides suggestive evidence that these obstacles likely decreased. Descriptive 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 without a corresponding drop in the number of required procedures. In addition, one might also reasonably expect that this improvement in government performance will facilitate economic growth, since building permits proxy for greater investment flows into the local economy (Berman, Felter, Kapstein, and Troland, 2013). These estimates are robust to the inclusion of a time-varying control for population, the inclusion of region-by-year fixed effects, and the inclusion of town-specific linear time trends, which allow each town to follow a different trend over time.

These findings have important implications for policy. First, by showing that automated election technology reduced election fraud, I demonstrate that electoral accountability can be increased by preventing candidates from manipulating vote totals in their favor. Perhaps more importantly, results here demonstrate the positive effects of investing in credible elections on an outcome. To the extent that results here generalize to other contexts, the findings indicate that reducing election fraud can result in meaningful differences in the type of government performance that directly affects economic development.

## 2.2 Institutional Background

### 2.2.1 Introduction of Automated Elections and Research Design

The Philippines is a constitutional republic in Southeast Asia. It is currently divided into 18 administrative regions, containing a total of 81 provinces, and 1634 towns (Philippine Statistics Authority, 2015). Elections for national and local positions are held every three years, except for the presidency and vice-presidency, which are held every six years. Prior to 2010, these elections

used a manual election system. The use of a manual election system provided many fraudulent ways to manipulate election outcomes even after the votes have been cast. I specifically focus on the types of election fraud that occur during or after Election Day (Mala and Pangilinan, 2011). Examples of these 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 in which voters were to write down the names of the candidates they wished to vote for. The 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, the downside was that it potentially allowed people other than the voters to change their vote after ballots have already been 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. Lastly, and 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 could take more than 30 days (Mugica, 2015). The longer time window provided many opportunities to commit election fraud after ballots have already been cast, for example by changing the vote totals during the tallying

process, or by intercepting and changing the election returns reported by the polling centers (Mala and Pangilinan, 2011).

The Philippines has tried to switch to automated elections since the 1990s. After several false starts and a successful piloting of various voting technologies in regional elections in the Autonomous Region of Muslim Mindanao (ARMM),[1] the Commission on Elections finally chose and deployed the automated election system in the May 2010 national and local elections. The new system addressed many of the vulnerabilities of the manual election system, from the security of ballots to the speed and accuracy of vote tallying. First, the new ballots come with security features and are bar-coded in UV ink to the specific precincts they were issued for. This means that the new ballots cannot be easily duplicated or used in other precincts. Second, 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. 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, since the precinct-level optical scanning 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. Randomized manual audits conducted after the 2010

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[1] 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.

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).

### 2.2.2 Construction in the Philippines

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 to comply with these regulations also provide an opportunity for corrupt officials to request and receive bribes. Firms may be tempted to pay such bribes in the hopes of speeding up the application process. Thus, the quality of local governments affects the number of building permits that can be approved each year through their control over red tape and the likelihood of firms being asked for bribes.

New construction, as well as repairs and improvements to existing structures, require building permit approval from the Office of Local Building Officials. Obtaining building permit approval often requires ancillary permits from several different agencies as well, which greatly increases firms' exposure to corruption. Table A.1 summarizes World Bank data 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 takes 169 days in the capital city of Manila, while in the adjacent city of Makati the waiting time is 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.

The wide variation in waiting times is noteworthy because there is evidence that longer approval times are associated with requests for bribes in many countries around the world (Freund, Hallward-Driemeier, and Rijkers, 2015). Figure A.1 plots the kernel density of how

long it takes to get a building permit approved in the Philippines, by whether or not firms are 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 A.2 presents regression results estimating the strength of this relationship 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. Taken together, this suggests there may be room for improvements in government performance to increase the number of approved building permits by streamlining the permitting process and reducing corruption.

Such improvements could well 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 just a one percent increase in bribe incidence is associated with a three 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 such regulatory burdens are lighter, improving government performance in the building permitting process can encourage more investment and consequently greater economic growth.

## 2.3     Introduction of Automated Elections and Research Design

### 2.3.1 Measuring Election Fraud from Vote Totals

I obtained elections data from the Commission on Elections in the Philippines. The data contain 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 elections data are available for 2001, 2004, 2007, 2010, and 2013 elections. This yields three elections in the manual election period and two elections in the automated election period. Two of the elections, years 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 vote totals, I employ the digit-based tests proposed by Beber and Scacco (2012), and validated by Weidmann and Callen (2013). The idea behind these tests is that absent election fraud, 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) suggest that people favor some digits over others when they attempt to manipulate vote totals. The implication of this is 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 areas and low-fraud areas, before and after automated elections. I use chi-squared tests to determine whether the observed frequencies of the last digits follow the predicted frequencies from a uniform distribution, and Mann-Whitney U tests to determine whether the observed frequencies of last digits differ between high-fraud areas and low-fraud areas.

### 2.3.2 Election Hotspots

To identify historically high-fraud areas, I obtained a list of towns that were consistently declared election hotspots from 2001 to 2007 (Eder and Barrientos, 2007). Election hotspots are towns where the Philippine National Police (PNP) expects election-related violence 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).[2]

As shown in Figure A. 2, consistent election hotspots are geographically distributed all over the Philippines. This means that estimated 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 shocks.

The implicit assumption in identifying high-fraud areas using the list of consistent election hotspots is that consistent election hotspots are also the towns that experienced greater election fraud during the manual election period. The digit-based tests discussed earlier confirm that the consistent election hotspots did in fact exhibit greater election fraud compared to 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 the presence of groups that carry out election

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[2] I also investigated the possibility of identifying high-fraud and low-fraud towns empirically by examining applying the digit-based technique to historical vote totals. 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.

fraud may be shared between local politicians[3] (Centre for Humanitarian Dialogue, 2011). Since they appear to share characteristics of high election fraud areas and yet are not in the list of consistent election hotspots, it is ambiguous whether they should be considered part of the comparison group or the treatment group. Because of this, 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.

### 2.3.3 Data on Approved Building Permits

Lastly, data on approved building permits come from the Philippine Statistics Authority. Each year, field personnel in each town gather data from the Office of Local Building Officials. Building permit data are based on copies of original application forms of approved building permits as well as from demolition permits. Table A.3 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.

### 2.3.4 Empirical Strategy

The fact that the automated election system was deployed in all towns regardless of the pre-existing level of fraud generates plausibly exogenous variation in election fraud around the 2010 elections. In particular, the towns that previously experienced relatively more election fraud will experience a relatively greater reduction in election fraud. This allows me to 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

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

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. I employ digit-based tests to verify that fraud did in fact decrease more in the towns that were consistent election hotspots relative to the comparison group. Specifically, I formally test whether there was more election fraud, as measured by zeroes occurring more often as the last digit, in the consistent election hotspots than in the other towns during the manual election years. I then test whether both groups exhibit any evidence of election fraud during the automated election years. I do this by using goodness-of-fit tests to test the observed frequencies of last digits against predicted frequencies from a uniform distribution, and by using the Mann-Whitney U test to compare observed distributions of last digits against each other.[4]

Fixed effects ordinary least squares (OLS) panel data models are estimated to determine the impact of reducing election fraud on how many building permits get approved each year. The OLS model estimated is a generalized difference-in-differences specification:

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

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 specification where 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

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

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. Robust standard errors are clustered at the town level.

Since 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. I also formally test this by including an indicator for the year before automated elections were adopted, and by plotting estimated differences over time. 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 until that same year, if hotspots and non-hotspots had started diverging even before the 2010 elections, then the improvement in government performance might be due to something other than the reduction in election fraud. Lastly, I include town-specific linear time trends, which allows each town to follow a different trend over time. To the extent that the estimates are robust to these specifications, these tests provide some evidence to support the validity of this research design.

**2.4 Results**

**2.4.1 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 do the same for the other towns. This allows me to compare how much election fraud levels in hotspots changed after the adoption of automated elections relative to the change experienced by non-hotspots during the same time period. Figure A.3 summarizes the 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-squared 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 ten percent of the time. However, the election fraud experienced by hotspot towns seems to have been much worse. Zeroes occur as the last digit almost 20 percent of the time in the hotspot towns, but only about 12 percent of the time in other towns. Using a Mann-Whitney 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 the non-hotspot towns during the manual election period. That is, although the last digits from non-hotspot towns are also not uniformly distributed in the manual election period, indicating the presence of some election fraud, the observed frequency of zeros is still much smaller than what is observed in the hotspot towns.

Election fraud drops significantly in the automated election period in both hotspots and non-hotspots. Figure A.3 shows that each digit is now equally likely to appear as the last digit of a vote total. Formally, chi-squared tests fail to reject the null hypothesis that the observed

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frequencies of last digits from either hotspots or non-hotspots follow the predicted frequencies from a uniform distribution during the automated election period. In addition, a Mann-Whitney 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. 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.[5]

**2.4.2 Impact on Government Performance**

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 A.4 I graph the log of the total number of building permits approved over time in hotspots compared to other towns. There are a few things worth noting in this graph. First, both sets of towns appear to track each other in terms of changes in total building permits approved in the years before 2010. This 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 building permits approved in 2010 in the hotspots relative to the other towns. It then stays at that higher level throughout the automated election period. That the effect manifests immediately during the first automated election year also gives further evidence that it is the reduction in

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[5] To provide 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.

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 takes 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, since election fraud was reduced throughout the automated election period, we should expect that government performance should continue at a higher level for as long as they are held accountable in elections.

In Figure A.5, I graph the estimated divergence between hotspots and non-hotspots over time. The figure graphs coefficients from a dynamic difference-in-differences model that controls for town and year fixed effects as well as region-by-year fixed effects. The estimated differences are relative to the average difference in building permits three or more years before automated elections. In line with Figure A.4, Figure A.5 shows that there is little evidence of divergence before the switch to automated elections, and that there is a sharp and sustained increase in building permits after the election reform occurs.

Estimation results are shown in Table A.4. 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, I add a time-varying control for town-level population and this does not change the estimate. In Column 3, 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 similar towns are assigned to administrative regions in the Philippines, it is likely that

this is the more appropriate comparison to make. The estimate in this preferred specification is 14.9 percent, and is statistically significant at the five percent level. Column 4 adds an indicator for the year 2009, which formally tests whether the hotspots and other towns began to diverge even before the switch to automated elections. Since the estimated coefficient on this leading indicator is not statistically significant, there is little evidence of divergence before automated elections. This is also in line with the parallel pre-trends shown in Figure A.4. Finally, column 5 adds town-specific linear time trends. Town-specific linear time trends allow for each town to follow a different trend. The estimate in column 4 is a 16.7 percent increase in approved building permits. For each of these specifications, robust standard errors are clustered at the town level.

Importantly, all of the estimates are significant at the 5 percent level. Taken together, these estimates 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.

### 2.4.3 Potential Mechanisms

I now examine potential mechanisms through which reducing election fraud may lead to a significant improvement in government performance. Government performance might be improving because different, higher-performing people are getting elected to office, or even just 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 the time when they run. Incumbent candidates typically win by a margin of 30 percentage points over their challengers, reflecting the advantage that

incumbents 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 A.5, 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 during 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 changed. To do so, I examine whether incumbent candidates get a smaller share of the total vote in the automated election period. Table A.6 presents evidence in favor of this mechanism. Across specifications, the estimated effect is approximately a 10 percentage point reduction in the incumbent victory margin. That is, the difference between the vote share obtained by winning incumbents and the vote share obtained by their closest challenger narrowed by 10 percentage points. This represents the erosion of about a third of the average victory margin formerly experienced by winning incumbent. Since a decrease in the incumbent candidates' victory margins would imply an increase in the vote share received by challengers, this would mean that elections between challengers and incumbents become 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 appear to decrease the number of incumbents that win re-election.

## 2.5 Discussion and Conclusion

The fundamental question addressed in this section 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.

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. Secondary descriptive data from World Bank surveys 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 are also consistent with this explanation, as the average waiting time actually decreased by 26 days. 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 capturing investment in the local economy.

3. DOES ELECTORAL PRESSURE LEAD TO BETTER GOVERNMENT PERFORMANCE?

**3.1 Introduction**

Poor and unresponsive governments are often characterized by corruption and a lack of effort in performing assigned responsibilities (Grindle, 2004; Olken and Pande, 2012). Elections are meant to mitigate this problem by providing voters with an accountability mechanism to incentivize responsive performance. The underlying idea is that in response to this electoral pressure, incumbent politicians will exert effort in the performance of their duties to be rewarded at the next election. The focus of this section is to determine whether electoral pressure affects government performance.

Economic theory predicts that pressure from upcoming elections might lead incumbent politicians to strategically perform better in times of increased electoral pressure (Rogoff, 1990; Bechtel and Hainmueller, 2011). Empirical evidence of such strategic responses has been documented in the form of political business cycles in government spending (Akhmedov and Zhuravskaya, 2004; Besley and Burgess, 2002; Besley and Case, 1995; Khemani, 2004; Drazen and Eslava, 2010; De Haan and Klomp, 2016; Repetto, 2017), corrupt behavior (Ferraz and Finan, 2011), and police expenditure and hiring (Levitt, 1997; Guillamon, Bastida, and Benito, 2011). This section complements this literature by examining an outcome that more directly measures the effort government puts into their responsibilities, which is arguably more in line with existing theory. Specifically, I ask whether local governments in the Philippines evacuate more of their affected population in preparation for tropical cyclones, an activity that requires significant political capital and effort due to the need to identify, warn, persuade, and transport people who would otherwise be incapable or unwilling to evacuate. In addition, in contrast to the

spending measures used in the existing literature, the outcome used here likely has large and direct consequences for public safety and mortality.

To this end, I use Philippine data to examine whether increased electoral pressure affects government performance, as measured by the number of people they evacuate each year in preparation for tropical cyclones. The Philippines is an archipelago in Southeast Asia that experiences tropical cyclones several times each year. To prepare for these storms, governors lead evacuation efforts to move their affected population to evacuation shelters. They coordinate and request resources from the national government to accomplish this. Evacuation of the affected population serves two goals: 1) protect people from the damaging and potentially fatal effects of tropical cyclones; and 2) provide temporary food and shelter to those that might otherwise be homeless due to the destruction of homes that is commonplace with these storms. To identify the effect of electoral pressure on evacuations, I exploit institutional variation in electoral pressure in the Philippines generated by term limits and the remaining time until the next election. Because third-term governors are barred from seeking re-election, upcoming elections matter less to them than to governors that are still eligible to run. Thus, the eligible governors are under greater electoral pressure than the ineligible governors. I also exploit variation over time and compare evacuation performance in the final year of governors' terms. The idea is that in the final year of their terms, eligible governors are under increased electoral pressure due to the salience of the upcoming election while ineligible governors face only the same pressures as earlier in their terms. The identifying assumption underlying this approach is that absent the increase in electoral pressure from the upcoming election, evacuations in provinces whose governors were eligible to run for re-election would have trended similarly compared to provinces whose governors were ineligible to run. I show graphical evidence in

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favor of this assumption, as both groups of provinces appear to have evacuated at similar rates during the first two years of governors' terms.

Results from fixed effects panel data models indicate that increased electoral pressure leads to a statistically significant 20 percentage point increase in the fraction of the affected population that is evacuated. This finding is robust to various specifications, including adding controls for tropical cyclone distance and provincial population. As a falsification check, I also show that this effect manifests only in the final year of the three-year term and not earlier in the term. Finally, I show that this effect grows as elections get closer, which is to be expected if electoral pressure is driving the increase in evacuations.

These findings are important for several reasons. By focusing on an outcome that more directly measures effort, I show that electoral pressure can affect government behavior beyond its spending choices. Importantly, the effort that governments put into the task under study can have positive effects on public safety in the wake of natural disasters. This has an immediate and direct effect on citizen well-being, since being served in evacuation shelters shields them from life-threatening situations and gives them access to food and medicine during a vulnerable time. Overall, the findings suggest that even in contexts where government is generally ineffectual, electoral pressure does seem to generate additional effort by politicians.

The rest of this section is organized as follows. The next subsection discusses the tropical cyclone risk in the Philippines, and presents the institutional background of Philippine politics. Then, Subsection 3.3 discusses the data used in the study and the empirical strategy. Subsection 4 presents the results from various estimations. Subsection 3.5 concludes.

**3.2 Institutional Background**

**3.2.1 Tropical Cyclone Exposure and Evacuation Process in the Philippines**

The Philippines is an archipelago in Southeast Asia, an area that is most often affected by tropical cyclones. The country is regularly in the path of tropical cyclones that form in the Pacific Ocean and move toward mainland Asia or Japan. The Philippines is affected by around 20 tropical cyclones each year, of which nine tropical cyclones make landfall on average. The peak season for tropical cyclones is July to September, but tropical cyclones can affect the Philippines at any time. These tropical cyclones come in varying intensities and sizes, and affect different parts of the country (Takagi and Esteban, 2016). The tropical cyclone risk, coupled with other natural disaster risks, makes the Philippines the third-ranked country by exposure to natural hazards (United Nations, 2015).

With a population of 100 million, the Philippines has a substantial number of people living in areas that are prone to flooding and landslides. Preparing for tropical cyclones requires evacuating these people, both to prevent casualties and because flooding and landslides would otherwise leave these people homeless and without food and medicine due to the destruction of homes that is commonplace with these storms (Diacon, 1992). Under the 1991 Local Government Code, much of this responsibility has been devolved to local governments. As the highest ranking local executive officials, governors oversee evacuation efforts in their province. However, the national government still provides the infrastructure and services to support local evacuation efforts. The task of governors is to coordinate activities within their province and request assistance from the national government when necessary (World Bank, 2005). Figure A.6 illustrates how the evacuation process works in the Philippines. Once a tropical cyclone enters the Philippine Area of Responsibility, the Philippine Atmospheric Geophysical and

Astronomical Services Administration issues periodic forecasts. These forecasts are used to identify the affected population and notify their local government units, starting with the provinces and down the chain to the municipalities. These local government units warn the affected population and issue evacuation orders if necessary. The provincial governor coordinates these efforts and ensures that the necessary resources are available. The governor's task is made even more difficult when people are reluctant to obey evacuation orders for fear of losing valuables to looters (Manila, 2013).[6] When carrying out a mass evacuation, the provincial governor may have to request transportation assistance, such as trucks and boats from the police and/or the military, and send them to the places in which they are needed. In conjunction with these efforts, national government agencies such as the Department of Social Welfare and Development and the Department of Health preposition food packs and medical supplies in the evacuation shelters. Once served in evacuation shelters, people stay until it is safe to return to their homes.[7]

### 3.2.2 Elections and Term Limits in the Philippines

At present, the Philippines is divided into 18 administrative regions, containing a total of 81 provinces, 145 cities, and 1,489 municipalities (Philippine Statistics Authority, 2015). As there is no elective office for administrative regions, the highest ranking local government official is the provincial governor. Elections for governor are held with all other national and local elections every three years in May, with the winning candidates taking their office in July. Candidates

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[6] In December 2016, a tropical cyclone was forecast to affect a province in the Philippines around Christmas Day. Because of the upcoming festivities, affected people did not want to evacuate. To incentivize evacuation, their governor offered roast pork, a traditional festival meal, in the shelters (Tantiangco, 2017).

[7] Source: phone interview with NDRRMC office.

must declare their candidacy in the preceding October. This setup makes the end of their term the period during which elections are arguably most salient.

In the Philippines, governors, along with all other local officials, are limited to serving three consecutive three-year terms. After serving in one capacity for nine consecutive years, governors are termed out/ineligible and must step down or seek election to a different office. Although political dynasties have been prohibited under the 1987 Philippine Constitution, it is possible for termed out politicians to circumvent the three-term limit by running for a different office (Querubin, 2012). This makes it easier for a former three-term governor to run for governor again after sitting out one term. I account for this by excluding ineligible governors who run for other office. Finally, it should also be noted that local parties are not necessarily allied with ruling national parties. In the Philippines, local elections are dominated by families.

## 3.3 Data and Empirical Strategy

### 3.3.1 Data

I obtain data on evacuations, casualties, and damages due to tropical cyclones from the National Disaster Risk Reduction and Management Council. The data cover the period 2005-2014, and are reported at the province-tropical cyclone level. The data are culled from status updates and reports posted by the National Disaster Risk Reduction and Management Council. These reports include information for each province on the number of affected population, number of people evacuated, number of casualties and assessed damages attributable to each tropical cyclone that affected that province. From this data, the fraction of people evacuated in each province for each tropical cyclone is constructed by dividing the number of people evacuated by the number of people affected. The affected population is defined by identifying at-risk areas and summing the total population in those areas. The number of people evacuated is counted by evacuation camp

managers deployed by the Department of Social Welfare and Development. Since they are employees of the national government, these camp managers do not report to the governors. Fatalities are counted if there is sufficient evidence that they can be attributed to the tropical cyclone instead of other causes. This is a less straightforward process compared to counting the number of evacuees.

I construct a balanced panel by annualizing this tropical cyclone-level data for each province. To account for the degree of tropical cyclone exposure, I merge this dataset with actual tropical cyclone track data from the International Best Track Archive for Climate Stewardship. I use this to calculate the closest distance that each tropical cyclone's eye ever got to each province. I classify provinces as being directly hit by a tropical cyclone if the eye of the tropical cyclone passes within 47 kilometers of the province. The area defined by the 47 kilometer radius is typically where the strongest winds and heaviest precipitation falls.

To measure re-election incentives, I use election outcomes for all gubernatorial races during over the period 2001-2016 from the Commission on Elections. Given the three-consecutive terms limit, I am able to identify ineligible incumbents beginning with the 2007 election. Thus, the final data cover the time period 2007-2014. Table A.7 describes the sample of unique governors in the data and breaks it down by the number of consecutive terms that they serve. There is roughly an even distribution of governors when broken down by this metric. Of the 47 governors that are termed out after serving three consecutive terms, 21 seek re-election to a different provincial office in the next election. I exclude these 21 governors from the analysis, because governors that seek re-election to a different provincial office when they become ineligible to run for governor differ in important ways from ineligible governors that do not run again. Since governors can serve multiple terms, each governor can appear in the data more

than once, and so Table A.8 breaks down the province-level data by the term of their governor. Most of the provinces are headed by a first-term governor, and provinces are affected by about 2 tropical cyclones each year regardless of the election eligibility of their governor. On average, approximately 39,800 people are evacuated out of a total affected population of 116,400 for an evacuation rate of 34 percent. There are fewer than 20 fatalities attributable to tropical cyclones.

### 3.3.2 Empirical Strategy

I exploit variation in electoral pressure arising from the rule that prohibits incumbents who have already served three consecutive terms from seeking reelection to the same office. This term limit allows me to compare the difference between tropical cyclone preparations in provinces whose governors are eligible to run and provinces whose governors are no longer eligible to run, and who do not seek re-election to a different provincial office. Since these groups may still differ along aspects that would affect how thoroughly they evacuate, I use the remaining time to the next elections (or equivalently, the remaining time in a governor's term) as a second difference. The idea is that the time to the next elections should only matter to the governors who can still seek reelection, because voters can no longer hold ineligible governors accountable for their performance at the next elections.

I estimate fixed effects panel data models to determine the impact of electoral pressure on the number of evacuees per affected population. The OLS panel data model estimated can be thought of as a generalized difference-in-differences specification:

$$EVAC_{it} = \beta_1(eligible_{it} * yearbeforeelections_t) + \beta_2 eligible_{it} + \beta_3 yearbeforeelections_t + \beta_4 storms_{it} + c_i + u_t + \varepsilon_{it}$$

Where $EVAC_{it}$ is the number of people evacuated relative to the affected population in a province $i$ in year $t$, $eligible_{it}$ is an indicator for whether the governor of province $i$ is in the first

or second consecutive term during year $t$, *yearbeforeelections$_t$* is an indicator for whether it is 12 months from the end of a term, *storms$_{it}$* controls for the number of times a province $i$ is affected by a tropical cyclone in year $t$ and $c_i$ , $u_t$ are province fixed effects and year fixed effects, respectively. Robust standard errors are clustered at the province level. Other specifications also include controls for population as well as the number of direct hits a province got each year.

The identifying assumption underlying this approach is that absent the increase in electoral pressure due to the upcoming election, evacuations in provinces whose governors can seek re-election would have trended similarly with evacuations in provinces whose governors are ineligible to run. I examine the validity of this assumption in the following ways. I start by graphically examining whether the fraction of people evacuated at the start of a three-year term is similar across provinces regardless of the term their governors are in. If evacuations in provinces headed by governors in different terms already trend differently in the years prior to the last year of the term, then they might not be valid comparisons for each other. I also examine whether estimates are robust to including a control for province-level population, a determinant of how many people need to be evacuated. Under the identifying assumption, population should not change more over time in provinces headed by eligible governors. For example, if population increases more during the final year in provinces when they are headed by eligible governors, then evacuations in those provinces might have increased ahead of elections relative to provinces headed by ineligible governors even without the increase in electoral pressure. Finally, I examine whether the results are robust to including indicators for governor fixed effects, which account for governors that serve more than once or drop out during the time period under study. This

ensures that any effects are not driven by compositional differences in the behavior of governors who reach their third term and those who do not.

## 3.4 Results

### 3.4.1 Effect on Evacuations

I start by graphing the raw data of the fraction of the affected population evacuated in provinces, by term and breaking down three-year terms into first two years and last year of the term (increased electoral pressure). This is shown in Figure A.7. This graph highlights two important features of the data. First, it appears that people are evacuated at similar rates in the first two years of a term regardless of the type of governor. This is also confirmed using t-tests. Second, the graph shows suggestive evidence that increased electoral pressure affects evacuations. For eligible governors, the fraction of the affected population that ends up being evacuated increases during the last year of their terms, by about 6 percentage points for first-term governors and almost 25 percentage points for second-term governors.[8] Strikingly, there is no corresponding effect for the ineligible governors, as evacuations seem to have proceeded at the same rate during the start of the term compared to the end of the term. Taken together, this pattern of results suggests that the spikes in evacuations at the end of the first two terms are due to electoral pressure, and not a more general last-year-of-office effect.

Estimation results are shown in Table A.9. Each column presents estimates from a different specification. All specifications include indicators for whether governors are eligible to run and for whether it is the year before the next election. In column 1, where only province and

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[8] Although these are just raw averages, it is possible that the significantly higher increase in evacuations for second-term governors is because the governors that make it to a second term are different from first-term governors in a manner that matters for responding to electoral pressure. The increase in evacuations is statistically different from zero for both first and second term governors, indicating that the response does not come solely from second term governors. And if governors that get re-elected to a second term are better at evacuating people, governors that make it to a third term should be just as effective.

year fixed effects are added, the impact of increased electoral pressure is a 23.2 percentage point increase in the number of evacuees. This effect is large and statistically significant at the 5 percent level. It means that for every 100 people identified as affected, an additional 23 are evacuated when a province is headed by a governor who faces increased electoral pressure. In column 2, I add a control for provincial population, which accounts for the possibility that provinces that have a big and/or growing population simply evacuate more of their affected population. The estimate from this specification is a 22.5 percentage point increase. Column 3 includes province and year fixed effects, as well as a time-varying control for the number of direct hits that a province took from tropical cyclones each year. Again, the estimated effect is steady at about 22.7 percentage points. In Column 4, I estimate a specification that includes province and year fixed effects and adds governor fixed effects to account for changes in the composition of governors. This results in an estimated effect of 20.4 percentage points, which is similar in magnitude and statistically significant at the 10 percent level. In Column 5, I include all of the time varying controls discussed earlier and in Column 6, I add governor fixed effects to the set of controls. These specifications result in similar estimates.

**3.4.2 Robustness**

Table A.10 presents the results of robustness checks in which I vary how I define the period of increased electoral pressure. All specifications include indicators for whether governors are eligible to run, for whether it is the year before the next election, and for province and year fixed effects. In the main table, electoral pressure was defined to increase during the last 12 months of a three-year term. Since it is likely that electoral pressure increases as it gets closer to the next election, estimates should be very small when the treatment period is defined to start far away from the upcoming election, and grow as the treatment period is defined to start closer and

closer to the upcoming election.[9] Thus, defining the treatment period to start during the last 20 months of a three-year term is akin to a falsification check because we would not expect to estimate a treatment effect, while we would expect effects to be strongest when the treatment period is defined to cover the last few months of a term.

Column 7 in Table A.10 reproduces the estimated effect in Column 1 of Table A.9, where the cutoff for the treatment period is defined as the last 12 months of the term. The first columns represent the falsification checks, where I show that if the treatment period is defined to start further away from the end of the term, I do not estimate an effect. I start being able to estimate a marginally significant treatment effect in Column 3, when increased electoral pressure is defined to start in the last 18 months of the term. This corresponds to the time period beginning in January of the year before elections. Columns 1 and 2 show smaller point estimates that are not statistically significant when the time period is expanded even further back to capture the peak tropical cyclone season of the year prior (that is, two peak seasons before the next elections). Columns 8 to 10 report estimates when treatment period of increased electoral pressure is progressively narrowed from 11 months to 9 months. The estimates remain positive and statistically significant, and sometimes slightly larger in magnitude than the baseline specification in Column 7. In Column 9 for example, when treatment is defined to start in the last 10 months of the term, I estimate that increased electoral pressure leads to a 24.7 percentage point increase in evacuations.

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[9] Narrowing the time window further than 9 months is not feasible in this setting, as not many tropical cyclones hit between November and June.

**3.5 Discussion and Conclusion**

The research question examined in this section is whether increased electoral pressure by itself leads governors to exert greater effort in the performance of their responsibilities. The setting provided by the Philippines allows me to isolate the causal effect because of term limits that divide governors into a group that faces electoral pressure and a group that faces much less pressure. I exploit this variation and the salience from upcoming elections to identify effects. I measure government performance along an important aspect of government responsibilities in the Philippines, the number of evacuations in preparation for tropical cyclones. This measure has the advantage of isolating effort from spending, as it is the national government that provides resources that may be needed by provinces. Evacuating the affected population only requires effort from governors in identifying and coordinating the necessary resources. This is a potentially lifesaving preparedness measure, and one that disproportionately benefits the poor and the vulnerable.

Results indicate a substantial effect of electoral pressure on evacuations. I estimate that increased electoral pressure leads to a statistically significant 20 percentage point increase in the fraction of the affected population evacuated. I show that the estimate is robust to various specifications, including controlling for tropical cyclone distance, population, and governor fixed effects. I also show that this effect grows as elections get closer and decays as elections are farther away. This finding complements previous literature that finds evidence of political business cycles in spending, and shows that electoral pressure also results in increases along more effort-based measures of government performance.

# 4. DOES CONFLICT EXPOSURE INCREASE IN-GROUP BIAS? EVIDENCE FROM EXPERIMENTS IN THE PHILIPPINES

## 4.1 Introduction

This section examines the lasting effects of conflict exposure on in-group bias. Past studies document a link between conflict and subsequent economic development (Davis and Weinstein, 2002; Miguel and Roland, 2005; Bellows and Miguel, 2009; Blattman and Miguel, 2010; Bauer Blattman, Chytilova, Henrich, Miguel, and Mitts, 2016). Potential mechanisms through which this effect works include infrastructure, human capital accumulation and health, and lasting changes in individual preferences. In this section, we focus on the last mechanism. Existing studies on the lasting effects of civil conflict exposure on individual preferences have found seemingly contradictory results. On the one hand, studies usually find that conflict exposure in general leads to greater in-group favoritism (Whitt and Wilson, 2007; Rohner, Thoenig, and Zilibotti, 2013; Voors, Nillesen, Verwimp, Bulte, Lensink, and Van Soest, 2012). On the other hand, recent work on Tajikistan by Cassar, Grosjean, and Whitt (2013) find that conflict can lead to lower trust within localities. They hypothesize that the reason they find nuanced results was because the nature of the Tajik civil war made it difficult to identify friend from foe. Because of this, it was possible to be victimized by people that would normally be considered in-group. Because most work in this literature focus on single countries where there is typically only one form of conflict, they are unable to decompose the effect of different types of conflict. Our section contributes to the literature by providing evidence that the interaction between identity and the nature of the conflict people are exposed to affects the direction of the effect of conflict exposure on in-group bias

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To do this, we exploit a unique dataset based on the Philippine setting. Specifically, we use data gathered using lab-in-the-field experiments and merge it with conflict data from the Uppsala Conflict Data Program. Doing this allows us to tie community-level conflict exposure in their hometown to their individual preferences as measured from their choices in experimental tasks. We focus on their actions in the dictator games. Specifically, we measure in-group bias by taking the difference between the amount they send to a partner of the same religion and the amount they send to a partner of a different religion.

Using the Philippine setting allows us to study this research question because of the presence of two types of conflict: a religious conflict driven by Muslim separatists and an indiscriminate conflict driven by communist insurgents, as well as the presence of two dominant groups: Christians and Muslims. We exploit the fact that, given how small municipalities are in the Philippines, the location of conflict incidents is likely to be as good as random. Because the participants in the experiments are mostly migrants to the capital city of Manila, to the extent that their decision to migrate is not driven by conflict in their hometown, we also observe variation in conflict exposure generated by the timing of the conflict incidents relative to when participants leave their hometown. In exploiting these two sources of variation, we make the assumption that participants not exposed to conflict provide a good counterfactual to the participants exposed to conflict. We do our best to provide evidence in favor of this assumption. We show that towns that experienced at least one conflict incident share similar observable characteristics to towns that did not experience a conflict incident. We also test whether migrants from conflict towns are similar to migrants from non-conflict towns. While we have more males and Muslims from conflict towns, we show that accounting for this difference as well as a large set of controls does not significantly change our estimates.

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Our main result is that the effect of conflict exposure on religious in-group bias does depend on the type of conflict exposure and the identity of the participant. Specifically, we find that being exposed to the religious conflict leads Muslims to become biased against fellow Muslims, while being exposed to the indiscriminate conflict leads to Muslims to favor fellow Muslims more. We are also able to determine that this effect is driven by changes in the amount sent to in-group partners, rather than a change in how participants treat their out-group. These effects are robust to a wide range of controls. We control for individual characteristics, conflict characteristics, and hometown characteristics and show that our estimates remain stable across various specifications.

These results contribute to the literature in two ways. First, we present evidence that different types of conflict can lead to different effects on individual preferences. This matters because it provides another explanation for why some countries recover from conflict at different rates than others. Second, we reconcile previous findings in the literature that document opposite effects on in-group bias. Using the Philippine setting, we are able to show that being victimized by people normally considered in-group affects preferences differently than when the conflict is indiscriminate.

The rest of this section is organized as follows. The next subsection lays out the relevant details of the Philippine setting, and discusses the history of conflict in the Philippines. Subsection 4.3 describes the data we use in the study as well as our empirical strategy. In Subsection 4.4, we present evidence to support the validity of our empirical strategy before showing our results. Subsection 4.5 concludes.

## 4.2 Institutional Background

The Philippines is a majority Christian nation with a substantial Muslim minority. Roman Catholicism is the dominant religion, but there is a large concentration of Muslims in the southern regions of the country. For the most part, Muslims in the northern Philippines are migrants from the south. There are also several ethnic groups in the Philippines, but because of the geographic nature of the split between Christians and Muslims, ethnic identities tend to be nested within religious identity. There are eight major ethnic identities represented in this study: Tagalog, Cebuano, Bisaya, Ilocano, Tausug, Yakan, Maguindanao, and Maranao. The first four ethnic groups are mostly Christian, while the final four ethnic groups are mostly Muslim. In addition, the Tausugs and Yakans are the predominant ethnic groups in the areas where the Muslim separatists are based. There are two main long-running conflicts in the Philippines: 1) a communist insurgency, and 2) a Muslim separatist movement. The sources of these conflicts are different. The root cause of the communist insurgency is deep socioeconomic inequality. Importantly, the armed communist group in the Philippines does not discriminate based on religion or ethnicity. In fact, its members shed these identities when they join the communist movement. On the other hand, the conflict involving Muslim separatists is based on religious/ethnic identity (Schiavo-Campo and Judd, 2005). Because of where the Muslim separatists base their operations out of, two ethnicities in particular are associated with the Muslim separatists: the Tausug and the Yakan. These conflicts date back to the late 1960s, and incidents tend to be geographically dispersed throughout the country. Figure A. 8 shows a map of the Philippines with the towns that reportedly experienced conflict highlighted in red. This map shows that these towns are geographically distributed all over the Philippines. This implies that it is unlikely for a few, potentially different areas to drive our results. Figure A. 9 shows how

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the number of reported conflict incidents evolved over time. Across the entire Philippines, conflict incidents were relatively fewer in the late 1970s to early 1980s. Since then however, conflict incident reports have been much higher with the exception of the late 1990s. We discuss how conflict incidents are recorded in Section 3.1.2

## 4.3 Data and Empirical Strategy

### 4.3.1 Data on Social Preferences

We use field experiments to measure social preferences. The experiments were conducted from September-October 2009, in three sites in Metro Manila that have established Muslim settlements: Maharlika Village in Taguig City, Barangay Culiat in Quezon City, and Greenhills in San Juan City. The first two locations are low-income communities while the third is a commercial area. The choice of sites ensures that there is a mix of religious and ethnic identities represented in the experiments. In total, 328 individuals participated in the experiments, of which 199 migrated to Metro Manila from elsewhere. In each site, we held pure Muslim sessions, mixed Muslim and Christian sessions, and pure Christian sessions. The experiments were spread across 17 sessions. Table A.11 breaks down our sample by type of session.

Participants in the experiment did five tasks: risk preference game, time preference measure, dictator game, public good game and trust game. They were paid based on their decisions in one randomly chosen task. We use the dictator game to measure altruism toward in-group and out-group members. We conducted four dictator games, and in each game participants were asked to divide 500 Philippine pesos (approximately 10 dollars) between themselves and another participant who is of the: 1) same religion; 2) different religion; 3) same ethnicity; and 4) different ethnicity. The first two games are used to measure in-group/out-group preferences based on religious identity while the last two games are used to measure preferences based on

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ethnic identity. Figure A. 10 shows a translated decision page for a participant whose is Muslim (religion) and Maranao (ethnicity). Senders are partnered with recipients of unknown ethnicity, but who are of the same religion in dictator game 1, and the different religion in dictator game 2 Because ethnicities are nested within religions, we only vary the ethnicity of the receiver in dictator games 3 and 4. We use the difference between the amounts sent in dictator games 1 and 2 to measure religious in-group bias, and we use the difference between the amounts sent in dictator games 3 and 4 to measure ethnic in-group bias. The participants were informed that their partners also took part in the experiment, but are possibly not in the same session.

**4.3.2 Conflict Data**

Data on conflict come from two sources. The main source of data is the Uppsala Conflict Data Program. (Allansson, Melander, Themnér, 2017). However, because the UCDP data is only available from 1989 onward, we supplement it using data compiled by Abinales and Ramos.[10] Both datasets mainly rely on local newspapers, television, and radio sources to report conflict incidents. An incident in the data reflects an armed encounter between two armed groups, or between an armed group and civilians. Both datasets record the date and location of each incident. This allows us to tag participants who may have been exposed to conflict when they were still in their hometown.[11]   In addition, the UCDP data contain information on casualties as well as the groups involved in each incident. While we are unable to perfectly identify whether each of our participants was directly affected by the conflict incidents, the combined conflict dataset does allow us to construct a community-level measure of exposure to conflict in their

[10] Patricio Abinales is a professor at the University of Hawaii at Manoa. Data was requested via email.
[11] If there is underreporting of conflict incidents, it will serve to bias our results downward since there will potentially be participants who were exposed to conflict that we do not account for.

hometown. Using this data, we tag 120 of our participants as having been exposed to conflict in their hometown before migrating to Metro Manila. In Table A.12, we summarize the characteristics of the conflicts to which our subjects were exposed. Over the course of their stay in hometowns that experienced conflict incidents, subjects were exposed to 4 Muslim-rooted conflict incidents. More than 1 in 2 subjects were exposed to a communist conflict incident. These conflict incidents were deadly, and casualties include civilians in addition to the armed groups participating in the conflict.

### 4.3.3 Empirical Strategy

Our empirical strategy relies on the quasi-random nature of the timing and location of conflict incidents. We examine the difference in preferences between individuals whose hometowns had at least one conflict incident before they migrated to Metro Manila, and individuals whose hometowns did not experience a conflict incident. In doing this, the underlying assumption is that town-level exposure to conflict was orthogonal to individual preferences. Given the nature of our data, there are two potential sources of bias: 1) selective migration to Metro Manila, and 2) communities were targeted by the groups involved in the conflicts. We deal with these by focusing on participants who migrated to Metro Manila, and by accounting for hometown-level characteristics that may be associated with conflict incidence. We also show that our estimates are robust to accounting for a wide range of controls for individual, conflict, and hometown characteristics.

Our results are based on estimating cross-section models of the following form,

$$outcome_i = \alpha + \beta_1 conflict_i + \beta_2 muslim_i + \beta_3(conflict_i * muslim_i) + \beta_4(muslimconflict_i * muslim_i)$$
$$+ \beta_5(communistconflict_i * muslim_i) + \beta_6 muslimconflict_i + \beta_7 communistconflict_i + \varepsilon_i$$

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where outcome$_i$ can take one of three forms: 1) religious in-group bias, as measured by the difference in the amounts sent in dictator games 1 and 2, 2) amount sent in dictator game 1, and 3) amount sent in dictator game 2. Separately estimating the effect of conflict on the dictator games allows us to determine whether the effect on religious in-group bias is driven by senders being biased in favor of or against members of the same religion. We also include indicators for whether subjects are Muslim and whether they were exposed to conflict. We also break down conflict exposure by type of conflict. In doing so, we are mainly interested in estimating $\beta_4$ and $\beta_5$, which decomposes the effect of conflict by the interaction between identity and the type of conflict the subject was exposed to. All specifications include controls for session characteristics, and standard errors are heteroscedasticity-robust. In other specifications, we also include controls for hometown-level characteristics, individual-level characteristics, and conflict characteristics. That our estimates do not significantly change when we include each of these sets of controls in turn alleviates concerns of omitted variable bias.

### 4.4 Results

### 4.4.1 Selection and Exogeneity

We assess the plausibility of our assumptions by examining Figure A. 11 shows that individuals who come from towns that experienced conflict do not appear to have all migrated to Metro Manila right after the initial conflict incident. Rather, some people migrate years after the first incident, and even years after the last conflict incident. It is possible that they migrate for reasons other than conflict exposure.

Table A.13 shows the estimated differences in observable characteristics between towns that experienced conflict compared to towns that did not. We only have data on population, land

area, poverty incidence, as well as road density in cities. Table A.13 shows that hometowns where conflict incidents occurred appear similar to hometowns where no conflict incidents occurred. Ex ante, we would expect this since towns in the Philippines cover only a small land area, so where exactly conflict occurs is likely to be as good as random.

Similarly, Table A.14 shows the estimated differences in observable characteristics between individuals who migrated to Manila, separately by whether they come from towns that experienced conflict compared to towns that did not. We do observe significantly more males in the exposed to conflict group. Because most of our sample consists of migrants to Metro Manila, this raises the concern that males are more likely to be driven to migrate as a result of conflict in the community. Nevertheless, we control for these observable town-level and individual-level characteristics and show that accounting for potential differences does not change our results.

**4.4.2 Effect of Conflict Exposure on In-group Bias**

It is helpful to first examine the pattern of giving in the dictator game. One focal point we observe in our data is that dictators split the pie equally between the two players. Figure A. 12 shows the fraction of dictators choosing the equal split, by the religion of the recipient and then by conflict exposure. We observe that the fraction of dictators choosing the equal split appears to be bigger whenever partners are of the same religion and dictators were not exposed to conflict. However, dictators who are exposed to conflict appear to be less likely to choose the equal split when partnered with a member of the same religion.

Estimation results are shown in Tables A. 15 and A. 16. Table A.15 presents the effect of conflict exposure on religious in-group bias, as measured by the difference between the amounts sent in dictator games 1 and 2. In Columns 1 and 2, we only examine the effect of any conflict exposure. This is typically what previous work in this area are able to estimate, because they

only observe one type of conflict in their setting. When we do this, we find that conflict exposure results in a decrease in religious in-group bias. Column 2 shows that this result is robust to including controls for individual characteristics. In Columns 3 to 6, we separate the effect of the two types of conflict and allow for their effects to vary with the religious identity of the sender. We find that the type of conflict affects the direction of the effect on in-group bias. Column 3 shows that Muslims exposed to the Muslim separatist conflict reduce in-group bias, as measured by a decrease of 110 pesos in the difference between amounts sent in dictator games 1 and 2. On the other hand, Muslims exposed to the communist conflict increase their religious in-group bias, as measured by an increase of 72 pesos in the difference between amounts sent in dictator games 1 and 2. In Columns 4 to 6, respectively, we show that the direction of these effects are robust to the inclusion of controls for individual characteristics (age, education, marital status), conflict characteristics (civilian and armed group casualties), and hometown characteristics (population density, land area and poverty incidence). While these estimates are not large, it may be because we construct our dependent variable as the difference between two decisions that subjects make. If conflict exposure affects the amount sent in both dictator games 1 and 2 in the same direction, then the estimates presented in Table A.15 are possibly attenuated. This is possible if conflict exposure makes subjects less inclined to split the money, but more so depending on the interaction between the nature of the conflict and the identity of the partner.

We investigate this possibility in Table A.16. We estimate the effect of conflict exposure separately on the amounts sent in dictator game 1 (Panel A) and dictator game 2 (Panel B). We use the same specifications, and we find a similar pattern of results across specifications. In Columns 1 and 2, when we only measure the effect of general conflict exposure, we find that it decreases religious in-group bias. In Columns 3 to 6, when we account for the two types of

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conflict and interact them with religious identity, we find that Muslims exposed to religious conflict decrease the amount they send regardless of the partner. However, comparing the same columns between Panel A and Panel B, the estimated reduction is greater when their partner is also Muslim. Panel A shows that even when accounting for individual, hometown, and conflict characteristics at the same time, we estimate that Muslims exposed to the religious conflict decrease the amount sent to a Muslim partner by at least 275 pesos. Notably, our estimates are all statistically significant at the 1 percent level. By comparison, Panel B shows that the effect of the same type of conflict when the partner is not of the same religion is in the same direction, but much smaller in magnitude. Our largest estimate is a reduction of 185 pesos. Taken together, the results in Panel A and B suggest that the effect of religious conflict on religious in-group bias presented in Table A.15 is driven more by a reduction in the degree to which Muslims are biased in favor of fellow Muslims, instead of a decrease in their bias against Christians.

On the other hand, Columns 3 to 6 in Panel A of Table A.16 show that Muslims exposed to the indiscriminate communist conflict favor fellow Muslims more. Our most conservative estimate is that Muslims exposed to the indiscriminate communist conflict increase the amount they send to a Muslim partner by 157 pesos. Again, these estimates are all statistically significant at the 1 percent level. Estimates from the same specifications presented in Panel B show again that the effect on the amount sent to a Christian partner is comparatively weaker, and none of our estimates are statistically different from zero. This pattern of results suggests that the effect of indiscriminate conflict presented in Table A.15 is driven by an increase in the degree to which Muslims favor fellow Muslims.

**4.5 Discussion and Conclusion**

The question we investigate in this section is whether the effect of conflict exposure is mediated by the interaction between the type of conflict and identity. The Philippines presents a unique opportunity to test this hypothesis because of the presence of two ongoing types of conflict: a religious conflict and an indiscriminate conflict, as well as the existence of two big groups: Christians and Muslims. The combination of these allows us to interact the two types of conflict with the two dominant identities. Our findings indicate that these interactions yield opposing effects. Being exposed to the indiscriminate conflict leads Muslims to favor fellow Muslims, which is the modal result in previous studies. However, being exposed to the religious conflict leads Muslims to become more biased against other Muslims. Because the main difference is the type of conflict exposure, we are able to conclude that it is being targeted by one's in-group that leads to the puzzling effect on in-group bias. In doing so, we reconcile seemingly contradictory findings in the literature, where some studies find that conflict exposure leads to bias against the in-group while most other studies find that conflict exposure leads to greater in-group favoritism. While we are limited in that our participants appear to have migrated from conflict/non-conflict areas at different rates based on gender, to our findings are quantitatively unchanged when we allow for these differences.

# 5. SUMMARY AND CONCLUSIONS

The three essays presented in this dissertation present evidence on the relationship between public institutions and economic behavior. Section 1 shows that automating elections can reduce election fraud regardless of the pre-existing level of election fraud. More importantly, I show that this also resulted in improved government performance, as implied by an increase in building permits over time in the previously high-fraud towns relative to the low-fraud towns. In Section 2, I show that politicians evacuate a higher fraction of their affected population to prepare for tropical cyclones when they face pressure from upcoming elections. Since the resources needed to evacuate their population are provided by the national government, my results can be interpreted evidence of increased effort by politicians in response to electoral pressure. Finally, Section 3 presents evidence that conflict exposure affects religious in-group bias. That is, Muslims exposed to inter-religious conflict become biased against other Muslims, while Muslims exposed to indiscriminate conflict become biased against Christians.

Because these essays focus on the Philippine setting, the findings are particularly relevant to the developing country experience. Weak electoral institutions and civil conflicts are problems that plague many developing countries. The first two sections together show how strengthening elections can incentivize politicians to improve their performance at economically important aspects of their jobs. The last section implies that recovery efforts following conflict may benefit from the taking into account both the type of conflict exposure as well as the identities of the groups involved.

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FIGURES AND TABLES

## A.1 Election Fraud

Figure A. 1. Kernel Density of the Amount of Time it Takes to Get a Building Permit, by Whether or not a Firm is Asked for a Bribe
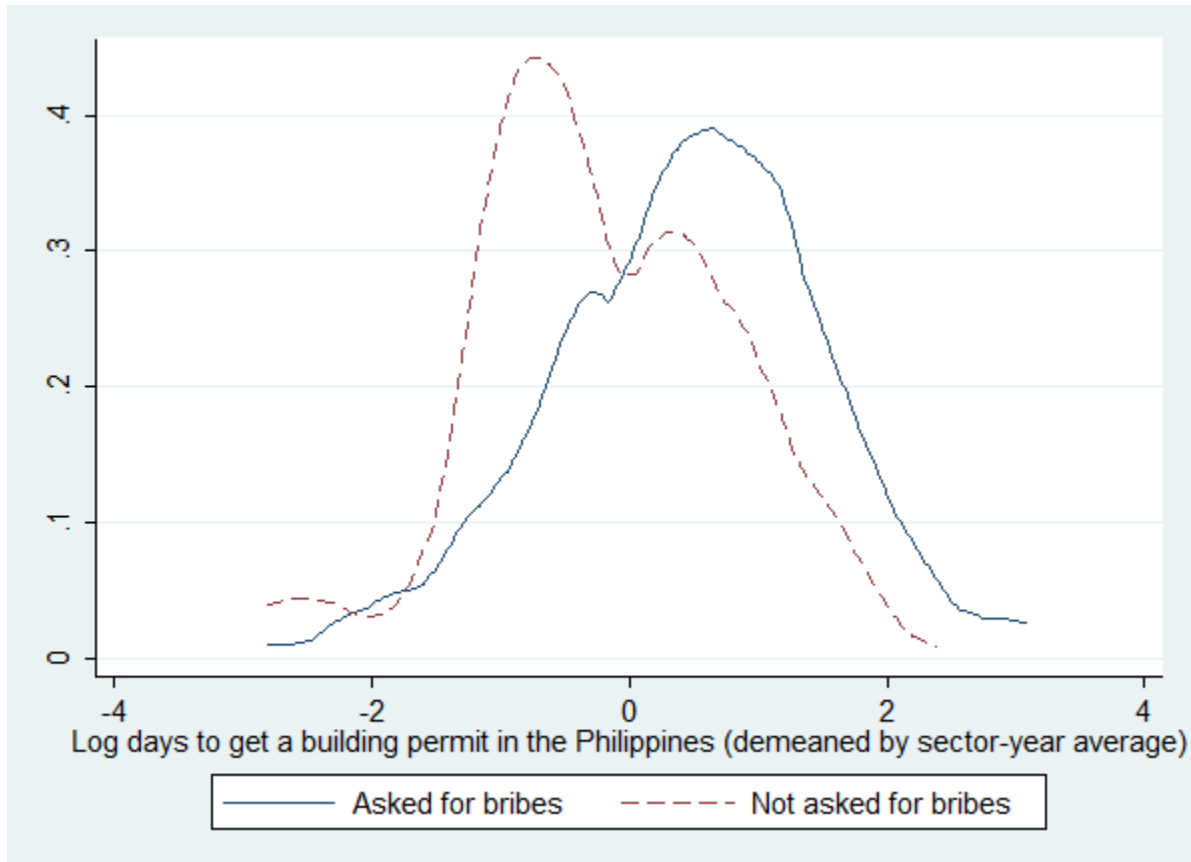


Notes: These graphs show the difference in the time it takes to obtain approval for a building permit depending on whether or not a firm is asked for bribes. The unit of observation is an individual firm. Waiting time is measured in log days, and are demeaned by the sector-year average waiting times.
Source of data: World Bank Enterprise Survey of the Philippines in 2009 and 2015.

Figure A. 2. Map of the Philippines, with Consistent Election Hotspots Highlighted



Notes: 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 A. 3. Distribution of Last Digits of Vote Totals From Mayoral Races, Manual Election
Period vs Automated Election Period

    A.   Consistent Election Hotspot Towns.
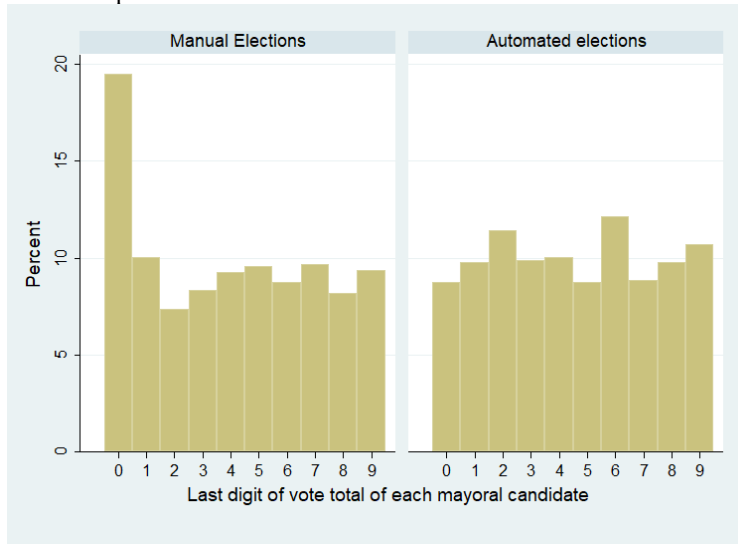


    B.   Other towns
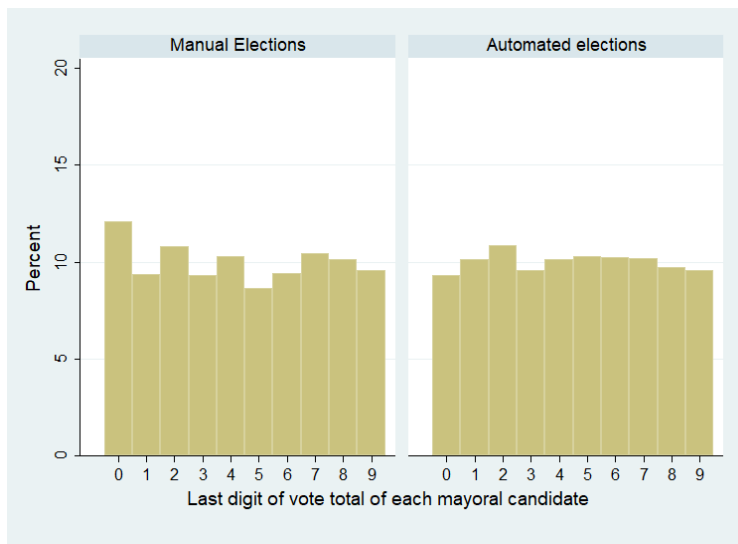


Notes: Each discrete distribution is tested against the predicted last digit frequencies from a uniform distribution. The manual election histograms show last digits of vote totals for mayoral elections in 2001, 2004, and 2007. The automated election histograms show data from the 2010 and 2013 mayoral elections. 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. In the automated election period, both groups of towns last digit distributions that are indistinguishable from a uniform distribution.

Figure A. 4. Log of Total Building Permits Approved, Years 2006 – 2015, by Hotspot Incidence
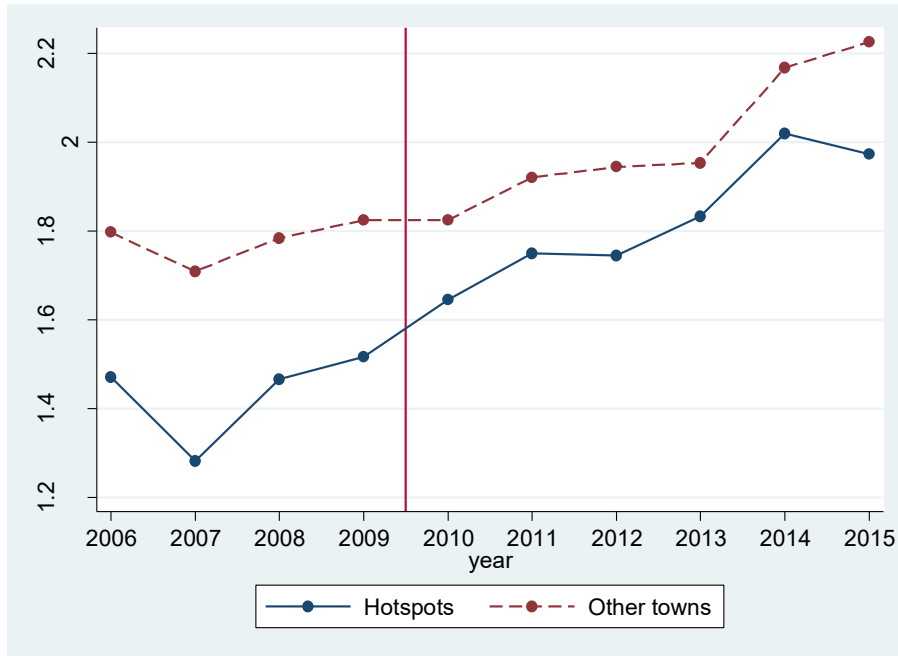


Notes: The graph shows the time series plot of the log of total building permits approved. The blue unbroken line is for hotspot towns while the red dashed line is for the other towns. The vertical line between 2009 and 2010 represents the shift in election systems. The 2010 elections were held in May, with winning candidates taking office in June 2010.

Figure A. 5. Estimated Divergence in Building Permits Before and After the Shift to Automated Elections Between Hotspots and Other Towns, Relative to Three or More Years Before
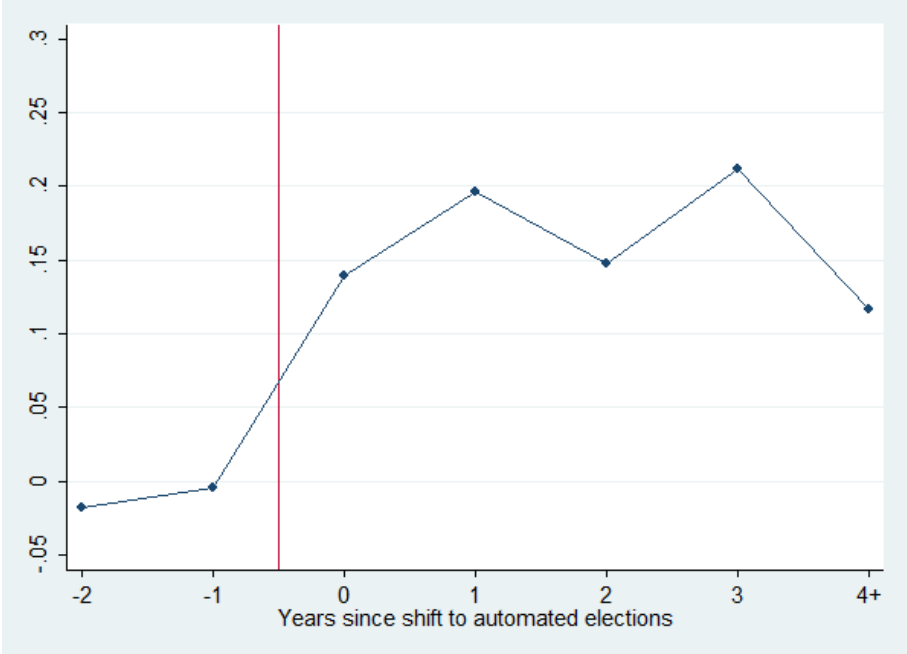


Notes: The graph shows the estimated difference over the period 2008-2015 between building permits approved in hotspots and non-hotspots, relative to the difference in the first two years of the data. The estimates come from a regression that allows for dynamic effects, and includes indicators for town and year fixed effects as well as region-by-year fixed effects.

Table A. 1. 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 |

Notes: The unit of observation is a city (twenty cities were sampled by the World Bank). 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).
Source: World Bank Doing Business Reports, 2008 and 2011.

**Table A. 2.  The Relationship Between Being Asked for a Bribe and the Waiting Time for Approval of Building Permit in the Philippines**

Dependent variable: Log (1+ days to get building permit)

| 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 | x |
| Control for worker productivity |  |  |  | x | x |
| Controls for firm visibility and interaction with government officials |  |  |  |  | x |

Notes:  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 A. 3. Descriptive Statistics for Municipalities, by Historical Election Fraud Incidence

| | All municipalities | 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) |
| Observations | 7070 | 1760 | 5310 |

Notes: Each cell contains the mean with the standard deviation in parentheses. The unit of observation is a municipality in a given year. The time period is 2006-2015.

Table A. 4. The Effect of Reducing Election Fraud on the Total Number of Building Permits That Get Approved

| Dependent variable: Log(number of building permits) | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Hotspot*automated election | 0.166** | 0.166** | 0.149** | 0.145** | 0.167** |
| | (0.0706) | (0.0706) | (0.0665) | (0.0737) | (0.082) |
| Hotspot*year before automated election | | | | -0.0163 | |
| | | | | (0.0657) | |
| Observations | 7070 | 7070 | 7070 | 7070 | 7070 |
| Town and year fixed effects | x | x | x | x | x |
| Control for population | | x | x | x | x |
| Region-by-year fixed effects | | | x | x | x |
| Town-specific time trends | | | | | x |

Notes: 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

Table A. 5. The Effect of Reducing Election Fraud on the Probability That Incumbent Candidates are Re-elected

| Dependent variable: Incumbent win rate | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Hotspot*automated election | 0.00457 | -0.0171 | -0.0581 | 0.00501 |
| | (0.0602) | (0.0622) | (0.0640) | (0.0701) |
| Year before automated 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 |
| Province-specific time trends | | | | x |

Notes: Each column represents a separate regression. The unit of observation is town-year. 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 A. 6. The Effect of Reducing Election Fraud on Incumbent Candidates' Victory Margins

| Dependent variable: Incumbent victory margin | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Hotspot*automated election | -0.102* | -0.0967* | -0.118** | -0.125 |
| | (0.0513) | (0.0553) | (0.0541) | (0.437) |
| | | | | |
| Election year before automated 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 |

Notes: Each column represents a separate regression. The unit of observation is candidate-year. 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

## A.2 Tropical Cyclone Evacuations

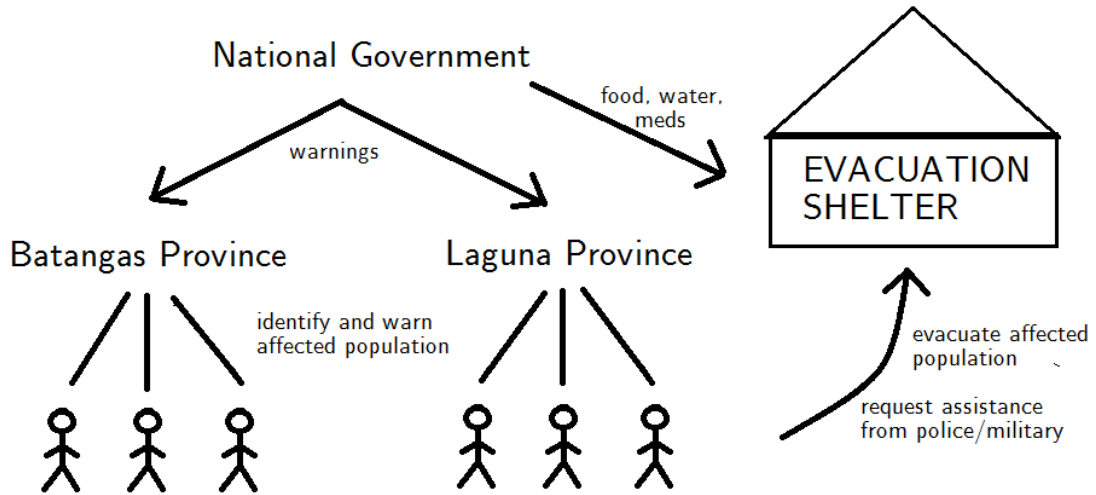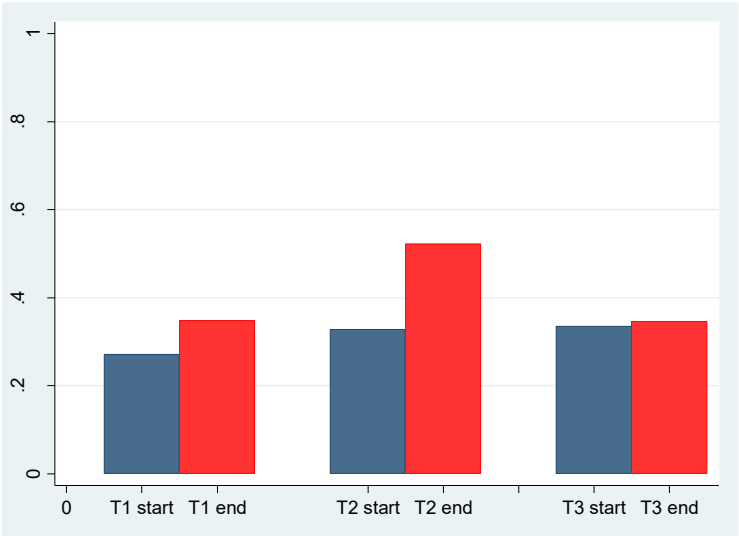Figure A. 6. How the Evacuation Process Works in the Philippines

Figure A. 7. Fraction of Affected Population Evacuated, by Term of Governor and Time to the Next Election



Notes: This figure shows the variation in the fraction of the affected population evacuated annually, by term and by proximity to the next election. Evacuations increase in the final year of both first and second term governors, while no change is observed for the third term governors.

Table A. 7. Breakdown of Governors

| | | |
|---|---|---|
| Total number of governors | 146 | |
| Number of consecutive terms achieved: | | |
| 1 | 56 | |
| 2 | 43 | |
| 3 | 47 | |
| Ran for other provincial office: | 21 | |
| Vice-Governor | | 2 |
| Congress | | 17 |
| Provincial Board | | 2 |

Notes: The above table shows how many unique governors are in the data, broken down by the highest number of consecutive terms they achieved. The table also shows how many of the termed-out incumbents seek election to a different provincial office.

Table A. 8. Summary Statistics, by Term of Governor

**Panel A. Breakdown of province-years by term of governor**

| Governor is in: | Frequency | Percent |
|---|---|---|
| 1st term | 332 | 51.88 |
| 2nd term | 185 | 28.91 |
| 3rd term | 123 | 19.22 |

**Panel B. Summary statistics**

| Annual Averages: | Number of storms | Total Evacuated | Total Dead | Total Affected |
|---|---|---|---|---|
| All terms | 2.19 | 39810 | 17 | 120309 |
| | (1.40) | (119090) | (214) | (297947) |
| Gov. is in 1st term | 2.15 | 31670 | 20 | 109838 |
| | (1.43) | (105851) | (288) | (319506) |
| Gov. is in 2nd term | 2.30 | 52415 | 16 | 140789.5 |
| | (1.32) | (137539) | (76) | (294947) |
| Gov. is in 3rd term | 2.12 | 42823 | 9 | 97226 |
| | (1.41) | (122162) | (62) | (206233) |

Notes: Above is a summary of the combined typhoons-elections data, where the detailed data on typhoons is annualized to form a balanced panel of provinces across time. Standard deviation in parentheses.

Table A. 9. The Effect of Increased Electoral Pressure on the Number of Evacuees

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Eligible to run for re-election*approaching end of term | 0.232** | 0.225** | 0.227** | 0.204* | 0.219* | 0.201 |
|  | (0.109) | (0.109) | (0.112) | (0.119) | (0.112) | (0.120) |
| Observations | 472 | 472 | 472 | 472 | 472 | 472 |
| Province and year FE | x | x | x | x | x | x |
| Control for HH population |  | x |  |  | x | x |
| Control for distance of storms |  |  | x |  | x | x |
| Governor FE |  |  |  | x |  | x |

Notes: Each column represents a separate regression. The unit of observation is province-year. The time period spans the years 2007-2014. Robust standard errors are clustered at the province level.
* Significant at the 10% level
** Significant at the 5% level
*** Significant at the 1% level

Table A. 10. The Effect of Increased Electoral Pressure on Number of Evacuees, Varying the Cutoff

| Dependent variable: evacuation rate | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Months before end of term: | 20 months | 19 months | 18 months | 16 months | 14 months | 13 months | 12 months | 11 months | 10 months | 9 months |
| Eligible to run for re-election*approaching end of term | 0.129 | 0.131 | 0.186* | 0.187* | 0.189* | 0.222** | 0.232** | 0.189* | 0.247** | 0.198* |
| | (0.109) | (0.107) | (0.104) | (0.104) | (0.105) | (0.109) | (0.109) | (0.0969) | (0.110) | (0.106) |
| Observations | 472 | 472 | 472 | 472 | 472 | 472 | 472 | 472 | 472 | 472 |

Robust standard errors clustered at the province level

Notes: Each column represents a separate regression. The unit of observation is province-year. The time period spans the years 2007-2014. Robust standard errors are clustered at the province level.
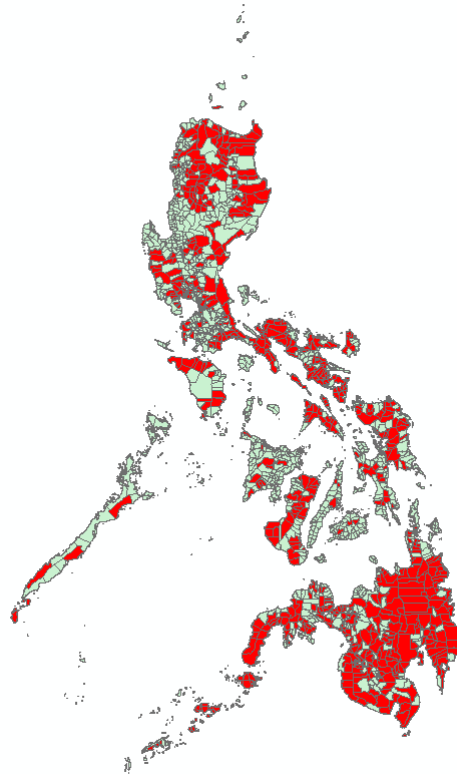
* Significant at the 10% level
** Significant at the 5% level
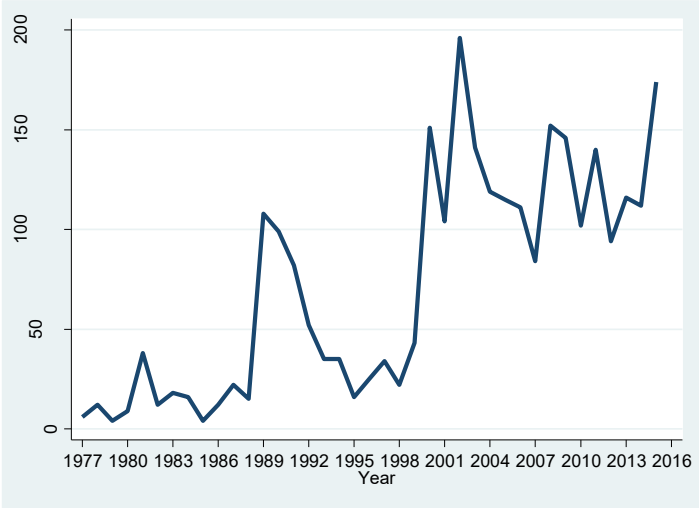*** Significant at the 1% level

## A.3 Conflict

Figure A. 8. Towns in the Philippines with at Least One Conflict Incident, 1977-2016

Source of data: Combined Uppsala Conflict Data Program and the Abinales-Reyes data.

Figure A. 9. Number of Conflict Incidents Every Year, 1977-2016



Notes: This data shows the evolution of conflict incidents over time. Conflict peaks in the late 1980s to early 1990s, and then again beginning in the early 21st century.
Source of data: Combined Uppsala Conflict Data Program and the Abinales-Reyes data.

Figure A. 10. Sample Dictator Game Task Page for Subject who is Muslim and Maranao, English Translation

# TASK 3 DECISION:

Decision page of sender

Partner 1

**Religion:**
Muslim
Ethnicity:
Unkown

Amount to keep for yourself | Amount to send to partner

[ ] + [ ] = **P500**

Partner 2

**Religion:**
Christian
Ethnicity:
Unkown

Amount to keep for yourself | Amount to send to partner

[ ] + [ ] = **P500**

Partner 3

**Religion:**
Muslim
Ethnicity:
Maranao

Amount to keep for yourself | Amount to send to partner

[ ] + [ ] = **P500**

Partner 4

**Religion:**
Muslim
Ethnicity:
Maguindanao

Amount to keep for yourself | Amount to send to partner

[ ] + [ ] = **P500**

Notes: The above is a translated version of the decision page from the booklet that is given to participants.

Figure A. 11. Distribution of Subjects by Timing of Migration Relative to Conflict Exposure

Panel A. Year Migrated vs Year of First Conflict.
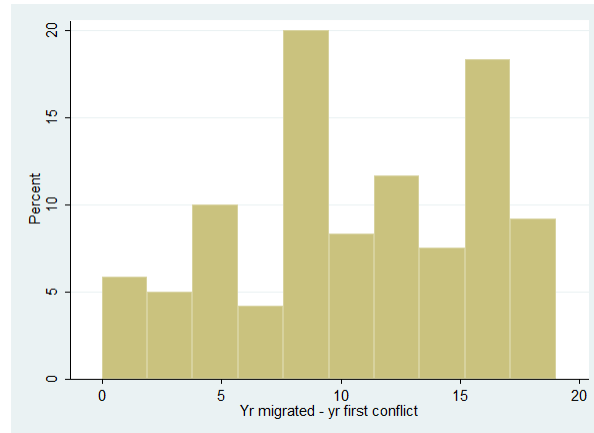


Panel B. Year Migrated vs Year of Last Conflict.
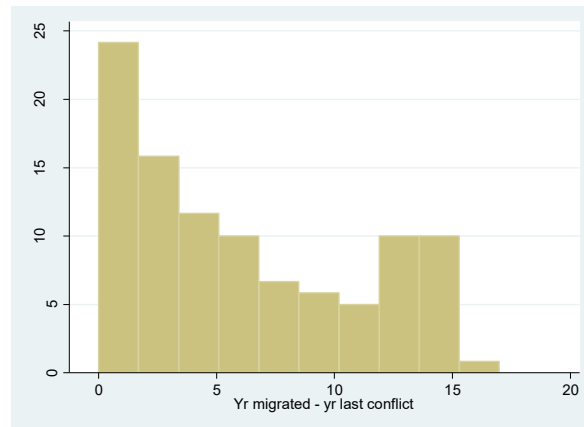


Notes: The above graphs show a histogram of participants by the years lapsed since the first conflict (Panel A) and the second conflict (Panel B).

Figure A. 12. Fraction of Participants Choosing to Split Equally, by Religion of Partner and Conflict Exposure
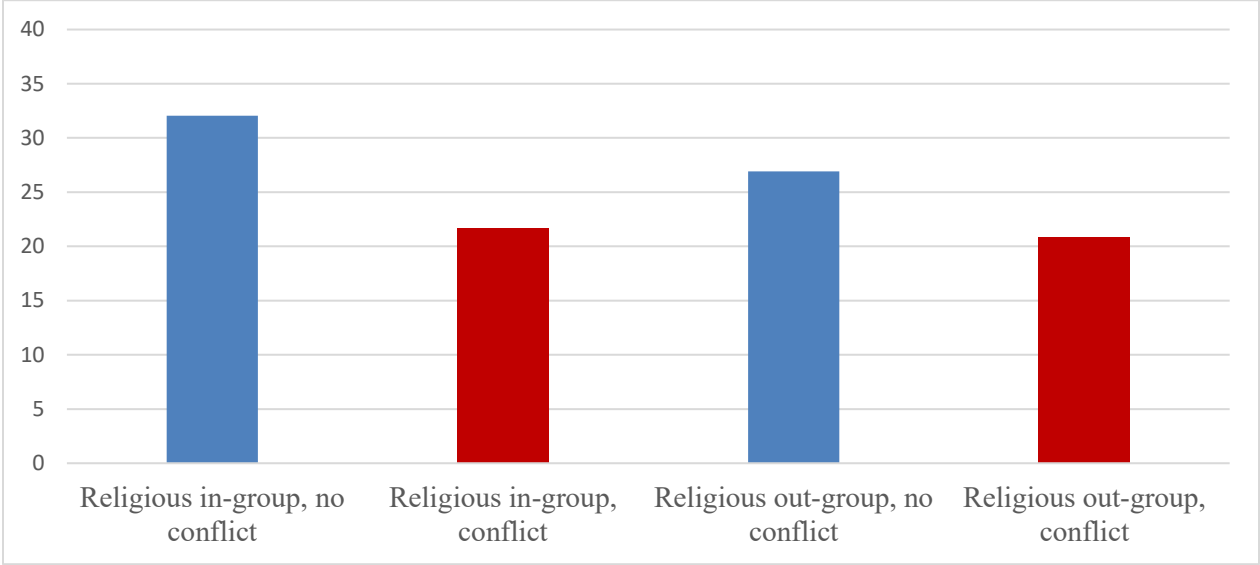


Notes: This figure shows the fraction of participants choosing to split the Php 500 equally between them and their partner, separately for the identity of their partner and by conflict exposure.

Table A. 11. Number of Participants in Each Session

| Session | Location | Type | Ramadan | Number of participants |
|---|---|---|---|---|
| 1 | Taguig | Pure Muslim | Yes | 22 |
| 2 | Taguig | Pure Muslim | Yes | 26 |
| 3 | Taguig | Mixed | Yes | 24 |
| 4 | Taguig | Mixed | Yes | 20 |
| 5 | Taguig | Pure Christian | Yes | 20 |
| 6 | Culiat | Pure Muslim | Yes | 15 |
| 7 | Culiat | Pure Muslim | Yes | 20 |
| 8 | Culiat | Mixed | Yes | 25 |
| 9 | Culiat | Mixed | Yes | 24 |
| 10 | Culiat | Pure Christian | No | 20 |
| 11 | Greenhills | Pure Muslim | No | 17 |
| 12 | Greenhills | Pure Muslim | No | 15 |
| 13 | Greenhills | Mixed | No | 20 |
| 14 | Greenhills | Mixed | No | 22 |
| 15 | Greenhills | Pure Muslim | No | 8 |
| 16 | Greenhills | Pure Christian | No | 18 |
| 17 | Taguig | Pure Muslim | No | 12 |
|  |  |  | TOTAL | 328 |

Table A. 12. Summary Statistics (Average Per Subject Exposed to Conflict)

| | |
|---|---|
| Civilian casualties | 5.82 |
| | (5.64) |
| Government casualties | 3.08 |
| | (5.29) |
| Communist conflict incidents | 0.59 |
| | (2.35) |
| Muslim conflict incidents | 3.64 |
| | (5.42) |
| Conflict incidents | 4.42 |
| | (6.17) |
| Observations | 120 |

Notes: This table presents the subject-level average on each conflict characteristic that we have data on. The Abinales-Reyes data do not have these variables.
Source of data: Uppsala Conflict Data Program.

Table A. 13. Summary Statistics of Hometowns, by Conflict Incidence

|  | No conflict | Conflict | p-value |
|---|---|---|---|
| Land area | 313.4074 | 432.4135 | 0.2596 |
|  | (54.5177) | (100.8331) |  |
| Population density in 2000 | 695.0031 | 437.3908 | 0.4700 |
|  | (220.9424) | (100.6478) |  |
| Population density in 1995 | 780.2941 | 508.1743 | 0.4584 |
|  | (250.7943) | (115.5898) |  |
| Population density in 1990 | 853.0722 | 563.8938 | 0.4700 |
|  | (273.3394) | (128.1075) |  |
| Poverty incidence | 0.3965 | 0.4269 | 0.5084 |
|  | (0.0260) | (0.0389) |  |
| Observations | 50 | 26 |  |
| Road density (cities) | 2.2 | 0.5618 | 0.1348 |
|  | (0.7827) | (0.2022) |  |
| Observations | 8 | 5 |  |

Notes: This table shows the means on the hometown-level observable characteristics by conflict incidence. The last column shows the p-value from a hypothesis test of whether no conflict and conflict towns are different on each characteristic.

Table A. 14. Individual Characteristics, by Conflict Incidence

|  | No conflict | Conflict | p-value |
|---|---|---|---|
| College | 0.43 | 0.53 | 0.1932 |
|  | (0.06) | (0.05) |  |
| Married | 0.56 | 0.47 | 0.22 |
|  | (0.06) | (0.05) |  |
| Male | 0.35 | 0.58 | 0.002 |
|  | (0.05) | (0.5) |  |
| Observations | 79 | 120 |  |

Notes: This table shows the means on the individual-level observable characteristics by conflict incidence, for migrants to Manila. The last column shows the p-value from a hypothesis test of whether individuals from no conflict and conflict towns are different on each characteristic.

Table A. 15. Effect of Conflict Exposure on Religious In-Group Bias

Dependent Variable: Difference in Amounts Sent in Dictator Games 1 and 2

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Experienced conflict | -62.38*** | -63.57*** | 72.00 | 94.31* | 57.37 | 68.01 |
|  | (21.39) | (21.60) | (52.30) | (52.37) | (51.41) | (56.64) |
| Muslim and experienced muslim conflict |  |  | -110.0* | -116.0* | -123.1* | -123.6* |
|  |  |  | (65.01) | (67.03) | (71.55) | (69.97) |
| Muslim and experienced communist conflict |  |  | 71.61 | 99.95* | 74.09 | 69.93 |
|  |  |  | (54.58) | (55.66) | (70.30) | (72.18) |
| Observations | 188 | 188 | 188 | 188 | 188 | 187 |
| Controls for religion and exposure to conflict | x | x | x | x | x | x |
| Controls for session characteristics | x | x | x | x | x | x |
| Controls for individual characteristics |  | x |  | x | x | x |
| Controls for conflict characteristics |  |  |  |  | x | x |
| Controls for hometown characteristics |  |  |  |  |  | x |

Notes: 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

Table A. 16. Effect of Conflict Exposure on Giving to Religious In-Group (Out-Group)

Panel A. Dependent Variable: Amount Sent in Dictator Game 1 (Religious In-Group)

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Experienced conflict | -44.48 | -50.36 | 133.6** | 170.2*** | 157.1** | 141.4** |
|  | (29.81) | (30.96) | (58.44) | (53.20) | (64.41) | (67.30) |
| Muslim and experienced muslim conflict |  |  | -290.8*** | -301.8*** | -290.3*** | -276.7*** |
|  |  |  | (67.02) | (62.59) | (69.26) | (75.33) |
| Muslim and experienced communist conflict |  |  | 157.2*** | 198.6*** | 194.1*** | 204.7*** |
|  |  |  | (50.86) | (52.05) | (64.44) | (65.80) |
| Observations | 188 | 188 | 188 | 188 | 188 | 187 |
| Controls for religion and exposure to conflict | x | x | x | x | x | x |
| Controls for session characteristics | x | x | x | x | x | x |
| Controls for individual characteristics |  | x |  | x | x | x |
| Controls for conflict characteristics |  |  |  |  | x | x |
| Controls for hometown characteristics |  |  |  |  |  | x |

Panel B. Dependent Variable: Amount Sent in Dictator Game 2 (Religious Out-Group)

|  | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Experienced conflict | 17.90 | 13.21 | 61.60 | 75.91 | 99.72 | 73.39 |
|  | (29.05) | (27.74) | (89.63) | (75.16) | (78.01) | (81.13) |
| Muslim and experienced muslim conflict |  |  | -180.8* | -185.9** | -167.2* | -153.2 |
|  |  |  | (104.3) | (90.53) | (98.06) | (97.38) |
| Muslim and experienced communist conflict |  |  | 85.60 | 98.62 | 120.0 | 134.8 |
|  |  |  | (87.72) | (77.11) | (92.64) | (90.40) |
| Observations | 188 | 188 | 188 | 188 | 188 | 187 |
| Controls for religion and exposure to conflict | x | x | x | x | x | x |
| Controls for session characteristics | x | x | x | x | x | x |
| Controls for individual characteristics |  | x |  | x | x | x |
| Controls for conflict characteristics |  |  |  |  | x | x |
| Controls for hometown characteristics |  |  |  |  |  | x |

Notes: 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