

Damaging Democracy? Security Provision and Turnout in Afghan Elections[†]

Luke N. Condra, Michael Callen, Radha K. Iyengar, James D. Long, Jacob N. Shapiro[‡]

Abstract

In emerging democracies, elections are encouraged as a route to democratization. However, not only does violence often threaten these elections, but citizens often view as corrupt the security forces deployed to combat violence. We examine the effects of such security provision. In Afghanistan's 2010 parliamentary election, polling centers with similar histories of pre-election violence unintentionally received different deployments of the Afghan National Police, enabling identification of police's effects on turnout. Using data from the universe of polling sites and various household surveys, data usually unavailable in conflict settings, we estimate increases in police presence decreased voter turnout by an average of 30%. Our results adjudicate between competing theoretical mechanisms through which security forces could affect turnout, and show behavior is not driven by voter anticipation of election-day violence. This highlights a pitfall for building government legitimacy via elections in weakly institutionalized and conflict-affected states.

[†] The authors are grateful for the help and support provided by Democracy International, the International Security and Assistance Forces in Afghanistan, and Afghan Ministry of Interior. Eli Berman, James Dobbins, James Fearon, Clark Gibson, Stathis Kalyvas, Alex Kuo, Eddy Malesky, Gerard Padró i Miquel, Jon Pevehouse, Eric Schwab, Niloufer Siddiqui, and participants at the International Relations Colloquium of the University of Wisconsin-Madison and the Order, Conflict, and Violence Workshop of Yale University provided helpful comments. Torben Behmer, Mohammad Isaqzadeh, Shahim Kabuli, and Manu Singh provided expert research assistance. We thank Andrew Shaver and Austin Wright for sharing their data. The Harvard Academy for International and Area Studies [Long] and The U.S. Department of Defense's Minerva Initiative (AFOSR Grant FA9550-09-1-0314) provided generous funding. The authors fully and solely designed and executed the study and any opinions, findings, conclusions are those of the authors alone and do not reflect views of the United States Department of Defense, Democracy International, or institutions with which the authors hold affiliation.

[‡] Graduate School of Public & International Affairs, University of Pittsburgh; Rady School of Management, UCSD; RAND Corporation; Department of Political Science, University of Washington; Department of Politics, Princeton University, respectively.

1. INTRODUCTION

In modern democracies, elections form the fundamental mechanism to aggregate citizen preferences and delegate power to representatives (Cox, 1997; Przeworski, Stokes, & Manin, 1999). In conflict-prone transitioning societies, the strengthening of emergent democratic institutions requires citizen participation in the voting process to bolster public confidence in elections and solidify regime legitimacy (Norris, 2014). Problematically, elections in developing countries are associated with an increased probability of conflict (Hafner-Burton, Hyde, & Jablonski, 2014; Hyde & Marinov, 2012), including from non-state actors who strategically use violence to deter voting and undermine support for the government (Berrebi & Klor, 2006; Stedman, 1997). Accordingly, governments and international donors have invested heavily in security sector reform of the police and military for post-conflict reconstruction in fragile states to enhance governance and safeguard the population (Berg, 2014; Perito, 2009; Sedra, 2006)¹, including assigning police to polling stations to protect poll workers and allow citizens to vote safely (Doyle & Sambanis, 2006; Fortna, 2008).

Although literature on modern state-building emphasizes the broad role that security provision plays in the legitimization of the state and its monopolization of violence (Fukuyama, 2004; Levi, 1988), scholars have paid less attention to whether and how the police may directly shape electoral processes by increasing or decreasing the likelihood of election day violence and turnout. And despite the significant investment in safeguarding elections, such efforts do not always prove successful in encouraging voting. If, for example, insurgents adjust their strategies in response to changes in the location and levels of security deployment, or if the police assigned to polling stations lack capacity or

¹ In Iraq, the US spent \$26 billion on “Security and Rule of Law” through 2012 (Bowen, 2013) and \$71 billion on reconstruction of Afghan security forces through March 2017 (SIGAR, 2017, p. 5). These amounts include tens of millions of dollars specifically for election security.

are predatory, then additional security force deployments may *deter* participation. Whether and under what conditions increases in security provision actually help fragile states hold elections and democratize remains poorly understood in theory and practice.

A lack of clarity on whether policing assists elections may arise because of two distinct mechanisms we highlight that shape police's effects on voting, each of which generates contrasting empirical implications for turnout. First, police may directly affect the production of insurgent violence and therefore a citizen's opportunity cost of voting. Literatures on the economics of crime (e.g., Draca, Machin, & Witt, 2011; Levitt, 1997) and the political economy of insurgency (e.g., Biddle, Friedman, & Shapiro, 2012) demonstrate that increasing the number of security forces can decrease local levels of crime and violence through deterrence. Securing polling locations with more police should therefore increase electoral turnout by protecting polling stations for voters and decreasing insurgent violence. At the same time, extra security deployments carry the risk of presenting non-state violent actors with additional targets and/or incentives to signal resolve by increasing attacks (Hultman, 2007; Kydd & Walter, 2006).² Anticipating this, citizens might stay home if they believe that their likelihood of exposure to insurgent violence is increasing in the presence of police during elections.

Policing may also shape turnout through a second mechanism, related to citizens' perceptions of security officials. Ideally, a police officer serves as a symbol of the government that fosters order (Serchuk, 2006). When police serve the public good, police deployment around elections should increase feelings of security and work to increase voter turnout. However, in many developing countries, security forces are predatory, poorly trained, and perceived as corrupt by the citizens whom they are supposed to protect (Olken & Pande, 2012). Therefore, citizens could view the election as an

² For example, opposition by many US lawmakers to President Bush's plan for a "surge" of troops in Iraq argued that additional deployments would increase targets for insurgents (CNN, 2007).

opportunity to punish the government for corruption embodied in its security forces (Finan & Ferraz, 2008), which implies that turnout increases alongside increases in the level of security force presence. Conversely, scholars have shown that negative interactions with police lead to decreased trust and confidence in police (Nagin & Telep, 2017) and perceptions of state legitimacy (Sunshine & Tyler, 2003), implying less willingness to cooperate or engage with the government (Tyler, 2005; Tyler & Fagan, 2009). More police could therefore cause citizens to avoid interaction with security officials – for example, for fear of abuse or bribery – and opt-out of participation in democratic institutions and voting, *even if* police deter violence.

In this paper, we examine whether and how policing shapes citizens' electoral behavior in contexts where insurgents threaten democratic institutions. Specifically, we study how variation in the Afghan government's assignment of levels of policing in polling centers' immediate vicinity influenced voter turnout in Afghanistan's 2010 parliamentary (*Wolesi Jirga*) election -- one that was threatened by considerable insurgent activity in the run-up to, and threats on, election day (Callen & Long, 2015). We do this by combining rich and unique data on the assignment of Afghan security force levels to polling centers, event conflict data mapped to precincts, certified voter turnout data, and survey data on voters' perceptions of the police and government. The wealth of data we bring to bear is noteworthy because scholars often lack the data to address these questions in conflict settings.

The Afghan government was under considerable pressure to prevent insurgent violence during this election through security deployments, both to avoid criticism that the government was inept at preventing violence during voting and to cooperate with the NATO-led International Security Assistance Force's (ISAF) goal of protecting voters as a key benchmark for the government to assert its political control. As such, the government began coordinating security with ISAF much earlier than they had for the 2009 election, and planned a deployment of tens of thousands of security force officers in and near polling sites, stationing at least one officer at each site's entrance. Afghan voters therefore could not have voted without seeing and interacting with at least one security official. However,

worried about insurgent violence and the potential disruption of the election, the government assigned *additional* police and security officers to some polling centers and areas adjacent to them.³ Voters in these locations would have been exposed to more policing at or near their polling center. Because deployment happened prior to the election (albeit only a few days to a week), there was enough time for locals to see the enhanced police presence where they reside and vote. This is key because it means that voters could have adjusted their voting behavior in anticipation of changes in the level of insurgent violence and that they would have had more interactions with the police where force levels were increased. Therefore, both mechanisms that impact turnout were potentially in play, though the *ex ante* effect of the deployments on turnout remains unclear. Estimating the effects of increased policing on election outcomes, like election day violence and turnout, necessarily faces inferential problems if the government specifically deployed more police to areas where it anticipated more violence or if a confounding factor correlated with anticipated violence also drives turnout.

Therefore, to isolate the effect of police deployments on violence and turnout, we make use of an artifact of the Afghan government's security force assignment process. The chief determinant of police level assignment to polling centers was the history of insurgent violence experienced in the surrounding area because the government believed that previous violence served as the best forecast for how much violence a polling center would experience on election day. Using micro-data on incidents of insurgent violence against Afghan government and coalition forces, we show that polling centers with similar

³ Author compiled the observer reports from the largest international election observation mission in 2009 and 2010. Although only anecdotal, the reports independently verify that polling stations visited by observers consistently had one officer assigned at the polling station entrance in safer areas, and more officers assigned in and near stations in more violent areas.

histories of violence in the same district were assigned *different* levels of police deployment.⁴ This mismatch is due in no small part to the fact that many officials at multiple institutions managed the process, while also overseeing other pressing policy issues that competed for the principals' attention. Because of that multi-layered process, we argue that conditional on past insurgent violence in the area of the polling center, assignment of (additional) police deployment level was orthogonal to anticipated violence on election day. Once we control for expected violence in a more detailed and systematic way than planning officials did – using the same ISAF administrative data on the history of violence around polling centers that they used in their deliberations – variation in treatment assignment to polling centers within the same district should be quasi-random. This allows us to estimate the exogenous impact of police on actual election violence and turnout. The credibility of our identification strategy is strengthened by our own first-hand observation of the police deployment planning process,⁵ as well as multiple interviews of relevant officials through whom we gained detailed institutional knowledge of the way in which police deployment was assigned. The data enable us to not only estimate the overall relationship between security deployment and turnout, but to also test the logic of competing

⁴ As one example, consider two polling centers 0.25 km apart in Sarkani district of Kunar Province. While they have the exact same history of violence in terms of insurgent attacks, the polling center at a clinic received a High Security Deployment designation, while the polling center at a nearby school was given a Low Security Deployment designation.

⁵ One of the authors was working at ISAF headquarters during the run-up to the election and sat in on a number of planning meetings for the ANSF deployments. Author did not serve in an advisory capacity to ISAF or the Afghan government, so Author should be considered a passive observer with detailed knowledge of the process through which the government developed its deployment schedule.

hypotheses on the mechanism explaining behavior. Both mechanisms may operate to some degree, but the question is which predominates.

To preview results, we find that polling centers that were assigned extra police did not experience a statistically significant change in insurgent violence, but these centers did experience a marked *decrease* (about 30% on average) in turnout relative to similar polling centers that received fewer security forces. These results are robust to accounting for multiple possible threats to identification, including measures of electoral competitiveness, ethnic demography, electoral fraud, and other characteristics of polling centers potentially correlated with police presence. To explore whether this negative result of police on turnout arises from the mechanisms we highlight, we employ analysis of primary survey data collected from potential voters before and after the election to find out why citizens did or did not (plan to) vote. Consistent with police not having a significant effect on increasing or decreasing election day violence, evidence from these data casts doubt on police affecting turnout through the mechanism of perceived voter safety: citizens surveyed around polling centers designated to receive higher policing levels were no more likely to cite fear of violence on election day as a reason for not voting than citizens located around polling centers designated to receive fewer police. Instead, the evidence is consistent with the effect operating through a (negative) “trust” mechanism: extra policing seems to have had the unintended effect of deterring turnout because voters did not want to interact with a corrupt police force.

While Afghanistan’s modern political history is distinct, our results speak to similar challenges of reducing violence to protect elections in many developing countries that have moved towards democracy over the last 25 years (Collier, 2009). The 2010 parliamentary elections were an important test of the Afghan government’s ability to independently establish and manage electoral institutions that gain citizen compliance and increase government legitimacy (Callen & Long, 2015); but, the election took place at a time when insurgents could credibly threaten to disrupt elections through violence (Coburn & Larson, 2014). This is a common feature in fragile states in conflict-prone settings,

including Libya, Colombia, Yemen, and Egypt, where transitional democratic regimes use elections to strengthen government legitimacy but non-state actors threaten violence.

Moreover, Afghanistan provides a particularly illustrative case for the study of the relationship between security and voter turnout because all polling center entrances were guarded by at least one Afghan police officer. Regulations governing the voting process in Afghanistan effectively force citizens to interact with police in order to vote, so potential voters knew they would have at least brief exchanges with the police. However, increased deployments near some polling stations created additional contacts between voters and police that may have negatively affected turnout since Afghanistan, like many developing democracies, suffers from extensive corruption in the police. Similar deployment of security forces to protect voters is a standard practice in post-conflict countries that have insurgencies or the potential for election-day violence, including police officers stationed at polling stations in East Timor, Sierra Leone, Iraq, Uganda, and Kenya. Results from Afghanistan suggest that the problem of security sector corruption across the developing world will continue to pose numerous challenges for encouraging participation in democratic institutions, especially as governments contemplate increases in deployments. We discuss further the comparative insights of our results to numerous literatures in the Conclusion.

2. POLICING, VIOLENCE, AND ELECTIONS IN EMERGING DEMOCRACIES

Fragile conflict-prone states frequently face violence aimed at disrupting nascent democratic institutions, including elections (Collier, 2009). Security forces therefore play a crucial role in supporting elections in countries emerging from, or still engaged in, violent conflict (Binkerhoff, 2007;

Leach & Kingsbury, 2013) by protecting polling stations and voters.⁶ While election violence sometimes arises from incumbents or parties contesting office who strategically attack or threaten the opposing side's voters to garner support among their base (Gutiérrez-Romero, 2014; Wilkinson, 2004), violence may also result from non-state actors who strategically use violence to deter voting and undermine support for formal political actors, including the government (Berrebi & Klor, 2006; Stedman, 1997). In light of observed spikes in insurgent violence designed to disrupt voting (Condra, Long, Shaver, & Wright, 2017), governments threatened by insurgency must therefore decide how to allocate security forces to safeguard elections (Mashego & Hartley, 2016; "Police gear up to prevent election violence," 2008). In fragile states where successful elections depend crucially on securing voting, does increasing police help or hurt turnout?

To answer this question, we identify two mechanisms that theoretically can directly or indirectly influence the potential relationship between security deployment and turnout, but that produce empirically opposing predictions. While either mechanism may function to some degree, we are interested in which predominates. First, police may directly affect the production of insurgent violence and therefore citizens' opportunity cost of voting. On the surface, it appears intuitive that increased

⁶ Past experience with violence and crime could affect individuals' propensity to mobilize politically (Bateson, 2012; Bellows & Miguel, 2008; Blattman, 2009), and turn out to vote. Although only a few studies look at the effects of violence on the propensity to vote in emerging democracies, scholars highlight a number of other factors driving turnout in these contexts, including allegiance to ethnic groups (Horowitz, 1985), commitment to democratic principles (Bratton, Mattes, & Gyimah-Boadi, 2004), positive incentives from political actors like vote-buying (Chandra, 2004; Posner, 2005), social pressure (Jung & Long, 2015), and individuals' demographic characteristics (Kasara & Suryanarayan, 2015).

policing should assist electoral processes by providing protection for polling workers to conduct the election and for citizens to feel secure enough to vote. A “deterrence” model in the economics of crime literature suggests that the deployment of additional police reduces illicit behavior by raising the probability of detection and punishment of illegal actions (Becker, 1968; Levitt & Miles, 2006). Studies have found that the deployment of additional police can, under certain conditions, reduce violence (Di Tella & Schargrotsky, 2004; Draca et al., 2011; Levitt, 1997), and, extending to insurgent settings, studies demonstrate that deploying additional forces can reduce insurgent activity (Berman, Felner, Shapiro, & Troland, 2013; Biddle et al., 2012). Therefore, if the police function as designed, their presence should decrease election-day violence and bolster turnout by deterring potential insurgent activity.

However, while a deterrence effect may obtain with increases in security provision generally, it may not operate the same way in conflict-prone settings. Increasing security forces could generate new targets for insurgents, especially those intent specifically on disrupting elections with violence by attacking government institutions and voters to undermine the process. Insurgents may also increase violence in an effort to bargain for political concessions (Hultman, 2007) or to signal their strength and resolve (Kydd & Walter, 2006).⁷ Therefore, added police may depress turnout if citizens anticipate that their presence signals a greater likelihood of insurgent violence on election day.

⁷ This trade-off was made salient in the Bush Administration’s “surge” in Iraq and Obama’s “surge” in Afghanistan. While the aim for both was to eventually decrease overall levels of insurgent violence, both policies sparked concerns that the additional force lay-downs would potentially increase violence in the short-term as insurgents ramped up attacks in response. For example, US Senator Norm Coleman argued that the 2007 troop surge in Iraq, “would put more American soldiers in the crosshairs of sectarian violence and create more targets” (CNN, 2007).

Aside from the question of whether policing affects turnout through violence, we argue that a second mechanism related to citizens' perceptions of trust and legitimacy in the police can play a role in motivating or demotivating political action (Tyler, 2004, 2006). As the most visible embodiment of the state's coercive power for many people, police provide the most routine interaction citizens have with the state on any frequent basis (Soss & Weaver, 2017), especially where non-state actors threaten violence against civilians. Ideally, a police officer serves as a symbol of the government that fosters order (Serchuk, 2006). When police are perceived to be acting in service of the public good, police deployment around elections should work to increase turnout, building on increasing levels of political engagement among the population. But police corruption is pervasive across emerging democracies. Particularly in weak states struggling against political violence, being forced to pay bribes by police is a part of daily life for citizens (Fried, Lagunes, & Venkataramani, 2010). In countries as diverse as Uganda, Vietnam, the Philippines, Kenya, El Salvador, Pakistan, and Venezuela (among many others), the police are perceived as the most corrupt of all public institutions (Hardoon & Heinrich, 2013). The nature of citizens' interactions with police affects citizens' trust and confidence in the police, as well as their perceptions of the state's legitimacy (Lerman & Weaver, 2014; Nagin & Telep, 2017).

In contexts where citizens view the police as ineffective, corrupt, or predatory, electoral turnout could either be positively or negatively affected. Citizens might react to reports of political corruption by deciding to punish guilty politicians at the polls (Finan & Ferraz, 2008), thereby discouraging further state corruption. This mechanism is at least suggested in multiple cases where, despite perceived rampant corruption in the administrative state, turnout in elections is equal to or *higher* than the least corrupt countries in the world.⁸

⁸ For example, Sudan ranked 172 out of 178 in Transparency International's (TI) 2010 Corruption Perceptions Index and turnout in that election is estimated at 72% (IDEA, 2016). Similarly, even in the

More likely, perceptions of corruption within the security forces could delegitimize the state in the eyes of citizens (Seligson, 2002) – as has been shown in Afghanistan specifically (B. Rubin, 2007; Torabi & Delesgues, 2007) -- and reduce citizens' desire to lend further legitimacy to the government and participate in elections overseen by these agents. Indeed, evidence shows that increasing citizens' knowledge of political corruption leads them to withdraw from political processes, *decreasing* turnout (Chong, De La O, Karlan, & Wantchekon, 2015; Stockemer, LaMontagne, & Scruggs, 2013). Police abuse causes citizens to hold negative views of the government and to decrease participation in other institutions and processes in public life (Nagin & Telep, 2017).

We therefore deduce the following hypotheses regarding the potential effects of policing on turnout through our two mechanisms:

H1a - *Deterrence*: additional police *decrease* violence and therefore *increase* turnout;

H1b – *Signaling/Targeting*: additional police *increase* violence and therefore *decrease* turnout;

H2a – *Punishment*: conditional on violence, additional police *increase* turnout because citizens want to punish the state for state agents' corrupt and predatory behavior;

H2b – *Trust/Legitimacy*: conditional on violence, additional police *decrease* turnout because citizens' increased interaction with corrupt and predatory state agents causes a decline in trust of state institutions and willingness to engage them.

3. SETTING

Elections have played an important role in efforts to democratize, legitimize, and strengthen the Afghan state over the past 15 years. After the overthrow of the Taliban in 2001, the Constitutional Loya Jirga (CLJ) ratified Hamid Karzai as leader of the Afghan Interim Authority at the Bonn Conference, and

face of extreme insurgent violence, turnout in Iraq's most recent elections (which consistently ranks near the bottom of TI's index) is above 60% (IDEA, 2016). In Venezuela, 58 of 167 in TI's 2015 index, turnout was estimated at over 73% recently (IDEA, 2016).

worked to create democratically-elected presidential and parliamentary institutions. The government sought to legitimize these institutions by holding the first presidential elections in 2004 (which Karzai won), followed by the first parliamentary (*Wolesi Jirga*) elections in 2005. Karzai was re-elected in 2009, followed by 2010 parliamentary elections. But like other weak states, ongoing violence has threatened these elections from armed actors not participating in the formal political process.

Insurgent violence is a real worry for policymakers and voters in Afghanistan, which means that the deterrence/signaling mechanism is reasonably in play. In the 2009 presidential election, there were over 400 attacks on election day, including at polling stations and against security forces deployed to protect voters (NDI, 2010). For the 2010 parliamentary race, the Afghan government worked closely with several important actors, including the ANSF and ISAF, to begin security coordination efforts much earlier than they had in 2009. This included a risk-assessment of all potential polling stations in order to determine which stations required additional security, along with rapid training of new police officers (NDI, 2011). Ultimately, 52,000 Afghan National Police officers and 63,000 Afghan National Army officers were deployed near polling centers on election day in 2010 (NDI, 2011). Afghan voters could not have voted without seeing and interacting with at least one police officer at polling station entrances. In a low deployment area, a voter would see one officer at the entrance making sure that no one was armed as they entered (this included poll workers and voters). In areas with higher deployments, there was more than one officer at the entrance, or additional police outside the walls of, or in alleys and roads leading into, polling centers. Citizens had more interaction with police around these polling centers.

While the government's security deployment planning for the 2010 parliamentary election largely focused on how to safeguard elections by deterring insurgent violence, the Afghan Ministry of Interior (MOI) also worried specifically about how to encourage public trust in the police ahead of the force laydown (Author, 2010), possibly in part because of the considerable evidence that depicts the ANP as largely corrupt and predatory (Felbab-Brown, 2013; Giustozzi & Isaqzadeh, 2012). Police in

Afghanistan have long engaged in a wide range of criminal activity (Felbab-Brown, 2013; Rashid, 2008; B. Rubin, 2007). The competence of the police force is heavily criticized and the government poorly manages this institution. Attrition and desertion rates are high, and the MOI has been unable to attract high quality, educated recruits. In 2011, the Minister admitted that 90% of the force was illiterate. Aside from incompetence, drug addiction, and involvement in the narcotics trade (Giustozzi & Isaqzadeh, 2012), there is considerable evidence that the police mistreat civilians (Giustozzi, 2008). Giustozzi and Isaqzadeh (2012) document a litany of corruption allegations from witnesses throughout Afghanistan, including truck drivers who reported being regularly asked for bribes on the primary ring road connecting major cities. A 2010 survey focusing on police indiscipline uncovered the practice of guns being taken from recruits before going off duty because recruits were using them to rob civilians. Police frequently bribe civilians: 25% of UN survey respondents reported having paid at least one bribe to police in the previous year (UNODC, 2010) (surely an undercount), and the police and the justice system were the two sectors perceived to be the most corrupt in 2006, according to a survey fielded by a prominent Afghan NGO (Torabi & Delesgues, 2007). Police reportedly have also engaged in rape, torture, and extrajudicial executions of civilians (Giustozzi, 2008).

In addition, data from several public opinion surveys (which we describe in more detail below) demonstrate that citizens perceive the police as predatory and abusive. Pashtuns, the largest ethnic group, had a particularly challenging relationship with the ANP, who are commonly thought to be dominated by Tajiks and Uzbeks, ahead of the 2010 election. Of 19,579 respondents in the two Afghan National Quarterly Assessment Report (ANQAR) waves of quarterly surveys immediately before the election, 31% of Pashtuns reported seeing the police engage in corrupt acts while only 17% of Tajiks and 9% of Uzbeks reported the same (Table 1, Panel B). These are not simply reflections of the geographic dispersion of corruption and ethnicity. Pashtun respondents are significantly more likely than others to report having seen corruption even when adding province fixed-effects to a range of regression models in the ANQAR data. A different survey of residents in Pashtun-dominated Kandahar

Province (Table 1, Panel A) revealed that 73% of respondents disagreed a little or a lot to the statement, “ANP officers treat members of the local community with respect”; similarly, 74% disagreed with the statement, “ANP officers are well respected by local people.” A full 62% of Kandahar respondents agreed with the statement, “ANP officers in my area sometimes beat people up.” 64% agreed, either a little or completely, with the statement, “most ANP officers are corrupt.” To the statement, “ANP officers put the interests of their community before their own interests,” 70% disagreed. In sum, there is considerable and systematic evidence that many Afghan civilians perceive police as corrupt and predatory.

4. DATA AND ESTIMATION STRATEGY

4.1 Data

In our analysis we employ data from six sources: (1) levels of policing assigned to polling centers, which constitutes the treatment variable, (2) certified voter turnout data from the Afghan Independent Election Commission (IEC), our primary dependent variable, (3) violence data collected by ISAF, (4) data from ISAF’s ANQAR surveys, (5) new survey data that we collected from 5,000 individuals across 471 polling center catchment areas in nineteen provinces across all regions, and (6) survey data of the 369 residents of Kandahar referenced above. SI Table 1 provides summary statistics.

Policing: To select polling centers that warranted higher levels of police deployment, the Afghan MOI, in coordination with ISAF, developed a three-tier categorization. “Secure” polling centers received no additional police above and beyond their normal baseline level (i.e., Low Security Deployment). These centers had one police officer responsible for checking voters for weapons before they could enter to vote. “Medium insecurity” polling centers were to receive an extra deployment of police (i.e., Medium Security Deployment), and “highly insecure” polling centers were to receive even more additional police (i.e., High Security Deployment). For example, medium and highly insecure centers had additional security officers adjacent to the polling center at road or alley entrances to the

voting area and potentially at risk of insurgent attack. Police were home (not deployed) during the Muslim holy month of Ramadan (August 11-September 9), and were deployed to polling centers a few days to a week leading up to the election on September 18.

Voter Turnout: Turnout data comes from the IEC and show the total number of ballots cast by polling center, which contains multiple polling stations (NDI, 2011, p. 32).⁹

Insurgent Violence: As a measure of violence affecting civilians, we use recently declassified incident reports submitted by ISAF and Afghanistan security forces that report combat occurring between ISAF units and insurgents, commonly known as “significant activity” or SIGACTs (Shaver & Wright, 2016). The data provide the incident’s date, time, and georeferenced location. We use the subset of that dataset of insurgent attacks for the period March through December 2010 (29,324 incidents covering roughly six months before the election and three months after). We create a count of incidents occurring within a radius of one kilometer for each polling center to isolate the impact of police in the immediate vicinity of the location they deployed to protect.¹⁰ While the SIGACTs data

⁹ Parliamentary candidates run “at-large” within a province and voters cast a single non-transferable vote. Each province returns multiple members to parliament based on population. Winning candidates are those who garner the most votes within the province corresponding to its number of allocated seats.

¹⁰ We examine violence within one kilometer of the polling center as the dependent variable because the Afghan government mandated a force laydown of one kilometer around polling centers for the ANP. Author was privy to information on force laydown protocols in his capacity as an accredited observer for the 2010 election, and was briefed by IEC about security provisions multiple times. Additionally, interviews with officials involved in the deployment planning process confirm this force laydown plan (Author, 2014a, 2014b, 2014c), as do independent election reports (FEFA, 2011, p. 46).

do not capture all violence that civilians experience, it is the most complete and comprehensive dataset measuring violence during this time period.¹¹

Surveys: We use three different survey datasets related to Afghans' opinions about the police to test observable implications of our theoretical mechanisms. The first dataset comes from ISAF's ANQAR survey waves (Berman, Callen, Felter, & Shapiro, 2011; Blair, Imai, & Lyall, 2014). The samples for the surveys are a nation-wide poll of Afghans aged eighteen or older, and UN and World Food Program population statistics (region, province, and district-level) are used to draw the sample. We use waves 7 (9,191 respondents from March 2010) and 8 (10,388 respondents, May/June 2010), taken closest to the election. The second survey dataset comes from two surveys we designed and administered of households living in the immediate vicinity of polling sites. We fielded a baseline in August 2010 (before police were deployed for the election) and an endline in late November/early December 2010. Our baseline (2,904 respondents) comprises 450 polling centers in nineteen of thirty-four provincial capitals. Our endline (3,100 respondent) includes 471 polling centers (7.8% operating on election day), matching the baseline sample with twenty-one centers added in Kabul. We selected our sample by identifying polling centers scheduled to open on election day and deemed secure by

¹¹ Other studies demonstrate that civilians bear considerable risk of being killed or wounded during the course of SIGACTs (Condra & Shapiro, 2012). As a measure of electoral violence, SIGACTs are highly preferable to other possible measures primarily because others (e.g., UNAMA, National Democratic Institute) do not provide information at sufficient temporal or geographic specificity to enable matching of incidents to polling center locations.

ISAF and ANP.¹² The third survey dataset is geocoded survey responses from 369 Afghan civilians between August and October 2010 in eighty-four villages of Maiwand and Arghandab districts of Kandahar province collected by a commercial entity with experience in social research. The data record general demographic information and answers to questions about perceptions of armed actors in Pashtun areas. Villages were selected within key terrain districts with between five and twenty individuals surveyed, depending on population density.

4.2 Estimating Police's Effect on Insurgent Violence and Turnout

Our knowledge of the Afghan government's plans to protect and encourage voting in the 2010 election via police deployment across polling centers is based heavily on Author's role providing independent objective assessment of research efforts for the Commander of International Forces in Afghanistan. This role included attending weekly meetings at the MOI reviewing police deployment plans in the lead-up to the election with Ministry officials and ISAF regional commanders responsible for deciding how to deploy police. Authors' post-election interviews with other MOI officials (referenced below) serve as independent corroboration of the way in which the government decided to deploy police and accords with public documentation of this process published after the election (FEFA, 2011; NDI, 2011).

Critically, decisions about deployment categorization were based largely on the history of violence around the polling center, as the best measure officials had of anticipated violence on election day (Author, 2010). Officials from the National Police Command Center, ISAF, and MOI met at least

¹² To obtain a representative sample of respondents living near polling sites, enumerators employed a random walk pattern starting at the polling site, with random selection of every fourth house or structure. Respondents within households were randomly selected via Kish grid.

monthly in early 2010, and reviewed data collection of daily incidents of insurgent attacks across the country to determine the categorization of polling centers, according to a former MOI official who was involved personally in these planning meetings (Author, 2014c). These officials had a good sense of the broad trends in insurgent violence within districts, but did not consider the dynamics at each individual center within a district in all but the most prominent cases. Officials split polling centers into three categories: High Security Deployment, Medium Security Deployment, and Low Security Deployment, with Medium/High designated to receive more police.

To systematically gauge how *anticipated* violence around polling areas affected the level of assigned police deployment, we aggregate all insurgent attacks that occurred within a 1-km radius of a polling center in the five months leading up to the election. Using this much more detailed assessment, we show that the officials' categorization process, which relied on their (less detailed) sense of violence, led to sites located in areas with similar histories of violence, road access, and even population characteristics receiving different levels of police.

Indeed, once district traits are taken into account, the local history of violence around a polling center – either trends or levels – does almost nothing to explain its security status (Table 2).¹³ Thus, citizens could not decide *ex ante* whether to vote based on where police were likely to be deployed because that assignment is conditionally random. In sum, detailed examination of the assignment process through personal participation, interviews of officials, and statistical analysis of the data lead us to believe that, conditional on past insurgent violence in the area of the polling center, assignment of police deployment level was orthogonal to anticipated violence on election day. Therefore, we argue that once we control for expected violence in a more detailed and systematic way than planning

¹³ Broadening the geographic area (to 2 or 5-km buffers) that might have been relevant for anticipating the level of election-day violence does not affect these results.

officials did, variation in treatment assignment of additional police to polling centers within the same district is quasi-random, allowing us to estimate the causal impact of police on turnout.

Our analysis of police deployment's effect on election violence and turnout is limited to a subsample of the full 5,524 polling centers in operation in the election. For inclusion, the polling center must meet three criteria. First, to avoid including polling centers where electoral fraud occurred, we omit all 1,324 centers that reported turnout in excess of an average of 590 votes across the polling sites within that center. Polling sites within each center were designed to have no more than 600 ballots cast, so any polling center whose average site was close to that level is suspicious, and the IEC used this threshold in their decisions to nullify results (DI, 2011, p. 33).¹⁴ Second, we exclude all 1,502 non-fraudulent polling centers located in districts that do not have at least one polling center with a medium or high security provision classification, since we require within-district variation in security classifications for estimation. Finally, in regressions that control for turnout in 2009, centers that were operational in 2010 but not in 2009 drop out of the model. After this pruning, we perform analysis on 1,823 out of a possible 2,031 polling centers that have security classification designations from the MOI in 2010. Within that sample, the breakdown of security classification is 1,448 Low Security Deployment (79%), 181 Medium Security Deployment (10%), and 194 High Security Deployment (11%).

In our full model we estimate the following using Ordinary Least Squares:

$$Police_i = \beta_1(V_{t-1})_i + \beta_2(V_{t-2})_i + \beta_3(V_{t-3})_i + \beta_4(V_{t-4})_i + \gamma_1(\overline{Vpre}_i) + \gamma_2(\overline{Vpre}_i^2) + \gamma_3(\overline{Vpre}_i^3) + d_i + \mu_i$$

¹⁴ Below we note whether inclusion of fraudulent polling centers in the sample affects the results. Callen and Long (2015) discuss the mechanics of electoral fraud in the 2010 election and why 590 votes or more per station is a likely indicator of fraud.

where, $Police_i$ is the security classification assigned to the polling center (low, medium, or high), the $V_{t-k,i}$ are four lags of insurgent violence (1 to 4 weeks prior to the election), and \overline{Vpre}_i is the average weekly violence in the earlier part of the 2010 fighting season (weeks $t-5$ to $t-22$, April through July), and d_i is a district fixed effect (or province in some specifications). We report robust standard errors clustered at the district level because that is the geographic level at which the ANP and Taliban operational command structures are typically organized.

If previous violence predicted treatment assignment, we would expect to see this reflected in the results in Table 2. The lags of violence and cubic polynomial in fighting season averages generally do not predict treatment assignment very well (column 1), explaining less than 1% of the variance in security classifications. In column 2, we add province fixed effects to the model, which improves the model fit marginally, bringing the r^2 up to 0.07. Adding district fixed effects in column 3 increases the explained variance to 32%. In the final model (column 4), we exclude the weekly lags of violence and the cubic polynomial in average previous violence remains jointly significant.¹⁵ SI Table 4 replicates Table 2 but also includes polling centers that exhibited evidence of fraud (based on the definition above) and polling centers that were open in 2010 but were not in 2009. This does not change the results, providing evidence against the concern that police deployment was correlated with electoral fraud.

The evidence shows that past violence in the vicinity of polling centers (as officials' metric for anticipated election-day violence) does not explain much variation in treatment of deployment. This is not surprising. As noted, this is in part due to assignments being based on district and higher-level data on violence, while we rely on much more local variation in violence levels in our data. The assignment

¹⁵ F -tests for joint significance on violence lags show that we can reject the null hypothesis that the lags are jointly zero in these specifications.

strategy resulted in polling centers (in the same district) with similar histories of violence being given different deployment assignments. We therefore proceed to estimate the effect of policing on violence and turnout levels around election time, controlling for the previous history of violence at each polling center.

Before doing so, we note two possible objections to our strategy and how we address them. First, while officials may have publicly and privately stated that treatment was assigned according to history of violence, one might worry that they assigned levels of police on some other (unknown to us) basis, perhaps in order to accomplish political objectives. In the SI, we analyze a set of factors that could plausibly affect both police deployment and turnout, rendering our main results spurious. The results from these tests of competing explanations support our main results and substantiate our claim of exogenous variation in treatment assignment.

Second, despite our best and repeated efforts, we have not been able to secure data on *actual* security force deployments (including number of police deployed) to individual polling centers around the election. While our interviews with multiple officials with first-hand knowledge of the deployment schedule and process suggest that the security deployment categorization scheme was adhered to in the field (Author, 2014a, 2014c), we cannot empirically verify with full certainty that polling center protocols were followed by provincial-level officials. As such, we argue that our estimates of police deployments can be interpreted as Intention-to-Treat (ITT) estimates, where perfect compliance with assignment protocols (deployment) are not easily verified by researchers (Dunning, 2012), a standard challenge in research that employs experimental or quasi-experimental designs in the field (e.g., Hyde, 2007). In the SI we show that our estimates are robust to controlling for other potential confounds. Moreover, the magnitude of the estimated effect on turnout in our analysis makes it unlikely that such unobserved factors could overwhelm those effects. Following Altonji, Elder, and Taber (2005) and Nunn and Wantchekon (2011), we estimate how strong bias from unobservables would have to be (relative to selection on observables) to explain away our treatment effects (SI Table 7).

5. MAIN RESULTS

5.1 Police's Effect on Violence

We first examine police's effect on election-day violence, where added police could decrease violence through a deterrent effect, or increase violence through signaling resolve. For all regressions in Table 3 that estimate the effect of security deployment on violence, we measure police deployments two ways: first as a binary variable which takes a value of '1' if the polling center was classified to receive any additional police deployment; and second, we include dummy variables for polling centers that received 'medium' or 'high' security provision classifications. That latter specification is estimated using OLS:

$$\Delta V_i = \alpha_1(M_i) + \alpha_2(H_i) + \beta_1(V_{t-1})_i + \beta_2(V_{t-2})_i + \beta_3(V_{t-3})_i + \beta_4(V_{t-4})_i + \gamma_1(\overline{Vpre}_i) + \gamma_2(\overline{Vpre}_i^2) + \gamma_3(\overline{Vpre}_i^3) + d_i + \mu_i,$$

where M_i and H_i are dummy variables for whether a polling center was assigned a medium or high security provision classification, the $V_{t-k,i}$ are lags of weekly violence prior to the last period of the difference, and the \overline{Vpre}_i are prior fighting season violence as before.

Across the specifications in Table 3, ΔV_i is a series of differences in violence within one kilometer of the polling center: election week versus the week prior to election (columns 1-2), election week versus the average violence in the four-week run up to the election (columns 3-4), the average violence during the four weeks after the election versus the average violence during the four weeks prior to the election (columns 5-6), and the average violence during the eight weeks after the election versus the average violence during the eight weeks prior to the election (columns 7-8). All models include district fixed effects and robust standard errors clustered by district.

As the results show, there is not a strong discernible effect of security status of a polling center on the change in violence experienced at polling centers before and during/after the election. Conditional on taking into account historical violence in a district (again, as the best measure officials had of

anticipated violence on election-day), within-district increases of deployments of police neither systematically increased nor decreased the level of election day violence. Given our identifying assumptions, this means that the first mechanism of police’s effect on violence is not likely to account for any changes in turnout. However, to assess further this mechanism as an explanation for observed effects on turnout, we test observable implications of the mechanism using our primary survey data in the next section.

5.2 Police’s Effect on Turnout

While we do not observe effects on violence, did added police affect voter turnout? To assess the effect of deployment of extra police on turnout at polling centers we estimate a series of regressions like:

$$T_{2010} = \alpha_1(M_i) + \alpha_2(H_i) + \beta_1(V_{t-1})_i + \beta_2(V_{t-2})_i + \beta_3(V_{t-3})_i + \beta_4(V_{t-4})_i + \gamma_1(\overline{Vpre}_i) + \gamma_2(\overline{Vpre}_i^2) + \gamma_3(\overline{Vpre}_i^3) + T_{2009} + d_i + \mu_i,$$

where raw turnout in 2010 is a function of the same covariates as those in the violence regressions, with the addition of turnout in 2009 as a control in some models.

Table 4A reports results using the binary measure of police, with the least secure polling centers receiving a value of ‘1’ (i.e., high or medium deployment) and the most secure category receiving a value of ‘0’ (i.e., low deployment). Table 4B reports results of models that include dummy variables for the medium and high security classifications (high/medium/low deployment), with the most secure classification (low deployment) as the omitted category.

In column 1, we report the most basic regression of turnout on the binary security deployment classification variable (Medium or High). Column 2 adds district fixed effects (also included in models 3-4, 6-8). Column 3 adds a control for predicted levels of violence at polling centers, generated from a linear regression of election-day violence on four (week) lags of violence, violence in the previous five months, and the squared and cubic levels of violence over that period. Column 4 includes controls for

previous levels of violence in the area. The models estimated in columns 5-8 are the same in specification as their respective counterparts in columns 1-4, except that they include a control for turnout in 2009.¹⁶

Both tables show an estimated negative effect of having a higher security classification (medium or high) on raw turnout. Across specifications (columns 1-4), a polling center that is designated to receive more police (i.e., medium or high) experiences a statistically and substantively significant decrease in turnout relative to the previous year's election, an estimated 30% change. This relationship holds when we control for 2009 turnout (columns 5-9).

Before summarizing our analysis of turnout, we briefly discuss the issue of electoral fraud (in the form of ballot stuffing to inflate vote totals). We noted above that our identification strategy is unaffected when including fraudulent polling centers in the analysis (SI Table 4). In terms of turnout, one could imagine police presence being either positively or negatively associated with fraudulent turnout, depending on whether police facilitated or hindered political operatives' altering of results.

SI Table 5 replicates models in Table 4 but include in the sample polling centers that were not open in 2009 (except in models controlling for 2009 turnout) and polling centers that were fraudulent in 2010. Those results suggest that the effect of police deployment on turnout in 2010 is limited to those polling centers that were also open in 2009 and are substantively stronger when we drop these polling centers from the analysis. This is consistent with the possibility that overt fraud is positively correlated with police presence. Panel B, Table 4 indicates that the effects on turnout are larger in Medium than

¹⁶ We do not use first differences of turnout across the two elections because such a specification is more restrictive than one that has outcomes in the period of interest as the dependent variable and includes previous levels as an additional control, because it allows for the coefficient to be different than -1.

High Security Deployment polling centers, which also is consistent with higher levels of police deployment correlating with greater electoral corruption (i.e., higher turnout), though we cannot be sure. Importantly, note that any fraud coordinated with security force deployments would lend a *positive* bias to estimates of the effect of deployments on turnout, so a negative effect should lend even more confidence in the causal nature of the estimated effects.

Taken together, these results are striking. We make use of the fact that previous levels of violence do not predict security classification of polling centers and that after controlling for previous levels of violence, the majority of variation in treatment assignment is left unexplained. This allows for the identification of the effect of extra police on turnout and violence. Extra police had an unintended and deleterious effect on turnout. We urge some caution interpreting the magnitude of these results. First, as noted, we are estimating an intent-to-treat effect since the actual election-day deployments were not verified. Second, we cannot relate the security classification designation of polling centers to exact levels of increased policing. But while the substantive importance of the effect is hard to pin down, the directionality is clear and striking.

6. EXPLORING THE MECHANISM

In interpreting these results, we compare them to the observable implications of the mechanisms discussed in relation to the theoretical literature. The evidence does not support arguments that increasing police deployment boosted turnout, either by increasing citizens' perceptions of the safety of the polling center or by motivating them to punish perceived corruption through turning out. We turn instead to the explanations that would expect a *decrease* in turnout.

6.1 Signaling/Targeting Results

The first of these explanations is that citizens associate police with a decrease in voter safety on election day: if security forces are attractive targets for insurgents' strategic use of violence, then citizens might

stay home because the increased presence of police around a polling center right before the election signals to citizens that they should expect heightened violence on election day. Given that the Taliban warned of violence on and around election days in an effort to deter voting (Adler, 2002; Farmer, 2010; NDI, 2011), this is possible. Coburn and Larson (2014, pp. 168-169) emphasize the heightened awareness of violence in Afghanistan in 2010. “More of the people we interviewed and spoke with in 2010 seemed to be taking the threat of violence seriously than they did in 2009”, and “it was more the threat of violence in the days leading up to voting [in 2010] that reshaped individual choices than the actual instances of violence that did eventually occur [on election day].”

To investigate this as a systematic explanation of behavior, we test an observable implication of the argument. If citizens were to report why they stayed home and did not vote, we should observe that around polling centers that were designated Medium or High Security Deployment, a greater percentage of citizens would cite fear of election-day violence as the reason they stayed home than citizens living around the Low Security Deployment polling centers.

To test this, we look at our surveys prior to, and after, the election. Those respondents who reported that they would not vote (baseline) or did not vote (endline) in the 2010 election were asked for a reason. We regress the number of respondents indicating “insecurity; I fear/feared attacks” on a binary variable indicating the polling center’s security classification (medium/high vs. low security). As in the other models, we include violence over the previous five months, as well as the squared and cubed terms, as regressors. Table 5 reports results.

Based on survey data, there is no evidence that people living around medium or high security-classified polling centers did not vote because they feared heightened violence there on election day. When queried after the election, being located near a medium/high security polling center is not associated with a higher likelihood of citing violence and insecurity as a reason for not voting (column 2). This is not to say that insecurity was meaningless in citizens’ decisions of whether to turn out: about 17% of citizens who did not vote cited this as the reason retrospectively (and 17% prospectively). But

there is no difference in frequency across polling centers designated for different security deployment levels, and there were many other reasons citizens gave for not voting, including a lack of interest, undesirable candidates, and not being eligible, among others.

We would want to know whether variation in security deployment levels across polling centers was associated with more pre-election citations of violence as a reason for not *planning* to vote. We test this in column 1 using our baseline survey data but note that the baseline survey was fielded in August, before the security force laydown began. Unfortunately, this means that when citizens responded in the baseline survey, they had not yet experienced variation in police deployment and so we cannot adequately test this implication of the mechanism in the pre-election phase.

Still, the results in column 1 should further assuage concerns about our identification strategy or spuriousness of the results. If there was an unobserved factor driving the assignment of security force deployment levels that was also correlated with citizens' anticipation of violence, we would expect to observe variation across polling center categories in the number of people citing fear of violence as a reason they did not plan to vote. But we do not. This lack of systematic evidence for a violence signaling mechanism is corroborated by assessments of this question from scholars who observed behavior first-hand and expected that anticipated violence would drive people away from the polls. While Coburn and Larson (2014, p. 169) point to the threat of violence as a factor in voting behavior, they also revealingly admit that in spite of this threat, "relatively few of the people we talked to appeared to actually decide not to vote based on this threat." While we cannot completely rule out this explanation, the evidence we have does not support it.

6.2 Trust/Legitimacy Results

Instead, the evidence is consistent with the explanation that voters stayed away from polling centers in 2010 with higher levels of police deployment in order to avoid interaction with the police, particularly on the way to the polls and in the vicinity of them, because every voter would have confronted multiple

police officers in the process of voting. To advance this argument, we explore variation in these attitudes in the population with survey data.

Responses from our pre-election survey taken between August/early September (before deployment of extra police) and our post-election survey in late November/early December show how police deployments affected variation in responses to questions designed to evaluate people's views of the police and the government more generally (Table 6). An observable implication of our argument is that negative views of the police should correlate with other proxies related to views of the government (as the police are the main representatives of the government visible at the elections): appropriate authority for dispute resolution, importance of paying taxes, and performance of the central government (columns 3-5). Presence of the police should be less likely to correlate with more general opinions about Afghanistan's regime type and satisfaction with Afghan democracy (columns 1-2).

Panel A regresses the pre-election (baseline) mean response value to these questions at the polling center-level on security classification, as well as previous levels of violence. Panel B provides regression results from post-election responses as the dependent variable. In Panel C, we take the difference between post-election and pre-election responses as the dependent variable and regress that change on security classification and previous violence.

Focusing on the differenced results in Panel C, attitudes about democracy do not change much with security status (columns 1-2).¹⁷ Substantial negative movements occur, however, in responses to specific questions that should be affected by interaction with corrupt police. First, the percentage of respondents who would trust either district officials or the police in a dispute decreases pre- to post-

¹⁷ "In your opinion, is Afghanistan a democracy?"; "Overall, how satisfied are you with the way democracy works?"

election in areas designated to receive more police (column 3).¹⁸ Second, respondents in areas receiving additional security forces become less likely to view paying taxes as important (column 4), though this is not a statistically strong result.¹⁹ Third, respondents' rating of how well the central government is doing its job decreases (column 5).²⁰ These results support the corruption mechanism linking security force deployment to reduced turnout, consistent with the theory that focuses on the police as a symbol of legitimate government and law and order.²¹

We argue that once citizens became aware of heightened police presence in their area leading up to the election and on election day, their prior (even election-week) experience with the police's corrupt and predatory behavior caused them to avoid polls altogether to avoid potentially injurious interaction with police. Our results in this section support the interpretation of the negative effect that police had on turnout in the 2010 election as a reaction against perceived corruption; this bears directly on the legitimacy of the Afghan state. The evidence supports two key arguments. First, descriptively, and like many other emerging democracies, many people view the ANP as corrupt, dishonest, and predatory.

¹⁸ "If you had a dispute with a neighbor, who would you trust to settle it?"

¹⁹ "How important is it to pay taxes?"

²⁰ "Does the central government do an [excellent/good/just fair/poor] job?"

²¹ Note that in Table 6, any natural bias resulting from imbalance across treatment categories prior to deployment of security forces (e.g., citizens around Medium/High polling centers are more likely to view Afghanistan as a democracy in our survey) would cause us to expect that bias to *increase* turnout around polling centers that were to receive more police, not decrease it. That is, the imbalance makes it more difficult to detect the size and sign of the treatment effect consistent with our argument linking exposure to corrupt police with lower turnout. The fact that we observe *lower* turnout in these polling centers is a reason not to worry about any imbalance in the survey data.

Second, exposure to the police negatively impacted Afghans' view of the police and government performance to provide two key public goods essential to state-building: justice and redistribution (Table 6). The deployment of police might therefore reduce citizen's expression of consent and desire to participate in practices that legitimize the government, like voting.

7. CONCLUSION

In this article, we investigate the importance of strong institutions for ensuring successful elections (Brancati & Snyder, 2011), but we also focus on the overlooked institution of the state's security forces. We propose a theoretical framework for understanding how efforts to increase state legitimacy through elections relate to efforts for improved security provision through policing as governments enact force laydowns to protect electoral institutions and voters. We highlight two separate mechanisms that potentially produce contrasting effects on turnout related to violence and voter safety, and perceptions of the police. This theoretical link should inform how we think about strengthening both of these institutions going forward in Afghanistan and in other conflict prone countries, particularly given our results and the public's perceptions of security forces. Our findings indicate that conditional on history of insurgent violence in the surrounding area, polling centers designated to receive higher levels of police deployment experienced roughly 30% less turnout on election day in the 2010 national election relative to polling centers in the same district designated to receive low levels of police deployment. The results are inconsistent with observable implications of a violence signaling mechanism driving voter behavior, but they are supportive of a trust/legitimacy mechanism, whereby citizens stay home in order to avoid interactions with corrupt security forces.

Our results are discouraging for the prospects of democratization and the legitimization of the Afghan state. Police deterred voter turnout and hurt key indicators of democracy, despite the government and international community's aim for the opposite. This is problematic for the development of participatory democracy, especially given that others have demonstrated the

painstakingly slow pace of political change in Afghanistan, even with sustained and concentrated programs designed to promote such attitudinal and behavioral transformation (Beath, Christia, & Enikolopov, 2013). The quantitative analysis presented here accords qualitatively with the concerns of election observers and the Afghan election commission about turnout and the implied lack of government support when participation declines (Aikins & Hewad, 2010; Maroney, 2010; A. J. Rubin & Gall, 2010).

Our contributions are several. First, we lend insights to a broad literature in economics on policing's effect on crime (Levitt & Miles, 2006) and the effects of deploying additional forces to reduce insurgent violence (Berman et al., 2013; Biddle et al., 2012). We add to these studies by focusing specifically on violence arising from insurgent activity and its effect on voting behavior, and we identify the competing mechanisms through which corrupt security forces could either increase or decrease turnout beyond their deployment that reflected previous patterns of violence. While scholars and policymakers intuit that security force deployment should boost turnout by deterring violence, in a critical setting with rich data allowing for a novel identification strategy, we show that in Afghanistan's election, extra policing did not substantively increase or decrease insurgent violence, but did negatively affect turnout through the perceived corruption of security forces. This provides an important caveat to the view that institutional strengthening among electoral and security sectors necessarily and simultaneously works in tandem. Whereas a government conducting elections requires a voting population secure enough to vote, underlying corruption and predation by some government agents may cause institutions to have counterproductive interactions and ultimately undermine democratic processes and outcomes.

Second, in laying out the possible mechanisms through which security deployment could affect electoral turnout, we demonstrate how insights from the literature on the violence-reducing effects of police and from research on how citizen interaction with police affects attitudes and behavior are illuminating for understanding how safeguarding elections affects democratic participation. We show

that regardless of the first order effects that policing has on violence, it can also have second order negative effects in contexts of corruption. In many countries, security forces are poorly trained and/or perceived as corrupt by the citizens whom they are supposed to protect (Olken & Pande, 2012). Since exposure to corruption delegitimizes the state in the eyes of citizens (Seligson, 2002), securing elections with corrupt police potentially discourages participation in elections, thereby subverting their core purpose of legitimizing the state. Moreover, by analyzing these dynamics in a conflict-ridden, emerging democracy, we broaden the utility of these literatures, which tend to focus on the developed country context.

Third, our results provide insights for ongoing debates among scholars and policymakers on the conditions under which elections should be held in conflict environments (Brancati & Snyder, 2011; Flores & Nooruddin, 2012). While early elections may increase the likelihood of violence (Brancati & Snyder, 2013; Höglund, Jarstad, & Kovacs, 2009), the international community consistently pushes countries emerging from or engaged in conflict to hold elections as a key benchmark for state-building (Bush, 2015; Hyde, 2011a), and donors provide substantial diplomatic and technical assistance to improve electoral processes (Hyde, 2011b; Kelley, 2012), including investments in security forces. But, rushing to implement elections in countries whose institutions are not equipped to handle a democratic transition may produce hybrid regimes (Fukuyama, 2014; Zakaria, 1997) and reignite violence (Mansfield & Snyder, 2005; Snyder, 2000). We do not resolve the question of electoral timing in fragile states, but our focus on the effects of security provision provides an important contribution by underscoring a condition under which elections are salutary: when the police are not corrupt. Thus, governments, activists, and the international community may want to first and more fully address corruption in the police before they plan for elections.

References

- Adler, E. (2002). Constructivism and International Relations. In W. Carlsnaes, Thomas Risse and Beth A. Simmons (Ed.), *Handbook of International Relations* (pp. 95-118). London: Sage Publications.
- Aikins, M., & Hewad, G. (2010, October 24). Losing Legitimacy after Afghanistan's Elections. *The AfPak Channel*. Retrieved from http://afpak.foreignpolicy.com/posts/2010/10/25/losing_legitimacy_after_afghanistans_elections
- Altonji, J. G., Elder, T. E., & Taber, C. R. (2005). Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. *Journal of Political Economy*, 113(1), 151-184.
- Author (2010, April 22). [Meeting with Afghan Ministry of Interior officials, Kabul, Afghanistan].
- Author (2014a, June 4). [Interview with Afghanistan Ministry of Interior official, Kabul, Afghanistan].
- Author (2014b, June 7). [Interview with former Afghanistan Independent Elections Commission official, Kabul, Afghanistan].
- Author (2014c, June 2). [Interview with former Afghanistan Ministry of Interior official, Kabul, Afghanistan].
- Bateson, R. (2012). Crime Victimization and Political Participation. *American Political Science Review*, 106(3), 570-587.
- Beath, A., Christia, F., & Enikolopov, R. (2013). Empowering Women through Development Aid: Evidence from a Field Experiment in Afghanistan. *American Political Science Review*, 107(3), 540-557.
- Becker, G. S. (1968). Crime and Punishment: An Economic Approach. *Journal of Political Economy*, 76(2), 169-217.
- Bellows, J., & Miguel, E. (2008). War and Institutions: New Evidence from Sierra Leone. *African Economic Development*, 96(2), 394-399.
- Berg, L.-A. (2014). From Weakness to Strength: The Political Roots of Security Sector Reform in Bosnia and Herzegovina. *International Peacekeeping*, 21(2), 149-164.
- Berman, E., Callen, M., Felter, J. H., & Shapiro, J. N. (2011). Do Working Men Rebel? Insurgency and Unemployment in Afghanistan, Iraq, and the Philippines. *Journal of Conflict Resolution*, 55(4), 496-528.
- Berman, E., Felter, J. H., Shapiro, J. N., & Troland, E. (2013). Modest, Secure and Informed: Successful Development in Conflict Zones. *American Economic Review: Papers and Proceedings*, 103(3), 512-517.
- Berrebi, C., & Klor, E. (2006). On Terrorism and Electoral Outcomes: Theory and Evidence from the Israeli-Palestinian Conflict. *Journal of Conflict Resolution*, 50(6), 899-925.
- Biddle, S., Friedman, J. A., & Shapiro, J. N. (2012). Testing the Surge: Why Did Violence Decline in Iraq in 2007? *International Security*, 37(Summer), 7-40.
- Binkerhoff, D. W. (Ed.) (2007). *Governance in Post-Conflict Societies*. London: Routledge.
- Blair, G., Imai, K., & Lyall, J. (2014). Comparing and Combining List and Endorsement Experiments: Evidence from Afghanistan. *American Journal of Political Science*, 58(4), 1043-1063.

- Blattman, C. (2009). From Violence to Voting: War and Political Participation in Uganda. *American Political Science Review*, 103(2), 231-247.
- Bowen, S. W., Jr. (2013). *Learning from Iraq: A final report from the Special Inspector General for Iraq Reconstruction*. Retrieved from http://pdf.usaid.gov/pdf_docs/pcaac502.pdf
- Brancati, D., & Snyder, J. L. (2011). Rushing to the Polls: The Causes of Premature Postconflict Elections. *Journal of Conflict Resolution*, 55(3), 469-492.
- Brancati, D., & Snyder, J. L. (2013). Time to Kill: The Impact of Election Timing on Postconflict Stability. *Journal of Conflict Resolution*, 57(5), 822-853.
- Bratton, M., Mattes, R., & Gyimah-Boadi, E. (2004). *Public Opinion, Democracy, and Market Reform in Africa*. New York: Cambridge University Press.
- Bush, S. S. (2015). *Taming of Democracy Assistance*. New York: Cambridge University Press.
- Callen, M., & Long, J. D. (2015). Institutional Corruption and Election Fraud: Evidence from a Field Experiment in Afghanistan. *American Economic Review*, 105(1), 354-381.
- Chandra, K. (2004). *Why Ethnic Parties Succeed: Patronage and Ethnic Head Counts in India*. Cambridge: Cambridge University Press.
- Chong, A., De La O, A. L., Karlan, D., & Wantchekon, L. (2015). Does Corruption Information Inspire the Fight or Quash the Hope? A Field Experiment in Mexico on Voter Turnout, Choice, and Party Identification. *Journal of Politics*, 77(1), 55-71.
- CNN. (2007). Bush will add more than 20,000 troops to Iraq.
- Coburn, N., & Larson, A. (2014). *Derailing Democracy in Afghanistan*. New York: Columbia University Press.
- Collier, P. (2009). *Wars, Guns, and Votes*. New York: Harper.
- Condra, L. N., Long, J. D., Shaver, A., & Wright, A. (2017). *The Logic of Insurgent Electoral Violence*. Working paper retrieved from <https://lukecondra.files.wordpress.com/2017/03/clsw-10mar2017.pdf>
- Condra, L. N., & Shapiro, J. N. (2012). Who Takes the Blame? The Strategic Effects of Collateral Damage. *American Journal of Political Science*, 56(1), 167-187.
- Cox, G. W. (1997). *Making Votes Count*. Cambridge: Cambridge University Press.
- DI. (2011). *Democracy International Election Observation Mission Afghanistan Parliamentary Elections 2010: Final Report*. Available from: http://democracyinternational.com/media/DI%20Afghanistan%202010%20EOM%20Final%20Report_web.pdf.
- Di Tella, R., & Schargrodsky, E. (2004). Do Police Reduce Crime? Estimates Using Allocation of Police Forces after a Terrorist Attack. *American Economic Review*, 94(1), 115-133.
- Doyle, M. W., & Sambanis, N. (2006). *Making War and Building Peace*. Princeton: Princeton University Press.
- Draca, M., Machin, S., & Witt, R. (2011). Panic on the Streets of London: Police, Crime, and the July 2005 Terror Attacks. *American Economic Review*, 101(5), 2157-2181.
- Dunning, T. (2012). *Natural Experiments in the Social Sciences*. New York: Cambridge University Press.
- Farmer, B. (2010, September 17). Afghan voters face blizzard of threats ahead of Saturday's parliamentary elections. *The Telegraph*. Retrieved from <http://www.telegraph.co.uk/news/worldnews/asia/afghanistan/8009529/Afghan-voters-face-blizzard-of-threats-ahead-of-Saturdays-parliamentary-elections.html>

- FEFA. (2011). *Afghanistan Parliamentary Election Observation Mission 2010*. Kabul, Afghanistan. Available from: http://fefa.org.site/images/Reports/Election_observation_report_2.zip.
- Felbab-Brown, V. (2013). *Aspiration and Ambivalence*. Washington, DC: Brookings Institution Press.
- Finan, F., & Ferraz, C. (2008). Exposing Corrupt Politicians: The Effect of Brazil's Publicly Released Audits on Electoral Outcomes. *Quarterly Journal of Economics*, 123(2), 703-745.
- Flores, T. E., & Nooruddin, I. (2012). The Effect of Elections on Postconflict Peace and Reconstruction. *Journal of Politics*, 74(2), 558-570.
- Fortna, V. P. (2008). *Does Peacekeeping Work? Shaping Belligerents' Choices after Civil War*. Princeton: Princeton University Press.
- Fried, B. J., Lagunes, P., & Venkataramani, A. (2010). Corruption and Inequality at the Crossroad: A Multimethod Study of Bribery and Discrimination in Latin America. *Latin American Research Review*, 45(1), 76-97.
- Fukuyama, F. (2004). *State-Building: Governance and World Order in the 21st Century*. Ithaca: Cornell University Press.
- Fukuyama, F. (2014). *Political Order and Political Decay: From the Industrial Revolution to the Globalization of Democracy*. New York: Farrar, Straus and Giroux.
- Giustozzi, A. (2008). *Koran, Kalashnikov, and Laptop*. New York: Columbia University Press.
- Giustozzi, A., & Isaqzadeh, M. (2012). *Policing Afghanistan*. New York: Columbia University Press.
- Gutiérrez-Romero, R. (2014). An Inquiry into the Use of Illegal Electoral Practices and Effects of Political Violence and Vote-buying. *Journal of Conflict Resolution*, 58(8), 1500-1527.
- Hafner-Burton, E. M., Hyde, S. D., & Jablonski, R. S. (2014). When Do Governments Resort to Election Violence? *British Journal of Political Science*, 44(1), 149-179.
- Hardoon, D., & Heinrich, F. (2013). *Global Corruption Barometer*. Available from: <http://www.transparency.org/gcb2013>.
- Höglund, K., Jarstad, A. K., & Kovacs, M. S. (2009). The Predicament of Elections in War-Torn Societies. *Democratization*, 16(3), 530-577.
- Horowitz, D. L. (1985). *Ethnic Groups in Conflict*. Berkeley: University of California Press.
- Hultman, L. (2007). Battle Losses and Rebel Violence: Raising the Costs for Fighting. *Terrorism and Political Violence*, 19(2), 205-222.
- Hyde, S. D. (2007). The Observer Effect in International Politics: Evidence from a Natural Experiment. *World Politics*, 60(1), 37-63.
- Hyde, S. D. (2011a). Catch Us If You Can: Election Monitoring and International Norm Diffusion. *American Journal of Political Science*, 55(2), 356-369.
- Hyde, S. D. (2011b). *The Pseudo-Democrat's Dilemma: Why Election Monitoring Became an International Norm*. Ithaca: Cornell University Press.
- Hyde, S. D., & Marinov, N. (2012). Which Elections Can Be Lost? *Political Analysis*, 20, 191-210.
- IDEA. (2016). *Voter Turnout Dataset*. Retrieved from: <http://www.idea.int/themes/voter-turnout>
- Jung, D., & Long, J. D. (2015). *Democracy, Insurgency, and Voter Turnout in Afghanistan*. University of Washington and Emory University. Working Paper retrieved from https://static1.squarespace.com/static/55af94b0e4b0cc8a396f8c40/t/56fbda84b09f95e5feb7d204/1459346053103/JL_Afghanistan.pdf

- Kasara, K., & Suryanarayan, P. (2015). When Do the Rich Vote Less Than the Poor and Why? Explaining Turnout Inequality across the World. *American Journal of Political Science*, 59(3), 613-627.
- Kelley, J. (2012). *Monitoring Democracy: When International Election Observation Works, and Why It Often Fails*. Princeton: Princeton University Press.
- Kydd, A. H., & Walter, B. F. (2006). The Strategies of Terrorism. *International Security*, 31(1), 49-80.
- Leach, M., & Kingsbury, D. (Eds.). (2013). *The Politics of Timor-Leste: Democratic Consolidation After Intervention*. Ithaca: Southeast Asia Program Publications.
- Lerman, A., & Weaver, V. M. (2014). *Arresting Citizenship: The Democratic Consequences of American Crime Control*. Chicago: University of Chicago Press.
- Levi, M. (1988). *Of Rule and Revenue*. Berkeley: University of California Press.
- Levitt, S. D. (1997). Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime. *American Economic Review*, 87(3), 270-290.
- Levitt, S. D., & Miles, T. J. (2006). Economic Contributions to the Understanding of Crime. *Annual Review Law and Social Science*, 2, 147-164.
- Mansfield, E. D., & Snyder, J. (2005). *Electing To Fight: Why Emerging Democracies Go To War*. Cambridge: MIT Press.
- Maroney, S. (2010, September 19). Observers Debate Legitimacy of Afghanistan Election. *Voice of America*. Retrieved from <http://www.voanews.com/english/news/Afghan-Observers-Question-Legitimacy-of-Election-103232149.html>
- Mashego, P., & Hartley, W. (2016, July 28). Available from: <http://www.bdlive.co.za/national/2016/07/28/police-look-to-prevent-violence-disrupting-elections>). Police look to prevent violence disrupting elections. *Business Day Live*.
- Nagin, D. S., & Telep, C. W. (2017). Procedural Justice and Legal Compliance. *Annual Review Law and Social Science*, 13(1).
- NDI. (2010). *The 2009 Presidential and Provincial Council Elections in Afghanistan*. Retrieved from Washington, DC: https://www.ndi.org/sites/default/files/Elections_in_Afghanistan_2009.pdf
- NDI. (2011). *The 2010 Wolesi Jirga Elections in Afghanistan*. Washington, DC. Available from: <https://www.ndi.org/files/Afghanistan-2010-election-observers-final-report.pdf>.
- Norris, P. (2014). *Why Electoral Integrity Matters*. New York: Cambridge University Press.
- Nunn, N., & Wantchekon, L. (2011). The Slave Trade and the Origins of Mistrust in Africa. *American Economic Review*, 101(7), 3221-3252.
- Olken, B. A., & Pande, R. (2012). Corruption in Developing Countries. *Annual Review of Economics*, 4, 479-509.
- Perito, R. M. (2009). *Afghanistan's Police: The Weak Link in Security Sector Reform* (227). Retrieved from Washington, D.C.: https://www.usip.org/sites/default/files/afghanistan_police.pdf
- Police gear up to prevent election violence. (2008, November 21). Available from: <http://www.irinnews.org/report/81613/ghana-police-gear-prevent-election-violence>). *IRIN News*.
- Posner, D. N. (2005). *Institutions and Ethnic Politics in Africa*. Cambridge: Cambridge University Press.
- Przeworski, A., Stokes, S. C., & Manin, B. (Eds.). (1999). *Democracy, Accountability, and Representation*. New York: Cambridge University Press.

- Rashid, A. (2008). *Descent into Chaos*. New York: Viking.
- Rubin, A. J., & Gall, C. (2010, September 24). Widespread Fraud Seen in Latest Afghan Elections. *New York Times*.
- Rubin, B. (2007). Saving Afghanistan. *Foreign Affairs*, 86(1), 57-78.
- Sedra, M. (2006). Security Sector Reform in Afghanistan: The Slide towards Expediency. *International Peacekeeping*, 13(1), 94-110.
- Seligson, M. A. (2002). The Impact of Corruption on Regime Legitimacy: A Comparative Study of Four Latin American Countries. *Journal of Politics*, 64(2), 408-433.
- Serchuk, V. (2006, July 17). Cop Out: Why Afghanistan Has No Police. *The Weekly Standard*.
- Shaver, A., & Wright, A. (2016). *Are Modern Insurgencies Predictable? New Evidence from Afghanistan and Iraq*. Retrieved from https://scholar.princeton.edu/sites/default/files/ashaver/files/aremoderninsurgenciespredictable_shaverwright.pdf
- SIGAR. (2017). *SIGAR Quarterly Report to the United States Congress*. Retrieved from Washington, D.C.: <https://www.sigar.mil/pdf/quarterlyreports/2017-04-30qr.pdf>
- Snyder, J. (2000). *From Voting to Violence*. New York: W.W. Norton.
- Soss, J., & Weaver, V. (2017). Police Are Our Government: Politics, Political Science, and the Policing of Race–Class Subjugated Communities. *Annual Review of Political Science*, 20, 565-591.
- Stedman, S. J. (1997). Spoiler Problems in Peace Processes. *International Security*, 22(2), 5-53.
- Stockemer, D., LaMontagne, B., & Scruggs, L. (2013). Bribes and Ballots: The Impact of Corruption on Voter Turnout in Democracies. *International Political Science Review*, 34(1), 74-90.
- Sunshine, J., & Tyler, T. R. (2003). The Role of Procedural Justice and Legitimacy in Shaping Public Support for Policing. *Law & Society Review*, 37(3), 513-548.
- Torabi, Y., & Delesgues, L. (2007). *Afghan Perceptions of Corruption: A Survey across Thirteen Provinces*. http://www.iwaweb.org/docs/reports/psm/afghan_perception_of_corruption_2006.pdf
- Tyler, T. R. (2004). Enhancing Police Legitimacy. *The Annals of the American Academy of Political and Social Science*, 593(84-99).
- Tyler, T. R. (2005). Policing in Black and White: Ethnic Group Differences in Trust and Confidence in the Police. *Police Quarterly*, 8(3), 322-342.
- Tyler, T. R. (2006). Psychological Perspectives on Legitimacy and Legitimation. *Annual Review of Psychology*, 57, 375-400.
- Tyler, T. R., & Fagan, J. A. (2009). Legitimacy and Cooperation: Why Do People Help the Police Fight Crime in Their Communities? In S. Rice & M. White (Eds.), *Race, Ethnicity, and Policing: New and Essential Readings* (pp. 84-117). New York: NYU Press.
- UNODC. (2010). *Corruption in Afghanistan: Bribery as Reported by the Victims*. <https://www.unodc.org/documents/data-and-analysis/Afghanistan/Afghanistan-corruption-survey2010-Eng.pdf>
- Wilkinson, S. I. (2004). *Votes and Violence*. New York: Cambridge University Press.
- Zakaria, F. (1997). The Rise of Illiberal Democracy. *Foreign Affairs*, 76(6), 22-43.

Table 1. Summary Statistics of Afghans' Perceptions of Police

VARIABLES	Observations	Mean (% agree)	Std. Dev.
<i>Panel A: Kandahar survey (August-October 2010)</i>			
ANP officers in my area are illiterate	369	0.724	0.448
ANP officers treat members of the local community with respect	369	0.274	0.446
ANP officers are well respected by local people	369	0.263	0.441
ANP officers in my area sometimes beat people up	369	0.623	0.485
Most ANP officers are corrupt	369	0.637	0.482
ANP officers put the interests of their community before their own interests	369	0.298	0.458
<i>Panel B: ANQAR survey (March and May/June 2010)</i>			
Seen/experienced the police engage in corrupt acts (Pashtun respondents)	10,507	0.322	0.467
Seen/experienced the police engage in corrupt acts (Tajik respondents)	5,039	0.172	0.378
Seen/experienced the police engage in corrupt acts (Uzbek respondents)	1,204	0.0880	0.283

Table 2. Treatment Assignment and Violence

VARIABLES	(1) Recent violence and cubic polynomial in past violence nearby	(2) Adding Province FE	(3) Adding District FE	(4) Dropping last 4- weeks of violence District FE
SIGACTs (1-week lag)	0.031 (0.072)	0.058 (0.074)	0.067 (0.089)	
SIGACTs (2-week lag)	0.039 (0.065)	-0.001 (0.067)	-0.004 (0.086)	
SIGACTs (3-week lag)	-0.063 (0.058)	-0.080 (0.062)	-0.007 (0.071)	
SIGACTs (4-week lag)	0.137** (0.059)	0.111* (0.061)	-0.169* (0.086)	
Total violence previous 5 months	0.128 (0.275)	-0.019 (0.323)	-0.558* (0.335)	-0.626*** (0.239)
Total violence squared	-0.203 (0.155)	-0.128 (0.181)	0.267 (0.183)	0.215 (0.146)
Total violence cubed	0.018 (0.019)	0.013 (0.022)	-0.019 (0.023)	-0.021 (0.019)
Constant	1.308*** (0.029)	1.316*** (0.027)	1.333*** (0.007)	1.331*** (0.007)
Observations	1823	1823	1823	1823
R-squared	0.004	0.068	0.316	0.314

Notes: Dependent variable is polling center security deployment category (1=Low, 2=Medium, 3=High). Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Police's Effect on Violence (Timing Change at Election)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Election week vs. week before	Election week vs. week before	Election week vs. 4-week pre-election average	Election week vs. 4-week pre-election average	4-week average post vs. 4-week average before	4-week average post vs. 4-week average before	2-month average post vs. 2-month average before	2-month average post vs. 2-month average before
Medium or High Security Deployment	-0.009 (0.035)		0.019 (0.037)		0.006 (0.012)		0.014 (0.013)	
Medium Security Deployment		0.006 (0.056)		0.050 (0.057)		0.029 (0.025)		0.021 (0.019)
High Security		-0.021 (0.040)		-0.008 (0.041)		-0.013 (0.013)		0.009 (0.012)
SIGACTs (2-week lag)	0.045 (0.184)	0.046 (0.185)						
SIGACTs (3-week lag)	-0.047 (0.130)	-0.046 (0.131)						
SIGACTs (4-week lag)	-0.063 (0.130)	-0.062 (0.131)						
SIGACTs (5-week lag)	0.343 (0.209)	0.343 (0.210)	0.251 (0.195)	0.250 (0.195)	-0.045 (0.074)	-0.046 (0.074)		
SIGACTs (6-week lag)			-0.035 (0.208)	-0.034 (0.208)	-0.060 (0.052)	-0.059 (0.053)		
SIGACTs (7-week lag)			0.131 (0.158)	0.132 (0.158)	-0.034 (0.069)	-0.033 (0.069)		
SIGACTs (8-week lag)			-0.156 (0.176)	-0.156 (0.177)	-0.079 (0.062)	-0.079 (0.062)		
Total violence previous 5 months	0.538 (0.678)	0.534 (0.680)	0.646 (0.601)	0.642 (0.605)	-0.365 (0.248)	-0.367 (0.251)	-0.639*** (0.197)	-0.640*** (0.197)
Total violence squared	0.252 (0.492)	0.254 (0.492)	0.028 (0.392)	0.029 (0.392)	0.229* (0.133)	0.230* (0.132)	-0.065 (0.118)	-0.065 (0.118)
Total violence cubed	-0.070 (0.065)	-0.070 (0.065)	-0.054 (0.052)	-0.054 (0.052)	-0.041** (0.017)	-0.041** (0.017)	0.008 (0.016)	0.008 (0.016)
Constant	0.062*** (0.017)	0.062*** (0.017)	0.050*** (0.018)	0.050*** (0.018)	-0.005 (0.006)	-0.005 (0.007)	0.006 (0.008)	0.006 (0.008)
Observations	1823	1823	1823	1823	1823	1823	1823	1823
R-squared	0.503	0.503	0.510	0.510	0.645	0.646	0.790	0.790

Notes: All regressions include district fixed effects. Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Table 4A. Effect of Police Deployment Classification on Polling Center-level Turnout in 2010 Election

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medium/High Security	-51.52***	-33.36***	-33.59***	-33.85***	-54.34***	-29.84***	-30.00***	-30.36***
Deployment	(14.82)	(11.93)	(12.03)	(12.15)	(13.09)	(10.79)	(10.91)	(11.05)
SIGACTs Prediction			-2.70				-1.95	
			(13.64)				(14.58)	
SIGACTs (1-week lag)				-9.18				-2.74
				(17.74)				(19.80)
SIGACTs (2-week lag)				4.10				-8.61
				(16.67)				(17.18)
SIGACTs (3-week lag)				-9.50				-4.47
				(19.50)				(18.07)
SIGACTs (4-week lag)				-12.99				-10.77
				(22.96)				(24.92)
Total violence previous 5 months				28.27				7.80
				(83.18)				(74.88)
Total violence squared				9.81				23.91
				(51.97)				(47.89)
Total violence cubed				-2.52				-3.931
				(6.08)				(5.39)
Turnout in 2009					0.40***	0.36***	0.36***	0.36***
					(0.05)	(0.05)	(0.05)	(0.05)
Constant	339.12***	335.38***	335.85***	336.21***	243.74***	248.09***	248.44***	249.35***
	(9.11)	(2.45)	(3.48)	(4.01)	(14.15)	(13.00)	(12.70)	(12.56)
District FE	N	Y	Y	Y	N	Y	Y	Y
N	1823	1823	1823	1823	1823	1823	1823	1823
R2	0.02	0.43	0.43	0.43	0.14	0.49	0.49	0.49

Notes: Robust standard errors clustered on district in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4B. Effect of Police Deployment Classification on Polling Center-level Turnout in 2010 Election, Controlling for Turnout in 2009 Election

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medium Security Deployment	-91.48*** (18.65)	-45.28*** (16.96)	-45.64*** (16.84)	-46.26*** (16.99)	-91.03*** (16.82)	-39.88** (16.72)	-40.15** (16.58)	-41.30** (16.68)
High Security Deployment	-14.24 (18.27)	-23.19 (14.23)	-23.37 (14.38)	-23.35 (14.48)	-20.07 (16.08)	-21.26* (12.80)	-21.40 (13.01)	-21.10 (13.27)
SIGACTs Prediction			-3.15 (13.68)				-2.32 (14.55)	
SIGACTs (1-week lag)				-9.00 (17.76)				-2.59 (19.76)
SIGACTs (2-week lag)				2.64 (16.58)				-9.88 (17.02)
SIGACTs (3-week lag)				-9.94 (19.60)				-4.86 (18.15)
SIGACTs (4-week lag)				-13.49 (23.25)				-11.22 (25.08)
Total violence previous 5 months				31.26 (85.45)				10.46 (76.83)
Total violence squared				9.03 (52.69)				23.22 (48.54)
Total violence cubed				-2.40 (6.15)				-3.82 (5.44)
Turnout in 2009					0.40*** (0.05)	0.36*** (0.05)	0.36*** (0.05)	0.36*** (0.05)
Constant	339.12*** (9.12)	335.48*** (2.45)	336.03*** (3.47)	336.35*** (4.01)	244.40*** (14.06)	248.29*** (13.00)	248.71*** (12.70)	249.58*** (12.55)
District FE	N	Y	Y	Y	N	Y	Y	Y
N	1823	1823	1823	1823	1823	1823	1823	1823
R2	0.027	0.429	0.429	0.432	0.148	0.49	0.49	0.491

Notes: Robust standard errors clustered on district in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Violence as Reason for Not Voting

VARIABLES	(1) Insecurity/Fear of Attacks (Baseline)	(2) Insecurity/Fear of Attacks (Endline)
Medium or High Security Deployment	0.022 (0.097)	-0.200 (0.129)
Total violence previous 5 months	-1.426 (3.043)	-0.251 (1.206)
Total violence squared	-3.273 (18.071)	-2.748 (7.495)
Total violence cubed	25.271 (28.089)	11.572 (11.724)
Constant	0.120*** (0.039)	0.220*** (0.014)
Observations	130	130
R-Squared	0.208	0.278

Notes: All regressions include district fixed effects. Robust standard errors are clustered at the district level.
 *** p<0.01, ** p<0.05, * p<0.1

Table 6. Attitudes vis-à-vis the Police

<i>Panel A: Pre-Election Responses</i>					
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Is Afghanistan a democracy?	Satisfaction with Afghan democracy	Will you use Courts or Police to Solve Dispute?	How Important Is It to Pay Your Taxes?	Is the central government doing a good job?
Medium or High Security Deployment	0.184*** (0.049)	-0.186 (0.129)	0.134 (0.082)	0.019 (0.039)	0.129 (0.156)
Total violence previous 5 months	-0.822 (2.337)	0.089 (0.994)	-1.233 (1.913)	-1.030 (1.216)	-2.080 (1.693)
Total violence squared	-3.646 (15.833)	-4.269 (7.059)	13.316 (14.321)	4.515 (11.766)	11.745 (16.417)
Total violence cubed	18.254 (26.216)	10.968 (11.847)	-24.238 (25.941)	-10.116 (23.061)	-17.586 (32.188)
Constant	0.679*** (0.024)	0.872*** (0.010)	0.273*** (0.024)	0.515*** (0.012)	0.569*** (0.023)
Observations	130	121	130	130	130
R-squared	0.217	0.223	0.189	0.372	0.318
<i>Panel B: Post-Election Responses</i>					
Medium or High Security Deployment	0.104 (0.123)	0.082 (0.174)	-0.027 (0.047)	-0.126* (0.062)	-0.260*** (0.024)
Total violence previous 5 months	1.641** (0.629)	-0.476 (0.999)	2.524** (0.928)	1.171 (1.101)	1.843** (0.620)
Total violence squared	-10.231* (5.558)	0.423 (6.848)	-19.786*** (6.113)	-8.173 (9.443)	-18.565** (7.386)
Total violence cubed	10.825 (10.856)	-2.953 (11.408)	32.101*** (9.831)	16.546 (17.455)	37.047** (15.746)
Constant	0.682*** (0.008)	0.798*** (0.011)	0.198*** (0.010)	0.440*** (0.009)	0.449*** (0.013)
Observations	130	128	130	130	130
R-squared	0.431	0.243	0.343	0.188	0.320

Panel C: Change in Responses

Medium or High Security Deployment	-0.079 (0.158)	0.274 (0.310)	-0.162*** (0.052)	-0.145 (0.100)	-0.389** (0.176)
Total violence previous 5 months	2.463 (2.516)	-0.491 (1.837)	3.757 (2.136)	2.201 (1.748)	3.923* (2.140)
Total violence squared	-6.586 (18.402)	1.421 (12.717)	-33.102** (13.516)	-12.688 (11.631)	-30.310* (15.929)
Total violence cubed	-7.429 (31.589)	-6.253 (21.035)	56.338** (23.072)	26.662 (19.251)	54.633* (27.551)
Constant	0.003 (0.022)	-0.083*** (0.018)	-0.074** (0.030)	-0.076*** (0.019)	-0.119*** (0.021)
Observations	130	120	130	130	130
R-squared	0.214	0.122	0.372	0.251	0.344

Notes: All regressions include district fixed effects. Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

Damaging Democracy? Security Provision and Turnout in Afghan Elections

Supporting Information

This Supporting Information (SI) provides robustness checks on our main results, as well as tests of alternate explanations. While we have shown that the assignment of polling centers to categories of police was not a function of previous levels of violence and have argued for plausibly exogenous variation in treatment assignment, we further test that claim in two principal ways.

Sensitivity of Treatment Effects to the Inclusion of Observed Controls

First, we test whether our results are robust to the inclusion of other, observed controls in our regression specifications. We consider a set of four principal factors that might correlate with police deployment levels *and* with turnout, such that estimated effects of police on turnout and violence around election day that we report would reflect those factors and not the impact of police deployments. Our estimates remain remarkably robust to the inclusion of variables capturing these potential confounds, consistent with our interpretation of the estimates.

The first is the competitiveness of the election in 2009. It is not immediately clear how to sign the bias in this case with respect to the theoretical effect that competitiveness in the previous election should have on both turnout and police levels in the next election. Its effect on turnout seems likely to be positive, based on the reasoning that one's vote is more likely to be consequential in a competitive area than in one where a candidate won handily last time. The effect on police deployment is ambiguous.

Second, the strategic deployment of police could have been designed to help the political fortunes of President Karzai and politicians close to him. In the main text we note that results on turnout were consistent with the possibility that police were positively associated with electoral fraud (Panel B of Table 4; SI Tables 5A-B). Here, we conduct a different test of a threat to inference that could emanate from political use of police. In their study of fraud in the 2010 Afghan election, Callen and Long (2015) use measures of political connectedness of candidates running in the 2010 election. They explain how candidates' connections to Provincial or District Election Officials might affect how much fraud is associated with the electoral returns reported for a polling center, particularly fraud that is perpetrated at the Provincial Aggregation Center and levels above the polling center itself. If this connectedness affected police deployment and turnout, we should expect the bias to be in the positive

direction. Given that these are factors operating at the district- or province-level (Rundlett and Svulik (2016) note that fraud often is perpetrated at the local –rather than central – level of political organization), we control for them through the inclusion of district- and province-fixed effects in our model specifications.

Third, ethnicity could play a role in levels of turnout and police deployment. We might expect more police to be allocated to areas where more violence is expected (e.g., heavily Pashtun areas where the Taliban was more active) and higher turnout in areas where voters are more supportive of non-Taliban rule of the state (e.g., non-Pashtun areas). We do not have data that would allow for us to control for this at the polling center level, given the lack of a recent publicly available census. However, numerous studies note that ethnic groups in Afghanistan are geographically clustered, such that there is ethnic homogeneity concentrated at a local level. The geographic clustering of ethnic groups by district to the extent that there is a low level of heterogeneity in ethnic mixes of the population across districts, the inclusion of district fixed effects controls for this factor.

Note that in the cases above where the direction of the bias is not ambiguous, the sign is hypothesized to be positive and thus, presents less of a problem for our results. If the hypothesized effect of the factor on police deployment is positive, then this constitutes a ‘hard’ test when we examine the effect of police deployment on violence and find a weak negative effect. SI Table 2 summarizes these factors, their expected correlations with the treatment and outcome variables, and the resulting expected bias.

To test the plausibility of these alternate explanations, we use data on the 2010 election to replicate our main results from regressing police deployment levels on turnout (Tables 4A-B) and include these other factors individually as controls in regression specifications. These results are shown in SI Table 3. Following our main specifications, in both Panels A and B, the dependent variable is the turnout in the 2010 election, but in Panel B, we include turnout in the 2009 election as a control variable, as in the main analysis.

There is little evidence to support that these set of potentially confounding factors affect our results: the core negative effect of police on turnout is consistently strong in most specifications. Including district or province fixed effects (columns 1 and 4) – to control for connections of election officials to President Karzai – does not noticeably change the estimated coefficient on the security classification variable.

We also include dummy variables coding the majority ethnicity of the district in which the polling center is located. In column 2, we include a dummy for whether the district is Pashtun-majority or not. In column 3, we include dummies for the other main ethnic groups, as well. (Province fixed effects are

included in each model.) Controlling for ethnicity in this way does not alter our estimates of the police's effect on turnout.

In column 5, we include a measure of the competitiveness of each polling center in the 2009 election, the log of the difference between President Karzai's vote share and challenger Abdullah Abdullah's vote share. Including this measure slightly reduces the size of the estimated effect of police on turnout.

SI Tables 4-5 replicate results from Tables 2 and 4, respectively, but include polling centers that would be deemed fraudulent by our definition in 2010, as well as polling centers open in 2010 but not open in 2009. Results are largely unaffected.

Finally, we provide further evidence that supports our identification assumption that after controlling for a polling center's history of violence, we can treat the assignment of police as essentially random. Above, we tested several threats to this assumption by identifying possible ways in which police might be assigned through mechanisms other than history of violence, which was stated as determinant by officials responsible for developing the plan. Another concern with our identification strategy might be that within districts, polling centers that are geographically adjacent may differ not only in their security classification, but also might differ systematically in ways that correlate with the assignment strategy. For example, the population of people living within polling center catchment areas could differ by living standards or socio-demographic characteristics that correlate with having extra policing (and the degree to which our survey was limited to more urban places would increase the likelihood of finding such an effect).

To address this concern, SI Table 6 shows within-district, across-polling center correlation (one-way analysis of variance) on individuals' socio-demographic characteristics and perceptions of government, as recorded in our survey of voters across 471 polling centers discussed in the main text. We lack such data for the entire sample of polling stations in our analysis, given the limited sample of the survey within 19 provincial capitals.

We expect that polling center catchment areas will be similar across centers within a district on living standard and socio-demographic characteristics, and we note our expected degree of correlation in Column 2. Panel A confirms this general expectation. Access to consistent electricity has a high degree of correlation at the local level, as do the proportion of respondents of the same ethnicity (Pashtun, Tajik, and Uzbek). Therefore, accounting for across-district variation in these measures (i.e., via district fixed effects), polling centers within districts are highly similar across living standard and socio-demographic features and would therefore not explain or confound our results.

Next, as a robustness check on the correlation of survey responses generally, Panel B includes the intra-class correlation of responses on questions that we *would not* expect to correlate highly within polling centers within districts, accounting for across-district variation, with expectations listed in Column 2. These include three questions on national issues, including ratings of President Karzai’s performance in office, respondents’ beliefs about whether or not the central government will maintain control in Afghanistan, and performance ratings of central government services. As our results show, these national-level issues do not correlate strongly at the local level, demonstrating i) that they are therefore likely orthogonal to the security assignment per our identification strategy and ii) that there is not a latent or spurious factor driving all survey responses to correlate highly that an analysis of intra-class correlation would miss.

Taken together, these results strengthen confidence in our identification strategy and that accounting for across-district heterogeneity, there is not significant within-district, across-polling center heterogeneity that does not correlate with local living standard and socio-demographic characteristics but does strongly correlate with the assignment strategy of policing.

Estimating Bias from Unobservables

While we have shown the robustness of our results to the inclusion of other potential observable confounds in the models, one might still worry that a small amount of selection on unobservable factors could explain the effects. To further increase confidence in the causal nature of our results, we pursue a second approach, estimating how large any bias arising from selection on unobservable factors would have to be to render the true effect of the treatment (police deployment classification) to be zero. Altonji, Elder, and Taber (2005) develop an approach that uses the degree of selection on observables to quantitatively assess the degree of omitted variable bias.¹ We follow Nunn and Wantchekon (2011, pp. 3237-3238) in implementing this approach, comparing estimates of the treatment effect ($\hat{\beta}$) from two regressions: one with a full set of controls ($\hat{\beta}^F$) and one with a restricted set of controls ($\hat{\beta}^R$). The ratio of these estimates ($\hat{\beta}^F / (\hat{\beta}^R - \hat{\beta}^F)$) is increasing in the size of the effect of unobservables that would be necessary to explain away the treatment effect.

SI Table 7 provides results of this approach for two sets of equations previously estimated. In row 1, the “restricted” estimating equation regresses 2010 turnout on measures of previous violence and

¹ A key assumption of this approach is that the part of the outcome related to the set of observables in the model has the same relationship with the treatment variable as the part of the outcome that is related to unobservables (Altonji et al., 2005, p. 154).

district fixed effects (SI Table 3A, column 4). The “full” estimating equation adds 2009 turnout as a covariate (SI Table 3B, column 4). The calculated ratio is 8.70, which means that to attribute the entire estimated treatment effect to selection effects, selection on unobservables would have to be almost 9 times greater than selection on 2009 turnout, which seems unreasonable. Row 2 compares a “restricted” model that controls for previous violence, ethnicity dummies, 2009 turnout and province fixed effects to a “full” model that controls for previous violence, 2009 turnout, and district fixed effects. The ratio comparing estimated treatment effects here is 3.72, indicating that selection on unobservables would have to be almost 4 times as large as selection on observables (moving to a model with district fixed effects) to account for the effect. Again, this seems unreasonably large.

SI Table 1. Descriptive Statistics

Variables	Observations	Mean	Std. Dev.
SIGACTs (1-week lag)	72,920	0.0470	0.344
SIGACTs (2-week lag)	72,920	0.0468	0.342
SIGACTs (3-week lag)	72,920	0.0465	0.341
SIGACTs (4-week lag)	72,920	0.0461	0.341
SIGACTs (5-week lag)	72,920	0.0455	0.338
SIGACTs (6-week lag)	72,920	0.0447	0.336
SIGACTs (7-week lag)	72,920	0.0440	0.334
SIGACTs (8-week lag)	72,920	0.0435	0.332
Raw Turnout (2010)			
Does the Government do a Good Job with Resources? (Yes/No) (pre)	130	0.543	0.308
Does the Government do a Good Job with Resources? (Yes/No) (post)	130	0.451	0.288
Satisfaction with Afghan Democracy? (5-pt scale) (pre)	121	0.863	0.188
Satisfaction with Afghan Democracy? (5-pt scale) (post)	128	0.779	0.232
Is Afghanistan a Democracy? (pre)	130	0.657	0.287
Is Afghanistan a Democracy? (post)	130	0.694	0.244
Will you use Courts or Police to Solve a Dispute? (Yes/No) (pre)	130	0.278	0.234
Will you use Courts or Police to Solve a Dispute? (Yes/No) (post)	130	0.212	0.210
How Important is it to Pay Your Taxes? (Very/Not) (pre)	130	0.491	0.305
How Important is it to Pay Your Taxes? (Very/Not) (post)	130	0.454	0.265

SI Table 2. Expected Effect of Other Factors on Police and Turnout

<i>Factor</i>	<i>Expected Effect on Police</i>	<i>Expected Effect on Turnout</i>
Competitiveness of the 2009 election	?	(+) Voters turn out if their vote will be consequential.
President Karzai's interests	?	?
Ethnic politics	(+) Pashtun areas expected to be more violence and could receive more police.	(+) Non-Pashtun areas supportive of non-Taliban rule and more likely to turn out.

SI Table 3: Addressing Potential Confounders

Panel A: 2010 Turnout

Variables	(1) Province FE	(2) Pashtun Dummy (Province FE)	(3) Ethnicity Dummies (Province FE)	(4) District FE	(5) Competitiveness (District FE)
Medium or High Security Deployment	-37.7*** (13.0)	-37.6*** (13.0)	-38.9*** (13.9)	-33.9*** (12.1)	-25.6** (12.0)
Ismaili			-33.7 (25.0)		
Mixed			-65.1 (43.1)		
Nuristani			21.9 (80.2)		
Pashai			-69.3 (58.5)		
Pashtun			-74.8 (54.3)		
Tajik			-236.6*** (62.8)		
Waziri			-97.3 (60.5)		
SIGACTs (1-week lag)	-20.7 (19.8)	-20.5 (19.7)	-11.2 (18.6)	-9.2 (17.7)	-0.5 (18.8)
SIGACTs (2-week lag)	18.0 (20.1)	18.2 (20.1)	11.7 (18.4)	4.1 (16.7)	-4.0 (17.1)
SIGACTs (3-week lag)	-12.6 (19.3)	-12.6 (19.3)	-5.0 (18.6)	-9.5 (19.5)	-11.4 (18.3)
SIGACTs (4-week lag)	14.5 (21.2)	15.1 (20.8)	7.6 (19.8)	-13.0 (23.0)	-16.2 (24.2)
Total violence previous 5 months	-162.3* (92.5)	-163.5* (93.2)	-127.7 (89.3)	28.3 (83.2)	37.0 (82.2)
Total violence squared	103.1* (59.8)	103.2* (59.8)	84.8 (57.3)	9.8 (52.0)	3.8 (50.7)
Total violence cubed	-14.4* (7.3)	-14.4* (7.4)	-12.0* (7.0)	-2.5 (6.1)	-1.3 (5.8)
Pashtun Majority District		-7.7 (24.1)			

Log(|Karzai VS - Dr. Abdullah VS|)

Constant	340.7*** (7.1)	343.6*** (12.4)	415.2*** (44.4)	336.2*** (4.0)	-23.2*** 203.3*** (25.3)
N	1823	1823	1823	1823	1817
R-squared	0.210	0.210	0.240	0.431	0.458

Panel B: 2010 Turnout (Controlling for 2009 Turnout)

Medium or High Security Deployment	-38.3*** (11.9)	-38.1*** (11.9)	-38.5*** (12.7)	-30.4*** (11.1)	-27.2** (11.2)
Ismaili			-48.7** (21.6)		
Mixed			-58.2 (40.2)		
Nuristani			-12 (77.1)		
Pashai			-81 (56.1)		
Pashtun			-77.2 (52.4)		
Tajik			-177.8*** (62)		
Waziri			-104.7* (57.4)		
SIGACTs (1-week lag)	-8.1 (19.1)	-7.9 (19)	-2 (18.4)	-2.7 (19.8)	-0.8 (20.3)
SIGACTs (2-week lag)	5.2 (18.9)	5.4 (18.8)	1.4 (17.6)	-8.6 (17.2)	-11.9 (17)
SIGACTs (3-week lag)	-1.2 (16)	-1.1 (15.9)	2.6 (15.8)	-4.5 (18.1)	-4.9 (18)
SIGACTs (4-week lag)	14.9 (20.5)	15.7 (20.5)	10.8 (19.9)	-10.8 (24.9)	-13.8 (25.6)
Total violence previous 5 months	-163.8** (82.8)	-165.3** (83.7)	-136.4* (79.7)	7.8 (74.9)	12.3 (75)
Total violence squared	111.7** (51.9)	111.8** (52)	96.5* (49.7)	23.9 (47.9)	21.8 (47.9)
Total violence cubed	-15.6** (6.5)	-15.6** (6.5)	-13.6** (6.1)	-3.9 (5.4)	-3.4 (5.4)
Pashtun Majority District		-10			

		(20.1)			
Log(Karzai VS - Dr. Abdullah VS)					-6.8*
					(3.9)
Turnout 2009	0.4***	0.4***	0.4***	0.4***	0.3***
	(0)	(0)	(0)	(0.1)	(0.1)
Constant	248.7***	252.5***	324.4***	249.4***	216.2***
	(12.8)	(16)	(43.6)	(12.6)	(24.3)
N	1823	1823	1823	1823	1817
R-squared	0.3	0.301	0.317	0.491	0.496

Notes: In Column 3 (Ethnicity dummies), Hazara is the reference category. Robust standard errors are clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1

SI Table 4. Treatment Assignment and Violence

VARIABLES	(1) Recent violence and cubic polynomial in past violence nearby	(2) Adding Province FE	(3) Adding District FE	(4) Dropping last 4- weeks of violence District FE
SIGACTs (1-week lag)	-0.045 (0.075)	-0.019 (0.075)	0.014 (0.083)	
SIGACTs (2-week lag)	0.018 (0.06)	-0.047 (0.059)	-0.03 (0.068)	
SIGACTs (3-week lag)	0.015 (0.071)	0.004 (0.066)	0.064 (0.053)	
SIGACTs (4-week lag)	0.064 (0.053)	0.049 (0.051)	-0.071 (0.065)	
Total violence previous 5 months	0.274 (0.306)	0.13 (0.319)	-0.571 * (0.311)	-0.478 (0.3)
Total violence squared	-0.205 (0.169)	-0.124 (0.167)	0.223 (0.152)	0.165 (0.157)
Total violence cubed	0.019 (0.02)	0.013 (0.02)	-0.02 (0.017)	-0.016 (0.018)
Constant	1.363 *** (0.032)	1.371 *** (0.027)	1.388 *** (0.008)	1.387 *** (0.009)
N	2290	2290	2290	2290
R-squared	0.003	0.104	0.335	0.333

Notes: Unlike Table 2, these models include polling centers whose turnout results in 2010 indicate fraud, as well as those polling centers not open in 2009. Dependent variable is polling center security deployment category (1=Low, 2=Medium, 3=High). Robust standard errors are clustered at the district level.

*** p<0.01, ** p<0.05, * p<0.1

SI Table 5A. Effect of Police Deployment Classification on Polling Center-level Turnout in 2010 Election

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medium/High Security	2.196	-6.806	-33.592***	-7.205	-34.110**	-19.069*	-30.000***	-19.973*
Deployment	(17.473)	(11.744)	(12.03)	(11.89)	(13.846)	(11.199)	(10.906)	(11.537)
SIGACTs Prediction			-2.701				-1.946	
			(13.641)				(14.583)	
SIGACTs (1-week lag)				-1.929				7.527
				(13.839)				(17.795)
SIGACTs (2-week lag)				-3.327				-7.985
				(14.811)				(15.924)
SIGACTs (3-week lag)				8.166				-3.208
				(10.32)				(17.943)
SIGACTs (4-week lag)				-2.499				-16.647
				(16.468)				(18.136)
Total violence previous 5 months				-56.852				19.884
				(68.807)				(77.715)
Total violence squared				48.412				15.797
				(36.393)				(41.71)
Total violence cubed				-6.621				-1.916
				(4.536)				(5.146)
Turnout in 2009					0.512***	0.358***	0.360***	0.357***
					(0.045)	(0.045)	(0.052)	(0.045)
Constant	372.901***	375.114***	335.854***	375.825***	240.847***	278.008***	248.438***	278.261***
	(12.356)	(2.887)	(3.479)	(3.682)	(14.618)	(12.219)	(12.696)	(11.998)
District FE	N	Y	Y	Y	N	Y	Y	Y
N	2290	2290	1823	2290	2116	2116	1823	2116
R2	0	0.495	0.429	0.496	0.192	0.538	0.489	0.539

Notes: Unlike Table 4A, these models include polling centers whose turnout results in 2010 indicate fraud, as well as those polling centers not open in 2009. Robust standard errors clustered on district in parentheses. *** p<0.01, ** p<0.05, * p<0.1

SI Table 5B. Effect of Police Deployment Classification on Polling Center-level Turnout in 2010 Election, Controlling for Turnout in 2009 Election

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medium Security Deployment	-33.409 (21.76)	-17.65 (14.497)	-45.643*** (16.842)	-18.541 (14.418)	-64.948*** (17.488)	-31.312** (15.745)	-40.149** (16.575)	-33.173** (15.853)
High Security Deployment	36.437* (20.612)	2.691 (14.497)	-23.365 (14.384)	2.684 (14.707)	-4.119 (17.539)	-8.609 (14.303)	-21.396 (13.01)	-8.827 (14.684)
SIGACTs Prediction			-3.146 (13.683)				-2.322 (14.552)	
SIGACTs (1-week lag)				-0.926 (13.916)				8.061 (17.596)
SIGACTs (2-week lag)				-4.132 (14.688)				-9.122 (15.847)
SIGACTs (3-week lag)				7.835 (10.336)				-3.646 (18.034)
SIGACTs (4-week lag)				-2.512 (16.597)				-17.141 (18.331)
Total violence previous 5 months				-57.091 (70.153)				21.346 (79.601)
Total violence squared				49.212 (36.877)				15.737 (42.34)
Total violence cubed				-6.734 (4.587)				-1.891 (5.202)
Turnout in 2009					0.513*** (0.045)	0.358*** (0.045)	0.360*** (0.052)	0.357*** (0.045)
Constant	372.901*** (12.359)	375.231*** (2.862)	336.032*** (3.466)	375.961*** (3.661)	240.557*** (14.525)	278.169*** (12.193)	248.708*** (12.699)	278.480*** (11.963)
District FE	N	Y	Y	Y	N	Y	Y	Y
N	2290	2290	1823	2290	2116	2116	1823	2116
R2	0.009	0.495	0.429	0.496	0.199	0.539	0.49	0.54

Notes: Unlike Table 4B, these models include polling centers whose turnout results in 2010 indicate fraud, as well as those polling centers not open in 2009. Robust standard errors clustered on district in parentheses. *** p<0.01, ** p<0.05, * p<0.1

SI Table 6. Intraclass Correlation of Survey Responses by Local and National Issues

	(1)	(2)
	Intraclass Correlation by Polling Center (within district)	Expected Intraclass Correlation
<i>Panel A</i>		
Electricity	0.603	High
Pashtun	0.894	High
Tajik	0.744	High
Uzbek	0.630	High
<i>Panel B</i>		
Karzai Performance	0.329	Low
Government Control	0.265	Low
Central Govt. Services	0.310	Low

SI Table 7. Estimating Bias from Unobservables

Controls in restricted set	Controls in full set	$\hat{\beta}^F / (\hat{\beta}^R - \hat{\beta}^F)$
District FE, violence	District FE, violence, 2009 turnout	8.70
Province FE, violence, ethnicity dummies, 2009 turnout	District FE, violence, 2009 turnout	3.72

Notes: ‘Restricted’ model in row 1 corresponds to SI Table 3A, column 4; ‘full’ model in row 1 corresponds to SI Table 3B, column 4. ‘Restricted’ model in row 2 corresponds to SI Table 3B, column 3; ‘full’ model in row 2 corresponds to SI Table 3B, column 4. Ratio is calculated using the estimated coefficients for medium/high security deployment in the ‘full’ and ‘restricted’ models.

References

- Altonji, J. G., Elder, T. E., & Taber, C. R. (2005). Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. *Journal of Political Economy*, *113*(1), 151-184.
- Callen, M., & Long, J. D. (2015). Institutional Corruption and Election Fraud: Evidence from a Field Experiment in Afghanistan. *American Economic Review*, *105*(1), 354-381.
- Nunn, N., & Wantchekon, L. (2011). The Slave Trade and the Origins of Mistrust in Africa. *American Economic Review*, *101*(7), 3221-3252.
- Rundlett, A., & Svobik, M. W. (2016). Deliver the Vote! Micromotives and Macrobehavior in Electoral Fraud. *American Political Science Review*, *110*(1), 180-197.