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

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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.

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1. INTRODUCTION

In modern democracies, elections serve as the fundamental mechanism to aggregate citizen preferences and delegate power to representatives (Cox, 1997; Przeworski, Stokes, & Manin, 1999). In conflict-prone and transitioning societies, citizen participation in elections is widely understood to strengthen emergent democratic institutions and solidify regime legitimacy (Norris, 2014). Governments and international donors have therefore invested heavily in ensuring that elections can be safely held, including assigning police to safeguard polling stations (Doyle & Sambanis, 2006; Fortna, 2008) and carrying out broader security sector reforms (Berg, 2014; Perito, 2009; Sedra, 2006). Despite such efforts, election cycles in these settings are often associated with an increased probability of conflict (Hafner-Burton, Hyde, & Jablonski, 2014). In some countries, insurgents strategically use pre-election violence to intimidate the population and prevent citizens from turning out to support the government (Berrebi & Klor, 2006). For instance, although the governments of Colombia, Nigeria, and Thailand increased security measures to protect recent polls, insurgents still attacked critical election infrastructure. While security provision plays a vital role in legitimizing the state and its monopolization of violence (Fukuyama, 2004; Levi, 1988), whether (and how) intensifying police presence during elections helps to secure voting, and thus increase participation, remains unclear.

Applying theoretical insights from counterinsurgency theory and the economics of crime literature produces ambiguous predictions. Evidence from modern conflicts suggests that increasing a country's security force presence can deter insurgent attacks (Corps, 2006; Friedman, 2011) and should thus increase turnout by reducing the threat of insurgent violence. This logic is similar to that driving arguments asserting that increased policing deters crime (Becker, 1968; Levitt, 1997). However, deploying additional forces may present armed rebels with additional targets, leading them to inflict more violence in the short term (Kydd & Walter 2006). An increase in the number of police around polling centers could thus *decrease* turnout if citizens perceive voting to be more dangerous. Under both of these theories, police

deployments *indirectly* impact turnout based on whether they deter or attract insurgent electoral violence, which in turn shapes citizens' cost–benefit calculations.

This paper explores an alternative dynamic inherent in the political economy of corruption that more directly links police presence with turnout in fragile states. We argue that changes in policing can *directly* shape citizens' decisions about whether to vote based on how they perceive those security officials. Ideally, a country's police force serves as a positive and trustworthy symbol of the state's coercive authority. But security forces in many developing countries are poorly trained and even predatory, and citizens believe them to be corrupt (Olken & Pande, 2012). If people view the police as venal or are fearful of their interactions with them, many individuals will choose not to vote if it requires interacting with officers. In fragile democratizing states, deploying additional corrupt police may therefore reduce turnout and undermine government efforts to build regime legitimacy *even if* a heightened police presence deters rebel violence.

To examine whether and how policing shapes citizens' electoral behavior, we study voter turnout in Afghanistan's 2010 parliamentary (*Wolesi Jirga*) election. This election provides a unique opportunity to study the impact of policing on turnout for three reasons. First, the election was threatened by considerable violence prior to and on election day (Callen & Long, 2015). Second, there was tremendous variation in policing levels around polling centers. While each polling center entrance had an officer assigned to thwart attacks, additional security force deployments varied drastically. Thus, some voters might have only seen one officer at a polling station, while others might have had to pass through multiple checkpoints on their way to vote. Third, this election provides unusually rich data, including: internal government records of planned police deployments; precise event-level records of violence before, during, and after the election; certified voter turnout numbers; and survey data from potential voters before the election on why they did or did not plan to vote, and after the election on why they actually did so or not, alongside their perceptions of the police and government.

Our empirical approach leverages an artifact of the Afghan government's security force assignment process to isolate the effects of police deployments. Estimating the effects of increased policing on election day violence and turnout necessarily faces inferential problems if the government deployed more police to areas where it anticipated more violence, or if a confounding factor correlated with anticipated violence also drove turnout. In our setting, police were assigned to polling centers based on the historical level of insurgent violence in the area. Yet this process was managed by officials at multiple institutions operating without clear lines of authority, who all had other pressing policy issues competing for their attention. These management challenges, combined with force limitations, often resulted in polling centers with similar histories of violence in the same district being assigned *different* levels of deployment.[1]

Conditional on past insurgent violence in the area of the polling center, the assignment of *additional* police appears orthogonal to anticipated election day violence. This means that once we control for expected violence in a more detailed and systematic way than planning officials did, variation in the assignment of security force levels to polling centers within the same district should be 'as-if random.' This approach allows us to estimate the exogenous impact of police on actual election violence and turnout. Our empirical strategy allows us to systematically examine the potential first-order effects of security force deployment on rebel violence and public sentiment, as well as their second-order effects on levels of voter participation. The credibility of this strategy is strengthened by our first-hand observation of the police deployment planning process, as well as multiple interviews of relevant officials (described below).

After controlling for the history of violence in an area, we find that polling centers that were assigned extra police did not experience a statistically significant change in insurgent violence, on average, but they

[1] For example, two polling centers 0.25 km apart in Sarkani district (Kunar Province) had the exact same history of violence in terms of insurgent attacks, but the polling center at a clinic received a High Security Deployment designation while the center at a nearby school was assigned a Low Security Deployment designation.

did experience a marked *decrease* (of about 30%, on average) in turnout relative to similar polling centers assigned fewer security forces. These results are robust to accounting for multiple threats to identification, including measures of electoral competitiveness, ethnic demography, electoral fraud, spatial spillovers, and other characteristics of polling centers that are potentially correlated with police presence. While variation in the number of security forces assigned to protect various polling centers no doubt affected the Taliban's strategic calculations about the feasibility of carrying out violent election attacks, increased security did not consistently produce first-order decreases in violence; it did, however, affect second-order processes related to citizens' decisions about whether to vote.

Next, we explore why extra policing might have decreased voter turnout. In post-election surveys, citizens who live near polling centers designated to receive higher policing levels were no more likely to cite a fear of violence on election day as a reason for not having voted than those who live near centers designated to receive fewer police. This accords with the hypothesis that deployments did not have a statistically significant effect on violence. Measures of trust in the state, however, dropped significantly from pre-election to post-election surveys in places assigned to receive higher policing levels, as one would expect if individuals in those areas had more negative interactions with security forces on election day. These findings cast doubt on the possibility that police deployments affected turnout by making voters fear insurgent violence, but they are consistent with the claim that extra policing had the unintended effect of deterring turnout because voters did not want to interact with a corrupt police force. These results concur with qualitative information about voting in this election specifically and with views of the Afghan security forces generally.

We make several contributions. First, our research addresses a core challenge confronting emerging democracies characterized by conflict and corruption: how do government efforts to deter and combat insurgent violence affect political participation and governance? Fragile states invest heavily in the occupational and technical training of the army and police to inspire public confidence in their ability to defeat insurgents. Yet we show that the effectiveness of deploying more police to encourage voting – and

thereby enhance regime legitimacy – depends not only on whether such policies effectively deter insurgent violence, but also on how citizens' perceptions of the security forces affects their decisions about whether to vote. Where police are perceived to be corrupt and predatory, increasing security deployments may be detrimental to democratization.

Second, we contribute to two burgeoning empirical literatures. The first evaluates the impact of counterinsurgency efforts on rebel activity (Berman et al., 2013; Biddle, Friedman, & Shapiro, 2012), echoing economic studies of the effects of policing on crime (Draca, Machin, & Witt, 2011; Levitt, 1997). The second literature comprises a host of studies that seek to identify the effects of election violence on voters' attitudes and behavior after terrorist and insurgent attacks (Berrebi & Klor, 2008; Condra, Long, Shaver, & Wright, forthcoming; Getmansky & Zeitzoff, 2014). Both literatures face the challenge of accounting in some way for insurgents' strategic adjustment in response to anticipated changes in force levels and locations. Our research design is similar to other studies that exploit plausibly random sources of variation in insurgent (Wright, 2018) or counterinsurgent (Lyall, 2009) strategies to assess the effectiveness of counterinsurgency policies. In our study, security force deployments were assigned primarily by using the history of violence around polling centers to estimate levels of election-day violence. But the fact that polling centers with similar histories of violence in the same district received different numbers of security personnel, and that these assignments were not publicized before the election, means that insurgents would not have been able to anticipate and strategically adjust for the planned deployments. However, voters had sufficient time to see the enhanced police presence in their localities, and for deployment levels to affect their decisions about whether to turn out to vote. To strengthen confidence in our results, we clearly specify our identification assumptions and test the robustness of our results against a series of threats to inference.

Last, we examine an important case that is broadly applicable to other developing democracies where non-state violent actors credibly threaten to disrupt elections. The 2010 parliamentary elections were an important test of the Afghan government's ability to independently establish and manage electoral

institutions to gain citizen compliance. It therefore worked to secure election infrastructure by allocating more of its limited security resources to areas it believed to be most vulnerable to violent election-day attacks. Other inchoate regimes in post-conflict countries with the potential for election-day violence similarly alter their deployments of security forces to protect voters, including stationing police officers at polling stations in East Timor, Sierra Leone, Egypt, Yemen, Colombia, Iraq, Uganda, and Kenya. Yet we find that in Afghanistan, extra policing did not substantively increase or decrease insurgent violence, but *did* negatively affect turnout through the perceived corruption of officers. Many developing countries that have moved towards democracy over the last 25 years suffer similar patterns of corruption in the security ranks (Chayes, 2015). But an increased police presence on election day is unlikely to result in a peaceful election with increased voter turnout if exposure to corruption delegitimizes the state and makes citizens reticent to vote (Seligson, 2002).

2. POLICING, VIOLENCE, AND ELECTIONS IN EMERGING DEMOCRACIES

In transitioning societies, elections serve as important indications of citizens' compliance with the regime (Lindberg 2006; Norris 2014), but citizens in developing democracies must overcome numerous obstacles to voting (Ferree, Jung, Dowd, & Gibson, 2018), including threats to their physical safety (Collier & Vicente 2012). Fragile states, in particular, frequently face violence aimed at disrupting elections (Collier, 2009). While election violence sometimes arises from incumbents or parties who threaten the opposing side's voters (Wilkinson 2004), it may also result from non-state insurgent actors who strategically use attacks to prevent turnout and undermine support for the government (Berrebi & Klor, 2006). Since voting and security are both critical considerations for state-building, governments must decide how to allocate forces to safeguard elections.

2.1 Prior Approaches

Prior approaches from counterinsurgency theory and studies on the economics of crime suggest that security force increases could *increase* voter turnout by deterring violence and protecting voters. Classical counterinsurgency doctrine (Galula, 1964; Kitson, 1971; Thompson, 1966)[2] rests on the assumption that increasing the levels and strategic position of troops will deter rebel violence and help fragile governments build state capacity, strengthen legitimacy, and win "hearts and minds" (Berman, Shapiro, & Felter, 2011; Sedra, 2017, p. 187). In both Iraq and Afghanistan, the United States and its coalition partners advanced troop "surges" in response to rising sectarian violence and anti-government rebellion (Feaver, 2008; Rashid, 2012), and there is evidence that these increases in troop levels played an important role in supporting counterinsurgency goals (Berman, Felter, Shapiro, & Troland, 2013; Biddle, Friedman, & Shapiro, 2012). The belief in a surge deterrence effect follows canonical theories in the economics of crime literature which hold that increasing the presence of law enforcement deters and reduces crime (Di Tella & Schargrodsky, 2004; Draca et al., 2011; Levitt, 1997) by raising the probability of the detection and punishment of illegal actions (Becker, 1968; Levitt & Miles, 2006). Applied to elections, counterinsurgency principles would guide governments to increase deployments in order to deter insurgent-inspired violence and thereby boost turnout.

Governments in countries emerging from, or still engaged in, violent conflict often rely on a security presence to support elections (Binkerhoff, 2007). US President Barack Obama ordered such an increase to enhance election security and public support for the Afghan government (Rashid, 2012, p. 88). The Kenyan government deployed 95,000 officers to safeguard roughly 20,000 polling locations in 2013 (Ombati, 2017), but added election security in Coast Province, where the irredentist Mombasa Republican Council group had attacked voting registration centers before the election and would eventually hit two police convoys on the morning of the election (Smith, 2013). In 2017, Kenya further increased its election-

[2] For tactical application of these principles, see UK Army (2009) and U.S. Marine Corps (2006).

day deployment to 150,000 officers nation-wide in part due to fears of growing threats from al-Shabaab. In 2011, the Ugandan government increased the number of polling stations and the police assigned to them in previously rebel-held areas of the North that were voting for the first time in a national election, but that continued to face threats from the Lord's Resistance Army.

While fragile states conduct elections in part to strengthen institutions and deploy police to secure those elections, added police may *depress* turnout if citizens anticipate that the heightened security presence signals a greater likelihood of insurgent violence on election day. Augmented force levels may *increase* short-term insurgent violence in many conflict settings. Security forces may act as attractive targets for insurgents seeking to demonstrate their strength and resolve, and to bargain for political concessions (Kydd & Walter, 2006). Insurgents attack in election periods and against election targets to undermine the authority and legitimacy of the government (Condra et al., forthcoming), including rebel attacks against candidates, polling stations, and voters in Colombia (Moloney, 2006), Kenya (Smith, 2013), Iraq (AP, 2014), Somalia (Reuters, 2016), Thailand (Fifield, 2016), and India (Rahman, 2009). For example, in Iraqi elections, "insurgents are aiming to shatter [Prime Minister Nouri al-](Maliki's) legitimacy and sow more chaos among the different political groups" and signal that "Iraq is not stable or secure, and is still in their hands" (quoted in Abbas, 2009). Indeed, debates over the troop "surge" in Iraq (Feaver, 2008), designed to decrease sectarian fighting over the long term, highlighted the risk that it could spark a short-term rise in violence. For these reasons, linking force levels to electoral participation via a deterrence mechanism produces ambiguous empirical predictions.

2.2 Our Approach

To determine whether and how policing increases electoral participation in fragile states, we identify a separate mechanism drawn from the political economy of corruption literature that also directly links election security force deployment with electoral participation. As the most visible embodiment of the state's coercive power, a police officer ideally serves as a symbol of authority who fosters order and

protects the population (Serchuk, 2006). As the police often provide the most routine interaction citizens have with the state on a regular basis (Soss & Weaver, 2017), the nature of these interactions affects their confidence in the state security apparatus as well as their perceptions of broader government legitimacy (Lerman & Weaver, 2014; Nagin & Telep, 2017), especially where non-state actors threaten violence against civilians. Understanding the effects of police presence on elections requires us to study how citizens' perceptions of police affect their electoral behavior.

Principally, we argue that the levels of trust and legitimacy citizens attribute to the police will subsequently motivate their political behavior (Tyler, 2006), specifically their willingness to signal compliance with the democratic regime by voting. When citizens believe the police are acting in service of the public good, police deployments should increase citizens' political engagement. Counterinsurgency strategies are most successful when security patrols are conducted within the community to establish trust, build rapport, and send a strong signal that the government supports citizens (Lyall & Wilson III 2009). Accordingly, Afghan and US officials tasked police with gathering information about insurgents through positive interactions with civilians (Malkasian, 2013, pp. 155, 213) in order to capture Taliban fighters and thwart their attacks. Therefore, a "surge" of forces not only attempts to make life safe for citizens in conflict environments; it also helps to build goodwill and redounds to positive perceptions of other institutions. If building up the strength of any one institution – like the police – reinforces other institutions – such as elections – an increase in policing may help increase turnout by improving overall perceptions of the state.

Yet security forces in many developing countries are predatory, poorly trained, and seen as corrupt by citizens (Olken & Pande, 2012; Sedra, 2017). Particularly in weak states struggling against political violence, citizens are regularly forced to pay bribes to the police (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 deemed the most corrupt of all public institutions (Hardoon & Heinrich, 2013). Police abuse causes citizens to hold negative views of the government and

to decrease their participation in other institutions and processes in public life (Nagin & Telep, 2017), as exposure to corruption delegitimizes the state and makes citizens reticent to express compliance (Seligson, 2002).

Perceptions of the police as ineffective, corrupt, or predatory could plausibly have either a positive or negative effect on electoral turnout. On the one hand, citizens may punish corrupt politicians at the polls (Finan & Ferraz, 2008). There is some evidence of this in emerging European democracies (Kostadinova, 2009), and it is at least suggested in multiple fragile states where, despite perceived rampant corruption in the administrative state, election turnout is equal to or *higher* than in the least corrupt countries in the world.[3]

On the other hand, and as we argue, perceived corruption is more likely to *reduce* turnout where police are used to protect elections. If the police are not viewed positively – because people are fearful of their interactions with them or view them as untrustworthy – their presence during elections is unlikely to inspire confidence in voters. Previous studies have shown that increasing citizens' knowledge of political corruption can lead them to disengage from politics and decrease turnout (Chong, De La O, Karlan, & Wantchekon, 2015; Stockemer, LaMontagne, & Scruggs, 2013). Individuals are less likely to vote if they believe the regime is corrupt because police erode political efficacy,[4] which affects citizens' capacity for collective action and desire to participate (Dimitrova-Grajzl, Simon, & Fischer, 2010). If citizens believe

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

[4] Political efficacy is defined as "the feeling that individual political action does have, or can have, an impact upon the political process, i.e. the sense that it is worthwhile to perform one's civic duties" (Campbell, Gurin, & Miller, 1954, p. 187).

the security forces are corrupt, they are less likely to view the state as legitimate (Seligson, 2002), as has been shown in Afghanistan (Torabi & Delesgues, 2007). This reduces their desire to signal compliance by participating in elections overseen by these agents.

Therefore, if increased policing dissuades citizens from voting because they wish to avoid interacting with security officials who they see as corrupt and predatory, augmenting force levels during elections is unlikely to encourage voter turnout where the police are corrupt generally *even if* their presence deters violence. We therefore hypothesize that, conditional on levels of violence, additional police will *decrease* turnout because citizens' increased interactions with corrupt and predatory state agents discourage further engagement with them. Therefore, determining whether police deployments to protect elections assist the government's use of democratic institutions to enhance legitimacy and gain citizen compliance requires evaluating whether more police deter violence, as well as how citizens view those police and what they represent.

3. SETTING

Elections have played an important role in efforts to democratize, legitimize, and strengthen the Afghan state since the overthrow of the Taliban by the US, Coalition, and Afghan resistance forces in 2001. The new Afghan government sought to legitimize its fledgling democratic institutions by holding the first presidential elections in 2004 (which Hamid Karzai won), followed by the first parliamentary (*Wolesi Jirga*) elections in 2005. Karzai was re-elected in 2009, followed by parliamentary elections in 2010 and a presidential race in 2014. Election violence has been a core concern for policymakers and voters in Afghanistan. Insurgent attacks against state targets during election campaigns and on election day are orders of magnitude higher than in non-election periods (Condra et al., forthcoming), and violent spikes depress turnout. In the 2014 election, Condra et al. (forthcoming) estimate that every election-day early morning direct fire attack reduced district-level turnout by 9% to 14%, and a pre-election improvised explosive device deployed on a road decreased the total number of ballots cast at connected polling centers

by an average of 7,400 votes.[5] Further, the authors show that public opinion surveys indicate that exposure to this violence increased citizens' dissatisfaction with the election process.

This history of insurgent violence and its effects help explain why Afghan government officials viewed robust turnout as critically important to enhancing public perceptions of government legitimacy.[6] Preventing electoral violence was of paramount concern to officials in charge of security for the 2010 race. Insurgent violence was on the rise, and officials worried that the election would experience more attacks than the 2009 polls (Bergen, 2010). As Hincks (2018) explains, "[The Taliban] wants to make the state look weak, and what better way of doing so than making it clear that the state is unable to protect its own people?" The Taliban's guerrilla-style attacks around elections do not seek to gain territory, but rather to "humiliate the government where it is most visible" (Fisher, 2018). Officials from the Independent Election Commission (IEC) expressed fears that security would need to be sufficiently increased to allow voting to occur (Filkins & Wafa, 2010). The Afghan government thus worked closely with several key actors, including the Afghan National Security Forces and the International Security Assistance Force (ISAF), to begin security coordination efforts for 2010 much earlier than they had for 2009. In what was an "unprecedented" effort to secure this election (Nakamura & Londono, 2010), the government began training hundreds of thousands of new security personnel to deploy for the election; the training was focused on combating insurgent violence rather than civil policing methods.[7] This government planning

[5] About 6.6 million ballots were cast in the first round of the 2014 election (NDI, 2014).

[6] For example, officials from the National Police Command Center (NPCC) reached out to senior and influential civilian intermediaries in Lashkar Ga district (Helmand Province) to broker deals with the Taliban. In exchange for insurgents allowing citizens to turn out at the polls, the government promised increased development aid through Provincial Reconstruction Teams and the Ministry of Rural Rehabilitation and Development (Condra, 2014c).

[7] To accelerate force deployment, Afghan National Army (ANA) training was shortened in 2010 from 12 to 6 weeks (Sedra, 2017, p. 173). After the responsibility for training Afghan National Police (ANP) recruits shifted from the US State Department to the US military in 2010, "the training course for the

process included a risk assessment of all potential polling stations in order to determine which required additional security (NDI, 2011).

Ultimately, 52,000 ANP officers and 63,000 ANA officers were assigned 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, since citizens (including poll workers) were subjected to body searches before entering to make sure that no one was armed (Condra, 2014c; Nakamura & Londono, 2010). In a low-deployment area, a voter would see at least one officer at the entrance. Areas with higher deployments had more than one officer at the entrance, or additional police outside the walls of, or in alleys and roads leading into, polling centers.

While the government's security deployment largely focused on deterring insurgent violence, the Afghan Ministry of Interior (MOI) also sought to encourage public trust in the police ahead of the surge (Iyengar, 2010). Indeed, the ANP's reputation as corrupt and predatory severely damaged public trust in the institution. As Perito (2009, p. 5) describes, long delays in salary payments for police motivated them to engage in "petty corruption that undermined public confidence." The police force is 90% illiterate, and attrition and desertion rates are high; they have a long history of engaging in a wide range of criminal activity (Felbab-Brown, 2013; Rashid, 2008).

There is also considerable evidence that the police mistreat civilians (Giustozzi & Isaqzadeh, 2012). A 2010 survey focusing on police malfeasance revealed that recruits were forbidden to carry guns when they were off duty after reports that they were using them to rob civilians. Police frequently bribe civilians: "[W]hen [Afghan citizens] interact with the police, it is often to pay bribes or illegal taxes" (Sedra, 2017, p. 179); 25% of UN survey respondents reported having paid at least one bribe to police in the previous

police predominantly comprised paramilitary tactics, with less than a week dedicated to instruction on civil police duties, including the constitution, the conduct of criminal proceedings and human rights" (Sedra, 2017, p. 186).

year (UNODC, 2010) (surely an undercount), and the police and the justice system were perceived to be the two most corrupt sectors in 2006 (Torabi & Delesgues, 2007). Police reportedly have also engaged in rape, torture, and extrajudicial executions of civilians (Giustozzi, 2008): "In 2010, nearly 200 policemen were accused of murder and just over 4,600 were involved in crimes in 3,026 separate cases sent to the Attorney General in Kabul'" (Sedra, 2017, pp. 179-180).

Data from several public opinion surveys (described in more detail below) ahead of the 2010 election demonstrate that citizens perceived the police as predatory and abusive. Pashtuns, the largest ethnic group, have a particularly challenging relationship with the ANP, which is commonly thought to be dominated by Tajiks and Uzbeks. 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, compared to only 17% of Tajiks and 9% of Uzbeks (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 Kandahar Province (which is overwhelmingly populated by Pashtuns) (Table 1, Panel A) revealed that 73% of respondents disagreed a little or a lot with 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." In addition, 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 the police as corrupt and predatory.[8]

4. DATA AND ESTIMATION STRATEGY

4.1 Data

We examine how policing affects electoral quality by determining whether Afghan policing deployments worked to reduce violence and increase turnout in the 2010 election. We use data from seven sources: (1) levels of policing assigned to polling centers, which constitutes the intervention of interest, (2) certified voter turnout data from the IEC, our primary dependent variable, (3) insurgent violence data collected by ISAF, (4) data from ISAF's ANQAR surveys, (5) primary survey data that we collected from 5,000 individuals across 471 polling center catchment areas in 19 provinces across all regions before and after the election, (6) survey data of the 369 residents of Kandahar referenced above, and (7) data from the Asia Foundation's 2010 nationwide survey of 6,348 respondents fielded before the election. We provide a brief description of the data sources here; the Supporting Information (SI) contains a more detailed discussion of our data sources (SI Table 1 provides summary statistics).

Levels of 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 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 received an extra deployment of police (i.e., Medium Security Deployment), and "highly insecure" polling centers received even more additional police (i.e., High

[8] The ANQAR and Kandahar survey data indicate that people are generally willing to report on corruption, but there are differences in rates across ethnicities, suggesting the need to consider location fixed effects to partial out these differences in the regression analyses that follow.

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. Police were home (not deployed) during the Muslim holy month of Ramadan (August 11 to 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. It shows the total number of ballots cast at each polling center, which contains multiple polling stations (NDI, 2011, p. 32).

Insurgent Violence: We use declassified incident reports submitted by ISAF and Afghanistan security forces that report combat occurring between ISAF units and insurgents, commonly known as "significant activity" reports (SIGACTs). These reports provide the incident's date, time, and georeferenced location. We use the subset of that dataset from March to 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 1 km radius of each polling center to isolate the impact of police in the immediate vicinity of the location they were deployed to protect.[9] While the SIGACTs do not capture all conceivable violence that civilians experienced, we discuss in the SI why they are a valid and useful measure of threats to civilians who were deciding whether to turn out to vote.

Surveys: We use three different survey datasets related to Afghans' opinions about the police to test our main hypothesis, including measures of local civilians' perceptions of potential violence emanating from voting (separately from the objective SIGACTs measure) and views of the police, including the likely harm and corruption they produce.[10]

[9] We examine violence within 1 km of polling centers in accordance with the Afghan government mandate of a force laydown of 1 km around polling centers. Author [Long] obtained this information while serving as an accredited observer of the 2010 election and was briefed by the IEC about security provisions multiple times. Interviews with officials involved in the deployment planning process confirm this (Condra, 2014a, 2014b, 2014c), as do independent election reports (FEFA, 2011, p. 46).

[10] The SI describes the sampling procedures, survey questions and implementation more fully.

4.2 Identification Strategy

To estimate the effect of police force levels on insurgent violence and turnout, we rely on our knowledge of the Afghan government's plans to protect voting in the 2010 election via police deployment across polling centers and a plausibly exogenous source of variation in deployment increases, which we now explicate.

Based in part on the authors' own interactions with the officials responsible for securing elections, we know the government and ISAF expressed significant concerns about the threat that insurgents posed to the election.[11] Officials recognized that violence not only threatened democracy by depressing turnout, but also demonstrated the weakness of the Afghan state and lent credibility to the Taliban's claim that the government lacked legitimacy (Condra et al., forthcoming). Thus, to boost turnout, the government focused on deploying security forces to deter violence.

Polling centers were classified as requiring low, medium, or high security deployments based largely on past levels of insurgent violence around the polling center (Iyengar, 2010).[12] Officials had a general sense of the broad trends in insurgent violence within districts, but only considered the dynamics at individual centers in the most prominent cases. Members of the NPCC, ISAF, and MOI met at least

[11] Iyengar provided independent objective assessments of research efforts for the ISAF commander and attended weekly meetings at the MOI with ministry officials and ISAF regional commanders responsible for deciding how to deploy police. Iyengar did not serve in an advisory capacity to ISAF or the Afghan government, so should be considered a passive observer with detailed knowledge of the process the government used to develop its deployment schedule.

[12] Multiple officials, across different organizations involved during and after the planning process articulated this decision rule to us (Condra, 2014a, 2014b, 2014c; Iyengar, 2010), which concurs with documentation of the process published after the election (FEFA, 2011; NDI, 2011).

monthly in early 2010 to review daily attacks across the country to determine the categorization of polling centers, according to a former MOI official who attended these meetings (Condra, 2014c).

To systematically gauge how the government's levels of *anticipated* insurgent violence around polling areas affected the force deployments, 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, resulted in sites located in areas with similar histories of violence, road access, and even population characteristics receiving different force levels. 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 was conditionally random.

Therefore, a detailed examination of the assignment process – which involved personal participation, interviews of officials, and statistical analysis of the data – leads us to believe that, conditional on past insurgent violence in the area around the polling center, assignment of police deployment level was orthogonal to anticipated violence on election day. Controlling for levels of expected violence in a more detailed and systematic way than planning officials did reveals that variation in the assignment of additional police to polling centers within the same district was 'as if random,' which allows us to estimate the causal impact of police on turnout. While the official records on the assignment process are insufficient to definitively demonstrate the conditional independence of assignment to different categories of security force deployment, our interviews and analysis provide no reason to think the assignment was correlated with unobservable factors that would also influence turnout. Still, because of this ambiguity, we conduct

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

a bounding exercise to quantify how large the selection on unobservables would have to be to account for our results (SI Table 7).

4.3 Estimating the Effect of the Augmented Police Presence on Insurgent Violence and Turnout

Our main analysis of the effect of enhanced police deployments on election violence and turnout is limited to a subsample of the 5,524 total polling centers. Our subsample includes polling centers that meet three criteria. First, to avoid including polling centers where electoral fraud is suspected to have 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 sites' average is close to that level is suspicious; the IEC used this threshold 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 our 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 analyze 1,823 out of 2,031 polling centers that have MOI security classification designations in 2010. Within that sample, the breakdown of security classification is 1,448 Low Security Deployment (79%), 181 medium (10%), and 194 high (11%).

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

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

[14] Below, we address whether including fraudulent polling centers in the sample affects the results. Callen and Long (2015) discuss the mechanics of rigging in 2010 and why 590 votes or more per station is a likely fraud indicator.

where, $Police_i$ is the security classification assigned to the polling center (low, medium, or high), $V_{t-k,i}$ 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 levels of violence predicted security force classification, we would expect to see this reflected in the results of Table 2. The lags of violence and cubic polynomial of the fighting season average levels of violence generally do not predict the assigned level of security force deployment 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 those that were open in 2010 but not in 2009. This does not change the results, providing further evidence that police deployment was not correlated with electoral fraud.

The evidence shows that past violence within 1 km of polling centers explains little of the variation in security force deployments. This is not surprising, partly because assignments were based on district- and higher-level data on violence, while we rely on much more local variation in violence levels for our data. Since the assignment strategy resulted in polling centers (in the same district) with similar histories

[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.

of violence receiving different deployment assignments, we estimate the effect of policing on violence and turnout, 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 security force deployment levels were assigned according to the area's history of violence, they could have used 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 of these tests of competing explanations support our main results and substantiate our claim of exogenous variation in assignments of security force levels.

Second, despite our best and repeated efforts, we have been unable to secure data on the number of police deployed to individual polling centers or the precise degree to which forces in the field adhered to the deployment schedule. While our interviews with multiple officials with first-hand knowledge of the deployment schedule and process suggest that the security deployment categorization scheme was followed (Condra, 2014a, 2014c), we cannot empirically verify with full certainty that provincial-level officials followed polling center protocols. We therefore argue that our estimates of police deployments can be interpreted as intention-to-treat estimates, which take into account the fact that researchers cannot easily verify perfect compliance with assignment protocols (in our case, deployment) (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. 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 the estimated effects of security force deployment we find (SI Table 7). The magnitude of the estimated effect on turnout in our analysis makes it unlikely that such unobserved factors could overwhelm those effects.

5. MAIN RESULTS

5.1 Effect of Enhanced Police Presence on Election-Day Violence

Deploying additional police on election day could decrease violence if deterrence was successful, or increase violence if it was not and extra deployments acted as targets for insurgent violence. For all regressions in Table 3 that estimate the effect of security deployment on violence, we measure police deployments in two ways: first as a binary variable that takes a value of '1' if the polling center was designated to receive additional police; and second, we include dummy variables for polling centers that received 'medium' or 'high' security provision classifications. We estimate these specifications using OLS:

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

where M_i and H_i are dummy variables for whether a polling center was assigned a medium or high security provision classification, $V_{t-k,i}$ 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 denotes a series of differences in violence within 1 km of the polling center: election week versus the week prior to the election (columns 1-2), election week versus the average violence in the four weeks before 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.

The results show that a polling center's security status does not have a strong discernible effect on the change in violence experienced at polling centers before or during/after the election. Conditional on accounting for the level of historical violence in a district (again, as the best measure officials had of

anticipated violence on election day), within-district increases of police deployments neither systematically increased nor decreased the level of election-day violence. Given our identifying assumptions, this means that *increased police deployments – by either deterring or attracting insurgent violence – are unlikely to account for any observed changes in turnout*.

5.2 Effect of Enhanced Police Presence on Turnout

Next, we assess the effect of deploying extra police on turnout at polling centers by estimating a series of regressions including:

$$T_{2010} = \alpha_1(M_i) + \alpha_2(H_i) + \beta_1(V_{t-1})_i + \beta_2(V_{t-2})_i + \beta_3(V_{t-3})_i + \beta_4(V_{t-4})_i + \gamma_1\left(\overline{Vpre}_i\right) +$$

$$\gamma_2(\overline{Vpre}_i^2) + \gamma_3(\overline{Vpre}_i^3) + \delta X_i + d_i + \mu_i, \qquad (3)$$

where raw turnout in 2010 is a function of the same covariates as those in the violence regressions. X_i is a vector of controls, which varies across models. Here, we present the results of models that control for the factors we believe are the most likely confounds.[16] The first control is turnout in 2009. Second, we include the log of the absolute value of the difference in vote share between Karzai and Abdullah in the 2009 election, which we use as a measure of electoral competitiveness. We reason that turnout might increase for more competitive elections, since citizens will see their vote as more valuable. Third, we test for possible spatial spillovers by including (1) a dummy variable if a polling center within 1 km was also assigned medium/high deployment, and (2) a count of the total number of polling centers within 1 km.

Table 4A reports the results using the binary measure of police deployment classification, coding the least secure polling centers as 1 (i.e., combining high and medium deployment categories) and the most secure as 0 (i.e., low deployment). Table 4B reports the results of models that include dummy variables

[16] In the SI we develop the logic of these tests more fully and report the results of additional robustness checks.

for the medium and high security classifications, with the most secure classification (low deployment) as the omitted category.

In column 1 of both tables, we report the most basic regression of turnout on the binary security deployment classification variable (combined medium/high), including a district fixed effect. Column 2 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 3 has controls for previous levels of violence in the area. In column 4, we include a measure of electoral competitiveness. Column 5 includes variables designed to account for the possibility that there may have been spatial externalities in the assignment of police deployment classification. The models estimated in columns 6–10 have the same specification as their respective counterparts in columns 1–5, except that they include a control for turnout in 2009.[17]

Both tables show that having a higher security classification (medium or high) has an estimated negative effect on raw turnout. Across specifications (columns 1–5), polling centers that were assigned more police (i.e., medium or high) experienced a statistically and substantively significant 30% *decrease* in turnout relative to the previous election. This relationship holds when we control for electoral competitiveness (columns 4 and 9), spatial spillovers (columns 5 and 10), and 2009 turnout (columns 6–10).

The null result on violence predicting the assigned level of security force deployment is unaffected when we include fraudulent polling centers (those that engaged in ballot stuffing to inflate vote totals) in the analysis (SI Table 4). The main results on turnout are consistent with a police presence being either

[17] We do not use first differences of turnout across the two elections because this 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), since it allows for the coefficient to be different than -1.

positively or negatively associated with fraudulent polling stations, depending on whether they facilitated or hindered political operatives' altering of results. SI Tables 5A and 5B replicate Tables 4A and 4B, respectively, but include fraudulent polling centers and those that were not open in 2009. The null results in columns 1–5 of SI Tables 5A–B – which include all polling centers – suggest that any bias due to fraud is in the opposite direction of our results (dropping clearly fraudulent polling centers weakens the result). Including fraudulent polling stations that were also open in 2009 and controlling for 2009 turnout (columns 6–10 of both tables) produces results that are statistically similar to the restricted sample in Tables 4A–B but substantively smaller, again suggesting any bias due to fraud is in the opposite direction of our main result. SI Table 5B indicates that the effects on turnout are larger in medium than high security deployment polling centers, which is also consistent with a correlation between higher levels of police deployment and greater electoral corruption (i.e., higher turnout). Again, this is biased against the main result. Overall, the evidence suggests that fraud systematically coordinated with security force deployments is lending a *positive* bias to estimates of the effect of deployments on turnout, so the negative conditional correlation we observe in Tables 4A–B should produce even more confidence in the causal nature of the estimated effects.

Taken together, these results are striking. Previous levels of violence do not predict the security deployment classification of polling centers, and after controlling for previous levels of violence, the majority of variation in these assignments is left unexplained. This allows us to identify the effect of extra police on turnout and violence. Extra police had an unintended and deleterious effect on turnout, but not apparently through a violence deterrence mechanism. We therefore next turn to explanations that predict a *decrease* in turnout and compare our main results to the observable implications in relation to our main hypothesis.

6. EXPLAINING A REDUCTION IN TURNOUT

6.1 Police as Targets?

Citizens could associate a heightened police presence with a higher probability of insurgent violence and stay home because of a perceived decrease in voter safety – even if, as we have shown, extra police did not increase levels of insurgent violence. Some qualitative accounts of Afghan elections suggest this possibility,[18] although reports differ on whether the threat of violence actually deterred voters.[19] The Asia Foundation's annual nationwide survey for 2010 fielded prior to the election found that the proportion of respondents who identified fear for their safety as the reason for the lack of freedom of expression in their area was lower in 2010 (25%) than in any previous year (Tariq, Ayoubi, & Haqbeen, 2011, p. 95). Similarly, "there appeared to be less violence than during the [2009] election...Afghan security officials dismissed the [2010] attacks as 'insignificant' and said they did not hamper voting, adding that 92 percent of polling stations were open Saturday" (AP, 2010). However, if an enhanced police presence signaled the likelihood of greater violence to citizens, this may have convinced them to stay home.

To more systematically test the implications of this possibility, we use primary survey data. Respondents who reported that they would not vote (baseline) or did not vote (endline) in the 2010 election were asked why. If the signaling mechanism was driving behavior, we would expect a greater percentage

[18] The Taliban warned of violence on and around election day in an effort to deter voting in 2009 and 2010 (Farmer, 2010; NDI, 2011). According to some observers, "[m]ore 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]" (Coburn & Larson, 2014, pp. 168-169).

[19] Other accounts suggest the threat of violence was not an important determinant of local turnout. For example, while Coburn and Larson (2014, p. 169) point to the threat of violence as a factor in voting behavior, they also provide numerous first-hand accounts which demonstrate that "relatively few of the people we talked to appeared to actually decide not to vote based on this threat."

of citizens around polling centers that were designated medium or high security deployment to cite fear of election-day violence as the reason they stayed home, relative to the proportion of citizens around low-security centers who cited the same reason. We regress the number of respondents that cited "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).

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 did not influence citizens' decisions about whether to turn out: about 17% of citizens who did not vote cited this as the reason retrospectively (and 17% prospectively).[20] But there is no difference in frequency across polling centers with different security deployment levels, and citizens gave many other reasons for not voting, including a lack of interest, undesirable candidates, and not being eligible.

In column 1, we test whether variation in security deployment levels across polling centers is associated with more pre-election citations of violence as a reason for not *planning* to vote. There is no evidence of this either, though we note that because the baseline survey was fielded in August (before the security force laydown began) these citizens had not yet experienced variation in police deployment. Therefore, we cannot adequately test this implication of the mechanism in the pre-election phase. Still, the results in column 1 further assuage concerns about our identification strategy or the spuriousness of the results. If an unobserved factor is driving the assignment of security force deployment levels and

[20] These data were collected from a small subset of polling centers, but this figure is in line with that reported by the Asia Foundation's nationwide survey fielded prior to the election: of those who said they were somewhat or very unlikely to vote in the upcoming election, 22% cited "insecurity" as the reason (Tariq et al., 2011, p. 114). This should lessen concerns that the polling centers included in our survey are dissimilar from the full set of polling centers along dimensions that would affect respondents' level of fear of insecurity, and decisions about whether to turn out.

correlates 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 planned not to vote. We do not, and this lack of systematic evidence of a violence signaling mechanism is corroborated by both quantitative survey evidence from Afghanistan that previous violence exposure and actual attacks did not correlate with individuals' self-reported turnout (Jung & Long, 2018), as well as qualitative assessments of scholars who observed the voting.

6.1 Corrupt Police?

Next, we test our hypothesis that voters were more likely to avoid polling centers with higher levels of police deployment in order to avoid additional interactions with police who they perceived to be corrupt and predatory. Data from several sources suggest that Afghan citizens feared interacting with the police during the election not because such deployments signaled increased insecurity to civilians through anticipated swells in insurgent violence, but rather because of the police themselves. The Asia Foundation's 2010 nationwide pre-election survey (Tariq et al., 2011) suggests that Afghans have a substantial fear of the police and of voting in national elections. Respondents were asked to give their level of fear (none, some, a lot) when voting in a national election and when encountering ANP officers (SI Table 10). Roughly 53% reported having some or a lot of fear of an encounter with the ANP, and 60% did so for voting; the union of those two represents 70% of the population. Fear of the police is therefore correlated with not desiring to vote. Among the group with no fear of voting (who might be the most likely to turn out), fully 44% reported some or a lot of fear of the ANP. Of those with some fear of voting (who might be the most sensitive to factors that increase their fear of turning out), 63% had some or a lot of fear about the ANP. Of those expressing a lot of fear of voting (who might be expected to stay home), 78% reported some or a lot of fear of an ANP encounter.

Additional pre-election accounts of police behavior are instructive for understanding election day police–citizen interactions. As a NATO report from 2010 noted, "most Afghans had come to view the

ANP as lawless armed men, rather than trusted law enforcement officials" (quoted in Sedra, 2017, p. 182). In the few months before the election, the government began deploying more soldiers and police and set up more checkpoints on roads: "Besides choking off Taliban routes into Kandahar, the checkpoints are designed to persuade Afghans that the arrival of U.S.-trained forces will end endemic police corruption" (Nissenbaum, 2010b). But U.S. officials noted that regular Afghan police stationed there set up their own checkpoints a few hundred yards away from the U.S.-monitored ones.[21] On election day in Kabul, civilians complained that 800 soldiers kept people from voting at a high school that served as a polling center (Boone, 2010). Since the government deployed the ANA and ANP in concentric circles around polling centers to inspect vehicular and foot traffic, more police meant more interactions with police. Once citizens became aware of the 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 may therefore have led them to stay home to avoid potentially injurious interactions with police.

Responses from our pre- and post-election surveys demonstrate 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). As prominent scholars in the well-established procedural justice literature have noted, "[p]eople's reactions to authorities are shaped by their judgments about how fairly those authorities make decisions and how respectfully they treat the people over whom they exercise their authority" (Tyler, 2005, p. 326). If lower turnout in medium-/high-deployment areas is due to increasing perceptions of the police as corrupt and the state as illegitimate, police deployments that produce more interactions with citizens should negatively correlate with views of the government as a legitimate authority on many

[21] As the commander of coalition forces in southern Afghanistan noted: "I suspect that the (Afghan National Police) are quietly trying to create another checkpoint around the corner so that their income isn't too disrupted" (Nissenbaum, 2010b). In other parts of the country, police set up checkpoints expressly to "shake down" citizens (Nissenbaum, 2010a).

dimensions: whether the state is the appropriate authority for dispute resolution,[22] the importance of paying taxes,[23] and the performance of the central government (columns 3–5).[24] But police deployment should be less likely to correlate with more general opinions about Afghanistan's regime type and satisfaction with Afghan democracy (columns 1–2).[25] 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). Yet substantial negative movements occur in responses to three questions that suggest interactions with corrupt police. First, the percentage of respondents who would trust either district officials or the police in a dispute decreased from before to after the election in areas designated to receive more police during the election (column 3). 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. Third, respondents' rating of how well the central government is doing its job decreases (column 5). For most citizens, the police are the most visible embodiment of the central government (Soss & Weaver, 2017); their perceptions of central government performance are thus linked

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

[23] "How important is it to pay taxes?"

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

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

to the quality of their interactions with the police. These results are consistent with the theory based on the police as a symbol of legitimate government and law and order.[26]

Why would these potential interactions with more corrupt police drive down turnout in this case? We suggested above that the level of personal political efficacy might be particularly important for determining whether deploying additional corrupt security forces around elections increases or decreases turnout. Consistent with this argument, we note that political efficacy is low in Afghanistan, even compared to other countries struggling against corruption. According to a 2010 nationwide survey, only 12% of respondents felt that they could have a lot of influence over government decisions through political participation; 25% felt they had no influence at all. In some regions (the southeast, greater Kabul, Hazarajat) a full third of respondents said they had no ability to influence government decisions (Tariq, Ayoubi, & Haqbeen, 2010, pp. 96-97). To put these attitudes into perspective, consider the levels of political efficacy in other emerging democracies where corruption was a governance problem in this period (TI, 2010) and for which we have comparable data. The percentage of respondents reporting that their vote would have no influence on government policy was only 7.6% in Mexico (2006), 11.4% in Latvia (2010), 4.8% in the Philippines (2010), 6.3% in Peru (2011), and 10.6% in Thailand (2007) (CSES, 2015). Indeed, there is compelling evidence that making Afghans aware of official corruption does not make them more likely to turn out. Wright, Condra, and Claudy (2018) compare survey responses from right before and after news of the Kabul Bank scandal broke in 2010, which triggered large-scale bank

[26] In Table 6, any natural bias resulting from an imbalance across security force deployment categories prior to deploying security forces (e.g., citizens around medium-/high-security polling centers are more likely to view Afghanistan as a democracy in our survey) would cause us to expect bias to *increase* turnout around polling centers that were to receive more police. The imbalance makes it more difficult to detect the size and sign of the effect of interest, which is 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.

runs and implicated President Karzai's family. While the news changed perceptions of government corruption among citizens most likely to be affected by the scandal, there was no evidence that these citizens were more likely to vote in the parliamentary elections held two weeks later.

In interpreting the negative effect of police deployments on turnout in the 2010 election, our results support the argument that voters wanted to avoid interactions with police, who they see as corrupt and predatory. Exposure to the police negatively affected Afghans' view of the police and government performance in providing two key public goods essential to state-building: justice and redistribution (Table 6). Augmented police deployments might therefore reduce citizens' expression of consent and desire to participate in practices that legitimize the government, such as voting.

7. CONCLUSION

Optimism regarding the increase in transitions from authoritarianism to democracy worldwide since the 1970s has recently given way to grave concerns about democratic retrenchment (Diamond, 2008; Fukuyama, 2015). Although emerging democracies may hold elections, these polls mask deep-seated institutional and normative threats to the consolidation of democracy, including official corruption and insurgent violence. In this article, we explore the consequences of government efforts to secure elections from violence by deploying additional police. The study contributes to our understanding of when and how government responses to insurgent violence affect political representation and governance. We highlight factors that may produce contrasting effects on turnout. Our findings indicate that polling centers in Afghanistan designated to receive more police protection experienced roughly 30% less turnout on election day in the 2010 national election relative to polling centers in the same district designated to receive less police protection. The results are inconsistent with implications that more police signaled more violence and kept citizens away from the polls. They are instead consistent with citizens staying home in order to avoid interactions with corrupt security forces.

In the broader context of securing polities against violence and encouraging democratization, our results provide insights for ongoing debates among scholars and policymakers on the conditions under which elections should be held in conflict environments. While early elections may increase the likelihood of violence (Brancati & Snyder, 2013), 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). 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 with institutions ill equipped to handle a democratic transition may produce hybrid regimes (Fukuyama, 2014) and reignite violence (Mansfield & Snyder, 2005).

In this respect, our results may appear discouraging. If police perceived to be corrupt can depress voter turnout and damage key indicators of democracy, state-building efforts of governments and the international community are unlikely to succeed at strengthening legitimacy. The quantitative analysis presented here aligns qualitatively with the concerns of election observers, security officials, and the Afghan election commission that low turnout implies a lack of support for the government (Maroney, 2010; Rubin & Gall, 2010). However, 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.

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Table 1. Summary Statistics of Afghans' Perceptions of Police

VARIABLES	Observations	Mean (% agree)	Std. Dev.
Panel A: Kandahar survey (August-October 2010)			
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
Panel B: ANQAR survey (March and May/June 2010)			
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. Assignment of Police Deployment Classification 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	0.058	0.067	
	(0.072)	(0.074)	(0.089)	
SIGACTs (2-week lag)	0.039	-0.001	-0.004	
	(0.065)	(0.067)	(0.086)	
SIGACTs (3-week lag)	-0.063	-0.080	-0.007	
	(0.058)	(0.062)	(0.071)	
SIGACTs (4-week lag)	0.137**	0.111*	-0.169*	
	(0.059)	(0.061)	(0.086)	
Total violence previous 5 months	0.128	-0.019	-0.558*	-0.626***
	(0.275)	(0.323)	(0.335)	(0.239)
Total violence squared	-0.203	-0.128	0.267	0.215
	(0.155)	(0.181)	(0.183)	(0.146)
Total violence cubed	0.018	0.013	-0.019	-0.021
	(0.019)	(0.022)	(0.023)	(0.019)
Constant	1.308***	1.316***	1.333***	1.331***
	(0.029)	(0.027)	(0.007)	(0.007)
Observations	1,823	1,823	1,823	1,823
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 clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Effect of Police Deployment Classification on Violence (Timing Change at Election)

VARIABLES	(1) Election week vs. week before	(2) Election week vs. week before	(3) Election week vs. 4-week pre-election average	(4) Election week vs. 4-week pre-election average	(5) 4-week average post vs. 4-week average before	(6) 4-week average post vs. 4-week average before	(7) 2-month average post vs. 2-month average before	(8) 2-month average post vs. 2-month average before
Medium or High Security Deployment	-0.009		0.019		0.006		0.014	
	(0.035)		(0.037)		(0.012)		(0.013)	
Medium Security Deployment		0.006		0.050		0.029		0.021
		(0.056)		(0.057)		(0.025)		(0.019)
High Security		-0.021		-0.008		-0.013		0.009
		(0.040)		(0.041)		(0.013)		(0.012)
SIGACTs (2-week lag)	0.045	0.046						
	(0.184)	(0.185)						
SIGACTs (3-week lag)	-0.047	-0.046						
	(0.130)	(0.131)						
SIGACTs (4-week lag)	-0.063	-0.062						
	(0.130)	(0.131)						
SIGACTs (5-week lag)	0.343	0.343	0.251	0.250	-0.045	-0.046		
	(0.209)	(0.210)	(0.195)	(0.195)	(0.074)	(0.074)		
SIGACTs (6-week lag)			-0.035	-0.034	-0.060	-0.059		
			(0.208)	(0.208)	(0.052)	(0.053)		
SIGACTs (7-week lag)			0.131	0.132	-0.034	-0.033		
			(0.158)	(0.158)	(0.069)	(0.069)		
SIGACTs (8-week lag)			-0.156	-0.156	-0.079	-0.079		
			(0.176)	(0.177)	(0.062)	(0.062)		
Total violence previous 5 months	0.538	0.534	0.646	0.642	-0.365	-0.367	-0.639***	-0.640***
	(0.678)	(0.680)	(0.601)	(0.605)	(0.248)	(0.251)	(0.197)	(0.197)
Total violence squared	0.252	0.254	0.028	0.029	0.229*	0.230*	-0.065	-0.065
	(0.492)	(0.492)	(0.392)	(0.392)	(0.133)	(0.132)	(0.118)	(0.118)
Total violence cubed	-0.070	-0.070	-0.054	-0.054	-0.041**	-0.041**	0.008	0.008
	(0.065)	(0.065)	(0.052)	(0.052)	(0.017)	(0.017)	(0.016)	(0.016)
Constant	0.062***	0.062***	0.050***	0.050***	-0.005	-0.005	0.006	0.006
	(0.017)	(0.017)	(0.018)	(0.018)	(0.006)	(0.007)	(0.008)	(0.008)
Observations	1,823	1,823	1,823	1,823	1,823	1,823	1,823	1,823
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 clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4A. Effect of Police Deployment Classification (Medium/High combined) on Polling Center-level Turnout in 2010 Election

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medium/High Security Deployment	-33.36***	-33.59***	-33.85***	-25.60**	-35.98***	-29.84***	-30.00***	-30.36***	-27.19**	-32.22***
	(11.93)	(12.03)	(12.15)	(11.96)	(12.79)	(10.79)	(10.91)	(11.05)	(11.17)	(11.70)
SIGACTs Prediction		-2.70					-1.95			
		(13.64)					(14.58)			
SIGACTs (1-week lag)			-9.18	-0.52	-7.86			-2.74	-0.79	-1.46
			(17.74)	(18.85)	(17.65)			(19.80)	(20.26)	(19.45)
SIGACTs (2-week lag)			4.10	-4.04	3.49			-8.61	-11.86	-9.20
			(16.67)	(17.08)	(16.98)			(17.18)	(16.96)	(17.22)
SIGACTs (3-week lag)			-9.50	-11.42	-1.55			-4.47	-4.85	3.66
			(19.50)	(18.34)	(18.31)			(18.07)	(17.98)	(17.50)
SIGACTs (4-week lag)			-12.99	-16.17	-13.57			-10.77	-13.81	-11.34
			(22.96)	(24.18)	(22.48)			(24.92)	(25.62)	(24.39)
Total violence previous 5 months			28.27	36.95	41.40			7.80	12.28	21.15
			(83.18)	(82.20)	(80.06)			(74.88)	(74.96)	(74.08)
Total violence squared			9.81	3.76	1.72			23.91	21.81	15.67
			(51.97)	(50.70)	(51.07)			(47.89)	(47.87)	(47.85)
Total violence cubed			-2.51	-1.27	-1.68			-3.93	-3.41	-3.08
			(6.083)	(5.77)	(6.00)			(5.39)	(5.36)	(5.41)
Log(|Karzai VS – Abdullah				-23.24***					-6.816*	
VS|)				(4.43)					(3.95)	
PC treated within 1km(=1)					5.92					3.93
					(23.94)					(21.41)
Total PCs within 1km					-8.85**					-9.03***
					(3.84)					(3.47)
Turnout in 2009						0.36***	0.36***	0.36***	0.33***	0.36***
						(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Constant	335.38***	335.85***	336.21***	203.31***	342.99***	248.09***	248.44***	249.35***	216.17***	256.19***
	(2.45)	(3.48)	(4.01)	(25.27)	(5.03)	(13.00)	(12.70)	(12.56)	(24.26)	(12.87)
N	1,823	1,823	1,823	1,817	1,823	1,823	1,823	1,823	1,817	1,823
R2	0.429	0.429	0.431	0.458	0.435	0.489	0.489	0.491	0.496	0.494

Notes: All models include district fixed effects. "PC treated within 1km" is a dummy variable that equals 1 if any polling center (PC) within a 1 km radius received treatment; "Total PCs within 1km" is a variable equal to the number of PCs within a 1 km radius. Robust standard errors clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4B. Effect of Police Deployment Classification (Medium/High separated) on Polling Center-level Turnout in 2010 Election

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medium Security Deployment	-45.28***	-45.64***	-46.26***	-34.10*	-48.46***	-39.88**	-40.15**	-41.30**	-36.63**	-43.39**
	(16.70)	(16.84)	(16.99)	(17.60)	(17.49)	(16.72)	(16.58)	(16.68)	(17.35)	(16.94)
High Security Deployment	-23.19	-23.37	-23.35	-18.54	-24.54*	-21.26*	-21.40	-21.10	-19.35	-22.00
	(14.23)	(14.38)	(14.48)	(13.89)	(14.82)	(12.80)	(13.01)	(13.27)	(13.22)	(13.99)
SIGACTs Prediction		-3.15					-2.32			
		(13.68)					(14.55)			
SIGACTs (1-week lag)			-9.00	-0.43	-7.79			-2.59	-0.69	-1.41
			(17.76)	(18.83)	(17.66)			(19.76)	(20.22)	(19.40)
SIGACTs (2-week lag)			2.64	-4.93	2.05			-9.88	-12.85	-10.47
			(16.58)	(17.00)	(16.89)			(17.02)	(16.82)	(17.07)
SIGACTs (3-week lag)			-9.94	-11.71	-1.99			-4.86	-5.16	3.25
			(19.60)	(18.40)	(18.44)			(18.15)	(18.05)	(17.60)
SIGACTs (4-week lag)			-13.49	-16.42	-14.06			-11.22	-14.09	-11.77
			(23.25)	(24.34)	(22.70)			(25.08)	(25.77)	(24.51)
Total violence previous 5 months			31.26	38.85	44.50			10.46	14.36	23.94
			(85.45)	(83.94)	(82.38)			(76.83)	(76.66)	(76.03)
Total violence squared			9.03	3.27	0.88			23.22	21.29	14.90
			(52.70)	(51.28)	(51.79)			(48.54)	(48.45)	(48.50)
Total violence cubed			-2.40	-1.20	-1.55			-3.82	-3.33	-2.96
			(6.15)	(5.82)	(6.06)			(5.44)	(5.40)	(5.46)
Log(|Karzai VS – Abdullah VS|)				-23.16***					-6.71*	
				(4.44)					(3.97)	
PC treated within 1km(=1)					2.69					1.04
					(24.04)					(21.72)
Total PCs within 1km					-8.87**					-9.05***
					(3.83)					(3.46)
Turnout in 2009						0.36***	0.36***	0.36***	0.33***	0.36***
						(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Constant	335.48***	336.03***	336.35***	203.86***	343.21***	248.29***	248.71***	249.58***	216.79***	256.49***
	(2.45)	(3.47)	(4.01)	(25.37)	(5.03)	(13.00)	(12.70)	(12.55)	(24.38)	(12.85)
N	1,823	1,823	1,823	1,817	1,823	1,823	1,823	1,823	1,817	1,823
R2	0.429	0.429	0.432	0.458	0.435	0.490	0.490	0.491	0.497	0.495

Notes: All models include district fixed effects. "PC treated within 1km" is a dummy variable that equals 1 if any polling center (PC) within a 1 km radius received treatment; "Total PCs within 1km" is a variable equal to the number of PCs within a 1 km radius. Robust standard errors clustered at the district level 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	-0.200
	(0.097)	(0.129)
Total violence previous 5 months	-1.426	-0.251
	(3.043)	(1.206)
Total violence squared	-3.273	-2.748
	(18.071)	(7.495)
Total violence cubed	25.271	11.572
	(28.089)	(11.724)
Constant	0.120***	0.220***
	(0.039)	(0.014)
Observations	130	130
R-Squared	0.208	0.278

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

Table 6. Attitudes vis-à-vis the Police

Panel A: Pre-Election Responses

VARIABLES	(1) Is Afghanistan a democracy?	(2) Satisfaction with Afghan democracy	(3) Will you use courts or police to solve dispute?	(4) How important is it to pay your taxes?	(5) Is the central government doing a good job?
Medium or High Security Deployment	0.184***	-0.186	0.134	0.019	0.129
	(0.049)	(0.129)	(0.082)	(0.039)	(0.156)
Total violence previous 5 months	-0.822	0.089	-1.233	-1.030	-2.080
	(2.337)	(0.994)	(1.913)	(1.216)	(1.693)
Total violence squared	-3.646	-4.269	13.316	4.515	11.745
	(15.833)	(7.059)	(14.321)	(11.766)	(16.417)
Total violence cubed	18.254	10.968	-24.238	-10.116	-17.586
	(26.216)	(11.847)	(25.941)	(23.061)	(32.188)
Constant	0.679***	0.872***	0.273***	0.515***	0.569***
	(0.024)	(0.010)	(0.024)	(0.012)	(0.023)
Observations	130	121	130	130	130
R-squared	0.217	0.223	0.189	0.372	0.318

Panel B: Post-Election Responses

Medium or High Security Deployment	0.104	0.082	-0.027	-0.126*	-0.260***
	(0.123)	(0.174)	(0.047)	(0.062)	(0.024)
Total violence previous 5 months	1.641**	-0.476	2.524**	1.171	1.843**
	(0.629)	(0.999)	(0.928)	(1.101)	(0.620)
Total violence squared	-10.231*	0.423	-19.786***	-8.173	-18.565**
	(5.558)	(6.848)	(6.113)	(9.443)	(7.386)
Total violence cubed	10.825	-2.953	32.101***	16.546	37.047**
	(10.856)	(11.408)	(9.831)	(17.455)	(15.746)
Constant	0.682***	0.798***	0.198***	0.440***	0.449***
	(0.008)	(0.011)	(0.010)	(0.009)	(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	0.274	-0.162***	-0.145	-0.389**
	(0.158)	(0.310)	(0.052)	(0.100)	(0.176)
Total violence previous 5 months	2.463	-0.491	3.757	2.201	3.923*
	(2.516)	(1.837)	(2.136)	(1.748)	(2.140)
Total violence squared	-6.586	1.421	-33.102**	-12.688	-30.310*
	(18.402)	(12.717)	(13.516)	(11.631)	(15.929)
Total violence cubed	-7.429	-6.253	56.338**	26.662	54.633*
	(31.589)	(21.035)	(23.072)	(19.251)	(27.551)
Constant	0.003	-0.083***	-0.074**	-0.076***	-0.119***
	(0.022)	(0.018)	(0.030)	(0.019)	(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 clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Damaging Democracy?
Security Provision and Turnout in Afghan Elections

Supporting Information

In this Supporting Information, we provide further information about the data used in the empirical analysis, robustness checks on our main results, and tests of alternate explanations.

A. Data Appendix

A.1 *Turnout*

In the main analysis, we use turnout data from the Independent Election Commission (IEC) of Afghanistan for the 2009 and 2010 elections (http://afghanistanelectiondata.org/open/data). The IEC's procedures for auditing and certifying election results and investigating complaints, as well as election observation, are detailed extensively in NDI (2010) and NDI (2011). In particular, we refer readers to NDI (2011, pp. 36-38), which describes the institutional chain of custody for vote counting, recording, and certification in the 2010 election.

A.2 *ANQAR survey data*

The first survey dataset we employ consists of two waves from the International Security Assistance Force's (ISAF's) Afghanistan Nationwide Quarterly Research (ANQAR) survey (Berman, Callen, Felter, & Shapiro, 2011), fielded closest to the election by D3 Systems' local subsidiary, the Afghan Center for Socio-Economic and Opinion Research (ACSOR). The sample was drawn using a stratified multi-stage cluster design. D3 used the 2015–2016 updated figures provided by the Afghan government's Central Statistics Office (CSO).[1] D3 selected this data because, similar to the 2010–2011 update, much of the 2015–2016 update is based on data drawn from the Ministry of Rural Reconstruction and Development, relying on results from the National Reconstruction Vulnerability

[1] Afghanistan has no official census. The CSO has attempted to provide updates since 2003, but its base is influenced by figures from the 1979 census. The CSO has received support from the UN, the Ministry of Rural Reconstruction and Development, and the World Food Programme to issue updates. D3 reviewed the data used in our analysis and maintains they are acceptable replacements for the 2006 estimates.

Assessment (NRVA) and based on a detailed cataloging of households. While the proportions by province have mostly changed in a uniform, formulaic manner, the additional use of NRVA data increases the detail of the estimates.

Primary sampling units (PSU) were allocated across all of Afghanistan's 34 provinces using proportional stratification. Urban/rural status and province serve as the strata. Villages were considered rural, while towns, cities, and metros were considered urban. Settlements or neighborhoods within randomly selected districts were chosen by simple random sampling. Districts were selected via probability proportional to size systematic sampling. Districts serve as the PSU. A random walk method using a fixed sampling interval was performed from the starting point. For example, selecting every third house on the right in rural areas and every fifth house on the right in urban areas. After selecting a household, interviewers were instructed to utilize a Kish grid to randomize the target respondent within the household from a list with their names and ages in descending order. The Kish grid provides a random selection criteria based on which visit the household represents in his or her random walk and the number of inhabitants living in the household. Under no circumstances were interviewers allowed to substitute an alternate member of the household for the selected respondent. If the respondent refused to participate or was not available after three callbacks, the interviewer then moved onto the next household according to the random walk. Typically, interviewers were required to make two callbacks before replacing the household. These callbacks were made at different times of the same day or on different days in order to increase the chances of engaging the respondent. Data from surveys were subjected to three separate quality control tests using proprietary software, and a subset of surveys were randomly selected for double data entry (reported error rate of 0.1%). Further information about the survey is on file with the corresponding author.

A.3 *Kandahar survey data*

The second survey dataset we use consists of responses from (mostly Pashtun) residents of Kandahar Province, in southern Afghanistan, from the election period. We use geocoded survey responses from 369 Afghan civilians collected between August and October 2010 in 84 villages of Maiwand and Arghandab districts. The data were 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 5–20 individuals surveyed, depending on population density.

A.4 *Primary survey data*

In the main analysis, we use two surveys that authors Callen and Long (2015) designed and administered to households living in the immediate vicinity of polling sites in August (baseline) and November/early December (endline) 2010. Our baseline survey (2,904 respondents) comprises 450 polling centers (7.8% operating on election day) in 19 of 34 provincial capitals. Our endline survey (3,100 respondents) includes 471 polling centers, matching the baseline sample with 21 centers added in Kabul. We selected our sample by identifying polling centers scheduled to open on election day and deemed secure by ISAF and the Afghan National Police (ANP). To obtain a representative sample of respondents living near polling centers, enumerators employed a random walk pattern starting from the polling site, and randomly selecting every fourth house or structure. Respondents within households were randomly selected using a Kish grid, with a 50% female sample. To improve the accuracy of the responses and avoid response bias, respondents could choose to take the survey in Dari or Pashto, the first language spoken by nearly all Afghans. Female respondents were interviewed by female enumerators. These data are more fully described in Callen and Long (2015), Callen, Isaqzadeh, Long, and Sprenger (2014), and Berman, Callen, Gibson, Long, and Rezaee (2018), which also address how the survey design and question wording guard against response bias. The survey data collection was approved by the University of California, San Diego's Institutional Review Board (#101052S).

A.5 *The Asia Foundation survey data*

We use data from the Asia Foundation's nationwide survey of Afghans administered in 2010 (wave 5). ACSOR administered this survey to a random, representative sample of 6,348 Afghan adults. The survey methodology is extensively described in Tariq, Ayoubi, and Haqbeen (2011, p. 179 ff.), including field dates, sampling, weighting, household and respondent selection, callback methods and results, enumerator training, and quality control methods.

A.6 *Violent Incidence, i.e. SIGACTs*

These data were shared with us courtesy of Andrew Shaver and Austin Wright, and are more fully described in Shaver and Wright (2017) and available upon request from those authors. These data were released to those authors by the U.S. Department of Defense as the Significant Activities (SIGACTs) Dataset and were collected by Afghanistan's military and police forces and ISAF during the course of Operation Enduring Freedom. Incidents in the data constitute insurgent attacks perpetrated against ISAF and the Afghan security forces, such as direct fire, indirect fire, and improvised explosive devices, and are coded with military grid coordinates, often to the specific minute of occurrence. Data

were collected according to "well-established military protocol and with the use of advanced georeferencing and collation technologies, ensuring that many report details were both objectively measured and captured with a high degree of precision" (Shaver & Wright, 2017). The systematic nature of data collection, coupled with the extreme geographic and temporal precision of incident coding, make these data much less susceptible to common biases inherent in data compiled based on media reporting (Weidmann, 2016). For examples of empirical analysis that use SIGACTs data, see Condra and Shapiro (2012), Biddle, Friedman, and Shapiro (2012), and Shaver and Shapiro (forthcoming) in Iraq, and Condra, Long, Shaver, and Wright (forthcoming) in Afghanistan.

We argue that these data are a valid and useful measure of threats to civilians who are deciding whether to turn out to vote for three reasons. First, the dataset contains the most complete and comprehensive data measuring violence during this period. No other dataset comes close to it, either in the precision of coding incident locations or in the sheer number of incidents recorded.[2] Second, we now have considerable evidence that these incidents carry considerable risk of harm to civilians, even though the primary targets of the attacks are government forces and installations. As a general point, this is true in Afghanistan (Shaver & Shapiro, forthcoming; Wright, Condra, Shapiro, & Shaver, 2017) as well as Iraq (Berman, Shapiro, & Felter, 2011; Condra & Shapiro, 2012), where we have comparable data.

More importantly for this study, we now have high-quality causal evidence from Afghan elections that the violent events recorded in the SIGACTs data not only injure and kill civilians at a significant rate, but they also (negatively) affects citizens' willingness to turn out and vote (Condra et al., forthcoming). Citizens' beliefs about the ability of local insurgents to target and attack appears to be a major factor impacting citizens' risk assessments, and the SIGACTs event data are a useful metric for the larger universe of violence informing those assessments. Third, precisely because insurgents seek to disrupt the electoral process by attacking state targets (not civilians), the type of violence recorded in administrative event data is exactly the right type to use to measure citizens' perceived threat to themselves on and around election day.

The presence of other violence against civilians not captured in these data does not, in and of itself, pose a problem for inference. To be problematic, the following would have to be true. First, some other dimension of violence targeting civilians would have to be orthogonal to SIGACTs. Second, officials

[2] For example, other datasets, such as those from the UN Assistance Mission in Afghanistan, the Global Terrorism Database, or the National Democratic Institute, do not provide information at sufficient temporal or geographic specificity to enable matching of incidents to polling center locations.

would have had to assign treatment *not* according to an assessment of the history of SIGACTs around polling centers, as they claimed, but according to this other measure of insurgent violence against civilians. Assigning treatment in this way would have required officials to have access to a similarly systematic measure of that non-SIGACTs violence, something we found no evidence of in many years working on the conflict. Third, the other dimension of violence targeting civilians would have had to systematically depress turnout in the same way as the dimension associated with SIGACTs. These conditions seem unlikely to be met. We use the SIGACTs data because it is geographically precise, nearly comprehensive in scope for violence against state targets during the period of study, systematically collected, and empirically substantiated as a valid measure of citizens' perceived risk of turning out to vote.

B. Robustness Checks

While we have shown that the assignment of levels of police deployment to polling centers was not a function of previous levels of violence, and have argued for plausibly exogenous variation in this assignment, we further test that claim in two principal ways.

B.1 *Sensitivity of Effects of Interest to the Inclusion of Observed Controls*

We test whether our results are robust to the inclusion of controls for other observables. We consider four principal factors that might correlate with both police deployment levels *and* turnout, such that the 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 robust to the inclusion of variables capturing these potential confounds, which is consistent with our interpretation of the estimates.

The first is the competitiveness of the 2009 election. 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 the 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 the political use of police. In their study of fraud in the 2010 Afghan election, Callen and Long (2015) use measures of the political connectedness of candidates running in the 2010 election. They explain how candidates' connections to provincial- or district-level election officials might affect the level of fraud associated with the electoral returns reported for a polling center, particularly fraud that is perpetrated at the Provincial Aggregation Center and other levels above the polling center. If this connectedness affected police deployment and turnout, we should expect the bias to be in the positive direction. Given that these factors operate at the district or province level (Rundlett and Svolik (2016) note that fraud often is perpetrated at the local – rather than central – level of political organization), we control for them by including 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 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 the local level. Ethnic groups are geographically clustered by district, to the extent that there is a low level of heterogeneity in ethnic mixes of the population across districts. Therefore including 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 the bias is in the opposite direction of our hypothesized results. Examining the effect of police deployment on violence and finding a weak negative effect thus constitutes a "hard" test. SI Table 2 summarizes these factors, their expected correlations with the independent and dependent 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 suggest that these sets 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 that code 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. (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 the 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 in 2009. The results are largely unaffected.

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 the area's history of violence, which officials responsible for developing the plan identified as the determinant. 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 does 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.

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: ratings of President Karzai's performance in office, respondents' beliefs about whether 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 that i) they are likely orthogonal to the security assignment per our identification strategy and ii) 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.

Finally, we test for spatial externalities (SI Tables 8A–B and 9), following the approach in Callen and Long (2015). In SI Tables 8A–B, we regress polling center security deployment classification on election-day violence, replicating models in Table 3 of the main text, but adding controls for the number of polling stations at various distances that were also assigned medium/high deployment: a dummy variable that equals 1 if any polling center within a 1 (2) km radius was assigned medium/high deployment; a variable equal to the number of polling centers within a 1 (2) km radius; and a dummy variable that equals 1 if one (or two, or three) polling center within a 1 (2) km radius was treated. SI Tables 8A and 8B collapse medium/high deployment as treatment; Table 8A includes the set of treated polling centers listed above within a 1 km radius, while SI Table 8B expands the radius to 2 km. The null result of the impact of police deployments on violence is unchanged once we control for nearby police deployments. In SI Table 9, we regress turnout on police deployment classification, replicating model 8 of Tables 4A–B, but add various controls for nearby deployments: a dummy variable that equals 1 if any polling center within a 1 km radius was assigned medium/high deployment; a variable equal to the number of polling centers within a 1 km radius; and a dummy variable that equals 1 if one (or two, or three) polling center within a 1 km radius was assigned medium/high deployment. The

addition of these controls does not alter the size or significance of the estimated effects of police deployment classification on either violence or turnout reported in the main results.

B.2 *Estimating Bias from Unobservables*

While we have shown that our results are robust 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 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.[3] We follow Nunn and Wantchekon (2011, pp. 3237-3238) in implementing this approach, comparing estimates of the police deployment classification effect $\left(\hat{\beta}\right)$ from two regressions: one with a full set of controls $\left(\hat{\beta}^F\right)$ and one with a restricted set of controls $\left(\hat{\beta}^R\right)$. The ratio of these estimates $\left(\hat{\beta}^F/\left(\hat{\beta}^R-\hat{\beta}^F\right)\right)$ is increasing in the size of the effect of unobservables that would be necessary to explain away the police deployment classification effect.

SI Table 7 provides the 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 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 effect of police deployment 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 effects of interest 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.

[3] 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 independent variable as the part of the outcome that is related to unobservables (Altonji et al., 2005, p. 154).

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
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

Factor	*Expected Effect on Police*	*Expected Effect on Turnout*
Competitiveness of the 2009 election	?	(+) Voters turn out if they believe their vote will be consequential.
President Karzai's interests	?	?
Ethnic politics	(+) Pashtun areas expected to be more violent and could receive more police.	(+) Non-Pashtun areas support 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***	-37.6***	-38.9***	-33.9***	-25.6**
	(13.0)	(13.0)	(13.9)	(12.1)	(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	-20.5	-11.2	-9.2	-0.5
	(19.8)	(19.7)	(18.6)	(17.7)	(18.8)
SIGACTs (2-week lag)	18.0	18.2	11.7	4.1	-4.0
	(20.1)	(20.1)	(18.4)	(16.7)	(17.1)
SIGACTs (3-week lag)	-12.6	-12.6	-5.0	-9.5	-11.4
	(19.3)	(19.3)	(18.6)	(19.5)	(18.3)
SIGACTs (4-week lag)	14.5	15.1	7.6	-13.0	-16.2
	(21.2)	(20.8)	(19.8)	(23.0)	(24.2)
Total violence previous 5 months	-162.3*	-163.5*	-127.7	28.3	37.0
	(92.5)	(93.2)	(89.3)	(83.2)	(82.2)
Total violence squared	103.1*	103.2*	84.8	9.8	3.8
	(59.8)	(59.8)	(57.3)	(52.0)	(50.7)
Total violence cubed	-14.4*	-14.4*	-12.0*	-2.5	-1.3
	(7.3)	(7.4)	(7.0)	(6.1)	(5.8)
Pashtun Majority District		-7.7			
		(24.1)			

Log(|Karzai VS - Dr. Abdullah VS|)					-23.2***
					(4.4)
Constant	340.7***	343.6***	415.2***	336.2***	203.3***
	(7.1)	(12.4)	(44.4)	(4.0)	(25.3)
N	1,823	1,823	1,823	1,823	1,817
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***	-38.1***	-38.5***	-30.4***	-27.2**
	(11.9)	(11.9)	(12.7)	(11.1)	(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	-7.9	-2	-2.7	-0.8
	(19.1)	(19)	(18.4)	(19.8)	(20.3)
SIGACTs (2-week lag)	5.2	5.4	1.4	-8.6	-11.9
	(18.9)	(18.8)	(17.6)	(17.2)	(17)
SIGACTs (3-week lag)	-1.2	-1.1	2.6	-4.5	-4.9
	(16)	(15.9)	(15.8)	(18.1)	(18)
SIGACTs (4-week lag)	14.9	15.7	10.8	-10.8	-13.8
	(20.5)	(20.5)	(19.9)	(24.9)	(25.6)
Total violence previous 5 months	-163.8**	-165.3**	-136.4*	7.8	12.3
	(82.8)	(83.7)	(79.7)	(74.9)	(75)
Total violence squared	111.7**	111.8**	96.5*	23.9	21.8
	(51.9)	(52)	(49.7)	(47.9)	(47.9)
Total violence cubed	-15.6**	-15.6**	-13.6**	-3.9	-3.4
	(6.5)	(6.5)	(6.1)	(5.4)	(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	1,823	1,823	1,823	1,823	1,817
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. Assignment of Police Deployment Classification and Violence
(including "fraudulent" 2010 Polling Centers and Polling Centers not open in 2009)

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	-0.019	0.014	
	(0.075)	(0.075)	(0.083)	
SIGACTs (2-week lag)	0.018	-0.047	-0.03	
	(0.06)	(0.059)	(0.068)	
SIGACTs (3-week lag)	0.015	0.004	0.064	
	(0.071)	(0.066)	(0.053)	
SIGACTs (4-week lag)	0.064	0.049	-0.071	
	(0.053)	(0.051)	(0.065)	
Total violence previous 5 months	0.274	0.13	-0.571*	-0.478
	(0.306)	(0.319)	(0.311)	(0.3)
Total violence squared	-0.205	-0.124	0.223	0.165
	(0.169)	(0.167)	(0.152)	(0.157)
Total violence cubed	0.019	0.013	-0.02	-0.016
	(0.02)	(0.02)	(0.017)	(0.018)
Constant	1.363***	1.371***	1.388***	1.387***
	(0.032)	(0.027)	(0.008)	(0.009)
N	2,290	2,290	2,290	2,290
R-squared	0.003	0.104	0.335	0.333

Notes: Unlike Table 2, these models include polling centers with turnout results from 2010 that indicate fraud, as well as those that were not open in 2009. The dependent variable is polling center security deployment category (1=Low, 2=Medium, 3=High). Robust standard errors clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

SI Table 5A. Effect of Police Deployment Classification (Medium/High combined) on Polling Center-level Turnout in 2010 Election (including "fraudulent" 2010 Polling Centers and Polling Centers not open in 2009)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medium/High Security Deployment	-6.81	-6.58	-7.21	-11.97	-16.45	-19.07*	-18.66*	-19.97*	-16.96	-26.84**
	(11.74)	(11.81)	(11.89)	(12.45)	(12.28)	(11.20)	(11.27)	(11.54)	(11.54)	(11.74)
SIGACTs Prediction		3.72					5.68			
		(9.47)					(12.99)			
SIGACTs (1-week lag)			-1.93	11.82	0.13			7.53	9.79	9.07
			(13.84)	(17.76)	(14.11)			(17.80)	(18.18)	(17.71)
SIGACTs (2-week lag)			-3.33	-5.86	-5.12			-7.99	-10.94	-9.70
			(14.81)	(15.93)	(15.01)			(15.92)	(15.66)	(15.75)
SIGACTs (3-week lag)			8.17	-11.48	12.08			-3.21	-3.90	5.69
			(10.32)	(17.94)	(9.50)			(17.94)	(17.75)	(17.31)
SIGACTs (4-week lag)			-2.50	-23.06	-5.62			-16.65	-18.25	-17.92
			(16.47)	(17.64)	(15.65)			(18.14)	(18.30)	(18.22)
Total violence previous 5 months			-56.85	48.57	-38.33					
			(68.81)	(84.95)	(65.78)					
Total violence squared			48.41	-2.31	38.41					
			(36.39)	(45.29)	(35.44)					
Total violence cubed			-6.62	0.62	-5.34					
			(4.54)	(5.55)	(4.43)					
Log(|Karzai VS – Abdullah VS|)				-24.97***					-7.69**	
				(4.04)					(3.71)	
PC treated within 1 km(=1)					45.10*					32.48
					(23.91)					(23.66)
Total PCs within 1 km					-8.88**					-9.94***
					(4.22)					(3.59)
Turnout in 2009						0.36***	0.36***	0.36***	0.33***	0.36***
						(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Constant	375.11***	374.476***	375.83***	227.82***	381.68***	278.01***	277.10***	278.26***	241.71***	284.81***
	(2.89)	(3.43)	(3.68)	(23.60)	(5.02)	(12.22)	(12.18)	(12.00)	(22.46)	(12.25)
N	2,290	2,290	2,290	2,110	2,290	2,116	2,116	2,116	2,110	2,116
R2	0.495	0.495	0.496	0.511	0.499	0.538	0.538	0.539	0.544	0.542

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

SI Table 5B. Effect of Police Deployment Classification (Medium/High separated) on Polling Center-level Turnout in 2010 Election (including "fraudulent" 2010 Polling Centers and Polling Centers not open in 2009)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medium Security Deployment	-17.65	-17.44	-18.54	-22.42	-26.39*	-31.31**	-30.84*	-33.17**	-28.93*	-39.17**
	(14.50)	(14.50)	(14.42)	(17.24)	(14.90)	(15.75)	(15.69)	(15.85)	(16.29)	(16.02)
High Security Deployment	2.69	2.96	2.68	3.28	-7.32	-8.61	-8.30	-8.83	-7.02	-15.82
	(14.50)	(14.60)	(14.71)	(14.88)	(14.81)	(14.30)	(14.42)	(14.68)	(14.54)	(14.97)
SIGACTs Prediction		3.82					5.41			
		(9.67)					(13.01)			
SIGACTs (1-week lag)			-0.93	12.22	0.98			8.06	10.24	9.51
			(13.92)	(17.64)	(14.19)			(17.60)	(17.99)	(17.54)
SIGACTs (2-week lag)			-4.13	-6.66	-5.82			-9.12	-11.86	-10.76
			(14.69)	(15.89)	(14.92)			(15.85)	(15.61)	(15.70)
SIGACTs (3-week lag)			7.84	-11.83	11.75			-3.65	-4.28	5.26
			(10.34)	(18.00)	(9.55)			(18.03)	(17.82)	(17.42)
SIGACTs (4-week lag)			-2.51	-23.40	-5.62			-17.14	-18.62	-18.40
			(16.60)	(17.80)	(15.76)			(18.33)	(18.48)	(18.37)
Total violence previous 5 months			-57.09	49.59				21.35	24.03	35.02
			(70.15)	(86.62)				(79.60)	(79.39)	(78.40)
Total violence squared			49.21	-2.28				15.74	13.08	7.88
			(36.88)	(45.86)				(42.34)	(42.21)	(42.28)
Total violence cubed			-6.73	0.62				-1.89	-1.37	-1.05
			(4.59)	(5.61)				(5.20)	(5.17)	(5.25)
Log(|Karzai VS – Abdullah VS|)				-24.92***					-7.60**	
				(4.05)					(3.73)	
PC treated within 1km(=1)					43.82*					30.64
					(23.87)					(23.80)
Total PCs within 1km					-8.84**					-9.96***
					(4.22)					(3.57)
Turnout in 2009						0.36***	0.36***	0.36***	0.33***	0.36***
						(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Constant	375.23***	374.58***	375.96***	228.30***	381.80***	278.17***	277.30***	278.48***	242.28***	285.04***
	(2.86)	(3.41)	(3.66)	(23.64)	(5.01)	(12.19)	(12.15)	(11.96)	(22.50)	(12.21)
N	2,290	2,290	2,290	2,110	2,290	2,116	2,116	2,116	2,110	2,116

R2	0.495	0.495	0.496	0.511	0.5	0.539	0.539	0.54	0.544	0.543

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

SI Table 5B. Effect of Police Deployment Classification (Medium/High separated) on Polling Center-level Turnout in 2010 Election (including "fraudulent" 2010 Polling Centers and Polling Centers not open in 2009)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medium Security Deployment	-33.409	-17.65	-17.439	-18.541	-64.948***	-31.312**	-30.838*	-33.173**
	(21.76)	(14.497)	(14.495)	(14.418)	(17.488)	(15.745)	(15.693)	(15.853)
High Security Deployment	36.437*	2.691	2.955	2.684	-4.119	-8.609	-8.298	-8.827
	(20.612)	(14.497)	(14.601)	(14.707)	(17.539)	(14.303)	(14.415)	(14.684)
SIGACTs Prediction			3.818				5.413	
			(9.673)				(13.013)	
SIGACTs (1-week lag)				-0.926				8.061
				(13.916)				(17.596)
SIGACTs (2-week lag)				-4.132				-9.122
				(14.688)				(15.847)
SIGACTs (3-week lag)				7.835				-3.646
				(10.336)				(18.034)
SIGACTs (4-week lag)				-2.512				-17.141
				(16.597)				(18.331)
Total violence previous 5 months				-57.091				21.346
				(70.153)				(79.601)
Total violence squared				49.212				15.737
				(36.877)				(42.34)
Total violence cubed				-6.734				-1.891
				(4.587)				(5.202)
Turnout in 2009					0.513***	0.358***	0.358***	0.357***
					(0.045)	(0.045)	(0.045)	(0.045)
Constant	372.901***	375.231***	374.575***	375.961***	240.557***	278.169***	277.301***	278.480***
	(12.359)	(2.862)	(3.406)	(3.661)	(14.525)	(12.193)	(12.148)	(11.963)
District FE	N	Y	Y	Y	N	Y	Y	Y
N	2,290	2,290	2,290	2,290	2,116	2,116	2,116	2,116
R2	0.009	0.495	0.495	0.496	0.199	0.539	0.539	0.540

Notes: Unlike Table 4B, these models include polling centers with turnout results from 2010 that indicate fraud, as well as those that were not open in 2009. Robust standard errors clustered at the district level 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) Intraclass Correlation by Polling Center (within district)	(2) Expected Intraclass Correlation
Panel A		
Electricity	0.603	High
Pashtun	0.894	High
Tajik	0.744	High
Uzbek	0.630	High
Panel B		
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.

SI Table 8A. Effect of Police Deployment Classification on Violence (Timing Change at Election) – Spatial Treatment Externalities (1 km)

	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
Med/High Security Deployment	-0.042	-0.038	-0.006	-0.002	0.001	0.0002	0.015	0.016
	(0.046)	(0.046	(0.042)	(0.042)	(0.013)	(0.013)	(0.013)	(0.014)
PC treated within 1 km(=1)	0.209		0.153		0.027		-0.006	
	(0.151)		(0.121)		(0.03)		(0.017)	
Total PCs within 1 km	-0.018	-0.02	-0.027	-0.028	-0.015***	-0.014***	-0.001	-0.002
	(0.018)	(0.018)	(0.017)	(0.017)	(0.005)	(0.005)	(0.009)	(0.01)
1 treated PC within 1 km(=1)		0.205		0.146		0.033		-0.01
		(0.172)		(0.142)		(0.035)		(0.018)
2 treated PCs within 1 km(=1)		0.062		0.027		0.032		-0.019
		(0.082)		(0.075)		(0.031)		(0.023)
3 treated PC within 1 km(=1)		0.911		0.837		-0.147*		0.128
		(0.694)		(0.783)		(0.082)		(0.105)
SIGACTs (2-week lag)	0.04	0.039						
	(0.185)	(0.184)						
SIGACTs (3-week lag)	-0.03	-0.027						
	(0.131)	(0.131)						
SIGACTs (4-week lag)	-0.067	-0.09						
	(0.131)	(0.129)						
SIGACTs (5-week lag)	0.356*	0.359*	0.269	0.271	-0.037	-0.038		
	(0.21)	(0.21)	(0.196)	(0.197)	(0.073)	(0.073)		
SIGACTs (6-week lag)			-0.032	-0.029	-0.058	-0.059		
			(0.198)	(0.197)	(0.049)	(0.049)		
SIGACTs (7-week lag)			0.156	0.153	-0.017	-0.017		
			(0.158)	(0.156)	(0.066)	(0.066)		
SIGACTs (8-week lag)			-0.166	-0.162	-0.086	-0.087		
			(0.173)	(0.172)	(0.063)	(0.063)		
Total violence previous 5 months	0.566	0.547	0.736	0.7	-0.313	-0.304	-0.634***	-0.639***
	(0.678)	(0.667)	(0.601)	(0.575)	(0.247)	(0.246)	(0.19)	(0.19)
Total violence squared	0.229	0.25	-0.059	-0.042	0.179	0.175	-0.068	-0.065
	(0.487)	(0.48)	(0.398)	(0.387)	(0.122)	(0.122)	(0.113)	(0.113)
Total violence cubed	-0.068	-0.069	-0.043	-0.046	-0.035**	-0.034**	0.008	0.008
	(0.064)	(0.064)	(0.052)	(0.051)	(0.015)	(0.015)	(0.015)	(0.015)
Constant	0.073***	0.072***	0.067***	0.066***	0.006	0.006	0.007	0.007
	(0.019)	(0.019)	(0.017)	(0.017)	(0.008)	(0.008)	(0.011)	(0.011)
Observations	1,823	1,823	1,823	1,823	1,823	1,823	1,823	1,823
R-squared	0.506	0.509	0.513	0.516	0.649	0.65	0.79	0.791

Notes: Replicates Table 3, but includes: a dummy variable that equals 1 if any polling center (PC) within a 1 km radius received treatment; total PCs within 1 km is a variable equal to the number of PCs within a 1 km radius; a dummy variable that equals 1 if one (two, and three) PC within a 1 km radius was treated. All regressions include district fixed effects. Robust standard errors clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

SI Table 8B. Effect of Police Deployment Classification on Violence (Timing Change at Election) – Spatial Treatment Externalities (2 km)

	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
Med/High Security Deployment	0.005	0.004	0.034	0.032	0.008	0.009	0.015	0.015
	(0.036)	(0.036)	(0.036)	(0.036)	(0.015)	(0.015)	(0.014)	(0.014)
PC treated within 2km(=1)	-0.051		-0.065		-0.014		-0.006	
	(0.064)		(0.07)		(0.025)		(0.015)	
Total PCs within 2km	0.001	0.002	-0.005	-0.005	-0.007**	-0.007**	-0.002	-0.002
	(0.009)	(0.009)	(0.008)	(0.008)	(0.003)	(0.003)	(0.004)	(0.004)
1 treated PC within 2km(=1)		-0.044		-0.065		-0.019		-0.009
		(0.074)		(0.08)		(0.029)		(0.017)
2 treated PCs within 2km(=1)		-0.109		-0.093		-0.004		-0.003
		(0.15)		(0.12)		(0.023)		(0.018)
3 treated PC within 2km(=1)		0.034		0.06		-0.01		0.021
		(0.223)		(0.216)		(0.034)		(0.035)
SIGACTs (2-week lag)	0.046	0.047						
	(0.185)	(0.185)						
SIGACTs (3-week lag)	-0.049	-0.051						
	(0.13)	(0.13)						
SIGACTs (4-week lag)	-0.059	-0.059						
	(0.133)	(0.132)						
SIGACTs (5-week lag)	0.342	0.344	0.254	0.255	-0.04	-0.04		
	(0.212)	(0.212)	(0.199)	(0.199)	(0.073)	(0.073)		
SIGACTs (6-week lag)			-0.031	-0.031	-0.056	-0.056		
			(0.205)	(0.204)	(0.049)	(0.049)		
SIGACTs (7-week lag)			0.149	0.147	-0.012	-0.012		
			(0.165)	(0.164)	(0.064)	(0.064)		
SIGACTs (8-week lag)			-0.155	-0.155	-0.077	-0.077		
			(0.176)	(0.176)	(0.061)	(0.061)		
Total violence previous 5 months	0.537	0.525	0.691	0.685	-0.31	-0.308	-0.619***	-0.618***
	(0.687)	(0.675)	(0.606)	(0.601)	(0.249)	(0.249)	(0.188)	(0.188)
Total violence squared	0.251	0.256	-0.029	-0.025	0.161	0.159	-0.076	-0.077
	(0.5)	(0.496)	(0.397)	(0.389)	(0.122)	(0.122)	(0.11)	(0.109)
Total violence cubed	0.07	-0.071	-0.047	-0.047	-0.032**	-0.032**	0.009	0.01
	(0.066)	(0.066)	(0.052)	(0.051)	(0.015)	(0.015)	(0.015)	(0.014)
Constant	0.063***	0.062***	0.065***	0.064***	0.011	0.011	0.01	0.01
	(0.023)	(0.024)	(0.019)	(0.02)	(0.009)	(0.009)	(0.012)	(0.012)
Observations	1,823	1,823	1,823	1,823	1,823	1,823	1,823	1,823
R-squared	0.503	0.503	0.511	0.511	0.649	0.65	0.791	0.791

Notes: Replicates Table 3, but includes: a dummy variable that equals 1 if any polling center (PC) within a 2 km radius received treatment; total PCs within 2 km is a variable equal to the number of PCs within a 2 km radius; a dummy variable that equals 1 if one (two, and three) PC within a 2 km radius was treated. All regressions include district fixed effects. Robust standard errors clustered at the district level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

SI Table 9. Effect of Police Deployment Classification on Polling Center-level Turnout in 2010 Election, Controlling for Turnout in 2009 Election – Spatial Treatment Externalities

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Medium/High Security Deployment	-32.224***		-26.516**		-33.412***	
	(11.702)		(12.148)		(11.69)	
Medium Security Deployment		-43.388**		-38.240**		-43.284**
		(16.937)		(17.56)		(16.884)
High Security Deployment		-21.996		-16.414		-24.304*
		(13.985)		(14.125)		(13.774)
PC treated within 1 km(=1)	3.929	1.038				
	(21.405)	(21.716)				
Total PCs within 1 km	-9.034***	-9.048***			-8.721**	-8.734**
	(3.471)	(3.457)			(3.484)	(3.472)
1 treated PC within 1 km(=1)			-18.944	-19.387		
			(16.799)	(16.798)		
2 treated PCs within 1 km(=1)			-2.838	-2.89		
			(2.139)	(2.135)		
3 treated PCs within 1 km(=1)					2.302	-0.036
					(21.833)	(22.173)
SIGACTs (1-week lag)	-1.46	-1.409	-2.953	-2.794	0.52	0.535
	(19.454)	(19.398)	(19.623)	(19.579)	(19.636)	(19.592)
SIGACTs (2-week lag)	-9.2	-10.47	-7.157	-8.493	-8.945	-10.084
	(17.218)	(17.073)	(17.169)	(17.033)	(17.051)	(16.935)
SIGACTs (3-week lag)	3.656	3.254	1.236	0.915	2.254	1.916
	(17.502)	(17.596)	(17.21)	(17.337)	(17.505)	(17.593)
SIGACTs (4-week lag)	-11.335	-11.774	-11.055	-11.536	-5.416	-5.896
	(24.391)	(24.506)	(23.814)	(23.968)	(23.407)	(23.477)
Total violence previous 5 months	21.146	23.941	25.266	28.467	24.824	27.241
	(74.083)	(76.034)	(74.394)	(76.486)	(73.961)	(75.684)
Total violence squared	15.666	14.899	10.907	9.902	9.804	9.226
	(47.854)	(48.498)	(47.631)	(48.332)	(47.014)	(47.597)
Total violence cubed	-3.076	-2.959	-2.443	-2.297	-2.639	-2.544
	(5.411)	(5.455)	(5.351)	(5.409)	(5.311)	(5.354)
Turnout in 2009	0.359***	0.359***	0.364***	0.364***	0.360***	0.359***
	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)
Constant	256.191***	256.486***	255.178***	255.533***	256.334***	256.579***
	(12.871)	(12.846)	(13.065)	(13.034)	(12.772)	(12.76)
District FE	Y	Y	Y	Y	Y	Y
N	1,823	1,823	1,823	1,823	1,823	1,823
R2	0.494	0.495	0.493	0.494	0.497	0.497

Notes: Replicates column 8, Tables 4A-B, but includes: a dummy variable that equals 1 if any polling center (PC) within a 1 km radius received treatment; total PCs within 1 km is a variable equal to the number of PCs within a 1 km radius; a dummy variable that equals 1 if one (two, and three) PC within a 1 km radius was treated. Robust standard errors clustered on district in parentheses. *** p<0.01, ** p<0.05, * p<0.1

SI Table 10. Citizens' Fear of Voting and Fear of ANP

		Fear when encountering ANP officers		
		No fear	Some/a lot of fear	Total
Fear of voting in a national election	No fear	1,714	806	2,520
		58.84%	24.07%	40.25%
	Some/a lot of fear	1,199	2,542	3,741
		41.16%	75.93%	59.75%
	Total	2,913	3,348	6,261
		100%	100%	100%

Notes: Pearson's $\chi_1^2 = 782.85$, $p<0.001$. Cells provide N and (column) percentages for responses to the question: "Now I will read you five different activities that you could participate in. Please, tell me, whether you would participate in the following activities with 'no fear', 'some fear' or a 'lot of fear'?" No response and refuse to answer were omitted for ease of presentation but does not affect results. Data are from the Asia Foundation's nationwide Afghanistan survey (wave 5), conducted in 2010, prior to the national election.

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